When and How are People 'Behavioral'? Evidence from Intertemporal Choices*

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Abstract: How malleable are preferences over short time horizons and in what ways do preferences change? In the laboratory, we estimate the effects of prior exertion of self-control, consumption of a sugared drink, and consumption of a placebo drink on intertemporal monetary choice. All three treatments increase savings among subjects with lower cognitive ability. To understand these results, we estimate the effects of the treatments on the three parameters of a simple decision utility model. All treatments primarily affect utility curvature, with smaller effects on present bias. These two parameters are closely correlated within individuals, in that increased utility curvature corresponds to more present bias. We argue that these patterns are best explained by a treatment effect on income-as-consumption error.

Keywords: Time preferences, present bias, availability heuristic, experiment.
JEL-codes: C91, D90

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1 – Introduction

Biases, heuristics and non-standard economic models of behavior are not meant to describe the decision making of all individuals. Just as there is across-individual heterogeneity in the degree of risk aversion and time discounting, there is variation in both the existence and magnitude of a variety of behavioral traits, including loss aversion, present bias, and the availability heuristic. A more controversial notion is that there is within-individual variation in these traits as well. Most literature on this topic focuses on traumatic events that precipitate a one-time, permanent shock, focusing on changes in one particular preference or 'behavioral' parameter (Cassar *et al*. 2011; Malmendier and Nagel 2011; Moya, 2011; Voors *et al*. 2012; Bucciol and Zarri 2013; Kim and Lee 2013; Byder *et al*. 2014; Callen *et al*. 2014; Ingwersen 2014; Cameron and Shah 2015; Imas *et al*. 2015). Less attention is paid to the more troublesome issue of frequent, transitory changes and to disentangling which of multiple possible biases are affected. The task of identifying not only *who* is 'behavioral', but *when* and *how* they are 'behavioral' (i.e. which traits are affected) remains a significant challenge.

In this paper we study intertemporal monetary choice, a situation in which the presence of one specific 'behavioral' trait –present bias—does not change the sharp predictions of the standard rational choice model. To see this, consider a choice between \$20 to be received immediately and \$25 to be received in a month. Provided access to credit at an annual interest rate of roughly 1350% or less --which exceeds even the most predatory credit card rates or late bill-payment penalties-- even the most impatient and/or present-biased individual should opt to receive \$25 in a month. More generally, regardless of discount rates or present bias, individuals should arbitrage experimental interest rates with available market rates in any time-preference-over-money experiment, behaving as if their preferences were linear and always picking corner solutions (Chabris *et al*. 2008). Choices that take a different form must therefore involve other, or multiple behavioral biases, and it is interesting to know not only which individuals make such choices, but when they make them and how the choices deviate from 'rational' ones.

In our experiment we study intertemporal choices over money using an adaptation of a Convex Time Budget task from Andreoni and Sprenger (2012). This allows us to estimate three parameters of a decision utility function for each subject and to measure the effect of three interventions on those parameters. The three parameters are the exponential discount factor δ, which describes an individual's overall preference for receiving money sooner relative to later; the present bias parameter β, which describes the change to an individual's sooner-later preference when "sooner" is "immediately"; and the utility curvature parameter α, which measures the rate at which the marginal decision utility of money in a specific time period decays, and in a large part determines the intertemporal elasticity of substitution between experimental payments received at different times. Values of α under one are inconsistent with the present-value maximizing behavior described above, and suggest that experimental subjects are treating income from the experimenter as if it were a type of consumption with diminishing marginal utility. Such *income-as-consumption bias* can be understood as a form of availability bias, according to which individuals decide based on the readily available knowledge (Tversky and Kahneman 1973), or narrow bracketing, according to which individuals assess the consequences of each of several choices taken in isolation or do not relate their experimental decisions to the broader market context in which they live (Read *et al*. 1999; Rabin and Weizsacker 2009). Our three interventions are motivated in part by the Israeli parole board study of Danziger *et al.* (2011), who found that two factors that vary at high temporal frequency for almost all humans –prior decision-making and nutrition—had large and robust effects on decision quality.[1] They also relate to a recent literature in psychology studying the effects of these factors on a variety of outcomes, including impulse control of various forms (e.g. Muraven and Baumeister 2000). Accordingly, we examine intertemporal choice behavior in a treatment that involves prior impulse control, a treatment with a nutrition supplement and a treatment with a placebo nutrition supplement.

In our baseline treatment, we find that both α and β are significantly below 1, indicating the existence of both present bias *and* income-as-consumption error. Furthermore, all three of our treatments move α and β closer to 1, mitigating these biases. These effects are limited to individuals in our study with lower cognitive ability, a group identified in the literature as being more subject to behavioral biases.[2] We argue that the most plausible interpretation of our treatments is that all of them induced some lower-ability subjects to pay more attention to the economic environment of their decision and to frame their decisions in the context of the extra-laboratory capital market options available to them, generating choice patterns consistent with lower utility curvature. While this might seem surprising for our prior impulse control treatment, we note that this treatment requires subjects to repeatedly resist the temptation to make the easy ('snap') decision; thus it could have alerted them to the idea that obvious choices aren't always the best ones.[3] The high level of price-sensitivity among our more cognitively able subjects in all treatments suggests they may have already been attuned to this possibility.

Our results have four main implications for future research. One is methodological: by estimating the co-movement of multiple parameters of a *decision utility* function in response to exogenous

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[1] Since a judge's reputation is harmed more by inappropriately granting, as opposed to inappropriately refusing parole, the measure of (average) decision quality is the number of times parole was granted. Despite the essentially random arrival of cases, Danziger *et al*. found that parole was much more likely to be granted early in the day as opposed to later in the day, and following the board's midmorning snack.

[2] See for example Frederick (2005), Shamosh and Gray (2008), Dohmen *et al*. (2010), Rustichini *et al*. (2012), and Benjamin *et al*. (2013).

[3] Some studies by psychologists (discussed below) find that prior impulse control reduces subjects' ability to refrain from precipitous or unconsidered actions like food and alcohol consumption, aggression and reliance on racial or gender stereotypes. These actions however differ considerably from the series of intertemporal financial choices under different prices we study here.

treatments, we show how some insight into the types of behavioral biases that are affected by those treatments can be gained. Substantively, we show that minor changes in the immediate decision environment of the sort that occur frequently in the course of a typical human day can affect economic decisions, sometimes in unexpected ways. This may have implications for the way in which important financial decisions are presented to people and the conditions under which consumers should be encouraged to make (and avoid making) those decisions. We also show that behavioral biases, which are most often studied one-at-a-time, can interact: in our context, present bias cannot affect decisions unless narrow bracketing is also present. Finally, and closely related, we note that –in part due to its practicality and low cost-- many researchers use intertemporal monetary choice to measure discounting and present bias in laboratory and field studies.[4] Somewhat paradoxically, our results suggest that these efforts can only hope to succeed by creating conditions that induce income-as-consumption error, i.e. that lead subjects to ignore the extra-laboratory consequences of their decisions. Determining whether measures of discounting and present-bias obtained under such conditions predict individual behavior outside the context in which they are measured is an important question for further research.

2 – The Experiment and Background

As noted, our experiment compares aspects of intertemporal monetary choice elicited under four conditions. The Baseline treatment involves no prior intervention, the Prior Impulse Control treatment involves first participating in the Stroop (1935) task, which requires subjects to repeatedly make decisions that conflict with automatic responses, and the Sugar (Placebo) treatments involve first consuming a sugared (non-sugared) beverage. Our use of prior decision-making and nutrition supplementation to shift behavior is motivated not only by the results of Danziger *et al.* (2011), but also by a substantial literature in psychology on the resource-based model of self control (Muraven and Baumeister 2000). This model posits that the ability to refrain from impulsive behavior is constrained by one's current stock of a resource that is (a) depleted by prior impulse controlling activities, and (b) replenished by nutrition.[5] Some authors (especially Gailliot *et al.* 2007, 2009) have even claimed that blood glucose –which can be quickly raised by consuming sugar-- *is* this scarce resource. The robustness of both these experimental effects (for prior impulse-controlling activity and nutrition) and the mechanisms by which they might

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[4] Notably, Augenblick *et al*. (2015) study the allocation of *effort* over time. Consistent with the notion that intertemporal arbitrage opportunities are lower for effort than income, they find substantially more evidence of present bias than we do here. Implementing their approach in a survey or in the field, however, poses substantial logistical hurdles.

[5] Examples of 'impulsive' behavior that have been studied in this way include food and alcohol consumption (Baumeister *et al.* 1998; Vohs and Heatherton 2000; Muraven *et al*. 2002; Kahan *et al*. 2003), aggressive reactions (DeWall *et al.* 2007; Stucke and Baumeister 2006) and suppressing stereotypes (Gordijn *et al.* 2004). Effects of sugar consumption have been demonstrated on behaviors including inflicting pain on others, the use of racial stereotypes and slurs, and support for social welfare (see Gailliot and Baumeister 2007) for a review). In economics, Dickinson *et al*. (2014) show that a nutritional intervention increases the likelihood of Bayesian choices over reinforcement heuristic-based choices..

work are now subject to considerable debate, however.[6] Still, both nutrition and prior self-controlling activity exhibit substantial short-term variability in the course of daily human activity, making their effects –if any-- on the fundamental preference parameters underlying most economic models of considerable interest nonetheless.

The two experiments most closely related to this paper both study the impact of temporary manipulations of the decision environment on intertemporal financial choices.[7] Ifcher and Zarghamee (2011) find that induced *positive affect* leads to more patient choices involving money. Because their 'early' payment was always on the day of the experiment, however, they cannot distinguish present bias from discounting; also their explicitly nonstructural approach does not generate a treatment effect on utility curvature and hence on income-as-consumption bias. Wang and Dvorak (2010) estimate the effects of a sugared drink on choices between current and future income. Again, their exclusive focus on present-versus-future decisions and their imposition of a one-parameter hyperbolic discount function makes it impossible to distinguish discounting from present bias or utility curvature. Like us, they find a patience-enhancing effect of sucrose consumption relative to no drink at all. In contrast to us, however, they find that a sugar-free beverage *reduced* patience relative to no drink at all. That said, it is hard to compare their results to ours because both the above effects are identified by within-subject comparisons in a situation where it is impossible to reverse the order of treatments.[8]

2.1 – Treatments

Our experiment consists of three types of sessions: Baseline, Prior Impulse Control and Drink. Within each session type, there are five distinct parts, the orders of which change across session types. In a Drink session, the phases are: (1) consumption of drink and entry questions, (2) rest to allow any sucrose in the drink to be metabolized into blood glucose, (3) elicitation of time preferences, (4) other decisions (Stroop task), and (5) an exit survey that includes Frederick's (2005) Cognitive Reflection Test (CRT). The structure of the Baseline sessions is similar to the Drink sessions, except that no beverage is given. In Prior Impulse Control sessions, we invert the order between the Stroop task and the elicitation of time preferences (and no drink is given). Finally, within the Drink sessions, we have two conditions

[6] On the role of glucose, see Molden *et al*. (2012) and Sanders *et al*. (2012), who show that rinsing one's mouth with a sugared beverage without swallowing has similar effects to ingesting sugar, and Kurzban (2010) who points out that the brain's overall consumption of glucose is too small for variation in mental activity to have meaningful effects on blood glucose. On the role of prior impulse control, see the meta-analyses by Carter and McCullough (2013, 2014), who argue that publication bias may account for much of the observed effect. In response to these concerns, the Association for Psychological Science (2014) recently issued an open call for pre-registered replications of a standardized, detailed protocol for measuring the depleting effect of prior impulse control.

[7] Benjamin *et al*. (2013) manipulate subjects' cognitive load by asking them to remember strings of seven numbers *while* making their intertemporal choice decisions. They detect no statistically significant effects, but caution that their manipulation appears not to have had much power.

