Title

Decision-environment effects on intertemporal financial choices: How relevant are resource-depletion models?

Authors

Kuhn, Michael A
Kuhn, Peter
Villeval, Marie Claire

Publication Date

2017-05-01

DOI

10.1016/j.jebo.2017.02.014

Peer reviewed

Decision-Environment Effects on Intertemporal Financial Choices:

How Relevant are Resource-Depletion Models?

Michael A. Kuhn[a]
Peter Kuhn[b]
Marie Claire Villeval[c]

January 22, 2017

Abstract: A large literature in psychology studies the effects of the immediate decision environment on behavior, and conceptualizes both cognitive capacity and self-control as scarce resources that can be depleted by recent use, and replenished by factors like rest and nutrition. We assess the relevance of resource-depletion models for intertemporal financial decisions by estimating the effects of three interventions –prior impulse-controlling activity, consumption of a sugared drink, and consumption of a placebo (sugar-free) drink-- on intertemporal monetary choices in a cash-advance framework. These manipulations have large impacts on the demand for advances, but contrary to resource-based models prior impulse-controlling activity and placebo drink consumption *increase* patience. To understand these effects, we estimate treatment effects on the three parameters of a decision utility model for every subject in our sample. All treatments reduce utility curvature and present-bias, and these movements are highly correlated. Together, we argue that these patterns suggest that the treatments are acting not on subjects' fundamental utility parameters but on subjects' tendencies to frame financial decisions narrowly (within the frame of the lab experiment) versus broadly (in the context of their other financial options). Thus, while decision environments have large effects on intertemporal financial decisions, both the direction and the mechanisms underlying these effects appear to be quite different from those suggested by resource-depletion models.

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

Contact Information:
[a]Michael Kuhn, corresponding author. University of Oregon, Department of Economics, 1285 University of Oregon, Eugene, OR 97403 USA. E-mail: mkuhn@uoregon.edu. Web page: pages.uoregon.edu/mkuhn
[b]Peter Kuhn, University of California at Santa Barbara, Department of Economics, Santa Barbara, CA 93106, USA. E-mail: pjkuhn@econ.ucsb.edu. Web page: http://www.econ.ucsb.edu/~pjkuhn/pkhome.html.
[c]Marie Claire Villeval, Université de Lyon, F-69007, France; CNRS, GATE Lyon St Etienne, 93, Chemin des Mouilles, F-69130, Ecully, France. E-mail: villeval@gate.cnrs.fr. Web page: http://www.gate.cnrs.fr/perso/villeval/

*This research has been supported by a grant from the French National Research Agency (ANR, EMCO program, HEIDI grant) and was performed within the framework of the LABEX CORTEX (ANR-11-LABX-0042) of Université de Lyon, within the program "Investissements d'Avenir" (ANR-11-IDEX-007) operated by the French National Research Agency (ANR). We thank James Andreoni, Charles Sprenger, Ernst Fehr, Andrew Oswald, and Andrew Schotter for valuable feedback, and participants at the BLUE workshop at the University of Edinburgh, at the ASFEE and HEIDI-CORTEX conferences in Lyon, at the CAGE conference on Individual Characteristics and Economic Decisions at the University of Warwick, and seminar presentations at Monash University in Melbourne, New-York University at Abu Dhabi, Queensland University of Technology in Brisbane, University of Copenhagen, University of Innsbruck, University of Rennes, University of Zurich, Paris School of Economics, ENS Cachan, IZA, and Chapman University for useful comments.

1 – Introduction

While popular media is replete with advice to avoid making financial decisions in certain situations – for example, when one is tired, emotional, rushed, distracted, or hungry[1]— little carefully-controlled research has studied the effects of the *immediate decision environment* on intertemporal financial choices. This lack of attention is surprising in view of the fact that psychologists have been studying decision-environment effects on task performance for over a century. The psychology literature suggests at least four different reasons why decision environments might affect choice. One such reason is suggested by an influential literature documenting decision-environment effects on an array of 'impulsive' behaviors, such as food and alcohol consumption, aggression, stereotyping and inflicting pain on others.[2] For example, engaging in prior impulse-controlling activity increases the frequency of impulsive decisions, while consumption of a sugared drink reduces it. To interpret these effects, proponents conceptualize self-control --the ability to refrain from impulsive choices— as a scarce resource that can be depleted by recent use, and that can be replenished by factors like rest and nutrition. Thus, it is possible that environmental factors that reduce *willpower* will lead to financial decisions that emphasize immediate gratification over long-term wellbeing.

Second, even simple financial decisions –such as the optimal time to receive a payment or pay a bill-- can be cognitively demanding, requiring the decision maker to keep track of multiple consequences and accounts simultaneously. Thus, environmental factors like distraction and fatigue can also affect decision quality by changing the amount of *cognitive resources* that are available. Danziger et al.'s (2011) widely-cited demonstration of decision fatigue among Israeli parole judges illustrates this phenomenon: the frequency of 'easy', non-reflective decisions (denying parole) increased monotonically with the number of previous decisions that were made, until the judges took a break for rest and nutrition.[3] Thus, environmental factors that cause cognitive depletion can lead to decisions that rely on simple rules of thumb instead of taking all aspects of the decision into account. Notably, both willpower- and cognitive-depletion effects are *resource-based* models (Muraven et al. 1998, Muraven and Baumeister 2000) in the sense that they hypothesize a resource that can be depleted by use, and replenished by rest and nutrition. As such, both perspectives posit a negative relationship between recent use of a resource and the performance of tasks requiring it.

[1] See for example Burton (2016), FinancialHighway.com (2010), Striepe 2016) and Williams (2016).

[2] See Baumeister et al. (1998), Vohs and Heatherton (2000) Muraven et al. (2002) and Kahan et al. (2003) on food and alcohol; (DeWall et al. (2007) and Stucke and Baumeister (2006) on aggression; Gordijn et al. (2004) on stereotypes; and Gailliot and Baumeister 2007 for a review. Recent meta-analyses by Carter and McCullough (2013, 2014), have questioned the robustness of some of these findings, citing publication bias as an important factor, though this interpretation has been questioned by Hagger and Chatzisarantis (2014).

[3] A similar pattern applies to physicians' tendencies to prescribe unnecessary antibiotics (Linder et al., 2014). Neuroscientists also measure reductions in various types of cognitive performance, such as visual learning, after subjects' cognitive resources like working memory have been depleted by previous tasks (Anguera et al, 2014).

Third, psychologists who study *learning* and economists who study training have shown that in contrast to causing fatigue, recent performance of a task can sometimes have *positive* effects on performance. Not only is this true for unfamiliar tasks, which exhibit a well-documented learning curve (Ebbinghaus 1885), recent practice can also improve the performance of familiar tasks. Dramatic evidence is provided by Hockenberry and Helmchen (2014) and Ramdas et al. (2014), who show that surgeons' performance of specific operations improves dramatically with very recent performance of the same operation. Even breaks of a single day can cause substantial increases in patient deaths. These findings suggest that recent performance of a task, or practice with a closely-related task could improve task performance by building or refreshing a skill. Finally, psychologists have shown that task performance is improved when *attention* is allocated to it (Posner 1980, 2016). Thus, any environmental factors that make decision-makers more alert and attentive could also affect how they make intertemporal financial decisions. For example, marketing researchers have found that simple cognitive 'warm-up' exercises that orient a consumer's attention to the task at hand can improve the consistency of product choices (Kim et al. 2015).[4]

Motivated by these findings, this paper measures the effects of three interventions that have been widely used to study decision-environment effects in the psychology literature – performance of the Stroop (1935) task, which requires subjects to repeatedly make decisions that conflict with automatic responses, consumption of a sugared drink, and consumption of a sugar-free drink—on the intertemporal monetary choices of 270 subjects. These are, respectively, our prior-impulse-control (PIC), Sugar, and Placebo treatments. Intertemporal choices are studied using an adaptation of a Convex Time Budget (CTB) task from Andreoni and Sprenger (2012) that brackets the terms and interest rates of most short-term consumer debt. In addition to overall effects, we pay close attention to behavioral differences between subjects with different cognitive abilities. We do this, in part, because a substantial economics literature documents large differences in intertemporal choice behavior between high- and low-cognitive-ability subjects (e.g. Frederick 2005, Shamosh and Gray 2008, Dohmen et al. 2010, Benjamin et al. 2013, and Rustichini et al. 2016). Second, there are theoretical reasons to expect decision-environment effects to differ by cognitive ability, and evidence that these effects differ in consumer choice contexts.[5] Finally,

[4] Kim et al. studied the stability of consumer choices between pairs of products to the range of products in the choice set. In their study, subjects with below-average cognitive reflection test (CRT) scores exhibited product-range effects: their choices were sensitive to irrelevant options. The experimenters then exposed subjects to a cognitive warm-up exercise—a simple questionnaire that asked them to describe how they use the product and indicate the features that were most important to them. The cognitive warm-up reduced product-range effects and stabilized decisions among the low-CRT subjects.

[5] Theoretically, a higher initial stock of cognitive ability should attenuate decision-environment effects in models based of cognitive depletion. Empirically, the cognitive warm-up exercises in Kim et al.'s (2015) consumer choice study had no effect on higher-CRT subjects, who exhibited no product range effects to begin with.

since high-ability persons are dramatically over-represented in our sample, breaking out a lower-ability sample provides results that are more likely to apply to a larger share of the population.

We find that (relative to a no-treatment baseline) our three treatments have large impacts on the demand for cash advances in our lower cognitive score sample, which represents about the 50th-90th percentiles of French high school graduates. Contrary to willpower-based resource models, we find that prior impulse control *reduces* the demand for money sooner in this population by almost 30 percent. And while our result that sugar consumption reduces sooner-money demand is consistent with resource-based models, our finding that sugar-free drink consumption has an almost identical effect is hard to reconcile with any model where caloric intake restores resources.[6] None of the above effects are present among our high cognitive ability subjects, who represent the top 10 percent of high-school graduates and who make more patient decisions in the Baseline treatment compared to the lower ability sample.

To shed further light on these effects, we exploit the richness of our CTB data to estimate three parameters of a decision utility function for each subject, and we measure the effect of our 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. Theoretically, we argue that distinguishing these three effects help to distinguish between decision-environment effects that affect subjects' willpower, versus effects that act on cognitive factors like awareness of prices and broader bracketing of decisions. Specifically, if the treatments act on subjects' ability to forgo current or early consumption, they will act on the β and δ parameters. If they act on cognitive factors like subjects' awareness of prices, or on their tendency to bracket their decisions narrowly by focusing on experimental income without reference to their other income, assets and expenditures, they should act on our estimate of α as well. We refer to the latter behavior, which treats experimental income as a form of consumption with diminishing marginal utility, as 'income-as-consumption bias' (Chabris et al. 2008) and note that it can be considered as a form of availability bias (responding to readily available knowledge, as in Tversky and Kahneman 1973), or as a form of narrow bracketing (focusing on the consequences of choices in isolation from each other, as in Read et al. 1999; Rabin and Weizsacker 2009).

[6] Gailliot et al. (2007, 2009) have claimed that blood glucose *is* the scarce resource that 'fuels' impulse control. These claims have, however, been questioned by Kurzban (2010), Molden et al. (2012) and Sanders et al. (2012).

This approach distinguishes our paper from two other experiments closely related to our paper that 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 cannot isolate 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. 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.[8,9]

Consistent with our reduced-form results, we find no differences between the Sugar and Placebo treatments, and that all treatment effects on the decision-utility parameters are confined to our lower-score sample. Within that sample, all three treatments move α and β towards one, and these parameters are highly correlated across individuals as well as treatments. Because both small-stakes utility curvature and present bias are consumption, not income, phenomena, we consider this strong evidence that our treatments are reducing income-as-consumption bias among lower-score subjects. Overall, our results suggest the following about decision-environment effects on intertemporal choices. First, since the treatments act on utility curvature, i.e. on subjects' sensitivity to prices, they do not appear to be acting primarily on willpower—i.e. on subjects' desires for immediate or early gratification. Second, while 'refreshing' subjects with a drink seems to increase their responsiveness to prices, the irrelevance of sugar to this effect argues against any resource-based model in which nutrition or blood glucose plays a central role (Gailliot et al. 2007, 2009). This, plus the fact that Prior Impulse Control reduces income-as-consumption bias, suggests that learning and attention effects are the best way to understand our estimated treatment effects.

