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Essays in Time and Risk

A dissertation submitted in partial satisfaction of the
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by

Charles David Sprenger

Committee in charge:

Professor James R. Andreoni, Chair
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2011

The dissertation of Charles David Sprenger is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California, San Diego

2011

DEDICATION

To friends and family and family friends.

EPIGRAPH

Today, it is as widespread as its real meaning is generally misunderstood.

—Maurice Allais

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ABSTRACT OF THE DISSERTATION

Essays in Time and Risk

by

Charles David Sprenger

Doctor of Philosophy in Economics

University of California, San Diego, 2011

Professor James R. Andreoni, Chair

In this dissertation I focus on novel mechanisms for eliciting time and risk preferences and using these methods to test neoclassical and behavioral economic models. In Chapter 1, a new methodology for eliciting time preferences, the *Convex Time Budget*, is introduced. In Chapter 2, the Convex Time Budget is extended to explore the relationship between hyperbolic discounting and payment risk. In Chapter 3, a new measure of risk preferences, the *Uncertainty Equivalent*, is introduced and used to differentiate several models of risk preferences. In Chapter 4, I generate a new test distinguish between competing models of reference dependent preferences in risky choice.

Chapter 1

Estimating Time Preferences From Convex Budgets

Abstract

Experimentally elicited discount rates are frequently higher than what one would infer from market interest rates and seem unreasonable for economic decision-making. Such high rates have often been attributed to present bias and hyperbolic discounting. A commonly recognized bias of standard elicitation techniques is the use of linear preferences for identification. When attempts are made to correct this bias with additional experimental measures, researchers find exceptional degrees of utility function curvature. We present a new methodology for identifying time preferences, both discounting and utility function curvature, from simple allocation decisions. We estimate annual discount rates substantially lower than normally obtained, dynamically consistent discounting, and moderate utility function curvature.

1.1 Introduction

Understanding and estimating time preferences is obviously of great importance to economists, marketers, and policy makers. Consumers decide how much to invest in savings, education, real estate, and life insurance, how much to diet, exercise, and smoke, whether to marry, when to have children, and what to leave in their wills.

While there has been substantial research estimating time preferences using aggregate consumption data,[1] the bulk of the effort has occurred in laboratory environments.[2] Among the many laboratory techniques employed, many recent studies have favored multiple price lists (MPL) with monetary payments.[3]

With MPLs, individuals are asked multiple times to choose between smaller payment amounts closer to the present and larger amounts further into the future. The interest rate increases monotonically in a price list, such that the point where an individual switches from preferring sooner payments to later payments carries interval information about their intertemporal preferences. Assuming time-separable stationary preferences and linear utility, individual discount rates can be bounded and potentially calculated from MPL switching points.[4]

A notable feature of MPLs (and other experimental methods) is that they yield high average discount rates. Estimates of annual discount rates over one hundred percent are common (Frederick et al., 2002). This is curiously at odds with aggregate models of discounting which imply much lower annual discount rates (Gourinchas and Parker, 2002; Cagetti, 2003). A possible explanation for this difference may lie in experimenters' frequent assumption of linear utility, which leads to upwards-biased discount rate estimates if utility is concave.[5] An important step in correcting this bias comes from Andersen et al. (2008) who separately administered MPLs and price list

[1]Examples include Hausman (1979); Gourinchas and Parker (2002); Cagetti (2003); Laibson et al. (2003, 2005).

[2]For a survey of the literature, see Frederick et al. (2002). Recent contributions include Harrison et al. (2002, 2005); Andersen et al. (2008); Benhabib et al. (2007); Tanaka et al. (2010).

[3]The MPL with monetary payments in economics was motivated and popularized by Coller and Williams (1999) and Harrison et al. (2002). In psychology, a similar technique was employed by Kirby et al. (1999) and has been implemented in several economic laboratory experiments, including Chabris et al. (2008a,b).

