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Risk Preferences Are Not Time Preferences*

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Abstract

Risk and time are intertwined. The present is known while the future is inherently risky. This is problematic when studying time preferences since uncontrolled risk can generate apparently present-biased behavior. We systematically manipulate risk in an intertemporal choice experiment. Discounted expected utility performs well with risk, but when certainty is added common ratio predictions fail sharply. The data cannot be explained by Prospect Theory, hyperbolic discounting, or preferences for resolution of uncertainty, but seem consistent with a direct preference for certainty. The data suggest strongly a difference between risk and time preferences.

JEL classification: D81, D90

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1 Introduction

Understanding individual decision-making under risk and over time are two foundations of economic analysis.[1] In both areas there has been research to suggest that standard models of expected utility (EU) and exponential discounting are flawed or incomplete. Regarding time, experimental research has uncovered evidence of a present bias, or hyperbolic discounting (Frederick et al., 2002). Regarding risk, there are number of well-documented departures from EU, such as the Allais (1953) common consequence and common ratio paradoxes.

An organizing principle behind expected utility violations is that they seem to arise as so-called 'boundary effects' where certainty and uncertainty are combined. Camerer (1992), Harless and Camerer (1994) and Starmer (2000) indicate that violations of expected utility are notably less prevalent when all choices are uncertain. This observation is especially interesting when considering decisions about risk taking over time. In particular, certainty and uncertainty are combined in intertemporal decisions: the present is known and certain, while the future is inherently risky. This observation is problematic if one intends to study time preference in isolation from risk. A critical question raised by our recent paper Andreoni and Sprenger (Forthcoming), which the study in this paper was designed to address, is whether behaviors identified as dynamically inconsistent, such as present bias or diminishing impatience, may instead be generated by unmeasured risk of the future, and exacerbated by non-EU boundary effects.[2] The primary objective of this paper is to explore this possibility in detail.

[1]Ellingsen (1994) provides a thorough history of the developments building towards expected utility theory and its cardinal representation. Frederick et al. (2002) provide a historical foundation of the discounted utility model from Samuelson (1937) on, and discuss the many experimental methodologies designed to elicit time preference.

[2]Machina (1989) discusses non-EU preferences generating dynamic inconsistencies. The link was also hypothesized in several hypothetical psychology studies (Keren and Roelofsma, 1995; Weber and Chapman, 2005), and Halevy (2008) shows that hyperbolic discounting can be reformulated in terms of non-EU probability weighting similar to the Prospect Theory formulations of Kahneman and Tversky (1979); Tversky and Kahneman (1992).

The focus here will be the model of discounted expected utility (DEU).[3] An essential prediction of the DEU model is that intertemporal allocations should depend only on *relative* intertemporal risk. For example, if a sooner reward will be realized 100 percent of the time and a later reward will be realized 80 percent of the time, then intertemporal allocations should be identical to when these probabilities are 50 percent and 40 percent, respectively. This is simply the common ratio property as applied to intertemporal risk in an ecologically relevant situation where present rewards are certain and future rewards are risky. The question for this research is whether the common ratio property holds both on and off this boundary of certainty in choices over time.

We ask this question in an experiment with 80 undergraduate subjects at the University of California, San Diego. Our test employs a method we call Convex Time Budgets (CTBs), developed in Andreoni and Sprenger (Forthcoming) and employed here under experimentally controlled risk. In CTBs, individuals allocate a budget of experimental tokens to sooner and later payments. Because the budgets are convex, we can use variation in the sooner times, later times, slopes of the budgets, and relative risk, to allow both precise identification of utility parameters and tests of structural discounting assumptions.[4]

We construct our test using two baseline risk conditions: 1) A risk-free condition where all payments, both sooner and later, will be made 100 percent of the time; and 2) a risky condition where, independently, sooner and later payments will be made only 50 percent of the time, with all uncertainty resolved during the experiment. Notice, under

[3]Interestingly, there are relatively few noted violations of the expected utility aspect of the DEU model. Loewenstein and Thaler (1989) and Loewenstein and Prelec (1992) document a number of anomalies in the *discounting* aspect of discounted utility models. Several examples are Baucells and Heukamp (2009); Gneezy et al. (2006) and Onay and Onculer (2007) who show that temporal delay can generate behavior akin to the classic common ratio effect, that the so-called 'uncertainty effect' is present for hypothetical intertemporal decisions, and that risk attitudes over temporal lotteries are sensitive to assessment probabilities, respectively.

[4]Prior research has relied on multiple price lists (Coller and Williams, 1999; Harrison et al., 2002), which require linear utility for identification of time preferences, or which have been employed in combination with risk measures to capture concavity of utility functions (Andersen et al., 2008). Our paper, Andreoni and Sprenger (Forthcoming), provides a comparison of the two approaches. In addition, recent work by Gine et al. (2010) shows that CTBs can be effectively used in field research.

the standard DEU model, CTB allocations in these two conditions should yield identical choices. The experimental results clearly violate DEU: 85 percent of subjects violate common ratio predictions and do so in more than 80 percent of opportunities. As we show, these violations in our baseline cannot be explained by non-EU concepts such as Prospect Theory probability weighting (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992; Tversky and Fox, 1995) temporally dependent probability weighting (Halevy, 2008), or preferences for early resolution of uncertainty (Kreps and Porteus, 1978; Chew and Epstein, 1989; Epstein and Zin, 1989).

Next we examine four conditions with differential risk, but common ratios of probabilities. For instance, we compare a condition in which the sooner payment is made 100 percent of the time while the later payment is made only 80 percent of the time, to one where the probabilities of each are halved, making both payments risky. We document substantial violations of common ratio predictions favoring the sooner certain payment. We mirror this design with conditions where the later payment has the higher probability, and find substantial violations of common ratio predictions favoring the *later* certain payment. Moreover, subjects who violate common ratio in the baseline conditions are more likely to violate DEU in these four additional conditions.

Our results reject DEU, Prospect Theory, and preference-for-resolution models when certainty is present. Perhaps most importantly, however, is that when certainty is not present subjects' behavior closely mirrors DEU predictions. Interestingly, this is close to the initial intuition for the Allais paradox. Allais (1953, p. 530) argued that when two options are far from certain, individuals act effectively as expected utility maximizers, while when one option is certain and another is uncertain a "disproportionate preference" for certainty prevails. This intuition may help to explain the frequent experimental finding of present-biased preferences when using monetary rewards (Frederick et al., 2002). That is, perhaps certainty, not intrinsic temptation, may be leading present payments to be disproportionately preferred.

We are not the first to suggest differences in risk can create apparent nonstationarity. For example, it is explicitly addressed in explorations of present bias and Prospect Theory (Halevy, 2008), and is implied by the dynamic inconsistency of non-EU models (Green, 1987; Machina, 1989). But since our results are inconsistent with Prospect Theory, they point to a different model of decision-making. Though elaboration of this model will be left to future work, we do offer some speculation in the direction of direct preferences for certainty (Neilson, 1992; Schmidt, 1998; Diecidue et al., 2004).[5]

In Section 2 of this paper, we develop the relevant hypotheses under DEU. In Section 3 we describe our experimental design and test these hypotheses. Section 4 presents results and Section 5 is a discussion and conclusion.

2 Conceptual Background

To motivate our experimental design, we briefly analyze decision problems for discounted expected utility, preference-for-resolution models, and Prospect Theory. When utility is time separable and stationary, the standard DEU model is written,

$$U = \sum_{k=0}^{T} \delta^{t+k} E[v(c_{t+k})],$$

governing intertemporal allocations. Simplify to assume two periods, t and $t+k$, and that consumption at time t will be c_t with probability p_1 and zero otherwise, while consumption at time $t+k$ will be c_{t+k} with probability p_2 and zero otherwise.[6] Under

[5]These models, termed u-v preferences, feature a discontinuity at certainty similar to the discontinuity at the present of β-δ time preferences (Laibson, 1997; O'Donoghue and Rabin, 1999). Importantly, u-v preferences necessarily violate first order stochastic dominance at certainty.

[6]For ease of explication we abstract away from additional intertemporal utility arguments used in the literature such as background consumption, intertemporal reference points, or Stone-Geary style utility shifters (Andersen et al., 2008; Andreoni and Sprenger, Forthcoming). However, the arguments are maintained with the more general utility function, $v(c_t - \omega)$, under the assumption that ω is not reoptimized in response to the experiment.

the standard construction, utility is

$$p_1\delta^t v(c_t) + p_2\delta^{t+k} v(c_{t+k}) + ((1-p_1)\delta^t + (1-p_2)\delta^{t+k})v(0).$$

Suppose an individual maximizes utility subject to the future value budget constraint

$$(1+r)c_t + c_{t+k} = m,$$

yielding the marginal condition

$$\frac{v'(c_t)}{\delta^k v'(c_{t+k})} = (1+r)\frac{p_2}{p_1},$$

and the solution

$$c_t = c_t^*(p_1/p_2; k, 1+r, m).$$

A key observation in this construction is that intertemporal allocations will depend only on the relative risk, p_1/p_2, and *not* on p_1 or p_2 separately. This is a critical and testable implication of the DEU model.

Hypothesis: For any (p_1, p_2) and (p_1', p_2') where $p_1/p_2 = p_1'/p_2'$, $c_t^*(p_1/p_2; k, 1+r, m) = c_t^*(p_1'/p_2'; k, 1+r, m)$.

This hypothesis is simply an intertemporal statement of the common ratio property of expected utility and represents a first testable implication for our experimental design. In further analysis it will be notationally convenient to use θ to indicate the *risk adjusted gross interest rate*,

$$\theta = (1+r)\frac{p_2}{p_1},$$

such that the tangency can be written as

$$\frac{v'(c_t)}{\delta^k v'(c_{t+k})} = \theta.$$

Provided that $v'(\cdot) > 0, v''(\cdot) < 0$, c_t^* will be increasing in p_1/p_2 and decreasing in $1+r$. As such, c_t^* will be decreasing in θ. In addition, for a given θ, c_t^* will be decreasing in $1 + r$. An increase in the interest rate will both raise the relative price of sooner consumption and reduce the consumption set.

There exist important utility formulations such as those developed by Kreps and Porteus (1978), Chew and Epstein (1989), and Epstein and Zin (1989) where the common ratio prediction does not hold. Behavior need not be identical if the uncertainty of p_1 and p_2 are resolved at different points in time, and individuals have preferences over the timing of the resolution of uncertainty. Our experimental design purposefully focuses on cases where all uncertainty is resolved immediately, before any payments are received, and as such the formulations of Kreps and Porteus (1978); Chew and Epstein (1989), and the primary classes discussed by Epstein and Zin (1989) will each reduce to standard expected utility.[7]

Of additional importance is the role of background risk. Dynamically inconsistent behavior may be related to time-dependent uncertainty in future consumption (see, e.g., Boyarchenko and Levendorskii, 2010). If individuals face background risk compounded with the objective probabilities, it will change the ratio of probabilities. However, a common ratio prediction will be maintained even if background risk differs across time periods. That is, when mixing (p_1, p_2) with background risk one arrives at the same probability ratio as when mixing (p_1', p_2') when $p_1/p_2 = p_1'/p_2'$.

[7] That is, when "... attention is restricted to choice problems/temporal lotteries where all uncertainty resolves at t = 0, there is a single 'mixing' of prizes and one gets the payoff vector [EU] approach" (Kreps and Porteus, 1978, p. 199). However, not all of the classes of recursive utility models discussed by Epstein and Zin (1989) will reduce to expected utility when all uncertainty is resolved immediately. The weighted utility class (Class 3) corresponding to the models of Dekel (1986) and Chew (1989) can accomodate expected utility violations even without a preference for sooner or later resolution of uncertainty.

