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Do People Bundle Sequences of Choices? An Experimental Investigation

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Abstract. Economists and psychologists have sought to model and explain both impulsive behavior and the costly but often successful mechanisms by which people control it. Ainslie [1975][1992][2001] suggests that self-control is often achieved on account of a phenomenon he calls "choice bundling." This refers to re-framing of series of discrete choices as single choices over whole series. Whereas other core elements of Ainslie's account of self-regulation, such as hyperbolic discounting and intrapersonal bargaining among temporally distinguished selves have been subject to extensive modeling by economists, choice bundling has been absent from the economic literature because it has never been empirically isolated in a controlled setting that meets the methodological requirements of the discipline. We report a laboratory experiment that fills this gap. Subjects made choices between smaller, sooner and larger, later real monetary rewards under experimental treatments that allowed us to discriminate between choice bundling, reliance on pre-commitment, and possible magnitude effects on intertemporal discounting. Risk preference measures were used to obtain accurate discounting estimates, based on estimation of mixture models that incorporate exponential, hyperbolic and quasi-hyperbolic discounting functions. We use structural econometric procedures which are well established in the literature on binary choice and find strong support for the hypothesis that subjects bundled choices when conditions allowed them to do so, and consequently exhibited different discounting behavior in these conditions.

JEL codes: D03, D83, D90

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1. The Idea of Choice Bundling

According to familiar folk wisdom, at some present time t_1 people often claim to prefer that at future times $t_2, \ldots, t_n$ they will not drink alcohol to excess, make reckless impulse purchases, underinvest in their pensions, or procrastinate. Then, when temptations of these kinds arrive, they choose in ways that seem to conflict with these earlier reported preferences. No doubt many instances of such inconsistency can be explained away as cheap talk, but a large, partially connected, literature in economics and psychology rests on the assumption that many cannot: efforts to forestall procrastination, addiction, impulsive consumption, and simple thoughtless bad habits lead to real behavior with real opportunity costs. Furthermore, unsuccessful efforts of this kind, to mitigate this behavior, lead to apparent intertemporal preference inconsistencies that are challenging to rationalize in theoretical models. For example, as observed by Schelling [1980], smokers attempting to quit often destroy their cigarettes, only to go to the shop for more when their resolve collapses. The addiction literature is rife with similar examples of costly, unsuccessful attempts at commitment to future abstinence (Elster [1979][1997]). On the other hand, many people avoid habitual consumption of addictive substances, wait for investments to mature before harvesting them, save for rainy days and golden years, and meet their work deadlines.

A general theory of intertemporal choice, whether framed in terms of actions or in terms of revealed preferences, needs to accommodate these familiar facts. Consequently, temporally inconsistent preference dynamics, in which choices over immediate consumption imply preference structures in tension with preferences over the same choices arising further into the future, have been modeled by a number of economists. Examples include Becker and Murphy [1988], Akerlof [1991], O'Donoghue and Rabin [1999][2000][2001], Gul and Pesendorfer [2001], Bénabou and Tirole [2004], Benhabib and Bisin [2004], Bernheim and Rangel [2004], Fudenberg and Levine [2006][2012] and Heidhues and Koszegi [2009]. The feature common to all of these models, except the first,[1] is that they relax the standard assumption of exponential discounting. They differ from one another in the specific structural departures they propose, and in the psychological "stories" they suggest to render their models intuitively applicable to human behavior as it is encountered in daily life and observed in the behavioral laboratory. All of the models feature parameters that, on specific settings, yield standard exponential discounting with implied intertemporal preference consistency as a special case. In general the models do not seek to *explain* why some people reveal intertemporally consistent choices and others do not. Rather, they provide specifications that *characterize* this heterogeneity, and structures that can potentially be used to identify it in choice data.

Schelling [1978][1980][1984] is unusual among economists in offering reflections on the phenomenological[2] differences between succumbing to past resolutions, and resisting temptations to choose in contradiction to those past resolutions. Informally contemplating his experience as a reformed smoker who kicked the habit only following a struggle and a course of false starts, he described strategic subtleties involved in the self-management of motivations, decisions to adopt new personal policies, and responses to opportunities for exploiting loopholes in such policies. For example, a smoker might promise to pay fines levied by her friends and family if they catch her smoking, then arrange to make solitary trips out of town for no compelling purpose except to be able to smoke without fear of detection. Schelling [1978][1980][1984] suggests that such complications explain why most addicts achieve stable abstinence or controlled consumption only

[1] Becker and Murphy [1988] retain exponential discounting in their model of addiction, along with the assumption of utility maximization at each point of choice. The cost, as pointed out by various critics such as Ross, Sharp, Vuchinich and Spurrett [2008; §3.1], is that they inadvertently change the subject: their model explains habitual consumption rather than addiction. In habituation, past choice of an alternative increases the relative value of that alternative in subsequent choice sets. Addiction involves additional properties. First, due to preoccupation and/or physical dependence, the agent suffers costs if the habituated alternative is not chosen. Second, the addict incurs further costs in efforts to reduce expected future consumption of the addictive target. The second property rules out the possibility of a "contented addict." This follows the clinical literature in regarding addiction as inherently a disorder.

[2] Phenomenology is the name of the branch of philosophy that studies how mental states "feel" from (as it were) "the inside." For reasons explored by Lyons [1986] and Dennett [1991], we share widespread doubt among scientists and economists that phenomenological reports should be regarded as observations of data, though obviously the reports themselves are data. But we see no harm in letting such reports inspire hypotheses and conjectures.

following a series of failed attempts: they must learn from experience how to design personal policies that lack undermining loopholes. This pattern is well documented in the empirical clinical literature on addiction (Heyman [2009]). Such complexities cannot be directly modeled using only simple utility functions that rank or weight (smoking, abstinence) as outcomes and impose exponential intertemporal discounting.

Stepping outside of economics, ideas along the lines of those of Schelling [1978][1980][1984] have been pursued more systematically by Ainslie [1975][1992][2001]. From his perspective as a clinical psychiatrist and behavioral researcher, he distinguishes a range of strategies by which people commonly attempt to increase the likelihood that their future behavior will correspond to policies they lay down for themselves in the present. The strategy to which Ainslie has devoted the most attention is referred to as *reward bundling*. He presents this as a variety of mental framing, in which a person at time t_1 represents a binary choice as being drawn from a set of two extended sequences of rewards Rt_1, Rt_2, … Rt_k and Rt'_1, Rt'_2, … Rt'_k, rather than from a set of two discrete states of affairs Rt_i or Rt'_i that are independent of subsequent alternatives. For example, if a person faces a choice between going jogging in today's wind and rain or staying in bed for another hour, she might frame the choice as merely applying to her activities and experiences over the hour in question. Alternatively, the person might represent herself as choosing between being two different kinds of people, over a time frame that extends indefinitely into the future: is she the sort of person who jogs every day come what may, or the sort of person who jogs only when conditions are just right? If the person finds staying warm in bed to be a more pleasant prospect for the immediate hour than a cold, wet, jog, and nothing else is at stake, then she has no reason to choose the latter. But some people in such circumstances successfully exhort themselves to get up and brave the elements. Ainslie hypothesizes that the key to explanation, at least in many cases, is that the person may be motivated to believe that she will be resolute about maintaining fitness in the future. But that belief would be in tension with the belief she will have to adopt about herself if she stays in bed today, if she then chooses to view her decision today as the best available predictive evidence about how she will behave tomorrow or next week We might thus assign to the agent a standing utility function according to which she prefers to jog most of the time. Her choice today is then predicted by assigning to her a belief that she increases the probability of realizing this preference if she chooses to jog today because, she believes, today's decision has a (typically non-deterministic) causal influence on future choices.

Choosing between jogging now and sleeping in now is one sort of choice that people obviously make often when choices are vulnerable to procrastination. But more than a figure of speech is involved in saying that choosing between being generally conscientious about fitness and being generally lazy is *also* a sort of choice people really make, extended over time rather than at the moment of discrete choice. The point goes deeper than the familiar one that people might subjectively frame their choices in varying ways. Economic theory is flexible with respect to who the decision-making agent is (i.e., an individual, a household, a company, a country) and the horizons over which choices are made. No plausible general theoretical principle in economics requires an atomistic ontology of choices, according to which a *bundled* choice is always identical to a mere unstructured sequence of discrete choices. Applied to our example, an "event atomist"[3] would insist that the choice to "be the jogging type" is simply identical with the choice to jog at t_1 and the choice to jog at t_2 and, … the choice to jog at t_k. But such insistence would amount to forcing metaphysical dogma on economic modeling. The person's decision not to jog on her birthday is consistent with her decision to be the kind of person who jogs rather than sleeps in, weather notwithstanding. But reduction of the bundled choice event to the set of discrete choice events provides no formal basis for recognizing this consistency, relative to the quite different case where the person goes back to sleep after merely framing the thought "to hell with being virtuous."

[3] This concept has been rigorously studied by philosophers, such as Davidson [1980], but we will not detour here into these details.

So much for *a priori* philosophizing. Before we can decide how seriously we ought to take these reflections in modeling real behavior for designing economic experiments, we need to know something we hitherto do not: whether bundling choices has detectible and reproducible effects under carefully incentivized conditions.

In psychology, the earliest parametric experiments on incentivized choice were performed with animal subjects. Herrnstein [1961] conducted an experiment in which pigeons chose between two possible reward schedules. He found that their relative frequency of choice approximately equaled the relative frequency of reinforcement of each schedule. Herrnstein's finding is known as the *matching law*, because the relative rate of responding on a key *matches* the relative rate of reinforcement. Inspired by the original matching law, additional factors that affect the relative distribution of behavior were examined. Chung and Herrnstein [1967] studied how delay affects choices and found that the relative frequency of responses, again in pigeons, matches the relative immediacy of reinforcement. The introduction of delayed rewards confronted the researchers with the possible tension between animals' preference for larger rewards over smaller ones, on the one hand, and their preference for earlier rewards over later ones, on the other hand. Evidently these two choice tendencies are in conflict when subjects have to choose between smaller, sooner (SS) rewards and larger, later (LL) rewards. Ainslie [1974] pointed out that the matching law implies reversal of preference between LL and SS rewards as a function of delay, and speculated that this might create an incentive for pre-commitment to LL alternatives. This pre-commitment was observed using both differential rates of pecking by pigeons (Rachlin and Green [1972]) and single choices. Specifically, Ainslie [1974] found that pigeons are more likely to choose LL over SS when at a distance from both, if this choice blocked them from facing options to choose SS over LL when close to SS.

Ainslie [1975][1992][2001] understands choice bundling as a solution to problems of dynamic choice inconsistency that can arise when agents discount future rewards non-exponentially.

Thus, evidence for such discounting might feature in explanations of choice bundling, *if* there are in fact bundled choices to be observed in the first place. Put another way Ainslie's explanation gains some empirical support, though not decisive confirmation, if non-exponential discounting and choice bundling are observed to systematically co-occur. To date we have such evidence only for rats (Ainslie and Monterosso [2003]). Avowed evidence in the case of people is limited to two experiments: Kirby and Guastello [2001] and Hofmeyr, Ainslie, Charlton and Ross [2010]. However, both of these experiments suffered from crucial methodological limitations, sufficient to defeat any claim that bundled choice has been observed in people. We discuss them in comparison to our design in section 8.

The experiment we report here compares observations of choices embedded in series with choices not so embedded. We employ treatments that allow us to control for effects of immediacy or "viscerality" (Loewenstein [1996]), and varying reward magnitudes. Our analysis allows for subject heterogeneity as between exponential, hyperbolic (Mazur [1987], and quasi-hyperbolic (Phelps and Pollack [1968] and Laibson [1997]) discounting, and is tested against a Luce interpretation of behavioral error. In light of these methodological refinements, we report the first valid inference of reward bundling in humans from properly controlled observations.

Section 2 reviews models of discounting, section 3 explains our basic experimental design, section 4 documents experimental procedures, section 5 considers various analytical methods to apply to the data we collect, section 6 presents results, section 7 discusses the main findings, and section 8 compares previous experiments with humans. Section 9 concludes by discussing implications of this analysis and sketches of new research topics that open for study by economists if our findings inspire suggested expansion of modeling protocols to allow for choice bundling.

2. Models of Discounting

Ainslie [1992][2001] argues that choice bundling is the most common strategy by which people manage the consequences of hyperbolic discounting, which he takes to be the natural, baseline, structure of intertemporal discounting in animals, including humans. Analytically, the relationship between non-exponential discounting and choice bundling admits of two general interpretations, between which we wish to remain agnostic. We explain the basis for both alternative interpretations and our neutrality among them after reviewing the models of discounting we employ in analyzing our experimental data.

Several models of time preferences have been proposed by economists and psychologists to describe observed choice in SS/LL decisions. The discount factor D equates the present utility of a reward with its future value for a given time horizon τ: $U(x_t) = DU(x_{t+\tau})$, where $U(x_t)$ is the utility of the reward x if received at time t, and $U(x_{t+\tau})$ is the utility of the reward x if received at the later time $t+\tau$. The model is general in the sense that it does not assume any specific functional form for the instantaneous utility function U or the discount function D. Most models of time preference specify the discount factor D in a way that imposes costs for delaying the reward x from t to $t+\tau$. The rate of devaluation per time unit can be derived from the various specifications of the discount factor D, and is denoted by d. The general model allows for the special case where $U(\cdot)$ is linear. We consider four major models of time preferences, although many others have been proposed: see Andersen, Harrison, Lau and Rutström [2014b] for a detailed review.

The *exponential discounting* model is the standard representation of time preference. It was introduced by Samuelson [1937], although he did not propose it as an empirical hypothesis about any hypothesized latent structures. In the exponential model the discount factor is $D^E(\tau) = 1/(1+\delta)^\tau$, in discrete time units.[4] Like all discounting models, the exponential model incorporates

[4] The specification in continuous time shows why the model is called "exponential": $D^E(\tau)=\exp(-\delta\tau)$.

the assumption that time delay is costly: the utility of a reward received later is diminished compared to the same reward received sooner. However, exponential discounting assumes that decline in utility occurs at a constant rate $d = \delta$. Hence it may be viewed as assuming a constant variable cost of time delay and no fixed cost.

Strotz [1965] drew attention to a "preference reversal" pattern in which individuals might reverse SS/LL decisions made at time t as they approach time $t + \tau$, exhibiting less and less patience as time $t + \tau$ approaches. To allow for this pattern, Ainslie [1975] proposed that latent time discounting mechanisms in animals, including humans, are best described by the *hyperbolic discounting model.* Hyperbolic discounting still assumes variable costs of delay. However, unlike exponential discounting, hyperbolic discounting does not assume that variable costs of delay are constant. Ainslie proposed a non-parameterized specification of the discount factor D that describes this non-constant pattern $D^{H1}(\tau)=1/\tau$, which implies the discount rate $d^{H1}(\tau)=\tau^{(1/\tau)}-1$. Specifications of the hyperbolic model with parameters that allow for behavioral estimation have been suggested, such as the Mazur [1987] specification $D^{H2}(\tau)=1/(1+K\tau)$, for some parameter K, implying the discount rate $d^{H2}(\tau)=(1+K\tau)^{(1/\tau)}-1$. An agent whose time preferences are described by hyperbolic discounting will show declining discount rates with an increasing horizon. The closer a reward is in time, the steeper is the discounting gradient between its present and future value. Such preferences allow for preference reversals.

Another deviation from the exponential model is *quasi-hyperbolic discounting*. The model was originally introduced by Phelps and Pollack [1968] for inter-generational transfers, and adopted by Laibson [1997]for inter-temporal choices within individual people. It has sometimes been hypothesized as describing latent valuation structures driven by appetites aroused exclusively by immediate reward prospects that stimulate sense organs: see Loewenstein [1996] and McClure et al. [2004]. Economists more often use it as a more tractable stand-in for less convenient hyperbolic models, as in Harris and Laibson [2003]. The quasi-hyperbolic model, in discrete time units, is

$D^{QH}(\tau)=1$ at $\tau=0$ and $D^{QH}(\tau)=\beta/(1+\delta)^{\tau}$ for $\tau>0$. The quasi-hyperbolic model has a jump-discontinuity in the discount factor at $\tau=0$ and behaves thereafter like the exponential model. The model implies the discount rate $d^{QH}(\tau)=[\beta/(1+\delta)^{\tau}]^{(-1/\tau)}-1$. The parameter β introduces a fixed cost component to time delay that is absent in exponential and hyperbolic discounting. If the fixed cost parameter $\beta > 0$ takes a value strictly less than 1 it reduces the utility of the reward by a fixed *proportion* of the utility of the principal, as soon as any delay occurs.[5] After this initial cost of time delay wears off, the discount rate converges towards δ.[6]

The final discounting model we consider views agents as perceiving time in a distorted way. This perception is comparable to probability weighting in decision making under risk: in an intertemporal context, agents are modeled as "speeding up" or "slowing down" their perception of time horizons. One specification is due to Read [2001], based on the Weibull distribution: $D^{W}(\tau)=\exp(-\delta\tau^{(1/s)})$, for $\delta > 0$ and $s > 0$. The parameter s is responsible for the "speeding up" or "slowing down" of perceived time horizons, so that when $s < 1$ perceived horizons $\tau > 1$ are longer than the actual horizon, and vice versa when $s > 1$.[7] The perceived time is then discounted exponentially, and hence this Weibull specification collapses to the exponential model when $s = 1$.

These discounting models are all understood, at least by economists, as models of aspects of preference. Most economists understand preferences as revealed by choices. This Revealed Preference Theory (RPT) understanding admits of two general interpretations, which Ross [2014]

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5 An alternative specification of the fixed cost, due to Benhabib, Bisin and Schotter [2010], is directly in terms of some reward level x* that must be received to compensate for any time delay. This specification has significantly different predictions to the quasi-hyperbolic model: as the stakes increase, the effect of the fixed cost of time delay becomes smaller and smaller, whereas they stay the same with the quasi-hyperbolic model since they are a fixed *proportion* of the principal in that model.

6 In empirical work it is possible to see estimates in which $\beta > 1$, corresponding to a setting in which the experimenter is offering subjects a cheaper savings or commitment device than the field counterpart. The conventional *a priori* expectation, absent these considerations, is that $\beta \leq 1$.

7 This specification is sensitive to the units with which τ is measured, since these effects are reversed if $\tau < 1$. We always specify discrete time intervals in which $\tau = 1$ is the smallest time interval considered.

labels "mathematical" and "empirical" RPT respectively.[8] According to mathematical RPT, defined by the axioms of Weak or Strong RPT (Houthakker [1950]), preferences are extensionally identical with choices in the temporal limit. This renders it analytically impossible to represent some choices as involving errors, which in turn makes it difficult to coherently represent preference reversals of the kind made possible by non-exponential discounting as constituting problems that agents might be motivated to circumvent.

In the face of this difficulty we might appeal to empirical RPT, where the conditions for construction of ordinal utility functions are given by the axioms of Generalized RPT (Afriat [1967]) and applied so as to understand preferences as rationalized descriptions of *patterns* of choices that can include errors. On this interpretation of the relationship between preferences and choices, we would say that choice bundling can *give rise to* exponential discounting.

On the other hand, most psychologists, including Ainslie [1992][2001], along with some economists who subscribe to a revisionist interpretation of behavioral economics, understand preferences as latent states. On this interpretation, choice bundling is the most empirically important of several possible mechanisms for choosing consistently over time *despite* the hypothesized persistence of latent non-exponential discounting. This latent discounting structure is cited to explain why similar agents facing similar decisions who do not bundle choices are more likely to choose inconsistently over time. This understanding of discounting as separable from observed choice has motivated efforts to identify discounting structures on the basis of non-behavioral evidence. McClure et al. [2004] and Glimcher et al. [2007] interpret their respective neuroimaging experiments as providing such evidence,[9] but do not attempt to link it to other evidence, behavioral or otherwise, of choice bundling.

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[8] Hands [2013] provides a deep and reflective survey of the broader research program of RPT.

[9] The two sets of researchers offer divergent interpretations of the neural response patterns they view as encoding non-exponential discounting in subjects' brains.

On *both* the psychologists' understanding and the empirical revealed preference theorist's understanding, we should expect to observe correlation between dispositions to bundle choices and evidence of non-exponential discounting in un-bundled choices. Thus testing for such correlations in suitably controlled conditions entails a general hypothesis that is neutral as between the psychologists' and the revealed preference theorists' interpretations of the conjectured relationship between choice bundling and intertemporal discounting. Observation of the hypothesized correlation would be compatible with the possibility that some or all people sometimes bundle choices for reasons unrelated to intertemporal discounting. Such observation would, however, provide motivation for facing the more challenging design task of trying to identify causal relationships between choice bundling and discounting dispositions, of the kind characterized by Ainslie [1992][2001] on the basis of his phenomenological reflections. Our investigation is conducted in the spirit of the reminder that experiments make it possible "to range beyond the confines of current theory to establish empirical regularities which can enable theorists to see in advance what are the difficult problems on which it is worth their while to look" (Smith [1994; p.114]).

3. Experimental Design

Our experiment consisted of eight treatment conditions, each conducted in a separate laboratory session. Four of the treatments served as control treatments for specific inferences, and allow us to account for the possibility of both non-exponential discounting and the magnitude effect. The other four treatments served as our bundling treatments, and presented different types of bundled choices. The experimental design is summarized in Table 1 and more fully explained below.

In each treatment subjects made many choices between SS or LL amounts of money. The task presentation followed a multiple-price list (MPL) format for the elicitation of time preferences, introduced by Coller and Williams [1999]. Each subject faced ten decision screens, and each decision screen comprised seven decision rows. Hence each subject made 70 binary decisions in total. An appendix documents all parameters of the choice battery.

Treatment FED0 was the initial control, and all other treatments were variations of this treatment. The SS amounts presented to subjects were $10, $30, $60, $100 and $300. The SS amounts were to be paid on the day of the experimental session, so there was a zero front-end delay. The LL amounts were to be paid 28 days after the session. The LL amounts were calculated by applying an annual interest rate from the day of the SS payout to the day of the LL payout. The annual interest rates ranged from 5 percent to 500 percent, and were not displayed to subjects. A typical decision screen from FED0 is presented in Figure 1.

The SS amount, $100 in Figure 1, remains constant across all decision rows of a given decision screen. The LL amount increases with each decision row within a given decision screen, since a higher interest rate was applied to each subsequent row. The payout dates are highlighted and the current date is circled on the calendar at the top of the decision screen in order to provide a visual aid. In each decision row the subject stated a preference for the SS or LL option. Each choice was made by clicking a button under the preferred option, resulting in the preferred option being highlighted. The hypothetical subject in Figure 1 chose the SS option in the first four decision rows and the LL option from row 5 onward.

Because hyperbolic and quasi-hyperbolic discounting specifications predict that discount rates decline with the time horizon, we included additional control treatments to allow for this possibility. By varying the front-end delay (FED), we are able to estimate structural forms of non-exponential discounting models following Anderson, Harrison, Lau and Rutström [2014b]. Treatment FED1 introduced a front-end delay of one day to all choices. That is, any SS payment would take place on the *day after* the experimental session and any LL payment would be made *29 days after* the session. Treatment FED35 introduced an even longer front-end delay of 35 days, yielding SS payments 35 days after the session and LL payments 63 days after the session. Figure 2 exhibits a typical decision screen from treatment FED35. In Figure 2 the day of the decision is again circled on the calendar, and the two dates of delayed payment are shaded orange, to make the SS/LL tradeoff and the FED as transparent as possible. In both the FED1 and FED35 treatments, the monetary amounts and the span of 28 days between the SS and LL dates remained the same as in the FED0 treatment.

Another theoretical consideration addressed by the design is the hypothesized *magnitude effect*. This effect refers to a behavioral regularity commonly reported in the psychological literature that humans tend to exhibit lower implied discount rates when payoffs are larger. The empirical evidence supporting the magnitude effect is not as convincing as many believe, as documented by Andersen, Harrison, Lau and Rutström [2013]. Nonetheless, we evaluate the potential impact of this confound by varying the principal significantly within each session. This provides a rich, within-subject control across all sessions and treatments, and which we can explicitly allow for in our structural analysis. As noted above, the principal was $10, $30, $60, $100 and $300 for different choices for each subject.

The treatments discussed thus far are necessary to allow us to control for the effects of varying FED and reward magnitude on the inferred discount rates. But they do not allow us to test for bundling behavior: they only establish a clean baseline. To test for bundling, we must present subjects with sequences of time-delayed choices.[10] The treatments that follow continue the use of ten decision screens with seven decision rows each, but introduce multiple SS/LL choices per decision row. These treatment conditions are, henceforth, collectively referred to as *bundling treatments*.

[10] These choices are over non-stochastic amounts of money. If they had been over risky lotteries, one would need to account for "correlation aversion," which is an interaction between risk preferences and time preferences that arises when one allows non-separable intertemporal utility functions. Andersen, Harrison, Lau and Rutström [2018] review the theory, and develop an experimental design to estimate correlation aversion. Many of the field applications of bundling do involve risky lotteries, of course, so this extension and complication is likely to be an important one for future research.

Treatment Free2 introduced a series of two decisions per row, with each atomic decision consisting of SS and LL options as before. Figure 3 shows a typical Free2 decision screen. In a given decision row, the two SS monetary amounts are identical and the two LL monetary amounts are identical, but the payout dates differ across the four options. As in treatment FED0, the earlier SS amount was to be paid on the day of the experimental session and the earlier LL amount was due 28 days later, and both are shaded as orange on the calendar. And as in treatment FED35, the later SS amount was to be paid 35 days after the experimental session and the later LL amount was due 63 days after the session, and both are shaded as blue on the calendar to differentiate them from the earlier SS/LL choice. As in all treatments, the horizon between the SS and LL dates within a given atomic choice was set constant at 28 days.

Treatment Free2 implements a *free choice condition*. Subjects were free to choose either the SS or LL option in each choice independent from the other choice made in the same row. For example, in Figure 3, we see in the first decision row that the hypothetical subject chose the SS option for the first choice in the sequence and then chose the LL option for the second choice in the sequence.

In contrast to the Free2 treatment, the Forced2 treatment imposed a *forced choice condition*. The Forced2 treatment is identical to the Free2 treatment except that all choices in a given row were forced to be either SS or LL uniformly. That is, if a subject selected the SS (LL) option for one of the choices in a row, then the SS (LL) option was automatically and simultaneously selected for the other choice in the same row.

Finally, two additional treatments are included in the experimental design to extend the possibility of choice bundling to a series of three decisions. Treatment Free3 is another *free choice condition*. It is equivalent to treatment Free2, with the addition of a third choice to each choice sequence. In treatment Free3 each choice row comprises three atomic choices: the first choice in the sequence is between an SS amount payable the day of the session and an LL amount payable 28 days later; the next choice is between an SS amount in 35 days versus an LL amount 63 days; and the last choice continues the established date pattern with the SS amount in 70 days and the LL amount in 98 days. Similarly, the Forced3 treatment introduced a *forced choice condition* that was otherwise identical to treatment Free3. Subjects in treatment Forced3 were forced to choose the same option for all three decisions within a given choice sequence. The choice of SS or LL could differ across decision rows but not within rows: we did not enforce single switch points. A typical screen for treatment Forced3 is presented in Figure 4.

4. Experimental Procedures

The sample consisted of 230 undergraduate students at Georgia State University (GSU) across eight sessions.[11] A participation payment of $5 was paid to all subjects, regardless of their choices in the decision task. The payouts from the choice task were determined independently for each subject. After completing the task and the subsequent demographic survey, each subject was approached by a research assistant. The subject then rolled a 10-sided die to select one of the ten decision *screens* from the task. After the screen was selected, the subject rolled the 10-sided die again, until a number between 1 and 7 came up, to determine one of the seven decision *rows* on the chosen decision screen. The subject's decision in the specified row was then considered for payout. Subject payments for the experiment totaled $17,802, with a per-subject average just over $71.[12]

[11] The participants were recruited by email invitation, after registering into the subject pool of the Experimental Economics Center at GSU and arrived at the laboratory without knowing the exact nature of the experiment. Each subject drew a random number and was seated at a computer station corresponding with the random draw. Detailed instructions, provided in an appendix, were handed to them to review before being read aloud. The subjects were invited to ask questions to clarify the decision task; in practice, very few questions were asked. After any questions had been answered the choice task began. Once the task was completed, the subjects were prompted to complete a demographic survey. The payout selection procedure concluded each experimental session.

[12] To limit extreme experimental costs an additional stochastic procedure determined whether large amounts would be paid out. In treatments FED0, FED1, and FED35, if the selected decision was less than $100, the amount would be paid for certain. If the selected option was $100 or more, the subjects were asked to roll the 10-sided die one more time. If the outcome was 1, the subjects would be paid the specified amount on the date indicated by their decision. If any other number came up on the roll, the subject would not be paid for the task. These procedures were explained to subjects in the instructions. Note that this procedure did not incentivize subjects to choose SS amounts for security, because no LL amounts over $100 had corresponding SS amounts under $100. The payment procedure for treatments Free2, Forced2, Free3, and

Only the participation payment of $5 was paid in cash, and at the end of the experimental session. All payouts from the decision task were paid using the online payment service *PayPal*. Even if a selected payment was due on the day of the experimental session, the amount was paid via *PayPal* later that day. This common payment procedure kept transaction costs constant across treatments and across SS and LL choices.

Whenever an initial payment was due, a subject received an email notification one day in advance. No such notification was sent when second or third payments were due, since we assumed that the subject had become sufficiently familiar with the payment procedure. The initiation of each transfer on the payment date led to another email notification, automatically sent by *PayPal*, which informed the subject that the amount was available and could be claimed.[13]

5. Analytical Methods

The analysis of the choice data adopts complementary descriptive and structural methods. These methods allow different intuition and insights.

The simplest descriptive technique we employ is the use of flexible polynomial regression displays to examine the "reduced form" effects of various covariates on the probability of choosing LL. Several regression models are used to relate choice characteristics to behavior, without

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Forced3 was similar. However, if the individual chosen options in the decision row selected for payment were less than $100, the entire choice sequence (i.e., all selected options in the decision row) would be paid with certainty to the subject on the specified dates. If the individual chosen options in the selected row were $100 or more, the subject would roll a 10-sided die again. If the outcome of the roll was 1, the subject was paid on the specified dates for all decisions made in this row. Otherwise, they would not be paid for the task.

[13] In a few rare cases a subject failed to claim her payment. In this situation, the subject again received an email notification that explained that she had failed to claim her payment and that *PayPal* would return the fund to the experimenters if the amount remained unclaimed after a period of 30 days. It was conveyed to these subjects that the money would be sent again for a second 30 day period. The *PayPal* transfers would be terminated only if the subjects failed to claim their money a second time. If this happened, the subject would have to contact the experimenter, using a previously given email address, to arrange payment. The vast majority of transfers went as planned. Only eight payments were left unclaimed by five different subjects, after the period for all payments had passed. One participant left three payments unclaimed, another left two payments unclaimed, and three participants left one payment unclaimed.

incorporating theoretical assumptions about how exactly decisions are made. Hence these models and displays are viewed as *descriptive*.

The most informative econometric approach we adopt is the direct estimation by maximum likelihood of structural models of the data generating processes underlying observed behavior. This approach allows us to identify the processes at work in a way that makes conceptual sense, and informs our thinking about those processes rather than simply describing them. The methods are familiar and well-established, so we summarize here; an appendix provides details.

There are three key features of our structural approach.

First, we control for diminishing marginal utility in our inferences about time preferences. It follows from Jensen's Inequality that estimating a more concave curvature of the instantaneous utility function results in lower inferred discount rates for the same observed choice data. As our subjects were drawn from a pool in which we have measured distributions of risk preferences, based on choices amongst non-hypothetical lotteries on numerous occasions, we based these estimates on previous data from this pool.[14] Following Andersen, Harrison, Lau and Rutström [2014b], we estimated risk preferences assuming that choice patterns conformed more closely either to Expected Utility Theory (EUT) or Rank-Dependent Utility (RDU) due to Quiggin [1982] using a Prelec [1998] two-parameter probability weighting function. In each case all we need to infer from the model of risk preferences is an estimate of U'', the second-order derivative with respect to experimental income, whether or not this comes from an EUT or RDU model. Our estimates of the utility functions are joint with our estimates of the discounting functions, following Andersen, Harrison, Lau and Rutström [2008].

[14] See Coller, Harrison and Rutström [2012] for a full discussion of this methodology. The risk preference data were collected from a previous sample of 171 drawn from the same population.

Second, we allow for a range of discounting functions, including exponential, hyperbolic, quasi-hyperbolic, and Weibull, following Andersen, Harrison, Lau and Rutström [2014b]. We are therefore not tied to a particular specification of the alternative to exponential discounting.

Finally, following Coller, Harrison and Rutström [2012], Schuhr [2016] and Harrison, Hofmeyr, Ross and Swarthout [2018], we employ mixture models to characterize heterogeneity of decision-making processes with respect to a fraction of choices to be characterized by Exponential discounting and a fraction characterized by one of the specified non-exponential forms.