[8] In other words, in contrast to ours, their design does not permit comparison of a drink consumer who has not previously performed the time preference elicitation with a non-drink consumer in the same situation.

Table 1: Experimental Design

Treatment	***Task***				
	(1)	(2)	(3)	(4)	(5)
Baseline	Entry survey	Rest	Time preference task	Stroop task	Exit survey
Prior Impulse Control	Entry survey	Rest	Stroop task	Time preference task	Exit survey
Placebo	Sugar-free drink and entry survey	Rest	Time preference task	Stroop task	Exit survey
Sugar	Sugared drink and entry survey	Rest	Time preference task	Stroop task	Exit survey

corresponding to a drink containing sugar or a sugar-substitute. These variations give us four treatments: Baseline, Prior Impulse Control, Placebo and Sugar. Table 1 lays out the progression of the experiment for each treatment.

The comparison between the Prior Impulse Control treatment and the Baseline allows us to determine whether performing an initial task (which requires subjects to make decisions that frequently conflict with automatic responses) affects the decision to defer income in the time preference task. The comparison between the Sugar treatment and the Placebo treatment allows us to study whether the consumption of sugar affects time preferences. Finally, if time preferences react to the consumption and metabolization of sucrose rather than the drink itself, we expect to observe no differences in choices when comparing the Placebo treatment and the Baseline. We discuss each task and drink consumption in more detail below.

2.2 – Time Preference Elicitation

To elicit time preferences, we implement the Convex Time Budget (CTB) method of Andreoni and Sprenger (2012, henceforth AS). We use this technique for three reasons: 1) the use of convex budgets provides the informational efficiency necessary to estimate parameters for each individual separately, 2) no distributional assumptions on the residuals are needed, and 3) it does not require the elicitation of atemporal risk preferences to de-bias estimates of the discounting parameters (See Andreoni *et al*. (2015) for a direct comparison between this technique and typical multiple-price-list approaches). Thus, we respect individual heterogeneity and avoid the potential confounds associated with effects of our treatments on risk preferences and the unobserved drivers of choice.

In every choice, participants received a budget of 16 tokens to allocate between an early payment, c_t, and a late payment, c_{t+k}, with t the early payment date and k the delay between the two dates. Participants made 45 allocation decisions and one of these decisions was randomly selected at the end of the session for actual payment according to the allocation of tokens between the two dates. The 45 budgets combine three early payment dates (t = 0, 5, 15 weeks), three delay lengths (k = 5, 10, 15 weeks) and various price ratios. Thus, there were only seven paydays evenly spaced at five weeks intervals (0, 5, 10, 15, 20, 25, 30 weeks). For each (t,k) combination, participants had to make five decisions involving various interest rates. The value of a token at the late date, a_{t+k}, was always equal to €1, while the value of the token at the early date, a_t, varied between a minimum of €0.67 and a maximum of €0.99. Allocating all the tokens to the late payment date paid €16; allocating all the tokens to the early payment date paid a minimum of €10.72 and a maximum of €15.84. The progressions were defined in order to offer implied annual interest rates, compounded quarterly, between 4% and 845%. Table A1 in the Appendix presents all the choice sets.

The presentation of the 45 decisions was very similar to that in AS. A choice screen had nine decision tabs that were displayed successively and corresponded to the nine (t,k) combinations. The order between the nine tabs was randomly and independently determined for each participant to control for order effects. Each decision tab displayed five budget decisions presented in order of increasing gross interest rate. To facilitate decision-making by a better visualization of delays, each decision tab displayed a dynamic calendar highlighting the current date, the early date and the late date in different colors. It also displayed the values of a token at the early date and at the late date, together with the values in Euros of the earnings corresponding to the decisions. A sample decision tab is reproduced in the Appendix. The boxes for entering the allocation decisions were initially blank. As soon as a value was entered either for the early date or the late date, the other box was filled automatically to ensure that the total budget was 16 tokens and the corresponding payoffs in Euro at the two dates were also displayed.

This design allows us to estimate for each individual her discount rate, the curvature of her utility function (through the variations of k and of the gross interest rate), and her present bias and hyperbolic discounting (through the variation of t). In addition, it allows us to examine which, if any, of these dimensions is impacted by prior decision-making and sucrose consumption.

2.3 – Prior Impulse Control

We used the Stroop task (Stroop 1935) to force individuals to make a large number of decisions, some of which conflict with their automatic responses and some of which do not (for a survey of the test, see MacLeod 1991). In a typical Stroop test, individuals have to read the color of ink used to write words independently of the color names of words. In some trials, there is congruence between the color of the word and the color of the ink (the word "yellow" is written in yellow) but in other trials there is no congruence (the word "yellow" is written in red and the correct answer is red). The incongruent stimuli typically require more time and produce more mistakes than the congruent stimuli because the brain automatically decodes the semantic meaning of the word and needs to override its first reaction to identify the color of the ink. Shortcutting the automatic process requires self-control, and performance on the test is frequently used as a measure of that concept by psychologists.[9]

In our experiment, the participants' computer screen displayed a series of color words (black, blue, yellow, green and red) successively, and the participants were instructed to indicate, as quickly and accurately as possible, the ink color in which the word was written. The list of possible colors was displayed at the bottom of the screen and the participants had to press the button corresponding to the color of the ink, whether or not that matched the color name of the word (see instructions in Appendix). They had to complete congruent and incongruent Stroop trials in random order for 6 minutes. Although the task was not incentivized (to avoid creating a wealth effect compared to the other treatments), on average participants completed 126 trials (S.D. = 11.69; min=82, max=151). As expected, the time spent on incongruent words was significantly higher than on the congruent words (two-tailed *t*-test, $p < 0.001$).

2.4 – Drink Consumption

Following Gailliot *et al.* (2007), participants in each Drink session were given 14 ounces (40 centiliters) of a soft drink sweetened either with sugar or with a sugar substitute. Both types of drinks had the same appearance. The sugared drink contained 158 kilocalories and the placebo drink contained 10.[10] We used a double blind procedure to administer the drinks: neither the participants nor the experimenters were aware of the sugar content of the beverage.

After being invited to drink the beverage, participants could rest in silence and read magazines that we distributed during 10 minutes in order to allow the sucrose to be metabolized into glucose (Donohoe and Benton 1999). Three minutes before the end of this period, participants had to assess the beverage and to report their usual consumption of soft drinks.[11] In the Baseline and the Prior Impulse Control treatments, the same rest period of 10 minutes was implemented.

[9] See for example the discussion in Gailliot *et al*. (2007).

[10] Specifically, the drinks were Fanta "Citron frappe" and Fanta Zero "Citron frappe". They were dispensed in glasses and appear identical (see Figure A1 in the Appendix). Neither contains caffeine, though both contain ascorbic acid (vitamin C).

[11] The questions were: 1) Please rate your enjoyment of the beverage you just consumed, between 1 and 10. 2) How many calories do you think the beverage contained? 3) How often do you drink soft drinks (Coke, Pepsi, lemonade, ...): every day / every week / once or twice a month or less / less than twice a month? Although participants in the Placebo treatment assessed the beverage less positively (mean = 4.55, S.D. = 2.77) than those in the Sugar treatment (mean = 5.57, S.D. = 2.58) (two-tailed Mann-Whitney test, $p = 0.097$), they did not realize that they received a placebo. Indeed, they predicted the same number of calories contained in the beverage (mean = 124.16, S.D. = 86.26) than the participants placed in the Sugar treatment (mean = 140.41, S.D. = 98.26) ($p = 0.497$).

2.5 – Procedures

The experiment was computerized, using the REGATE-NG software. It consisted of 8 sessions conducted at the laboratory of the GATE (Groupe d'Analyse et de Théorie Economique) institute in Lyon, France. Undergraduate students from the local engineering and business schools were invited via the ORSEE software (Greiner 2015). Between 17 and 20 participants took part in each session, for a total of 149 participants. Two sessions of the Baseline treatment were implemented with a total of 34 participants; two sessions of the Prior Impulse Control treatment were implemented involving 40 participants; and four Drink sessions were implemented with 75 participants (37 in the sugar condition and 38 in the placebo condition).

The invitation message addressed to the participants of all treatments indicated that they may possibly have to drink a beverage containing sugar during the session and that individuals suffering or thinking that they may suffer from a pathology linked to blood glucose regulation (like diabetes) should abstain from participating. After signing up, all the participants in all the treatments were instructed not to drink or eat at least three hours prior to the beginning of the session in order to stabilize blood glucose levels. Upon arrival we recorded the time of their last intake. Since chronobiology may influence economic decision-making, all the sessions were run at noon, when the level of blood glucose is low.[12]

Upon arrival, the participants had to sign a consent form reminding them that they should not participate if they suffer from a disease related to failure of blood sugar regulation. Then participants randomly drew a tag from a bag assigning them to a terminal. The instructions for each segment were distributed and read aloud by the experimenter after the completion of the prior segment (see Appendix).

The elicitation of time preferences requires very strict procedural rules. To participate in the experiment, the students were required to own a personal bank account and were informed by the invitation message that they would be paid by a wire transfer to their bank account; a bank statement was required.[13] During the session, instructions informed the participants that a show-up fee of €5 ($6.5) would be wired to their bank account in addition to their other payoffs at two different dates, regardless of their decisions: half of the show-up fee amount would be paid at the early date and the other half at the late date indicated by the decision randomly selected at the end of the session for payment. Indeed, paying the show-up fee at a single date could have influenced the allocation decisions. Participants were also informed that the dates mentioned on the decision screens were the dates at which the wire transfers

[12] We did not measure baseline blood glucose levels, which would have required taking blood samples.

[13] Indicating in the message that payment would be wired to the bank account and that a beverage could have to be drunk may have led to a self-selection of participants. However, the sessions were booked as quickly as usual. In addition, we asked 44 students participating in another experiment with standard cash payment whether they owned a personal bank account; all of them answered positively. Moreover, there is no reason to believe that the two criteria for participating were correlated. Finally, the message did not mention that the payment could be made at two different dates.

would be ordered by the finance department.[14] To maximize the confidence of the participants about the payment of their earnings, they received a document stating that the bank transfer would be ordered by the well-known National Center for Scientific Research (CNRS). In addition, the document mentioned the name, email address and phone number of the professor in charge of the experiment who could be contacted in case of any problem with the payment.

At the end of each session, participants received a feedback on the decision randomly selected for payment, indicating their payoffs and the dates of the two wire transfers for this decision. Then, they had to complete an exit survey that included questions about their demographics and average mark on the final high school exam (Baccalauréat). Sessions lasted 60 minutes and participants averaged earnings of €20.43 ($26.62), with a standard deviation of €0.97 ($1.26), including the show up fee.

2.6 – Predictions

A simple but general model of choice between early and late tokens supposes that subjects solve:

$$\text{Max}_{X,Y} \quad U(X) + \lambda U(Y), \quad \text{subject to } RX + Y \leq M, \; X \geq 0, \text{ and } Y \geq 0, \tag{1}$$

where X is experimental income received in the early period, Y is experimental income received in the later period, $U' > 0$, $U'' \leq 0$, R is the price of sooner income offered by the experimenter, and M is the endowment. In our experiment, $\lambda < 1$ can vary with both the early payment date (t) and the amount of delay between the periods (k) to incorporate both discounting and present bias, but is fixed within any (t,k) cell. R, on the other hand, varies within a (t,k) cell as we experimentally manipulate the implied interest rate. For this model of preferences, Figure 1 illustrates our first two theoretical predictions:

Prediction 1: Because $\lambda < 1$, utility-maximizing subjects should consume more than half their endowment in the early period ($X > 8$) when $R = 1$.

Prediction 2: Because income and substitution effects reinforce each other in our design, early consumption, X, should fall monotonically as R rises.