A primary alternative to expected utility that may be relevant in intertemporal choice is Prospect Theory probability weighting (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992) and the related concept of rank-dependent expected utility (Quiggin, 1982). Probability weighting states that individuals 'edit' probabilities internally via a weighting function, $\pi(p)$. Though $\pi(p)$ may take a variety of forms, it is often argued to be monotonically increasing in the interval $[0, 1]$, with an inverted S-shaped, such that low probabilities are up-weighted and high probabilities are down-weighted (Tversky and Fox, 1995; Wu and Gonzalez, 1996; Prelec, 1998; Gonzalez and Wu, 1999). Probability weighting generates a common ratio prediction in some cases, but violates common ratio in others. In particular, if $p_1 = p_2$, $p'_1 = p'_2$, so $p_1/p_2 = p'_1/p'_2$, then it is also true that $\pi(p_1)/\pi(p_2) = \pi(p'_1)/\pi(p'_2) = 1$ as in DEU. However, for unequal probabilities, common ratio may be violated as the shape of the weighting function, $\pi(\cdot)$, changes the ratio of subjective probabilities.

An extension to Prospect Theory probability weighting is that probabilities are weighted by their temporal proximity (Halevy, 2008). Under this formulation, subjective probabilities are arrived at through a temporally dependent function $g(p, t) : [0, 1] \times \Re^+ \rightarrow [0, 1]$ where t represents the time at which payments will be made. Under a reasonable functional form of $g(\cdot)$, one could easily arrive at differences between the ratios $g(p_1, t)/g(p_2, t + k)$ and $g(p'_1, t)/g(p'_2, t + k)$ under a common ratio of objective probabilities.

These differences lead to a *new* risk adjusted interest rate similar to θ defined above,

$$\tilde{\theta}_{p_1,p_2} \equiv \frac{g(p_2, t + k)}{g(p_1, t)}(1 + r).$$

Note that either $\tilde{\theta}_{p_1,p_2} > \tilde{\theta}_{p'_1,p'_2}$ for all $(1+r)$ or $\tilde{\theta}_{p_1,p_2} < \tilde{\theta}_{p'_1,p'_2}$ for all $(1+r)$, depending on the form of $g(\cdot)$ chosen. Once one obtains a prediction as to the relationship between $\tilde{\theta}_{p_1,p_2}$ and $\tilde{\theta}_{p'_1,p'_2}$, it must hold for all gross interest rates. If c_t is decreasing in θ as discussed above, one should never observe a cross-over in behavior where for one gross interest rate c_t allocations are higher for (p_1, p_2) and for another gross interest rate c_t allocations are higher for (p'_1, p'_2). Such a cross-over is not consistent with either standard probability weighting or temporally dependent probability weighting of the form proposed by Halevy (2008). The central feature of these models is a separability between distorted probabilities and utility values. Because Prospect Theory is linear in *distorted* probabilities, it delivers a consistency in choice such that the applied distortions must be stable across interest rates.[8]

3 Experimental Design

In order to explore the development of Section 2 related to uncertain and certain intertemporal consumption, an experiment using Convex Time Budgets (CTB) (Andreoni and Sprenger, Forthcoming) under varying risk conditions was conducted at the Univeristy of California, San Diego in April of 2009. In each CTB decision, subjects were given a budget of experimental tokens to be allocated across a sooner payment, paid at time t, and a later payment, paid at time $t + k,\ k > 0$.[9] Two basic CTB environments consisting of 7 allocation decisions each were implemented under six different risk conditions. This generated a total of 84 experimental decisions for each subject. Eighty subjects participated in this study, which lasted about one hour.

[8] This stability may not be maintained under a combination of background risk and Prospect Theory probability weighting. The common ratio prediction may be violated if background risk and experimental payment risk are not evaluated separately or if background risk distributions are changing through time. Recent evidence suggests limited integration between risky experimental choice and background assets (Andersen et al., 2011), suggesting such arguments likely do not explain our results.

[9] An important issue in discounting studies is the presence of arbitrage opportunities. Subjects with even moderate access to liquidity should effectively arbitrage the experiment, borrowing low and saving high. Hence, researchers should be surprised to uncover the degree of present-biased behavior generally displayed in monetary discounting experiments (Frederick et al., 2002). The motivation of the present study is to explore the possibility that payment risk can rationalize such behavior even in the presence of arbitrage. Andreoni and Sprenger (Forthcoming) provide further discussion in this vein.

3.1 CTB Design Features

Sooner payments in each decision were always seven days from the experiment date ($t = 7$ days). We chose this 'front-end-delay' to avoid any direct impact of immediacy on decisions, including resolution timing effects, and to help eliminate differential transactions costs across sooner and later payments.[10] In one of the basic CTB environments, later payments were delayed 28 days ($k = 28$) and in the other, later payments were delayed 56 days ($k = 56$). The choice of t and k were set to avoid holidays, school vacation days and final examination week. Payments were scheduled to arrive on the same day of the week (t and k are both multiples of 7) to avoid weekday effects.

In each CTB decision, subjects were given a budget of 100 tokens. Tokens allocated to the sooner date had a value of a_t while tokens allocated to the later date had a value of a_{t+k}. In all cases, a_{t+k} was \$0.20 per token and a_t varied from \$0.20 to \$0.14 per token. Note that $a_{t+k}/a_t = (1+r)$, the gross interest rate over k days, and $(1+r)^{1/k}-1$ gives the standardized daily *net* interest rate. Daily net interest rates in the experiment varied considerably across the basic budgets, from 0 to 1.3 percent, implying annual interest rates of between 0 and 2116.6 percent (compounded quarterly). Table 1 shows the token values, gross interest rates, standardized daily interest rates and corresponding annual interest rates for the basic CTB budgets.

The basic CTB decisions described above were implemented in a total of six risk conditions. Let p_1 and p_2 be the (independent) probabilities that payment would be made for the sooner and later dates, respectively. The six conditions were $(p_1, p_2) \in \{(1, 1), (0.5, 0.5), (1, 0.8), (0.5, 0.4), (0.8, 1), (0.4, 0.5)\}$.

For all payments involving uncertainty, a ten-sided die was rolled immediately after all decisions were made to determine whether the payments would be sent. Hence, p_1 and p_2 were immediately known, independent, and subjects were told that different

[10] See below for the recruitment and payment efforts that allowed sooner payments to be implemented in the same manner as later payments. For discussions of front-end-delays in time preference experiments see Coller and Williams (1999); Harrison et al. (2005).

Table 1: Basic Convex Time Budget Decisions

t (start date)	k (delay)	Token Budget	a_t	a_{t+k}	$(1+r)$	Daily Rate (%)	Annual Rate (%)
7	28	100	0.20	0.20	1.00	0	0
7	28	100	0.19	0.20	1.05	0.18	85.7
7	28	100	0.18	0.20	1.11	0.38	226.3
7	28	100	0.17	0.20	1.18	0.58	449.7
7	28	100	0.16	0.20	1.25	0.80	796.0
7	28	100	0.15	0.20	1.33	1.03	1323.4
7	28	100	0.14	0.20	1.43	1.28	2116.6
7	56	100	0.20	0.20	1.00	0	0
7	56	100	0.19	0.20	1.05	0.09	37.9
7	56	100	0.18	0.20	1.11	0.19	88.6
7	56	100	0.17	0.20	1.18	0.29	156.2
7	56	100	0.16	0.20	1.25	0.40	246.5
7	56	100	0.15	0.20	1.33	0.52	366.9
7	56	100	0.14	0.20	1.43	0.64	528.0

random numbers would determine their sooner and later payments.[11]

The risk conditions serve several key purposes. To begin, the first and second conditions share a common ratio of $p_1/p_2 = 1$ and have $p_1 = p_2$. As discussed, in Section 2, DEU, preference-for-resolution models, and Prospect Theory probability weighting all make common ratio predictions in this context. Temporally dependent probability weighting of the form proposed by Halevy (2008) can generate common ratio violations in this context, but not cross-overs in experimental demands. Next, the third and fourth conditions share a common ratio of $p_1/p_2 = 1.25$, and only one payment is certain, the sooner 100 percent payment in the third condition. These conditions map to ecologically relevant decisions where sooner payments are certain and later payments are risky. That is, $(p_1, p_2) = (1, 0.8)$ is akin to decisions between the present and the future while $(p_1, p_2) = (0.5, 0.4)$ is akin to decisions between two subsequent future dates. In these conditions, DEU and preference-for-resolution models again make common ratio predictions, while probability weighting predicts violations if $\pi(1)/\pi(0.8) \neq \pi(0.5)/\pi(0.4)$. We mirror this design for completeness in the fifth

[11]See Appendix A.3 for the payment instructions provided to subjects.

and sixth conditions, which share a common ratio of $p_1/p_2 = 0.8$ and feature one later certain payment. Lastly, note that across conditions the sooner payment goes from being relatively less risky, $p_1/p_2 = 1.25$, to relatively more risky, $p_1/p_2 = 0.8$. Following the discussion of Section 2, subjects should respond to changes in relative risk, allocating smaller amounts to sooner payments when relative risk is low.

3.2 Implementation and Protocol

One of the most challenging aspects of implementing any time discounting study is making all choices equivalent except for their timing. That is, transactions costs associated with receiving payments, including physical costs and payment risk, must be minimized and equalized across all time periods. We took several unique steps in our subject recruitment process and our payment procedure in an attempt to accomplish this, once the experimentally manipulated uncertainty was resolved, as we explain next.

3.2.1 Recruitment and Experimental Payments

We recruited 80 undergraduate students. In order to participate in the experiment, subjects were required to live on campus. All campus residents are provided with individual mailboxes at their dormitories to use for postal service and campus mail. Each mailbox is locked and individuals have keyed access 24 hours per day.

All payments, both sooner and later, were placed in subjects' campus mailboxes by campus mail services, which allowed us to equate physical transaction costs across sooner and later payments. Campus mail services guarantees 100 percent delivery of mail, minimizing payment risk. This aspect of the design is crucial, as it is important that the riskiness of future payments be minimized to the greatest extent possible. Indeed, in a companion survey we find that 100 percent (80 of 80) of subjects believed they would receive their payments. Subjects were fully informed of the method of payment.[12]

Several other measures were also taken to equate transaction costs and minimize payment risk. Upon beginning the experiment, subjects were told that they would receive a $10 minimum payment for participating, to be received in two payments: $5 sooner and $5 later. All experimental earnings were added to these $5 minimum payments. Two blank envelopes were provided. After receiving directions about the two minimum payments, subjects addressed the envelopes to themselves at their campus mailbox. At the end of the experiment, subjects wrote their payment amounts and dates on the inside flap of each envelope such that they would see the amounts written in their own handwriting when payments arrived. All experimental payments were made by personal check from Professor James Andreoni drawn on an account at the university credit union.[13] Subjects were informed that they could cash their checks (if they so desired) at the university credit union. They were also given the business card of Professor James Andreoni and told to call or email him if a payment did not arrive and that a payment would be hand-delivered immediately. In sum, these measures serve to ensure that transaction costs and payment risk, including convenience, clerical error, and fidelity of payment were minimized and equalized across time.

One choice for each subject was selected for payment by drawing a numbered card at random. Subjects were told to treat each decision as if it were to determine their payments. This random-lottery mechanism, which is widely used in experimental economics, does introduce a compound lottery to the decision environment. Starmer and Sugden (1991) demonstrate that this mechanism does not create a bias in experimental response.

[12]See Appendix A.2 for the information provided to subjects.