6. Results

6.1 Descriptive Results

The first local polynomial regression, which concentrates on the choices from treatments FED0, FED1 and FED35, is presented in Figure 5. For each magnitude of the principal it shows how the fraction of LL choices varies with nominal annualized interest rate. The horizontal line at the 50 percent mark indicates where the observed choices are evenly split between SS and LL. Figure 5 illustrates that the fraction of LL choices increases with the nominal annual interest rate. One can also observe the general tendency that the fraction of LL choices increases with the magnitude of the principal. That is, the observed behavior *appears* to be consistent with a "magnitude effect," since more subjects choose LL when the magnitude of SS is higher. Only for principals of $60 and $100 can we see some deviations from this general tendency.

Figure 6 displays the fraction of LL choices across the three FED treatments and five principals, allowing one to see the interaction between the FED and principals. We observe a general tendency that the fraction of LL choices increases with the FED; more subjects choose LL when the delay of SS increases from no delay (FED0) to 1 day (FED1) and 35 days (FED35). Exponential discounting predicts that the fraction of LL choices should be the same across the three control treatments for each principal, whereas hyperbolic and quasi-hyperbolic discounting predicts that the fraction of LL choices increases when the FED increases. The results are therefore suggestive of some support in favor of hyperbolic or quasi-hyperbolic discounting for the three control treatments across the five principals.

The next step is to investigate whether there are significant differences between the data from the three control treatments and the *free conditions* of the *bundling treatments,* Free2 and Free3. , These are the central treatments that allow us to test for the effects of bundling.

Figure 7 considers the similarity of the first and second choices in treatment Free2. There is no significant difference between the first and second choices for low and high principals, respectively: the first and second choice patterns are almost identical for each of the five principals. This result is consistent with a tendency towards more exponential discounting in these treatments.

A similar picture emerges for the first, second, and third choices in the other *free condition*, treatment Free3. Figure 8 suggests the absence of significant differences between first, second and third choices. From this we infer that discount rates in each of these choices are the same, although we cannot infer that they are consistent with exponential discounting. For that we need to model the discounting processes explicitly.

6.2 Structural Estimation Results

Discounting parameters must be estimated as conditional on risk parameters, since errors from estimates of the utility function propagate into discounting estimates. The structural analysis of the risk data applies a Constant Relative Risk Aversion (CRRA) specification. We estimate a single EUT model and three RDU models, employing a power probability weighting function, an inverse-S function, or the Prelec probability weighting function. Based on the statistical significance of the estimated parameters and the log-likelihood value, the RDU specification with Prelec probability weighting provides the best model of the choice data. Therefore, this model of risk preferences is used for the joint estimation of time preference models.

To assess the data against the exponential, hyperbolic, and quasi-hyperbolic discounting models, the initial focus will be on the control treatments FED0, FED1, and FED35.

The estimated annualized discount rate for the exponential model is the parameter δ, and is 187% with a 95% confidence interval between 136% and 239%.

The parameter K for the hyperbolic discounting model is estimated to be 1.16 with a 95% confidence interval between 0.94 and 1.37. The annualized discount rate implied by the hyperbolic discounting model declines from 217% with a horizon of 1 day to 47 % when the horizon is extended to 5 years.

The estimated value of the quasi-hyperbolic parameter β is 0.97 and significantly different from 1, with a 95 percent confidence interval between 0.95 and 0.99: a formal *t*-test rejects the null hypothesis of exponential discounting (two-sided *p*-value of 0.033). The quasi-hyperbolic δ is estimated to be 129% with a 95% confidence interval between 73% and 184%.

The Weibull discounting parameter s is estimated to be 1.33 with a 95% confidence interval between 0.96 and 1.70. A formal hypothesis test shows that s is *not* significantly different from 1 at the 5 percent level of significance (*p*-value of 0.076). Hence, we cannot reject the null hypothesis of exponential discounting from the perspective of the Weibull specification.

We consider next estimates of two mixture models using responses from the three control treatments (FED0, FED1 and FED35) and the four bundling treatments (Free2, Forced2, Free3 and Forced3). In each mixture model controls are included for the various principals used, allowing them to affect all parameters of the discounting functions.

The first model is a mixture of exponential and hyperbolic discounting, and the second model is a mixture of exponential and quasi-hyperbolic discounting. Pooling over all responses we find a roughly even split between the exponential and hyperbolic discounting functions. The estimated value of the exponential mixture probability is 0.53 with a standard error of 0.14, and we cannot reject the null hypothesis that it is equal to 0.5 (*p*-value of 0.808). The estimated annualized exponential discount rate is equal to 64%, and the annualized hyperbolic discount rate varies between 576% for the 1-day horizon and 60% for the 5-year horizon.

Turning to the mixture model incorporating exponential and quasi-hyperbolic discounting, we find that the exponential mixture probability is 0.586 with a standard error of 0.091, and we cannot reject the hypothesis that it is equal to 0.5 (*p*-value = 0.341). The annualized exponential discount rate is equal to 75%. We can reject the null hypothesis that $\beta=1$ for the quasi-hyperbolic model (*p*-value < 0.001), and the predicted annualized quasi-hyperbolic discount rate varies between 3,020% for the 7-day time horizon and 345% for the 5-year time horizon.

Table 2 reports the estimates of key parameters from the two mixture models with controls for bundling treatments and magnitude effects.[15] Panel A shows the results for the mixture model between exponential and hyperbolic discounting. We observe a significant effect of the bundling treatment on the mixture probability: the marginal effect of bundling on the exponential mixture probability is +44.2 percentage points with a *p*-value of 0.021. Subjects in non-bundled treatments have an exponential mixture probability of 29.5%, whereas the exponential mixture probability is 73.7% in bundled treatments. We also observe an increase in the estimated exponential and hyperbolic discount rates in the bundling treatment, but the marginal effects are not statistically significant at the 5% significance level.

The treatment variable *second* is an indicator for bundling treatments with two decision tasks in comparison to bundling treatments with three decision tasks, which is the default in the statistical model. The marginal effect of having two decision tasks, compared to three, on the exponential mixture probability is negative and significant with a *p*-value of 0.013. Hence, the exponential

[15] An appendix contains complete estimates, including estimates for the magnitude effects. For present purposes these are "nuisance parameters" and are simply included to control for possible magnitude effects.

mixture probability is significantly *higher* when *three* decision tasks are bundled instead of two decision tasks. We also observe a significant marginal effect of the *forced* treatment variable on the exponential mixture probability and in the direction one would expect: subjects are constrained in the *forced* treatment to make the same choices across the (two or three) bundled decision tasks, which rules out non-exponential discounting.[16]

Panel B in Table 2 shows the results for the mixture model between exponential and quasi-hyperbolic discounting. There is again a significant effect of the bundling treatment on the mixture probability: the marginal effect on the exponential mixture probability is +42.9 percentage points with a *p*-value of 0.035. Subjects in non-bundled treatments have an exponential mixture probability of 29.2%, and the exponential mixture probability is 73.6% in bundled treatments. We also observe positive marginal effects of the bundling treatment on the estimated parameters in the two discounting functions, although none of the marginal effects are significant at conventional significance levels. The marginal effect of *second* on the exponential mixture probability is -23.6 percentage points and statistically significant with a *p*-value of 0.017.

The main conclusion from Panels A and B of Table 2 is that allowing bundling leads to dramatic increases in the fraction of choices better characterized by exponential discounting. Moreover, these findings are robust to the use of hyperbolic or quasi-hyperbolic discounting models as the alternative to exponential discounting.

[16] Each subject was presented with five magnitude treatments, embodied in the SS principal: $10, $30, $60, $100 and $300. These are within session treatments, and they are used as control variables in the equations for exponential and hyperbolic discounting as well as the error term. In the detailed estimates in an appendix, we observe magnitude effects on both exponential and hyperbolic discount rates: subjects have lower discount rates for higher magnitudes. The marginal effects of the magnitude treatments on the hyperbolic discounting parameter are large and statistically significant, whereas the marginal effects of magnitude on the exponential discounting parameter are smaller and not statistically significant.

7. Discussion

Descriptive analyses of our experimental data indicate that our subjects' choice behavior was consistent with hypothesized choice bundling. This interpretation is more systematically and rigorously supported by our structural model estimation. The evidence is comparably strong for both mixture models we consider, one that mixes exponential and hyperbolic discounting models, and another that mixes exponential and quasi-hyperbolic discounting models. The positive effect of multiple SS/LL pairs on the probability of exponential discounting in both models is highly significant and robust across a range of treatments motivated by previous theoretical literature.

Discussions of discounting and bundling in the psychological literature inspired by the work of Ainslie [1975][1992][2001] have almost invariably presupposed that people are naturally non-exponential discounters, and conjectures about bundling in this literature are conditional on this assumption. The theoretical background against which we structured our design and interpret our results is quite different. The most direct empirical evidence available, from Andersen, Harrison, Lau and Rutström [2013][2014b], suggests that *either* people latently hyperbolically discount but usually choose in a way that overcomes this preference[17] *or* often discount exponentially, at least where monetary rewards are concerned. Our experiment involves no *a priori* assumptions about frequencies of different time preference structures in our subject population. Of course, if "hyperbolicky" (hyperbolic or quasi-hyperbolic) discounting were absent or rare in the study population, our design would not achieve traction with respect to the attempt to determine whether there is a correlation between such discounting and choice bundling.

We observed such a correlation. That is, we observed hyperbolicky choice behavior for some subjects, after controlling for diminishing marginal utility, through joint estimation of time and risk

[17] This is the theory of Ainslie [2001].

preferences.[18] We then independently, and for the first time in human subjects, observed bundling that is *not* conditional on assumptions about discounting, and we found that these behaviors were correlated.

8. Previous Experiments

Previous experimental evidence for humans is limited to two experiments: Kirby and Guastello [2001] and Hofmeyr, Ainslie, Charlton and Ross [2010]. However, both of these experiments suffered from methodological limitations, sufficient to defeat any claim that bundled choice has been unambiguously observed in people.

Hofmeyr et al. [2010] employed an incentive-incompatible procedure for eliciting baseline preferences: they relied on a "titration algorithm" to determine SS/LL tradeoffs based on prior responses by the same subject. Harrison and Rutström [2008; §1.5] discuss the problems with incentive-compatibility of comparable titration designs to elicit risk preferences. Nor do Hofmeyr et al. [2010] correct inferences about discount rates for the effect of diminishing marginal utility, implying a serious and known mis-specification stressed by Andersen, Harrison, Lau and Rutström [2008].

Kirby and Guastello [2001] employed many design features that are similar to ours in broad terms.[19] However, they deceived subjects: subjects were informed that they would be contacted

[18] This general conclusion is consistent with previous evidence from Coller, Harrison and Rutström [2012] for comparable samples of university students in the United States. But it is not consistent with evidence from field experiments with adult Danes, reported in Andersen, Harrison, Lau and Rutström [2014b], so it is not something that can be always assumed to be present.

[19] The main goal of their experiment was to test whether choices of SS would be reversed when decisions were presented as parts of series of repeated decisions. Time horizons between deliveries of SS and LL alternatives were held constant, but delays from the experimental sessions were varied. Subjects were randomly assigned to three treatment conditions, each of which varied the way in which linkage between the elements of the presented series was suggested. In the imposed-linking condition, subjects were forced to choose either the SS or the LL option for all decision pairs in the series. In the free-linking condition, subjects were informed that they would be contacted again on the specified decision dates, and asked to choose from the remaining pairs of the series. Finally, a suggested-linking condition used the same

again to choose their preferred options in "future" components of the choice series with which they were presented, but these choices were in fact never elicited. They also did not control for effects of varying reward magnitudes across experimental conditions, and are forthright (p. 162ff.) about the significance of this limitation. They also relied on the Becker, DeGroot and Marschak (BDM) [1964] elicitation method (p.158), which is known to have major behavioral limitations despite being theoretically incentive-compatible.[20] They did not econometrically correct inferences about discount rates for the effect of diminishing marginal utility, leading to the same mis-specification noted above with respect to Hofmeyr et al. [2010]. They study the observable *effects* of bundling, but those effects could have been generated by many alternative latent processes.[21] Finally, they reported as a fact in subject instructions the very hypothesis about bundling that is under investigation: that a person's present choices are evidence about how she will choose in future, similar situations. Of course this generates a potential "experimenter demand effect" to explain their results in this case.

9. Conclusion

Many experimental and behavioral economists have long been familiar with the phenomenology of willpower due to Ainslie [1992][2001], which gives pride of place to an

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instructions as in the free-linking condition, but added the claim that "the choice that you make now is the best indication of how you will choose every time."

[20] Harrison [1992] discusses the "payoff dominance" problem with the BDM procedure: that it provides very poor incentives for precise revelation of present-value certainty equivalents. Rutström [1998] discusses the poor ability of the BDM procedure to recover true, "homegrown" values when compared to real-time English auctions. And Plott and Zeiler [2005] discuss at length the subject misconceptions that arise when using the BDM procedure, and what one has to do in order to train subjects to understand that it is indeed incentive-compatible.

[21] We do not interpret a possible effect from a forced link between the decisions as evidence for choice bundling. Rather, the inclusion of the Forced2 and Forced3 treatments in our design enables us to distinguish the effect that Kirby and Guastello [2001] found from genuine choice bundling that arise when subjects are free to vary choices over a sequence, in the Free2 and Free3 treatments.

experience he characterizes as choice bundling. Economists can find a clear echo of this idea in the work of one of their own tribe, Schelling [1978][1980][1984]. However, whereas other core themes emphasized by Ainslie [1992][2001], specifically hyperbolic discounting and strategic bargaining among temporally distinct selves, have featured prominently in the economic literature, choice bundling has been absent. We conjecture that this is due to the difficulty of designing an experiment that could in principle identify choice bundling, if it exists, having deterred previous research. Due to the observed heterogeneity of functional forms of discounting, the possible presence of magnitude effects, and the fact that discounting rates are known to be biased and inconsistent if diminishing marginal utility is not measured and factored into the analysis, choice bundling is intrinsically resistant to empirical identification in the absence of structural modeling.

The experiment and analysis reported here surmounts these barriers. Hence we can report that choice bundling, previously exhibited using methodologies that would be regarded as adequate by the standards of economists only in rats (Ainslie and Monterosso [2003]), has for the first time been observed in people. We suggest that this observation should motivate economists to take up the daunting task of building frame-dependence of the ontology of choice alternatives into their theoretical repertoire. We call the task "daunting" because it introduces a new class of potential endogeneity problems into economic modeling. If a person produces different behavior depending on whether she frames a choice as existential ("I choose to be the kind of person who is generally conscientious about fitness rather than indulgent about gustatory pleasure"), motivating ("I choose to jog today in order to avoid undermining my hope of choosing to jog in the future") or episodic ("I choose not to exercise today"), then the economist setting out to fit her response functions to a model must face the considerable nuisance of not being able to treat her choice set as exogenous. The ultimate point of the analysis of Ainslie [1992][2001], after all, is that people can willfully manage their choice framing. Indeed, according to him this capacity is constitutive of the real phenomenon of willpower.

Though the mission is challenging, experience suggests that it would be defeatist to regard it as impossible. To invoke a close analogy, Bacharach [2006] proposed that people can voluntarily frame their own agency in terms of identification with individualistic or collective utility functions. This implies that choice sets in games, and therefore likewise strategy sets, are not exogenously given. Few of the reviewers of Bacharach [2006] quarreled with the intuitive psychology underlying his claim, or with his catalogue of examples drawn from everyday social life; yet applications of his idea in economic studies are rare to nonexistent. The explanation for this is straightforward: Bacharach [2006] offered little guidance toward integration of his insight into formal modeling. However, Ross [2014, p.291-303] and Hofmeyr and Ross [2018] argue that the limitation has been at least partly surmounted by the recent theory of conditional games due to Stirling [2012], which shows how the axioms of standard game theory can be expanded to allow for Nash and Nash-Bayes equilibrium solutions to be recovered even when players can strategically re-frame their agency in response to social relationships and pressures.

We close by presenting a taste of some of the range of issues to which economists could make richer contributions if their model of choice allowed for bundling. We briefly sketch reasons for suspecting that the phenomenon is directly implicated in the motivational effectiveness of rationalization, in heterogeneous discounting of different classes of consumption goods and experiences by the same person, in chronic failures of will, in compulsive resistance to learning from experience, and in the experience of free will. The last might look like a topic that is of no concern to economists; however, the nature of the experience of free will may explain why most people are competent economic agents while being poor natural economists.

Rationalization. The perception that a much larger reward, or bundle of rewards, depends on a current choice puts pressure on a person to either forego a tempting satisfaction or to find some reason that the current choice is not a valid test case for the larger prospect. What is commonly called rationalization is the activity of distinguishing the case at hand from a larger category. For example, "New Year's Eve doesn't count," "I'll never have a chance to eat that dish again," "I'm on vacation," and endless other versions of "just this once." Without choice bundling and the recursive self-prediction to which it gives rise, this legalistic self-governance would be pointless. But are we to doubt that people regularly engage in it, and that it influences their choices?

Heterogeneous intrapersonal discounting. Within limits a person should be able to adopt various norms for discounting under varying circumstances, including exponential discounting and no discounting at all, as economists indeed sometimes recommend. These limits would be determined by the aggregate discounted value of the expected fruits of such a norm, relative to the discounted value of individual temptations as they drew near. If a person defines paying more than the prime interest rate as a defection from a prudent rule she has adopted, she will be more likely to lapse than if she defines paying greater than her current credit card rate as such a defection. The forms of self-management made possible by choice bundling can be exercised with more or less skill and more or less information.

Chronic failures of will and mental accounts. Serious lapses of will are likely to result in a person's discrimination of the relevant circumstances from the larger bundle that is the basis of willpower. In turn this can lead to what are in effect involuntary mental accounts: circumscribed areas of dyscontrol in which the person gives up on willpower ("I *have to* smoke after a meal," "I *can't* get myself to speak in public," etc.). People's experience of what would seem to be choices as involuntary barriers has otherwise puzzled theorists. In fact, we need not deny that these behavioral patterns are chosen. But the choices in question are of policies rather than of discrete actions, and are not typically proximate in time to the discrete actions. To the extent that an economic model cannot represent this, it is apt to mis-predict.

Compulsive character. A person's uncertainty about how she will interpret a current choice when looking back from the moment of a future choice will create an incentive to give temptations a wider berth than sheer calculation would require. If she is especially afraid of impulses and becomes aware of the rationale for this extra caution, she may come to perceive a failure to give such a wide berth as itself something of a lapse (Ainslie [1992]). Bodner and Prelec [2001] argue that the additional diagnostic utility of such self-signaling motivates drift into poles of scrupulous self-control or irresistible impulsiveness, a process that has been observed in a two-person analog of the intertemporal prisoner's dilemma by Monterosso, Ainslie, Toppi-Mullen and Gault [2002]. If the direction of drift is away from scrupulous self-control, the result may be a chronic circumscribed failure of will, as above.

"Free will." Small changes of perceived symbolism, and hence category membership, may change the implications of a self-signal, so that a person cannot be absolutely sure of what her motivation and hence her choice will be even in the near future. This will create in turn both the introspective opacity and sense of participation in one's own decisions that have been held to be key to the near-universal subjective experience of free will (Ainslie [2011]). In effect, the experience of free will arises from people's imperfect abilities to identify their own utility functions. The history of economic theory shows three broad kinds of response to such indeterminacy of utility specification from a focus on discrete choices. One response, forced if utility functions are understood as literal summaries of actual choices that must therefore predict all of those choices perfectly or be regarded as false, is to declare people "irrational" and abandon the methods and principles of economics in favor of psychology (Ariely [2008]). Another response is to construct normative metrics of welfare evaluation for individuals that only consider opportunity sets, effectively abandoning any hope of using any preference information (Sugden [2004][2018; ch.5]). An alternative response, which Harrison and Ross [2018] recommend, involves recognizing that utility functions are constructs that most usefully apply to *patterns* of choice, or what Mill and Marshall sagely called "tendencies." Bundling might be among the fundamental processes that make strong tendencies typical in human choice, and thus generate the phenomena that microeconomics is essentially about.

Table 1: Experimental Design

Treatment	Description of Treatment	Number of Subjects
FED0	The choice between a smaller amount "today" (i.e., a front-end delay of 0) or a larger amount in 28 days.	35
FED1	The same as FED0, but with a front-end delay of 1 day.	29
FED35	The same as FED0, but with a front-end delay of 35 days.	35
Free2	Two "free" decisions. One as in FED0, one as in FED35.	37
Forced2	Two "forced" decisions. One as in FED0, one as in FED35.	33
Free3	Three "free" decisions. One as in FED0, one as in FED35, and another pair of options.	31
Forced3	Three "forced" decisions. One as in FED0, one as in FED35, and another pair of options.	30
Total		230

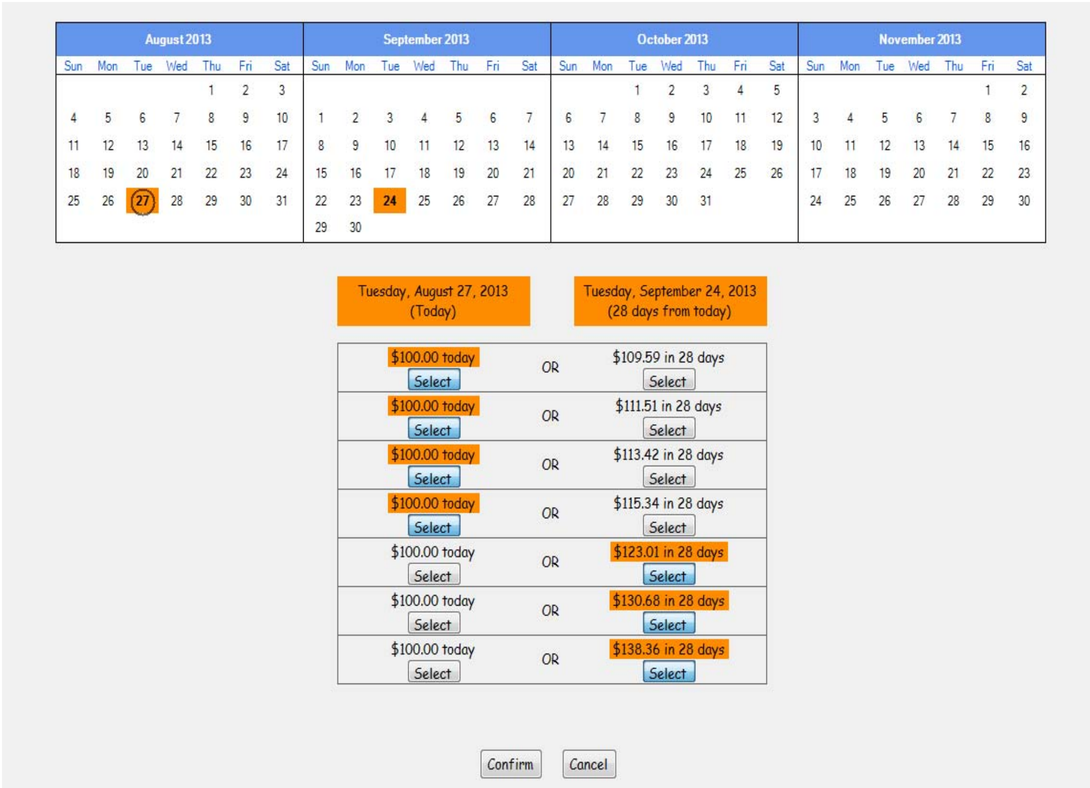

Figure 1: Decision Task in Treatment FED0

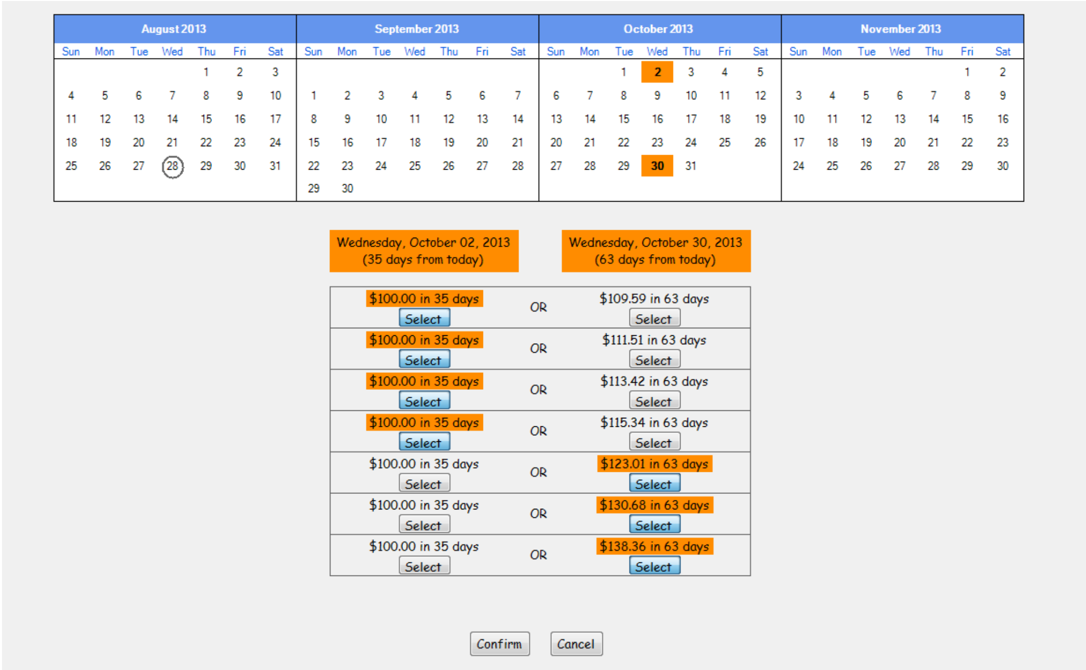

Figure 2: Decision Task in Treatment FED35

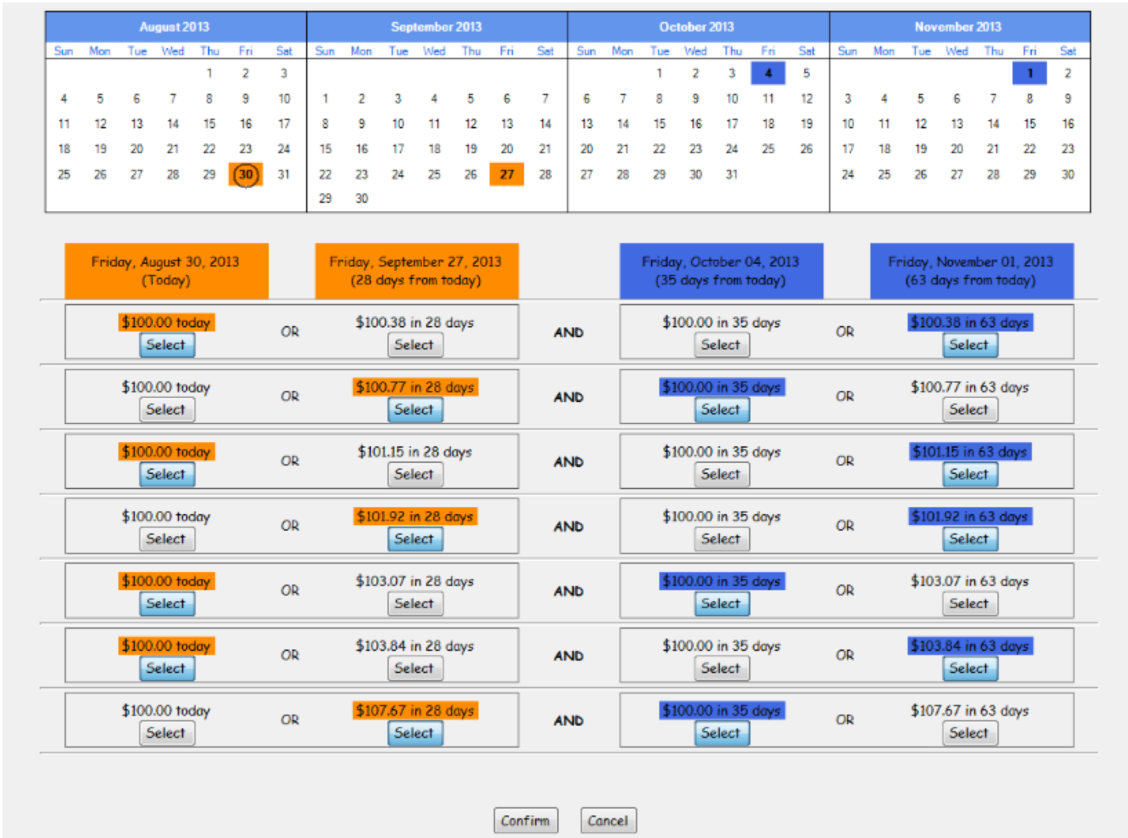

Figure 3: Decision Task in Treatment Free2

Note that decisions within the same choice row are independent.

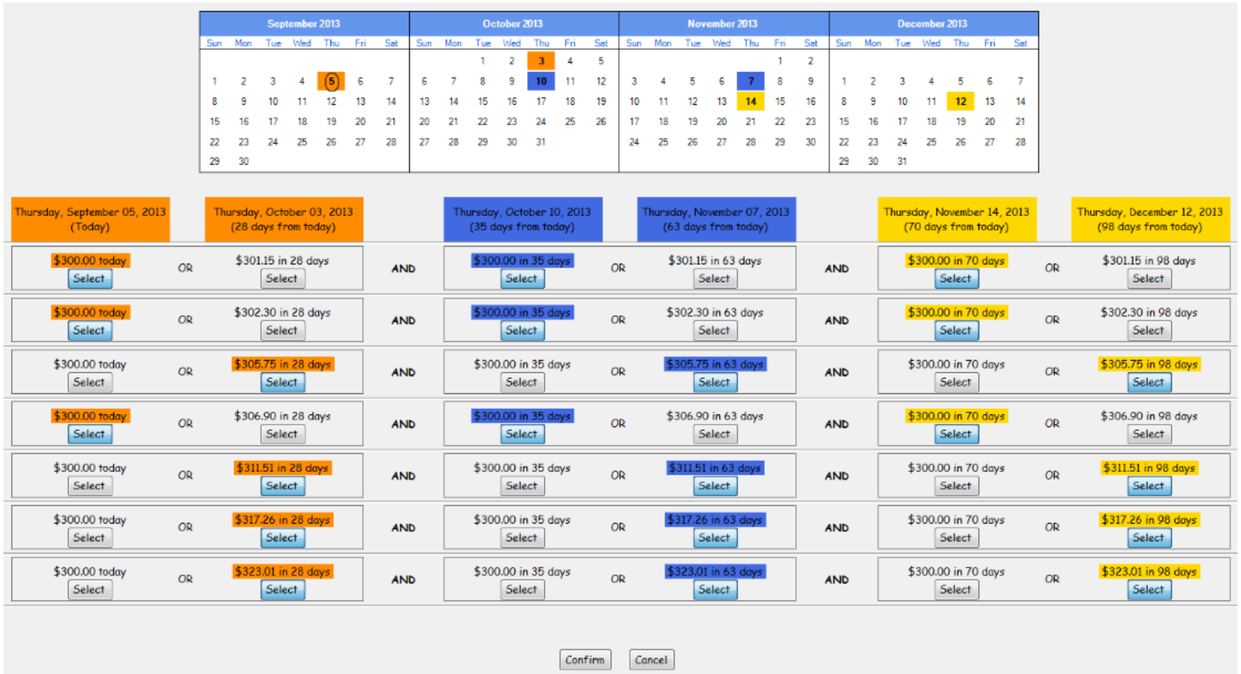

Figure 4: Decision Screen in Treatment Forced3

Note that all three decisions within the same choice row are forced to be uniform: all SS or all LL.