---

[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. Our sample (N=270) is also much larger than that of Wang and Dvorak's (N=65).

[9] More recent studies include Deck and Jahedi (2015), Bregu *et al*. (2016), and Jahedi *et al*. (2016), which study the reduced-form effects of cognitive load *during* the decision process, alcohol consumption, and sexual arousal, respectively on a variety of decisions. According to these papers, contemporaneous cognitive load raises impatience while sexual arousal has no effect. Jahedi *et al*. did not study the effects of alcohol on intertemporal choices.

Our results are most consistent with a scenario where all three of our treatments moved subjects from a quick decision making process to a more deliberative one –from System 1 to System 2 in the two-systems framework (Stanovich and West 2000, Kahneman 2011). [10] In all three treatments, a possible mechanism is focusing subjects' attention to the intertemporal decision task. For the PIC treatment, a 'training' effect is also possible: because PIC requires subjects to repeatedly resist the temptation to make the easy ('snap') decision, it could have alerted them to the idea that obvious choices aren't always the best ones. The consistently low levels of utility curvature among our more cognitively able subjects in all treatment suggest they may have already been attuned to this possibility. Notably, our study is not the only one to find effects of the Stroop task on economic decisions that seem to contrast to its effects on behaviors like food and alcohol consumption, aggression and reliance on racial or gender stereotypes. For example, Burger et al. (2011) find that the Stroop task reduces procrastination and leads to a higher completion rate. de Haan and van Veldhuizen (2015) find no larger attraction effects and anchoring effects for subjects who had first to perform the Stroop task. Together, all these results suggest that economic decisions may respond to decision environments in quite complex ways, which may differ from those suggested by resource-based models.

Our results have three implications beyond the appropriate empirical domain for resource-based models of willpower. The first is methodological: by estimating the co-movement of multiple parameters of a *decision utility* function in response to exogenous treatments, we show how some insight into the types of behavioral biases that are affected by those treatments can be gained. Our focus on the co-movement of multiple preference parameters avoids the problem of pre-judging which preference parameter is most affected by the treatments, and does so in a context where –as we would expect— parameters interact nonlinearly. Additional credibility is added, we believe, by allowing each subject to have their own utility function. This differs from the aggregate treatment effect approach of Carvalho et al. (2016). Second, 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 has 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. The implications are especially important for people with lower cognitive abilities. Finally, our results also have implications for a sizable recent literature on the elicitation of time preferences (*e.g.*, Augenblick et al. 2015). An important outstanding question in this literature is "under

[10] In dual-process theory, System 1 refers to instinctive, automatic and emotional thinking while System 2 refers to more deliberative and logical thinking or analytic intelligence. Neuroeconomic studies have also contributed to explore how intertemporal decisions are managed by System 1 and System 2. For example, Radu and McClure (2013) recall the higher contribution of System 2 (in particular the DLPFC activity) to the executive cognitive processing of the evaluation of delayed outcomes *vs*. immediate ones, compared to System 1 (in particular the nuclear accumbens activity) that reacts more to immediate rewards.

what conditions can monetary choice experiments reveal present bias?" Since monetary choice experiments can only reveal such bias if individuals treat income as they would treat consumption --a point long emphasized by authors working on the theory of present bias—our finding that the decision environment affects the amount of income-as-consumption bias offers a partial answer.

2 – The Experiment

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

-- Table 1 about here --

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 other aspects of the drink (such as taste, hydration and possible perception as a reward), 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 because the convex budgets provide the informational efficiency necessary to estimate parameters for each individual separately and it does not require the elicitation of atemporal risk preferences to de-bias estimates of the discounting parameters. 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. Since the subjects' endowment of tokens was fixed in the later period at €16 (with experimental variation in the price at which these can be converted into earlier income), the experimental prices have a straightforward interpretation as borrowing charges for short-term loans, most like a payday-advance with a €16 paycheck to be received in the future.[11] We chose the experimental rates and terms to bracket typical terms available on short-term consumer debt in the United States.

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 (through the

[11] If the token endowment was, instead, fixed in the early period, price variations would reflect changes in interest earned on savings and no longer have an unambiguous predicted effect on early consumption (because the income effects of higher rates make lenders better off).

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 psychologists frequently use performance on the test as a measure of self-control.[12]

In our experiment, the participants' computer screens 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. A 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 six minutes. Although the task was not incentivized (to avoid creating a wealth effect compared to the other treatments), on average participants completed 127 trials (S.D. = 13.77; min = 76, max = 161). 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.[13] 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 relax and read magazines that we distributed for 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,

[12] See, for example, the discussion in Gailliot et al. (2007).
[13] 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).

participants had to assess the beverage and to report their usual consumption of soft drinks.[14] In the Baseline and the Prior Impulse Control treatments, the same rest period of 10 minutes was implemented.

2.5 – Procedures

The experiment was computerized using the REGATE-NG software. It consisted of 19 sessions conducted at GATE-Lab, Lyon, France. There were two rounds of data collection. The first featured eight sessions with two Baseline and PIC sessions and four Drink sessions. The second round featured 11 sessions with four Baseline sessions, two PIC sessions and five Drink sessions. Both rounds of collection involved the same procedures; the purpose of the second round was to improve our sample size. Participants, mostly undergraduate students from several local universities, were invited via the ORSEE software (Greiner 2015).[15] Between 9 and 20 participants took part in each session, for a total of 280 participants. Six sessions of the Baseline treatment were implemented with a total of 84 participants; four sessions of the Prior Impulse Control treatment were implemented involving 65 participants; and nine Drink sessions were implemented with 131 participants (66 in Sugar and 65 in Placebo).

One of our Drink sessions appears to have suffered a procedural failure. It is important for the interpretation of the results that subjects not perceive a difference in sugar content between the sugared and non-sugared drinks, and this is the case in all our drink sessions but one ($p > 0.418$ in all sessions, and $p = 0.353$ pooling all sessions). In session 15, however, the six subjects given a sugared drink reported perceived calories of 51 kCal on average and the four given a placebo drink reported an average of 181 kCal, a highly significant difference ($p = 0.005$) in the unexpected direction, and a large outlier in perceived calories of both drinks.[16] To avoid possible contamination of the results by procedural errors, we excluded the 10 subjects from this session from our analysis.

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

[14] 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 = 4.98, S.D. = 2.51) than those in the Sugar treatment (mean = 5.97, S.D. = 2.42) (two-tailed Mann-Whitney test, $p = 0.021$), they did not realize that they received a placebo. Indeed, they predicted the same number of calories contained in the beverage (mean = 127.14, S.D. = 84.07) than the participants placed in the Sugar treatment (mean = 131.86, S.D. = 106.232, two-tailed Mann-Whitney test of difference, $p = 0.811$).

[15] Among our 270 retained subjects, 225 (83.33%) were students. Among these students, 152 (67.56%) are from very selective engineering and business universities. The mean age is 21.68 years for the students (S.D. =2.30) and 37.07 years for the non-students (S.D.=13.36).

[16] Figure A2 in the Appendix shows the complete distribution of perceived calories by drink and session.

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.[17] 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 for instructions).

The elicitation of time preferences requires strict procedural rules. To participate in the experiment, the subjects were required to have 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.[18] During the session, instructions informed the participants that a show-up fee of €5 ($6.5 at the time of the study) 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 would be ordered by the finance department.[19] 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 were informed of 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). In the second round of data collection, this survey asked subjects how much importance they assigned to the price of an early token when making their decisions. Possible

[17] We did not measure baseline blood glucose levels, which would have required taking blood samples. The first round of collection took place on each day of the week in January; the second round took place on each day of the week in late April-early May. In total, we ran 4 sessions on Mondays, 5 on Tuesdays and Wednesdays, 2 on Thursdays and 3 on Fridays.

[18] 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 subjects participating in another experiment with a 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.

[19] 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.

responses were: ‘very important,’ ‘somewhat important’, ‘little importance’ or ‘no importance’. Sessions lasted 60 minutes and participants averaged earnings of €20.43 ($23.00, at the time of the study), with a standard deviation of €0.94 ($1.06), including the show up fee.

2.6 – Predictions

Subjects’ predicted demand in our experiment depends on whether they use a narrowly-bracketed type of decision-making and perceive payments received from the experimenters as a distinct form of consumption with diminishing marginal utility, or whether they frame their experimental decisions within the broader context of the borrowing and lending options they can access outside the lab. In what follows we refer to the former type of decision-making as *System 1* behavior, and the second as *System 2*, according to the dual-process theory (e.g., Stanovich and West 2000, Kahneman 2011).[20] Defining X (Y) as income received from the experimenters in the early (late) period and R as the relative price of early income, we make the following predictions.

Prediction 1 (System 1 behavior): If subjects treat experimental income as a distinct form of consumption with diminishing marginal utility,

a) subjects should demand more than half their endowment in the early period ($X > 8$) when $R = 1$.

b) early demand should fall monotonically as the experimental price of early income, R, rises above 1.

Under the assumptions of System 1 behavior, experimental subjects solve:

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

where $U' > 0$, $U'' < 0$, and the function U represents the utility derived from experimental payments.[21] M is the endowment of later-period income (€16 in our experiment). In the experiment, λ 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. Part (a) of Prediction 1 follows from the fact that subjects discount later consumption ($\lambda < 1$). Part (b) follows from the fact that income and substitution effects reinforce each other in our design (which fixes the value of the endowment in the future period.) Both parts of Prediction 1 are illustrated in Figure 1.

-- Figure 1 about here --

[20] Narrow-framing, including mental accounting, is associated to intuitive thinking (and to System 1), as individuals tend to consider each decision in isolation and pay little attention to the bigger picture (Kahneman 2011). Kahneman (2003) states that narrow-framing may be an effect of accessibility of relevant information, whereas effort and concentration (associated to System 2) help people to have in mind a more complete set of considerations.

[21] U need not correspond in any obvious way to the utility derived from total consumption, which is a distinct concept.

Empirically, we use a standard β-δ functional form (Laibson 1997; O'Donoghue and Rabin 1999) for subjects' utility in equation (1):

$$U(X) + \lambda U(Y) = X^{\alpha} + \beta^{T} \cdot \delta^{k} \cdot Y^{\alpha}, \quad (2)$$

where T is an indicator for whether the sooner date is today (equal to 1 if $t = 0$, and 0 otherwise). Maximizing (2) subject to the experimental budget leads to the sooner-income demand function:[22]

$$X_{ij} = [M \cdot (\beta^{Tj} \cdot \delta^{kj} \cdot R_j)^{(1/(\alpha\text{-}1))}] \,/\, [1 + R_j \cdot (\beta^{Tj} \cdot \delta^{kj} \cdot R_j)^{(1/(\alpha\text{-}1))}] \quad (3)$$

Prediction 2: (System 2 behavior): If subjects have access to extra-laboratory capital markets where the relative price of early income is π, and take full advantage of the resulting arbitrage opportunities, then *regardless of their consumption utility function*, their optimal choices of experimental payments will consist of corner solutions that maximize the market value of experimental payments (Chabris et al. 2008, Augenblick et al. 2015). Thus, X=0 when $\pi < R$ and Y=0 when $\pi > R$. More succinctly, subjects will behave *as if* their utility function was given by equation (2) with α=β=1, and $\delta = \pi^{-\frac{1}{k}}$. Maximizing the present value of experimental payments implies:

$$\text{Max}_{X,Y} \quad \pi X + Y, \quad \text{subject to } RX + Y \leq M, \; X \geq 0, \text{ and } Y \geq 0, \quad (4)$$

which yields the corner solutions described. Note that (4) is the special case of (1) where $U'' = 0$ and $\lambda = 1/\pi$.[23] Note also that in aggregate data with heterogeneous access to credit across subjects, the demand curves generated from System 2 behavior will also satisfy the properties of Prediction 1—starting at above 8 units at R=1, then declining monotonically as R rises.