[13]Payment choice was guided by a separate survey of 249 undergraduate economics students eliciting payment preferences. Personal checks from Professor Andreoni, Amazon.com gift cards, PayPal transfers and the university stored value system TritonCash were each compared to cash payments. Subjects were asked if they would prefer a twenty dollar payment made via each payment method or $\$X$ cash, where X was varied from 19 to 10. Personal checks were found to have the highest cash equivalent value. That is, the highest average value of $\$X$.

Figure 1: Sample Decision Sheet

2009 Calendar

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

IN EACH ROW ALLOCATE 100 TOKENS BETWEEN

PAYMENT A (1 week from today) **AND** **PAYMENT B** (4 weeks later)

Date A: April 8, 2009 — **Chance A Sent: 40%**

Date B: May 6, 2009 — **Chance B Sent: 50%**

No.	A Tokens		Rate A $ per token	Date A	&	B Tokens		Rate B $ per token	Date B
1.	______	tokens at	$.20 each	on April 8	&	______	tokens at	$.20 each	on May 6
2.	______	tokens at	$.19 each	on April 8	&	______	tokens at	$.20 each	on May 6
3.	______	tokens at	$.18 each	on April 8	&	______	tokens at	$.20 each	on May 6
4.	______	tokens at	$.17 each	on April 8	&	______	tokens at	$.20 each	on May 6
5.	______	tokens at	$.16 each	on April 8	&	______	tokens at	$.20 each	on May 6
6.	______	tokens at	$.15 each	on April 8	&	______	tokens at	$.20 each	on May 6
7.	______	tokens at	$.14 each	on April 8	&	______	tokens at	$.20 each	on May 6

PLEASE MAKE SURE A + B TOKENS = 100 IN EACH ROW!

3.2.2 Instrument and Protocol

The experiment was done with paper and pencil. Upon entering the lab subjects were read an introduction with detailed information on the payment process and a sample decision with different payment dates, token values and payment risks than those used in the experiment. Subjects were informed that they would work through 6 decision tasks. Each task consisted of 14 CTB decisions: seven with $t = 7,\ k = 28$ on one sheet and seven with $t = 7,\ k = 56$ on a second sheet. Each decision sheet featured a calendar, highlighting the experiment date, and the sooner and later payment dates, allowing subjects to visualize the payment dates and delay lengths.

Figure 1 shows a decision sheet. Identical instructions were read at the beginning of each task providing payment dates and the chance of being paid for each decision. Subjects were provided with a calculator and a calculation sheet transforming tokens to payment amounts at various token values. Four sessions were conducted over two days. Two orders of risk conditions were implemented to examine order effects.[14] Each day consisted of an early session (12 p.m.) and a late session (2 p.m.). The early session on the first day and the late session on the second day share a common order as do the late session on the first day and the early session on the second day. No order or session effects were found.

4 Results

The results are presented in two sub-sections. First, we examine behavior in the two baseline conditions: $(p_1, p_2) = (1, 1)$ and $(p_1, p_2) = (0.5, 0.5)$. We document violations common ratio predictions at both aggregate and individual levels and show a pattern of results that is generally incompatible with various probability weighting concepts. Second, we explore behavior in four further conditions where common ratios maintain

[14] In one order, (p_1, p_2) followed the sequence $(1, 1), (1, 0.8), (0.8, 1), (0.5, 0.5), (0.5, 0.4), (0.4, 0.5)$, while in the second it followed $(0.5, 0.5), (0.5, 0.4), (0.4, 0.5), (1, 1), (1, 0.8), (0.8, 1)$.

but only one payment is certain. Subjects exhibit a preference for certain payments relative to common ratio when they are available, but behave consistently with DEU away from certainty.

4.1 Behavior Under Certainty and Uncertainty

Section 2 provided a testable hypothesis for behavior across certain and uncertain intertemporal settings. For a given (p_1, p_2), if $p_1 = p_2 < 1$ then behavior should be identical to a similarly dated risk-free prospect, $(p_1 = p_2 = 1)$, at all gross interest rates, $1 + r$, and all delay lengths, k. Figure 2 graphs aggregate behavior for the conditions $(p_1, p_2) = (1, 1)$ (blue diamonds) and $(p_1, p_2) = (0.5, 0.5)$ (red squares) across the experimentally varied gross interest rates and delay lengths. The mean earlier choice of c_t and a 95 percent confidence interval (+/− 1.96 standard errors) are graphed.

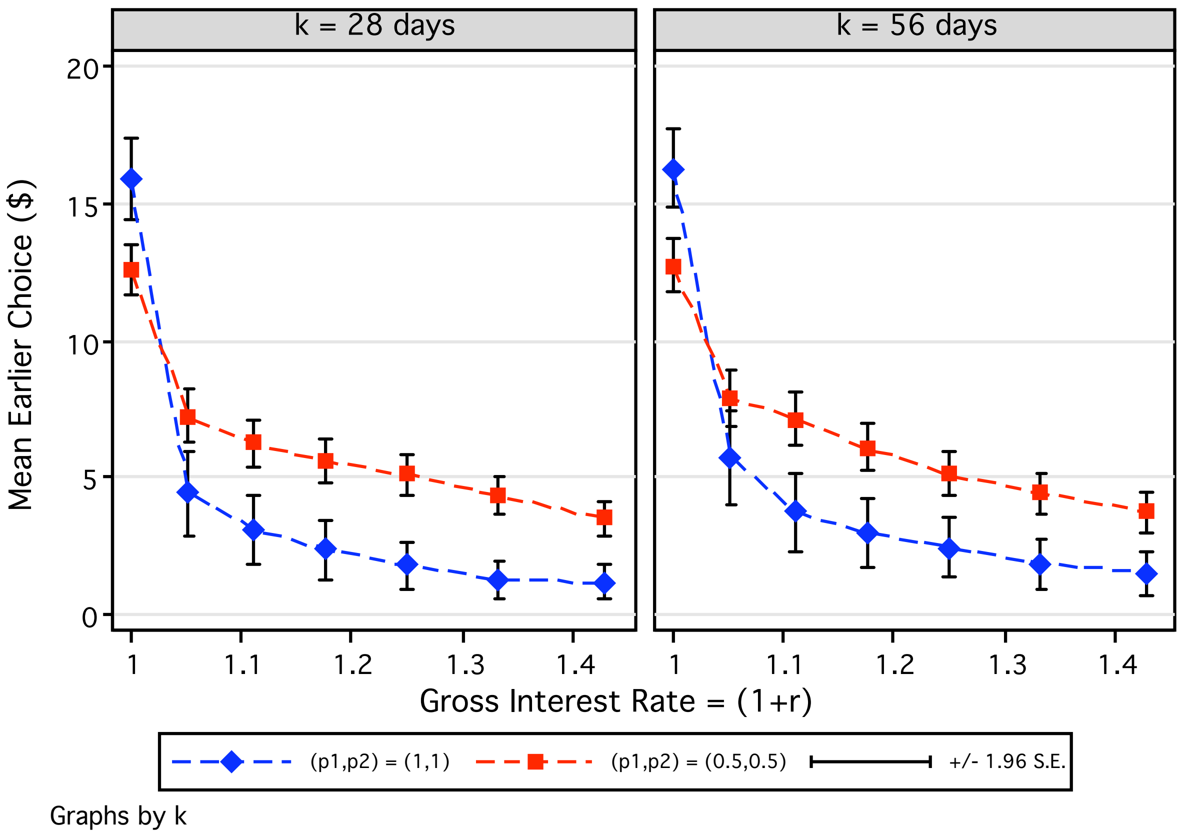
Under DEU, preference-for-resolution models, and standard probability weighting behavior should be identical across the two conditions. We find strong evidence to the contrary. In a hypothesis test of equality across the two conditions, the overall difference is found to be highly significant: $F_{14,79} = 6.07,\ p < .001$.[15]

The data follow an interesting pattern. In $(p_1, p_2) = (1, 1)$ and $(0.5, 0.5)$ conditions the allocation to sooner payments decrease as interest rates rise. However, at the lowest interest rate, c_t allocations are substantially higher in the $(1, 1)$ condition, and as the gross interest rate increases, $(1, 1)$ allocations drop steeply, crossing over the graph of the $(0.5, 0.5)$ condition.[16] This cross-over in behavior is in clear violation of discounted expected utility, all models that reduce to discounted expected utility when

[15]Test statistic generated from non-parametric OLS regression of choice on indicators for interest rate (7 levels), delay length (2 levels), risk condition (2 levels) and all interactions with clustered standard errors. F-statistic corresponds to null hypothesis that all risk condition terms have zero slopes. See Appendix Table A1 for regression.

[16]Indeed, in the $(1, 1)$ condition, 80.7 percent of allocations are at one or the other budget corners while only 26.1 percent are corner solutions in the $(0.5, 0.5)$ condition. We interpret the corner solutions in the $(1, 1)$ condition as evidence consistent with separability. See Andreoni and Sprenger (Forthcoming) for a full discussion of censoring issues in CTBs. The difference in allocations across conditions is obtained for all sessions and for all orders indicating no presence of order or day effects.

Figure 2: Aggregate Behavior Under Certainty and Uncertainty

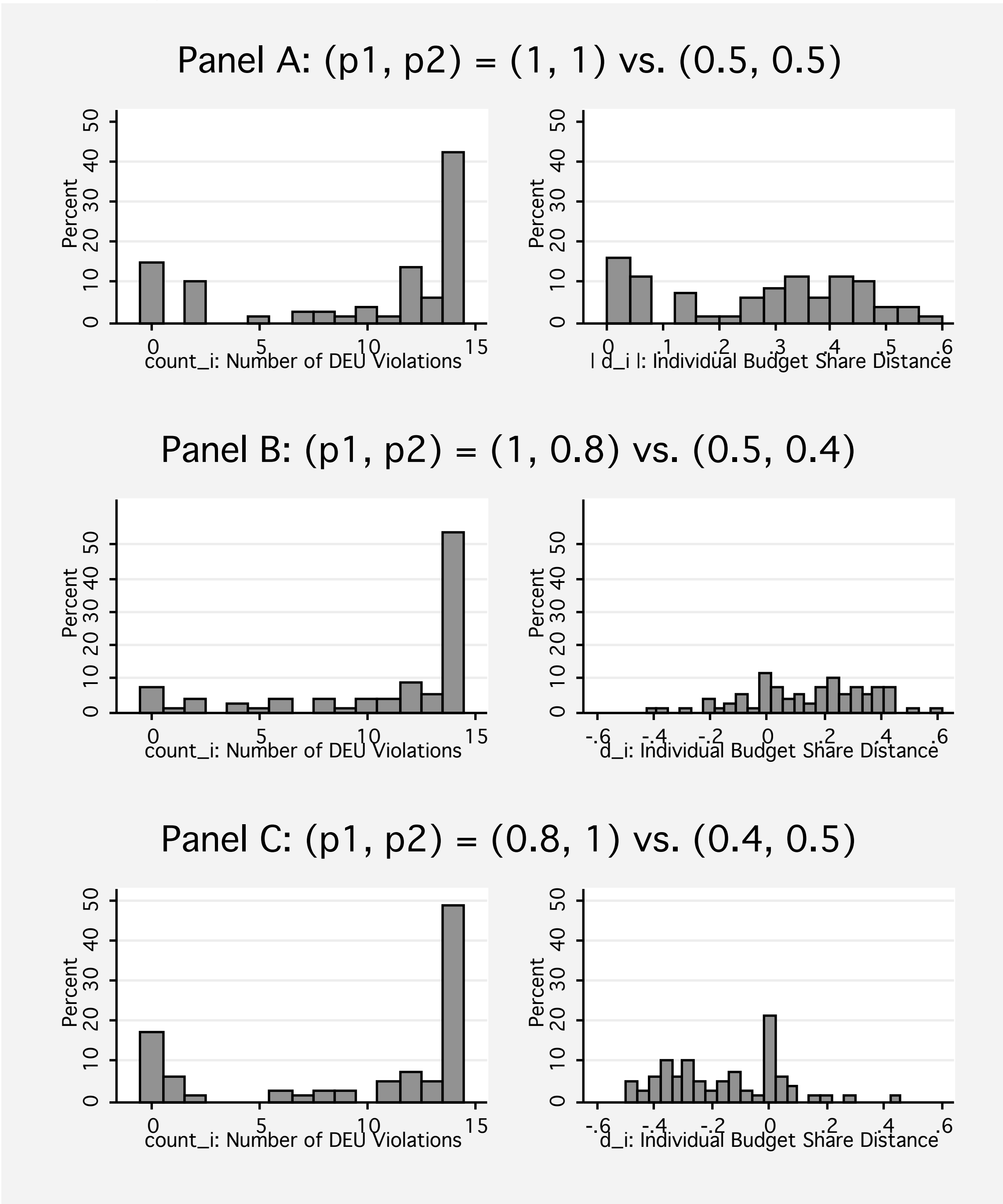


Note: The figure presents aggregate behavior for $N = 80$ subjects under two conditions: $(p_1, p_2) = (1, 1)$, i.e. no risk, in blue; and $(p_1, p_2) = (0.5, 0.5)$, i.e. 50 percent chance sooner payment would be sent *and* 50 percent chance later payment would be sent, in red. $t = 7$ *days* in all cases, $k \in \{28, 56\}$ *days*. Error bars represent 95 percent confidence intervals, taken as $+/-1.96$ standard errors of the mean. Test of H_0 : Equality across conditions: $F_{14,79} = 6.07$, $p < .001$.