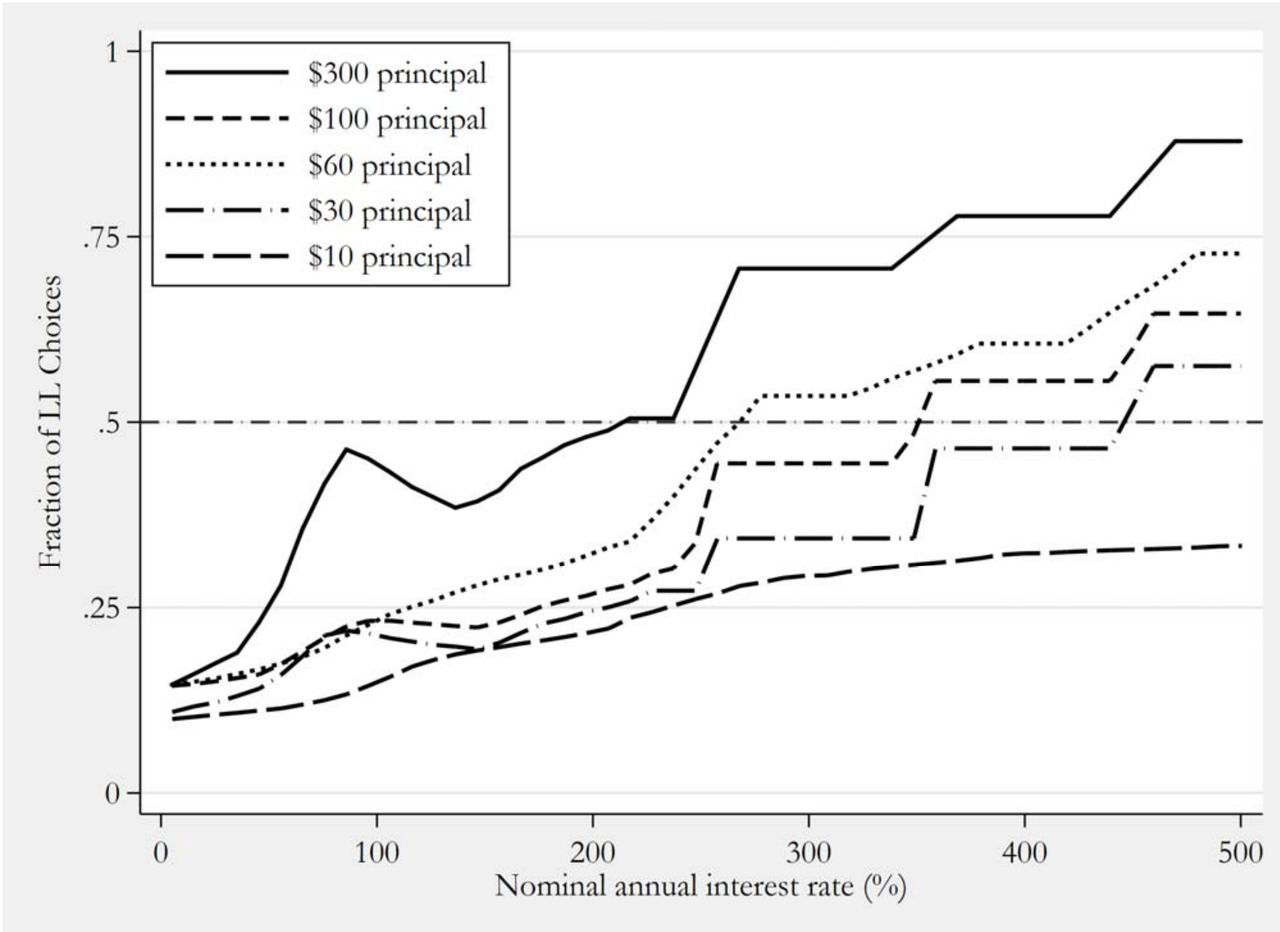

Figure 5: Fraction of LL Choices Across Principals in Treatments FED0, FED1 and FED35

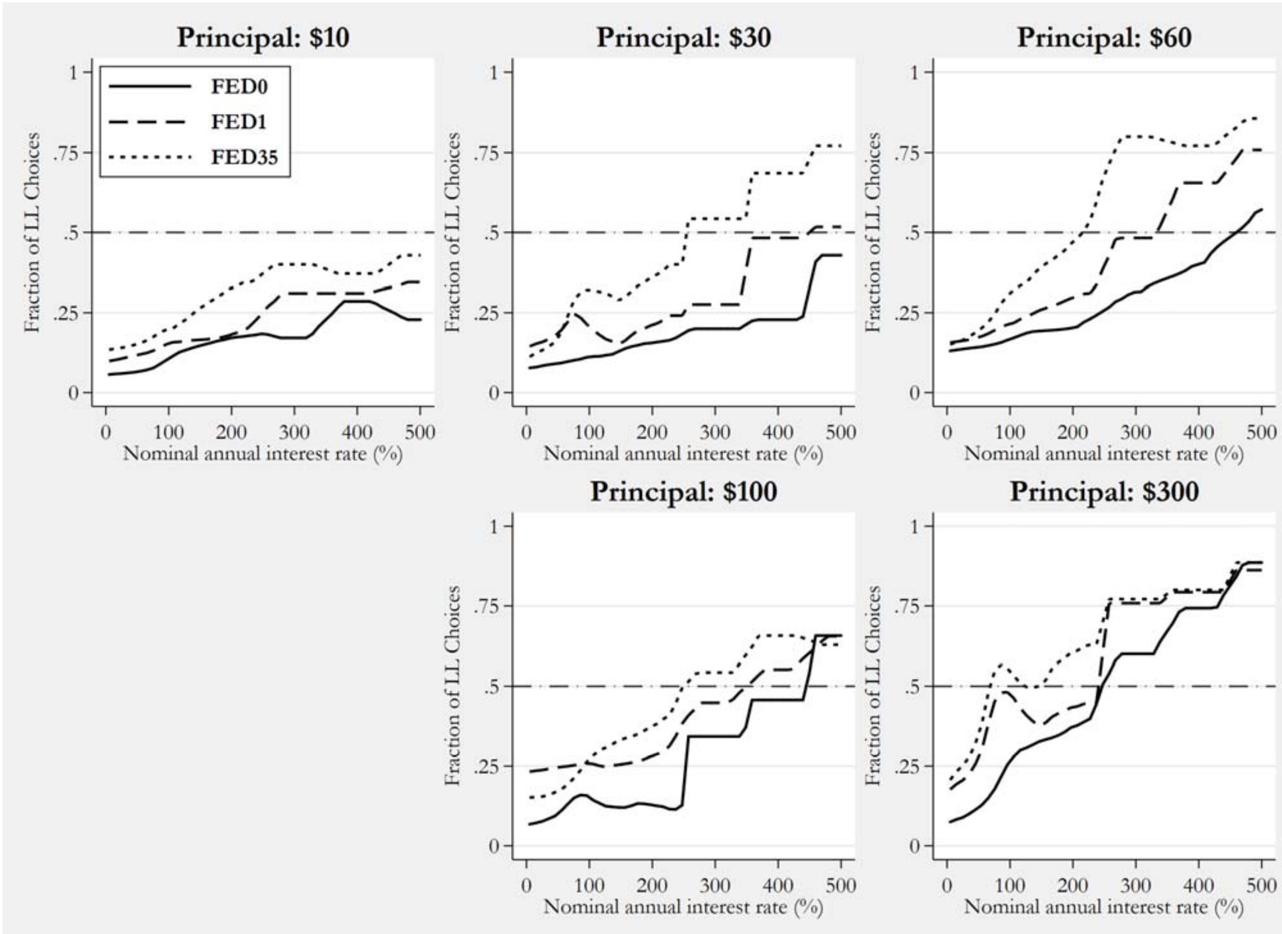

Figure 6: Fraction of LL Choices, by Principal, in Treatments FED0, FED1 and FED35

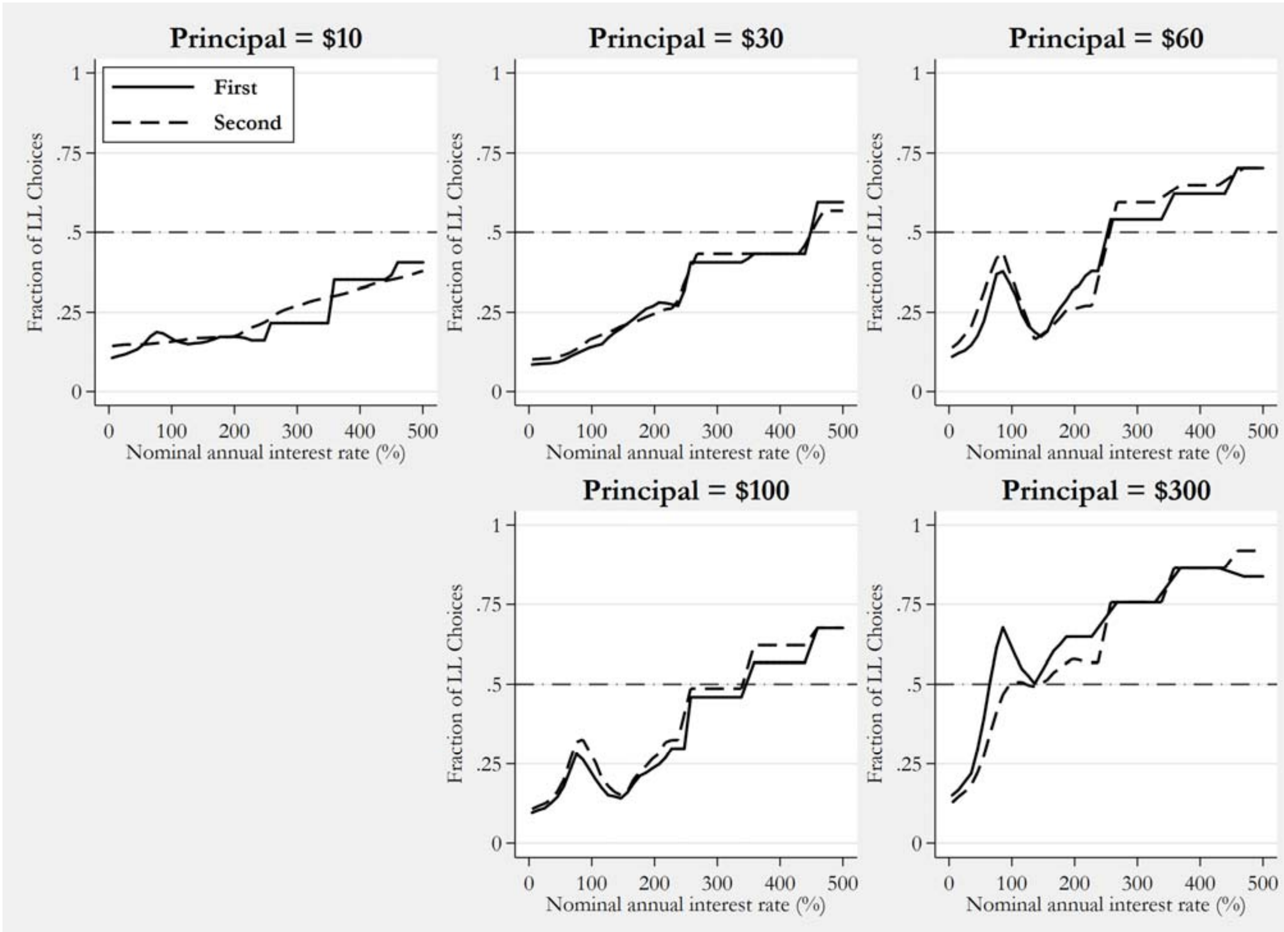

Figure 7: Comparison of First and Second Choices in Treatment Free2

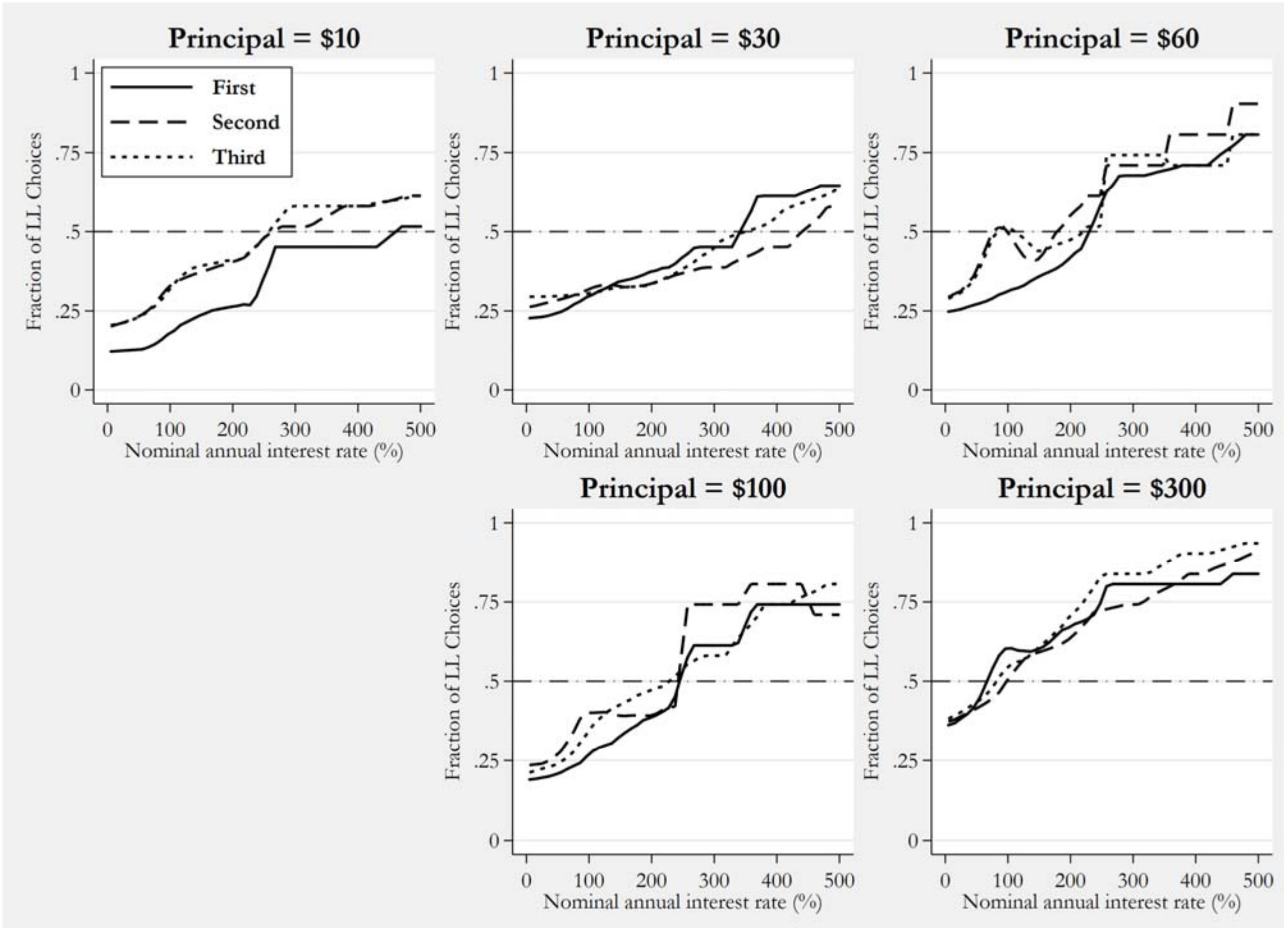

Figure 8: Comparison of First, Second and Third Choices in Treatment Free3

Table 2: Estimates of Mixture Models

A. Mixture of Exponential and Hyperbolic Discounting

	Point Estimate	Standard Error	*p*-value	[95% Confidence	Interval]
δ	0.088	0.073	0.232	-0.056	0.231
K	0.806	0.154	<0.001	0.504	1.108
π^E	0.295	0.138	0.033	0.024	0.566
π^H	0.705	0.138	<0.001	0.434	0.976
π^E bundled	0.442	0.192	0.021	0.066	0.818
π^E second	-0.240	0.096	0.013	-0.429	-0.051
π^E forced	0.466	0.233	0.046	0.009	0.924
δ bundled	0.052	0.100	0.605	-0.145	0.248
K bundled	0.494	0.805	0.540	-1.084	2.072

B. Mixture of Exponential and Quasi-Hyperbolic Discounting

	Point Estimate	Standard Error	*p*-value	[95% Confidence	Interval]
δ	0.107	0.083	0.200	-0.056	0.269
β	0.971	0.011	<0.001	0.949	0.993
δ^{QH}	0.682	0.244	0.005	0.204	1.161
π^E	0.292	0.125	0.020	0.046	0.537
π^H	0.708	0.125	<0.001	0.463	0.954
π^E bundled	0.429	0.203	0.035	0.031	0.828
π^E second	-0.236	0.099	0.017	-0.430	-0.042
π^E forced	0.474	0.197	0.016	0.087	0.860
δ bundled	0.027	0.109	0.804	-0.187	0.241
β bundled	0.005	0.040	0.894	-0.073	0.083
δ^{QH} bundled	0.963	1.387	0.487	-1.755	3.681

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APPENDICES

Do People Bundle Sequences of Choices?

An Experimental Investigation

George Ainslie, Glenn W. Harrison, Morten I. Lau, Don Ross,

Alexander Schuhr and J. Todd Swarthout

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Appendix A: Instructions

In the actual experiments the treatments were named differently than the names used in our text, in part to keep the research objective abstract.

Treatments T0, T1 and T3 correspond to what we refer to as FED0, FED and FED35, respectively.

Treatment T2 corresponds to what we refer to as HiMag in Appendix E.

Treatments T4 and T5 correspond to what we refer to as Free2 and Forced2, respectively.

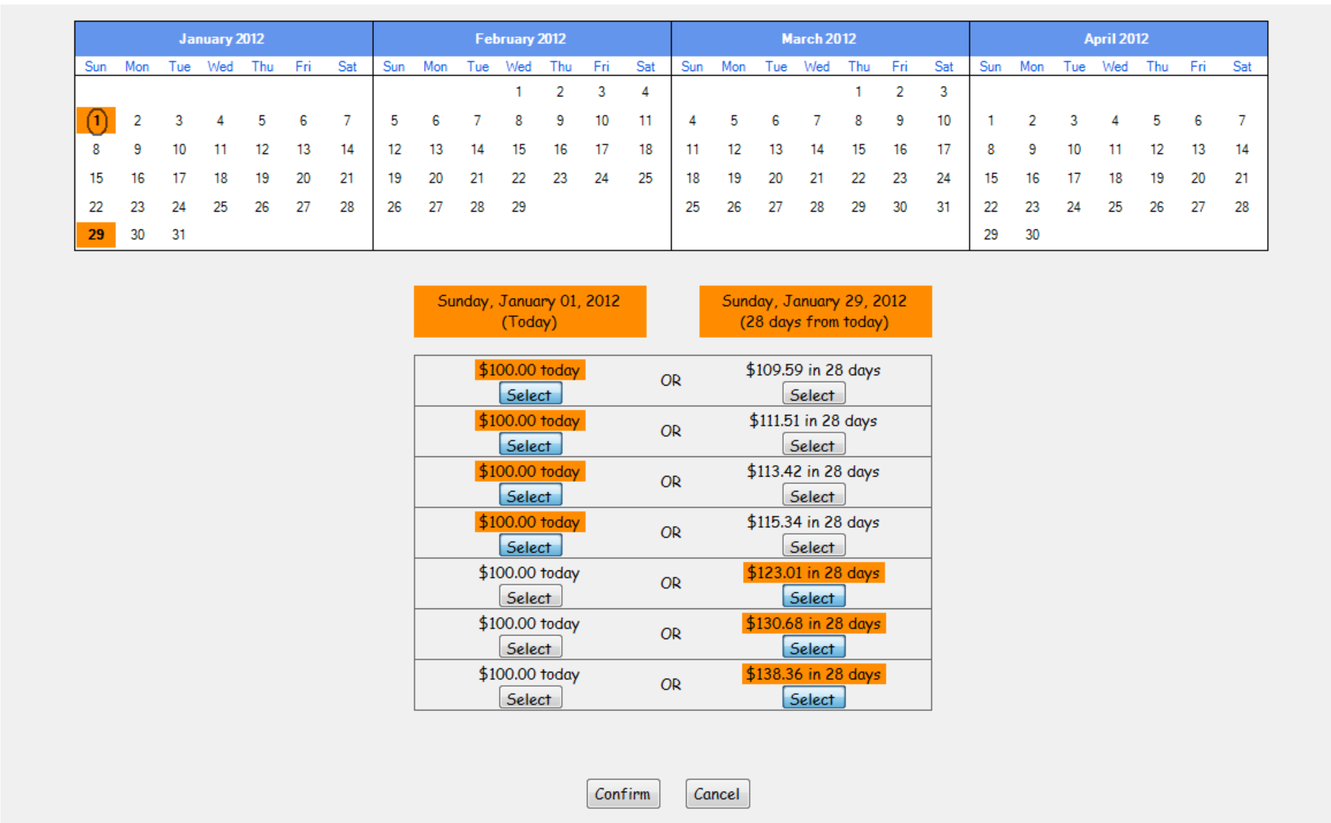
Treatments T6 and T7 correspond to what we refer to as Free3 and Forced3, respectively.

A. Instructions: Treatment FED0

B_T0

Sooner versus Later Payments

In this task you will make a number of choices between receiving an amount of money on a “sooner” date or a different amount of money on a “later” date. The sooner date will always be today, while the later date will be some weeks from today. An example of a decision screen is shown below. The dates and amounts in your task will differ. You will make all decisions on a computer.



This screen shows seven decisions. Each decision is presented on a different row. All decisions have the same format. For the purpose of explaining this task, assume for the moment that today is January 1, 2012. In the calendar, today’s date is enclosed in a black circle. Let’s look at the first decision in the example above (the one on the first decision row). The sooner choice pays $100 today, January 1, 2012 in the example, and the later choice pays $109.59 in twenty-eight days from today. You choose between these two options by clicking the button under the option you prefer.

We will present you with ten of these decision screens, with each screen having seven choices for you to make. You must make all seven choices on each decision screen before moving to the next decision screen. While on a single decision screen, the only difference between decisions is that the dollar amounts of the future payment will change. However, different decision screens will have different dollar amounts and future payment dates. So, you should make sure to pay attention to both the changing dollar amounts and changing dates as you make your decisions.

After you have worked through all of the decisions, we will select one of your ten decision screens by rolling a 10-sided die. Then we will roll a 10-sided die again, until a number between 1 and 7 comes up, to pick one decision row on that screen. When you make your choices you will not know which decision will be selected for payment. You should therefore treat each decision as if it might actually count for payment.

Once the decision screen and row are selected, we will look at the specific choice you made and the payout for that choice. If the payout is smaller (less than $100), you will actually be paid this amount. However, if the payout is larger ($100 or greater), you will roll a 10-sided die to determine whether or not you are actually paid this amount. If you roll a 1, you will actually be paid the amount, and on the date that you chose to receive it. If you roll a number other than 1, you will earn nothing in this task. This roll will be at the end of the experiment, and in private, when you are being paid.

For instance, suppose the decision screen in the above example was selected and you preferred the sooner date in the first four rows and the later date in the last three rows, as shown above. You would then roll a 10-sided die, until a number between 1 and 7 comes up, to select the row. Suppose the outcome was 5 and the fifth row is selected, where you preferred a payment of $123.01 in twenty-eight days. Since this payment is $100 or greater, you would roll the 10-sided die again, at the end of the experiment. If the outcome of your die-roll is 1, you will be paid $123.01 in twenty-eight days. However, if the outcome is 3, or anything other than 1, you will get nothing.

You will receive the money on the date stated in your preferred option. We will pay you using *PayPal*, which is an online payment service. We will explain more about *PayPal* in a few minutes. If you receive some money to be paid in the future you will also receive a written confirmation from Professor Harrison which guarantees that the money is to be paid to you on that date.

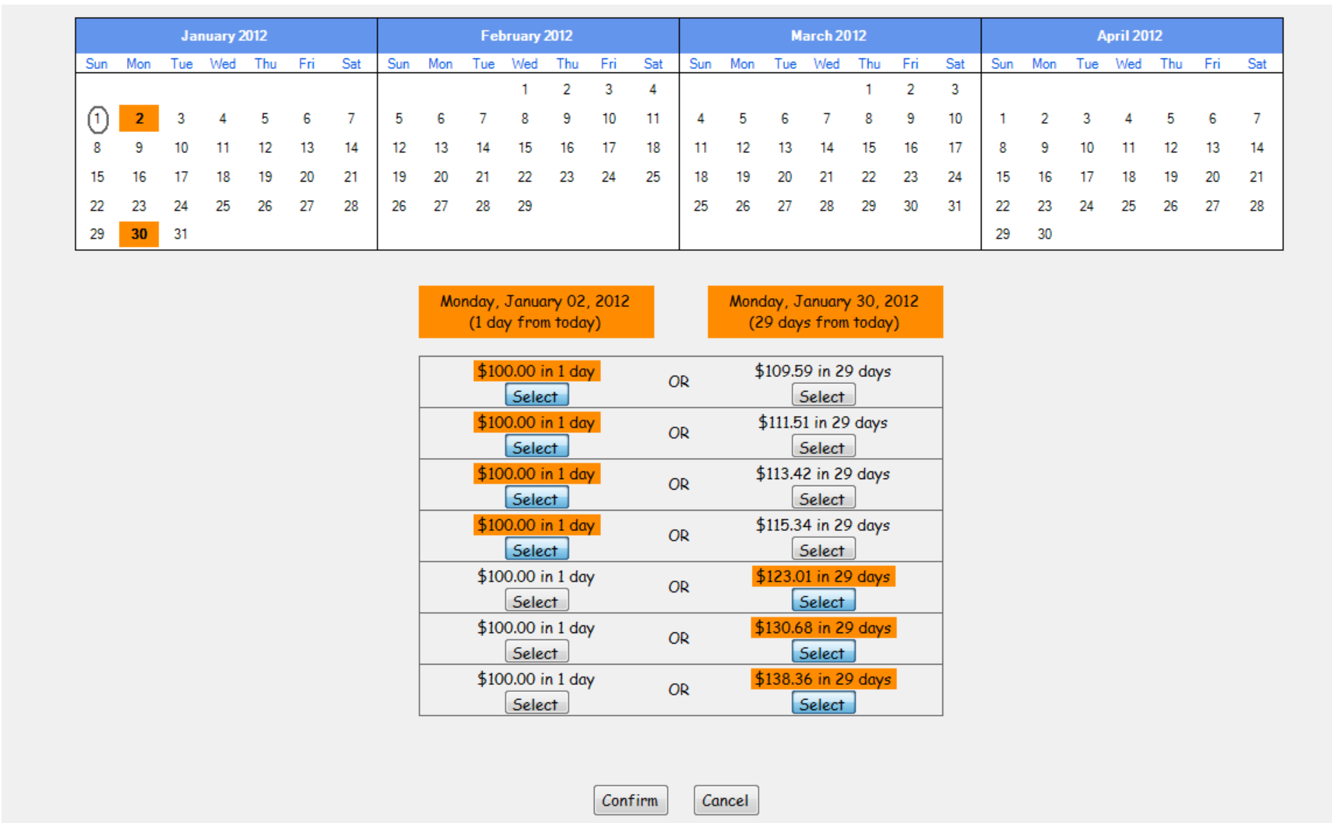
The money you receive from these choices is in addition to the show-up fee of $5, which is paid out at the end of the experiment as cash.

B. Instructions: Treatment FED1

B_T1

Sooner versus Later Payments

In this task you will make a number of choices between receiving an amount of money on a "sooner" date or a different amount of money on a "later" date. The sooner date will always be tomorrow, while the later date will be some weeks from today. An example of a decision screen is shown below. The dates and amounts in your task will differ. You will make all decisions on a computer.



This screen shows seven decisions. Each decision is presented on a different row. All decisions have the same format. For the purpose of explaining this task, assume for the moment that today is January 1, 2012. In the calendar, today's date is enclosed in a black circle. Let's look at the first decision in the example above (the one on the first decision row). The sooner choice pays $100 tomorrow, January 2, 2012 in the example, and the later choice pays $109.59 in twenty-nine days from today. You choose between these two options by clicking the button under the option you prefer.

We will present you with ten of these decision screens, with each screen having seven choices for you to make. You must make all seven choices on each decision screen before moving to the next decision screen. While on a single decision screen, the only difference between decisions is that the dollar amounts of the future payment will change. However, different decision screens will have different dollar amounts and future payment dates. So, you should make sure to pay attention to both the changing dollar amounts and changing dates as you make your decisions.

After you have worked through all of the decisions, we will select one of your ten decision screens by rolling a 10-sided die. Then we will roll a 10-sided die again, until a number between 1 and 7 comes up, to pick one decision row on that screen. When you make your choices you will not know which decision will be selected for payment. You should therefore treat each decision as if it might actually count for payment.

Once the decision screen and row are selected, we will look at the specific choice you made and the payout for that choice. If the payout is smaller (less than $100), you will actually be paid this amount. However, if the payout is larger ($100 or greater), you will roll a 10-sided die to determine whether or not you are actually paid this amount. If you roll a 1, you will actually be paid the amount, and on the date that you chose to receive it. If you roll a number other than 1, you will earn nothing in this task. This roll will be at the end of the experiment, and in private, when you are being paid.

For instance, suppose the decision screen in the above example was selected and you preferred the sooner date in the first four rows and the later date in the last three rows, as shown above. You would then roll a 10-sided die, until a number between 1 and 7 comes up, to select the row. Suppose the outcome was 5 and the fifth row is selected, where you preferred a payment of $123.01 in twenty-nine days. Since this payment is $100 or greater, you would roll the 10-sided die again, at the end of the experiment. If the outcome of your die-roll is 1, you will be paid $123.01 in twenty-nine days. However, if the outcome is 3, or anything other than 1, you will get nothing.

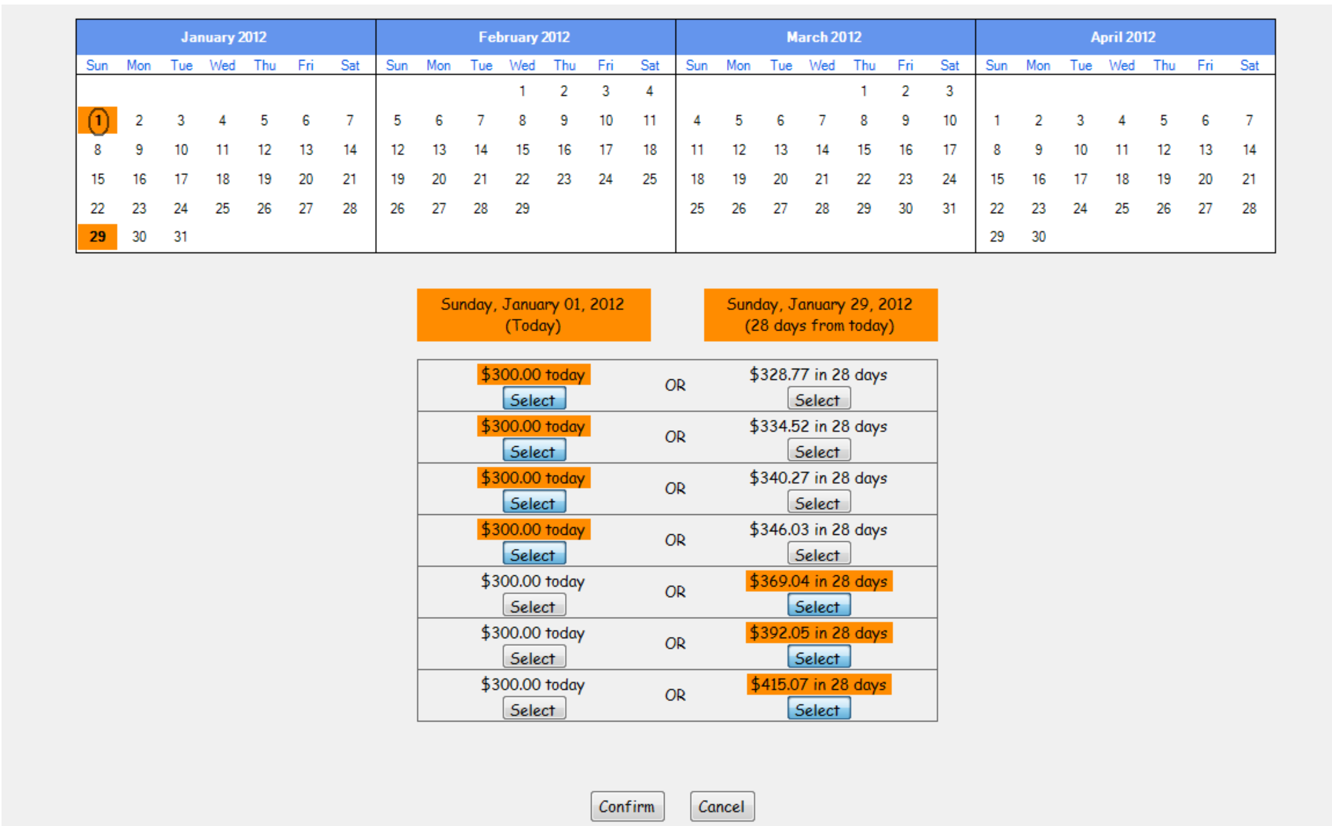
You will receive the money on the date stated in your preferred option. We will pay you using *PayPal*, which is an online payment service. We will explain more about *PayPal* in a few minutes. If you receive some money to be paid in the future you will also receive a written confirmation from Professor Harrison which guarantees that the money is to be paid to you on that date.

The money you receive from these choices is in addition to the show-up fee of $5, which is paid out at the end of the experiment as cash.

B_T2

Sooner versus Later Payments

In this task you will make a number of choices between receiving an amount of money on a "sooner" date or a different amount of money on a "later" date. The sooner date will always be today, while the later date will be some weeks from today. An example of a decision screen is shown below. The dates and amounts in your task will differ. You will make all decisions on a computer.