[4]Price list switch points indicate approximately where sooner and later payments are equally valued. Take a sooner payment, c_t a later payment c_{t+k}, and a utility function $U(c_t, c_{t+k})$. Under time-separable stationary utility, $U(c_t, c_{t+k}) = u(c_t) + \delta^k u(c_{t+k})$ and a switch point indicates where $u(c_t) \approx \delta^k u(c_{t+k})$. Under linear utility, $u(c) = c$ and δ is calculated as $\delta \approx (c_t/c_{t+k})^{1/k}$. Discount rates are then calculated as $IDR = (1/\delta) - 1$.

[5]Under linear utility, $u(c_t) = c_t$ and δ is calculated as $\delta_L \approx (c_t/c_{t+k})^{1/k}$. Rabin (2000a) shows that under expected utility theory, individuals should have approximately linear preferences for small stakes outcomes, such as those normally used in time preference experiments. However, a variety of studies show substantial curvature over small stakes outcomes (e.g., Holt and Laury, 2002). If there is curvature to the utility function, then $\delta_C \approx (u(c_t)/u(c_{t+k}))^{1/k}$. The direction of the bias $\delta_C - \delta_L$ depends on the shape of the utility function. Concavity generates downwards-biased discount factor (upwards-biased discount rate) estimates.

risk preference measures based on Holt and Laury (2002) (HL) to the same subjects. Using both time and risk price lists, they jointly estimated discounting and curvature parameters.[6] For brevity, we refer to this as the *Double Multiple Price List* (DMPL) approach.[7]

In this paper, we use a single, simple instrument to capture both discounting and concavity of utility in the same measure. Notice that the binary choice of an MPL task is akin to intertemporal optimization subject to a discontinuous budget. Though under linear preferences the discontinuity does not influence choice, individuals with concave utility will be constrained. The potentially problematic discontinuity suggests a simple solution: convexify the experimental budgets. Hence, we call our approach the *Convex Time Budget* (CTB) method.

Intertemporal allocations in CTBs are solutions to standard intertemporal constrained optimization problems. Analysis of the allocations is straightforward. Given a set of functional form assumptions about discounting and curvature of the utility function, preference parameters are estimable at either the group or individual level. Unlike preference parameters estimated from MPL data, which are identified as a set of possible values, CTBs allow for point identification of preference parameters. Additionally, structural assumptions such as the dynamic consistency of time preferences can be tested in simple and familiar ways.

In a computerized experiment with 97 subjects, we show that the CTB method can be used to generate precise estimates of discounting and curvature parameters at both the aggregate and individual levels. These estimates require a minimal set of structural assumptions and are easily implemented econometrically. On average, estimates of individual discount rates are found to be considerably lower than in previous studies. Across specifications, we estimate average annual discount rates between 25 and 35 percent. We reject linearity of utility, although we find far less

[6]Frederick et al. (2002) propose a similar strategy of separately identifying the utility function and discounting along with two other approaches for distinguishing time preferences from curvature: 1) eliciting utility judgements such as attractiveness ratings at two points in time; and 2) eliciting preferences over temporally separated probabilistic prospects to exploit the linearity-in-probability property of expected utility. The second approach is employed by Anderhub et al. (2001).

[7]Tanaka et al. (2010) employ a similar approach with a risk price list task designed to elicit loss aversion. However, they do not use the risk price list to inform curvature of the utility function in estimation of time preference parameters.

curvature than prior studies using price lists for risk preferences. Indeed, almost 35 percent of subjects exhibit behavior that is fully consistent with linear preferences. Finally, to our surprise, we find no evidence of present-bias or hyperbolic discounting.

We also compare within-subjects results of the computerized CTB and those obtained using a standard paper-and-pencil DMPL. Our design allows us to make individual level comparisons. Interestingly, though individual discounting correlates highly across elicitation mechanisms, estimated curvature from CTBs is found to be independent of DMPL risk experimental responses.