Prediction 3: Treatment effects under System 1 behavior. If subjects exhibit System 1 behavior, their estimated values of α should be less than one. Also, we expect subjects to discount the future ($\delta < 1$) and present-biased subjects will exhibit $\beta < 1$. Treatment effects will represent changes in the utility derived directly from experimental payments. If (as suggested by willpower-based resource models) engaging in prior impulse control reduces subjects' ability to resist immediate (earlier) payments, the PIC treatment should reduce our estimate of β (δ) to values farther below 1. If consuming sugar improves subjects' ability to resist immediate (earlier) payments, the Sugar treatment should raise the estimate of β (δ) to

[22] 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.

[23] In practical terms, we expect π to be close to 1 (corresponding to the approximately zero interest rate on a typical student's bank account) for all subjects whose optimal *overall* consumption plan over the payment interval (k) puts them into the lending domain. For borrowers, π is given by the best extra-laboratory option for converting later to sooner income, for example borrowing from friends or family, or paying a bill or credit card late. Prediction 2 assumes that experimental payments are not large enough to move subjects between the lending and borrowing domains.

values closer to 1 (compared to the sugar-free drink). Willpower resource models predict no treatment effects on α, and do not have a clear prediction for any effects of a non-sugared drink.

Prediction 4: Treatment effects under System 2 behavior. If subjects exhibit System 2 behavior, estimated values of α and β should equal one.[24] All experimental choices will be corner solutions (with all tokens allocated to one period only), and exhibit extreme sensitivity to the experimental price, R, by switching from one corner to the other at a critical price. None of our experimental treatments should affect behavior, because subjects maximize the present value of their experimental payments at their (potentially person-specific) market interest rate, which is not affected by the treatments.

Finally, we consider the possibility that our treatments could shift individuals from System 1 to System 2 behavior, or 'in the direction of' either System. This could happen, for example, if a treatment prompted subjects to pay more attention to the broader context and to the extra-laboratory consequences of their decisions. Comparing Predictions 3 and 4, we have:

Prediction 5: Treatment effects on which System is used. Treatments that shift individuals from System 1 to System 2 should raise the estimated values of α and β towards one.

Predictions 3-5 are summarized in Table 2. While these predictions are designed to highlight the implications of the resource-based self-control model and its interactions with System 1 versus System 2 behavior, it should be clear that our estimation method can also be viewed more agnostically, as a flexible way to estimate the effects of our treatments on subjects' demand functions for early experimental payments. For example, if a treatment increases subjects' demand for early payments regardless of whether the early period is the day of the experiment, the demand function in (4) accommodates this via the parameter δ (with an optional interpretation as an effect on the subjective discount rate). If a treatment increases subjects' attraction only to rewards that are received on the date of the experiment, this accommodated by β and can be interpreted as an increase in present bias. Finally, if treatments make subjects less responsive to the price of early income, this should be reflected in the estimated value of α, with the optional interpretation as a switch towards System 1 decision-making.

-- Table 2 about here --

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 report nonparametric and structural approaches to analyzing the treatment effects,

[24] β must equal 1 (no present bias) because intertemporal preferences are now driven by the market interest rate.

respectively. The final section presents some supporting evidence and robustness checks. Since, as noted, our subject pool is largely from selective universities, we present all of our experimental results separately according to our subjects' reported achievement on the French Baccalauréat exam to better assess their representativeness.[25] Specifically, we present results separately for the 110 subjects who earned "highest honors" on the exam (a score of 16 or more) versus the 160 other subjects. Only 9% of French Baccalauréat recipients earned highest honors, yet due to the selected nature of our sample, 40% of our subjects earned highest honors. This is our 'high-score' group. The remaining 'lower-score' group roughly represents the 50th through 90th percentiles of baccalaureate recipients. Since the latter group is more representative of the French population, we focus most of our discussion on it.[26] Our separate treatment of this group is also motivated by evidence from Stanovich and West (2000) that System 2 differentially affects persons with higher analytic intelligence when System 1 and System 2 cue different or opposite responses.

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 aggregate demand curves satisfy Prediction 1.

Result 1 – Consistent with Prediction 1, 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 aggregate demand curves for the early payment (X), separately by score and pooled across all treatments. The majority of the demand curves start above eight units of X at levels of R closest to one, then fall monotonically as R rises. The success of these basic predictions (which apply to both System 1 and 2 behavior) suggests that our participants' choices are informative for the preferences we wish to study. While the aggregate demand curves in Figure 2 suggest a continuous response to prices, it is important to note that corner solutions play an important role in many subjects' decisions. Specifically, 36% of the subjects make no interior choices (this percentage is 37% in AS (2012)). Further, among the subjects who make an interior choice decision at least once, 57%

[25] The French Baccalauréat exam is taken at the end of high school (*lycée*). In 2016, 78.6 percent of French youth had passed the Baccalauréat.
[26] Our results are not sensitive to the precise score cutoff used. We can also replicate our main results, dividing the sample by the results of the Cognitive Reflection Test (CRT) in lieu of the Baccalauréat score. This may represent a more direct measure of the ability of the subjects to switch off System 1 to System 2: see Section 3.4.

of the responses are corner decisions (50% in AS). We study these corner solutions in greater depth when we model the utility functions of individual subjects and their responses to treatments in Section 3.3.

-- Figure 2 about here --

Result 2 – There is aggregate evidence of statistically significant present bias in our data.

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.[27] 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 advance significantly more earnings. High-score subjects do the same for only the 15-week delay, and the estimate for lower-score subjects using a 5-week delay is only marginally significant. Using a price-free measure of early demand (tokens allocated to the sooner date), the t effect is to reduce demand by roughly 3% when t increases from 0 to 5 and by roughly 9% when t increases from 0 to 15.

-- Table 3 about here --

3.2 – Reduced Form Estimates of Treatment Effects

Result 3 – In the high-score group, none of the treatments have statistically significant effects on aggregate demand for early payment. In the lower-score group, both Prior Impulse Control and Drink reduce the demand for early payment, but Sugar has no effect above and beyond Drink.

Figure 3 presents the mean early payment demanded across treatments for the full sample and then separately according to high- and lower-score status. The top row compares the Baseline, Prior Impulse Control and Drink treatments. The bottom row compares the Placebo and Sugar treatments (within Drink). We find that both PIC and the Drink treatment reduce the demand for early payment –making subjects appear more patient. These effects are driven entirely by the behavior of lower-score subjects, where we find effect sizes of 29% and 21% for PIC and Drink, respectively. Within the Drink treatment, early payment demand in Sugar is slightly higher, although the levels are very similar.

-- Figure 3 about here --

The OLS regressions underlying the p-values in Figure 3 are shown in Panel A of Table 4; standard errors are clustered at the individual level. As suggested by the graphs, all treatment effects are driven by the lower-score sample. There is no significant effect of a sugared drink beyond the effect of a drink itself

[27] A regression approach is necessary because price ratios are not exactly balanced across the t dimension. This will not be necessary in treatment-effect specifications because prices are balanced across that dimension. Results are similar using a price-free measure of early demand, the number of tokens allocated to the early date.

and the null impact of the Sugar treatment in the full sample is quite precise: we can reject that a sugared beverage has a patience-increasing effect of any magnitude larger than *one-fifteenth* of a standard deviation in the full sample. Testing the PIC and Drink treatment effects across test score samples yields significant difference-in-difference estimates: a marginal $p = 0.063$ for PIC and $p = 0.037$ for Drink.

-- Table 4 about here --

To adjust for the fact that many of subjects' choices were at the endpoints of the budgets we offered, we present Tobit estimates in Panel B of Table 4. On each budget, the minimum early income was €0 and the maximum was €(16/*R*). The Tobit model adjusts for the data censoring at these points. As expected from the mechanical compression of the dependent variable in the OLS regressions, these coefficient magnitudes are considerably higher. The Tobit models also indicate significant PIC and Drink effects for the lower-score sample only. The magnitudes of these effects are large enough (in the case of column (2)) such that the full sample treatment effect is marginally statistically significant. Again, we find no statistically significant impact of Sugar above and beyond Placebo, especially not in the direction of increasing patience. Together, the results in Figure 3 and Table 4 cast doubt on the resource model as a predictor of early income demand. That model argues that willpower depletion should reduce, and sugar consumption should increase the demand for early income, with no clear implications for the effect of sugar-free drink consumption. Instead, we find that prior impulse control and sugar-free drink consumption increase the demand for early income, with no added benefit of sugar.

While these results question resource-based willpower models, we emphasize that they do not necessarily imply that subjects' physiological conditions are irrelevant to decision-making. Indeed, our data also indicate that subjects' demand for early income is strongly and positively correlated with the amount of time since they last ate before the experiment.[28] In the full sample, a one-hour increase in time predicts an extra €0.11 of demand ($p = 0.018$). In the lower-score sample, a one-hour increase in time predicts an extra €0.26 of demand ($p = 0.009$) in the Baseline treatment. In the Drink treatment, the correlation is a precise zero, and the difference in correlations between Baseline and Drink is marginally significant ($p = 0.075$). Again, whether the drink is sugared or not makes no significant difference ($p = 0.665$). Because time since last meal is not randomly assigned, this evidence should be interpreted with caution. Still, this evidence is consistent with estimated nutritional effects in other contexts (Briers et al. 2006; Danziger et al. 2011), and suggests that the effects of not just sugar but other consumables on economic decision-making may be considerably more complex than our current models hypothesize.

[28] The requirement was not to eat for three hours prior to the experiment. Fifty-eight percent of our subjects last ate more than four hours prior to the experiment, and 30 percent hadn't eaten for at least ten hours, with no significant difference between treatments, or between lower score and high score subjects.

3.3 – Treatment Effects on Individual Subjects in a Structural Model of Time Preferences

To learn more about *how* and *why* our treatments affected subjects' intertemporal income choices, we take advantage of the richness of our CTB data to estimate the parameters α, β and δ of each subject's decision utility function, and assess the resulting patterns in light of Predictions 1-5.[29] Since certain features of the data prevent us from estimating a complete parameter set for every subject, we proceed as follows. First, despite the fact that every subject faced effective annual interest rates ranging from 4% to 845%, 33 individuals exhibited no choice variation whatsoever. We drop these individuals since we cannot estimate their preference parameters. Second, 97 individuals made no interior choices. For these individuals, we set $\alpha = 1$ and estimate their β and δ 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.

Third, for the remaining individuals who made at least one choice from the interior of a budget, we estimate the demand function in equation (3) by NLS.[30] Finally, we discard the estimates of individuals for whom $\alpha < 0$, because these values violate the maximization assumptions, and discard the estimation results in which convergence for a parameter was not achieved (1 observation for α and 4 observations for β). In all, this procedure yields 227 estimates of α, 206 estimates of β and 204 estimates of r.

Result 4 - For lower-score subjects, the Prior Impulse Control and Drink Treatments move our estimates of α and β towards one, reducing both utility curvature and present bias. Sugar has no effect above and beyond Drink. None of the treatments have statistically significant effects on the high-score subjects.

Figure 4 shows the empirical CDFs of estimated preference parameters, by treatment.[31] For ease of interpretation, we present our results for the utility discounting parameter in terms of $r = \delta^{-365} - 1$, the yearly discount rate equivalent. Because some of our subjects exhibit 'future bias' ($\beta > 1$), these Figures also report *minus* the absolute distance of β from 1, $b = -|\beta - 1|$ as a general measure of distorted

---

[29] We also estimated equation (4), imposing the same preference parameters across all individuals and treatments. Our estimate of the aggregate yearly discount rate is 19.8% for lower-score subjects (S.E. = 4.7%) and 24.3% for high-score types (S.E. = 4.0%).[29] Our estimate of the β parameter is 0.981 (S.E. = 0.007) for lower-score and 0.987 (S.E. = 0.006) for high-score subjects, with both values significantly (marginal for high-score group) less than 1 (p = 0.010 and p = 0.051, respectively). Thus, we find evidence of present bias in the β-δ form. We also estimate significant utility curvature: α = 0.903 (S.E. = 0.008) for lower-score and 0.935 (S.E. = 0.005) for high-score individuals.