This screen shows seven decisions. Each decision is presented on a different row. All decisions have the same format. For the purpose of explaining this task, assume for the moment that today is January 1, 2012. In the calendar, today's date is enclosed in a black circle. Let's look at the first decision in the example above (the one on the first decision row). The sooner choice pays $300 today, January 1, 2012 in the example, and the later choice pays $328.77 in twenty-eight days from today. You choose between these two options by clicking the button under the option you prefer.

We will present you with ten of these decision screens, with each screen having seven choices for you to make. You must make all seven choices on each decision screen before moving to the next decision screen. While on a single decision screen, the only difference between decisions is that the dollar amounts of the future payment will change. However, different decision screens will have different dollar amounts and future payment dates. So, you should make sure to pay attention to both the changing dollar amounts and changing dates as you make your decisions.

After you have worked through all of the decisions, we will select one of your ten decision screens by rolling a 10-sided die. Then we will roll a 10-sided die again, until a number between 1 and 7 comes up, to pick one decision row on that screen. When you make your choices you will not know which decision will be selected for payment. You should therefore treat each decision as if it might actually count for payment.

Once the decision screen and row are selected, we will look at the specific choice you made and the payout for that choice. If the payout is smaller (less than $300), you will actually be paid this amount. However, if the payout is larger ($300 or greater), you will roll a 10-sided die to determine whether or not you are actually paid this amount. If you roll a 1, you will actually be paid the amount, and on the date that you chose to receive it. If you roll a number other than 1, you will earn nothing in this task. This roll will be at the end of the experiment, and in private, when you are being paid.

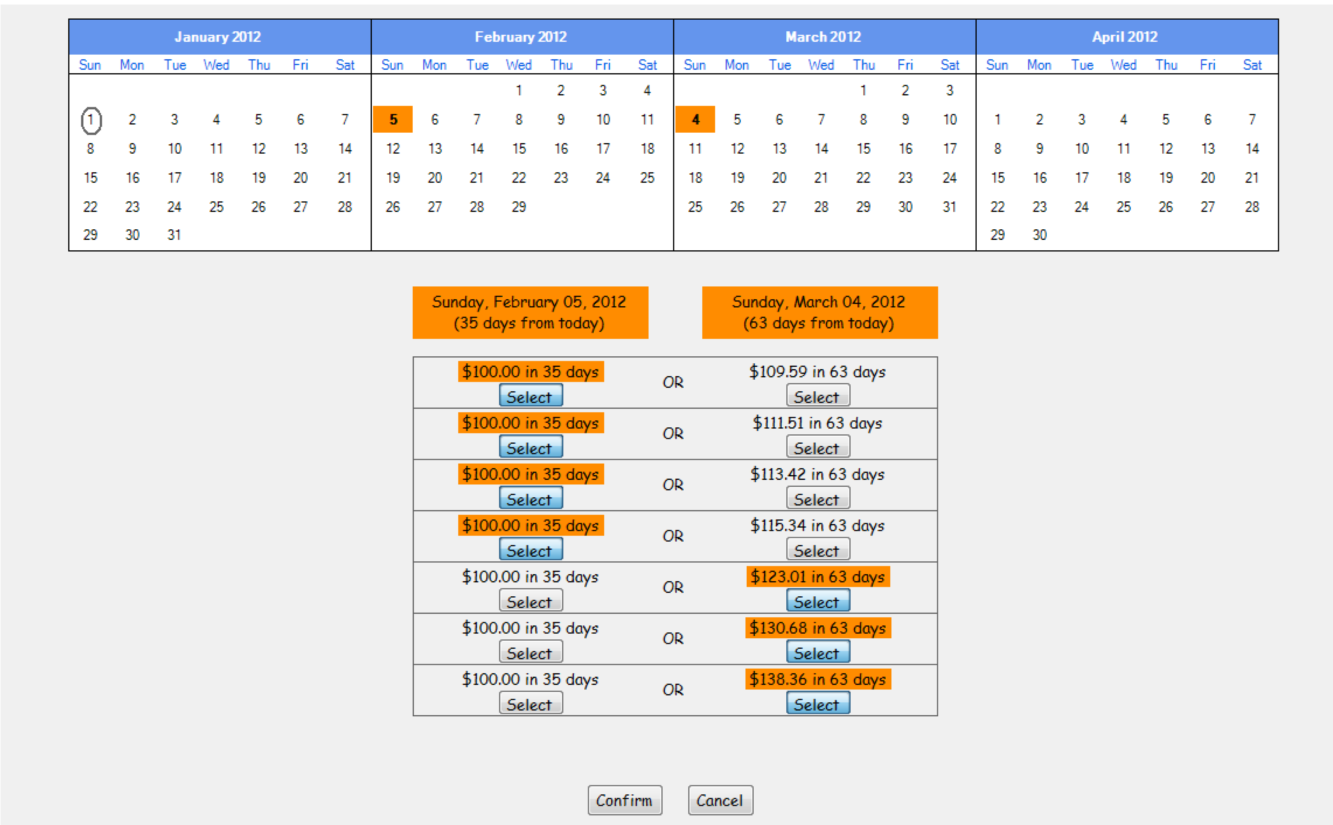
For instance, suppose the decision screen in the above example was selected and you preferred the sooner date in the first four rows and the later date in the last three rows, as shown above. You would then roll a 10-sided die, until a number between 1 and 7 comes up, to select the row. Suppose the outcome was 5 and the fifth row is selected, where you preferred a payment of $369.04 in twenty-eight days. Since this payment is $300 or greater, you would roll the 10-sided die again, at the end of the experiment. If the outcome of your die-roll is 1, you will be paid $369.04 in twenty-eight days. However, if the outcome is 3, or anything other than 1, you will get nothing.

You will receive the money on the date stated in your preferred option. We will pay you using *PayPal*, which is an online payment service. We will explain more about *PayPal* in a few minutes. If you receive some money to be paid in the future you will also receive a written confirmation from Professor Harrison which guarantees that the money is to be paid to you on that date.

The money you receive from these choices is in addition to the show-up fee of $5, which is paid out at the end of the experiment as cash.

Sooner versus Later Payments

In this task you will make a number of choices between receiving an amount of money on a "sooner" date or a different amount of money on a "later" date. The sooner date will be some weeks from today, while the later date will be even more weeks from today. An example of a decision screen is shown below. The dates and amounts in your task will differ. You will make all decisions on a computer.



This screen shows seven decisions. Each decision is presented on a different row. All decisions have the same format. For the purpose of explaining this task, assume for the moment that today is January 1, 2012. In the calendar, today's date is enclosed in a black circle. Let's look at the first decision in the example above (the one on the first decision row). The sooner choice pays $100 in thirty-five days from today, where "today" is January 1, 2012 in the example, and the later choice pays $109.59 in sixty-three days from today. You choose between these two options by clicking the button under the option you prefer.

We will present you with ten of these decision screens, with each screen having seven choices for you to make. You must make all seven choices on each decision screen before moving to the next decision screen. While on a single decision screen, the only difference between decisions is that the dollar amounts of the future payment will change. However, different decision screens will have different dollar amounts and future payment dates. So, you should make sure to pay attention to both the changing dollar amounts and changing dates as you make your decisions.

After you have worked through all of the decisions, we will select one of your ten decision screens by rolling a 10-sided die. Then we will roll a 10-sided die again, until a number between 1 and 7 comes up, to pick one decision row on that screen. When you make your choices you will not know which decision will be selected for payment. You should therefore treat each decision as if it might actually count for payment.

Once the decision screen and row are selected, we will look at the specific choice you made and the payout for that choice. If the payout is smaller (less than $100), you will actually be paid this amount. However, if the payout is larger ($100 or greater), you will roll a 10-sided die to determine whether or not you are actually paid this amount. If you roll a 1, you will actually be paid the amount, and on the date that you chose to receive it. If you roll a number other than 1, you will earn nothing in this task. This roll will be at the end of the experiment, and in private, when you are being paid.

For instance, suppose the decision screen in the above example was selected and you preferred the sooner date in the first four rows and the later date in the last three rows, as shown above. You would then roll a 10-sided die, until a number between 1 and 7 comes up, to select the row. Suppose the outcome was 5 and the fifth row is selected, where you preferred a payment of $123.01 in sixty-three days. Since this payment is $100 or greater, you would roll the 10-sided die again, at the end of the experiment. If the outcome of your die-roll is 1, you will be paid $123.01 in sixty-three days. However, if the outcome is 3, or anything other than 1, you will get nothing.

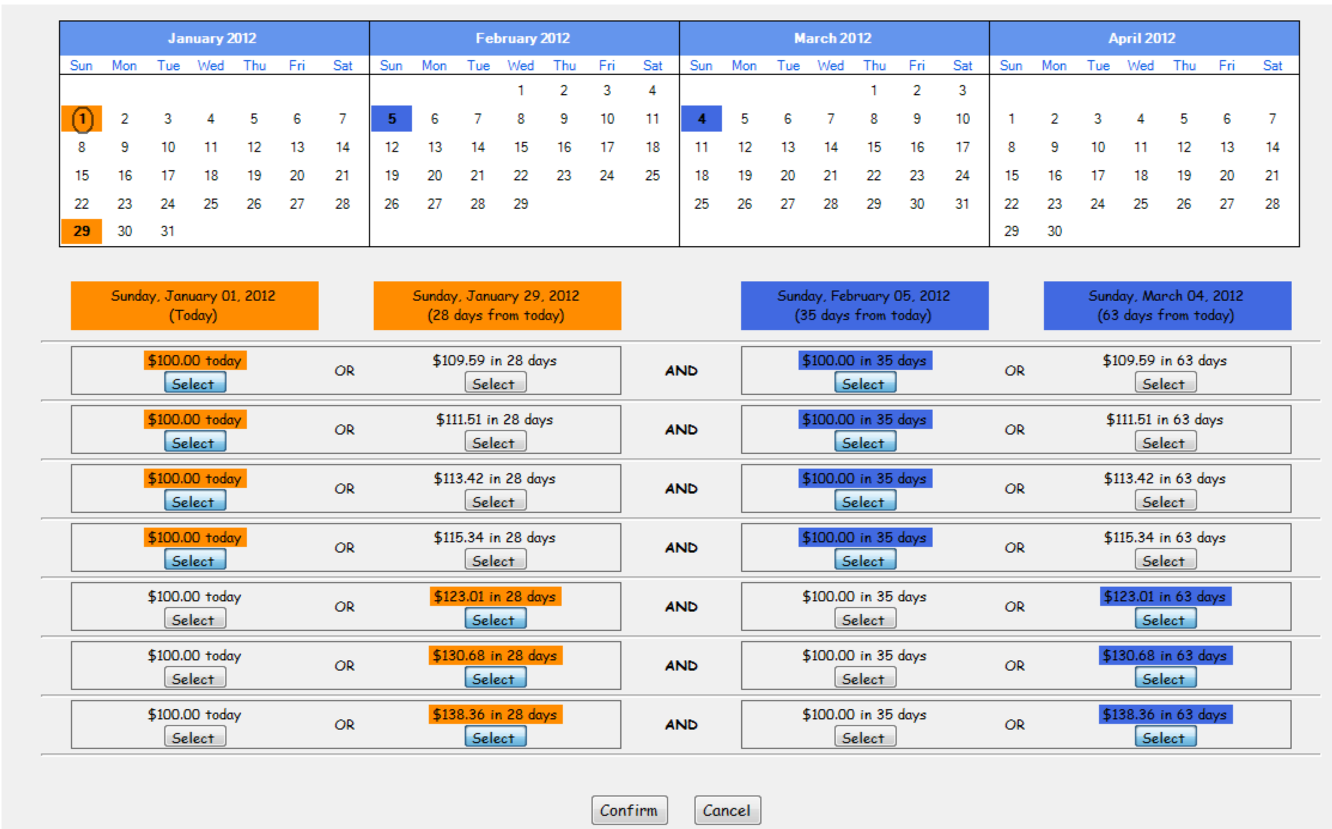
You will receive the money on the date stated in your preferred option. We will pay you using *PayPal*, which is an online payment service. We will explain more about *PayPal* in a few minutes. If you receive some money to be paid in the future you will also receive a written confirmation from Professor Harrison which guarantees that the money is to be paid to you on that date.

The money you receive from these choices is in addition to the show-up fee of $5, which is paid out at the end of the experiment as cash.

B_T4

Sooner versus Later Payments

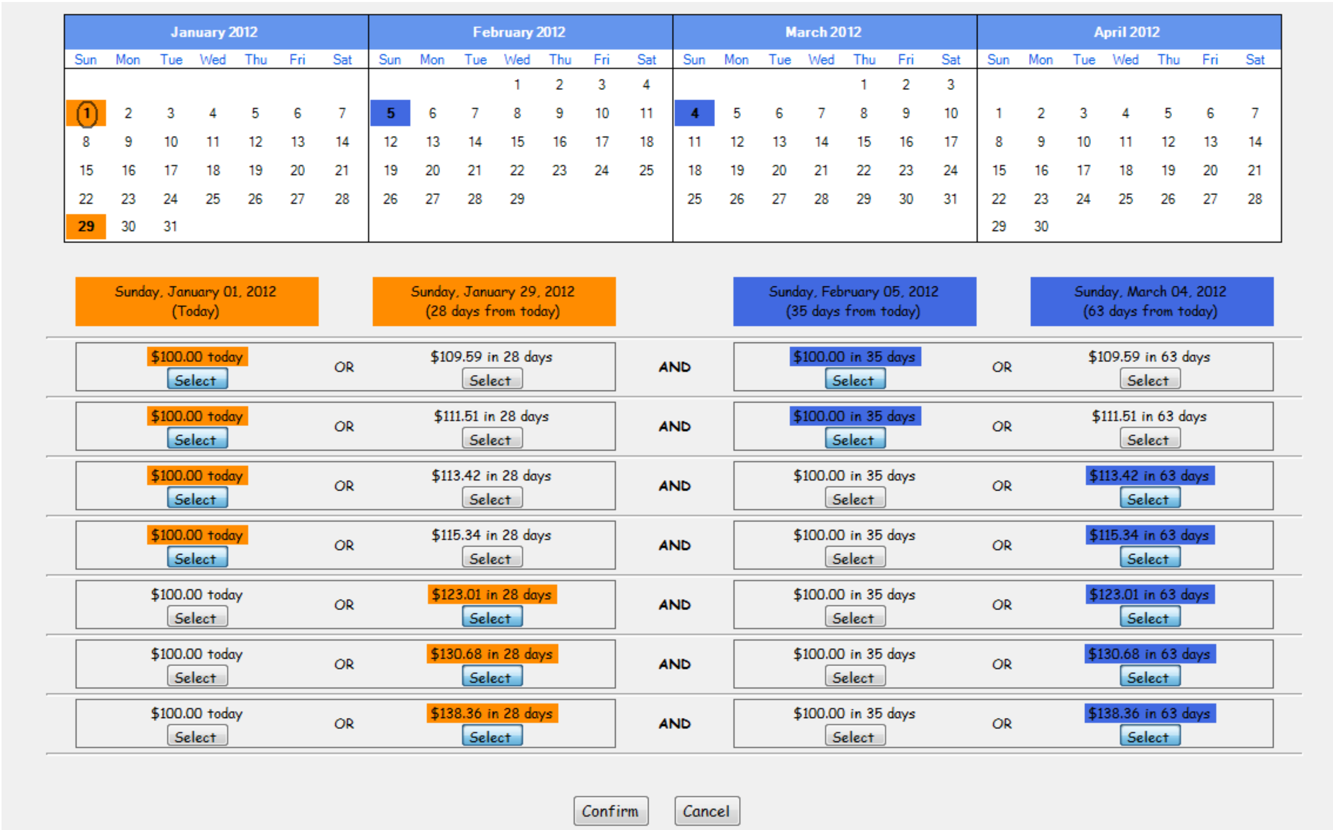
In this task you will make a number of choices between receiving an amount of money on a "sooner" date or a different amount of money on a "later" date. You will be presented with a series of decision <u>pairs</u> arranged in seven rows per decision screen. Each decision consists of a smaller amount that will be paid sooner and a larger amount that will be paid at a later date. The two pairs in a row involve the same amounts but different dates. An example of a decision screen is shown below. The dates and amounts in your task will differ. You will make all decisions on a computer.



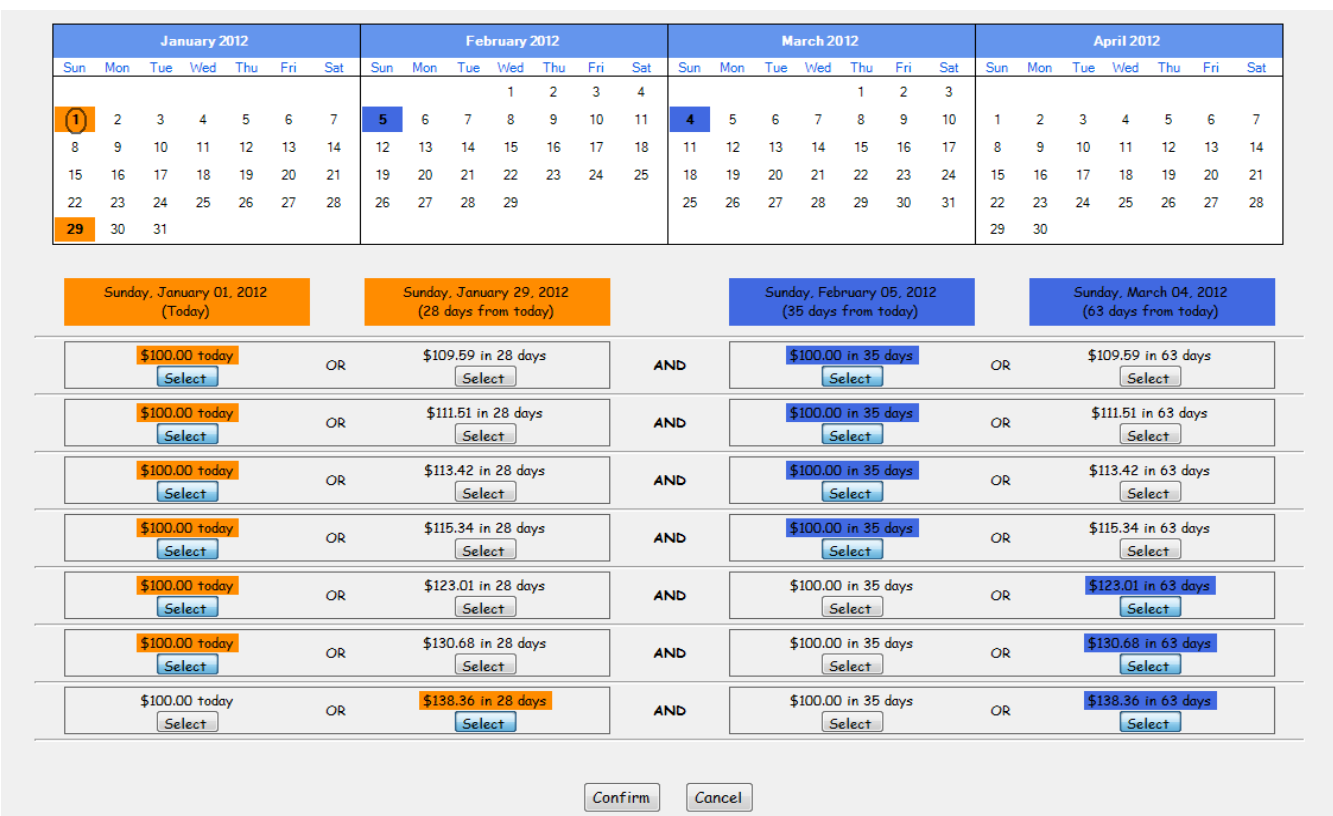
<u>This screen shows seven pairs of independent decisions</u>. Each decision pair is presented on a different row. All decision pairs have the same format. For the purpose of explaining this task, assume for the moment that today is January 1, 2012. In the calendar, today's date is enclosed in a black circle. Let's look at the first decision pair in the example above (the one on the first decision row). In the first decision, the sooner choice pays $100 today, January 1, 2012 in the example, and the later choice pays $109.59 in twenty-eight days from today. The second decision in this row offers the choice between $100 in thirty-five days or $109.59 in sixty-three days from today.

If you choose the sooner option for one decision in a <u>pair</u>, you are free to choose the sooner or later option for the other decision in that pair. And if you choose the later option for one decision in a <u>pair</u>, you are free to choose the sooner or later option for the other decision in that pair. In other words, it is possible to choose the sooner option in one decision and the later option in the other decision of the same pair, to choose the later option in one decision and the sooner option the other decision of the same pair, or to choose the same option in each decision of the same pair. You choose by clicking the button under the alternative you prefer.

The above screen example shows a situation in which someone chose the same options in each pair. Here is a screen example in which someone chose differently for the 3rd and 4th decision rows:



And here is an example in which someone chose differently for the 5th and 6th decision rows:



We will present you with ten of these decision screens, with each screen displaying seven decision pairs and fourteen choices for you to make. You must make all fourteen choices on each decision screen before moving to the next decision screen. While on a single decision screen, the only difference between decision pairs is that the dollar amounts of the future payment will change. However, pairs on different decision screens will have different dollar amounts and future payment dates. So, you should make sure to pay attention to both the changing dollar amounts and changing dates as you make your decisions.

After you have worked through all of the decisions, we will select one of your ten decision screens by rolling a 10-sided die. Then we will roll a 10-sided die again, until a number between 1 and 7 comes up, to pick one decision row on that screen. This means you will be paid for both choices that you made in that pair. When you make your choices you will not know which decision pair will be selected for payment. You should therefore treat each decision as if it might actually count for payment.

Once the decision screen and row are selected, we will look at the specific choices you made and the payouts for those choices. If the individual payouts in the selected decision row are smaller (less than $100), you will actually be paid both amounts. However, if the individual payouts in the selected decision row are larger ($100 or greater), you will roll a 10-sided die to determine whether or not you are actually paid these amounts. If you roll a 1, you will actually be paid the amounts, and on the dates that you chose to receive them. If you roll a number other than 1, you will earn nothing in this task. This roll will be at the end of the experiment, and in private, when you are being paid.

For instance, suppose the decision screen in the first screen example was selected and you preferred the sooner date in the first four rows and the later date in the last three rows, as shown above. You would then roll a 10-sided die, until a number between 1 and 7 comes up, to select the row. Suppose the outcome was 5 and the fifth row is selected. In this row you preferred a payment of $123.01 in twenty-eight days in the first decision and $123.01 in sixty-three days in the second decision. Since this payment is $100 or greater, you would roll the 10-sided die again, at the end of the experiment. If the outcome of your die-roll is 1, you will be paid $123.01 in twenty-eight days and another $123.01 in sixty-three days. However, if the outcome is 3, or anything other than 1, you will get nothing.

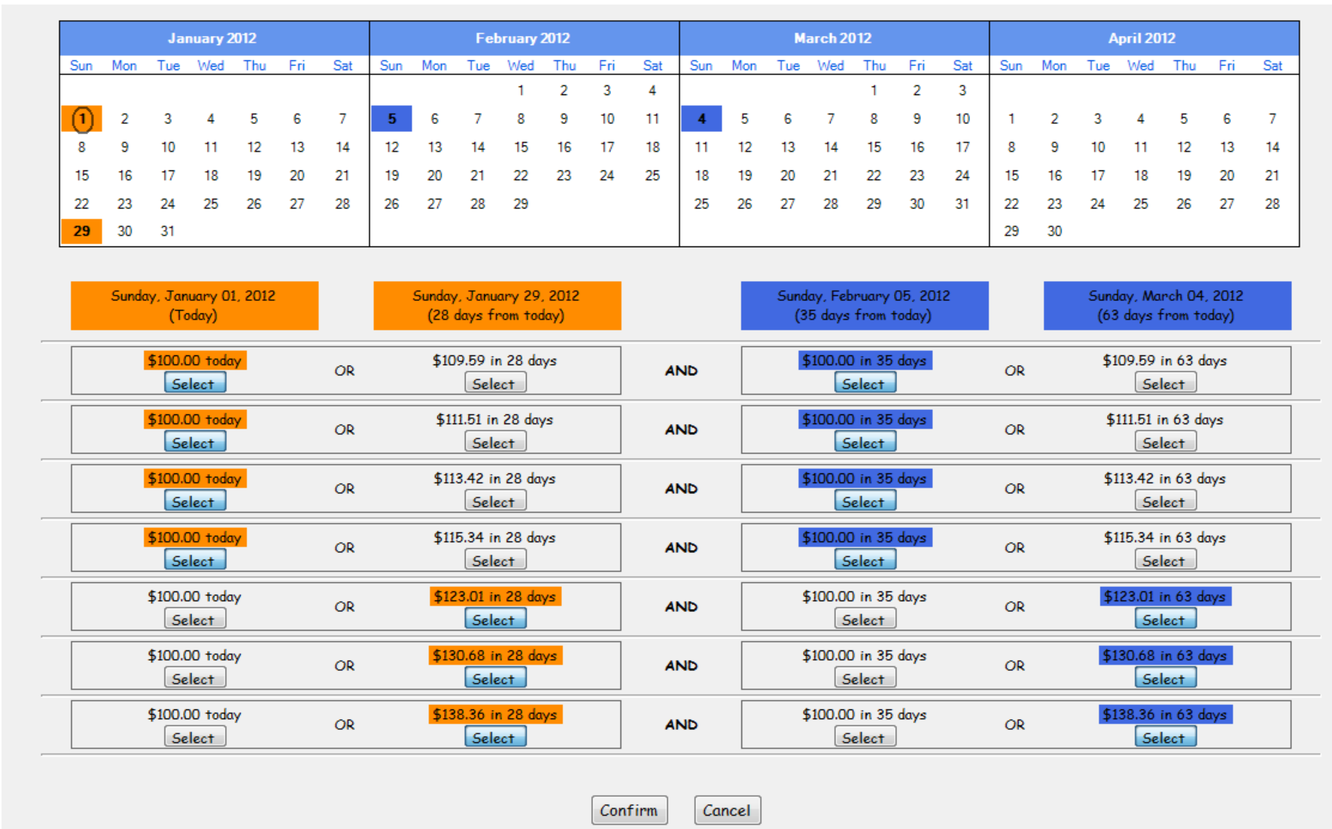
You will receive the money on the date stated in your preferred option. We will pay you using *PayPal*, which is an online payment service. We will explain more about *PayPal* in a few minutes. If you receive some money to be paid in the future you will also receive a written confirmation from Professor Harrison which guarantees that the money is to be paid to you on that date.

The money you receive from these choices is in addition to the show-up fee of $5, which is paid out at the end of the experiment as cash.

B_T5

Sooner versus Later Payments

In this task you will make a number of choices between receiving an amount of money on a "sooner" date or a different amount of money on a "later" date. You will be presented with a series of decision <u>pairs</u> arranged in seven rows per decision screen. Each decision consists of a smaller amount that will be paid sooner and a larger amount that will be paid at a later date. The two pairs in a row involve the same amounts but different dates. An example of a decision screen is shown below. The dates and amounts in your task will differ. You will make all decisions on a computer.



<u>This screen shows seven pairs of tied-together decisions</u>. Each decision <u>pair</u> is presented on a different row. All decision pairs have the same format. For the purpose of explaining this task, assume for the moment that today is January 1, 2012. In the calendar, today's date is enclosed in a black circle. Let's look at the first decision pair in the example above (the one on the first decision row). The first sooner choice pays $100 today, January 1, 2012 in the example, and the first later choice pays $109.59 in twenty-eight days from today. The second sooner choice pays $100 in thirty-five days from today and the second later choice pays $109.59 in sixty-three days from today.

If you choose the sooner option for one decision in a <u>pair</u>, you are also choosing the sooner option for the other decision in that pair. In other words, it is not possible to choose the sooner option in one decision and the later option in the other decision of the same pair. If you select the sooner options in one pair, you are free to select the later options in any other pair. You choose by clicking the button under the alternative you prefer.

We will present you with ten of these decision screens, with each screen displaying seven choices for you to make. You must make all seven choices on each decision screen before moving to the next decision screen. While on a single decision screen, the only difference between decision pairs is that the dollar amounts of the future payment will change. However, pairs on different decision screens will have different dollar amounts and future payment dates. So, you should make sure to pay attention to both the changing dollar amounts and changing dates as you make your decisions.

After you have worked through all of the decisions, we will select one of your ten decision screens by rolling a 10-sided die. Then we will roll a 10-sided die again, until a number between 1 and 7 comes up, to pick one decision row on that screen. This means you will be paid for both of the choices in that pair, which you made at the same time. When you make your choices you will not know which decision pair will be selected for payment. You should therefore treat each decision as if it might actually count for payment.

Once the decision screen and row are selected, we will look at the specific choice you made and the resulting payouts. If the individual payouts in the selected decision row are smaller (less than $100), you will actually be paid both amounts. However, if the individual payouts in the selected decision row are larger ($100 or greater), you will roll a 10-sided die to determine whether or not you are actually paid these amounts. If you roll a 1, you will actually be paid the amounts, and on the dates that you chose to receive them. If you roll a number other than 1, you will earn nothing in this task. This roll will be at the end of the experiment, and in private, when you are being paid.

For instance, suppose the decision screen in the above example was selected and you preferred the sooner date in the first four rows and the later date in the last three rows, as shown above. You would then roll a 10-sided die, until a number between 1 and 7 comes up, to select the row. Suppose the outcome was 5 and the fifth row is selected. In this row you preferred a payment of $123.01 in twenty-eight days in the first decision and, consequently, $123.01 in sixty-three days in the second decision. Since this payment is $100 or greater, you would roll the 10-sided die again, at the end of the experiment. If the outcome of your die-roll is 1, you will be paid $123.01 in twenty-eight days and another $123.01 in sixty-three days. However, if the outcome is 3, or anything other than 1, you will get nothing.

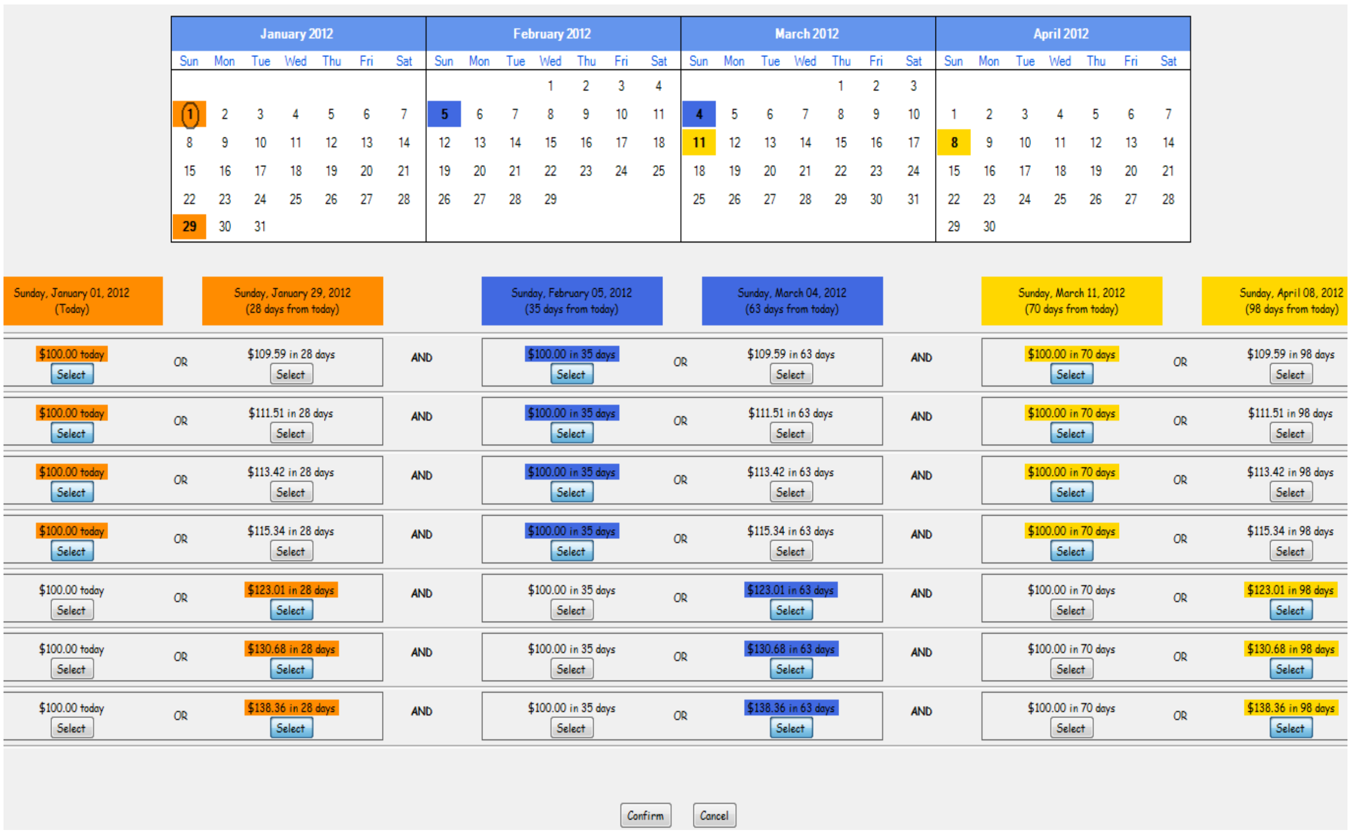
You will receive the money on the date stated in your preferred option. We will pay you using *PayPal*, which is an online payment service. We will explain more about *PayPal* in a few minutes. If you receive some money to be paid in the future you will also receive a written confirmation from Professor Harrison which guarantees that the money is to be paid to you on that date.