Our results raise several important questions for future research. First, why did we find no evidence of present bias or hyperbolic discounting? One hypothesis is that this may be the result of measures we took to equate transaction costs of sooner and later payments and to increase confidence of receiving future payments. This interpretation suggests that some of the behavior attributed to present bias in the literature may actually be an artifact of differential risk or transactions costs over sooner and later payments. A second, more fundamental, question is whether we should have expected to find present bias? Though present bias has been demonstrated many times in experiments using money, the underlying psychological models of temptation and self-control (Laibson, 1997; O'Donoghue and Rabin, 1999; Gul and Pesendorfer, 2001) make clear that present bias is about consumption utility rather than money. Indeed, if subjects have access to even modest amounts of liquidity, researchers should be surprised to measure any present bias in experiments with monetary rewards.[8] Third, we find substantial within-subject differences between our CTB and DMPL measures of utility function curvature. This may suggest a real difference in the utility parameters that apply in uncertain and certain environments. Utility differences across certainty and uncertainty arise in some form in many static and intertemporal models of decision making (Selden, 1978; Kreps and Porteus, 1978; Epstein and Zin, 1989; Schoemaker, 1982; Neilson, 1992; Schmidt, 1998; Diecidue et al., 2004) and were originally suggested by Allais (1953b).

The paper proceeds as follows: Section 2 explains the motivation of the CTB and design for the CTB experiment. Section 3 outlines several econometric specifications while Section 4 presents group and individual analysis. Section 5 concludes.

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[8]We thank Matthew Rabin for persistently and amicably reminding us of this point.

1.2 Experimental Design: Convex Time Budgets

In each decision of an MPL, subjects choose either an amount c_t, available at time t, or an amount $c_{t+k} > c_t$, available after a delay of $k > 0$ periods. Let $(1+r)$ be the experimental gross interest rate and m be the experimental budget.[9] Assuming some utility function, $U(c_t, c_{t+k})$, the MPL task asks subjects to maximize utility subject to the discrete budget set:

$$((1+r)c_t, c_{t+k}) \in \{(m, 0), (0, m)\}. \tag{1.1}$$

Assuming linear utility, the corner solution constraints of (1.1) are non-binding. However, if utility is concave, the constraints bind. One cannot infer discounting from MPL switch points.

Imagine, instead of (1.1), subjects choose c_t and c_{t+k} continuously along a convex budget set:

$$(1+r)c_t + c_{t+k} = m. \tag{1.2}$$

This is a standard future-value budget constraint. To operationalize (1.2) we provide subjects with a budget of experimental 'tokens.' Tokens can be allocated to either a sooner time t, or a later time $t+k$, at different 'token exchange rates.' The relative rate at which tokens translate into payments determines the gross interest rate, $(1+r)$. Subjects choose how many tokens to allocate to sooner and later periods. This is our Convex Time Budget (CTB) approach.

Substantial information can be obtained from allocations in this convex choice environment. Variations in delay lengths, k, and interest rates, $(1+r)$, allow for the identification of time discounting and utility function curvature. Variations to starting times, t, allow for the identification of present bias and hyperbolic discounting.

[9] Theoretically, extra-experimental interest rates and liquidity constraints should influence laboratory decisions (Coller and Williams, 1999). If subjects can borrow (save) at rates inferior (superior) to the rates offered in the lab, then they have an arbitrage opportunity. If subjects are credit constrained, they may choose sooner experimental payments to smooth consumption. In a controlled experiment with MPLs, Coller and Williams (1999) show that providing external interest rate information and elaborating possible arbitrage strategies makes treated subjects appear only slightly more patient. Meier and Sprenger (2010) show that objectively measured credit constraints taken from individual credit reports are generally uncorrelated with MPL responses. For further discussion on arbitrage opportunities and liquidity constraints see Appendix Section 4.5.

1.2.1 CTB Design Features

Our experiment was conducted at the University of California, San Diego in January of 2009. Subjects 45 convex budget decisions. These 45 budgets involve 9 combinations of starting times, t, and delay lengths, k, with annual interest rates that vary from zero to over 1000% per year.