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

[31] Figures 4 and 5 use this trim to improve the visualization: $0.75 < \alpha \leq 1$, $0.75 < \beta \leq 1$, $b > -0.15$, $-0.25 < r < 1.5$.

decision-making when rewards are immediately available (b is maximized at zero, indicating that neither present- nor future bias is present). According to Figure 4, for lower score subjects all three treatments shift the distributions of of α, β, and b to the right. Kolmogorov-Smirnov tests indicate that many of these shifts are statistically significant.[32] There are no significant shifters of the r distribution, nor do any of the treatments significantly affect the parameter distributions for the high-score group.

-- Figure 4 about here --

To quantify the size and significance of the distributional shifts shown in Figure 4, Table 5 reports estimates of treatment effects on the 25th, 50th and 75th percentiles of the individual-specific parameters. Our focus on median regressions is motivated by (a) their natural mapping into the above distributions of individual utility parameters, (b) the noise associated with estimating individual-level parameters, and (c) the inherent lack of cardinality in utility parameter estimates.[33]

-- Table 5 about here --

The results indicate that both PIC and Drink decrease curvature and present bias in the lower-score sample. We also see some significant treatment effects at the 25th and 50th percentiles in the full sample, but this is because the lower-score subjects make up that part of the distribution. Notably, there are stronger treatment effects for lower score subjects on b in addition to β, indicating that effect is to push β towards one, rather than to push β up in general. This is consistent with the treatments having a general 'de-biasing' effect. In general, the difference-in-difference treatment effects between lower- and high-score subjects are not estimated precisely enough to be statistically significant.[34]

Overall, while indicating that the immediate decision environment affects choice behavior, our individual estimates of treatment effects are inconsistent with several features of resource-based models. Specifically, in contrast to Prediction 3, we find a perverse impact of prior impulse control, no effect of a sugared drink compared to a sugar-free drink, and treatment effects on α. We also find treatment effects for the sugar-free drink. Instead, while only 97 of our 270 subjects exhibited 'pure' System 2 behavior (as defined in Prediction 4), our estimated treatment effects on α suggest that both the Drink and PIC treatments moved our subjects 'in the direction' of System 2 behavior, consistent with Prediction 5. Instead of depleting willpower, the Stroop task may have primed our subjects to be more reflective and

[32] α: p = 0.048, 0.014 and 0.049 for PIC, Placebo and Sugar respectively in the lower-score sample and p = 0.298, 0.549 and 0.946 for PIC, Placebo and Sugar in the high-score sample. β: p = 0.062, 0.201 and 0.484 for PIC, Placebo and Sugar for lower-score and p = 0.136, 0.249 and 0.197 for PIC, Placebo and Sugar for high-score. b: p = 0.067, 0.014 and 0.250 for PIC, Placebo and Sugar for lower-score and p = 0.381, 0.947 and 0.985 for PIC, Placebo and Sugar for high-score. r: p = 0.408, 0.552 and 0.527 for PIC, Placebo and Sugar for lower-score and p = 0.352, 0.661 and 0.529 for PIC, Placebo and Sugar for high-score.

[33] Unlike, for example, early payment demand, there is no clear sense in which a difference between $\alpha = 0.95$ and $\alpha = 0.9$ is of the same magnitude or importance as the difference between $\alpha = 0.9$ and $\alpha = 0.85$.

[34] Results obtained with OLS regressions at the mean are similar, once the outliers are trimmed. However, the size of the estimates is sensitive to the trim, making the quantile approach preferable.

trained them to become more price-sensitive. Indeed, by construction the Stroop task repeatedly draws subjects' attention to the fact that the obvious answer is not always the correct one. This is consistent with the recent studies of Burger et al. (2011) and de Haan and van Veldhuizen (2015) who found that performing the Stroop task does not increase behavioral biases in subsequent tasks.[35] Providing fasted subjects with a sweet-tasting drink, regardless of its sugar content appears to have had a similar effect. This suggests that a placebo effect might have occurred. This is consistent with a literature showing that a placebo beverage can influence decision-making.[36]

An additional advantage to the structural approach is that we can predict treatment effects for realistic payday advance decisions. For example, consider an individual offered a 2-week payday advance with a 15% charge, against a €1000 income receipt.[37] A lower-score individual with the median utility parameters in the Baseline treatment would choose a €112 advance. The median lower-score individual in PIC would advance €23 and in Drink would advance just €1. Similar predictions illustrate the importance of present-bias: a front-end delay on the receipt of above the advance would reduce Baseline demand to €73 and leave demand from the other treatments largely unaffected.

3.4 – Supporting Evidence and Robustness

We begin this Section with four pieces of evidence on whether treatments shifted subjects from System 1 to System 2 thinking. The first is based on the exit survey that was conducted during our second round of data collection. As explained in Section 2.5, to assess whether subjects were paying attention to the relative price of early consumption we asked them how much importance they assigned to the price of an early token. Using a Probit model, we estimated the impacts of our treatments on the likelihood that subjects report price to be 'very important' and predict the discrete effects of each treatment relative to the Baseline level of 8.3 percent. We found that all three treatments raise the share of subjects stating that price was very important: to 18.7 percent (p = 0.014) in Prior Impulse Control, to 19.7 percent (p = 0.008) in Placebo and to 21.7 percent (p = 0.002) in Sugar. Relatedly, we point to Carvalho et al. (2016), who find that randomized provision of savings accounts to poor households in Nepal decreases utility curvature in a CTB task. This is direct evidence that outside options, when made salient, matter for intertemporal financial choice.

---

[35] The Stroop task also seems not to have depleted other cognitive resources (like attention or working memory) that are needed to be aware of arbitrage opportunities. This is consistent with Muraven (1998) and Muraven and Baumeister (2000) who distinguish self-control from these other mental resources, and present evidence that depleting one does not necessarily deplete the other.

36 For example, in alcohol research, studies have shown that participants who receive a placebo drink may improve behavior in social situations and performance because they try to compensate for expected cognitive impairment and they reinforce their attention to situational cues (Testa et al. 2006). Molden et al. (2012), Sanders et al. (2012), Boyle et al. (2016) and Dang (2016) have shown a variety of placebo effects in the domain of self-control and resource consumption.

[37] So the maximum they can borrow is €1000/1.15 ≈ €870.

Second, we conducted tests to see which subjects' behavior was inconsistent with sooner income being an ordinary good: when price goes up, demand should go down. If our treatments reduced the number of these inconsistencies, this would add credence to the idea that subjects were considering their decisions more carefully and in the context of the prices they faced. We found that the PIC and Drink treatments significantly reduced both the number of ordinarity violations subjects made and the fraction of subjects making any violations. Roughly 30% of Baseline subjects made at least one violation, but only 11% of PIC subjects did so ($p = 0.003$) and 15% of Drink subjects did so ($p = 0.007$), while Sugar had no significant effect beyond Placebo ($p = 0.661$). The number of violations was reduced by about half moving from Baseline to PIC and Drink ($p = 0.047$ and 0.053, respectively). As with our main results, these effects were driven by the lower-score sample.

Third, the fact that the decision environment consistently has no effect on subjects with very high cognitive abilities is consistent with the idea that cognitive factors, like the ability to conceptualize the current decision in a wider context, play an important role (Stanovich and West 2000).

A final piece of evidence comes from the cross-sectional covariance of estimated decision-utility parameters among our subjects. If the primary source of heterogeneity among our subjects is differential adherence to System 1 versus System 2 (as opposed to differences in endowed 'patience'), then when adapting Prediction 5 to the general, cross-sectional context we should expect α and β to covary positively across subjects with some present bias ($\beta < 1$). For future-biased subjects ($\beta > 1$), α and β should covary negatively, as System 2 behavior again pulls both α and β towards one. If true, this would provide additional evidence that differences in adherence to System 2 decision-making, and not differences in patience, are most relevant for decision heterogeneity in our context. This is exactly what we find. Conditional on $\beta \leq 1$, individual estimates of α and β feature a correlation coefficient of 0.367 ($p < 0.001$, $N = 141$). The positive coefficient means that when curvature decreases, present bias also decreases. Conditional on $\beta \geq 1$ (future-bias), individual estimates of α and β are negatively correlated with coefficient -0.267 ($p = 0.032$, $N = 65$). The negative coefficient means that when curvature decreases, future bias also decreases. Pooling the two biases using b yields a correlation coefficient of -0.326 ($p < 0.001$, $N = 207$); as curvature decreases, consumption biases decrease. This correlation is not a result of our treatments moving both parameters simultaneously: limiting the sample to Baseline only yields a correlation coefficient between α and b of 0.271 ($p = 0.028$, $N = 66$).

We conclude this Section with three robustness checks. First, we ask whether our main results can be replicated using an alternative measure of cognitive ability. We used the Baccalauréat exam as our main measure because it is available for all subjects and is the result of a test that in most cases was conducted a number of years before our experiment. Our alternative measure --the CRT score-- was elicited at the end of our experiment and could therefore have been affected by our treatments. With that caveat in mind, we estimated treatment effect estimates split by CRT performance instead of by Baccalauréat score. While CRT score is quite predictive of Baccalauréat score, the two are far from perfectly correlated (correlation coefficient = 0.179, p = 0.003). Consistent with our results using the Baccalauréat, we found that the negative treatment effects on demand for early income are concentrated at low CRT scores. Regression results that replicate columns (2), (4) and (6) of Table 3 using CRT scores are provided in Appendix Table A2.

A second and related robustness check asks how our main results would change using a finer breakdown of Baccalauréat scores than whether subjects are above or below 16. This is examined in Appendix Figure A3, which shows demand for early payments as cubic functions of Baccalauréat score. Figure A3 shows a pattern very similar to our main results: PIC, Sugar and Placebo Treatments all significantly reduce early income demand relative to the baseline for a range of Baccalauréat scores (12-15) below the threshold for highest honors.

Our final robustness check investigates whether splitting the sample on dimensions other than cognitive ability would also reveal behavioral differences between groups. We use factor analysis to reduce our survey data to as few elements as possible and thus mitigate multiple hypothesis testing concerns. [38] Four factors explain 98% of the variance in responses to our 15 survey questions. Of these four retained factors, two are substantively correlated in the same direction with Baccalauréat performance and CRT score. The first we interpret as a "school-smarts" factor because it is positively related to being young, a student at one of the more selective schools in the sample, and not currently employed. The second we interpret as a "money-smarts" factor because it is *positively* related to being employed, to having more wealth and *not* currently being a student. We regress early demand on treatment indicators for PIC, Drink and Sugar, each factor, and all the interaction terms. In the baseline treatment, we find that students with more 'money-smarts' demand less early income (p = 0.052). More importantly, the PIC and Drink treatment effects decrease in magnitude as 'money-smarts' increase (p = 0.072 and p = 0.038, respectively). The school-smarts factor is never a significant predictor of demand in any treatment –either on its own or in an interaction. The only factor beyond money-smarts that has a significant effect on early demand captures the elapsed time since the subject last ate, but none of the cognitive variables. It predicts a stronger Drink impact for hungry subjects (p = 0.041).

[38] Our survey includes information on three factors that other researchers have connected to self-control: gender (Silverman 2003), age (Steinberg et al. 2009), and some correlates of wealth (Green et al. 1996). A full list of our survey variables is available upon request. While there are more than 15 questions, some of the information collected is redundant and we use 15 variables in the factor analysis. We use orthogonal factors that maximize the explained variation by each factor, so as to require as few as possible.

4 – Conclusion

This paper studies the effects of three interventions --prior exercise of impulse control (PIC), consumption of a sugared drink, and consumption of a sugar-free drink-- on intertemporal financial choices. Our choice task is designed to bracket the terms and interest rates associated with payday advances, and the interventions are events that occur frequently in the course of a typical human day. We find that these events have large effects on the demand for cash advances. For example, according to our structural estimates, the Drink (PIC) treatments should reduce a median lower-score subject's demand for a 2-week payday advance with a 15% charge from €112 to just €23 (€1). While these effects are only observed in our 'lower'-score sample, we remind the reader that this sample represents about the 50th-90th percentiles of Baccalauréat recipients in France. Thus, they apply to a large share of the population. While our estimates can say nothing about the bottom half of the ability distribution, they hint at the possibility of even larger effects for that group.