While Figure 1 illustrates the case with strictly positive utility curvature ($U'' < 0$), these two predictions apply to the linear utility case ($U'' = 0$) i.e. the case without income-as-consumption error as well. Specifically, if subjects choose total *consumption* according to (1) but have access to perfect capital markets, then regardless of their consumption utility function their demand for experimental *payments* should satisfy:

[14] The administration committed to respect exactly the dates of the transfers and sent us a feedback after each payment, showing that payments were actually made on time. We believe the transaction costs associated with this payment methodology are lower than the typical approach used in this type of experiment, which relies on personal checks or vouchers.

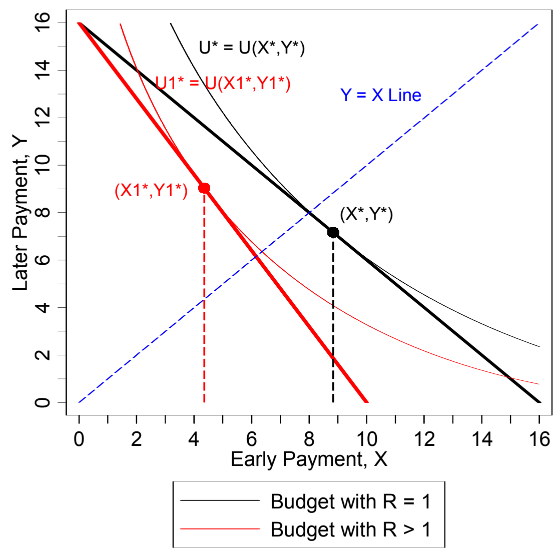

Figure 1: Predicted Behavior

$$\text{Max}_{X,Y} \quad rX + Y, \quad \text{subject to } RX + Y \leq M, \; X \geq 0, \text{ and } Y \geq 0, \qquad (2)$$

where r is the market price of sooner income. This is the special case of (1) where $U'' = 0$ and $\lambda = 1/r$. As is well known, the solution will consist of corner solutions that maximize the market value of experimental payments, specifically X=0 when $r<R$ and Y=0 when $r>R$. In what follows we refer to behavior according to (2) as *System 2* behavior, and behavior according to (1) with $U'' < 0$ as *System 1* behavior.

To estimate treatment effects on the decision-utility function, we employ a widely-used functional form for the utility function in equation (1). Specifically, individual i is assumed to have power income utility (with exponent α) that is additively separable across time periods in a β-δ form (Laibson 1997; O'Donoghue and Rabin 1999). Thus we use:

$$U(X) + \lambda U(Y) = X^{\alpha} + \beta^{T} \cdot \delta^{k} \cdot Y^{\alpha}, \qquad (3)$$

where k is the a delay between the two payment dates and T is an indicator for whether or not the sooner date is today (equal to 1 if t = 0, and 0 otherwise).[15]

Following AS, maximizing (3) subject to the experimental budget constraint leads to the sooner-income demand function:[16]

$$X_{ij} = [M \cdot (\beta^{Tj} \cdot \delta^{kj} \cdot R_j)^{(1/(\alpha-1))}] \,/\, [1 + R_j \cdot (\beta^{Tj} \cdot \delta^{kj} \cdot R_j)^{(1/(\alpha-1))}] \qquad (4).$$

[15] In most cases we present our results in terms of $r = \delta^{-365} - 1$, the yearly discount rate equivalent, for ease of interpretation.

[16] Note that equation (1) implies that the set of available allocations is convex: that the tokens can be infinitely divided. While we offer subjects 17 possible allocations along the budget frontier rather than an infinite number, we argue that this is a suitable approximation to convexity. Andreoni *et al*. (2015) perform a similar exercise with 6 allocations and find no evidence of bias due to discretization.

Our additional predictions depend on whether subjects exhibit System 1 or 2 decision-making, and on whether the treatments can move subjects between the two behavior modes. Suppose first that subjects behave according to System 1, in which case their experimental demands for sooner income to satisfy equation (4).

Prediction 3a: If subjects exhibit System 1 behavior, estimated values of α should be well below one. Treatments should act primarily on the present-bias parameter, β. According to the resource-based model of self-control, the Prior Impulse Control treatment should reduce β (away from one, thus increasing present bias) relative to the Baseline treatment by 'depleting' the subjects. Alternatively, the Prior Impulse Control treatment may increase β if the frequent feedback in the preliminary impulse-control task increases attention and teaches subjects to become more price-sensitive in the time-preference task. The Sugar treatment should raise β towards one (thus reducing present bias). The Placebo treatment should have no effect (unless a diet drink also builds the self-control resource), and none of the treatments should affect α or δ.

On the other hand, if utility is linear, subjects should maximize the present value of their experimental payments at their market interest rate (which is not affected by the treatments); this implies extreme price sensitivity and corner solutions:

Prediction 3b: If subjects exhibit System 2 behavior, none of the treatments should affect behavior. Subjects should be highly price-sensitive in all treatments, exhibiting values of α *and* β close to one in all cases, with frequent corner solutions.

Finally, what if a treatment shifts one or more individuals from System 1 to System 2 (for example, by prompting subjects to pay more attention to the extra-laboratory consequences of their decisions? Comparing Predictions 3a and 3b, we have:

Prediction 4: Treatments that shift individuals from making decisions according to System 1 to System 2 should raise the estimated values of α and β in equation (4) derived from experimental data.

While these predictions are designed to highlight the theoretical interest in the issue of cognitive resources and System 1 vs. System 2, it should be clear that our estimation method allows treatments to affect all three parameters of the demand function in (4) in a flexible way. Thus, for example, if a treatment increases subjects' demand for early rewards relative to late rewards regardless whether the early period is the day of the experiment, our structural estimates will interpret this as raising the subjective discount rate. If a treatment increases subjects' attraction only to rewards that are received on the date of the experiment, this will be interpreted as an increase in present bias (i.e. temporal inconsistencies and failures of willpower). Finally, if treatments make subjects less responsive to the price of early income, this is interpreted as a decrease in α, or as a switch towards System 1 decision-making.

In addition to their implications for treatment effects, the self-control and income-as-consumption models also have implications for the variation in estimated taste parameters between individuals. For example, if high-ability subjects perceive and understand market alternatives more clearly, they should exhibit values of α and β close to one, regardless of our experimental treatments (Prediction 3b). Of course, abler subjects would also exhibit less present bias (but not necessarily less curvature) if impulse control is positively correlated with intelligence. Finally, if adherence to an income-as-consumption heuristic is a main source of heterogeneity across individuals, we should expect to see a positive correlation between estimates of α and β across subjects more generally.

3 – Results

We present our results in four sections. The first section establishes a number of basic patterns in a pooled sample of all treatments, to provide context for the study of treatment effects. The second and third sections are nonparametric and structural approaches to analyzing the treatment effects, respectively. The final section presents some robustness checks. Since our subject pool is from a selective university, we present all of our experimental results separately according to our subjects' reported achievement on the French Baccalauréat exam to better assess their representativeness.[17] Specifically, we divide our participants in half relative to the median score in our sample, which was 16; this is also an important cutoff in the distribution of scores for student achievement. Because only 9% of French Baccalauréat recipients earned a score of 16 or higher, our 'high score' subjects clearly represent an elite level (about the top decile) of achievement among French high school graduates, while our 'lower-score' group roughly represents the 50th through 90th percentiles. Since the latter group is more representative of the French population, we focus much of our discussion on that group.[18]

3.1 – Overall Features of Behavior

This section presents two foundational results that verify aspects of our model and design, plus some simple descriptive statistics for the pooled sample across all treatments. The first result is that

[17] The French Baccalauréat exam is taken at the end of high school (*lycée*). In 2012, slightly over three quarters of French youth had passed the Baccalauréat.

[18] The results of the Cognitive Reflection Test (CRT) performed at the end of the sessions are highly correlated with the Baccalauréat score, and we can replicate all our main results using this measure of cognitive ability as well. However since subjects' CRT results could be affected by our treatments, we focus on the Baccalauréat-score based results.

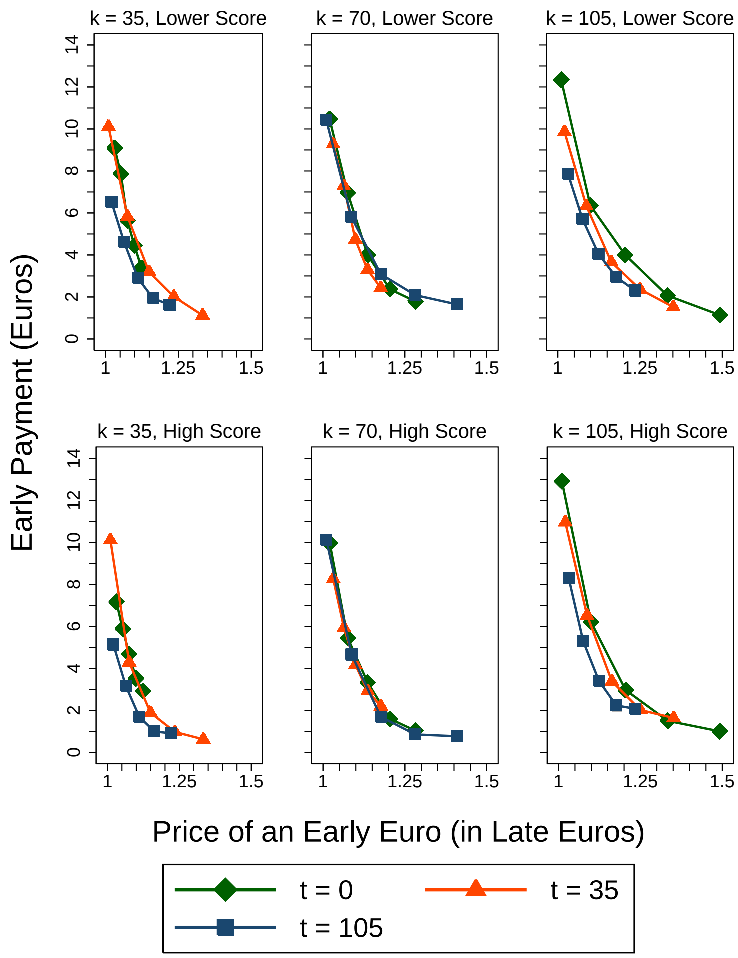

Figure 2: Demand Functions for Early Payment in Data

subjects' aggregate demand curves in the experiment satisfy two general predictions of utility-maximizing intertemporal behavior.

Result 1 – Consistent with Predictions 1 and 2, mean demand for early income exceeds half the 16-token endowment at interest rates near zero, then declines monotonically with the price of early income. This behavior characterizes both high- and lower-score participants.

Result 1 is illustrated in Figure 2, which plots the demand curves for the early payment (X), separately by score and pooled across all treatments. With the exception of the shortest delay length and latest start date for both groups, the demand curves all start at above eight units of X at levels of R closest

Table 3: Effect of Start Date, *t*, on Early Payment Demand

	Estimation Sample		
	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
Constant (t = 0, R = 1.01)	11.520	11.530	11.510
	(0.469)	(0.662)	(0.670)
1(t = 5 weeks)	-0.362	-0.508*	-0.218
	(0.220)	(0.297)	(0.327)
1(t = 15 weeks)	-1.182***	-1.184**	-1.180**
	(0.315)	(0.450)	(0.445)
Clusters	149	74	75
Observations	6705	3330	3375

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

Notes: Standard Errors in parentheses, clustered by individual. 45 observations (budgets) per cluster. All specifications feature price ratio dummies to flexibly control for price differences across choice sets.

to one, then fall monotonically as R rises.[19] The success of these basic predictions suggests that our participants' choices are informative for the preferences we wish to study. We note, however, that if subjects react to varying interest rates and delays, there is heterogeneity in our sample since 38% of the subjects make no interior choice decision (this percentage is 37% in AS (2012)). Among the subjects who make an interior choice decision at least once, 60% of the responses are corner decisions (50% in AS (2012)).