uncertainty is immediately resolved, standard probability weighting, and temporally dependent probability weighting.

The aggregate violations of common ratio documented above are also supported in the individual data. Out of 14 opportunities to violate common ratio predictions, individuals do so an average of 9.68 ($s.d. = 5.50$) times. Only fifteen percent of subjects (12 of 80) commit zero violations of expected utility. For the 85 percent of subjects who do violate expected utility, they do so in more than 80 percent of opportunities, an average of 11.38 ($s.d. = 3.99$) times. Figure 3, Panel A presents a histogram of $count_i$, each subject's number of violations across conditions $(p_1, p_2) = (1, 1)$ and $(0.5, 0.5)$.

Figure 3: Individual Behavior Under Certainty and Uncertainty



Note: The figure presents individual violations across three common ratio comparisons. The variable $count_i$ is a count of each individual's common ratio violations and, d_i is each individual's budget share difference between common ratio conditions. Bin size for d_i is 0.04.

More than 40 percent of subjects violate common ratio predictions in all 14 opportunities. This may be a strict measure of violation as it requires identical allocation across risk conditions. s a complementary measure, we also present a histogram of $|d_i|$, the individual average budget share difference between risk conditions. For each individual and each CTB, we calculate the budget share of the sooner payment, $(1+r)c_t/m$. The average of each individual's 14 budget share differences between common ratio conditions is the measure d_i. Here we consider the average absolute difference.[17] e mean value of $|d_i|$ is 0.27 ($s.d. = 0.18$), indicating that individual violations are substantial, around 27 percent of the budget share. Indeed 63.8 percent of the sample (51/80) exhibit $|d_i| > 0.2$, indicating that violations are unlikely to be simple random response error.

4.2 Behavior with Differential Risk

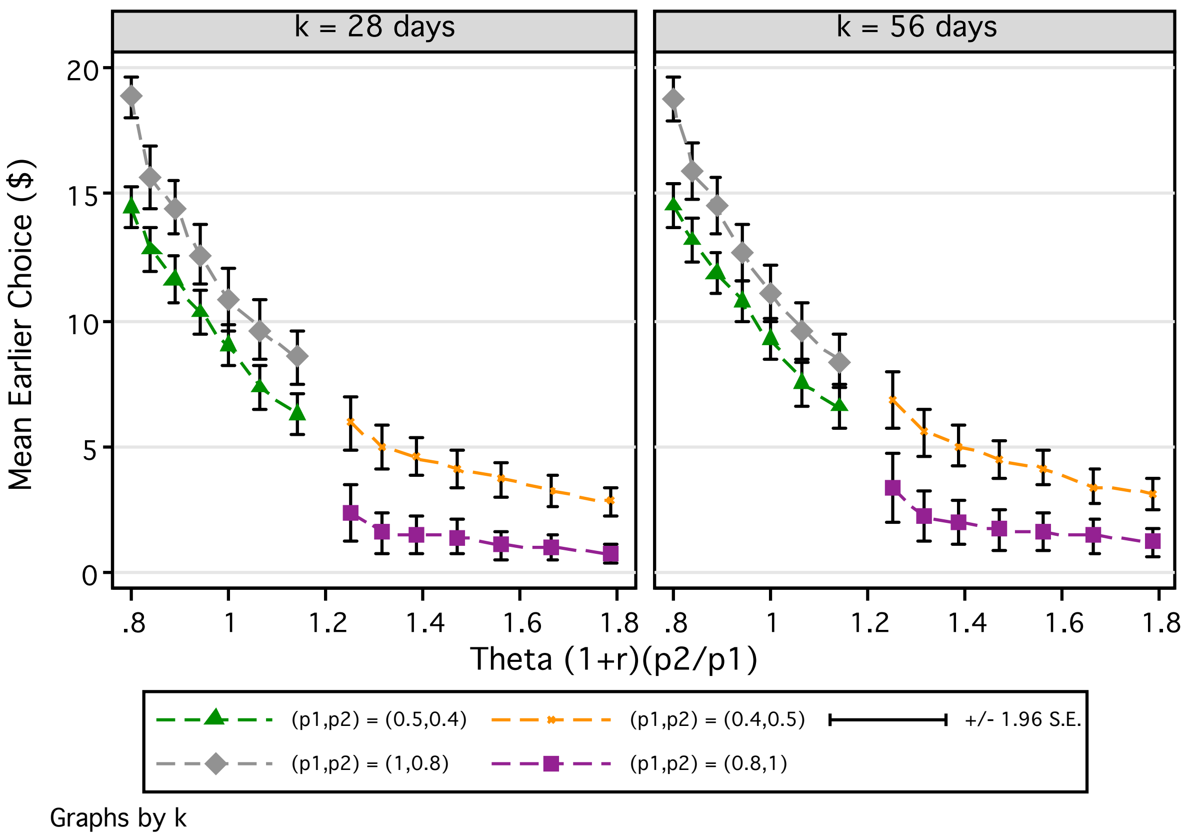
Next we explore the four conditions with differential risk. First, we discuss violations of common ratio when only one payment is certain. Second, we examine the three conditions where all payments are uncertain and document behavior consistent with discounted expected utility.

4.2.1 A Preference for Certainty

Figure 4 compares behavior in four conditions with differential risk but common ratios of probabilities. Condition $(p_1, p_2) = (1, 0.8)$ (gray diamonds) is compared to $(p_1, p_2) = (0.5, 0.4)$ (green triangles), and condition $(p_1, p_2) = (0.8, 1)$ (yellow circles) is compared to $(p_1, p_2) = (0.4, 0.5)$ (purple squares). The DEU model predicts equal allocations across conditions with common ratios. Interestingly, subjects' allocations demonstrate

[17] That is, the absolute value of each of the 14 differences is obtained prior to computing the average. When computing d_i across comparisons $(p_1, p_2) = (1, 0.8)$ vs. $(p_1, p_2) = (0.5, 0.4)$ and $(p_1, p_2) = (0.8, 1)$ and $(p_1, p_2) = (0.4, 0.5)$, the first budget share is subtracted from the second budget share to have a directional difference. Relative to common ratio, a preference for certainty would be exhibited by a positive d_i across $(p_1, p_2) = (1, 0.8)$ vs. $(p_1, p_2) = (0.5, 0.4)$ and a negative d_i across $(p_1, p_2) = (0.8, 1)$ and $(p_1, p_2) = (0.4, 0.5)$.

Figure 4: A Preference for Certainty



Note: The figure presents aggregate behavior for $N = 80$ subjects under four conditions: $(p_1, p_2) = (1, 0.8)$, $(p_1, p_2) = (0.5, 0.4)$, $(p_1, p_2) = (0.8, 1)$ and $(p_1, p_2) = (0.4, 0.5)$. Error bars represent 95 percent confidence intervals, taken as $+/-1.96$ standard errors of the mean. The first and second conditions share a common ratio as do the third and fourth. Test of H_0 : Equality across conditions $(p_1, p_2) = (1, 0.8)$ and $(p_1, p_2) = (0.5, 0.4)$: $F_{14,79} = 7.69$, $p < .001$. Test of H_0 : Equality across conditions $(p_1, p_2) = (0.8, 1)$ and $(p_1, p_2) = (0.4, 0.5)$: $F_{14,79} = 5.46$, $p < .001$.

a preference for certain payments relative to common ratio counterparts, regardless of whether the certain payment is sooner or later. Hypotheses of equal allocations across conditions are rejected in both cases.[18]

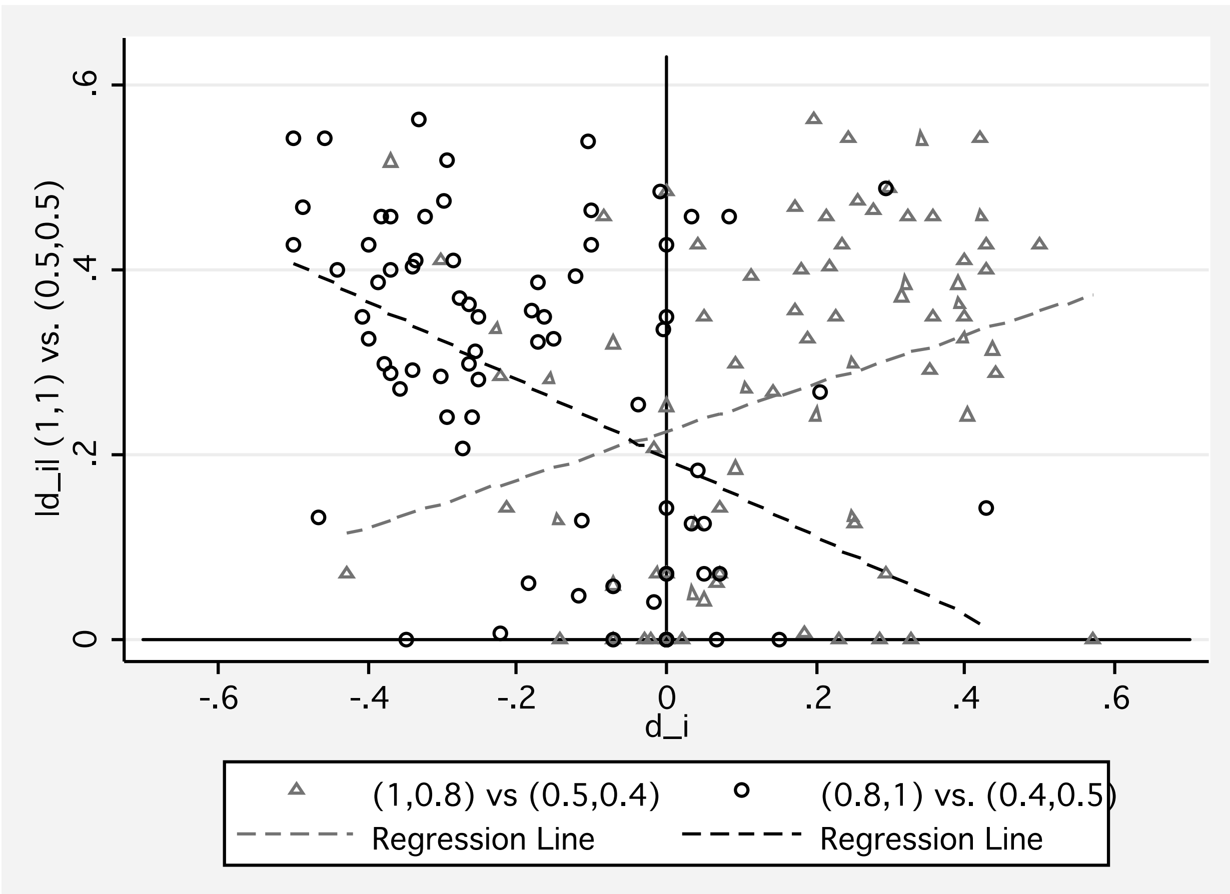
Figure 3, Panels B and C demonstrate that the individual behavior is organized in a similar manner. Individual violations of common ratio predictions are substantial. When certainty is sooner, across conditions $(p_1, p_2) = (1, 0.8)$ and $(p_1, p_2) = (0.5, 0.4)$, subjects commit an average of 10.90 ($s.d. = 4.67$) common ratio violations in 14 opportunities and only 7.5 percent of subjects commit zero violations. The average distance in budget shares, d_i, is 0.150 ($s.d. = 0.214$), which is significantly greater than zero ($t_{79} = 6.24,\ p < 0.01$), and in the direction of preferring the certain sooner payment. When certainty is later across conditions $(p_1, p_2) = (0.8, 1)$ and $(p_1, p_2) = (0.4, 0.5)$, subjects make an average of 9.68 ($s.d. = 5.74$) common ratio violations and 17.5 percent of subjects make no violations at all, similar to Panel A. The average distance in budget share, d_i, is -0.161 ($s.d. = 0.198$), which is significantly less than zero ($t_{79} = 7.27,\ p < 0.01$), and in the direction of preferring the certain later payment.