The money you receive from these choices is in addition to the show-up fee of $5, which is paid out at the end of the experiment as cash.

B_T6

Sooner versus Later Payments

In this task you will make a number of choices between receiving an amount of money on a "sooner" date or a different amount of money on a "later" date. You will be presented with a series of decision triples arranged in seven rows per decision screen. Each decision consists of a smaller amount that will be paid sooner and a larger amount that will be paid at a later date. The three triples in a row involve the same amounts but different dates. An example of a decision screen is shown below. The dates and amounts in your task will differ. You will make all decisions on a computer.

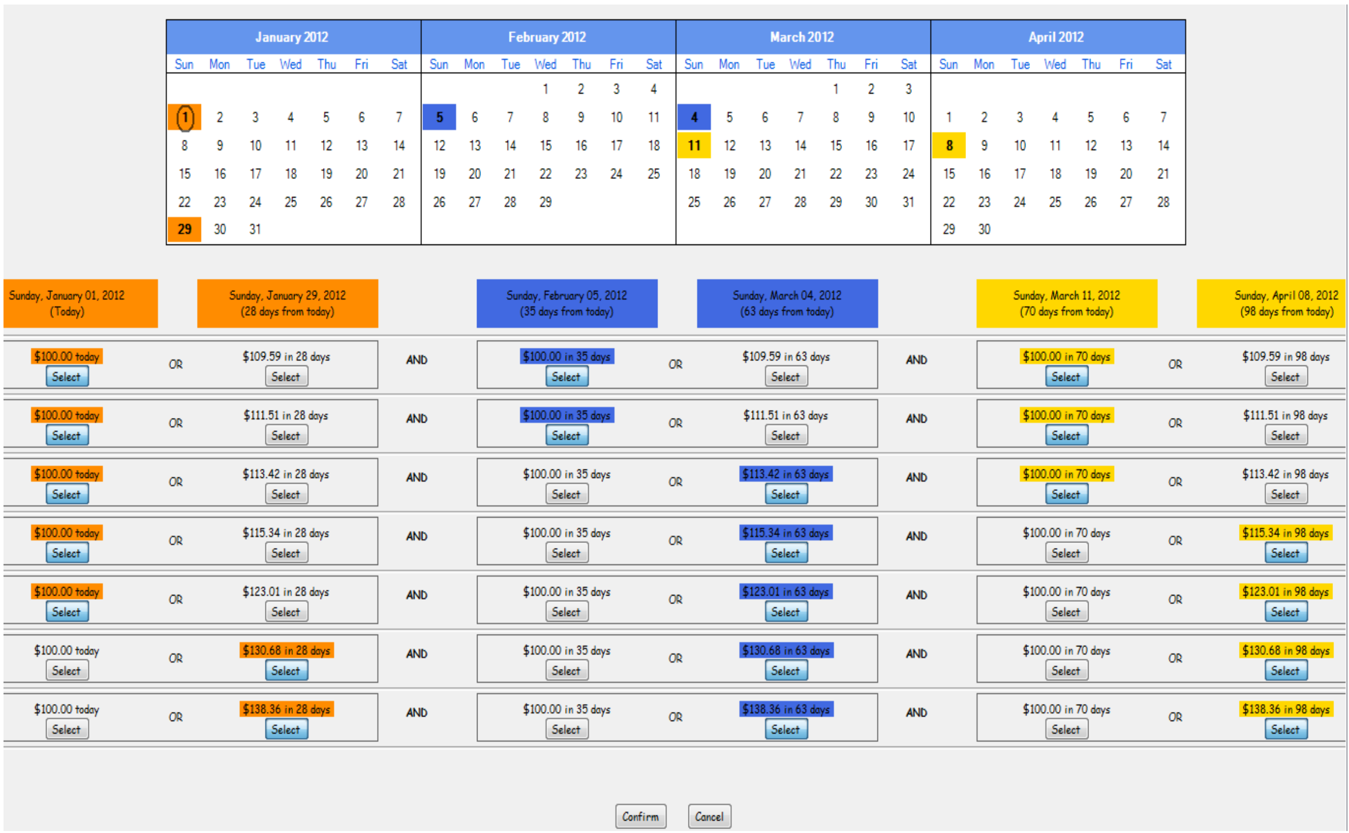


<u>This screen shows seven triples of independent decisions</u>. Each decision triple is presented on a different row. All decision triples have the same format. For the purpose of explaining this task, assume for the moment that today is January 1, 2012. In the calendar, today's date is enclosed in a black circle. Let's look at the first decision triple in the example above (the one on the first decision row). In the first decision, the sooner choice pays $100 today, January 1, 2012 in the example, and the later choice pays $109.59 in twenty-eight days from today. The second decision in this row offers the choice between $100 in thirty-five days or $109.59 in sixty-three days from today. The third decision in this row offers the choice between $100 in seventy days or $109.59 in ninety-eight days from today.

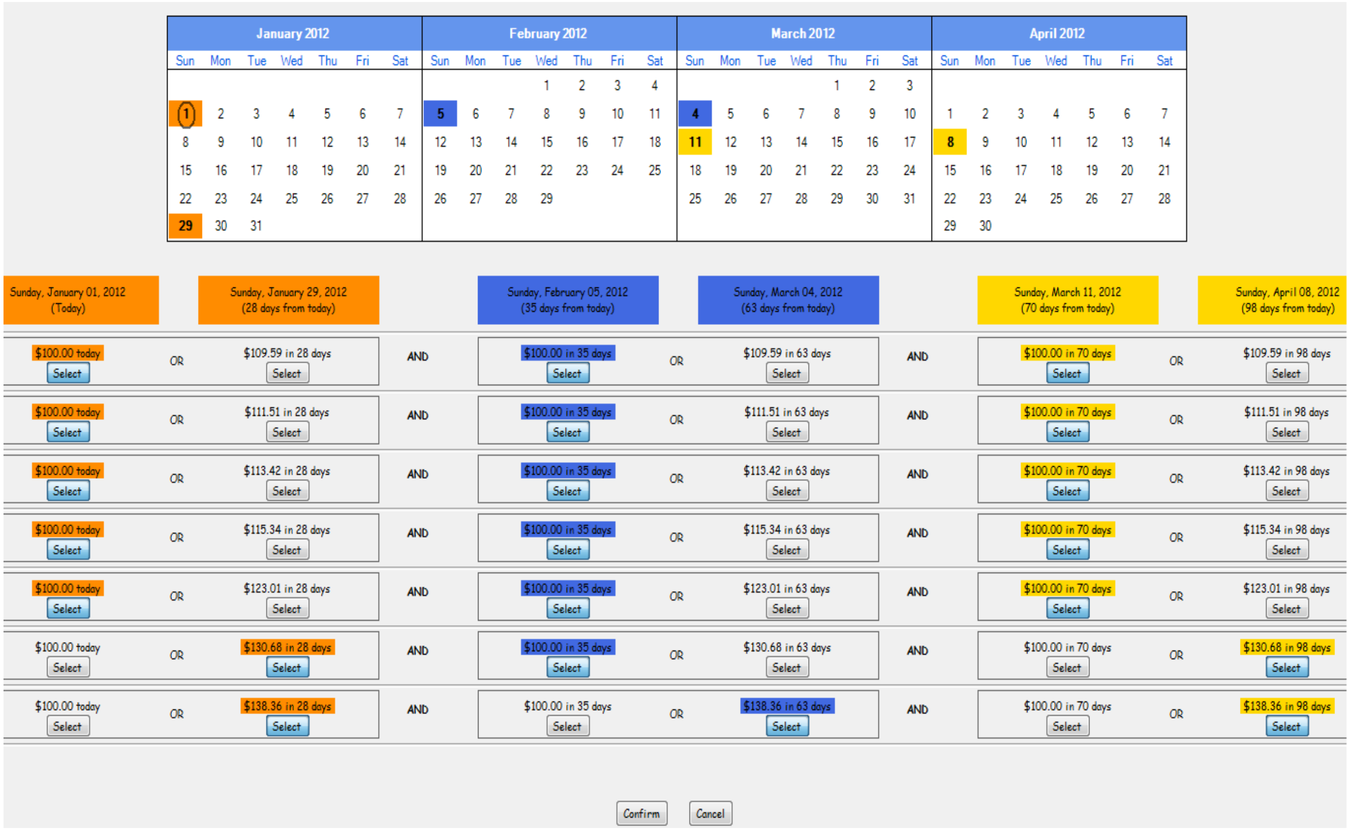
We will present you with ten of these decision screens, with each screen displaying seven decision <u>triples</u> and twenty-one choices for you to make. You must make all twenty-one choices on each decision screen before moving to the next decision screen. While on a single decision screen, the only difference between decision triples is that the dollar amounts of the future payment will change. However, triples on different decision screens will have different dollar amounts and future payment dates. So, you should make sure to pay attention to both the changing dollar amounts and changing dates as you make your decisions.

If you choose the sooner option for one decision in a triple, you are free to choose the sooner or later option for the other two decisions in that triple. And if you choose the later option for one decision in a triple, you are free to choose the sooner or later option for the other two decisions in that triple. In other words, it is possible to choose the sooner option in one decision and the later option in any of the other two decisions of the same triple, to choose the later option in one decision and the sooner option in any of the other two decisions of the same triple, or to choose the same option in each decision of the same triple. You choose by clicking the button under the alternative you prefer.

The above screen example shows a situation in which someone chose the same options in each triple. Here is a screen example in which someone chose differently for the 3rd and 4th decision rows:



And here is an example in which someone chose differently for the 5th and 6th decision rows:



After you have worked through all of the decisions, we will select one of your ten decision screens by rolling a 10-sided die. Then we will roll a 10-sided die again, until a number between 1 and 7 comes up, to pick one decision row on that screen. This means you will be paid for all three choices that you made in that triple. When you make your choices you will not know which decision triple will be selected for payment. You should therefore treat each decision as if it might actually count for payment.

Once the decision screen and row are selected, we will look at the specific choices you made and the payouts for those choices. If the individual payouts in the selected decision row are smaller (less than $100), you will actually be paid all three amounts. However, if the individual payouts in the selected decision row are larger ($100 or greater), you will roll a 10-sided die to determine whether or not you are actually paid these amounts. If you roll a 1, you will actually be paid the amounts, and on the dates that you chose to receive them. If you roll a number other than 1, you will earn nothing in this task. This roll will be at the end of the experiment, and in private, when you are being paid.

For instance, suppose the decision screen in the first screen example was selected and you preferred the sooner date in the first four rows and the later date in the last three rows, as shown above. You would then roll a 10-sided die, until a number between 1 and 7 comes up, to select the row. Suppose the outcome was 5 and the fifth row is selected. In this row you preferred a payment of $123.01 in twenty-eight days in the first decision, $123.01 in sixty-three days in the second decision, and $123.01 in ninety-eight days in the third decision. Since this payment is $100 or greater, you would roll the 10-sided die again, at the end of the experiment. If the outcome of your die-roll is 1, you will be paid $123.01 in twenty-eight days, $123.01 in sixty-three days, and another $123.01. However, if the outcome is 3, or anything other than 1, you will get nothing.

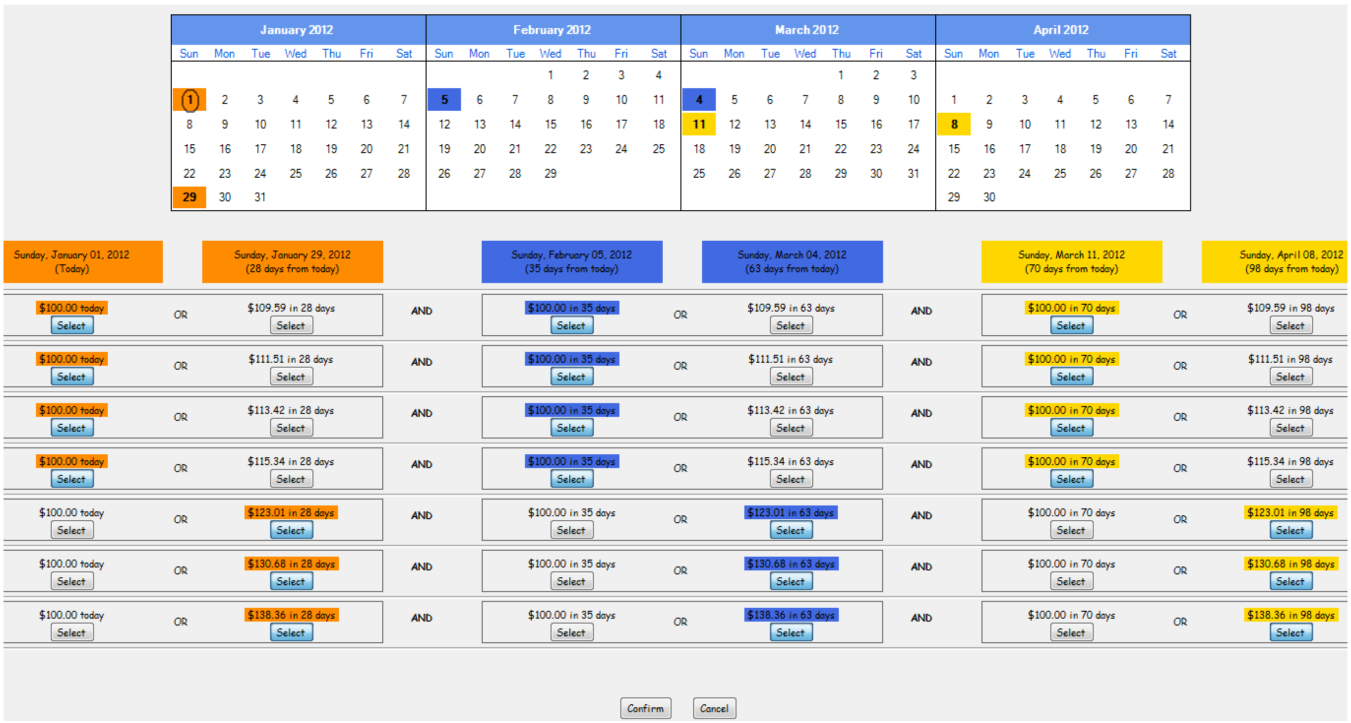
You will receive the money on the date stated in your preferred option. We will pay you using *PayPal*, which is an online payment service. We will explain more about *PayPal* in a few minutes. If you receive some money to be paid in the future you will also receive a written confirmation from Professor Harrison which guarantees that the money is to be paid to you on that date.

The money you receive from these choices is in addition to the show-up fee of $5, which is paid out at the end of the experiment as cash.

B_T7

Sooner versus Later Payments

In this task you will make a number of choices between receiving an amount of money on a "sooner" date or a different amount of money on a "later" date. You will be presented with a series of decision triples arranged in seven rows per decision screen. Each decision consists of a smaller amount that will be paid sooner and a larger amount that will be paid at a later date. The three triples in a row involve the same amounts but different dates. An example of a decision screen is shown below. The dates and amounts in your task will differ. You will make all decisions on a computer.



<u>This screen shows seven triples of tied-together decisions</u>. Each decision triple is presented on a different row. All decision triples have the same format. For the purpose of explaining this task, assume for the moment that today is January 1, 2012. In the calendar, today's date is enclosed in a black circle. Let's look at the first decision triple in the example above (the one on the first decision row). In the first decision, the sooner choice pays $100 today, January 1, 2012 in the example, and the later choice pays $109.59 in twenty-eight days from today. The second decision in this row offers the choice between $100 in thirty-five days or $109.59 in sixty-three days from today. The third decision in this row offers the choice between $100 in seventy days or $109.59 in ninety-eight days from today.

If you choose the sooner option for one decision in a <u>triple</u>, you are also choosing the sooner option for the other two decisions in that triple. In other words, it is not possible to choose the sooner option in one decision and the later option in the other decisions of the same triple. If you select the sooner options in one triple, you are free to select the later options in any other triple. You choose by clicking the button under the alternative you prefer.

We will present you with ten of these decision screens, with each screen displaying seven choices for you to make. You must make all seven choices on each decision screen before moving to the next decision screen. While on a single decision screen, the only difference between decision triples is that the dollar amounts of the future payment will change. However, triples on different decision screens will have different dollar amounts and future payment dates. So, you should make sure to pay attention to both the changing dollar amounts and changing dates as you make your decisions.

After you have worked through all of the decisions, we will select one of your ten decision screens by rolling a 10-sided die. Then we will roll a 10-sided die again, until a number between 1 and 7 comes up, to pick one decision row on that screen. This means you will be paid for all three choices in that triple, which you made at the same time. When you make your choices you will not know which decision triple will be selected for payment. You should therefore treat each decision as if it might actually count for payment.

Once the decision screen and row are selected, we will look at the specific choices you made and the resulting payouts. If the individual payouts in the selected decision row are smaller (less than $100), you will actually be paid all three amounts. However, if the individual payouts in the selected decision row are larger ($100 or greater), you will roll a 10-sided die to determine whether or not you are actually paid these amounts. If you roll a 1, you will actually be paid the amounts, and on the dates that you chose to receive them. If you roll a number other than 1, you will earn nothing in this task. This roll will be at the end of the experiment, and in private, when you are being paid.

For instance, suppose the decision screen in the above example was selected and you preferred the sooner date in the first four rows and the later date in the last three rows, as shown above. You would then roll a 10-sided die, until a number between 1 and 7 comes up, to select the row. Suppose the outcome was 5 and the fifth row is selected. In this row you preferred a payment of $123.01 in twenty-eight days in the first decision and, consequently, $123.01 in sixty-three days in the second decision, and $123.01 in ninety-eight days in the third decision. Since this payment is $100 or greater, you would roll the 10-sided die again, at the end of the experiment. If the outcome of your die-roll is 1, you will be paid $123.01 in twenty-eight days, $123.01 in sixty-three days, and another $123.01. However, if the outcome is 3, or anything other than 1, you will get nothing.

You will receive the money on the date stated in your preferred option. We will pay you using *PayPal*, which is an online payment service. We will explain more about *PayPal* in a few minutes. If you receive some money to be paid in the future you will also receive a written confirmation from Professor Harrison which guarantees that the money is to be paid to you on that date.

The money you receive from these choices is in addition to the show-up fee of $5, which is paid out at the end of the experiment as cash.

Appendix B: Structural Econometric Specification

Estimates of discounting parameters depend crucially on the curvature of the utility function, as stressed by Andersen, Harrison, Lau and Rutström [2008]. It follows from Jensen's Inequality that estimating a more concave curvature of the instantaneous utility function results in lower inferred discount rates for the same observed choice data. This theoretical insight has important design and econometric implications. With respect to experimental design, it suggests the use of separate tasks for the elicitation of risk attitudes and time preferences. We decided, instead, to use choice data that had been previously collected from samples drawn from the same population of GSU undergraduate students. Since the subjects are drawn from the same population one may assume that they are comparable and condition the discounting parameter estimates from one sample on the risk coefficient estimates from the other sample.[1] Econometrically, the interrelation of risk and time preferences implies their *joint estimation* via full-information maximum likelihood.

Assume that the utility of income from an experimental lottery choice task is defined by the following constant relative risk aversion (CRRA) specification:

$$U(x) = x^{(1-r)}/(1-r), \tag{B1}$$

where x is the lottery prize and r represents a coefficient that indicates the level of constant relative risk aversion. With this specification $r = 0$ describes risk-neutrality, $r < 0$ corresponds to risk-loving preferences, and $r > 0$ corresponds to risk-averse preferences.

When an estimate of r is considered in the ML iteration process, one can calculate the expected utility of a typical lottery i. If lottery i has j possible outcomes, its EU is given by

$$EU_i = \sum_j p(x_j)\, U(x_j) \tag{B2}$$

Then, for each decision pair an index is calculated that indicates the difference in the expected utility of both lotteries in a decision pair. Formally,

[1] When the likelihood of the discounting parameter is evaluated, conditional on risk parameters, nothing informs the statistical package that the choices over risky lotteries and the choices over SS/LL pairs were made by the same person. One can establish a connection in standard errors, by clustering by the subjects' ID numbers. However, the clustering over risky choices does not have to match the clustering over time-dated options. So these choices do not have to come from the same sample, although they should ideally come from the same population: see Coller, Harrison, and Rutström [2012; p.383-384].

$$\Delta EU = EU_L - EU_R, \tag{B3}$$

where EU_L is the "left" lottery and EU_R is the "right" lottery in a decision pair as presented to subjects. The function that links the latent index in (B3) to observed choice behavior is the cumulative density function (cdf) of the univariate normal distribution $\Phi(\cdot)$, resulting in a probit model. The probability of choosing the "right" lottery can be written

$$prob(choose\ R) = \Phi(\Delta EU). \tag{B4}$$

Thus the latent index in (B3) is linked to the observed choices by making the statement that lottery R is chosen, when the $\Delta EU > 0.5$.

This basic approach can be extended in several ways. An important addition is accounting for behavioral errors. The structural probit model cannot predict individual decision making with certainty. Decision makers may deviate from their true underlying preferences for a variety of reasons. Behavioral error specifications can account for various error sources, ranging from random deviations due to attention lapses to systematic violations related to the psychology of perception and judgment. A particularly influential behavioral error specification is due to Fechner [1860]. Its application to the evaluation of risky prospects was later popularized by Hey and Orme [1994]. The inclusion of the Fechner error specification expands the latent index in (B3) to

$$\Delta EU = (EU_L - EU_R)/\mu \tag{B5}$$

where the new parameter μ allows the otherwise deterministic EUT model to account for deviations from the underlying preference structure.

Wilcox [2008][2011] suggests an additional characterization of behavioral errors, called "contextual utilty." The intuition behind *contextual utility* originates from psychological experiments on signal detection and stimulus discrimination. These studies discovered that errors became more likely as the range of possible stimuli increase. Contextual utility respects this observation, by assuming that evaluative errors increase with the perceived range of outcomes. Econometrically, this implies that the standard deviation of the behavioral error is proportional to the range of utilities of the outcomes in a lottery pair. The contextual error specification is given by

$$\Delta EU = (EU_L - EU_R/\nu)/\mu \tag{B6}$$

where the new parameter ν is defined as the maximum utility over all outcomes minus the minimum utility over all outcomes in the lottery pair, i.e., over the *context* of that pair. This specification has a normalizing effect on the latent index, which remains in the unit interval. The contextual error specification is particularly parsimonious, since the parameter ν is defined by data, so that no additional parameter estimation is required. The specification also allows for inferences regarding "stochastically more risk averse" relationships. The latter refers to a stochastic notion of the familiar Arrow-Pratt metric of risk aversion. A *stochastically* risk averse subject is "on average" risk averse, but the metric is flexible enough to deal with choices that deviate from the subject's general risk aversion. With the latent index remaining within the bounds of the unit interval, one can compare the stochastic risk aversion of subjects who choose in dramatically different decision contexts (i.e. who face lotteries with very different prizes).

Once the parameters of interest are defined, structural estimation can be undertaken. The log-likelihood function is

$$LL^{EUT}(r, \mu; y, X) = \sum_i \begin{bmatrix} \left(ln\Phi(\Delta\text{EU}) \times \text{I}(y_i = 1)\right) + \\ \left(\ln\left(1 - \Phi(\Delta EU)\right) \times I(y_i = -1)\right) \end{bmatrix}, \tag{B7}$$

where the indicator function $I(\cdot)$ signifies whether the right $(y_i = 1)$ or the left $(y_i = -1)$ lottery is chosen. The parameters r and μ indicate the CRRA coefficient and the Fechner error term, respectively. The parameters can in principle be conditioned on a vector X of demographic characteristics. It is useful to constrain the parameter μ to be greater than zero.

People may not necessarily behave as if given probabilities affect their lottery evaluations with objective values. Instead, they may distort these probabilities in their perception – a process that can be described by attaching subjective weights to probabilities. However, early treatments of subjective decision weights by Edwards [1962] and Kahneman and Tversky [1979] resulted in (allegedly) implausible violations of first-order stochastic dominance. These difficulties are avoided by Rank Dependent Utility theory (RDU), due to Quiggin [1982], which derives probability weights from the entire distribution over ranked outcomes, not from individual probabilities. The resulting decision weights reflect the subjective distortion of objective probabilities. The RDU model, which nests the EUT model, is considered as an alternative data generating process (DGP) for the risk preference data.[2] This requires the introduction of a probability weighting function. A variety of weighting functions have been proposed in the literature, primarily by Quiggin [1982], Tversky and Kahneman [1992] and Prelec [1998].

Prelec [1998] contributes a flexible two parameter specification of probability weighting:

$$\omega(p) = \exp[-\eta(-\ln p)^{\phi}], \tag{B8}$$

with $\eta > 0$ and $\phi > 0$. This weighting function is derived from several axioms that reflect apparent regularities of probability weighting. The specification requires the estimation of two parameters η and ϕ. The log-likelihood function is then

$$LL^{RDU}(r, \eta, \phi, \mu; y, X) = \sum_i \left[\begin{array}{c} \left(ln\Phi(\Delta\text{RDU}) \times \text{I}(y_i = 1)\right) + \\ \left(\ln\left(1 - \Phi(\Delta RDU)\right) \times I(y_i = -1)\right) \end{array} \right]. \tag{B9}$$

There is no probability weighting when $\eta = \phi = 1$.

The estimation of discounting behavior requires joint estimation of risk and time preferences. Suppose the SS amount is available at time t, whereas the LL amount is delivered at time $t + \tau$. If exponential discounting holds, the present value of option SS is given by

$$PV_{SS}^{E} = (1/(1+\delta)^{t})\frac{(\upsilon+SS)^{(1-r)}}{1-r} + (1/(1+\delta)^{t+\tau})\frac{\upsilon^{(1-r)}}{1-r}, \tag{B10}$$

where δ is the discount rate and υ denotes some measure of background consumption. Expression (B10) says that the present value of option SS is the discounted utility of receiving the amount SS, integrated with the background consumption υ, at time t and the discounted utility of receiving nothing beyond the utility of background consumption at time $t + \tau$. Utility is described using the CRRA specification from (B4). Similarly, conditional on exponential discounting being the true latent process, the present value of option LL is given by

$$PV_{LL}^{E} = \left(\frac{1}{(1+\delta)^{t}}\right)\frac{\upsilon^{(1-r)}}{(1-r)} + \left(\frac{1}{(1+\delta)^{t+\tau}}\right)\frac{(LL+\upsilon)^{(1-r)}}{(1-r)} \tag{B11}$$

[2] There is, of course, a wealth of other models of choice under risk that could have been considered. A popular alternative that does not nest EUT of RDU is Prospect Theory, but several other models have been proposed by behavioral economists: see Starmer [2000] for a review. Cognitive psychologists have contributed various process models; Johnson and Busemeyer [2010] provides an overview of the literature from a psychological perspective.

A latent index, formally equivalent to the index considered in the risk aversion model, is defined as:

$$\Delta PV^{E} = \frac{PV_{LL}^{E} - PV_{SS}^{E}}{\nu}, \tag{B12}$$

where the parameter ν denotes the behavioral error specification.

The estimation maximizes the following conditional log-likelihood function:

$$LL^{E}(r, \eta, \phi, \delta, \mu, \nu; y, \upsilon, X) = \sum_i \begin{bmatrix} \left(ln\Phi(\Delta PV^{E}) \times I(y_i = 1)\right) + \\ (\ln(1 - \Phi(\Delta PV^{E})) \times I(y_i = -1)) \end{bmatrix}, \tag{B13}$$

where $y_i = 1$ and $y_i = -1$ denote the choice of LL and SS, respectively. The vector X includes observable demographic characteristics. The joint log-likelihood function, denoted LL^{RDU_E} , can then be written as

$$LL^{RDU\_E}(r, \eta, \phi, \delta, \mu, \nu; y, \upsilon, X) = LL^{RDU} + LL^{E} \tag{B14}$$

where LL^{RDU} is the aggregate log-likelihood for risk preference choices, assuming RDU with a Prelec probability weighting function, and LL^{E} is the aggregate log-likelihood for time preference choices, assuming exponential discounting.

Structural estimation is performed in a similar fashion when alternative discounting models are studied. Consider, for example, hyperbolic discounting as specified in (4). The present value of SS is then

$$PV_{SS}^{H2} = \left(\frac{1}{1+Kt}\right)\frac{(\upsilon+SS)^{(1-r)}}{(1-r)} + \left(\frac{1}{1+K(t+\tau)}\right)\frac{\upsilon^{(1-r)}}{1-r}. \tag{B15}$$

Similarly, the present value of LL is given by

$$PV_{LL}^{H2} = \left(\frac{1}{1+Kt}\right)\frac{\upsilon^{(1-r)}}{(1-r)} + \left(\frac{1}{1+K(t+\tau)}\right)\frac{(LL+\upsilon)^{(1-r)}}{(1-r)}. \tag{B16}$$

The latent index is then

$$\Delta PV^{H2} = \frac{PV_{LL}^{H2} - PV_{SS}^{H2}}{\eta}. \tag{B17}$$

The log-likelihood function of the hyperbolic discounting model is given by

$$LL^{H2}(r, \eta, \phi, K, \mu, \nu; y, \upsilon, X) = \sum_i \begin{bmatrix} \left(ln\Phi(\Delta PV^{H2}) \times I(y_i = 1)\right) + \\ \left(\ln\left(1 - \Phi(\Delta PV^{H2})\right) \times I(y_i = -1)\right) \end{bmatrix}. \tag{B18}$$

The joint log-likelihood function is

$$LL^{RDU\_H2}(r, \eta, \phi, K, \mu, \nu; y, \upsilon, X) = LL^{RDU} + LL^{H2}. \tag{B19}$$

One can expect that impulsive and controlled processes are both present in the population. Psychologists have contributed a rich literature on *dual process* models. The notion of dual cognitive processes has been applied to several areas in reasoning, social cognition, as well as judgment and decision making. The various models have in common that they assume that manifest behavior is determined by two modes of processing: an automatic process that is often associated with fast, unconscious, heuristic-driven, and impulsive cognition and a controlled process that is often associated with slow, conscious, deliberate, and sophisticated processing: see Barrett et al. [2004] and Evans [2008].

Different processes do not necessarily have to be reduced to a one-dimensional utility index, as most economic models suggest. An alternative strategy is exemplified by the SP/A model of Lopes [1987]: a psychological *dual criterion* approach to decision making under risk. This model proposes two criteria that people consult when evaluating lotteries. The security-potential criterion implies a probability weighting model, much like RDU. The aspiration criterion, on the other hand, introduces a reference point, stressed by Lopes and Oden [1995]. Both latent criteria are presumed to be employed independently but simultaneously.

Traditional economic models usually integrate those latent processes into a one-dimensional criterion. This is why they do not necessarily help us to understand how observed choices can result from the interplay of impulsive and controlled processes. By contrast, dual criteria models provide such a guideline. They allow for the possibility of multiple criteria for multiple decision processes, as noted by Andersen, Harrison, Lau and Rutström [2014a]. The relative contribution of each process to the observable outcome can be naturally estimated by a finite mixture model using maximum likelihood. This approach estimates the grand likelihood of the dual criteria model as the probability weighted average of the conditional likelihood estimates of each criterion being correct: for examples, see Harrison and Rutström [2009], Andersen, Harrison, Lau and Rutström [2014a] and Coller, Harrison and Rutström [2012].

Fortunately, accounting for multiple processes requires only a straightforward extension of the structural models considered so far. Suppose there are only two DGPs. Each individual process can be estimated as outlined above, where the likelihood of each outcome is the probability of its occurrence *conditional* on the assumed DGP being true. To assess the contribution of each process, a comprehensive overarching model is then constructed. With two individual models included, this involves the estimation of only one additional *mixture parameter*.