Result 2 – There is evidence of small but significant present bias in our data, among both high- and lower-test score participants.

Participants receive the first of their two payments either on the day of the experiment, 5 weeks after the experiment or 15 weeks after the experiment. To test for present bias, Table 3 presents regressions of early payment amounts on dummy variables for t = 5 and t = 15 as well as the price ratio while clustering standard errors at the individual level.[20] In contrast with AS (2012), we find evidence of present bias. If the date of first payment is immediate rather than 5 or 15 weeks in the future, lower-score subjects borrow significantly more of their endowment. High-score subjects do the same for only the 15-week delay.

[19] Because we do not observe choices from a zero-interest budget and Figure 2 indicates substantial non-linearity in the demand curves, we used our structural model to estimate choices at R = 1 to further test the prediction about income levels when R = 1. We found strong support, for all combinations of delay length and whether the early payment occurs immediately. The minimum predicted zero-interest demand is €9.32 (S.E. = 0.25).
[20] A regression approach is necessary because price ratios are not exactly balanced across the t dimension.

Finally, we note that there are only small and statistically insignificant differences between the early payment choices of high- and lower-Baccalauréat-score participants in our overall sample, which combines all treatments. Specifically, lower-score participants select a slightly higher overall level of early payment, and display slightly more present bias (which may be taken as a proxy of impulsivity), but neither gap is significant at conventional levels.[21] As the next section shows, however, this aggregate result obscures sizeable differences in the effects of treatment on the behavior of high- versus lower-score participants, which are informative about the mechanisms behind the treatment effects.

3.2 – Simple Estimates of Treatment Effects

Our first look at the effects of the various treatments is non-parametric. Figure 3 presents the mean demand for early payments across the Baseline, Prior Impulse Control, Placebo and Sugar treatments both pooled and separately by Baccalauréat score. Since these comparisons are between individuals, the treatments are balanced with respect to prices, delays and start dates.

Result 3 – In the full experimental sample, all three treatments reduce the demand for early payment, but the effect is (marginally) significant only for the Prior Impulse Control treatment. These aggregate results, however, mask large between-sample differences. In the lower test score sample, all three treatments significantly reduce the demand for early payment. In the high-score group, the Prior Impulse Control and Placebo treatments have no effect, while the sugared drink has a marginally significant patience-reducing effect.

Relative to the Baseline condition, overall demand for early income is lower in all three treatments. This reduction, however, is statistically significant only for the Prior Impulse Control treatment, and at a marginal level ($p = 0.097$). There is also no significant difference between the Placebo and Sugar treatments. In the lower-score sample, however, all three treatments reduce early income. The strongest effect is for the Sugar treatment (average demand for early payment reduced by 51% relative to Baseline), with reductions of 38% and 27% for the Prior Impulse Control and Placebo treatments respectively. The difference between the sugar and placebo effects is significant ($p = 0.056$), consistent with an effect of blood glucose (the magnitude of this difference is not affected by the beliefs about the number of calories in the drink). The magnitude of this additional sugar effect (32% relative to the placebo), however, is roughly the same size as the placebo beverage effect, suggesting a modest role for body-energy budgets

[21] Averaged across all choices, lower-score subjects allocate about €0.70 more experimental income (S.E. = 0.54, clustered by individual) to the earlier payment date than high-score participants. This difference is not significant. We add interaction terms between the dummy variables for $t = 5$ and $t = 15$ and high-score as well as a high-score level effect into the present bias regressions from Table 2. The gap between early demand when $t = 0$ versus $t = 5$ is about €0.31 smaller for high-score participants, but this difference is not significant (S.E. = 0.38). The signs and significances of the non-interacted dummies are unaffected.

relative to the other situational factors. Contrary to the predictions of willpower-based models, prior impulse control *reduces* lower-scoring subjects' demand for early payment.

Turning to the high-scoring subjects, only the Sugar treatment affects the demand for early income significantly (and *positively*) of the high-score subjects (average demand increased by 47%). This effect is not precise, however, and does not significantly differ from the placebo effect at conventional levels (p = 0.143). Tobit regressions that specify censoring points at the corner solutions obtain results that are qualitatively identical to all the results in Figure 3.

Another noteworthy aspect of Figure 3 is that high- and lower-score subjects differ substantially in their Baseline choices; the difference of €3.07 between the groups' early payment demand in the Baseline is highly significant (p = 0.011). Recalling that there was no significant *overall* difference between high- and lower-score participants, this suggests that, in essence, our three interventions have the effect of narrowing the difference in choices between high- and lower-score participants by reducing lower-scoring participants' demand for early payments. The next result probes the sources of these effects further.

Result 4 - The negative effect of all three treatments on lower-score participants' demand for early payments is strongest in cases where the price of early income is high.

Figures A2 and A3 in Appendix plot the demand curves for early payments for each (t,k) pair for lower-score and high-score participants, respectively. The lower-score subjects exhibit a similar level of demand across all treatments at low price levels. As the price of early income rises, early payments decline more rapidly in the Prior Impulse Control, Sugar, and Placebo treatments than in the Baseline. The high-score participants show a similar level of demand to the lower-score participants at low prices, but demand is highly price-sensitive in all four treatments. The treatments make the lower-score participants more price-sensitive, and thus more similar to the high-score participants' behavior.

To determine the statistical significance of the above effects, we define three price levels based on the relative value of early tokens. When early tokens are worth €0.90 or more we say the price is low, when they are worth between €0.80 and €0.90, we say the price is medium and when they are worth €0.80 or less, we say the price is high. Table 4 presents OLS regressions of early payment demand on the treatment dummy variables split by price level. At medium and high prices all three treatments have significant effects for the lower-score group and the magnitude of the sugar effect is larger at high as opposed to low prices. The sugar effect is significantly greater than the placebo effect in the medium price condition (p = 0.003) and borderline significantly greater in the high price condition (p = 0.105). At both medium and high prices, the effect of the placebo beverage (relative to the baseline) is larger in

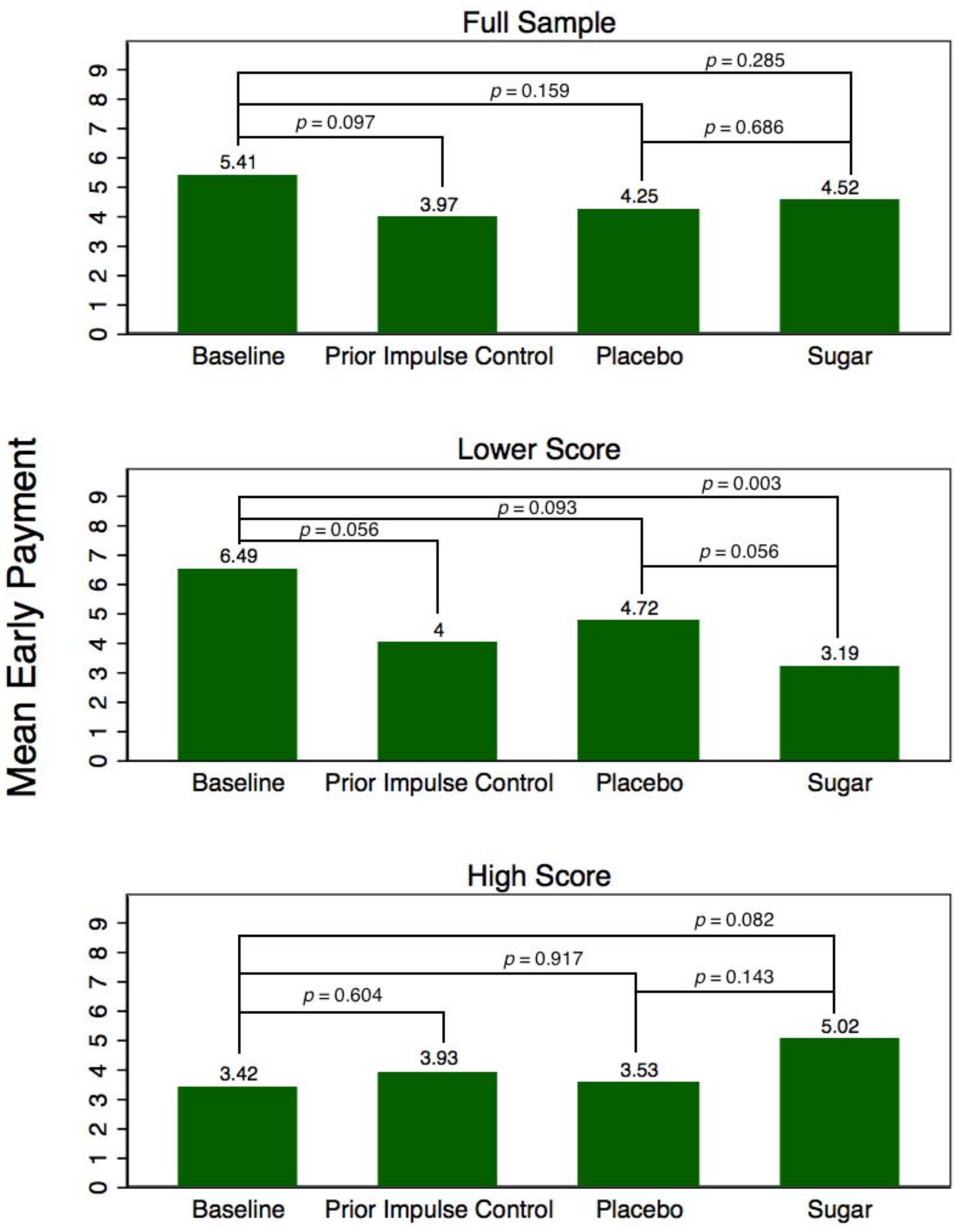

Figure 3: Demand for Early Payment by Treatment and Test Score

Notes: p-values are generated from regressions of the chosen early payment on treatment status with standard errors clustered at the individual level. The regression is run separately for lower- and high-score subjects. Each individual makes 45 decisions, leaving us with a sample size of 3330 (74 clusters) in the lower-score group and 3365 (75 clusters) in the high-score group.

Table 4: Treatment Effects on Early Payment Demand by Price Level

	Estimation Sample		
	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
Constant (Low Price, Baseline)	7.976	8.809	6.449
	(0.803)	(1.036)	(1.146)
Low Price X Prior Impulse Control	-1.778	-2.585	-0.276
	(1.093)	(1.590)	(1.460)
Low Price X Placebo	-0.865	-0.944	-0.492
	(1.074)	(1.316)	(1.703)
Low Price X Sugar	-0.530	-2.569	1.444
	(1.073)	(1.567)	(1.433)
Medium Price	-4.423***	-3.848***	-5.477***
	(0.534)	(0.645)	(0.876)
Medium Price X Prior Impulse Control	-1.287	-2.559**	1.171
	(0.862)	(1.171)	(0.844)
Medium Price X Placebo	-1.416*	-2.578**	0.789
	(0.793)	(1.040)	(0.754)
Medium Price X Sugar	-1.194	-4.321***	2.024***
	(0.803)	(0.943)	(0.657)
High Price	-5.764***	-5.550***	-6.157***
	(0.739)	(0.831)	(1.038)
High Price X Prior Impulse Control	-0.806	-2.019**	1.264*
	(0.738)	(1.010)	(0.653)
High Price X Placebo	-1.393**	-2.271**	0.268
	(0.686)	(0.967)	(0.454)
High Price X Sugar	-1.208*	-3.020***	0.995**
	(0.686)	(0.883)	(0.441)
Clusters	149	74	75
Observations	6705	3330	3375

*Notes: Standard Errors in parentheses, clustered by individual. 45 observations (budgets) per cluster. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

magnitude than the difference between the sugared- and sugar-free drink.[22] Column (3) suggests an elasticity-reducing effect of the Sugar treatment on the high-score subjects, but the effects are not statistically different from the Placebo effects in either medium or high price condition (p = 0.132 and p = 0.218 respectively). Unlike the larger estimated treatment effects on price sensitivity in column 2, these column 3 estimates are not supported by structural estimates of preference parameters that respect

[22] In the medium-price case, the sugar-free drink reduces early consumption by 2.578 units, while the additional effect of adding sugar to the drink is a reduction of (4.321 – 2.578 = 1.743 units). In the high price case, these two effects are 2.271 and 0.749 respectively. The p-values associated with these comparisons are 0.546 and 0.219 respectively.

individual heterogeneity (Table 5), which show no significant treatment effects on any parameters for the high-skill sample.