Our estimated treatment effects are inconsistent in a number of ways with resource-based models of willpower; especially versions of those models that associate resources with nutrition and blood glucose levels (Gailliot et al. 2007, 2009). Specifically, we find a 'wrong'-signed impact of prior impulse control, and no effect of a sugared drink compared to a sugar-free drink. Indeed, our large sample allows us to statistically reject a negative effect of the sugared beverage on loan demand (relative to the sugar-free beverage) of any magnitude larger than one-fifteenth of a standard deviation. We also find treatment effects for the sugar-free drink, which are hard to reconcile with the resource-based model unless water, carbonation or flavorings replenish subjects' willpower.[39]

To understand these effects, we used our Convex-Time-Budget data to estimate a three-parameter decision utility function for each of our subjects. The estimates show that the experimental treatments act, at least in part, on the utility curvature parameter (α), which does not play an obvious role in willpower-based models. One potential mechanism for our treatment effects is on cognitive effort --increasing reflective decision-making, which we formalize as 'System 2 behavior'. If this led to an increased attention to prices (Koszegi and Szeidl 2013) or a broader framing of choices in the context of market alternatives, it would explain our results. While it might seem surprising that prior impulse control can have this effect, we note that our PIC treatment –the Stroop task-- repeatedly reminds subjects that thoughtless but natural responses can be wrong. Thus it may have *trained* subjects to avoid such

[39] An extension of this research could compare decisions in the Placebo treatment to a new treatment where subjects would only be offered water. This would allow us to test whether our placebo generated a conditioned opponent response (Rozin *et al.* 1984, Fillmore and Vogel-Sprott 1992). In contrast to our findings for intertemporal financial choice, Dickinson *et al*. (2014) find that (relative to a non-sugared drink) consuming a sugared beverage did increase the likelihood of making a Bayesian choice over a heuristic-based choice in a switching task.

responses. Extensions of our work with additional treatments could explore the exact mechanisms by which this effect operates and measure directly how treatments affect attention and cognitive processes.

These findings have a number of implications. First, they suggest that intertemporal variation in cognitive factors, like attention and the ability to frame decisions in a broader context, may be a more important influence on intertemporal financial decisions than variation in willpower—i.e. in the ability to resist urges for immediate gratification. Indeed, framing economic decisions in their wider context is cognitively challenging. For example, it is much easier to assess the timing and riskiness of the portfolio in a single retirement account in isolation than to assess each account in the context of all of one's other assets and debts. Mullainathan and Shafir (2013) also emphasize scarcity of cognitive bandwidth as an important determinant of the quality of financial decisions in time- and money-poor households.

Second, by affecting these cognitive processes, interventions like a simple drink or a cognitive warm-up exercise may lead people to frame their financial choices more broadly and exhibit less present-biased decision-making. [40] Thus, future research may profitably focus on understanding the effects of these and other very short-term environmental factors on financial decision making. Are people making important choices in the environment and cognitive state that their long-run self would most appreciate? Especially for less-educated individuals in stressful circumstances, our results suggest that being alert and sated when making an important intertemporal choice should improve outcomes. Identifying additional aspects of the decision environment that encourage a broader frame deserve further study, both inside and outside the laboratory. Finally, our results also suggest that opportunistic sellers of some products may have an incentive to manipulate their customers' decision environment to *reduce* the customer's focus on prices and market alternatives. While this may not come as news to salespeople or marketing researchers, its consequences have hardly been explored by economists.

References

Andreoni, J., Sprenger, C. (2012). Estimating Time Preferences from Convex Budgets. *American Economic Review*, 102(7), 3333-3356.

Andreoni, J., Kuhn, M.A., Sprenger, C. (2015). Measuring Time Preferences: A comparison of experimental methods. *Journal of Economic Behavior & Organization*, 116, 451-464.

Anguera, Joaquin A., Jessica A. Bernard, Susanne M. Jaeggi, Martin Buschkuehl, Bryan L. Benson, Sarah Jennett, Jennifer Humfleet, Patricia Reuter-Lorenz, John Jonides, and Rachael D. Seidler (2012). The effects of working memory resource depletion and training on sensorimotor adaptation. *Behavioral Brain Research*, 228(1), 107–115.

[40] For those concerned about the external validity of our experimental measures of intertemporal choice behavior, 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).

Augenblick, N., Niederle, M., Sprenger, C. (2015). Working over Time: Dynamic Inconsistency in Real Effort Tasks. *Quarterly Journal of Economics*, 130(3), 1067-1115.

Baumeister, R. F., Bratslavsky, E., Muraven, M., Tice, D. M. (1998). Self-control depletion: Is the active self a limited resource? *Journal of Personality and Social Psychology*, 74(5), 1252–1265.

Benjamin, D.J., Brown, S.A., Shapiro, J.M. (2013). Who is "Behavioral"? Cognitive ability and anomalous preferences. *Journal of the European Economic Association*, 11(6), 1231-1255.

Boyle, N.B., Lawton, C.L., Allen, R., Croden, F., Smith, K., Dye, L. (2016). No effects of ingesting or rinsing sucrose on depleted self-control performance. *Physiology and Behavior*, 154,151-160.

Bregu, K., Deck, C., Ham, L., Jahedi, S. (2016). The Effects of Alcohol Use on Economic Decision Making. Working Paper, University of Arkansas.

Briers, B., Pandelaere, M., Dewitte, S., Warlop, L. (2006). Hungry for money: The desire for caloric resources increases the desire for financial resource and vice-versa. *Psychological Science*, 17(11), 939-943.

Burger, N., Charness, G., Lynham, J. (2011). Field and online experiments on self-control. *Journal of Economic Behavior and Organization*, 77 (3), 393-404

Burton, V.. (2016). Ten Strategies to Master Your Money. *CBN.com Finance*.

Carter, E. C., McCullough, M. E. (2013). Is ego depletion too incredible? Evidence for the overestimation of the depletion effect. *Behavioral and Brain Sciences*, 36(6), 683-684.

Carter, E. C., McCullough, M. E. (2014). Publication bias and the limited strength model of self-control: Has the evidence for ego depletion been overestimated? *Frontiers in Psychology*, 5(823).

Carvalho, L.S., Prina, S., Sydnor, J. (2016). The effect of saving on risk attitudes and intertemporal choices. *Journal of Development Economics*, 120, 41-52.

Chabris, C.F., Laibson, D., Schuldt, J.P. (2008). Intertemporal Choice. In S. Durlauf and L. Blume (Eds.), *New Palgrave Dictionary of Economics.* Palgrave Macmillan.

Dang, J. (2016). Testing the role of glucose in self-control: A meta-analysis. *Appetite*, 107(1), 222-230.

Danziger, S., Levav, J., Avanaim-Pesso, L. (2011). Extraneous Factors in Judicial Decisions. *Proceedings of the National Academy of Sciences*, 108(17), 6889-6892.

Deck, C., Jahedi, S. (2015). The Effect of Cognitive Load on Economic Decision Making: A Survey and New Experiments. *European Economic Review*, 78, 97-119.

De Haan, T., & Van Veldhuizen, R. (2015). Willpower depletion and framing effects. *Journal of Economic Behavior & Organization*, 117, 47-61.

DeWall, C. N., Baumeister, R. F., Stillman, T. F., Gailliot, M. T. (2007). Violence Restrained: Effects of Self-regulation and its Depletion on Aggression. *Journal of Experimental Social Psychology*, 43(1), 62-76.

Dickinson, D.L., McElroy, T., Stroh, N. (2014). Impact of Glucose on Bayesian versus Heuristic-based Decision Making. *Journal of Neuroscience, Psychology, and Economics*, 7(4), 237-247.

Dohmen, T., Falk, A., Huffman, D., Sunde, U. (2010). Are Risk Aversion and Impatience Related to Cognitive Ability? *American Economic Review*, 100(3), 1238-1260.

Donohoe, R. T., Benton, D. (1999). Blood glucose control and aggressiveness in females. *Personality and Individual Differences*, 26(5), 905– 911.

Ebbinghhaus, H. (1885). *Memory: A Contribution to Experimental Psychology*. Translated by Henry A. Ruge and Clara E. Bussenius. New York: Columbia University, 1913.

Fillmore, M., Vogel-Sprott, M. (1992). Expected effect of caffeine on motor performance predicts the type of response to placebo. *Psychopharmacology*, 106(2), 209-214.

Financialhighway.com (2010). Five Tips To Making Better Financial Decisions. February 25.

Frederick, S. (2005). Cognitive Reflection and Decision Making. *Journal of Economic Perspectives*, 19(4), 25-42.

Gailliot, M. T., Baumeister, R. F. (2007). The Physiology of Willpower: Linking Blood Glucose to Self-control. *Personality and Social Psychology Review*, 11(4), 303-327.

Gailliot, M.T., Baumeister, R.F., DeWall, C.N., Maner, J.K., Plant, E.A., Tice, D.M., Brewer, L.E., Schmeichel, B.J. (2007). Self-control relies on glucose as a limited energy source: willpower is more than a metaphor. *Journal of Personality and Social Psychology*, 92(2), 325-336.

Gailliot, M. T., Peruche, B. M., Plant, E. A., Baumeister, R. F. (2009). Stereotypes and Prejudice in the Blood: Sucrose drinks reduce prejudice and stereotyping. *Journal of Experimental Social Psychology*, 45(1), 288-290.

Gordijn, E. H., Hindriks, I., Koomen, W., Dijksterhuis, A., van Knippenberg, A. (2004). Consequences of stereotype suppression and internal suppression motivation: A self-regulation approach. *Personality and Social Psychology Bulletin*, 30(2), 212–224.

Green, L., Myerson, J., Lichtman, D., Rosen, S., Fry, A. (1996). Temporal discounting in choice between delayed rewards: the role of age and income. *Psychology and aging*, 11(1), 79.

Greiner, B. (2015). Subject Pool Recruitment Procedures: Organizing Experiments with ORSEE. *Journal of the Economic Science Association*, 1(1), 2015, 114-125.

Hagger, M. S., Chatzisarantis, N. L. D. (2014). It is premature to regard the ego-depletion effect as 'too incredible'. *Frontiers in Psychology, 5*, 298.

Hastings, J.S., Mitchell, O.S. (2011). How Financial Literacy and Impatience Shape Retirement Wealth and Investment Behaviors. Working paper, NBER No. 16740.

Hockenberry, J.M., Helmchen, Lorens A. (2014). The nature of surgeon human capital depreciation. *Journal of Health Economics* 37(C), 70-80.

Ifcher, J., Zarghamee, H. (2011). Happiness and Time Preference: The Effect of Positive Affect in a Random-Assignment Experiment. *American Economic Review*, 101(7), 3109-3129.

Jahedi, S., Deck, C., Ariely, D. (2016). Arousal and Economic Decision Making. Working Paper, RAND.

Kahan, D., Polivy, J., Herman, C. P. (2003). Conformity and dietary disinhibition: A test of the ego-strength model of self-regulation. *International Journal of Eating Disorders*, 32(2), 165–171.

Kahneman, D. (2003). Maps of bounded rationality: psychology for behavioral economics. *American Economic Review* 93(5), 1449-1475.

Kahneman, D. (2011). *Thinking, Fast and Slow*. Farrar, Straus and Giroux, New York.

Kim, M., Dessirier, J., van Hout, D., Lee, H. (2015) Consumer context-specific sensory acceptance tests: Effects of a cognitive warm-up on affective product discrimination. *Food Quality and Preference* 41, 163–171

Koszegi, B., Szeidl, A. (2013). A Model of Focusing in Economic Choice. *The Quarterly Journal of Economics*, 128(1), 53-104.

Kurzban, R. (2010). Does the brain consume additional glucose during self-control tasks? *Evolutionary Psychology*, 8(2), 244–259.