Suppose the choice data are generated by exponential *and* hyperbolic discounting behavior. Again, the specifications D^E and D^{H2} are used. The discounting parameters δ and K are jointly estimated with the risk parameter r from the CRRA specification in (B1). Using contextual utility, the behavioral error parameter μ is added. Similarly behavioral errors in each discounting model are captured by the parameter ν.[3] All those steps are identical to the ones that would be followed if each discounting model was estimated separately. However, a mixture model requires the additional step of writing a grand log-likelihood function, which is simply the sum of the probability weighted conditional likelihoods:

$$LL^{RDU\_E-H2}(r,\eta,\phi,\delta,K,\mu,\nu;y,\omega,X) = \sum_i \begin{matrix} \ln[\left(\pi^{RDU\_E} \times L_i^{RDU\_E}\right) + \\ \left((1-\pi^{RDU\_E}) \times L_i^{RDU\_H2}\right)] \end{matrix} \tag{B20}$$

The grand log-likelihood function LL^{RDU_E-H2} assumes that the latent discounting process is characterized in part by exponential and in part by hyperbolic discounting, and that risk preferences are consistent with RDU with a Prelec probability weighting function. It includes the new parameter π^E which is the probability of exponential discounting (with RDU consistent risk preferences) being the true model. Since only two latent processes are considered, the probability of hyperbolic discounting (with RDU consistent risk preferences) is necessarily (1-π^E).

Harrison and Rutström [2009; p.143-144] propose an interpretation of mixture models, according to which the mixture probability of a specific process is interpreted as the chance that the choice of a subject is consistent with this process. This interpretation has direct implications for the present experimental results: if reward bundling is effective, one can expect the mixture probability of controlled choice behavior, as described by exponential discounting, to be higher in bundling treatments.

[3] It is possible, and common, to use only one behavioral error term for all assumed discounting process in a mixture model. However, all mixture models reported here allowed for a separate behavioral error term for each discounting process. One reason for this approach is the expectation that separate error terms might facilitate the ML estimation by enhancing numerical stability. Moreover, there are also theoretical reasons for this approach: mixture models propose two or more data generating processes and it is perfectly possible that each of these processes is influenced by different behavioral errors. For the sake of simplicity, only one error term is mentioned during the introduction and theoretical discussion of mixture models.

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Appendix C: Experimental Parameters for Risk Preference Task

The column **qid** is the Question ID. Columns **pL1** through **pL4** refer to the probabilities for the 4 prizes of the left lottery; columns **xL1** through **xL4** refer to the monetary prizes of the left lottery, columns **pR1** through **pR4** refer to the probabilities for the 4 prizes of the right lottery; and columns **xR1** through **xR4** refer to the monetary prizes of the right lottery. The logic behind the selection of this battery is explained after the listing of parameters.

qid	pL1	pL2	pL3	pL4	xL1	xL2	xL3	xL4	pR1	pR2	pR3	pR4	xR1	xR2	xR3	xR4
allais1	0	1	0	0	0	5	0	0	.01	.89	.1	0	0	5	25	0
allais2	.89	.11	0	0	0	5	0	0	.9	0	.1	0	0	5	25	0
allais3	0	1	0	0	0	15	0	0	.01	.89	.1	0	0	15	75	0
allais4	.89	.11	0	0	0	15	0	0	.9	0	.1	0	0	15	75	0
camerer1	0	0	.2	.8	0	5	35	70	0	.1	0	.9	0	5	35	70
camerer10	0	.4	.6	0	0	5	35	70	0	.6	.2	.2	0	5	35	70
camerer11	0	.5	.4	.1	0	5	35	70	0	.7	0	.3	0	5	35	70
camerer12	0	.8	.2	0	0	5	35	70	0	.9	0	.1	0	5	35	70
camerer13	0	.2	.2	.6	0	5	35	70	0	.3	0	.7	0	5	35	70
camerer14	0	.6	.2	.2	0	5	35	70	0	.7	0	.3	0	5	35	70
camerer15	0	.65	.25	.1	0	5	35	70	0	.75	.05	.2	0	5	35	70
camerer16	0	.55	.3	.15	0	5	35	70	0	.65	.1	.25	0	5	35	70
camerer17	0	.35	.4	.25	0	5	35	70	0	.45	.2	.35	0	5	35	70
camerer18	0	.05	.55	.4	0	5	35	70	0	.15	.35	.5	0	5	35	70
camerer19	0	.05	.5	.45	0	5	35	70	0	.15	.3	.55	0	5	35	70
camerer2	0	0	.6	.4	0	5	35	70	0	.1	.4	.5	0	5	35	70
camerer20	0	.15	.3	.55	0	5	35	70	0	.25	.1	.65	0	5	35	70
camerer21	0	.05	.9	.05	0	5	35	70	0	.15	.7	.15	0	5	35	70
camerer22	0	.35	.3	.35	0	5	35	70	0	.45	.1	.45	0	5	35	70
camerer23	0	.45	.5	.05	0	5	35	70	0	.55	.3	.15	0	5	35	70
camerer3	0	0	.6	.4	0	5	35	70	0	.2	.2	.6	0	5	35	70
camerer4	0	.1	.4	.5	0	5	35	70	0	.3	0	.7	0	5	35	70
camerer5	0	0	1	0	0	5	35	70	0	.1	.8	.1	0	5	35	70
camerer6	0	0	1	0	0	5	35	70	0	.2	.6	.2	0	5	35	70
camerer7	0	.3	.4	.3	0	5	35	70	0	.5	0	.5	0	5	35	70
camerer8	0	.4	.2	.4	0	5	35	70	0	.5	0	.5	0	5	35	70
camerer9	0	.4	.6	0	0	5	35	70	0	.5	.4	.1	0	5	35	70
holt_laury1	0	.1	.9	0	0	20	16	0	0	.1	.9	0	0	38.5	1	0
holt_laury2	0	.2	.8	0	0	20	16	0	0	.2	.8	0	0	38.5	1	0
holt_laury3	0	.3	.7	0	0	20	16	0	0	.3	.7	0	0	38.5	1	0
holt_laury4	0	.4	.6	0	0	20	16	0	0	.4	.6	0	0	38.5	1	0

holt_laury5	0	.5	.5	0	0	20	16	0	0	.5	.5	0	0	38.5	1	0
holt_laury6	0	.6	.4	0	0	20	16	0	0	.6	.4	0	0	38.5	1	0
holt_laury7	0	.7	.3	0	0	20	16	0	0	.7	.3	0	0	38.5	1	0
holt_laury8	0	.8	.2	0	0	20	16	0	0	.8	.2	0	0	38.5	1	0
holt_laury9	0	.9	.1	0	0	20	16	0	0	.9	.1	0	0	38.5	1	0
lssM1	.2	.2	.4	.2	0	0	60	0	.2	.2	.4	.2	60	0	0	60
lssM1	.2	.2	.4	.2	0	60	25	0	.2	.2	.4	.2	25	25	0	65
lssM2	.2	.2	.4	.2	25	25	0	60	.2	.2	.4	.2	0	65	25	0
lssM2	.2	.2	.4	.2	60	25	0	60	.2	.2	.4	.2	0	65	25	0
lssP1	.22	.2	.38	.2	0	60	36	0	.22	.2	.38	.2	36	36	0	60
lssP2	.18	.2	.42	.2	36	36	0	60	.18	.2	.42	.2	0	60	60	0
lssP2	.18	.2	.42	.2	36	36	0	60	.18	.2	.42	.2	0	60	36	0
small_stakes1	.3	.1	.1	.5	18	34	34	38	.4	.1	.4	.1	13	46	46	49
small_stakes1	0	0	0	1	0	0	0	30	0	0	.5	.5	0	0	18	45
small_stakes1	0	0	0	1	0	0	0	30	0	0	.5	.5	0	0	12	49
small_stakes10	.4	.3	.1	.2	13	16	41	59	.3	.5	.1	.1	7	30	30	60
small_stakes10	0	0	0	1	0	0	0	20	0	0	.5	.5	0	0	2	39
small_stakes11	.1	.5	.1	.3	6	23	28	62	.2	.4	.3	.1	17	21	41	67
small_stakes11	0	0	0	1	0	0	0	80	0	0	.5	.5	0	0	62	99
small_stakes12	.2	.3	.2	.3	11	14	46	66	.1	.3	.4	.2	8	22	35	68
small_stakes12	0	0	0	1	0	0	0	80	0	0	.5	.5	0	0	65	96
small_stakes13	.2	.4	.3	.1	1	19	44	60	.3	.3	.2	.2	5	17	32	64
small_stakes13	0	0	0	1	0	0	0	80	0	0	.5	.5	0	0	70	91
small_stakes14	0	0	0	1	0	0	0	85	0	0	.5	.5	0	0	77	94
small_stakes14	.2	.4	.3	.1	2	50	52	64	.2	.3	.3	.2	7	34	59	68
small_stakes15	.4	.2	.3	.1	7	14	45	68	.4	.2	.3	.1	1	31	53	66
small_stakes15	0	0	0	1	0	0	0	75	0	0	.5	.5	0	0	62	90
small_stakes16	0	0	0	1	0	0	0	75	0	0	.5	.5	0	0	57	96
small_stakes16	.3	.3	.1	.3	0	14	30	64	.3	.3	.1	.3	5	13	43	67
small_stakes17	.1	.1	.6	.2	2	14	20	66	.2	.2	.3	.3	2	16	28	68
small_stakes17	0	0	0	1	0	0	0	75	0	0	.5	.5	0	0	60	93
small_stakes18	0	0	0	1	0	0	0	70	0	0	.5	.5	0	0	60	82
small_stakes18	.3	.1	.5	.1	2	32	43	67	.4	.2	.2	.2	9	22	61	62
small_stakes19	0	0	0	1	0	0	0	85	0	0	.5	.5	0	0	77	95
small_stakes19	.3	.2	.2	.3	9	12	50	62	.3	.3	.1	.3	10	15	16	67
small_stakes2	0	0	0	1	0	0	0	30	0	0	.5	.5	0	0	15	46
small_stakes2	.3	.3	.2	.2	21	27	35	42	.2	.3	.2	.3	13	26	41	53
small_stakes20	.1	.4	.4	.1	5	28	33	40	.2	.2	.2	.4	12	18	25	46
small_stakes20	0	0	0	1	0	0	0	70	0	0	.5	.5	0	0	58	85
small_stakes21	.3	.2	.1	.4	8	24	31	43	.4	.2	.3	.1	21	29	31	55
small_stakes21	0	0	0	1	0	0	0	130	0	0	.5	.5	0	0	112	149
small_stakes22	.1	.3	.2	.4	8	22	31	44	.3	.5	.1	.1	25	26	28	57
small_stakes22	0	0	0	1	0	0	0	130	0	0	.5	.5	0	0	115	146

small_stakes23	0	0	0	1	0	0	0	130	0	0	.5	.5	0	0	120	141
small_stakes23	.1	.1	.3	.5	6	31	34	37	.1	.3	.2	.4	8	22	31	44
small_stakes24	.3	.4	.1	.2	8	27	39	50	.2	.3	.2	.3	3	19	29	47
small_stakes24	0	0	0	1	0	0	0	135	0	0	.5	.5	0	0	127	144
small_stakes25	.2	.4	.3	.1	5	26	34	49	.1	.2	.1	.6	1	2	21	38
small_stakes25	0	0	0	1	0	0	0	125	0	0	.5	.5	0	0	112	140
small_stakes26	.2	.3	.2	.3	3	19	29	47	.3	.1	.5	.1	21	21	25	58
small_stakes26	0	0	0	1	0	0	0	125	0	0	.5	.5	0	0	107	146
small_stakes27	.3	.3	.1	.3	4	38	42	51	.1	.3	.5	.1	3	18	38	49
small_stakes27	0	0	0	1	0	0	0	125	0	0	.5	.5	0	0	110	143
small_stakes28	0	0	0	1	0	0	0	120	0	0	.5	.5	0	0	110	132
small_stakes28	.4	.3	.1	.2	7	36	47	56	.4	.1	.1	.4	9	10	28	53
small_stakes29	.5	.2	.2	.1	11	35	52	57	.2	.4	.3	.1	16	24	31	63
small_stakes29	0	0	0	1	0	0	0	135	0	0	.5	.5	0	0	127	145
small_stakes3	.2	.1	.4	.3	19	23	43	44	.3	.1	.3	.3	15	28	49	59
small_stakes3	0	0	0	1	0	0	0	20	0	0	.5	.5	0	0	10	31
small_stakes30	.3	.3	.1	.3	11	17	54	61	.3	.2	.3	.2	12	20	34	68
small_stakes30	0	0	0	1	0	0	0	120	0	0	.5	.5	0	0	108	135
small_stakes31	.3	.1	.2	.4	15	18	26	56	.3	.2	.2	.3	10	36	48	52
small_stakes32	.3	.1	.4	.2	17	23	42	58	.3	.2	.3	.2	8	35	44	52
small_stakes33	.1	.4	.1	.4	10	16	22	60	.4	.2	.2	.2	12	39	57	62
small_stakes34	.4	0	.3	.3	25	37	39	57	.3	.3	.2	.2	24	31	50	58
small_stakes35	.3	0	.4	.3	21	29	41	55	.2	.1	.4	.3	20	24	48	53
small_stakes36	0	.4	.5	.1	18	26	42	66	.2	.2	.1	.5	16	23	51	56
small_stakes37	.1	.3	.3	.3	14	18	52	52	0	.1	.8	.1	2	16	39	57
small_stakes38	.1	.3	.6	0	8	30	51	69	.4	.2	.2	.2	23	32	60	68
small_stakes39	.2	0	.4	.4	11	31	40	55	.3	.1	.3	.3	17	22	55	61
small_stakes4	.1	.3	.2	.4	13	13	48	63	.3	.5	.1	.1	25	26	28	57
small_stakes4	0	0	0	1	0	0	0	15	0	0	.5	.5	0	0	7	24
small_stakes40	.1	.4	0	.5	20	20	54	59	.3	.2	.1	.4	15	24	57	60
small_stakes5	.1	.1	.6	.2	2	14	20	66	.1	.4	.2	.3	22	29	33	69
small_stakes5	0	0	0	1	0	0	0	25	0	0	.5	.5	0	0	12	40
small_stakes6	.2	.1	.3	.4	8	16	52	59	.2	.4	.3	.1	22	24	38	44
small_stakes6	0	0	0	1	0	0	0	25	0	0	.5	.5	0	0	7	46
small_stakes7	.1	.3	.1	.5	2	13	25	64	.1	.4	.2	.3	19	19	32	55
small_stakes7	0	0	0	1	0	0	0	25	0	0	.5	.5	0	0	10	43
small_stakes8	.1	.2	.3	.4	11	20	38	67	.2	.2	.4	.2	10	25	26	53
small_stakes8	0	0	0	1	0	0	0	20	0	0	.5	.5	0	0	10	32
small_stakes9	.3	.1	.3	.3	17	22	55	61	.1	.5	.1	.3	12	32	45	64
small_stakes9	0	0	0	1	0	0	0	15	0	0	.5	.5	0	0	7	25

The battery consists of six categories of questions, flagged by the text in the variable **qid** which stands internally in the software for "question ID." We explain what each group is testing, and cite one of the primary sources. The specific parameters in our case typically differ, to ensure prize levels commensurate with our inferential objective, but maintain the logical structure of the original lotteries. There were 175 subjects making between 52 and 100 binary choices, drawn from this complete battery. On average each subject made 74 choices.

The first group are Allais Paradox questions, flagged as "allais." Conlisk [1989] presents a real version of the Allais Paradox (AP), with these binary choices

$$(\$5, 1.00) \text{ or } (\$0, 0.01; \$5, 0.89; \$25, 0.10) \qquad \text{(C1)}$$

and

$$(\$0, 0.89; \$5, 0.11) \text{ or } (\$0, 0.9; \$25, 0.1) \qquad \text{(C2)}$$

He finds no evidence of the AP. There are other studies with comparable findings, and these are no well-known lottery questions.

The second group, called "camerer," are common-ratio tests of EUT from Camerer [1989][1992]. By way of background to these two sets of common-ratio tasks, so-called "border effects" arise when one nudges the lottery pairs in common ratio tests and common consequence tests into the interior of the MM triangle, or moves them significantly into the interior. The striking finding is that EUT often performs better when one does this. Actually, the evidence is mixed in interesting ways.

First, Camerer [1992] generated a remarkable series of experiments in which EUT did very well for interior lottery choices, but his data was unfortunately from hypothetical choices (justified in his footnote 7). These lotteries were well off the border, as illustrated in the top MM triangle on the next page. These can be contrasted with those in Camerer [1989] that were on the border, as illustrated in the bottom MM on the next page. But Harless [1992] found that just nudging the lotteries off the boundary did not improve behavior under EUT for real stakes, although it did dramatically for hypothetical and large stakes.

The lotteries used by Camerer [1989][1992] are shown below. We are free to select low, medium and high prizes, since nothing in his design depends on that. Some claims in Camerer [1992] that stakes matter to the violations of EUT mix up (very) hypothetical choices and real choices, and can be ignored.

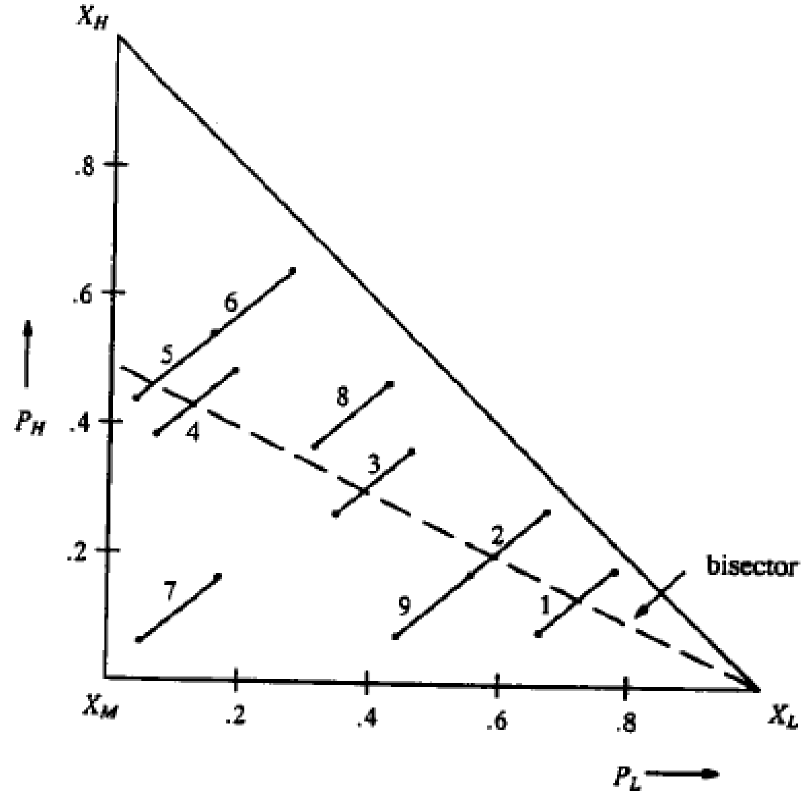

Table 9–2. Gamble Pairs Used in the New Experiment

Pair Number	less risky gamble P_L	P_M	P_H	more risky gamble P_L	P_M	P_H
1	0.65	0.25	0.10	0.75	0.05	0.20
2	0.55	0.30	0.15	0.65	0.10	0.25
3	0.35	0.40	0.25	0.45	0.20	0.35
4	0.05	0.55	0.40	0.15	0.35	0.50
5	0.05	0.50	0.45	0.15	0.30	0.55
6	0.15	0.30	0.55	0.25	0.10	0.65
7	0.05	0.90	0.05	0.15	0.70	0.15
8	0.35	0.30	0.35	0.45	0.10	0.45
9	0.45	0.50	0.05	0.55	0.30	0.15

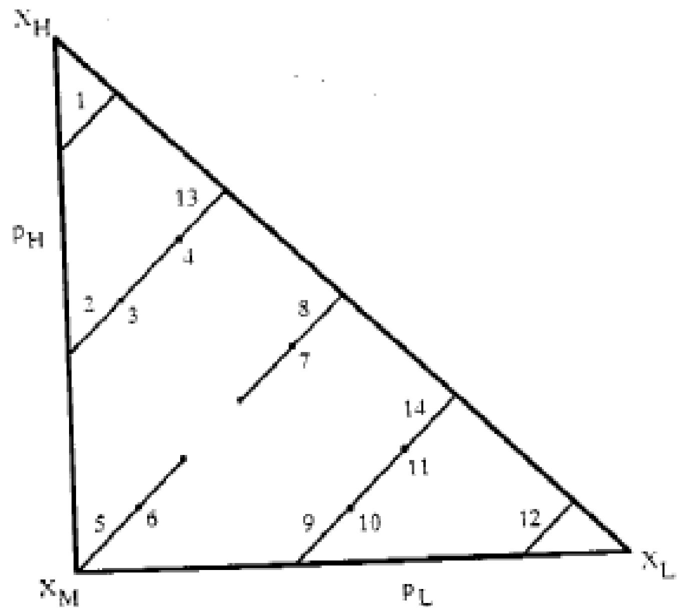

Table 2. Gamble pairs presented to subjects

Pair no.	Less risky gamble P_L	P_M	P_H	More risky gamble P_L	P_M	P_H
1	0	.2	.8	.1	0	.9
2	0	.6	.4	.1	.4	.5
3	0	.6	.4	.2	.2	.6
4	.1	.4	.5	.3	0	.7
5	0	1.0	0	.1	.8	.1
6	0	1.0	0	.2	.6	.2
7	.3	.4	.3	.5	0	.5
8	.4	.2	.4	.5	0	.5
9	.4	.6	0	.5	.4	.1
10	.4	.6	0	.6	.2	.2
11	.5	.4	.1	.7	0	.3
12	.8	.2	0	.9	0	.1
13	.2	.2	.6	.3	0	.7
14	.6	.2	.2	.7	0	.3

The third group, called “holt_laury,” is an implementation of the 10 multiple price list questions popularized by Holt and Laury [2002], where each binary choice is presented to the subjects separately in our design and in random order.

The fourth battery, called “lssM,” are tests of monotonicity due to Loomes, Starmer and Sugden [1992]. Monotonicity, or first-order stochastic dominance, can be violated under Regret Theory. The 5 questions below were developed by Loomes, Starmer and Sugden [1992] to test this hypothesis. As best one can tell, there is no difference between groups A and B apart from some differences in prizes. That is, one might just use the A prizes as well as the B prizes. In their evaluation of results they present both, and often pool them, and there is no discussion that they (A and B) lead to different theoretical predictions.

THE QUESTIONS USED IN THE EXPERIMENT

S-questions

Question 1	0·2	0·2	0·2	0·2	0·2
*S**	x_4	x_4	x_3	x_2	x_1
S	x_1	x_4	x_4	x_3	x_2

Group A: $x_1 = 0$, $x_2 = £4.00$, $x_3 = £8.00$, $x_4 = £10.00$.
Group B: $x_1 = 0$, $x_2 = £2.00$, $x_3 = £5.00$, $x_4 = £10.00$.

P-questions

Question 2	0·22	0·20	0·38	0·20
P1	y_1	y_3	y_2	y_1
*P2**	y_2	y_2	y_1	y_3

Question 3	0·18	0·20	0·42	0·20
*P1**	y_2	y_2	y_1	y_3
P2	y_1	y_3	y_2	y_1

Group A: $y_1 = 0$, $y_2 = £3.00$, $y_3 = £10.00$.
Group B: $y_1 = 0$, $y_2 = £6.00$, $y_3 = £10.00$.

M-questions

Question 4	0·2	0·2	0·4	0·2
M1	z_3	z_3	z_2	z_1
*M2**	z_2	z_2	z_1	z_4

Question 5	0·2	0·2	0·4	0·2
*M1**	z_2	z_2	z_1	z_3
M2	z_1	z_4	z_2	z_1

Group A: $z_1 = 0$, $z_2 = £5.00$, $z_3 = £12.00$, $z_4 = £13.00$.
Group B: $z_1 = 0$, $z_2 = £3.00$, $z_3 = £12.00$, $z_4 = £13.00$.

The fifth battery, called "lssP," are tests of transitivity proposed by Loomes, Starmer and Sugden [1991], in experiments that were part of the experiments reported in Loomes, Starmer and Sugden [1992]. The tests of transitivity involve 15 choice questions, and were again split into two samples I and II (presumably the same as A and B in Loomes, Starmer and Sugden [1992] paper). We focus on the choices given to sample I.

Subsample I

	0.3	0.3	0.4		Expected Value
A_1	18.00	0	0		5.40
B_1	8.00	8.00	0		4.80
C_1	4.00	4.00	4.00		4.00
	0.15	0.15	0.2	0.5	E.V.
A_2	18.00	0	0	0	2.70
B_2	8.00	8.00	0	0	2.40
C_2	4.00	4.00	4.00	0	2.00
	0.4	0.3	0.3		E.V.
A_3	10.00	3.00	3.00		5.80
B_3	7.50	7.50	1.00		5.55
C_3	5.00	5.00	5.00		5.00
	0.3	0.25	0.45		E.V.
A_4	15.00	0	0		4.50
B_4	7.00	7.00	0		3.85
C_4	0	6.00	6.00		4.20
	0.18	0.15	0.27	0.4	E.V.
A_5	.00	0	0	0	2.70
B_5	7.00	7.00	0	0	2.31
C_5	0	6.00	6.00	0	2.52

Q1: $\{A_1, B_1\}$	Q6: $\{B_4, C_4\}$	Q11: $\{B_3, C_3\}$	Q16: $\{A_2, B_2\}$	Q4: $\{B_5, C_5\}$
Q3: $\{A_1, C_1\}$	Q8: $\{A_4, C_4\}$	Q13: $\{A_3, C_3\}$	Q18: $\{A_2, C_2\}$	Q12: $\{A_5, C_5\}$
Q5: $\{B_1, C_1\}$	Q10: $\{A_4, B_4\}$	Q15: $\{A_3, B_3\}$	Q20: $\{B_2, C_2\}$	Q19: $\{A_5, B_5\}$

The sixth battery, called "small_stakes," used tests proposed by Cox and Sadiraj [2008; p. 33] to evaluate the calibration critiques of EUT offered by Hansson [1988] and Rabin [2000]. Give subjects a choice of a certain amount W for sure or some lottery (W-loss, 0.5; W+gain, 0.5) where the loss < W, gain > loss, and one varies the values of W for some feasible domain in terms of the experimental budget. To take some simple examples, consider

$20 or ($10, 0.5; $31, 0.5) (W1)

$30 or ($20, 0.5; $41, 0.5) (W2)

...

$90 or ($80, 0.5; $101, 0.5) (W3)

$100 or ($90, 0.5; $111, 0.5) (W4)

which is just W = {$20, $30, ... $90, $100}, loss = $10 and gain = $11. Although one finds "small stakes" risk aversion for W1 and W2, there is evidence from Harrison, Lau, Swarthout and Ross [2017] that one finds "small stakes" risk neutrality for W3 and W4. For choices like W1 and W2 one finds subjects tending to select the sure option, but for choices like W3 and W4 they are split equally between the two or prefer the risky lottery.

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Appendix D: Experimental Parameters for Time Preference Task

In each session, a subject saw 10 screens of discounting choices. On each screen presented to a subject there were 7 rows of choices (with varying number of columns depending on treatment), for a total of 70 choice rows. For each treatment, our discounting battery consisted of 5 sets of parameters (or MPLs, partitioned in the table below by horizontal rules), each with 17 rows, for a total of 85 choice rows. Each of the 5 MPLs was split into two subsets, with the initial 10 rows in one subset and the final 7 rows in the other subset. These 10 subsets defined the screens shown to each subject in random order. However, for each subset with 10 rows, only 7 rows were randomly selected and shown on each screen (iid for each subject/screen).

Our reasoning for this sampling strategy was due to the following factors: 1) wanting coverage of all 17 distinct growth rates; 2) practical space limitations from trying to have more than 7 rows per screen; and 3) willingness to forego some of the choices with lower growth rates where the rates were more tightly concentrated.

Note: for the bundled treatments, there are multiple lines per **qid** in the table below. The lines with a common **qid** define a single choice row, and the period value (of A, B, or C) indicates the sequence of bundled choices.

Note: Forced2 treatment values are not shown in the table below. This is because the Forced2 treatment values are identical to the Free2 treatment values (these treatments differed only in terms of the allowable responses). Similarly, the Forced3 treatment values are not shown.

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
FED0	T0-01	A	0	10	28	10.04	5.2
FED0	T0-02	A	0	10	28	10.08	10.4
FED0	T0-03	A	0	10	28	10.12	15.6
FED0	T0-04	A	0	10	28	10.15	19.6
FED0	T0-05	A	0	10	28	10.19	24.8
FED0	T0-06	A	0	10	28	10.23	30
FED0	T0-07	A	0	10	28	10.31	40.4
FED0	T0-08	A	0	10	28	10.38	49.5
FED0	T0-09	A	0	10	28	10.58	75.6
FED0	T0-10	A	0	10	28	10.77	100.4
FED0	T0-11	A	0	10	28	10.96	125.1
FED0	T0-12	A	0	10	28	11.15	149.9
FED0	T0-13	A	0	10	28	11.34	174.7
FED0	T0-14	A	0	10	28	11.53	199.4
FED0	T0-15	A	0	10	28	12.3	299.8
FED0	T0-16	A	0	10	28	13.07	400.2
FED0	T0-17	A	0	10	28	13.84	500.6
FED0	T0-18	A	0	30	28	30.12	5.2
FED0	T0-19	A	0	30	28	30.23	10
FED0	T0-20	A	0	30	28	30.35	15.2
FED0	T0-21	A	0	30	28	30.46	20
FED0	T0-22	A	0	30	28	30.58	25.2
FED0	T0-23	A	0	30	28	30.69	30
FED0	T0-24	A	0	30	28	30.92	40
FED0	T0-25	A	0	30	28	31.15	50
FED0	T0-26	A	0	30	28	31.73	75.2
FED0	T0-27	A	0	30	28	32.3	99.9
FED0	T0-28	A	0	30	28	32.88	125.1
FED0	T0-29	A	0	30	28	33.45	149.9
FED0	T0-30	A	0	30	28	34.03	175.1
FED0	T0-31	A	0	30	28	34.6	199.9
FED0	T0-32	A	0	30	28	36.9	299.8
FED0	T0-33	A	0	30	28	39.21	400.2
FED0	T0-34	A	0	30	28	41.51	500.1
FED0	T0-35	A	0	60	28	60.23	5
FED0	T0-36	A	0	60	28	60.46	10
FED0	T0-37	A	0	60	28	60.69	15
FED0	T0-38	A	0	60	28	60.92	20
FED0	T0-39	A	0	60	28	61.15	25
FED0	T0-40	A	0	60	28	61.38	30

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
FED0	T0-41	A	0	60	28	61.84	40
FED0	T0-42	A	0	60	28	62.3	50
FED0	T0-43	A	0	60	28	63.45	75
FED0	T0-44	A	0	60	28	64.6	99.9
FED0	T0-45	A	0	60	28	65.75	124.9
FED0	T0-46	A	0	60	28	66.9	149.9
FED0	T0-47	A	0	60	28	68.05	174.9
FED0	T0-48	A	0	60	28	69.21	200.1
FED0	T0-49	A	0	60	28	73.81	300
FED0	T0-50	A	0	60	28	78.41	400
FED0	T0-51	A	0	60	28	83.01	499.9
FED0	T0-52	A	0	100	28	100.38	5
FED0	T0-53	A	0	100	28	100.77	10
FED0	T0-54	A	0	100	28	101.15	15
FED0	T0-55	A	0	100	28	101.53	19.9
FED0	T0-56	A	0	100	28	101.92	25
FED0	T0-57	A	0	100	28	102.3	30
FED0	T0-58	A	0	100	28	103.07	40
FED0	T0-59	A	0	100	28	103.84	50.1
FED0	T0-60	A	0	100	28	105.75	75
FED0	T0-61	A	0	100	28	107.67	100
FED0	T0-62	A	0	100	28	109.59	125
FED0	T0-63	A	0	100	28	111.51	150
FED0	T0-64	A	0	100	28	113.42	174.9
FED0	T0-65	A	0	100	28	115.34	200
FED0	T0-66	A	0	100	28	123.01	300
FED0	T0-67	A	0	100	28	130.68	399.9
FED0	T0-68	A	0	100	28	138.36	500.1
FED0	T0-69	A	0	300	28	301.15	5
FED0	T0-70	A	0	300	28	302.3	10
FED0	T0-71	A	0	300	28	303.45	15
FED0	T0-72	A	0	300	28	304.6	20
FED0	T0-73	A	0	300	28	305.75	25
FED0	T0-74	A	0	300	28	306.9	30
FED0	T0-75	A	0	300	28	309.21	40
FED0	T0-76	A	0	300	28	311.51	50
FED0	T0-77	A	0	300	28	317.26	75
FED0	T0-78	A	0	300	28	323.01	100
FED0	T0-79	A	0	300	28	328.77	125
FED0	T0-80	A	0	300	28	334.52	150
FED0	T0-81	A	0	300	28	340.27	175
FED0	T0-82	A	0	300	28	346.03	200
FED0	T0-83	A	0	300	28	369.04	300
FED0	T0-84	A	0	300	28	392.05	400
FED0	T0-85	A	0	300	28	415.07	500
FED1	T1-01	A	1	10	29	10.04	5.2
FED1	T1-02	A	1	10	29	10.08	10.4
FED1	T1-03	A	1	10	29	10.12	15.6
FED1	T1-04	A	1	10	29	10.15	19.6
FED1	T1-05	A	1	10	29	10.19	24.8
FED1	T1-06	A	1	10	29	10.23	30
FED1	T1-07	A	1	10	29	10.31	40.4
FED1	T1-08	A	1	10	29	10.38	49.5
FED1	T1-09	A	1	10	29	10.58	75.6
FED1	T1-10	A	1	10	29	10.77	100.4
FED1	T1-11	A	1	10	29	10.96	125.1
FED1	T1-12	A	1	10	29	11.15	149.9
FED1	T1-13	A	1	10	29	11.34	174.7
FED1	T1-14	A	1	10	29	11.53	199.4
FED1	T1-15	A	1	10	29	12.3	299.8
FED1	T1-16	A	1	10	29	13.07	400.2
FED1	T1-17	A	1	10	29	13.84	500.6
FED1	T1-18	A	1	30	29	30.12	5.2
FED1	T1-19	A	1	30	29	30.23	10
FED1	T1-20	A	1	30	29	30.35	15.2
FED1	T1-21	A	1	30	29	30.46	20
FED1	T1-22	A	1	30	29	30.58	25.2
FED1	T1-23	A	1	30	29	30.69	30
FED1	T1-24	A	1	30	29	30.92	40
FED1	T1-25	A	1	30	29	31.15	50
FED1	T1-26	A	1	30	29	31.73	75.2
FED1	T1-27	A	1	30	29	32.3	99.9
FED1	T1-28	A	1	30	29	32.88	125.1
FED1	T1-29	A	1	30	29	33.45	149.9
FED1	T1-30	A	1	30	29	34.03	175.1
FED1	T1-31	A	1	30	29	34.6	199.9
FED1	T1-32	A	1	30	29	36.9	299.8