In sum, our nonparametric analysis shows that all three treatments (Prior Impulse Control, Placebo and Sugar) reduce early payment demand among subjects with lower Baccalauréat test scores, who are more representative of the educated French population than our high-score sample. This is most pronounced when the price of early income is high, so one way to phrase the results is to say the treatments make lower-score subjects more price-sensitive. Notably, high-score individuals are already very price-sensitive in the Baseline treatment, so the treatments have the effect of narrowing the behavioral gap between the two groups.

3.3 – Treatment Effects in a Structural Model of Time Preferences

To confront Predictions 3 and 4 more directly, we now measure whether the treatments affected different aspects of participants' preferences.[23] Specifically, we estimate Section 2.6's structural model of intertemporal preferences, where the treatments can affect each one of the three fundamental utility parameters (α, β, and δ). To identify these effects, we apply non-linear least squares (NLS) to the demand function for sooner tokens in equation (4). To analyze and test treatment effects, we replace α with

$$\alpha_1 + \alpha_2 \cdot D_i + \alpha_3 \cdot P_i + \alpha_4 \cdot S_i \qquad (5)$$

where D, P and S are treatment indicator variables, and make similar substitutions for β and δ (or r).

We first estimated equation (4) across individuals and without treatment effects, following our modification of the CTB technique introduced by AS in the calibration of prices. Our estimate of the aggregate yearly discount rate is 21.8% for lower-score types (S.E. = 5.9%) and 21.0% for high-score types (S.E. = 4.1%).[24] Our estimate of the β parameter is 0.976 (S.E. = 0.008) for lower-score and 0.988 (S.E. = 0.007) for high-score, with both values significantly less than 1 (p = 0.005 and p = 0.086, respectively). Thus, we find evidence of present bias in the β-δ form.[25] We also estimate significant utility curvature: α = 0.922 (S.E. = 0.008) for lower-score and 0.942 (S.E. = 0.005) for high-score individuals. To examine treatment effects using this structural model, we estimate a full set of utility parameters for each subjects and then compare the sets of estimates across treatments. We also estimate

[23] Because the demand functions implied by most theoretically interesting utility functions are nonlinear, the predicted marginal effects of each parameter depend on the levels of all the others, making simple regression tests like those in Table 4 only roughly informative about the effects of treatments on preference parameters. For example, while reduced utility curvature (higher α) is associated with higher price-sensitivity, it should also increase the response to k (the gap between the payment dates). Our structural analysis incorporates all these properties.

[24] The corresponding specification from AS (Table 2, column (3)) estimates a rate of 37.7% with a standard error of 8.7%. Because our maximum time horizon is slightly longer, we would expect a slightly lower estimate of the rate if individuals display some insensitivity to the exactness of dates far in the future.

[25] While this magnitude of present bias over pure allocations of money is not economically meaningful in our experiment, a 3% distortion of preferences could be very important for major financial decisions.

treatment-level aggregate utility parameters, but we focus on the sets of individual estimates because they respect across-individual heterogeneity.

Result 5: The aggregate treatment effects on the structural parameters are concentrated on α, the utility function curvature parameter. The estimates indicate that all three treatments reduce curvature among lower-ability subjects, thus moving them in the direction of System 2 behavior. Full results can be found in Appendix Table A2.

To implement our method that allows each subject to have his/her own set of utility parameters (α, β and δ), we make some adaptations that are dictated by the estimation results. First, we drop 16 individuals who exhibit no choice variation whatsoever. Second, we note that the solution to the optimization problem in (2) is well defined only if α is strictly between 0 and 1. Since the 17-option budget only approximates a fully convex budget, we say that we have evidence in favor of $\alpha < 1$ if an individual makes at least one choice from the interior of a budget (receiving some income now and some later). For these individuals, we apply the NLS technique discussed earlier.[26] Third, for the individuals with no interior choice, we enforce $\alpha = 1$ and estimate the other parameters using the switch point technique: as the interest rate climbs, where an individual switches from receiving all income sooner to receiving all income later puts an interval on their discount rate. We use the average midpoint of the switch intervals from the front-end-delay choice sets to identify δ and the difference between this and the average midpoint of the switch intervals from the no front-end-delay choice sets to identify β. Lastly, we trim extreme outlying estimates: $\alpha < 0$, $\beta > 2.5$, $r < -0.5$ and $r > 10$. This leaves us 130 estimates of α, 118 estimates of β and 114 estimates of r.

Result 6: The means of the individual treatment effects on the structural parameters are concentrated on α and β. The estimates indicate that all three treatments reduce both curvature and present bias among lower-ability subjects, thus moving them in the direction of System 2 behavior.

Table 5 reports estimates of treatment effects on the individual-specific parameters. Specifically, for each of the three parameters, we estimate a regression in which the participants' parameter estimates are the dependent variables and the three treatment indicators are the only regressors. The results indicate that both drinks significantly decrease curvature *and* present bias in the lower-score sample. Moreover, there is no robust evidence of a sugar effect that significantly exceeds the placebo effect. The Prior

[26] Two individuals in this group have very little choice variance, which makes the OLS technique employed by Andreoni *et al.* (2015) preferable for identification.

Table 5: Treatment Effects on Individual Utility Parameter Estimates

	Estimation Sample		
	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
***α* (Utility Curvature):**			
Baseline Level	0.929	0.893	0.984
	(0.021)	(0.022)	(0.037)
Prior Impulse Control	0.023	0.059*	-0.033
	(0.028)	(0.033)	(0.047)
Placebo	0.027	0.067**	-0.036
	(0.028)	(0.030)	(0.051)
Sugar	0.017	0.084**	-0.049
	(0.028)	(0.038)	(0.044)
***β* (Present bias):**			
Baseline Level	0.912	0.876	0.975
	(0.026)	(0.023)	(0.049)
Prior Impulse Control	0.102***	0.105**	0.069
	(0.037)	(0.042)	(0.062)
Placebo	0.078**	0.120***	0.008
	(0.036)	(0.038)	(0.067)
Sugar	0.064*	0.105**	0.000
	(0.036)	(0.049)	(0.057)
***r* (Ann. Disc. Rate):**			
Baseline Level	0.582	0.692	0.418
	(0.105)	(0.158)	(0.137)
Prior Impulse Control	-0.110	-0.291	0.115
	(0.147)	(0.238)	(0.179)
Placebo	-0.098	-0.229	0.105
	(0.142)	(0.212)	(0.189)
Sugar	-0.121	-0.458*	0.116
	(0.140)	(0.269)	(0.162)

** p < 0.10, ** p < 0.05, *** p < 0.01.*
Notes: Standard Errors in parentheses.

Impulse Control treatment reduces curvature and present bias in the lower-score group. The discount rate is not significantly affected by our treatments, although the estimates are often imprecise. None of the treatments has statistically significant effects on any of the utility parameters in our high cognitive ability sample.

Overall, our individual estimates of treatment effects are consistent with some features of willpower-based models (the sugar effect) and inconsistent with others (the prior impulse control effect; the fact that treatments affect estimated utility curvature). Instead, because of the specific utility parameters that are affected, our estimates are more consistent with the notion that the treatments shift subjects between behaving as if experimental income were consumption (System 1) towards taking account of non-experimental options when making experimental decisions (System 2). It seems that prior exercise of impulse control (in our Stroop task) has not depleted other cognitive resources (like attention or working memory) that are needed to be aware of arbitrage opportunities.[27] On the contrary, by (repeatedly) drawing subjects' attention to the fact that the obvious answer is not always the correct one, the Stroop task may have primed our subjects to be more reflective and trained them to become more price-sensitive.

Result 7: Present bias and utility curvature are highly correlated across persons in the individual structural estimates.

Conditional on $\beta \leq 1$ (individuals exhibit present bias), individual estimates of α and β are positively correlated (p = 0.02). Among these individuals, a one standard deviation decrease in α (increase in curvature) is associated with a 0.25 standard deviation decrease in β (increase in present bias). Conditional on $\beta \geq 1$ (subjects are future-biased), individual estimates of α and β are negatively correlated ($p < 0.01$). Among these individuals, a one standard deviation in α (increase in curvature) is associated with a 0.52 increase in β (increase in future bias). Recalling that present bias is a behavior theorized to affect consumption the fact that our treatments jointly affect α and β in this way suggests that our interventions reduce the likelihood that our lower-score individuals make income-as-consumption errors.

3.4 – Robustness

If our treatments reduce the income-as-consumption errors, we should observe that our treated participants are more likely to choose monetary allocations at corners. While our structural estimation procedure uses both interior and corner choices to identify the utility parameters, and while the procedure is compatible with any finite level of intertemporal substitutability, a possible concern is that the method breaks down in the limiting case of infinite substitutability across time periods, where all choices are predicted to be at corners.[28] Thus, our first robustness test is to study treatment effects on the frequency and type of corner solutions in two less parametric ways. The first of these, in Table 7, shows the frequency of interior solutions and the two types of corner solution, by treatment.

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[27] Muraven and Baumeister (2000) distinguish self control from these other mental resources, and present evidence that depleting one does not necessarily deplete the other. For example, Muraven et al. (1998) show that participation in a thought suppression exercise (which requires impulse control) reduced subsequent self-control, relative to participants who solved math problems that required the same amount of subjective effort. Conversely (and more directly relevant), performance on a difficult task that did not require self-control was unaffected by an initial task that required self-control (Muraven 1998).

[28] This is a consequence of assuming that individuals evaluate the utility of lab earnings as prospects independent of background consumption. One approach to this issue would be to incorporate background payments into the structural estimation. Since background consumption is independent of treatments this would have little effect on the results.

Table 7: Corner Choices by Treatment

	Sample		
	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
Baseline	24% Sooner Corner 47% Later Corner 29% Interior	29% Sooner Corner 34% Later Corner 37% Interior	16% Sooner Corner 71% Later Corner 13% Interior
Prior Impulse Control	18% Sooner Corner 62% Later Corner 20% Interior	15% Sooner Corner 56% Later Corner 29% Interior	21% Sooner Corner 68% Later Corner 11% Interior
Placebo	19% Sooner Corner 58% Later Corner 23% Interior	21% Sooner Corner 52% Later Corner 27% Interior	15% Sooner Corner 67% Later Corner 18% Interior
Sugar	18% Sooner Corner 53% Later Corner 29% Interior	13% Sooner Corner 65% Later Corner 22% Interior	20% Sooner Corner 49% Later Corner 31% Interior

While the overall share of corner solutions in Table 7 is high at 75% (consistent with AS and with Andreoni *et al.* 2015), column (2) also clearly shows that all treatments reduce interior choice frequency among our lower Baccalauréat score participants. The especially pronounced increase in later corner choices for this group is related to the economically, but not statistically significant changes to the discount rate induced by the treatments. Our second approach is to estimate treatment effects in a multinomial logit specification with three choice options: 1) sooner corner, 2) interior choice, and 3) later corner. Results are found in Appendix Table A3. Reassuringly, in the lower-score sample, the probability of choosing the sooner corner is significantly lower in the Prior Impulse Control and Sugar treatments than the Baseline, and the probability of choosing the later corner is significantly greater in the Prior Impulse Control, Sugar and Placebo treatments.