Laibson, D. (1997). Golden Eggs and Hyperbolic Discounting. *Quarterly Journal of Economics*, 112(2), 443-478.

Linder, J.A., Doctor, J.N., Friedberg, M.W., Nieva, H.R., Birks, C., Meeker, D., Fox, C.R. (2014) Time of Day and the Decision to Prescribe Antibiotics *JAMA Internal Med* 174(12), 2029-2031.

MacLeod, C.M. (1991). Half a Century of Research on the Stroop Effect: An Integrative Review. *Psychological Bulletin*, 109(2), 163-203.

Meier, S., Sprenger, C. (2010). Present-Biased Preferences and Credit Card Borrowing. *American Economic Journal – Applied Economics,* 2(1), 193-210.

Meier, S., Sprenger, C. (2012). Time Discounting Predicts Creditworthiness. *Psychological Science,* 23(1), 56-58.

Molden, D.C., Hui, C.M., Scholer, A.A., Meier, B.P., Noreen, E.E., D'Agostino, P.R., Martin, V. (2012). Motivational versus Metabolic Effects of Carbohydrates on Self-Control. *Psychological Science,* 23(10), 1137-1144.

Mullainathan, S., Shafir, E. (2013). Scarcity: Why Having So Little Means So Much. *Time Books, Henry Holt & Company LLC,* New York.

Muraven, M.R. (1998). Mechanisms of self-control failure: Motivation and limited resources. Doctoral dissertation, Case Western Reserve University, Cleveland, Ohio

Muraven, M.R., Baumeister, R.F. (2000). Self-regulation and Depletion of Limited Resources: Does self-control resemble a muscle? *Psychological Bulletin,* 126(2), 247-259.

Muraven, M.R, Collins, R. L., Neinhaus, K. (2002). Self-control and alcohol restraint: An initial application of the self-control strength model. *Psychology of Addictive Behaviors*, 16(12), 113–120.

Muraven, M.R., Tice, D. M., Baumeister, R. F. (1998). Self-control as a limited resource: Regulatory depletion patterns. *Journal of Personality and Social Psychology,* 74(3), 774-789.

O'Donoghue, T., Rabin, M. (1999). Doing It Now or Later? *American Economic Review,* 89(1), 103-124.

Posner, M.I. (1980). Orienting of attention. *Quarterly Journal of Experimental Psychology* 32 (1), 3–25.

Posner, M.I. (2016) Orienting of attention: Then and now. *Quarterly Journal of Experimental Psychology*, 69:10, 1864-1875.

Rabin, M., Weizsacker, G. (2009). Narrow Bracketing and Dominated Choices. *American Economic Review*, 99(4), 1508-1543.

Radu, P.T., McClure, S. M. (2013). Neuroeconomics and Addiction: Integrating Neuroscience, Cognition, and Behavioral Economics. *Biological Research on Addiction*, 2, 413-423.

Ramdas, K**.,** Saleh, K., Stern, D., Liu, H. (2014) Variety and Experience: Learning and Forgetting in the Use of Surgical Devices. unpublished paper, London Business School.

Read, D., Loewenstein, G., Rabin, M. (1999). Choice bracketing. *Journal of Risk and Uncertainty*, 19, 171-197.

Rozin, P., Reff, D., Mark, M., & Schull, J. (1984). Conditioned opponent responses in human tolerance to caffeine. *Bulletin of the Psychonomic Society*, 22(2), 117-120.

Rustichini, A., DeYoung, C.G., Anderson, J., Burks, S.V. (2016). Toward the Integration of Personality Theory and Decision Theory in Explaning Economic Behavior: An Experimental Investigation. *Journal of Behavioral and Experimental Economics*, forthcoming.

Sanders, M.A., Shirk, S.D., Burgin, C.J., Martin, L.L. (2012). The Gargle Effect: Rinsing the Mouth With Glucose Enhances Self-control. *Psychological Science*, 23(12), 1470-1472.

Shamosh, N.A., Gray J.R. (2008). Delay discounting and intelligence: A meta-analysis. *Intelligence*, 36(4), 289-305.

Silverman, I. W. (2003). Gender differences in delay of gratification: A meta-analysis. *Sex roles*, 49(9-10), 451-463.

Steinberg, L., Graham, S., O'Brien, L., Woolard, J., Cauffman, E., Banich, M. (2009). Age differences in future orientation and delay discounting. *Child development*, 80(1), 28-44.

Stanovich, K. E., West, R. F. (2000). Individual differences in reasoning: Implications for the rationality debate. *Behavioral and Brain Sciences*, 23(5), 645-665.

Striepe, B. (2016). Why You Should Never Make Decisions When You're Hungry. *Care2.com*, May 24.

Stroop, J.R. (1935). Studies of Interference in Serial Verbal Reactions. *Journal of Experimental Psychology*, 18 (6), 643-662.

Stucke, T. S., Baumeister, R. F. (2006). Ego depletion and aggressive behavior: Is the inhibition of aggression a limited resource? *European Journal of Social Psychology*, 36(1), 1–13.

Testa, M., Fillmore, M.T., Norris, J., Abbey, A., Curtin, J.J., Leonard, K.E., Mariano, K.A., Thomas, M.C., Nomensen, K.J., George, W.H., Van Zile-Tamsen, C., Livingston, J.A., Saenz, C., Buck, P.O., Zawacki, T., Parkhill, M.R., Jacques, A.J., Hayman, L.W. Jr. (2006). Understanding Alcohol Expectancy Effects: Revisiting the Placebo Condition. *Alcoholism Clinical & Experimental Research*, 30(2), 339-348.

Tversky, A., Kahneman, D. (1973). Availability: A heuristic for judging frequency and probability. *Cognitive Psychology*, 5(2), 207–232.

Vohs, K. D., Heatherton, T. F. (2000). Self-regulatory failure: A resource-depletion approach. *Psychological Science*, 11(3), 249–254.

Wang, X.T., Dvorak, R.D. (2010). Sweet future: Fluctuating Blood Glucose Levels Affect Future Discounting. *Psychological Science*, 21(2), 183-188.

Williams, G. (2016). How to Make Good Financial Decisions. *U.S. News and World Report*, July 12**.**