A (3×3) design was implemented with three sooner payment dates, $t = (0, 7, 35)$ days from the experiment date, crossed with three delay lengths, $(k = 35, 70, 98)$ days.[10] Thus there are nine (t, k) cells and within each cell are five CTB questions, generating 45 choices for each subject. We refer to each (t, k) combination as a 'choice set'. The t and k combinations used in our study were selected to avoid holidays (including Valentine's Day), school vacations, spring break, and final examination weeks. Payments were scheduled to arrive on the same day of the week (t and k are both multiples of 7), to avoid differential week-day effects.

In each CTB question, subjects were given a budget of 100 tokens. Tokens allocated to sooner payments had a value of a_t while tokens allocated to later payments had a value of a_{t+k}. In most cases, a_{t+k} was \$0.20 per token and a_t varied from \$0.20 to \$0.10 per token.[11] Note that $a_{t+k}/a_t = 1 + r$, the gross interest rate over k days, so $(1 + r)^{1/k}$ gives the standardized daily interest rate. Daily net interest rates in the experiment varied considerably across the 45 budgets, from 0 to around 1 percent per day implying annual interest rates of between 0 and 1300 percent (compounded quarterly).

Each choice set featured $a_{t+k} = \$0.20$ and $a_t = \$0.16$ $(1 + r = 1.25)$. In eight of the nine choice sets, one convex budget represented a pure income shift relative to this choice. This was implemented with $a_{t+k} = \$0.25$ and $a_t = \$0.20$ $(1 + r = 1.25$ again). In the remaining choice set, $(t, k) = (7, 70)$, we instead implemented $a_t = \$.20$ and $a_{t+k} = \$.20$, a zero percent interest rate. Table 2.1 shows the token rates, interest rates, standardized daily interest rates and corresponding annual interest rates for all 45 budgets.

[10] See below for the recruitment and payment efforts that allowed sooner payments, including those for $t = 0$, to be implemented in the same manner as later payments.

[11] In eight of 45 choices, a_{t+k} was \$0.25. If an individual allocated all her tokens in every choice to the later payment, she could expect to earn either \$20 or \$25. If she allocated all her tokens to the sooner payment in every choice, she would earn at least \$10.

Table 1.1: Choice Sets

t (start date)	k (delay)	Token Budget	a_t	a_{t+k}	$(1+r)$	Daily Rate (%)	Annual Rate (%)
0	35	100	0.19	0.2	1.05	0.147	65.3
0	35	100	0.18	0.2	1.11	0.301	164.4
0	35	100	0.16	0.2	1.25	0.64	528.9
0	35	100	0.14	0.2	1.43	1.024	1300.9
0	35	100	0.2	0.25	1.25	0.64	528.9
0	70	100	0.19	0.2	1.05	0.073	29.6
0	70	100	0.18	0.2	1.11	0.151	67.4
0	70	100	0.16	0.2	1.25	0.319	178.1
0	70	100	0.14	0.2	1.43	0.511	362.1
0	70	100	0.2	0.25	1.25	0.319	178.1
0	98	100	0.19	0.2	1.05	0.052	20.5
0	98	100	0.16	0.2	1.25	0.228	113
0	98	100	0.13	0.2	1.54	0.441	286.4
0	98	100	0.1	0.2	2	0.71	637.1
0	98	100	0.2	0.25	1.25	0.228	113
7	35	100	0.19	0.2	1.05	0.147	65.3
7	35	100	0.18	0.2	1.11	0.301	164.4
7	35	100	0.16	0.2	1.25	0.64	528.9
7	35	100	0.14	0.2	1.43	1.024	1300.9
7	35	100	0.2	0.25	1.25	0.64	528.9
7	70	100	0.2	0.2	1	0	0
7	70	100	0.19	0.2	1.05	0.073	29.6
7	70	100	0.18	0.2	1.11	0.151	67.4
7	70	100	0.16	0.2	1.25	0.319	178.1
7	70	100	0.14	0.2	1.43	0.511	362.1
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35	35	100	0.14	0.2	1.43	1.024	1300.9
35	35	100	0.2	0.25	1.25	0.64	528.9
35	70	100	0.19	0.2	1.05	0.073	29.6
35	70	100	0.18	0.2	1.11	0.151	67.4
35	70	100	0.16	0.2	1.25	0.319	178.1
35	70	100	0.14	0.2	1.43	0.511	362.1
35	70	100	0.2	0.25	1.25	0.319	178.1
35	98	100	0.19	0.2	1.05	0.052	20.5
35	98	100	0.16	0.2	1.25	0.228	113
35	98	100	0.13	0.2	1.54	0.441	286.4
35	98	100	0.1	0.2	2	0.71	637.1
35	98	100	0.2	0.25	1.25	0.228	113