Another robustness test consists of controlling for the impact of the initial physiological condition of our participants on decision-making. Indeed, if time preferences are dependent on physiological conditions, it would be encouraging if our treatment effects were moderated by the condition in which individual subjects entered the lab. While subjects were asked not to eat or drink for at least three hours prior to the experiment, our survey indicated that there was substantial variation in the degree of adherence to this request. Almost 19% of individuals report they had not eaten since the day before the experiment and around 7% had eaten within the three hour window prior to the experiment. We expect that subjects should have been more susceptible to the interventions the longer they went without eating.

Table 8: Treatment Effects on Demand for Early Payment with Meal Time Controls

	Estimation Sample		
	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
Constant (Baseline, just ate)	2.838	2.360	3.735
	(1.248)	(1.287)	(1.509)
Prior Impulse Control	1.357	1.281	1.401
	(1.645)	(2.069)	(2.079)
Placebo Effect	0.080	1.419	-2.326
	(1.524)	(1.630)	(2.159)
Sugar	2.591*	1.102	2.090
	(1.526)	(1.700)	(1.844)
Time Since Last Meal (hours)	0.434**	0.730***	-0.049
	(0.218)	(0.155)	(0.223)
Time X Prior Impulse Control	-0.474*	-0.677***	-0.217
	(0.261)	(0.241)	(0.301)
Time X Placebo	-0.183	-0.544**	0.421
	(0.246)	(0.214)	(0.289)
Time X Sugar	-0.608**	-0.773***	-0.122
	(0.250)	(0.232)	(0.270)
Clusters	149	74	75
Observations	6705	3330	3375

**p < 0.10, ** p < 0.05, *** p < 0.01.*
Notes: Standard Errors in parentheses, clustered by individual. 45 observations (budgets) per cluster.

Table 8 presents treatment effect regressions on demand for early payment with interactions between the Prior Impulse Control, Placebo and Sugar variables with the number of hours since last meal.

Consistent with our baseline results, we find no significant meal-time correlations for the high-scoring subjects; this group's decisions are also unaffected by the amount of elapsed time since their last meal. Lower-scoring subjects, on the other hand, become less patient as the time since their last meal increases; this behavior is consistent with Briers *et al.*'s (2006) and Danziger *et al.*'s (2011) evidence.[29] Also, as predicted, lower-scoring subjects' sensitivity to all three of our interventions increases with elapsed time since their last meal.[30] While this may not be surprising for the drink treatments, it is perhaps noteworthy that the Stroop test also has a larger patience-enhancing effect on hungry than on

[29] Briers *et al*. (2006) found that the desire for caloric resources increases the desire for money.
[30] Note that the uninteracted treatment effects no longer enter as significant because they are estimates specific to the intercept where the time since last meal is zero.

Table 9: Treatment Effects on Demand for Early Payment by CRT Score

	Estimation Sample			
	CRT = 0	***CRT = 1***	***CRT = 2***	***CRT = 3***
	(1)	(2)	(3)	(4)
Constant	6.916	5.061	3.039	4.654
(Baseline)	(0.997)	(0.965)	(1.165)	(2.410)
Prior Impulse	-3.580**	-1.457	0.796	1.296
Control	(1.405)	(1.635)	(1.345)	(3.015)
Placebo	-2.468*	0.251	0.096	-0.726
	(1.240)	(1.319)	(1.653)	(2.559)
Sugar	-2.409*	-0.217	1.143	-0.299
	(1.351)	(1.260)	(1.728)	(2.599)
Clusters	42	40	40	27
Observations	1890	1800	1800	1215

** p < 0.10, ** p < 0.05, *** p < 0.01.*
Notes: Standard Errors in parentheses, clustered by individual. 45 observations (budgets) per cluster.

recently nourished subjects. This finding reinforces our suggestion that engaging in a cognitively demanding task that requires resisting one's immediate impulse can (at least temporarily) improve a vulnerable subject's ability to focus on subsequent economic decisions.

To rule out mood or affect as potential drivers of our sugar or placebo effects, we use the mood and beverage enjoyment data that we elicited during the resting period before subjects got the instructions for the allocation decisions. The values are ranged from 1 (negative) to 10 (positive). First and foremost, mood is not predictive of demand in our experiment. Second, we use a specification identical to our hours-since-last-meal analysis, but replace that variable with the self-reported mood variable, and exclude individuals from the Prior Impulse Control treatment (since their mood elicitation took place prior to the Stroop task). Results are in Appendix Tables A4 and A5. We again find no substantive evidence that mood is related to demand for lower-score participants.[31]

To add credence to our use of the Baccalauréat exam score as a measure of cognitive ability, we present treatment effect estimates split by CRT performance instead of by Baccalauréat score (both measures are significantly correlated). As noted, these estimates should be interpreted with caution since the treatments may have affected the subjects' CRT performance, just as they affected the subjects' performance in the time-preference task. That said, consistent with our results using the Baccalauréat, we find significant effects of the treatments on time preferences only for those who failed to answer a single

[31] The same is true of elicited beverage enjoyment. Attempts to replicate the Ifcher and Zarghamee (2011) result by using our treatment variables as instruments for mood fail due to a lack of relevance: our treatments do not appear to affect mood.

CRT question correctly (slightly more than 70% of these individuals are in the lower-score group). Results are presented in Table 9.[32]

4 – Discussion and Conclusion

This paper studies the effects of prior decision-making activity and sugar consumption on intertemporal financial choices. A key innovation of our approach is an explicit model of choice, which allows us to distinguish three aspects of 'patience' that might be affected by the cognitive and physiological environment: discount rates, present bias, and price sensitivity. We find that intertemporal choices are sensitive to transient features of the choice environment, but not necessarily in ways that are consistent with the resource-based model that has been popular in the psychology literature. For example, exposure to the Stroop (1935) task prior to the elicitation of time preferences makes lower-test-score participants *less* present biased and *more* responsive to high prices for early income. And while drinking a sugared beverage ten minutes prior to the time preference task has the same effect (consistent with a willpower model), so does drinking a sugar-free beverage, raising doubts about the importance of body energy budgets relative to other situational factors.

Instead, the pattern of choices and treatment effects in our experiment suggest a possible role for models based on attention (for example, Koszegi and Szeidl 2013) or on cognitive factors like the ability to frame choices in a way that takes account of opportunities that are available outside the current choice environment. Models of this type are suggested by the fact that our experimental treatments affect preference curvature (α) as well as present bias (β), that our estimates of α and β are strongly correlated across persons, and the fact that our treatment effects vanish among subjects with very high cognitive abilities. They find additional support in the fact that these effects are observed in subjects who have been exposed to a decision task that requires attention and resistance to the temptation of giving thoughtless but natural responses, and who have received an immediate and frequent feedback on their errors. In this interpretation, differential adherence to an income-as-consumption heuristic might play an important role in explaining differential intertemporal choice behavior in our experiment, both across persons and across treatments. Indeed, it is natural for consumers to frame monetary choices in the context of easily available information –for example to assess the timing and riskiness of the portfolio in a single retirement account in isolation—and cognitively costly to do otherwise, i.e. to assess each account in the context of all other accounts, pensions, social security and real estate assets. Thus, aspects of the decision environment for intertemporal monetary choices that encourage a broader frame, perhaps by shifting

[32] Table 8's presentation of results for each possible CRT score raises the question of how our main results would change using a finer breakdown of Baccalauréat scores than whether subjects are above or below the median. To that end, appendix Figure A4 shows demand for early payments as cubic functions of Baccalauréat score. The results are mostly similar, although they do show strong sugar effect at the very bottom of the cognitive ability distribution.

persons from System 1 to System 2 reasoning, might deserve further study, both inside and outside the laboratory.

Regardless of the exact theoretical model that best explains them, we note that our estimated treatment effects are large in magnitude. Consider an individual offered a 2-week cash advance with a 15% charge. Money is borrowed against a €1000 income receipt. The average lower-score subject in the Baseline treatment would borrow €494. This falls to €106 in the Prior Impulse Control treatment, €47 in the Placebo treatment at €8 in the Sugar treatment. To illustrate the large role that present bias plays, consider that if the advance were offered with a 1-day delay, the average lower-score individual in the Baseline treatment would reduce demand from €494 to €241. We also remind the reader that the cognitive abilities of our 'lower-score' sample –where are treatment effects are concentrated—are still well above the national mean (representing about the 50th-90th percentiles of high school graduates), suggesting the possibility of even larger effects for the population as a whole. Payday loans such as the above are considered by many to be 'predatory' in that their short-term nature takes advantage of scope insensitivity in interest rates to charge above-market rates. In these situations, our finding that the treatment effects operate through the intertemporal elasticity of substitution indicates that unless consumers are highly attuned to their task at hand, they may ignore substantial price differences across assets or credit payments.[33]

Finally, the link we identify between shifts in elasticity and shifts in present bias sheds light on an important issue in the literature on the elicitation of time preference: Under what conditions can monetary choice experiments reveal present bias? We offer suggestive empirical evidence that they can only do so when individuals treat income as consumption, a point long emphasized by authors working on the theory of present bias. Moreover, our results indicate that treating income as consumption may not be a behavior limited to those who are credit-constrained, but that situational factors, like those researched heavily in psychology, may also matter.

[33] For those concerned about the external validity of our experimental measures, we point to existing literature that demonstrates a strong relationship between experimentally elicited impatience and wealth and health investment (Hastings and Mitchell, 2011), present bias and credit card debt (Meier and Sprenger, 2010) and time discounting and credit scores (Meier and Sprenger, 2012).

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Appendix for Online Publication