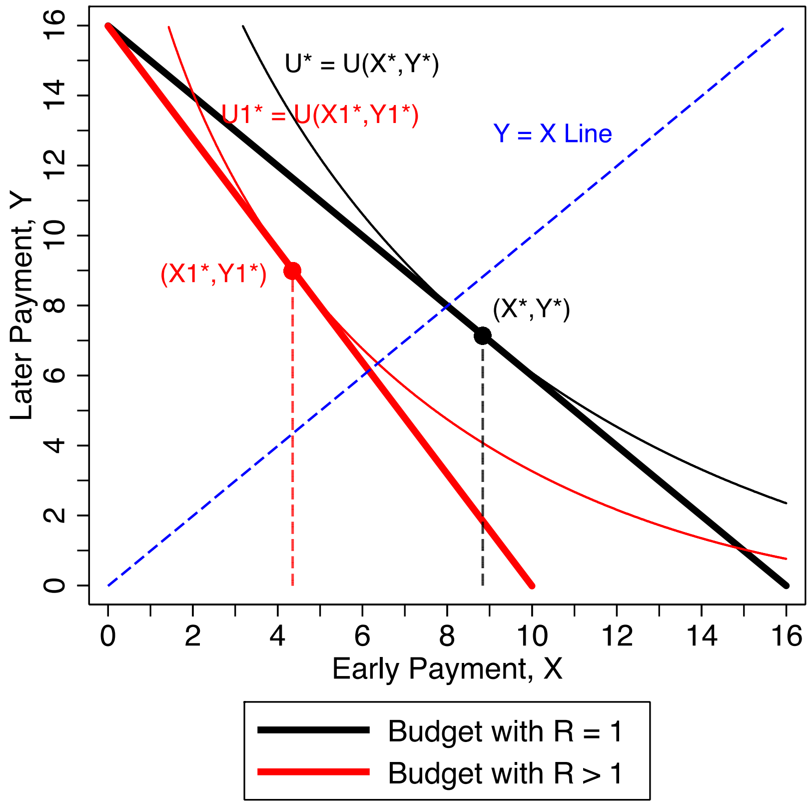


Figure 1: Predicted System 1 Behavior

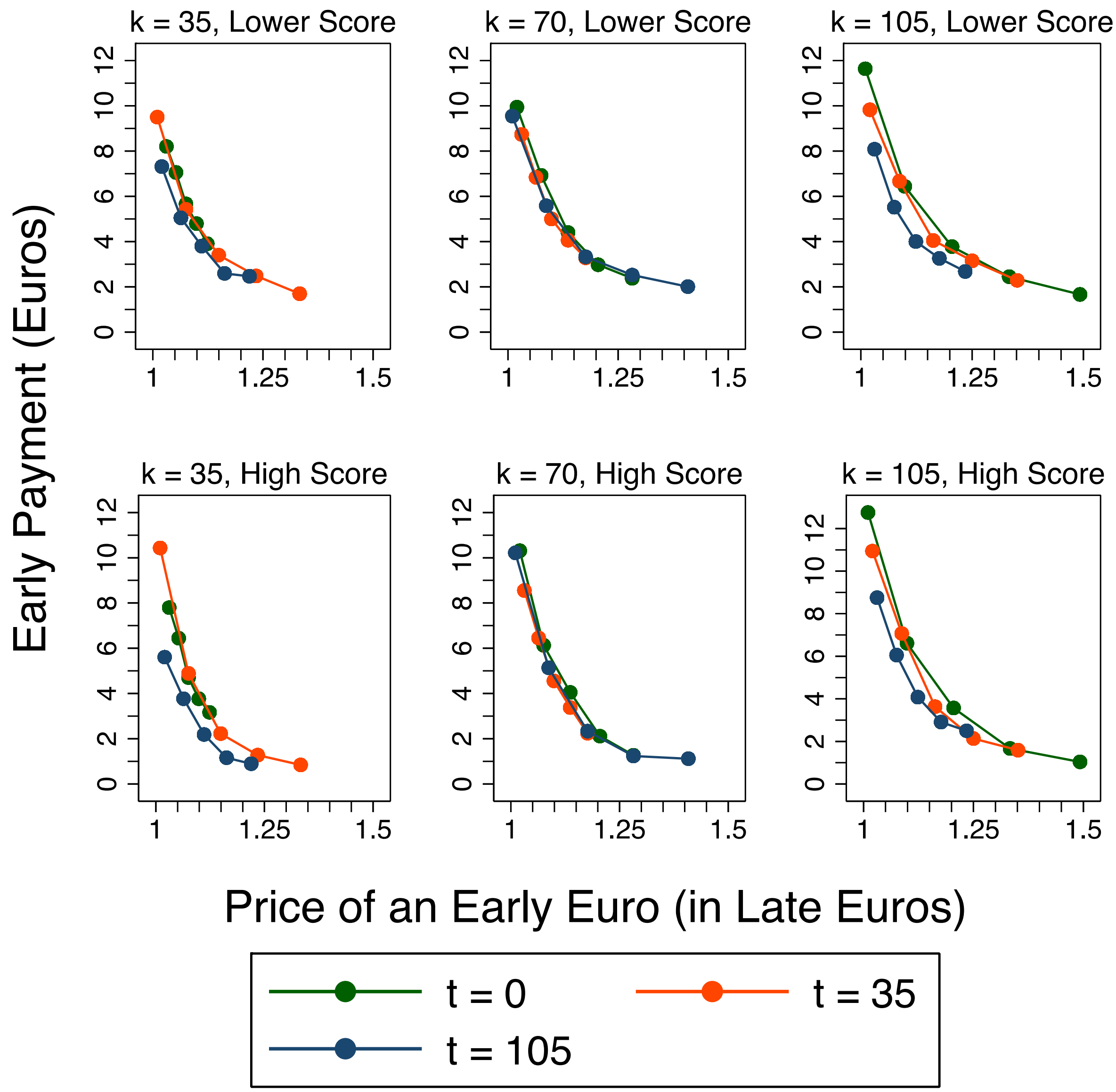


Figure 2: Demand Functions for Early Payment in Data

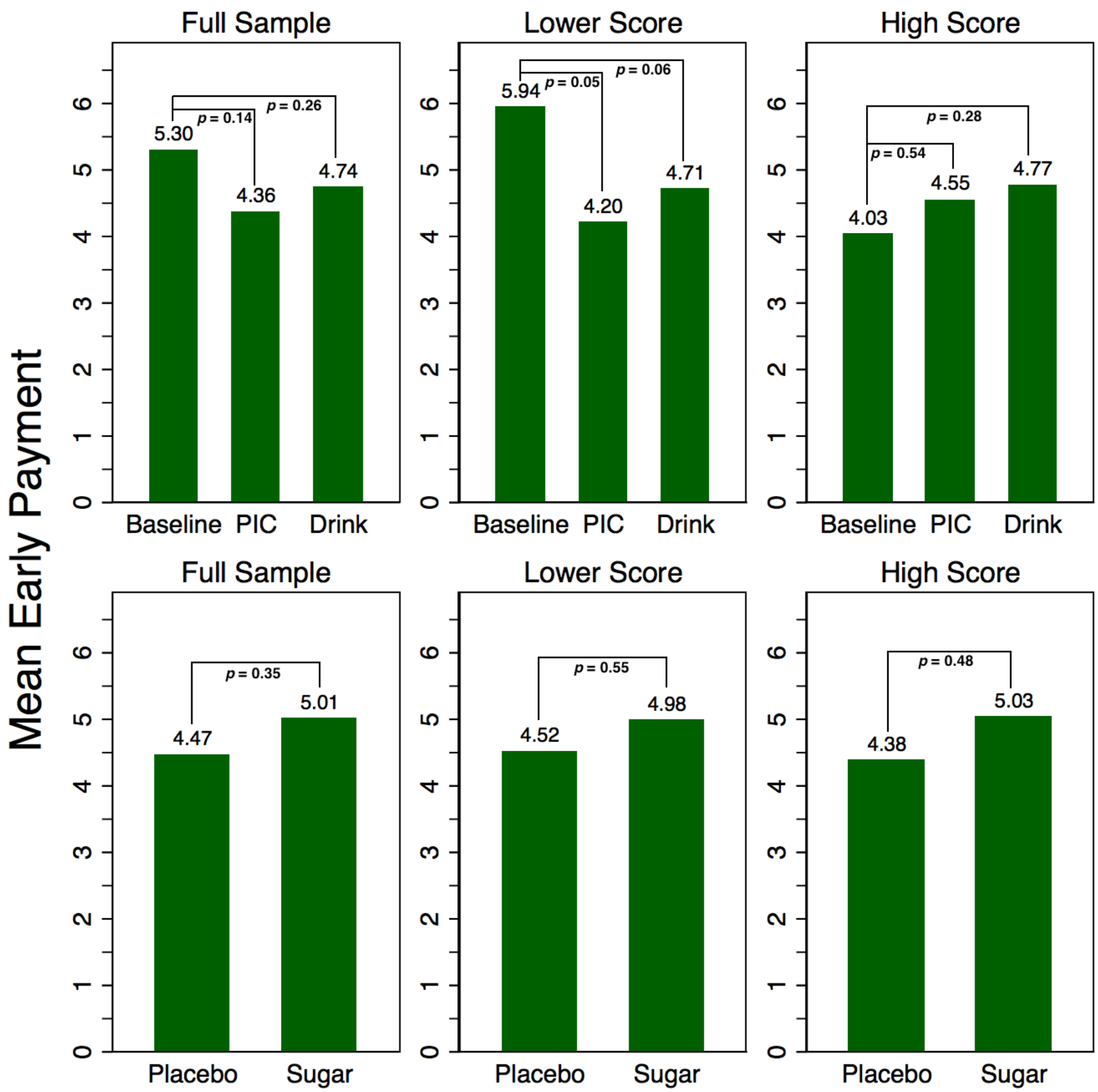


Figure 3: Demand for Early Payment by Treatment and Test Score

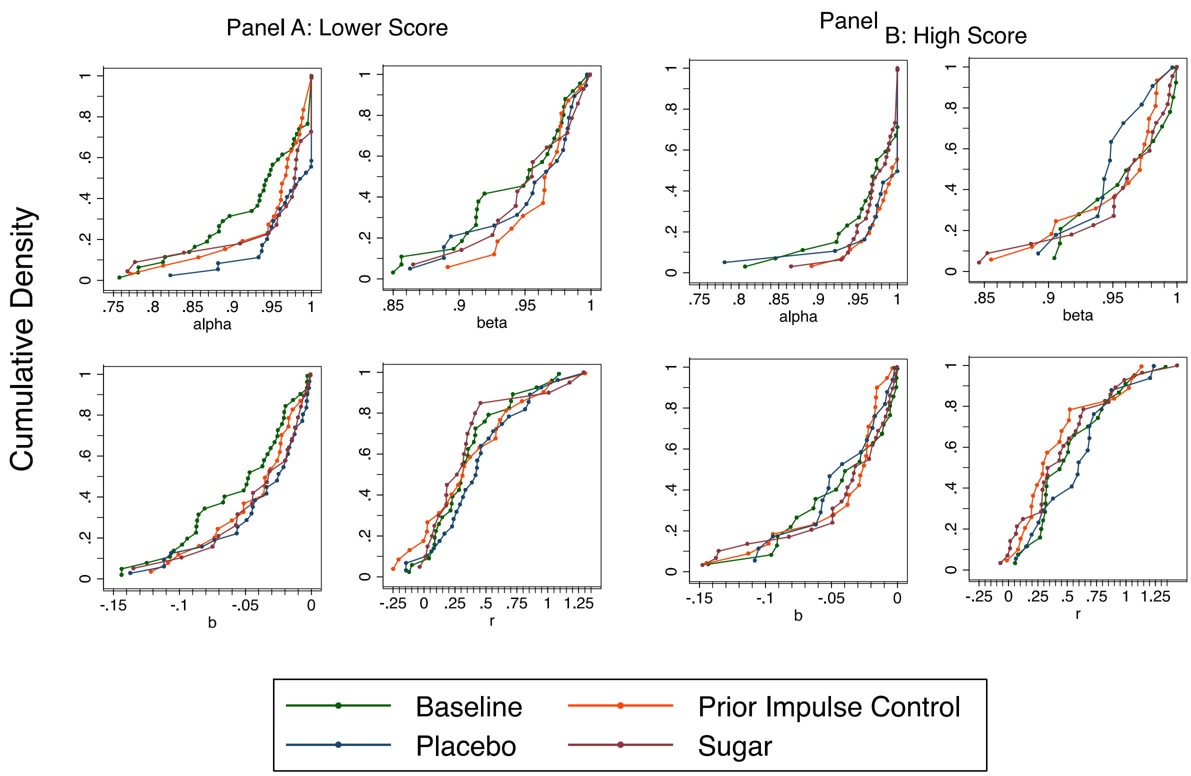


Figure 4: Distributions of Parameter Estimates

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

Table 2: Predicted Effects of Treatments on Estimated Parameter Values

	Parameter		
Treatment Type	α	β	δ
A. Willpower-increasing treatment:			
Within System 1	0	+ (towards one)	+ (towards one)
Within System 2	0	0	0
B. Treatment that moves decision-making towards System 2:	+ (towards one)	+ (towards one)	No prediction

Note: In the resource-based willpower literature, Sugar consumption is argued to increase willpower and Prior Impulse Control (PIC) is argued to reduce it.

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

	All Subjects	***Lower-Score***	***High-Score***
	(1)	(2)	(3)
Constant (t = 0, R = 1.01)	11.040	10.705	11.526
	(0.357)	(0.466)	(0.557)
1(t = 5 weeks)	-0.324*	-0.409*	-0.199
	(0.173)	(0.219)	(0.280)
1(t = 15 weeks)	-1.005***	-1.022***	-0.980**
	(0.239)	(0.300)	(0.395)
Clusters	270	160	110
Observations	12,150	7200	4950

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

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

Table 4: Early Payment Demand by Treatment and Test Score

	All Subjects		***Lower-Score***		***High-Score***	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Panel A: OLS						
PIC	0.938	0.938	-1.734**	-1.734**	0.521	0.521
	(0.632)	(0.632)	(0.865)	(0.865)	(0.852)	(0.852)
Drink	-0.565	-0.833	-1.226*	-1.422**	0.739	0.349
	(0.500)	(0.562)	(0.657)	(0.710)	(0.676)	(0.863)
Sugar		0.540		0.465		0.655
		(0.574)		(0.771)		(0.915)
Constant	5.301	5.301	5.937	5.937	4.030	4.030
	(0.409)	(0.409)	(0.541)	(0.541)	(0.503)	(0.503)
Panel B: Tobit						
PIC	-4.171*	-4.168*	-5.592**	-5.590**	0.541	0.541
	(2.391)	(2.390)	(2.819)	(2.818)	(4.439)	(4.435)
Drink	-2.552	-3.784*	-4.095**	-4.790**	2.516	-0.384
	(1.828)	(2.156)	(2.091)	(2.334)	(3.469)	(4.587)
Sugar		2.423		1.616		4.693
		(2.182)		(2.510)		(4.602)
Constant	1.202	1.205	3.859	3.860	-6.244	-6.233
	(1.516)	(1.515)	(1.635)	(1.634)	(3.401)	(3.397)
Clusters	270	270	160	160	110	110
Observations	12,150	12,150	7200	7200	4950	4950

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

Notes: Standard Errors are in parentheses, clustered by individual. We have 45 observations (budgets) per cluster. In this Table we do not need to control for price dummies since prices are balanced across treatments.

Table 5: Quantile Treatment Effects on Individual Utility Parameter Estimates

	All Subjects			***Lower-Score***			***High-Score***		
Quantile:	***25th***	***50th***	***75th***	***25th***	***50th***	***75th***	***25th***	***50th***	***75th***
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: *α*									
Baseline	0.88	0.96	1.00	0.87	0.94	0.98	0.95	0.97	1.00
Level	(0.02)	(0.01)	(0.00)	(0.04)	(0.01)	(0.01)	(0.03)	(0.01)	(0.00)
PIC	0.07**	0.02**	0.00	0.08	0.02	0.01	0.02	0.02	0.00
	(0.03)	(0.01)	(0.00)	(0.07)	(0.02)	(0.01)	(0.04)	(0.01)	(0.00)
Drink	0.07**	0.03**	0.00	0.08	0.04**	0.02**	0.01	0.01	0.00
	(0.03)	(0.01)	(0.00)	(0.07)	(0.02)	(0.01)	(0.05)	(0.01)	(0.00)
Sugar	-0.01	-0.01	-0.00	-0.04	-0.00	-0.00	-0.02	-0.01	-0.00
	(0.03)	(0.01)	(0.00)	(0.08)	(0.02)	(0.01)	(0.04)	(0.01)	(0.00)
Observations	227	227	227	128	128	128	99	99	99
Panel B: *β*									
Baseline	0.91	0.98	1.03	0.91	0.97	1.03	0.95	1.00	1.03
Level	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
PIC	0.05***	-0.00	-0.02	0.05*	0.01	-0.01	-0.00	-0.02	-0.03
	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.04)	(0.04)	(0.02)	(0.02)
Drink	0.03*	0.00	-0.02	0.04*	0.02	-0.02	-0.01	-0.04**	-0.03
	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)
Sugar	-0.00	-0.00	-0.01	-0.01	-0.01	0.00	0.01	0.02	-0.01
	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.04)	(0.04)	(0.02)	(0.02)
Observations	206	206	206	117	117	117	89	89	89
Panel C: *b*									
Baseline	-0.10	-0.05	-0.02	-0.14	-0.07	-0.03	-0.08	-0.04	-0.01
Level	(0.02)	(0.01)	(0.00)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
PIC	0.03	0.02*	0.00	0.07*	0.03*	0.01	0.03	0.01	-0.01
	(0.03)	(0.01)	(0.01)	(0.04)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
Drink	0.04	0.02	0.01*	0.09**	0.04**	0.02**	0.02	-0.01	-0.01
	(0.03)	(0.01)	(0.01)	(0.04)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
Sugar	-0.02	-0.00	-0.00	-0.03	-0.02	-0.01	-0.00	0.01	0.01
	(0.03)	(0.01)	(0.01)	(0.05)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
Observations	206	206	206	117	117	117	89	89	89
Panel D: *r*									
Baseline	0.22	0.36	0.79	0.12	0.35	0.91	0.31	0.47	0.76
Level	(0.05)	(0.06)	(0.14)	(0.08)	(0.08)	(0.24)	(0.07)	(0.14)	(0.20)
PIC	-0.10	-0.05	-0.18	-0.10	-0.08	-0.33	-0.12	-0.14	0.14
	(0.08)	(0.10)	(0.22)	(0.13)	(0.13)	(0.39)	(0.11)	(0.21)	(0.29)
Drink	0.02	0.10	-0.07	0.12	0.08	-0.22	-0.06	0.12	-0.03
	(0.08)	(0.10)	(0.22)	(0.12)	(0.13)	(0.37)	(0.11)	(0.22)	(0.31)
Sugar	-0.12	-0.13	-0.09	-0.12	-0.12	-0.27	-0.13	-0.16	0.13
	(0.09)	(0.10)	(0.24)	(0.14)	(0.15)	(0.44)	(0.11)	(0.21)	(0.30)
Observations	204	204	204	114	114	114	90	90	90

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

Notes: Standard Errors in parentheses.