Importantly, violations of discounted expected utility correlate across experimental comparisons. Figure 5 plots budget share differences, d_i, across common-ratio comparisons. The difference $|d_i|$ from condition $(p_1, p_2) = (1, 1)$ vs. $(p_1, p_2) = (0.5, 0.5)$ is on the vertical axis while d_i across the alternate comparisons is on the horizontal axis. Common ratio violations correlate highly across experimental conditions. The more an individual violates common ratio across conditions $(p_1, p_2) = (1, 1)$ and $(p_1, p_2) = (0.5, 0.5)$ predicts how much he or she will demonstrate a common-ratio violation towards certainty when it is sooner in $(p_1, p_2) = (1, 0.8)$ vs. $(p_1, p_2) = (0.5, 0.4)$, ($\rho = 0.31,\ p < 0.01$), and when it is later in $(p_1, p_2) = (0.8, 1)$ vs. $(p_1, p_2) = (0.4, 0.5)$,

[18]For equality across $(p_1, p_2) = (1, 0.8)$ and $(p_1, p_2) = (0.5, 0.4)$ $F_{14,79} = 7.69,\ p < .001$ and for equality across $(p_1, p_2) = (0.8, 1)$ and $(p_1, p_2) = (0.4, 0.5)$ $F_{14,79} = 5.46,\ p < .001$. Test statistics generated from non-parametric OLS regression of choice on indicators for interest rate (7 levels), delay length (2 levels), risk condition (2 levels) and all interactions with clustered standard errors. F-statistic corresponds to null hypothesis that all risk condition terms have zero slopes. See Appendix Table A1 for regression.

($\rho = -0.47,\ p < 0.01$). Table 2 presents a correlation table for the number of violations $count_i$, and the budget proportion differences d_i, across comparisons and shows significant individual correlation across all conditions and measures of violation behavior.

Figure 5: Violation Behavior Across Conditions



Note: The figure presents the correlations of the budget share difference, d_i, across common ratio comparisons. $|d_i|$ across conditions $(p_1, p_2) = (1, 1)$ and $(p_1, p_2) = (0.5, 0.5)$ is on the vertical axis. d_i across the alternate comparisons is on the horizontal axis. Regression lines are provided. Corresponding correlation coefficients are $\rho = 0.31,\ (p < 0.01)$ for the triangular points $(p_1, p_2) = (1, 0.8)$ vs $(p_1, p_2) = (0.5, 0.4)$ and $\rho = -0.47,\ (p < 0.01)$ for the circular points $(p_1, p_2) = (0.8, 1)$ vs $(p_1, p_2) = (0.4, 0.5)$. See Table 2 for more details.

These findings are critical for two reasons. First, the common ratio violations observed in this sub-section could be predicted by a variety of formulations of Prospect Theory probability weighting (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992; Tversky and Fox, 1995; Wu and Gonzalez, 1996; Prelec, 1998; Gonzalez and Wu,

Table 2: Individual Violation Correlation Table

		$count_i$	$count_i$	$count_i$	$\lvert d_i \rvert$	d_i	d_i
		$(1, 1)$ vs. $(0.5, 0.5)$	$(1, 0.8)$ vs. $(0.5, 0.4)$	$(0.8, 1)$ vs. $(0.4, 0.5)$	$(1, 1)$ vs. $(0.5, 0.5)$	$(1, 0.8)$ vs. $(0.5, 0.4)$	$(0.8, 1)$ vs. $(0.4, 0.5)$
$count_i$	$(1, 1)$ vs. $(0.5, 0.5)$	1					
$count_i$	$(1, 0.8)$ vs. $(0.5, 0.4)$	0.56 ***	1				
$count_i$	$(0.8, 1)$ vs. $(0.4, 0.5)$	0.71 ***	0.72 ***	1			
$\lvert d_i \rvert$	$(1, 1)$ vs. $(0.5, 0.5)$	0.84 ***	0.40 ***	0.52 ***	1		
d_i	$(1, 0.8)$ vs. $(0.5, 0.4)$	0.31 ***	0.34 ***	0.28 **	0.31 ***	1	
d_i	$(0.8, 1)$ vs. $(0.4, 0.5)$	-0.55 ***	-0.412 ***	-0.61 ***	-0.47 ***	-0.34 ***	1

Notes: Pairwise correlations with 80 observations. The variable $count_i$ is a count of each individual's common ratio violations and, d_i is each individual's budget share difference between common ratio conditions. *Level of significance*: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

1999; Halevy, 2008). Hence, the violations of DEU documented in this sub-section, unlike those of sub-section 4.1 cannot reject a Prospect Theory interpretation to the data. Recognizing that violations correlate highly across contexts that can and cannot be explained by probability weighting suggests that Prospect Theory cannot provide a unified account for the data. It is important to note, however, that Prospect Theory is primarily motivated for the study of decision-making under uncertainty. Clearly, more research analyzing Prospect Theory predictions in atemporal choices is required before conclusions can be drawn. In one recent example, Andreoni and Sprenger (2011) reach conclusions similar to those here in an atemporal environment.

Second, these results strongly suggest that a preference for certainty may play a critical role in generating dynamic inconsistencies. Here we have demonstrated that certain sooner payments are preferred over uncertain later payments in a way that is inconsistent with DEU at both the aggregate and individual levels. This phenomenon clearly did not involve intrinsic present bias because first, the present was not directly involved and, second, the effect can be reversed by making later payments certain.

4.2.2 When All Choices Are Uncertain

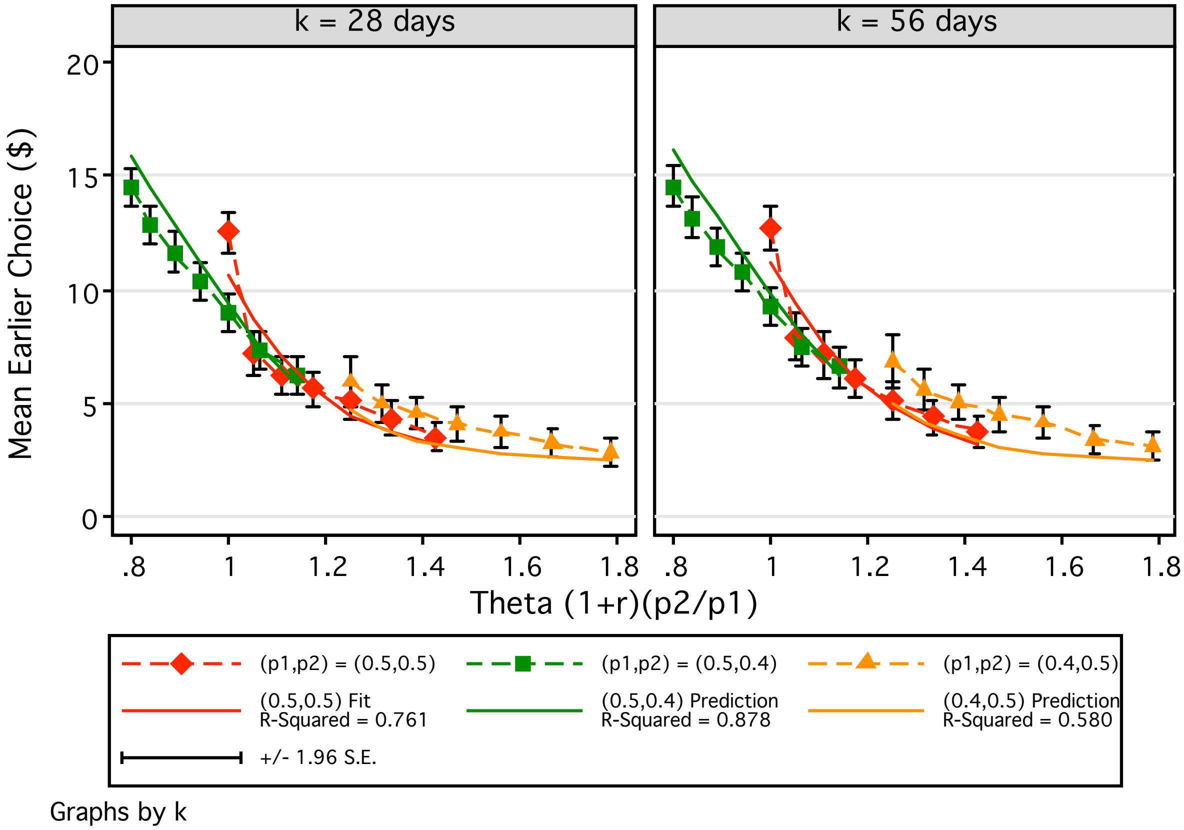
Figure 6 presents aggregate behavior from three risky situtations: $(p_1, p_2) = (0.5, 0.5)$ (red diamonds); $(p_1, p_2) = (0.5, 0.4)$ (green squares); and $(p_1, p_2) = (0.4, 0.5)$ (orange triangles) over the experimentally varied values of θ and delay length. The mean earlier choice of c_t is graphed along with error bars corresponding to 95 percent confidence intervals. We also plot predicted behavior based on structural discounting and utility estimates from the $(p_1, p_2) = (0.5, 0.5)$ data.[19] These out-of-sample predictions are plotted as solid lines in green and orange. The solid red line corresponds to the model fit for $(p_1, p_2) = (0.5, 0.5)$.