Figure A1 – Glasses containing either the Placebo or the Sugared Beverage

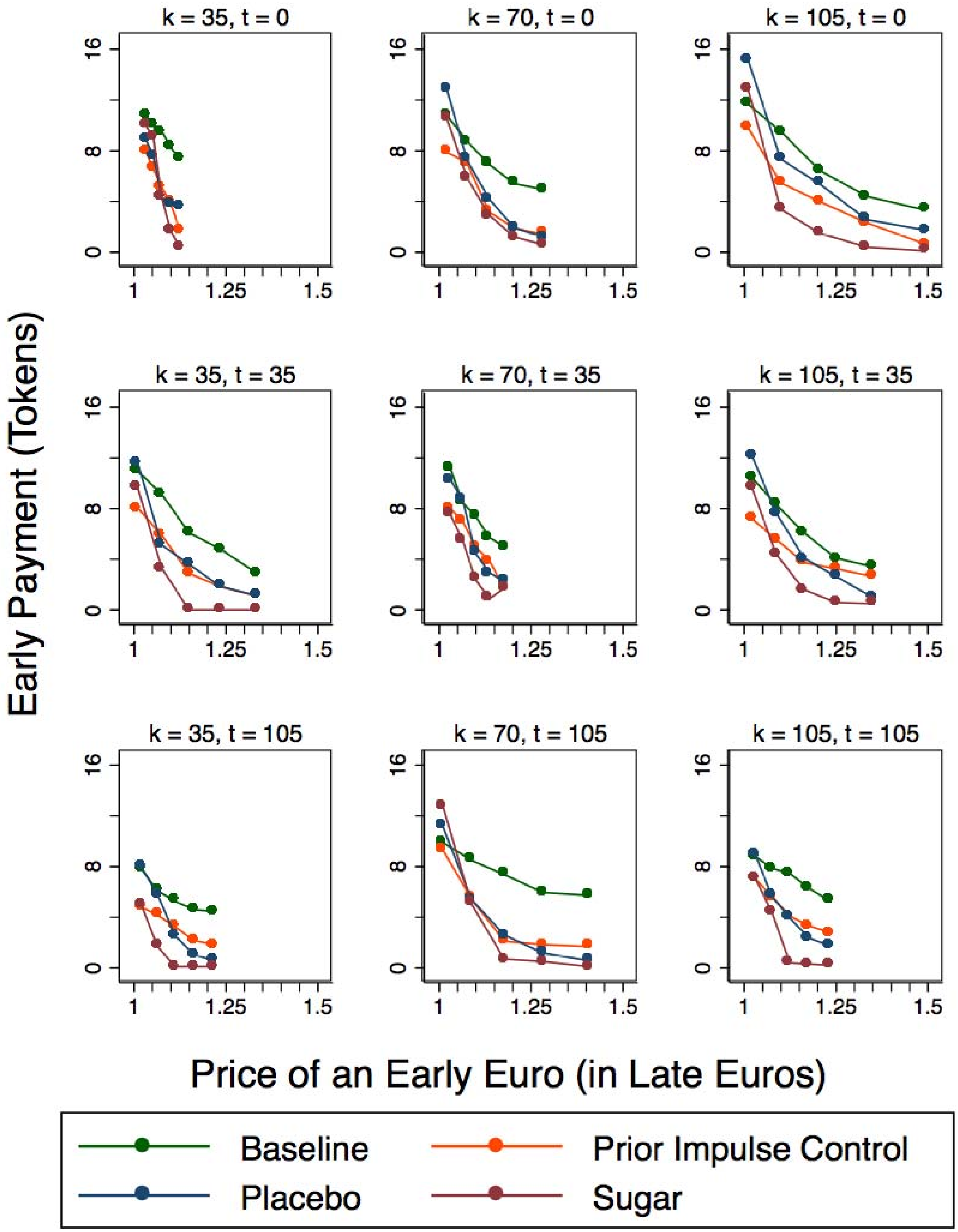


Figure A2: Demand Functions by Treatment, Lower-Score Sample

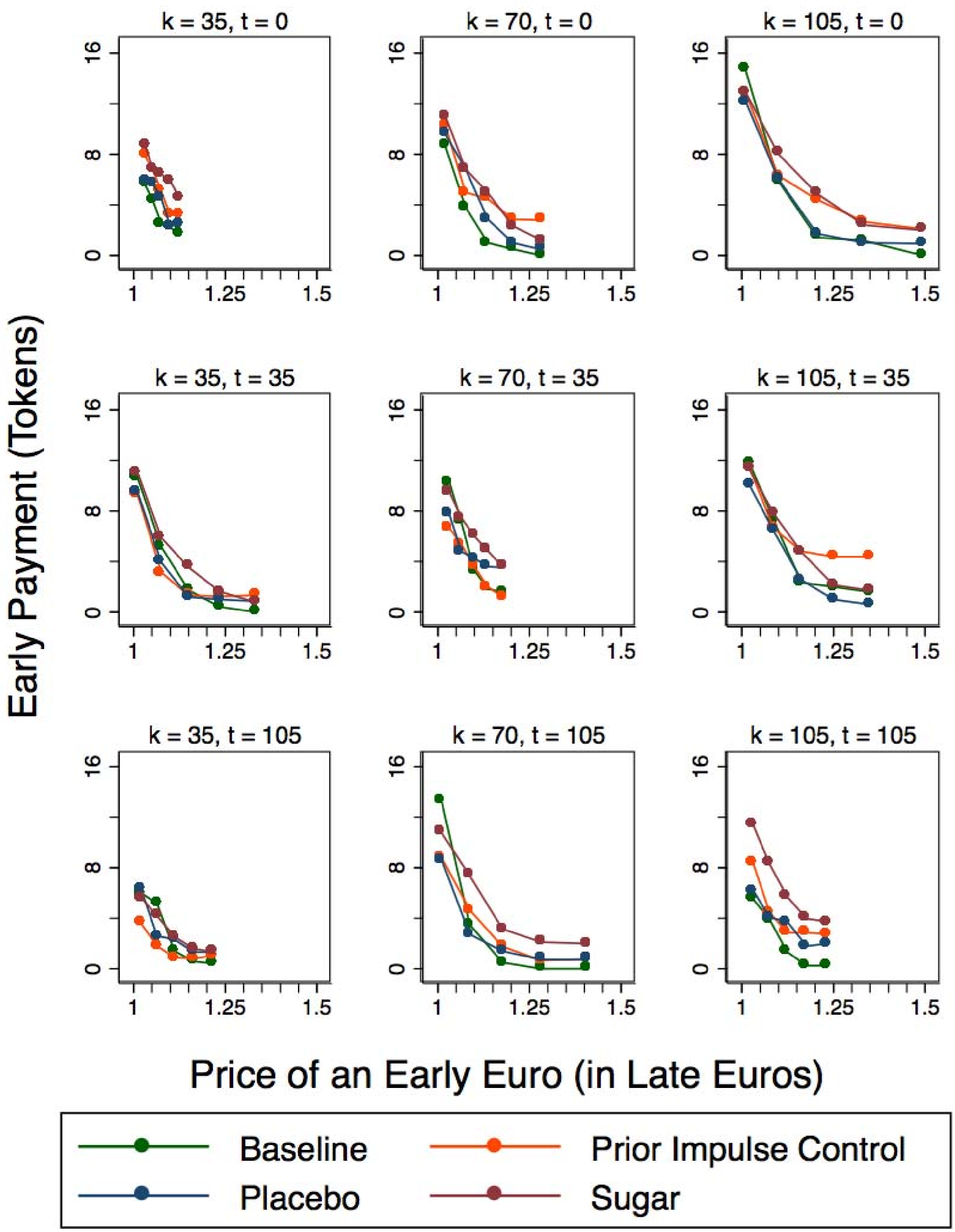


Figure A3: Demand Functions by Treatment, High-Score Sample

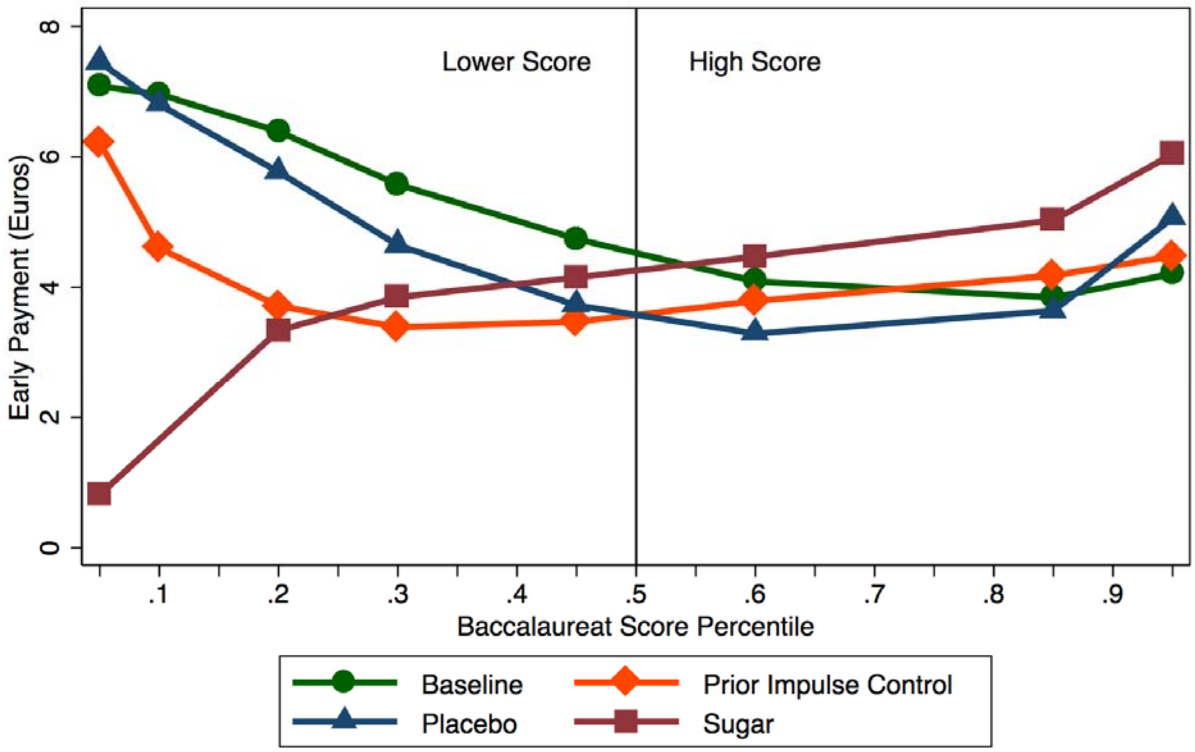


Figure A4: Predicted Demand by Baccalauréat Score

Notes: Predictions are from a regression of demand on a cubic in the subject's Baccalauréat Score. The data are trimmed in order to avoid estimating the polynomials on outliers.

Table A1: The 45 Choice Sets in the Time Preference Elicitation Task

Choice number	Early date t	Delay length k	Early value of 1 token a_t	Price of an Early Euro	Annual interest rate %	Maximum early payoff
1	0	5	0.97	1.03	36	15.52
2	0	5	0.95	1.05	65	15.2
3	0	5	0.93	1.08	100	14.88
4	0	5	0.91	1.10	141	14.56
5	0	5	0.89	1.12	189	14.24
6	5	10	0.97	1.03	17	15.52
7	5	10	0.94	1.06	36	15.04
8	5	10	0.91	1.10	59	14.56
9	5	10	0.88	1.14	85	14.08
10	5	10	0.85	1.18	116	13.6
11	15	15	0.97	1.03	11	15.52
12	15	15	0.93	1.08	28	14.88
13	15	15	0.89	1.12	47	14.24
14	15	15	0.85	1.18	70	13.6
15	15	15	0.81	1.23	96	12.96
16	0	10	0.98	1.02	11	15.68
17	0	10	0.93	1.08	44	14.88
18	0	10	0.88	1.14	85	14.08
19	0	10	0.83	1.20	139	13.28
20	0	10	0.78	1.28	208	12.48
21	5	15	0.98	1.02	7	15.68
22	5	15	0.92	1.09	32	14.72
23	5	15	0.86	1.16	64	13.76
24	5	15	0.8	1.25	103	12.8
25	5	15	0.74	1.35	154	11.84
26	15	5	0.98	1.02	23	15.68
27	15	5	0.94	1.06	82	15.04
28	15	5	0.9	1.11	164	14.4
29	15	5	0.86	1.16	278	13.76
30	15	5	0.82	1.22	432	13.12
21	0	15	0.99	1.01	4	15.84
32	0	15	0.91	1.10	37	14.56
33	0	15	0.83	1.20	82	13.28
34	0	15	0.75	1.33	144	12
35	0	15	0.67	1.49	231	10.72
36	5	5	0.99	1.01	11	15.84
37	5	5	0.93	1.08	100	14.88
38	5	5	0.87	1.15	246	13.92
39	5	5	0.81	1.23	479	12.96
40	5	5	0.75	1.33	845	12
41	15	10	0.99	1.01	5	15.84
42	15	10	0.92	1.09	51	14.72
43	15	10	0.85	1.18	116	13.6
44	15	10	0.78	1.28	208	12.48
45	15	10	0.71	1.41	339	11.36

Note: The value of a token at the late date, a_{t+k}, was always equal to €1. The price of an early euro (in late euros) is equal to $1/a_t$. The yearly interest rate assumes quarterly compounding.