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
FED1	T1-33	A	1	30	29	39.21	400.2
FED1	T1-34	A	1	30	29	41.51	500.1
FED1	T1-35	A	1	60	29	60.23	5
FED1	T1-36	A	1	60	29	60.46	10
FED1	T1-37	A	1	60	29	60.69	15
FED1	T1-38	A	1	60	29	60.92	20
FED1	T1-39	A	1	60	29	61.15	25
FED1	T1-40	A	1	60	29	61.38	30
FED1	T1-41	A	1	60	29	61.84	40
FED1	T1-42	A	1	60	29	62.3	50
FED1	T1-43	A	1	60	29	63.45	75
FED1	T1-44	A	1	60	29	64.6	99.9
FED1	T1-45	A	1	60	29	65.75	124.9
FED1	T1-46	A	1	60	29	66.9	149.9
FED1	T1-47	A	1	60	29	68.05	174.9
FED1	T1-48	A	1	60	29	69.21	200.1
FED1	T1-49	A	1	60	29	73.81	300
FED1	T1-50	A	1	60	29	78.41	400
FED1	T1-51	A	1	60	29	83.01	499.9
FED1	T1-52	A	1	100	29	100.38	5
FED1	T1-53	A	1	100	29	100.77	10
FED1	T1-54	A	1	100	29	101.15	15
FED1	T1-55	A	1	100	29	101.53	19.9
FED1	T1-56	A	1	100	29	101.92	25
FED1	T1-57	A	1	100	29	102.3	30
FED1	T1-58	A	1	100	29	103.07	40
FED1	T1-59	A	1	100	29	103.84	50.1
FED1	T1-60	A	1	100	29	105.75	75
FED1	T1-61	A	1	100	29	107.67	100
FED1	T1-62	A	1	100	29	109.59	125
FED1	T1-63	A	1	100	29	111.51	150
FED1	T1-64	A	1	100	29	113.42	174.9
FED1	T1-65	A	1	100	29	115.34	200
FED1	T1-66	A	1	100	29	123.01	300
FED1	T1-67	A	1	100	29	130.68	399.9
FED1	T1-68	A	1	100	29	138.36	500.1
FED1	T1-69	A	1	300	29	301.15	5
FED1	T1-70	A	1	300	29	302.3	10
FED1	T1-71	A	1	300	29	303.45	15
FED1	T1-72	A	1	300	29	304.6	20
FED1	T1-73	A	1	300	29	305.75	25
FED1	T1-74	A	1	300	29	306.9	30
FED1	T1-75	A	1	300	29	309.21	40
FED1	T1-76	A	1	300	29	311.51	50
FED1	T1-77	A	1	300	29	317.26	75
FED1	T1-78	A	1	300	29	323.01	100
FED1	T1-79	A	1	300	29	328.77	125
FED1	T1-80	A	1	300	29	334.52	150
FED1	T1-81	A	1	300	29	340.27	175
FED1	T1-82	A	1	300	29	346.03	200
FED1	T1-83	A	1	300	29	369.04	300
FED1	T1-84	A	1	300	29	392.05	400
FED1	T1-85	A	1	300	29	415.07	500
HiMag	T2-01	A	0	30	28	30.12	5.2
HiMag	T2-02	A	0	30	28	30.23	10
HiMag	T2-03	A	0	30	28	30.35	15.2
HiMag	T2-04	A	0	30	28	30.46	20
HiMag	T2-05	A	0	30	28	30.58	25.2
HiMag	T2-06	A	0	30	28	30.69	30
HiMag	T2-07	A	0	30	28	30.92	40
HiMag	T2-08	A	0	30	28	31.15	50
HiMag	T2-09	A	0	30	28	31.73	75.2
HiMag	T2-10	A	0	30	28	32.3	99.9
HiMag	T2-11	A	0	30	28	32.88	125.1
HiMag	T2-12	A	0	30	28	33.45	149.9
HiMag	T2-13	A	0	30	28	34.03	175.1
HiMag	T2-14	A	0	30	28	34.6	199.9
HiMag	T2-15	A	0	30	28	36.9	299.8
HiMag	T2-16	A	0	30	28	39.21	400.2
HiMag	T2-17	A	0	30	28	41.51	500.1
HiMag	T2-18	A	0	90	28	90.35	5.1
HiMag	T2-19	A	0	90	28	90.69	10
HiMag	T2-20	A	0	90	28	91.04	15.1
HiMag	T2-21	A	0	90	28	91.38	20
HiMag	T2-22	A	0	90	28	91.73	25.1
HiMag	T2-23	A	0	90	28	92.07	30
HiMag	T2-24	A	0	90	28	92.76	40

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
HiMag	T2-25	A	0	90	28	93.45	50
HiMag	T2-26	A	0	90	28	95.18	75
HiMag	T2-27	A	0	90	28	96.9	99.9
HiMag	T2-28	A	0	90	28	98.63	125
HiMag	T2-29	A	0	90	28	100.36	150.1
HiMag	T2-30	A	0	90	28	102.08	175
HiMag	T2-31	A	0	90	28	103.81	200
HiMag	T2-32	A	0	90	28	110.71	300
HiMag	T2-33	A	0	90	28	117.62	400.1
HiMag	T2-34	A	0	90	28	124.52	500
HiMag	T2-35	A	0	180	28	180.69	5
HiMag	T2-36	A	0	180	28	181.38	10
HiMag	T2-37	A	0	180	28	182.07	15
HiMag	T2-38	A	0	180	28	182.76	20
HiMag	T2-39	A	0	180	28	183.45	25
HiMag	T2-40	A	0	180	28	184.14	30
HiMag	T2-41	A	0	180	28	185.52	40
HiMag	T2-42	A	0	180	28	186.9	50
HiMag	T2-43	A	0	180	28	190.36	75
HiMag	T2-44	A	0	180	28	193.81	100
HiMag	T2-45	A	0	180	28	197.26	125
HiMag	T2-46	A	0	180	28	200.71	150
HiMag	T2-47	A	0	180	28	204.16	175
HiMag	T2-48	A	0	180	28	207.62	200
HiMag	T2-49	A	0	180	28	221.42	300
HiMag	T2-50	A	0	180	28	235.23	400
HiMag	T2-51	A	0	180	28	249.04	500
HiMag	T2-52	A	0	300	28	301.15	5
HiMag	T2-53	A	0	300	28	302.3	10
HiMag	T2-54	A	0	300	28	303.45	15
HiMag	T2-55	A	0	300	28	304.6	20
HiMag	T2-56	A	0	300	28	305.75	25
HiMag	T2-57	A	0	300	28	306.9	30
HiMag	T2-58	A	0	300	28	309.21	40
HiMag	T2-59	A	0	300	28	311.51	50
HiMag	T2-60	A	0	300	28	317.26	75
HiMag	T2-61	A	0	300	28	323.01	100
HiMag	T2-62	A	0	300	28	328.77	125
HiMag	T2-63	A	0	300	28	334.52	150
HiMag	T2-64	A	0	300	28	340.27	175
HiMag	T2-65	A	0	300	28	346.03	200
HiMag	T2-66	A	0	300	28	369.04	300
HiMag	T2-67	A	0	300	28	392.05	400
HiMag	T2-68	A	0	300	28	415.07	500
HiMag	T2-69	A	0	900	28	903.45	5
HiMag	T2-70	A	0	900	28	906.9	10
HiMag	T2-71	A	0	900	28	910.36	15
HiMag	T2-72	A	0	900	28	913.81	20
HiMag	T2-73	A	0	900	28	917.26	25
HiMag	T2-74	A	0	900	28	920.71	30
HiMag	T2-75	A	0	900	28	927.62	40
HiMag	T2-76	A	0	900	28	934.52	50
HiMag	T2-77	A	0	900	28	951.78	75
HiMag	T2-78	A	0	900	28	969.04	100
HiMag	T2-79	A	0	900	28	986.3	125
HiMag	T2-80	A	0	900	28	1003.56	150
HiMag	T2-81	A	0	900	28	1020.82	175
HiMag	T2-82	A	0	900	28	1038.08	200
HiMag	T2-83	A	0	900	28	1107.12	300
HiMag	T2-84	A	0	900	28	1176.16	400
HiMag	T2-85	A	0	900	28	1245.21	500
FED35	T3-01	A	35	10	63	10.04	5.2
FED35	T3-02	A	35	10	63	10.08	10.4
FED35	T3-03	A	35	10	63	10.12	15.6
FED35	T3-04	A	35	10	63	10.15	19.6
FED35	T3-05	A	35	10	63	10.19	24.8
FED35	T3-06	A	35	10	63	10.23	30
FED35	T3-07	A	35	10	63	10.31	40.4
FED35	T3-08	A	35	10	63	10.38	49.5
FED35	T3-09	A	35	10	63	10.58	75.6
FED35	T3-10	A	35	10	63	10.77	100.4
FED35	T3-11	A	35	10	63	10.96	125.1
FED35	T3-12	A	35	10	63	11.15	149.9
FED35	T3-13	A	35	10	63	11.34	174.7
FED35	T3-14	A	35	10	63	11.53	199.4
FED35	T3-15	A	35	10	63	12.3	299.8
FED35	T3-16	A	35	10	63	13.07	400.2

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
FED35	T3-17	A	35	10	63	13.84	500.6
FED35	T3-18	A	35	30	63	30.12	5.2
FED35	T3-19	A	35	30	63	30.23	10
FED35	T3-20	A	35	30	63	30.35	15.2
FED35	T3-21	A	35	30	63	30.46	20
FED35	T3-22	A	35	30	63	30.58	25.2
FED35	T3-23	A	35	30	63	30.69	30
FED35	T3-24	A	35	30	63	30.92	40
FED35	T3-25	A	35	30	63	31.15	50
FED35	T3-26	A	35	30	63	31.73	75.2
FED35	T3-27	A	35	30	63	32.3	99.9
FED35	T3-28	A	35	30	63	32.88	125.1
FED35	T3-29	A	35	30	63	33.45	149.9
FED35	T3-30	A	35	30	63	34.03	175.1
FED35	T3-31	A	35	30	63	34.6	199.9
FED35	T3-32	A	35	30	63	36.9	299.8
FED35	T3-33	A	35	30	63	39.21	400.2
FED35	T3-34	A	35	30	63	41.51	500.1
FED35	T3-35	A	35	60	63	60.23	5
FED35	T3-36	A	35	60	63	60.46	10
FED35	T3-37	A	35	60	63	60.69	15
FED35	T3-38	A	35	60	63	60.92	20
FED35	T3-39	A	35	60	63	61.15	25
FED35	T3-40	A	35	60	63	61.38	30
FED35	T3-41	A	35	60	63	61.84	40
FED35	T3-42	A	35	60	63	62.3	50
FED35	T3-43	A	35	60	63	63.45	75
FED35	T3-44	A	35	60	63	64.6	99.9
FED35	T3-45	A	35	60	63	65.75	124.9
FED35	T3-46	A	35	60	63	66.9	149.9
FED35	T3-47	A	35	60	63	68.05	174.9
FED35	T3-48	A	35	60	63	69.21	200.1
FED35	T3-49	A	35	60	63	73.81	300
FED35	T3-50	A	35	60	63	78.41	400
FED35	T3-51	A	35	60	63	83.01	499.9
FED35	T3-52	A	35	100	63	100.38	5
FED35	T3-53	A	35	100	63	100.77	10
FED35	T3-54	A	35	100	63	101.15	15
FED35	T3-55	A	35	100	63	101.53	19.9
FED35	T3-56	A	35	100	63	101.92	25
FED35	T3-57	A	35	100	63	102.3	30
FED35	T3-58	A	35	100	63	103.07	40
FED35	T3-59	A	35	100	63	103.84	50.1
FED35	T3-60	A	35	100	63	105.75	75
FED35	T3-61	A	35	100	63	107.67	100
FED35	T3-62	A	35	100	63	109.59	125
FED35	T3-63	A	35	100	63	111.51	150
FED35	T3-64	A	35	100	63	113.42	174.9
FED35	T3-65	A	35	100	63	115.34	200
FED35	T3-66	A	35	100	63	123.01	300
FED35	T3-67	A	35	100	63	130.68	399.9
FED35	T3-68	A	35	100	63	138.36	500.1
FED35	T3-69	A	35	300	63	301.15	5
FED35	T3-70	A	35	300	63	302.3	10
FED35	T3-71	A	35	300	63	303.45	15
FED35	T3-72	A	35	300	63	304.6	20
FED35	T3-73	A	35	300	63	305.75	25
FED35	T3-74	A	35	300	63	306.9	30
FED35	T3-75	A	35	300	63	309.21	40
FED35	T3-76	A	35	300	63	311.51	50
FED35	T3-77	A	35	300	63	317.26	75
FED35	T3-78	A	35	300	63	323.01	100
FED35	T3-79	A	35	300	63	328.77	125
FED35	T3-80	A	35	300	63	334.52	150
FED35	T3-81	A	35	300	63	340.27	175
FED35	T3-82	A	35	300	63	346.03	200
FED35	T3-83	A	35	300	63	369.04	300
FED35	T3-84	A	35	300	63	392.05	400
FED35	T3-85	A	35	300	63	415.07	500
Free2	T4-01	A	0	10	28	10.04	5.2
Free2	T4-01	B	35	10	63	10.04	5.2
Free2	T4-02	A	0	10	28	10.08	10.4
Free2	T4-02	B	35	10	63	10.08	10.4
Free2	T4-03	A	0	10	28	10.12	15.6
Free2	T4-03	B	35	10	63	10.12	15.6
Free2	T4-04	A	0	10	28	10.15	19.6
Free2	T4-04	B	35	10	63	10.15	19.6

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
Free2	T4-05	A	0	10	28	10.19	24.8
Free2	T4-05	B	35	10	63	10.19	24.8
Free2	T4-06	A	0	10	28	10.23	30
Free2	T4-06	B	35	10	63	10.23	30
Free2	T4-07	A	0	10	28	10.31	40.4
Free2	T4-07	B	35	10	63	10.31	40.4
Free2	T4-08	A	0	10	28	10.38	49.5
Free2	T4-08	B	35	10	63	10.38	49.5
Free2	T4-09	A	0	10	28	10.58	75.6
Free2	T4-09	B	35	10	63	10.58	75.6
Free2	T4-10	A	0	10	28	10.77	100.4
Free2	T4-10	B	35	10	63	10.77	100.4
Free2	T4-11	A	0	10	28	10.96	125.1
Free2	T4-11	B	35	10	63	10.96	125.1
Free2	T4-12	A	0	10	28	11.15	149.9
Free2	T4-12	B	35	10	63	11.15	149.9
Free2	T4-13	A	0	10	28	11.34	174.7
Free2	T4-13	B	35	10	63	11.34	174.7
Free2	T4-14	A	0	10	28	11.53	199.4
Free2	T4-14	B	35	10	63	11.53	199.4
Free2	T4-15	A	0	10	28	12.3	299.8
Free2	T4-15	B	35	10	63	12.3	299.8
Free2	T4-16	A	0	10	28	13.07	400.2
Free2	T4-16	B	35	10	63	13.07	400.2
Free2	T4-17	A	0	10	28	13.84	500.6
Free2	T4-17	B	35	10	63	13.84	500.6
Free2	T4-18	A	0	30	28	30.12	5.2
Free2	T4-18	B	35	30	63	30.12	5.2
Free2	T4-19	A	0	30	28	30.23	10
Free2	T4-19	B	35	30	63	30.23	10
Free2	T4-20	A	0	30	28	30.35	15.2
Free2	T4-20	B	35	30	63	30.35	15.2
Free2	T4-21	A	0	30	28	30.46	20
Free2	T4-21	B	35	30	63	30.46	20
Free2	T4-22	A	0	30	28	30.58	25.2
Free2	T4-22	B	35	30	63	30.58	25.2
Free2	T4-23	A	0	30	28	30.69	30
Free2	T4-23	B	35	30	63	30.69	30
Free2	T4-24	A	0	30	28	30.92	40
Free2	T4-24	B	35	30	63	30.92	40
Free2	T4-25	A	0	30	28	31.15	50
Free2	T4-25	B	35	30	63	31.15	50
Free2	T4-26	A	0	30	28	31.73	75.2
Free2	T4-26	B	35	30	63	31.73	75.2
Free2	T4-27	A	0	30	28	32.3	99.9
Free2	T4-27	B	35	30	63	32.3	99.9
Free2	T4-28	A	0	30	28	32.88	125.1
Free2	T4-28	B	35	30	63	32.88	125.1
Free2	T4-29	A	0	30	28	33.45	149.9
Free2	T4-29	B	35	30	63	33.45	149.9
Free2	T4-30	A	0	30	28	34.03	175.1
Free2	T4-30	B	35	30	63	34.03	175.1
Free2	T4-31	A	0	30	28	34.6	199.9
Free2	T4-31	B	35	30	63	34.6	199.9
Free2	T4-32	A	0	30	28	36.9	299.8
Free2	T4-32	B	35	30	63	36.9	299.8
Free2	T4-33	A	0	30	28	39.21	400.2
Free2	T4-33	B	35	30	63	39.21	400.2
Free2	T4-34	A	0	30	28	41.51	500.1
Free2	T4-34	B	35	30	63	41.51	500.1
Free2	T4-35	A	0	60	28	60.23	5
Free2	T4-35	B	35	60	63	60.23	5
Free2	T4-36	A	0	60	28	60.46	10
Free2	T4-36	B	35	60	63	60.46	10
Free2	T4-37	A	0	60	28	60.69	15
Free2	T4-37	B	35	60	63	60.69	15
Free2	T4-38	A	0	60	28	60.92	20
Free2	T4-38	B	35	60	63	60.92	20
Free2	T4-39	A	0	60	28	61.15	25
Free2	T4-39	B	35	60	63	61.15	25
Free2	T4-40	A	0	60	28	61.38	30
Free2	T4-40	B	35	60	63	61.38	30
Free2	T4-41	A	0	60	28	61.84	40
Free2	T4-41	B	35	60	63	61.84	40
Free2	T4-42	A	0	60	28	62.3	50
Free2	T4-42	B	35	60	63	62.3	50
Free2	T4-43	A	0	60	28	63.45	75

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
Free2	T4-43	B	35	60	63	63.45	75
Free2	T4-44	A	0	60	28	64.6	99.9
Free2	T4-44	B	35	60	63	64.6	99.9
Free2	T4-45	A	0	60	28	65.75	124.9
Free2	T4-45	B	35	60	63	65.75	124.9
Free2	T4-46	A	0	60	28	66.9	149.9
Free2	T4-46	B	35	60	63	66.9	149.9
Free2	T4-47	A	0	60	28	68.05	174.9
Free2	T4-47	B	35	60	63	68.05	174.9
Free2	T4-48	A	0	60	28	69.21	200.1
Free2	T4-48	B	35	60	63	69.21	200.1
Free2	T4-49	A	0	60	28	73.81	300
Free2	T4-49	B	35	60	63	73.81	300
Free2	T4-50	A	0	60	28	78.41	400
Free2	T4-50	B	35	60	63	78.41	400
Free2	T4-51	A	0	60	28	83.01	499.9
Free2	T4-51	B	35	60	63	83.01	499.9
Free2	T4-52	A	0	100	28	100.38	5
Free2	T4-52	B	35	100	63	100.38	5
Free2	T4-53	A	0	100	28	100.77	10
Free2	T4-53	B	35	100	63	100.77	10
Free2	T4-54	A	0	100	28	101.15	15
Free2	T4-54	B	35	100	63	101.15	15
Free2	T4-55	A	0	100	28	101.53	19.9
Free2	T4-55	B	35	100	63	101.53	19.9
Free2	T4-56	A	0	100	28	101.92	25
Free2	T4-56	B	35	100	63	101.92	25
Free2	T4-57	A	0	100	28	102.3	30
Free2	T4-57	B	35	100	63	102.3	30
Free2	T4-58	A	0	100	28	103.07	40
Free2	T4-58	B	35	100	63	103.07	40
Free2	T4-59	A	0	100	28	103.84	50.1
Free2	T4-59	B	35	100	63	103.84	50.1
Free2	T4-60	A	0	100	28	105.75	75
Free2	T4-60	B	35	100	63	105.75	75
Free2	T4-61	A	0	100	28	107.67	100
Free2	T4-61	B	35	100	63	107.67	100
Free2	T4-62	A	0	100	28	109.59	125
Free2	T4-62	B	35	100	63	109.59	125
Free2	T4-63	A	0	100	28	111.51	150
Free2	T4-63	B	35	100	63	111.51	150
Free2	T4-64	A	0	100	28	113.42	174.9
Free2	T4-64	B	35	100	63	113.42	174.9
Free2	T4-65	A	0	100	28	115.34	200
Free2	T4-65	B	35	100	63	115.34	200
Free2	T4-66	A	0	100	28	123.01	300
Free2	T4-66	B	35	100	63	123.01	300
Free2	T4-67	A	0	100	28	130.68	399.9
Free2	T4-67	B	35	100	63	130.68	399.9
Free2	T4-68	A	0	100	28	138.36	500.1
Free2	T4-68	B	35	100	63	138.36	500.1
Free2	T4-69	A	0	300	28	301.15	5
Free2	T4-69	B	35	300	63	301.15	5
Free2	T4-70	A	0	300	28	302.3	10
Free2	T4-70	B	35	300	63	302.3	10
Free2	T4-71	A	0	300	28	303.45	15
Free2	T4-71	B	35	300	63	303.45	15
Free2	T4-72	A	0	300	28	304.6	20
Free2	T4-72	B	35	300	63	304.6	20
Free2	T4-73	A	0	300	28	305.75	25
Free2	T4-73	B	35	300	63	305.75	25
Free2	T4-74	A	0	300	28	306.9	30
Free2	T4-74	B	35	300	63	306.9	30
Free2	T4-75	A	0	300	28	309.21	40
Free2	T4-75	B	35	300	63	309.21	40
Free2	T4-76	A	0	300	28	311.51	50
Free2	T4-76	B	35	300	63	311.51	50
Free2	T4-77	A	0	300	28	317.26	75
Free2	T4-77	B	35	300	63	317.26	75
Free2	T4-78	A	0	300	28	323.01	100
Free2	T4-78	B	35	300	63	323.01	100
Free2	T4-79	A	0	300	28	328.77	125
Free2	T4-79	B	35	300	63	328.77	125
Free2	T4-80	A	0	300	28	334.52	150
Free2	T4-80	B	35	300	63	334.52	150
Free2	T4-81	A	0	300	28	340.27	175
Free2	T4-81	B	35	300	63	340.27	175

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
Free2	T4-82	A	0	300	28	346.03	200
Free2	T4-82	B	35	300	63	346.03	200
Free2	T4-83	A	0	300	28	369.04	300
Free2	T4-83	B	35	300	63	369.04	300
Free2	T4-84	A	0	300	28	392.05	400
Free2	T4-84	B	35	300	63	392.05	400
Free2	T4-85	A	0	300	28	415.07	500
Free2	T4-85	B	35	300	63	415.07	500
Free3	T6-01	A	0	10	28	10.04	5.2
Free3	T6-01	B	35	10	63	10.04	5.2
Free3	T6-01	C	70	10	98	10.04	5.2
Free3	T6-02	A	0	10	28	10.08	10.4
Free3	T6-02	B	35	10	63	10.08	10.4
Free3	T6-02	C	70	10	98	10.08	10.4
Free3	T6-03	A	0	10	28	10.12	15.6
Free3	T6-03	B	35	10	63	10.12	15.6
Free3	T6-03	C	70	10	98	10.12	15.6
Free3	T6-04	A	0	10	28	10.15	19.6
Free3	T6-04	B	35	10	63	10.15	19.6
Free3	T6-04	C	70	10	98	10.15	19.6
Free3	T6-05	A	0	10	28	10.19	24.8
Free3	T6-05	B	35	10	63	10.19	24.8
Free3	T6-05	C	70	10	98	10.19	24.8
Free3	T6-06	A	0	10	28	10.23	30
Free3	T6-06	B	35	10	63	10.23	30
Free3	T6-06	C	70	10	98	10.23	30
Free3	T6-07	A	0	10	28	10.31	40.4
Free3	T6-07	B	35	10	63	10.31	40.4
Free3	T6-07	C	70	10	98	10.31	40.4
Free3	T6-08	A	0	10	28	10.38	49.5
Free3	T6-08	B	35	10	63	10.38	49.5
Free3	T6-08	C	70	10	98	10.38	49.5
Free3	T6-09	A	0	10	28	10.58	75.6
Free3	T6-09	B	35	10	63	10.58	75.6
Free3	T6-09	C	70	10	98	10.58	75.6
Free3	T6-10	A	0	10	28	10.77	100.4
Free3	T6-10	B	35	10	63	10.77	100.4
Free3	T6-10	C	70	10	98	10.77	100.4
Free3	T6-11	A	0	10	28	10.96	125.1
Free3	T6-11	B	35	10	63	10.96	125.1
Free3	T6-11	C	70	10	98	10.96	125.1
Free3	T6-12	A	0	10	28	11.15	149.9
Free3	T6-12	B	35	10	63	11.15	149.9
Free3	T6-12	C	70	10	98	11.15	149.9
Free3	T6-13	A	0	10	28	11.34	174.7
Free3	T6-13	B	35	10	63	11.34	174.7
Free3	T6-13	C	70	10	98	11.34	174.7
Free3	T6-14	A	0	10	28	11.53	199.4
Free3	T6-14	B	35	10	63	11.53	199.4
Free3	T6-14	C	70	10	98	11.53	199.4
Free3	T6-15	A	0	10	28	12.3	299.8
Free3	T6-15	B	35	10	63	12.3	299.8
Free3	T6-15	C	70	10	98	12.3	299.8
Free3	T6-16	A	0	10	28	13.07	400.2
Free3	T6-16	B	35	10	63	13.07	400.2
Free3	T6-16	C	70	10	98	13.07	400.2
Free3	T6-17	A	0	10	28	13.84	500.6
Free3	T6-17	B	35	10	63	13.84	500.6
Free3	T6-17	C	70	10	98	13.84	500.6
Free3	T6-18	A	0	30	28	30.12	5.2
Free3	T6-18	B	35	30	63	30.12	5.2
Free3	T6-18	C	70	30	98	30.12	5.2
Free3	T6-19	A	0	30	28	30.23	10
Free3	T6-19	B	35	30	63	30.23	10
Free3	T6-19	C	70	30	98	30.23	10
Free3	T6-20	A	0	30	28	30.35	15.2
Free3	T6-20	B	35	30	63	30.35	15.2
Free3	T6-20	C	70	30	98	30.35	15.2
Free3	T6-21	A	0	30	28	30.46	20
Free3	T6-21	B	35	30	63	30.46	20
Free3	T6-21	C	70	30	98	30.46	20
Free3	T6-22	A	0	30	28	30.58	25.2
Free3	T6-22	B	35	30	63	30.58	25.2
Free3	T6-22	C	70	30	98	30.58	25.2
Free3	T6-23	A	0	30	28	30.69	30
Free3	T6-23	B	35	30	63	30.69	30
Free3	T6-23	C	70	30	98	30.69	30

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
Free3	T6-24	A	0	30	28	30.92	40
Free3	T6-24	B	35	30	63	30.92	40
Free3	T6-24	C	70	30	98	30.92	40
Free3	T6-25	A	0	30	28	31.15	50
Free3	T6-25	B	35	30	63	31.15	50
Free3	T6-25	C	70	30	98	31.15	50
Free3	T6-26	A	0	30	28	31.73	75.2
Free3	T6-26	B	35	30	63	31.73	75.2
Free3	T6-26	C	70	30	98	31.73	75.2
Free3	T6-27	A	0	30	28	32.3	99.9
Free3	T6-27	B	35	30	63	32.3	99.9
Free3	T6-27	C	70	30	98	32.3	99.9
Free3	T6-28	A	0	30	28	32.88	125.1
Free3	T6-28	B	35	30	63	32.88	125.1
Free3	T6-28	C	70	30	98	32.88	125.1
Free3	T6-29	A	0	30	28	33.45	149.9
Free3	T6-29	B	35	30	63	33.45	149.9
Free3	T6-29	C	70	30	98	33.45	149.9
Free3	T6-30	A	0	30	28	34.03	175.1
Free3	T6-30	B	35	30	63	34.03	175.1
Free3	T6-30	C	70	30	98	34.03	175.1
Free3	T6-31	A	0	30	28	34.6	199.9
Free3	T6-31	B	35	30	63	34.6	199.9
Free3	T6-31	C	70	30	98	34.6	199.9
Free3	T6-32	A	0	30	28	36.9	299.8
Free3	T6-32	B	35	30	63	36.9	299.8
Free3	T6-32	C	70	30	98	36.9	299.8
Free3	T6-33	A	0	30	28	39.21	400.2
Free3	T6-33	B	35	30	63	39.21	400.2
Free3	T6-33	C	70	30	98	39.21	400.2
Free3	T6-34	A	0	30	28	41.51	500.1
Free3	T6-34	B	35	30	63	41.51	500.1
Free3	T6-34	C	70	30	98	41.51	500.1
Free3	T6-35	A	0	60	28	60.23	5
Free3	T6-35	B	35	60	63	60.23	5
Free3	T6-35	C	70	60	98	60.23	5
Free3	T6-36	A	0	60	28	60.46	10
Free3	T6-36	B	35	60	63	60.46	10
Free3	T6-36	C	70	60	98	60.46	10
Free3	T6-37	A	0	60	28	60.69	15
Free3	T6-37	B	35	60	63	60.69	15
Free3	T6-37	C	70	60	98	60.69	15
Free3	T6-38	A	0	60	28	60.92	20
Free3	T6-38	B	35	60	63	60.92	20
Free3	T6-38	C	70	60	98	60.92	20
Free3	T6-39	A	0	60	28	61.15	25
Free3	T6-39	B	35	60	63	61.15	25
Free3	T6-39	C	70	60	98	61.15	25
Free3	T6-40	A	0	60	28	61.38	30
Free3	T6-40	B	35	60	63	61.38	30
Free3	T6-40	C	70	60	98	61.38	30
Free3	T6-41	A	0	60	28	61.84	40
Free3	T6-41	B	35	60	63	61.84	40
Free3	T6-41	C	70	60	98	61.84	40
Free3	T6-42	A	0	60	28	62.3	50
Free3	T6-42	B	35	60	63	62.3	50
Free3	T6-42	C	70	60	98	62.3	50
Free3	T6-43	A	0	60	28	63.45	75
Free3	T6-43	B	35	60	63	63.45	75
Free3	T6-43	C	70	60	98	63.45	75
Free3	T6-44	A	0	60	28	64.6	99.9
Free3	T6-44	B	35	60	63	64.6	99.9
Free3	T6-44	C	70	60	98	64.6	99.9
Free3	T6-45	A	0	60	28	65.75	124.9
Free3	T6-45	B	35	60	63	65.75	124.9
Free3	T6-45	C	70	60	98	65.75	124.9
Free3	T6-46	A	0	60	28	66.9	149.9
Free3	T6-46	B	35	60	63	66.9	149.9
Free3	T6-46	C	70	60	98	66.9	149.9
Free3	T6-47	A	0	60	28	68.05	174.9
Free3	T6-47	B	35	60	63	68.05	174.9
Free3	T6-47	C	70	60	98	68.05	174.9
Free3	T6-48	A	0	60	28	69.21	200.1
Free3	T6-48	B	35	60	63	69.21	200.1
Free3	T6-48	C	70	60	98	69.21	200.1
Free3	T6-49	A	0	60	28	73.81	300
Free3	T6-49	B	35	60	63	73.81	300