We highlight two dimensions of Figure 6. First, the theoretical predictions are 1) that c_t should be declining in θ; and 2) that if two decisions have identical θ then c_t should be higher in the condition with the lower interest rate.[20] These features are observed in the data. Allocations of c_t decline with θ and, where overlap of θ exists c_t

[19] Appendix A.1.1 describes the estimation procedure, the methodology for which was developed in Andreoni and Sprenger (Forthcoming). Appendix A.1.1 documents that a common set of parameters cannot simultaneously rationalize the $(p_1, p_2) = (0.5, 0.5)$ and $(p_1, p_2) = (1, 1)$ data. Appendix Table A2, column (6) provides corresponding estimates based on the $(p_1, p_2) = (0.5, 0.5)$ and $(p_1, p_2) = (1, 1)$ data. In both conditions, discounting is estimated to be around 30 percent per year. While substantial risk aversion is estimated from $(p_1, p_2) = (0.5, 0.5)$, limited utility function curvature is obtained when $(p_1, p_2) = (1, 1)$. Of interest is the close similarity between the $(p_1, p_2) = (1, 1)$ estimates and those obtained in Andreoni and Sprenger (Forthcoming), where payment risk was minimized and no experimental variation of risk was implemented.

[20] As discussed in Section 2, c_t should be monotonically decreasing in θ. Additionally, if $\theta = \theta'$ and $1 + r \neq 1 + r'$ then behavior should be identical up to a scaling factor related to the interest rates $1 + r$ and $1 + r'$. c_t should be higher in the lower interest rate condition due to income effects.

Figure 6: Aggregate Behavior Under Uncertainty



Note: The figure presents aggregate behavior for $N = 80$ subjects under three conditions: $(p_1, p_2) = (0.5, 0.5)$, i.e. equal risk, in red; $(p_1, p_2) = (0.5, 0.4)$, i.e. more risk later, in green; and $(p_1, p_2) = (0.4, 0.5)$, i.e. more risk sooner, in orange. Error bars represent 95 percent confidence intervals, taken as $+/- 1.96$ standard errors of the mean. Solid lines correspond to predicted behavior using utility estimates from $(p_1, p_2) = (0.5, 0.5)$ as estimated in Appendix Table A2, column (6).

is generally higher for lower gross interest rates.[21] Second, out of sample predictions match actual aggregate behavior. Indeed, the out-of-sample calculated R^2 values are high: 0.878 for $(p_1, p_2) = (0.5, 0.4)$ and 0.580 for $(p_1, p_2) = (0.4, 0.5)$.[22]

Figure 6 demonstrates that in situations where all payments are risky, the results are

[21] This pattern of allocations is obtained for all sessions and for all orders indicating no presence of order or day effects.

[22] By comparison, making similar out of sample predictions using utility estimates from $(p_1, p_2) = (1, 1)$ yields predictions that diverge dramatically from actual behavior (see Appendix Figure A2) and lowers R^2 values to 0.767 and 0.462, respectively. This suggests that accounting for differential utility function curvature in risky situations allows for an improvement of fit on the order of 15-25 percent.

surprisingly consistent with the DEU model. Though subjects exhibited a preference for certainty when it is available, away from certainty they trade off relative risk and interest rates like expected utility maximizers, and utility parameters measured under uncertainty predict behavior out-of-sample extremely well.[23]

5 Discussion and Conclusion

Intertemporal decision-making involves a combination of certainty and uncertainty. The present is known while the future is inherently risky. In an intertemporal allocation experiment under varying risk conditions, we document violations of discounted expected utility's common ratio predictions. Additionally the pattern of results are inconsistent with various Prospect Theory probability weighting formulations. Subjects exhibit a preference for certainty when it is available, but behave largely as discounted expected utility maximizers away from certainty.

Our results have substantial implications for intertemporal decision theory. particular, present bias has been frequently documented (Frederick et al., 2002) and is argued to be a dynamically inconsistent discounting phenomenon generated by diminishing impatience through time. Our results suggest that present bias may have an alternate source. If individuals exhibit a preference for certainty when it is available, then present certain consumption will be favored over future uncertain consumption. When only uncertain future consumption is considered, individuals act more closely in line with expected utility and apparent preference reversals are generated.

Other research has discussed the possibility that certainty plays a role in generating present bias (Halevy, 2008). Additionally such a notion is implicit in the recognized dynamic inconsistency of non-expected utility models (Green, 1987; Machina, 1989),

[23]Prospect theory probability weighting would make a similar prediction as many of the functional forms used in the literature are near linear at intermediate probabilities (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992; Tversky and Fox, 1995; Wu and Gonzalez, 1996; Prelec, 1998; Gonzalez and Wu, 1999).

and could be thought of as preferring immediate resolution of uncertainty (Kreps and Porteus, 1978; Chew and Epstein, 1989; Epstein and Zin, 1989). Our results point in a new direction: that certainty, per se, may be disproportionately preferred. We interpret our findings as being consistent with the intuition of the Allais Paradox (Allais, 1953). Allais (1953, p. 530) argued that when two options are far from certain, individuals act effectively as discounted expected utility maximizers, while when one option is certain and another is uncertain a disproportionate preference for certainty prevails. This intuition is captured closely in the *u*-*v* preference models of Neilson (1992), Schmidt (1998), and Diecidue et al. (2004) predicting the observed behavior across our experimental conditions, is a feature of belief-dependent utility (Dufwenberg, 2008) and expectations-based reference dependence (Bell, 1985; Loomes and Sugden, 1986; Koszegi and Rabin, 2006, 2007), and may help researchers to understand the origins of dynamic inconsistency, build sharper theoretical models, provide richer experimental tests, and form more careful policy prescriptions regarding intertemporal choice.

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A Appendix: Not For Publication

A.1 Appendix Tables

Table A1: Non-Parametric Estimates of DEU Violations

	Comparison		
	$(p_1, p_2) = (1, 1)$ vs. $(0.5, 0.5)$	$(p_1, p_2) = (1, 0.8)$ vs. $(0.5, 0.4)$	$(p_1, p_2) = (0.8, 1)$ vs. $(0.4, 0.5)$
Dependent Variable:		c_t Allocations	
Risk Conditions			
Condition $(p_1, p_2) = (1, 1)$	3.350***		
	(0.772)		
Condition $(p_1, p_2) = (1, 0.8)$		4.418***	
		(0.558)	
Condition $(p_1, p_2) = (0.8, 1)$			-3.537***
			(0.684)
Interest Rate x Delay Length Categories			
$(1 + r, k) = (1.00, 28)$	-	-	-
$(1 + r, k) = (1.05, 28)$	-5.318***	-1.651***	-0.967*
	(0.829)	(0.316)	(0.452)
$(1 + r, k) = (1.11, 28)$	-6.294***	-2.818***	-1.382**
	(0.812)	(0.434)	(0.454)
$(1 + r, k) = (1.18, 28)$	-6.921***	-4.140***	-1.851***
	(0.780)	(0.490)	(0.455)
$(1 + r, k) = (1.25, 28)$	-7.438***	-5.449***	-2.222***
	(0.755)	(0.544)	(0.488)
$(1 + r, k) = (1.33, 28)$	-8.187***	-7.139***	-2.742***
	(0.721)	(0.668)	(0.496)
$(1 + r, k) = (1.43, 28)$	-9.039***	-8.164***	-3.126***
	(0.677)	(0.658)	(0.503)
$(1 + r, k) = (1.00, 56)$	0.193	0.073	0.873*
	(0.192)	(0.211)	(0.395)
$(1 + r, k) = (1.05, 56)$	-4.600***	-1.290***	-0.352
	(0.791)	(0.336)	(0.442)
$(1 + r, k) = (1.11, 56)$	-5.409***	-2.582***	-0.923
	(0.805)	(0.331)	(0.515)
$(1 + r, k) = (1.18, 56)$	-6.462***	-3.685***	-1.451**
	(0.796)	(0.480)	(0.513)
$(1 + r, k) = (1.25, 56)$	-7.436***	-5.227***	-1.812***
	(0.758)	(0.544)	(0.512)
$(1 + r, k) = (1.33, 56)$	-8.118***	-6.979***	-2.532***
	(0.740)	(0.652)	(0.493)
$(1 + r, k) = (1.43, 56)$	-8.775***	-7.882***	-2.833***
	(0.713)	(0.656)	(0.477)
Risk Condition Interactions: Relevant Risk Condition x			
$(1 + r, k) = (1.05, 28)$	-6.148***	-1.544*	0.134
	(1.111)	(0.602)	(0.421)
$(1 + r, k) = (1.11, 28)$	-6.493***	-1.574**	0.498
	(1.048)	(0.573)	(0.446)
$(1 + r, k) = (1.18, 28)$	-6.597***	-2.131**	0.849
	(0.981)	(0.708)	(0.463)
$(1 + r, k) = (1.25, 28)$	-6.666***	-2.584**	0.920
	(0.971)	(0.762)	(0.576)
$(1 + r, k) = (1.33, 28)$	-6.425***	-2.136**	1.319*
	(0.917)	(0.764)	(0.601)
$(1 + r, k) = (1.43, 28)$	-5.683***	-2.170**	1.443*
	(0.880)	(0.728)	(0.623)
$(1 + r, k) = (1.00, 56)$	0.192	-0.180	0.107
	(0.450)	(0.243)	(0.602)
$(1 + r, k) = (1.05, 56)$	-5.540***	-1.646**	0.156
	(1.088)	(0.616)	(0.557)
$(1 + r, k) = (1.11, 56)$	-6.734***	-1.781**	0.511
	(1.093)	(0.588)	(0.521)
$(1 + r, k) = (1.18, 56)$	-6.450***	-2.471***	0.747
	(1.040)	(0.719)	(0.644)
$(1 + r, k) = (1.25, 56)$	-6.006***	-2.576***	0.994
	(0.975)	(0.714)	(0.636)
$(1 + r, k) = (1.33, 56)$	-5.911***	-2.286**	1.604**
	(0.974)	(0.781)	(0.587)
$(1 + r, k) = (1.43, 56)$	-5.574***	-2.618***	1.639*
	(0.936)	(0.702)	(0.654)
Constant (Omitted Category)	12.537***	14.455***	5.950***
	(0.464)	(0.424)	(0.554)
H_0: Zero Condition Slopes	$F_{14,79} = 6.07$	$F_{14,79} = 7.69$	$F_{14,79} = 5.46$
	$(p < 0.01)$	$(p < 0.01)$	$(p < 0.01)$
# Observations	2240	2240	2240
# Clusters	80	80	80
R^2	0.429	0.360	0.173

Notes: Clustered standard errors in parentheses. $F_{14,79}$ statistics correspond to hypothesis tests of zero slopes for risk condition regressor and 13 risk condition interactions.

A.1.1 Estimating Preference Parameters

In this appendix we discuss structural estimation of intertemporal preference parameters. We document that a common set of DEU parameters cannot simultaneously rationalize the $(p_1, p_2) = (0.5, 0.5)$ and $(p_1, p_2) = (1, 1)$ data, providing structural support for the claim that risk preferences are not time preferences. Additionally, the parameter estimates are used out of sample to predict behavior both in Figure 6 and in Figure A2. The evidence indicates that away from certainty the data adhere closely to DEU parameters estimated from $(p_1, p_2) = (0.5, 0.5)$, but are far from those estimated from $(p_1, p_2) = (1, 1)$.