Table A2: Treatment Effects on Aggregate Utility Parameter Estimates

	Estimation Sample		
	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
***α* (Utility Curvature):**			
Baseline Level	0.904	0.860	0.961
	(0.015)	(0.027)	(0.007)
Prior Impulse Control	0.028	0.058*	-0.016
Effect	(0.018)	(0.031)	(0.013)
Placebo Effect	0.042**	0.087***	-0.014
	(0.016)	(0.028)	(0.013)
Sugar Effect	0.036**	0.105***	-0.030***
	(0.016)	(0.028)	(0.011)
***β* (Present bias):**			
Baseline Level	0.979	0.949	1.002
	(0.016)	(0.026)	(0.013)
Prior Impulse Control	0.006	0.045	-0.023
Effect	(0.018)	(0.027)	(0.018)
Placebo Effect	0.004	0.031	-0.014
	(0.018)	(0.029)	(0.018)
Sugar Effect	0.004	0.025	-0.016
	(0.019)	(0.029)	(0.018)
***r* (Ann. Disc. Rate):**			
Baseline Level	0.268	0.357	0.210
	(0.106)	(0.225)	(0.068)
Prior Impulse Control	-0.140	-0.267	-0.057
Effect	(0.124)	(0.256)	(0.097)
Placebo Effect	-0.046	-0.076	-0.076
	(0.121)	(0.236)	(0.114)
Sugar Effect	-0.016	-0.219	0.109
	(0.122)	(0.237)	(0.107)
Clusters	149	74	75
Observations	6705	3330	3375

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

Notes: Standard Errors in parentheses, clustered by individual. 45 observations (budgets) per cluster.

Table A3: Treatment Effects on Probability of Corner Solution Choice
Marginal Effects from Multinomial Logit Model

	Estimation Sample					
	All Subjects		***Lower-Score***		***High-Score***	
Corner Choice:	Sooner	Later	Sooner	Later	Sooner	Later
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.242	0.472	0.287	0.344	0.161	0.706
(Baseline)	(0.046)	(0.060)	(0.066)	(0.072)	(0.038)	(0.069)
Prior Impulse	-0.063	0.149*	-0.142*	0.217**	0.049	-0.030
Control Effect	(0.057)	(0.078)	(0.084)	(0.108)	(0.058)	(0.089)
Placebo Effect	-0.055	0.111	-0.077	0.179*	-0.009	-0.033
	(0.054)	(0.080)	(0.075)	(0.096)	(0.059)	(0.111)
Sugar Effect	-0.061	0.060	-0.156**	0.307***	0.040	-0.217**
	(0.054)	(0.078)	(0.071)	(0.105)	(0.053)	(0.091)
Clusters	149		74		75	
Observations	6705		3330		3375	

** p < 0.10, ** p < 0.05, *** p < 0.01*
Notes: Standard Errors in parentheses, clustered by individual. 45 observations (budgets) per cluster. The multinomial logit specification estimates the effect of our treatments on the probability of choosing either the sooner or later corner solution, with respect to an interior choice (all pooled). This table presents the marginal effects of changing the treatment indicators from 0 to 1, holding the other indicators constant at 0.

Table A4: Treatment Effects on Demand for Early Payment with Mood Controls

	Estimation Sample		
	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
Constant (Baseline, neutral mood)	5.168	7.898	2.638
	(2.506)	(3.551)	(2.112)
Placebo Effect	-1.144	-1.922	-0.412
	(0.834)	(1.163)	(0.998)
Sugar Effect	-0.878	-3.407***	1.567*
	(0.847)	(1.120)	(0.900)
Mood (-5 to 5 scale)	0.046	-0.260	0.160
	(0.438)	(0.598)	(0.424)
Mood X Placebo	-0.383	0.381	-1.008*
	(0.539)	(0.754)	(0.586)
Mood X Sugar	-0.141	0.648	-0.500
	(0.527)	(0.679)	(0.558)
Clusters	109	55	54
Observations	4905	2475	2430

** p < 0.10, ** p < 0.05, *** p < 0.01*

Notes: Standard Errors in parentheses, clustered by individual. 45 observations (budgets) per cluster. Mood is elicited on a 1-10 scale. We renormalize to -5 to 5 such that treatment effect estimates refer to neutral mood.

Table A5: Treatment Effects on Demand for Early Payment with Drink Enjoyment Controls

	Estimation Sample		
	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
Constant (Baseline, neutral enjoyment)	5.408	6.491	3.422
	(0.674)	(0.896)	(0.681)
Placebo Effect	-1.258	-1.852*	0.004
	(0.809)	(1.037)	(1.021)
Sugar Effect	-1.054	-3.344***	1.385
	(0.803)	(1.107)	(0.862)
Placebo X Enjoyment (-5 to 5 scale)	-0.231	-0.162	-0.318
	(0.160)	(0.172)	(0.277)
Sugar X Enjoyment (-5 to 5 scale)	0.295*	0.072	0.373*
	(0.172)	(0.191)	(0.196)
Clusters	109	55	54
Observations	4905	2475	2430

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

Notes: Standard Errors in parentheses, clustered by individual. 45 observations (budgets) per cluster. Enjoyment is elicited on a 1-10 scale. We renormalize to -5 to 5 such that treatment effect estimates refer to neutral enjoyment.

Instructions for the Drink session
(These instructions have been translated from French to English)

You are about to participate in an experimental session on decision-making.
The session consists of several parts. You will receive the instructions for each part after the previous part has been completed.

Part 1

Your computer screen will display a number of questions. We thank you for answering these questions with care.

Once all participants will have answered these questions, we will distribute glasses of a beverage that we will invite you to drink. Please do not drink the beverage before being expressly invited to do it.

Next, you will have to answer a few questions.

After you have answered these questions, you will have to wait for the next part. During this rest period, you are allowed to read books, newspapers or magazines. During this part and throughout the session, it is not allowed to talk to the other participants.

Part 2 *(distributed after completion of part 1)*

Your decisions
In this part, you will be asked to make a series of choices between payments you can receive at different dates. On each of nine decision screens, you will decide how to divide your payment for the experiment between two dates: an 'early' date and a 'late' date.
Altogether, you will make a total of 45 choices on the nine decision screens. These decision screens will be displayed in a random order. You will have the following options for payment dates:

Decide between payment today and payment in 5 weeks
Decide between payment in 5 weeks and payment in 15 weeks
Decide between payment in 15 weeks and payment in 30 weeks
Decide between payment today and payment in 10 weeks
Decide between payment in 5 weeks and payment in 20 weeks
Decide between payment in 15 weeks and payment in 20 weeks
Decide between payment today and payment in 15 weeks
Decide between payment in 5 weeks and payment in 10 weeks
Decide between payment in 15 weeks and payment 25 weeks

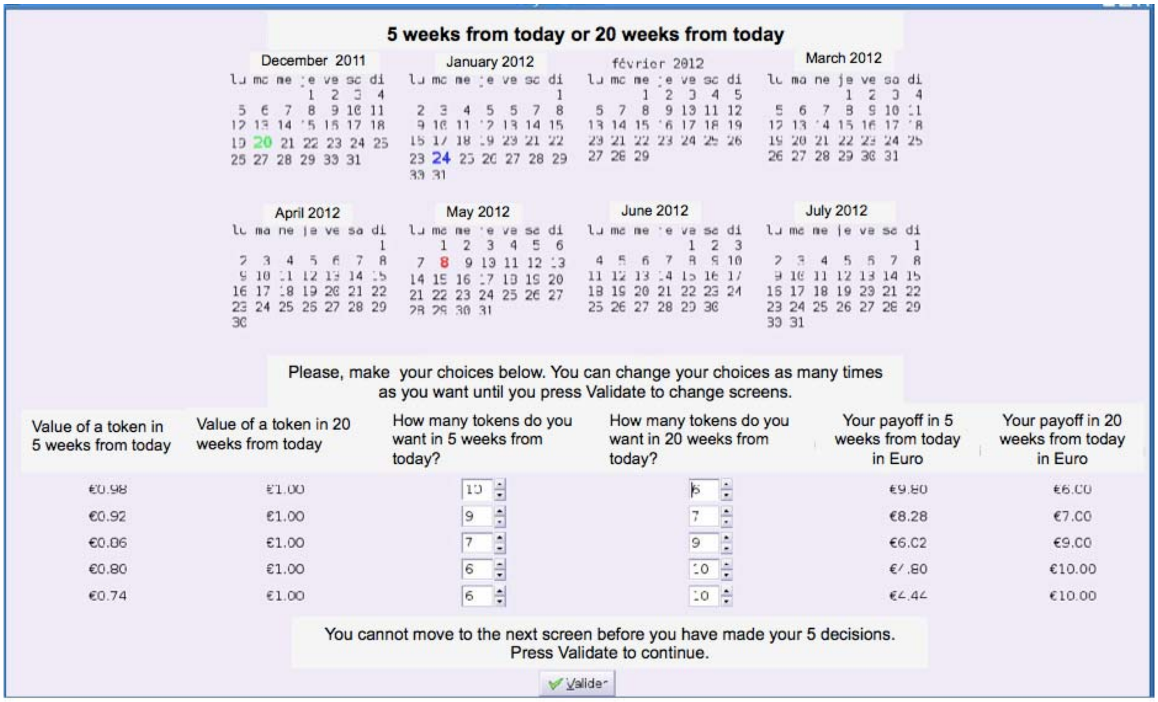
On each decision screen, we will provide you with the exact calendar dates of the above payments, so you know exactly which decision you are making. Today's date appears in green, the early payment date appears in blue and the late payment date appears in red. If the early date is today's date, only two colors appear on your screen (green and red).

You will be given 16 tokens to divide in each choice, but *the value of a token changes from choice to choice*. The real money payments associated with your token choices will be automatically calculated for you to see as you make your decisions.

To make your decisions, you can enter a number for the early payment (or the late payment) and move the up and down arrows. The box corresponding to the late payment (or the early payment, respectively) will be automatically updated by a number indicating the difference between 16 and the tokens assigned to the other date of payment. You can indifferently start by entering a decision for the early date or for the late date.

Once you have completed a set of five decisions, you must press the “Validate” button to move to the next decision screen.

Below is an example of a decision screen.



Your payment
At the end of the session, the computer program will randomly select one of the 45 decisions you made to be your earnings from participating in this experiment.

In addition, you will receive a €5 participation payment that will be split up into two payments of €2.50: one to go along with your earnings at the early and late dates associated with the randomly selected decision. You will thus receive two payments regardless of your decisions.

You will not be paid in cash today. You will be paid by wired transfers on your bank account on which you gave us a bank statement. The two payments will be done by the CNRS (National Center for Scientific Research) at the exact dates corresponding to the randomly selected decision.

For example, if the selected decision indicates that you have chosen x tokens today and y tokens in 10 weeks, the CNRS will wire the first payment on your account today and the second payment in 10 weeks from today.

Remember that each decision could be the one that counts! Treat each decision as if it could be the one that determines your payment.

If you have any question on these instructions, please raise your hand and we will answer your questions in private.

Part 3 *(distributed after completion of part 2)*

In this part, you will be presented with a series of color words (black, blue, yellow, green, red). These words will appear in different colors, sometimes matching the word (e.g., the word blue, written in blue), and sometimes not matching the word (e.g., the word blue, written in red).
Your job is to indicate, as quickly and accurately as possible, the color in which the word is written, whether or not that matches the word itself. Click the button that matches the color of the word. Try not to pay attention to the word, but just the color.
This task will last for six minutes.

Example:

In this example, the correct answer is « red ».

Data Collection Disclosure

We report all measures collected in our study here that were not mentioned in the body text:

Gender

Age

Student – student status indicator

Employed –employed indicator

Unemployed – unemployed indicator

Retired – retired status indicator

Business – indicator for student getting a business degree

Economics – indicator for student getting an economics degree

Engineering – indicator for student getting an engineering degree

Math – indicator for student getting a math degree

Medicine – indicator for student getting a medicine degree

Other school – indicator for any other type of degree program

Ecole Centrale – indicator for being a student at the Ecole Centrale de Lyon

Ecole de Management – indicator for being a student at the Ecole de Management de Lyon

Economics faculty – indicator for being a student of economics at Universite de Lyon

Other faculty – indicator for being a student in any other discipline at Universite de Lyon

Year of schooling – 1 to 5, years into education following Baccalauréat (high school)

Right-handed – indicator for being right-handed

Wealth – 1 to 10 score of self-reported wealth

Risk preference – 0 to 10 self-reported willingness to take risks