Appendix for Online Publication

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

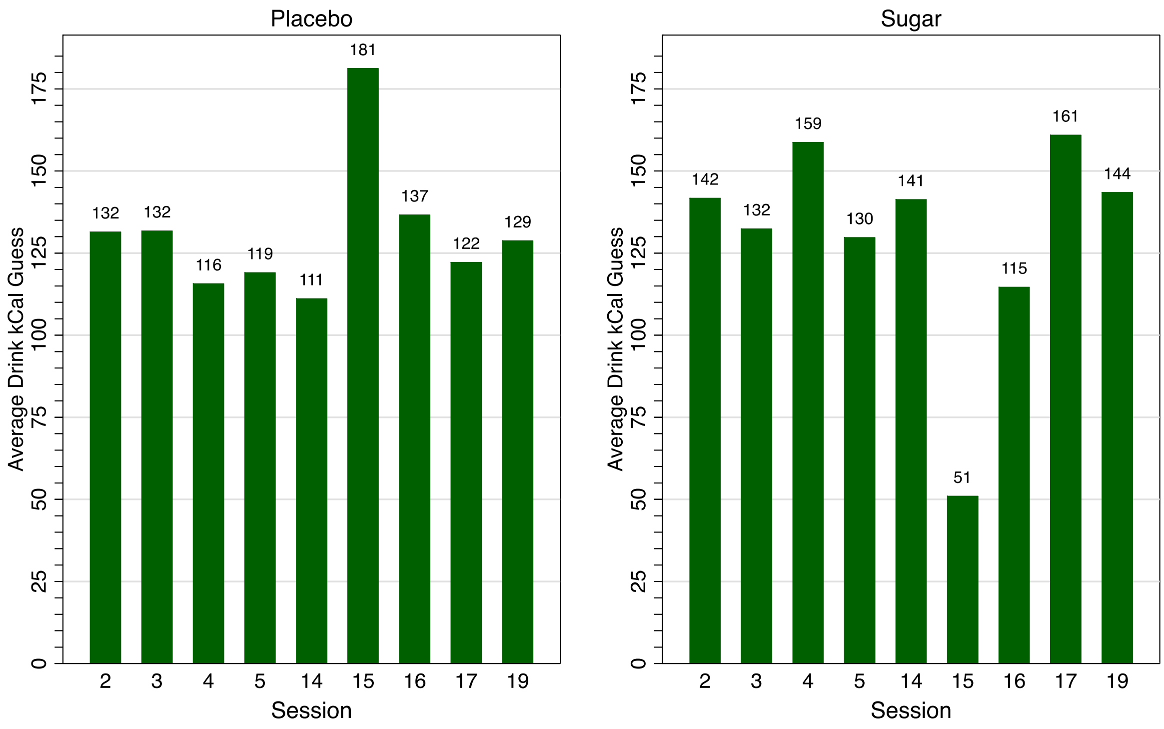


Figure A2: Drink Calorie Estimates by Session (Drink Sessions only)

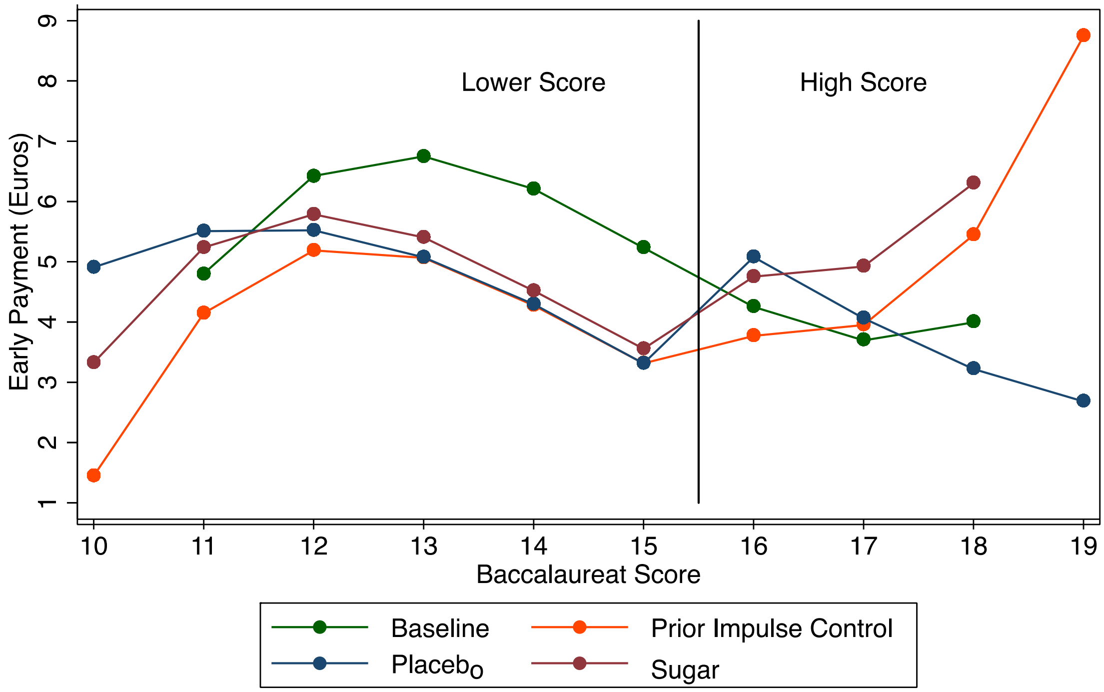


Figure A3: Predicted Values of Test-Score Cubic Model by Treatment

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 calculation assumes quarterly compounding.

Table A2: Treatment Effects on Demand for Early Payment by CRT Score

	CRT = 0	***CRT = 1***	***CRT < 2***	***CRT = 2***	***CRT = 3***	***CRT > 1***
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	6.249	5.641	6.047	3.882	5.165	4.441
(Baseline)	(0.659)	(0.642)	(0.489)	(0.715)	(1.161)	(0.652)
Prior Impulse	-1.897	-2.209**	-2.253***	0.646	0.261	0.354
Control	(1.351)	(1.072)	(0.854)	(1.029)	(1.768)	(924)
Drink	-2.231**	0.155	-1.139*	-0.223	-0.878	-0.458
	(0.941)	(0.896)	(0.688)	(1.284)	(1.307)	(0.887)
Sugar	1.578	-0.818	0.380	0.521	0.712	0.605
	(1.097)	(0.844)	(0.709)	(1.559)	(1.055)	(0.936)
Clusters	75	66	141	74	55	129
Observations	3375	2970	6345	3330	2475	5805

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

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

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

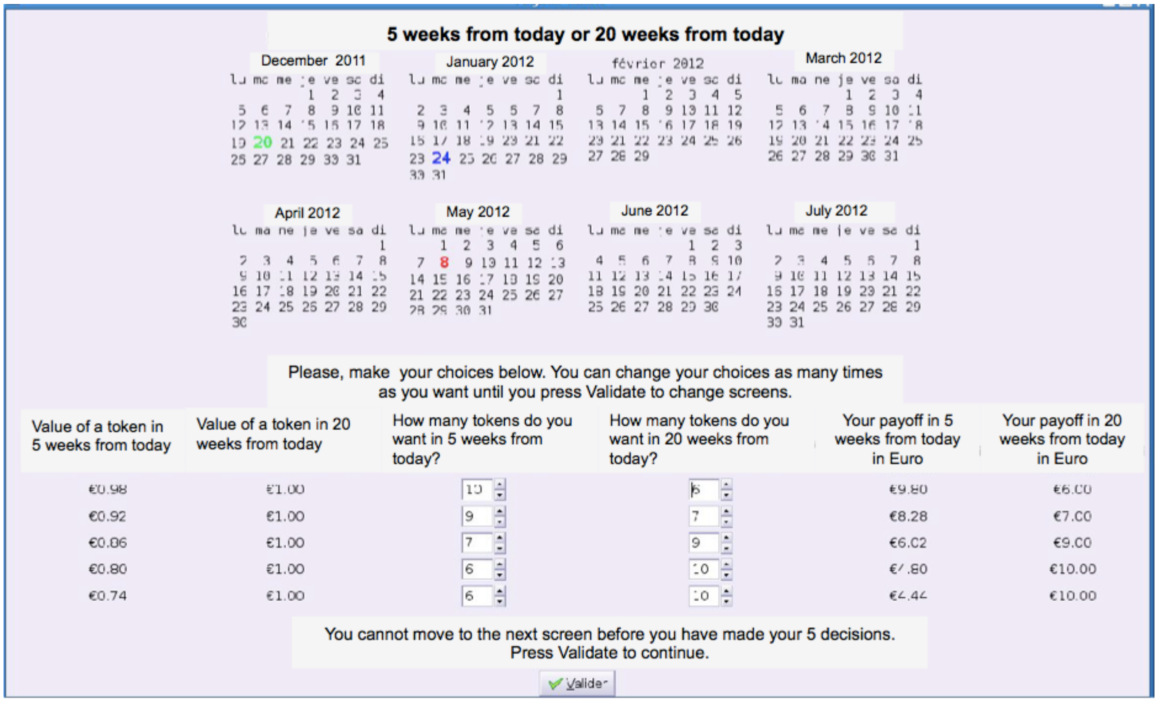
On each decision screen, we will provide you with the exact calendar dates of the above payments, so you know exactly which decision you are making. Today's date appears in green, the early payment date appears in blue and the late payment date appears in red. If the early date is today's date, only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

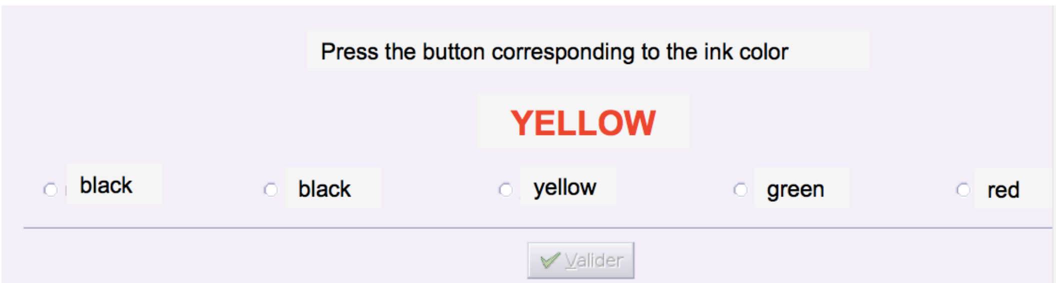
Remember that each decision could be the one that counts! Treat each decision as if it could be the one that determines your payment.

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 ».