Given structural assumptions, the design allows us to estimate utility parameters, following methodology developed in Andreoni and Sprenger (Forthcoming). We assume an exponentially discounted CRRA utility function,

$$U = p_1\delta^t(c_t - \omega)^\alpha + p_2\delta^{t+k}(c_{t+k} - \omega)^\alpha,$$

where δ represents exponential discounting, α represents utility function curvature and ω is a background parameter that could be interpreted as a Stone-Geary minimum.[24] We posit an exponential discounting function because for timing and transaction cost reasons no present payments were provided. This precludes direct analysis of present-biased or quasi-hyperbolic time preferences (Strotz, 1956; Phelps and Pollak, 1968; Laibson, 1997; O'Donoghue and Rabin, 1999). Under this formulation, the DEU solution function, c_t^*, can be written as

$$c_t^*(p_1/p_2, t, k, 1{+}r, m) = \frac{[1 - (\frac{p_2}{p_1}(1+r)\delta^k)^{\frac{1}{\alpha-1}}]}{[1 + (1+r)(\frac{p_2}{p_1}(1+r)\delta^k)^{\frac{1}{\alpha-1}}]}\omega + \frac{[(\frac{p_2}{p_1}(1+r)\delta^k)^{\frac{1}{\alpha-1}}]}{[1 + (1+r)(\frac{p_2}{p_1}(1+r)\delta^k)^{\frac{1}{\alpha-1}}]}m,$$

or

$$c_t^*(\theta, t, k, 1+r, m) = \frac{[1 - (\theta\delta^k)^{\frac{1}{\alpha-1}}]}{[1 + (1+r)(\theta\delta^k)^{\frac{1}{\alpha-1}}]}\omega + \frac{[(\theta\delta^k)^{\frac{1}{\alpha-1}}]}{[1 + (1+r)(\theta\delta^k)^{\frac{1}{\alpha-1}}]}m. \qquad (1)$$

We estimate the parameters of this function via non-linear least squares with standard errors clustered on the individual level to obtain $\hat{\alpha}$, $\hat{\delta}$, and $\hat{\omega}$. An estimate of the annual discount rate is generated as $1/\hat{\delta}^{365} - 1$, with corresponding standard error obtained via the delta method.

Table A2 presents discounting and curvature parameters estimated from the two conditions $(p_1, p_2) = (1, 1)$ and $(p_1, p_2) = (0.5, 0.5)$. In column (1), we estimate a baseline model where discounting, curvature, and background parameters are restricted to be equal across the two risk conditions. The aggregate discount rate is estimated to

[24] The ω terms could be also be interpreted as intertemporal reference points or background consumption. Frequently in the time preference literature, the simplification $\omega = 0$ is imposed or ω is interpreted as *minus* background consumption (Andersen et al., 2008) and calculated from an external data source. In Andreoni and Sprenger (Forthcoming) we provide methodology for estimating the background parameters and employ this methodology here. Detailed discussions of sensitivity and censored data issues are provided in Andreoni and Sprenger (Forthcoming) who show that accounting for censoring issues has little influence on estimates.

be around 27 percent per year and aggregate curvature is estimated to be 0.98. The background parameter, $\hat{\omega}$ is estimated to be 3.61.

Table A2: Discounting and Curvature Parameter Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\alpha}$	0.982		0.984			
	(0.002)		(0.002)			
$\hat{\alpha}_{(1,1)}$		0.987		0.987	0.988	0.988
		(0.002)		(0.002)	(0.002)	(0.002)
$\hat{\alpha}_{(0.5,0.5)}$		0.950		0.951	0.885	0.883
		(0.008)		(0.008)	(0.017)	(0.017)
Rate	0.274			0.285		0.284
	(0.035)			(0.036)		(0.037)
Rate$_{(1,1)}$		0.281	0.276		0.282	
		(0.036)	(0.039)		(0.036)	
Rate$_{(0.5,0.5)}$		0.321	0.269		0.315	
		(0.059)	(0.033)		(0.088)	
$\hat{\omega}$	3.608				2.417	2.414
	(0.339)				(0.418)	(0.418)
$\hat{\omega}_{(1,1)}$		2.281	2.106	2.285		
		(0.440)	(0.439)	(0.439)		
$\hat{\omega}_{(0.5,0.5)}$		4.397	5.260	4.427		
		(0.321)	(0.376)	(0.324)		
H_0: Equality		$F_{3,79} = 16.12$	$F_{2,79} = 30.47$	$F_{2,79} = 23.24$	$F_{2,79} = 37.97$	$F_{1,79} = 38.09$
		($p < 0.01$)	($p < 0.01$)	($p < 0.01$)	($p < 0.01$)	($p < 0.01$)
R^2	0.642	0.675	0.672	0.675	0.673	0.673
N	2240	2240	2240	2240	2240	2240
Clusters	80	80	80	80	80	80

Notes: NLS solution function estimators. Subscripts refer to (p_1, p_2) condition. Column (1) imposes the interchangeability, $v(\cdot) = u(\cdot)$. Column (2) allows different curvature, discounting and background parameters in each (p_1, p_2) condition. Column (3) restricts curvature to be equal across conditions. Column (4) restricts discounting to be equal across conditions. Column (5) restricts the background parameter ω to be equal across conditions. Column (6) restricts the background parameter ω and discounting to be equal across conditions. Clustered standard errors in parentheses. F statistics correspond to hypothesis tests of equality of parameters across conditions. Rate: Annual discount rate calculated as $(1/\hat{\delta})^{365} - 1$, standard errors calculated via the delta method.

In column (2), we estimate separate discounting, curvature and background parameters for the two risk conditions. That is, we estimate a certain $v(\cdot)$ and an uncertain $u(\cdot)$. Discounting is found to be similar across the conditions, around 30 percent per year ($F_{1,79} = 0.69,\ p = 0.41$).[25] In the certain condition, $(p_1, p_2) = (1, 1)$, we find almost linear utility while in the uncertain condition, $(p_1, p_2) = (0.5, 0.5)$, we estimate

[25]For comparison, using similar methodology without uncertainty Andreoni and Sprenger (Forth-

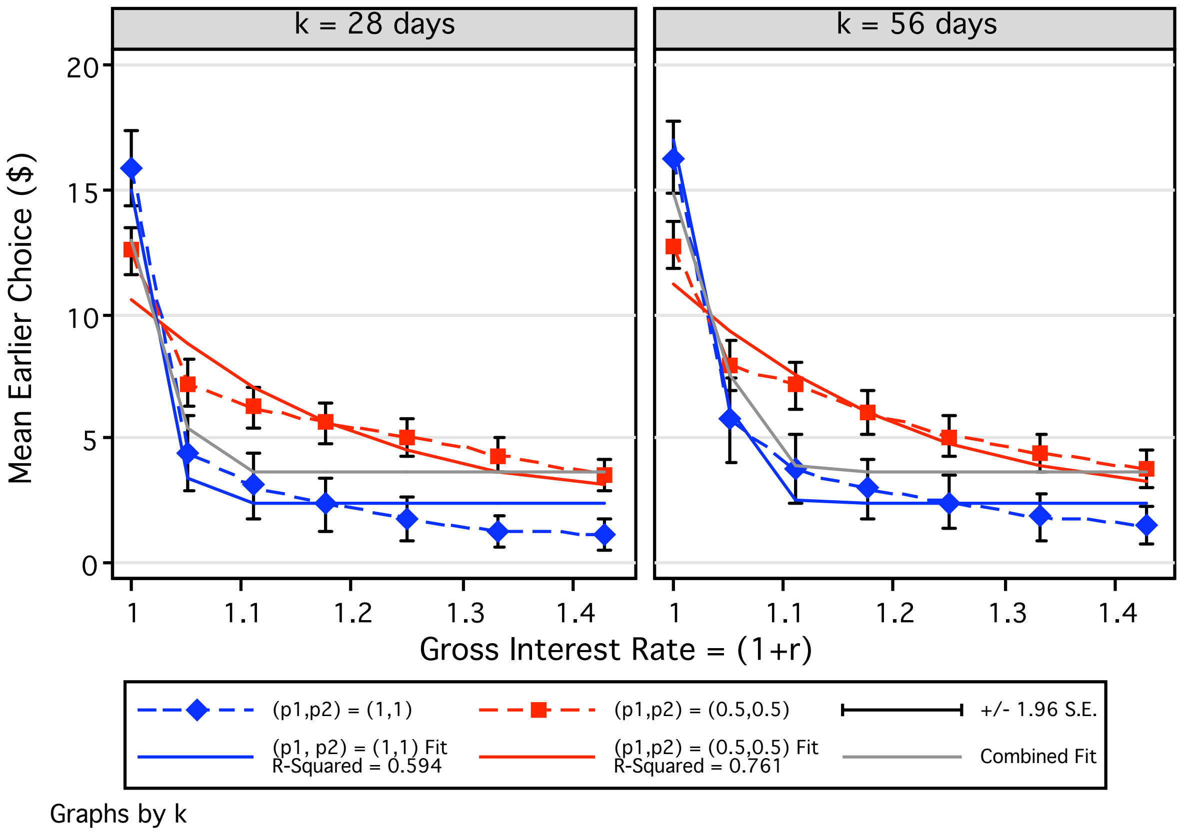
utility to be significantly more concave ($F_{1,79} = 24.09,\ p < 0.01$). In the certain condition, $(p_1, p_2) = (1, 1)$, we estimate a background parameter $\hat{\omega}_{1,1}$ of 2.28 while in the uncertain condition the background parameter is significantly higher at 4.40 ($F_{1,79} = 25.53,\ p < 0.01$). A hypothesis test of equal utility parameter estimates across conditions is rejected ($F_{3,79} = 16.12,\ p < 0.01$).

In Table A2, columns (3) through (6) we estimate utility parameters with various imposed restrictions. In column (3), we restrict curvature to be equal across conditions and obtain very similar discounting estimates, but a larger difference in estimated background parameters. In column (4), we restrict discounting to be equal across conditions and obtain a result almost identical to column (2). In column (5), we restrict background parameters to be equal and obtain very similar discounting estimates, but a larger difference in curvature. This finding is repeated in column (6) where discounting is restricted to be the same. Across specifications, hypothesis tests of equality of utility parameters are rejected.

To illustrate how well these estimates fit the data, Figure A1 displays solid lines with predicted behavior from the most restricted regression, column (6) and the common regression of column (1). The general pattern of aggregate responses is well matched by the column (6) estimates. Figure A1 reports separate R^2 values for the two conditions: $R^2_{1,1} = 0.594$; $R^2_{0.5,0.5} = 0.761$, and the model fits are substantially better than the combined model of column (1). For comparison a simple linear regression of c_t on the levels of interest rates, delay lengths and their interaction in each condition would produce $\tilde{R}^2$ values of $\tilde{R}^2_{1,1} = 0.443$; $\tilde{R}^2_{0.5,0.5} = 0.346$. The least restricted regression, column (2) creates very similar predicted values with R^2 values of 0.595 and 0.766. As the estimates show predicting either condition's responses from the other would lead to substantially worse fit. When using the $(p_1, p_2) = (0.5, 0.5)$ estimates of column (2) as a model for the $(p_1, p_2) = (1, 1)$ data, the R^2 value reduces to 0.466. And, when using the $(p_1, p_2) = (1, 1)$ estimates of column (2) as a model for the $(p_1, p_2) = (0.5, 0.5)$ data, the R^2 value reduces to 0.629.