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
Free3	T6-49	C	70	60	98	73.81	300
Free3	T6-50	A	0	60	28	78.41	400
Free3	T6-50	B	35	60	63	78.41	400
Free3	T6-50	C	70	60	98	78.41	400
Free3	T6-51	A	0	60	28	83.01	499.9
Free3	T6-51	B	35	60	63	83.01	499.9
Free3	T6-51	C	70	60	98	83.01	499.9
Free3	T6-52	A	0	100	28	100.38	5
Free3	T6-52	B	35	100	63	100.38	5
Free3	T6-52	C	70	100	98	100.38	5
Free3	T6-53	A	0	100	28	100.77	10
Free3	T6-53	B	35	100	63	100.77	10
Free3	T6-53	C	70	100	98	100.77	10
Free3	T6-54	A	0	100	28	101.15	15
Free3	T6-54	B	35	100	63	101.15	15
Free3	T6-54	C	70	100	98	101.15	15
Free3	T6-55	A	0	100	28	101.53	19.9
Free3	T6-55	B	35	100	63	101.53	19.9
Free3	T6-55	C	70	100	98	101.53	19.9
Free3	T6-56	A	0	100	28	101.92	25
Free3	T6-56	B	35	100	63	101.92	25
Free3	T6-56	C	70	100	98	101.92	25
Free3	T6-57	A	0	100	28	102.3	30
Free3	T6-57	B	35	100	63	102.3	30
Free3	T6-57	C	70	100	98	102.3	30
Free3	T6-58	A	0	100	28	103.07	40
Free3	T6-58	B	35	100	63	103.07	40
Free3	T6-58	C	70	100	98	103.07	40
Free3	T6-59	A	0	100	28	103.84	50.1
Free3	T6-59	B	35	100	63	103.84	50.1
Free3	T6-59	C	70	100	98	103.84	50.1
Free3	T6-60	A	0	100	28	105.75	75
Free3	T6-60	B	35	100	63	105.75	75
Free3	T6-60	C	70	100	98	105.75	75
Free3	T6-61	A	0	100	28	107.67	100
Free3	T6-61	B	35	100	63	107.67	100
Free3	T6-61	C	70	100	98	107.67	100
Free3	T6-62	A	0	100	28	109.59	125
Free3	T6-62	B	35	100	63	109.59	125
Free3	T6-62	C	70	100	98	109.59	125
Free3	T6-63	A	0	100	28	111.51	150
Free3	T6-63	B	35	100	63	111.51	150
Free3	T6-63	C	70	100	98	111.51	150
Free3	T6-64	A	0	100	28	113.42	174.9
Free3	T6-64	B	35	100	63	113.42	174.9
Free3	T6-64	C	70	100	98	113.42	174.9
Free3	T6-65	A	0	100	28	115.34	200
Free3	T6-65	B	35	100	63	115.34	200
Free3	T6-65	C	70	100	98	115.34	200
Free3	T6-66	A	0	100	28	123.01	300
Free3	T6-66	B	35	100	63	123.01	300
Free3	T6-66	C	70	100	98	123.01	300
Free3	T6-67	A	0	100	28	130.68	399.9
Free3	T6-67	B	35	100	63	130.68	399.9
Free3	T6-67	C	70	100	98	130.68	399.9
Free3	T6-68	A	0	100	28	138.36	500.1
Free3	T6-68	B	35	100	63	138.36	500.1
Free3	T6-68	C	70	100	98	138.36	500.1
Free3	T6-69	A	0	300	28	301.15	5
Free3	T6-69	B	35	300	63	301.15	5
Free3	T6-69	C	70	300	98	301.15	5
Free3	T6-70	A	0	300	28	302.3	10
Free3	T6-70	B	35	300	63	302.3	10
Free3	T6-70	C	70	300	98	302.3	10
Free3	T6-71	A	0	300	28	303.45	15
Free3	T6-71	B	35	300	63	303.45	15
Free3	T6-71	C	70	300	98	303.45	15
Free3	T6-72	A	0	300	28	304.6	20
Free3	T6-72	B	35	300	63	304.6	20
Free3	T6-72	C	70	300	98	304.6	20
Free3	T6-73	A	0	300	28	305.75	25
Free3	T6-73	B	35	300	63	305.75	25
Free3	T6-73	C	70	300	98	305.75	25
Free3	T6-74	A	0	300	28	306.9	30
Free3	T6-74	B	35	300	63	306.9	30
Free3	T6-74	C	70	300	98	306.9	30
Free3	T6-75	A	0	300	28	309.21	40

treatment	qid	period	FED (days)	Sooner ($)	Horizon (days)	Later ($)	Nominal rate (%)
Free3	T6-75	B	35	300	63	309.21	40
Free3	T6-75	C	70	300	98	309.21	40
Free3	T6-76	A	0	300	28	311.51	50
Free3	T6-76	B	35	300	63	311.51	50
Free3	T6-76	C	70	300	98	311.51	50
Free3	T6-77	A	0	300	28	317.26	75
Free3	T6-77	B	35	300	63	317.26	75
Free3	T6-77	C	70	300	98	317.26	75
Free3	T6-78	A	0	300	28	323.01	100
Free3	T6-78	B	35	300	63	323.01	100
Free3	T6-78	C	70	300	98	323.01	100
Free3	T6-79	A	0	300	28	328.77	125
Free3	T6-79	B	35	300	63	328.77	125
Free3	T6-79	C	70	300	98	328.77	125
Free3	T6-80	A	0	300	28	334.52	150
Free3	T6-80	B	35	300	63	334.52	150
Free3	T6-80	C	70	300	98	334.52	150
Free3	T6-81	A	0	300	28	340.27	175
Free3	T6-81	B	35	300	63	340.27	175
Free3	T6-81	C	70	300	98	340.27	175
Free3	T6-82	A	0	300	28	346.03	200
Free3	T6-82	B	35	300	63	346.03	200
Free3	T6-82	C	70	300	98	346.03	200
Free3	T6-83	A	0	300	28	369.04	300
Free3	T6-83	B	35	300	63	369.04	300
Free3	T6-83	C	70	300	98	369.04	300
Free3	T6-84	A	0	300	28	392.05	400
Free3	T6-84	B	35	300	63	392.05	400
Free3	T6-84	C	70	300	98	392.05	400
Free3	T6-85	A	0	300	28	415.07	500
Free3	T6-85	B	35	300	63	415.07	500
Free3	T6-85	C	70	300	98	415.07	500

Appendix E: Additional Estimation Results

A. Risk Preferences

The estimates below are for four models: EUT, RDU with Power Probability Weighting Function (pwf), RDU with Inverse-S pwf, and RDU with Prelec pwf. These are pooled over N = 171 subjects drawn from the same population as the bundling experiments. Since EUT is nested within RDU, we can directly examine the aggregate log-likelihood values to see that the RDU specifications all dominate the EUT specification, and that the RDU model with Prelec pwf is the best.

Figure E1 displays the three RDU pwf estimates, as well as the EUT special case for reference. Formal hypothesis tests of the restriction to EUT are listed below after each set of RDU estimates. Figure E2 then focusses on the preferred Prelec pwf, and shows implications for decision weights. The right panel in Figure E2 displays the effect of probability weighting for 2-prize, 3-prize and 4-prize lotteries with equi-probable outcomes, to see the "pure" effect of rank-dependent probability weighting. The effect is to give greater weight to extreme prizes when there are 3 or 4 prizes in the lottery, and greater weight to the worst prize when there are only 2 prizes.

Expected Utility Theory

Log pseudolikelihood = -8340.4709

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
r	.8262663	.0101722	81.23	0.000	.8063292	.8462033
mu	.1406211	.006117	22.99	0.000	.1286321	.1526101

RDU with Power Probability Weighting Function

Log pseudolikelihood = -8329.1901

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
r	.8138222	.010888	74.75	0.000	.7924822	.8351622
gamma	1.142514	.074594	15.32	0.000	.9963124	1.288716
mu	.146845	.0068445	21.45	0.000	.1334301	.16026

```
.          * test EUT
.          test gamma==1

           chi2(  1) =    3.65
         Prob > chi2 =    0.0561
```

RDU with Inverse-S Probability Weighting Function

Log pseudolikelihood = -8167.2717

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
r	.6825555	.0140615	48.54	0.000	.6549954	.7101156
gamma	.7191498	.0186792	38.50	0.000	.6825393	.7557603
mu	.1414775	.0065038	21.75	0.000	.1287304	.1542247

```
.         * test EUT
.         test gamma==1

            chi2(  1) =  226.07
          Prob > chi2 =    0.0000
```

RDU with Prelec Probability Weighting Function

Log pseudolikelihood = -8142.5023

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
r	.661865	.014231	46.51	0.000	.6339727	.6897572
eta	.9126632	.0298124	30.61	0.000	.8542318	.9710945
phi	.6234765	.0261367	23.85	0.000	.5722494	.6747035
mu	.1397306	.0064627	21.62	0.000	.127064	.1523972

```
.
.         * test EUT
.         test eta=phi=1

            chi2(  2) =  210.47
          Prob > chi2 =    0.0000
```

Figure E1: RDU Probability Weighting Functions

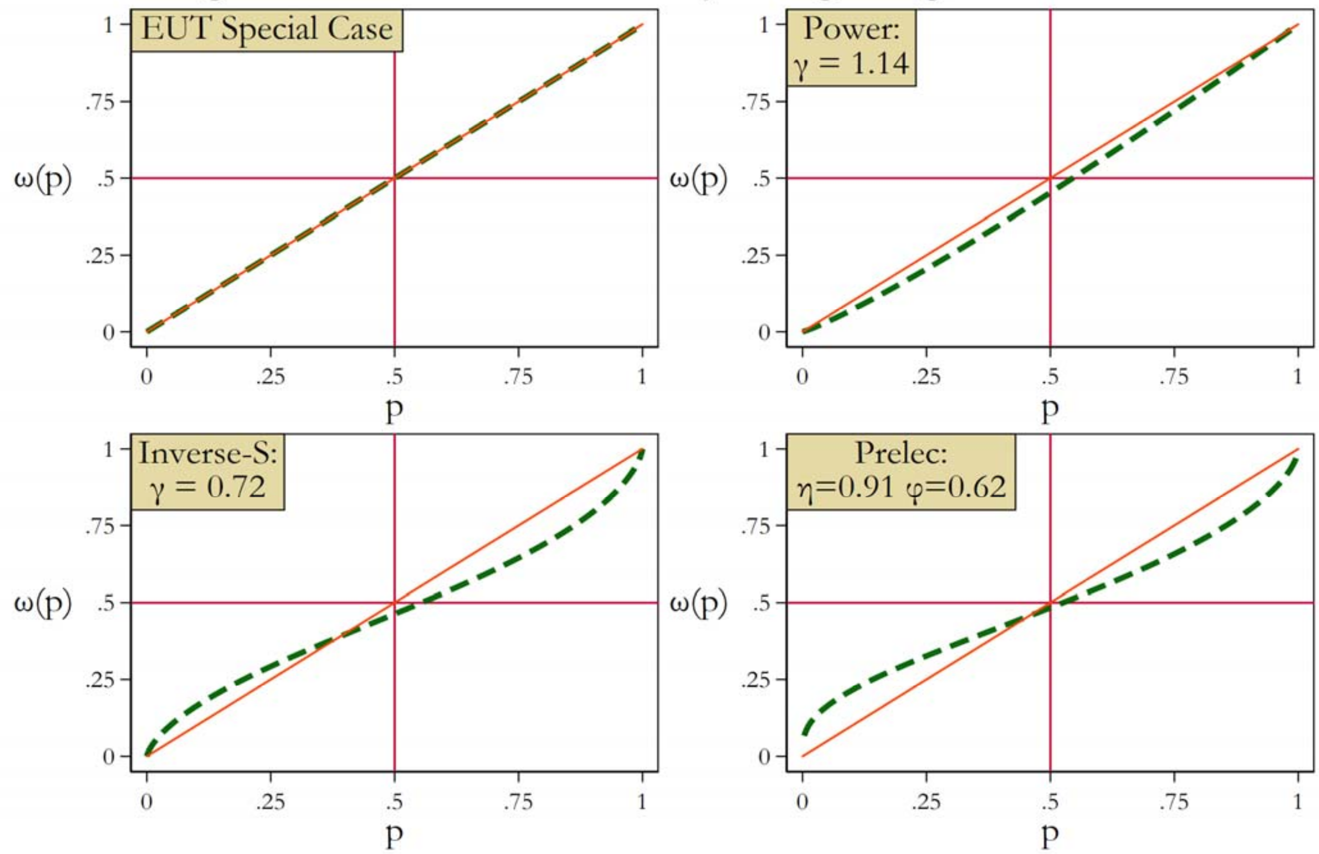
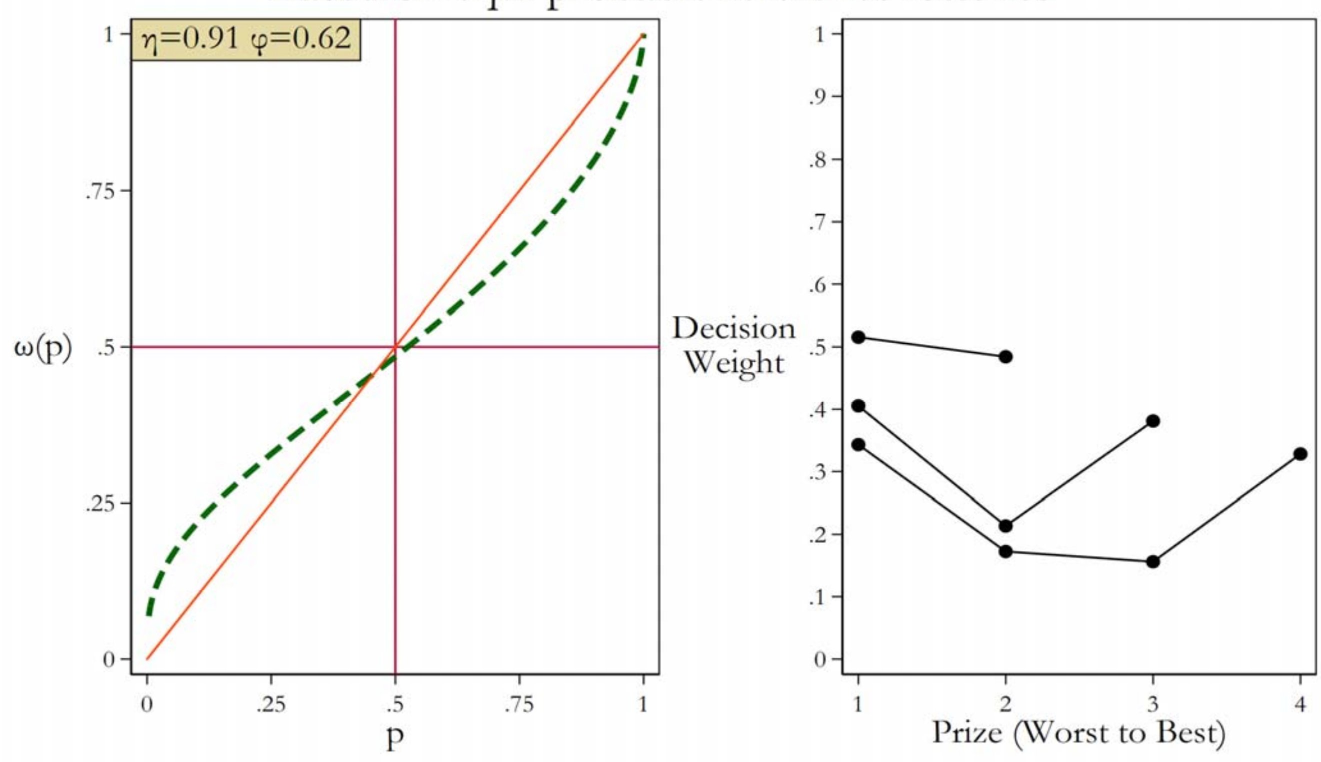


Figure E2: Prelec Probability Weighting and Implied Decision Weights for the Rank Dependent Model

Based on equi-probable reference lotteries



B. *Effects of Scaling Payoffs on Discounting*

A theoretical consideration addressed by the design is the hypothesized *magnitude effect*. As noted in the text, this effect refers to a behavioral regularity commonly reported in the psychological literature that humans tend to exhibit lower implied discount rates when payoffs are larger. Our basic design included a within-subjects control for this effect, by varying the principal in each and every session and for each subject.

In addition, we added one session to conduct a direct, between-subject test of the magnitude effect. This test is clean in the sense that it is the only change in the discounting task, but it is between-subjects, so not as powerful as the controls included in the main design. This extra session is referred to as treatment HiMag: it follows from the FED0 treatment and simply triples the FED0 SS amounts. Consequently, the SS amounts in the HiMag treatment range from $30 to $900. The HiMag LL rewards are calculated with the same interest rates as in the FED0 treatment. As with treatment FED0, the payout date for SS rewards was the day on which the session took place, while the payout date for LL rewards was 28 days after the session.[4]

We can consider the effect of this HiMag treatment by pooling the data from the HiMag and FED0 sessions and simply testing for a difference in the parameters of discounting models. The first set of estimates are for the Exponential discounting model, in which a dummy variable **scale** is added to the parameter for the discount rate, δ. The first set of estimates are for δ when **scale** = 0, and then the marginal and total effect of having **scale**=1 are estimated. The second set of estimates are for the Hyperbolic discounting model.

Assuming Exponential discounting, in this instance solely for descriptive purposes,[5] the discount rate with the lower scale is 368% with a standard error of 108%. The marginal effect of tripling the principals is -96% and is not statistically significant with a *p*-value of 0.12. The marginal effect is within one standard error of the baseline discount rate in FED0. Hence we reject the hypothesis that discounting

[4] In treatment HiMag, where the amounts were tripled, a selected amount was considered small and paid with certainty if it was less than $300. The payment of $300 or more was determined by the same stochastic mechanism described in the text for other treatments.

[5] This use of exponential discounting is common in the literature on the magnitude effect, surveyed in detail by Andersen, Harrison, Lau and Rutström [2013].

behavior is significantly affected by scale. In this instance the rigorous inferences from the structural analysis, controlling for U″ and noise around the predictions of a specific discounting model, differ from the apparent inferences from the descriptive analysis.

Since there is no FED, it is not possible to identify a Quasi-Hyperbolic model. The same qualitative result is obtained if one uses a Hyperbolic discounting model instead of the Exponential discounting model: as shown below, the marginal effect on the parameter *K* is negative and statistically insignificant with a *p*-value of 0.116.

Exponential Discounting

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
r	.6618651	.0142187	46.55	0.000	.6339969	.6897332
eta	.9126632	.0297868	30.64	0.000	.8542822	.9710441
phi	.6234766	.0261142	23.88	0.000	.5722937	.6746594
delta	3.68338	1.083254	3.40	0.001	1.560241	5.80652
muRA	.1397306	.0064571	21.64	0.000	.1270749	.1523863
muIDR	.041877	.0042702	9.81	0.000	.0335076	.0502465

. * marginal effect

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
delta_scale	-.9581774	.6161602	-1.56	0.120	-2.165829	.2494744

. * total effect

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
delta_scale	2.725203	.5228602	5.21	0.000	1.700416	3.74999

Hyperbolic Discounting

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
r	.6618649	.0142187	46.55	0.000	.6339967	.6897331
eta	.9126631	.0297868	30.64	0.000	.8542822	.9710441
phi	.6234764	.0261142	23.88	0.000	.5722936	.6746593
K	1.761581	.3198263	5.51	0.000	1.134733	2.388429
muRA	.1397306	.0064571	21.64	0.000	.1270749	.1523863
muIDR	.0863775	.0178411	4.84	0.000	.0514094	.1213455

```
.         * marginal effect
------------------------------------------------------------------------------
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
     K_scale |  -.2720977   .1732199    -1.57   0.116    -.6116025    .0674071
------------------------------------------------------------------------------

          * total effect
------------------------------------------------------------------------------
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
     K_scale |   1.489483    .173719     8.57   0.000        1.149    1.829966
------------------------------------------------------------------------------
```

C. *Models of Discounting*

These estimates are for individual discounting functions applied across the experimental treatments defined in the text. It is useful to show the raw estimates and then the transformed estimates, to make it clear how various constraints (e.g., non-negativity) have been implemented. For the Quasi-Hyperbolic and Weibull specifications the transformed coefficient estimates also include tests of the Exponential discounting special case.

Exponential Discounting

```
                                                Number of obs     =    397,470
                                                Wald chi2(0)      =          .
Log pseudolikelihood = -247977.33               Prob > chi2       =          .

                                    (Std. Err. adjusted for 270 clusters in id)
------------------------------------------------------------------------------
             |               Robust
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
r_           |
       _cons |  -.6716164   .0635196   -10.57   0.000    -.7961125   -.5471204
-------------+----------------------------------------------------------------
LNeta        |
       _cons |  -.0913884   .0326302    -2.80   0.005    -.1553424   -.0274345
-------------+----------------------------------------------------------------
LNphi        |
       _cons |  -.4724442   .0418758   -11.28   0.000    -.5545193   -.3903692
-------------+----------------------------------------------------------------
LNdelta      |
       _cons |   .6278592   .1399715     4.49   0.000       .35352    .9021984
-------------+----------------------------------------------------------------
LNmuRA       |
       _cons |  -1.968039   .0462012   -42.60   0.000    -2.058592   -1.877486
-------------+----------------------------------------------------------------
LNmuIDR      |
       _cons |  -3.134664   .0911954   -34.37   0.000    -3.313404   -2.955925
```

r: 1/(1+exp([r_]_cons))
eta: exp([LNeta]_cons)
phi: exp([LNphi]_cons)
delta: exp([LNdelta]_cons)
muRA: exp([LNmuRA]_cons)
muIDR: exp([LNmuIDR]_cons)

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
r	.661865	.0142157	46.56	0.000	.6340028	.6897272
eta	.9126631	.0297804	30.65	0.000	.8542947	.9710316
phi	.6234765	.0261086	23.88	0.000	.5723046	.6746484
delta	1.873595	.26225	7.14	0.000	1.359595	2.387596
muRA	.1397306	.0064557	21.64	0.000	.1270776	.1523836
muIDR	.0435144	.0039683	10.97	0.000	.0357366	.0512921

Hyperbolic Discounting

Number of obs = 397,470
Wald chi2(0) = .
Log pseudolikelihood = -247948.8   Prob > chi2 = .

(Std. Err. adjusted for 270 clusters in id)

	Coef.	Robust Std. Err.	z	P>|z|	[95% Conf.	Interval]
r_						
_cons	-.6687687	.0635471	-10.52	0.000	-.7933187	-.5442187
LNeta						
_cons	-.0913705	.0326141	-2.80	0.005	-.155293	-.027448
LNphi						
_cons	-.4729573	.0418791	-11.29	0.000	-.5550387	-.3908758
LN_K						
_cons	.1441531	.0935946	1.54	0.124	-.039289	.3275951
LNmuRA						
_cons	-1.967941	.0462063	-42.59	0.000	-2.058504	-1.877378
LNmuIDR						
_cons	-3.13657	.0919769	-34.10	0.000	-3.316841	-2.956298

r: 1/(1+exp([r_]_cons))
eta: exp([LNeta]_cons)
phi: exp([LNphi]_cons)
K: exp([LN_K]_cons)
muRA: exp([LNmuRA]_cons)
muIDR: exp([LNmuIDR]_cons)

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
r	.6612274	.0142349	46.45	0.000	.6333275	.6891273

eta	.9126795	.0297662	30.66	0.000	.8543388	.9710203
phi	.6231567	.0260972	23.88	0.000	.5720071	.6743063
K	1.155061	.1081075	10.68	0.000	.9431742	1.366948
muRA	.1397443	.0064571	21.64	0.000	.1270887	.1523999
muIDR	.0434315	.0039947	10.87	0.000	.0356021	.051261

Quasi-Hyperbolic Discounting

```
                                                Number of obs     =    397,470
                                                Wald chi2(0)      =          .
Log pseudolikelihood = -247926.71               Prob > chi2       =          .

                                   (Std. Err. adjusted for 270 clusters in id)
------------------------------------------------------------------------------
             |               Robust
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
r_           |
       _cons |  -.6716176   .0635196   -10.57   0.000    -.7961138   -.5471215
-------------+----------------------------------------------------------------
LNeta        |
       _cons |  -.0913884   .0326302    -2.80   0.005    -.1553424   -.0274345
-------------+----------------------------------------------------------------
LNphi        |
       _cons |  -.4724439   .0418758   -11.28   0.000     -.554519   -.3903689
-------------+----------------------------------------------------------------
beta_        |
       _cons |  -2.047029   .1082875   -18.90   0.000    -2.259269    -1.83479
-------------+----------------------------------------------------------------
LNdelta      |
       _cons |   .2519485   .2208169     1.14   0.254    -.1808447    .6847418
-------------+----------------------------------------------------------------
LNmuRA       |
       _cons |  -1.968039   .0462012   -42.60   0.000    -2.058592   -1.877486
-------------+----------------------------------------------------------------
LNmuIDR      |
       _cons |  -3.149456   .0945099   -33.32   0.000    -3.334692    -2.96422
------------------------------------------------------------------------------

          r:  1/(1+exp([r_]_cons))
        eta:  exp([LNeta]_cons )
        phi:  exp([LNphi]_cons)
       beta:  1.1/(1+exp([beta_]_cons))
   beta_exp:  1.1/(1+exp([beta_]_cons)) - 1
      delta:  exp([LNdelta]_cons)
       muRA:  exp([LNmuRA]_cons)
      muIDR:  exp([LNmuIDR]_cons)

------------------------------------------------------------------------------
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
           r |   .6618653   .0142157    46.56   0.000     .6340031    .6897275
         eta |   .9126631   .0297804    30.65   0.000     .8542947    .9710316
         phi |   .6234767   .0261086    23.88   0.000     .5723048    .6746485
        beta |   .9742118   .0120637    80.76   0.000     .9505675    .9978561
    beta_exp |  -.0257882   .0120637    -2.14   0.033    -.0494325   -.0021439
       delta |    1.28653   .2840876     4.53   0.000     .7297284    1.843331
        muRA |   .1397306   .0064557    21.64   0.000     .1270776    .1523836
       muIDR |   .0428754   .0040522    10.58   0.000     .0349334    .0508175
------------------------------------------------------------------------------
```

Weibull Discounting

```
                                                Number of obs     =    397,470
                                                Wald chi2(0)      =          .
Log pseudolikelihood = -247911.25               Prob > chi2       =          .

                                    (Std. Err. adjusted for 270 clusters in id)
------------------------------------------------------------------------------
             |               Robust
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
r_           |
       _cons |  -.6716157   .0635196   -10.57   0.000    -.7961117   -.5471196
-------------+----------------------------------------------------------------
LNeta        |
       _cons |  -.0913884   .0326302    -2.80   0.005    -.1553424   -.0274345
-------------+----------------------------------------------------------------
LNphi        |
       _cons |  -.4724444   .0418758   -11.28   0.000    -.5545195   -.3903693
-------------+----------------------------------------------------------------
LNdelta      |
       _cons |  -.4340106   .2264769    -1.92   0.055    -.8778971    .0098759
-------------+----------------------------------------------------------------
s_wei_       |
       _cons |   .2842131   .1394656     2.04   0.042     .0108655    .5575606
-------------+----------------------------------------------------------------
LNmuRA       |
       _cons |  -1.968039   .0462012   -42.60   0.000    -2.058592   -1.877486
-------------+----------------------------------------------------------------
LNmuIDR      |
       _cons |  -3.155662    .094096   -33.54   0.000    -3.340087   -2.971237
------------------------------------------------------------------------------

          r:  1 /(1 +  exp([r_]_cons))
        eta:  exp([LNeta]_cons )
        phi:  exp([LNphi]_cons)
      delta:  exp([LNdelta]_cons)
      s_wei:  exp([s_wei_]_cons)
  s_wei_exp:  exp([s_wei_]_cons) - 1
       muRA:  exp([LNmuRA]_cons)
      muIDR:  exp([LNmuIDR]_cons)

------------------------------------------------------------------------------
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
           r |   .6618648   .0142157    46.56   0.000     .6340027     .689727
         eta |   .9126631   .0297804    30.65   0.000     .8542947    .9710316
         phi |   .6234764   .0261086    23.88   0.000     .5723045    .6746482
       delta |   .6479054   .1467356     4.42   0.000     .3603089    .9355019
       s_wei |   1.328716   .1853102     7.17   0.000     .9655148    1.691917
   s_wei_exp |    .328716   .1853102     1.77   0.076    -.0344852    .6919173
        muRA |   .1397306   .0064557    21.64   0.000     .1270776    .1523836
       muIDR |   .0426102   .0040094    10.63   0.000     .0347518    .0504686
------------------------------------------------------------------------------
```

D. Estimates of Mixture Models

Table E1 is the same as Table 2, but with additional details provided for covariates.

Table E1: Estimates of Mixture Models

A. Mixture of Exponential and Hyperbolic Discounting

	Point Estimate	Standard Error	*p*-value	[95% Confidence Interval]	
δ	0.088	0.073	0.232	-0.056	0.231
K	0.806	0.154	0.000	0.504	1.108
π^E	0.295	0.138	0.033	0.024	0.566
π^H	0.705	0.138	0.000	0.434	0.976
π^E bundled	0.442	0.192	0.021	0.066	0.818
π^E second	-0.240	0.096	0.013	-0.429	-0.051
π^E forced	0.466	0.233	0.046	0.009	0.924
δ bundled	0.052	0.100	0.605	-0.145	0.248
δ second	-0.029	0.028	0.301	-0.085	0.026
δ forced	0.324	0.322	0.314	-0.307	0.955
δ magnitude10	0.584	0.523	0.265	-0.442	1.610
δ magnitude30	0.386	0.364	0.289	-0.327	1.099
δ magnitude60	0.133	0.120	0.265	-0.101	0.368
δ magnitude100	0.142	0.133	0.287	-0.119	0.403
K bundled	0.494	0.805	0.540	-1.084	2.072
K second	-0.345	0.278	0.215	-0.889	0.200
K forced	493.442	8830.990	0.955	-1.68e+04	17802.182
K magnitude10	2.085	0.529	0.000	1.048	3.121
K magnitude30	1.152	0.190	0.000	0.780	1.524
K magnitude60	0.526	0.095	0.000	0.341	0.712
Kmagnitude100	0.751	0.140	0.000	0.476	1.026

B. Mixture of Exponential and Quasi-Hyperbolic Discounting

	Point Estimate	Standard Error	*p*-value	[95% Confidence Interval]	
δ	0.107	0.083	0.200	-0.056	0.269
β	0.971	0.011	0.000	0.949	0.993
δ^{QH}	0.682	0.244	0.005	0.204	1.161
π^E	0.292	0.125	0.020	0.046	0.537
π^H	0.708	0.125	0.000	0.463	0.954
π^E bundled	0.429	0.203	0.035	0.031	0.828
π^E second	-0.236	0.099	0.017	-0.430	-0.042
π^E forced	0.474	0.197	0.016	0.087	0.860
δ bundled	0.027	0.109	0.804	-0.187	0.241
δ second	-0.036	0.035	0.311	-0.105	0.034
δ forced	0.416	0.445	0.349	-0.455	1.288
δ magnitude10	0.710	0.579	0.221	-0.426	1.845
δ magnitude30	0.463	0.388	0.232	-0.297	1.224

δ magnitude60	0.160	0.130	0.217	-0.094	0.414
δ magnitude100	0.172	0.144	0.231	-0.110	0.453
β bundled	0.005	0.040	0.894	-0.073	0.083
β second	0.033	0.033	0.313	-0.031	0.097
β forced	0.129	0.011	0.000	0.107	0.151
β magnitude10	-0.051	0.034	0.134	-0.117	0.016
β magnitude30	-0.030	0.018	0.096	-0.064	0.005
β magnitude60	-0.016	0.010	0.101	-0.036	0.003
β magnitude100	-0.003	0.009	0.720	-0.021	0.015
δ^{QH} bundled	0.963	1.387	0.487	-1.755	3.681
δ^{QH} second	-0.256	0.342	0.454	-0.927	0.414
δ^{QH} forced	6.70e+06	9.20e+06	0.466	-1.13e+07	2.47e+07
δ^{QH} magnitude10	3.614	1.946	0.063	-0.199	7.428
δ^{QH} magnitude30	1.600	0.648	0.014	0.330	2.871
δ^{QH} magnitude60	0.604	0.236	0.011	0.141	1.067
δ^{QH}magnitude100	1.240	0.542	0.022	0.178	2.302