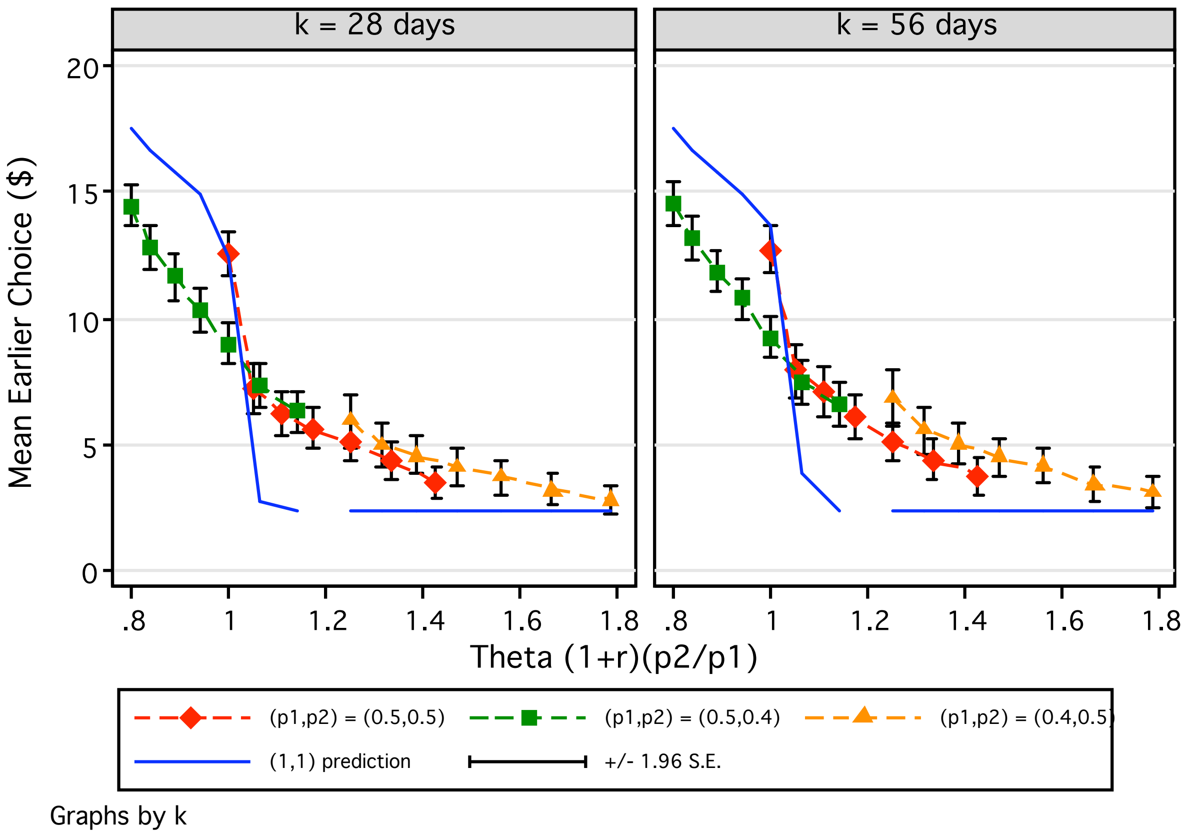
coming) find aggregate discount rate between 25-35 percent and aggregate curvature of around 0.92. These discount rates are lower than generally found in the time preference literature (Frederick et al., 2002). Notable exceptions of similarly low or lower discount rates include Coller and Williams (1999), Harrison et al. (2002), and Harrison et al. (2005) which all assume linear utility, and Andersen et al. (2008), which accounts for utility function curvature with Holt and Laury (2002) risk measures.

Figure A1: Aggregate Behavior Under Certainty and Uncertainty



Note: The figure presents aggregate behavior for $N = 80$ subjects under two conditions: $(p_1, p_2) = (1, 1)$, i.e. no risk, in blue; and $(p_1, p_2) = (0.5, 0.5)$, i.e. 50 percent chance sooner payment would be sent *and* 50 percent chance later payment would be sent, in red. $t = 7$ *days* in all cases, $k \in \{28, 56\}$ *days*. Error bars represent 95 percent confidence intervals, taken as $+/-1.96$ standard errors of the mean. Test of H_0 : Equality across conditions: $F_{14,79} = 6.07,\ p < .001$.

Figure A2: Aggregate Behavior Under Uncertainty with Predictions Based on Certainty



Note: The figure presents aggregate behavior for $N = 80$ subjects under three conditions: 1) $(p_1, p_2) = (0.5, 0.5)$, i.e. equal risk, in red; 2) $(p_1, p_2) = (0.5, 0.4)$, i.e. more risk later, in green; and 3) $(p_1, p_2) = (0.4, 0.5)$, i.e. more risk sooner, in orange. Error bars represent 95 percent confidence intervals, taken as $+/- 1.96$ standard errors of the mean. Blue solid lines correspond to predicted behavior using certain utility estimates from $(p_1, p_2) = (1, 1)$ as estimated in Table A2, column (6).

A.2 Welcome Text

Welcome and thank you for participating.

Eligibility for this study: To be in this study, you need to meet these criteria. You must have a campus mailing address of the form:

YOUR NAME
9450 GILMAN DR 92(MAILBOX NUMBER)
LA JOLLA CA 92092-(MAILBOX NUMBER)

Your mailbox must be a valid way for you to receive mail from now through the end of the Spring Quarter.

You must be willing to provide your name, campus mail box, email address, and student PID. This information will only be seen by Professor Andreoni and his assistants. After payment has been sent, this information will be destroyed. Your identity will not be a part of any subsequent data analysis.

You must be willing to receive your payment for this study by check, written to you by Professor James Andreoni, Director of the UCSD Economics Laboratory. The checks will be drawn on the USE Credit Union on campus. You may deposit or cash your check wherever you like. If you wish, you can cash your checks for free at the USE Credit Union any weekday from 9:00 am to 5:00 pm with valid identification (drivers license, passport, etc.).

The checks will be delivered to you at your campus mailbox at a date to be determined by your decisions in this study, and by chance. The latest you could receive payment is the last week of classes in the Spring Quarter.

If you do not meet all of these criteria, please inform us of this now.

A.3 Instruction and Examples Script

Earning Money:

To begin, you will be given a $10 minimum payment. You will receive this payment in two payments of $5 each. The two $5 minimum payments will come to you at two different times. These times will be determined in the way described below. Whatever you earn from the study today will be added to these minimum payments.

In this study, you will make 84 choices over how to allocate money between two points in time, one time is 'earlier' and one is 'later'. Both the earlier and later times will vary across decisions. This means you could be receiving payments as early as one week from today, and as late as the last week of classes in the Spring Quarter, or possibly other dates in between.

It is important to note that the payments in this study involve chance. There is a chance that your earlier payment, your later payment or both will not be sent at all. For each decision, you will be fully informed of the chance involved for the sooner and later payments. Whether or not your payments will be sent will be determined at the END of the experiment today. If, by chance, one of your payments is not sent, you will receive only the $5 minimum payment.

Once all 84 decisions have been made, we will randomly select one of the 84 decisions as the decision-that-counts. This will be done in three stages. First, we will pick a number from 1 to 84 at random to determine which is the decision-that-counts and the corresponding sooner and later payment dates. Then we will pick a second number at random from 1 to 10 to determine if the sooner payment will be sent. Then we will pick a third number at random from 1 to 10 to determine if the later payment will be sent. We will use the decision-that-counts to determine your actual earnings. Note, since all decisions are equally likely to be chosen, you should make each decision as if it will be the decision-that-counts. When calculating your earnings from the decision-that-counts, we will add to your earnings the two $5 minimum payments.

Thus, you will always get paid at least \$5 at the chosen earlier time, and at least \$5 at the chosen later time.

IMPORTANT: All payments you receive will arrive to your campus mailbox. On the scheduled day of payment, a check will be placed for delivery in campus mail services by Professor Andreoni and his assistants. Campus mail services guarantees delivery of 100% of your payments by the following day.

As a reminder to you, the day before you are scheduled to receive one of your payments, we will send you an e-mail notifying you that the payment is coming. On your table is a business card for Professor Andreoni with his contact information. Please keep this in a safe place. If one of your payments is not received you should immediately contact Professor Andreoni, and we will hand-deliver payment to you.

Your Identity:

In order to receive payment, we will need to collect the following pieces of information from you: name, campus mail box, email address, and student PID. This information will only be seen by Professor Andreoni and his assistants. After all payments have been sent, this information will be destroyed. Your identity will not be a part of subsequent data analysis.

On your desk are two envelopes: one for the sooner payment and one for the later payment. Please take the time now to address them to yourself at your campus mail box.

How it Works:

In each decision you are asked to divide 100 tokens between two payments at two different dates: Payment A (which is sooner) and Payment B (which is later). Tokens will be exchanged for money. The tokens you allocate to Payment B (later) will always be worth at least as much as the tokens you allocate to Payment A (sooner). The process is best described by an example. Please examine the sample sheet in you packet marked SAMPLE.

The sample sheet provided is similar to the type of decision sheet you will fill out in the study. The sample sheet shows the choice to allocate 100 tokens between Payment A on April 17th and Payment B on May 1st. Note that today's date is highlighted in yellow on the calendar on the left hand side. The earlier date (April 17th) is marked in green and the later date (May 1st) is marked in blue. The earlier and later dates will always be marked green and blue in each decision you make. The dates are also indicated in the table on the right.

In this decision, each token you allocate to April 17th is worth $0.10, while each token you allocate to May 1st is worth $0.15. So, if you allocate all 100 tokens to April 17th, you would earn 100x$0.10 = $10 (+ $5 minimum payment) on this date and nothing on May 1st (+ $5 minimum payment). If you allocate all 100 tokens to May 1st, you would earn 100x$0.15 = $15 (+ $5 minimum payment) on this date and nothing on April 17th (+ $5 minimum payment). You may also choose to allocate some tokens to the earlier date and some to the later date. For instance, if you allocate 62 tokens to April 17th and 38 tokens to May 1st, then on April 17th you would earn 62x$0.10 = $6.20 (+ $5 minimum payment) and on May 1st you would earn 38x$0.15 = $5.70 (+ $5 minimum payment). In your packet is a Payoff Table showing some of the token-dollar exchange at all relevant token exchange rates.

REMINDER: Please make sure that the total tokens you allocate between Payment A and Payment B sum to exactly 100 tokens. Feel free to use the calculator provided in making your allocations and making sure your total tokens add to exactly 100 in each row.

Chance of Receiving Payments:

Each decision sheet also lists the chances that each payment is sent. In this example there is a 70% chance that Payment A will actually be sent and a 30% chance that Payment B will actually be sent. In each decision we will inform you of the chance that the payments will be sent. If this decision were chosen as the decision-that-counts we would determine the actual payments by throwing two ten sided die, one for Payment A and one for Payment B.

EXAMPLE: Let's consider the person who chose to allocate 62 tokens to April 17th and 38 tokens to May 1st. If this were the decision-that-counts we would then throw a ten-sided die for Payment A. If the die landed on 1,2,3,4,5,6,or 7, the person's Payment A would be sent and she would receive $6.20 (+ $5 minimum payment) on April 17th. If the die landed 8,9, or 10, the payment would not be sent and she would receive only the $5 minimum payment on April 17th. Then we would throw a second ten-sided die for Payment B. If the die landed 1,2, or 3, the person's Payment B would be sent and she would receive $5.70 (+ $5 minimum payment) on May 1st. If the die landed 4,5,6,7,8,9, or 10, the payment would not be sent and she would receive only the $5 minimum payment on May 1st.

Things to Remember:

- You will always be allocating exactly 100 tokens.
- Tokens you allocate to Payment A (sooner) and Payment B (later) will be exchanged for money at different rates. The tokens you allocate to Payment B will always be worth at least as much as those you allocate to Payment A.
- Payment A and Payment B will have varying degrees of chance. You will be fully informed of the chances.
- On each decision sheet you will be asked 7 questions. For each decision you will allocate 100 tokens. Allocate exactly 100 tokens for each decision row, no more,

no less.

- At the end of the study a random number will be drawn to determine which is the decision-that-counts. Because each question is equally likely, you should treat each decision as if it were the one that determines your payments. Two more random numbers will be drawn by throwing two ten sided die to determine whether or not the payments you chose will actually be sent.

- You will get an e-mail reminder the day before your payment is scheduled to arrive.

- Your payment, by check, will be sent by campus mail to the mailbox number you provide.

- Campus mail guarantees 100% on-time delivery.

- You have received the business card for Professor James Andreoni. Keep this card in a safe place and contact Prof. Andreoni immediately if one of your payments is not received.