1.2.2 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 confidence, must be equalized across all time periods. We took several unique steps in our subject recruitment process and payment procedures in order to closely equate transaction costs over time, which we discuss in the following subsections.

Recruitment

In order to participate in the experiment, subjects were required to live on campus. All campus residents are provided with an individual mailbox at their dormitory. Students frequently use these mailboxes as all postal service mail and intra-campus mail are received at this mailbox. Each mailbox is locked and individuals have keyed access 24 hours per day.

By special arrangement with the university mail services office, we were granted same-day access to a specific subset of campus mailboxes. These mailboxes were located at staffed dormitory mail centers and so experimental payments could be immediately placed in a subject's locked mailbox. As such, subjects in our experiment were required to have one of the fixed number of campus mailboxes to which we had immediate access. We recruited 97 undergraduate freshman and sophomores meeting these criteria.

Experimental Payments

We employed six measures intended to equalize the costs of receiving payments. These measures not only attempt to equate transactions costs over sooner and later payments, but also to increase confidence that future payments will arrive. First, all sooner and later payments, including those for $t = 0$, were placed in subjects' campus mailboxes. Subjects were fully informed of the payment method and the special arrangement made with university mail services.[12] Eliminating in-lab payments ensures that subjects don't disproportionately prefer present in-lab payments

[12] See Appendix Section 1.6.5 for the information provided to subjects.

because they are more likely to be received than future extra-lab payments.

Second, upon beginning the experiment, subjects were told that they would receive a $10 thank-you payment for participating. This $10 was to be received in two payments: $5 sooner and $5 later, regardless of choices, and all experimental earnings were added to these two $5 thank-you payments. This eliminated any convenience gained by concentrating payments in one period – two checks were sent regardless.

Third, two blank envelopes were provided to each subject. After receiving directions about the two thank-you payments, subjects were asked to address the envelopes to themselves at their campus mailbox, thus minimizing clerical errors on our part.

Fourth, at the end of the experiment, subjects were asked to write their payment amounts and dates on the inside flap of both envelopes, so they would see and verify the amounts written in their own handwriting when payments arrived, thus eliminating the cost of remembering the future amounts owed to them.

Fifth, one choice for each subject was selected for payment by drawing a numbered card at random. All experimental payments were made by personal check from Professor James Andreoni drawn on an account at the campus credit union.[13] Individuals were informed that they could cash their checks (if they so desired) at this credit union, thus increasing the fidelity of the payment method.

Sixth, subjects were 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. This invitation to inconvenience a professor was intended to boost confidence that future payments would arrive as promised.

We believe that these efforts helped both equate transactions costs across payments, and engender experimenter trust. In an auxiliary survey, subjects were asked if they trusted that they would receive their experimental payments, and 97% of respondents replied yes.

[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 check payments were found to have the highest cash-equivalent value.

Protocol

A Java^{TM}-based client/server system was written to implement the CTB experiment. The server program sent budget information, recorded subject choices, and reported experiment earnings. The client program provided instructions to subjects, elicited choices, and administered a post-experiment questionnaire.

Upon starting the experiment, subjects read through directions and CTB examples. The CTB examples indicated to subjects that tokens could be allocated entirely to the sooner payment, entirely to the later payment or divided between the two. The objective was not to lead subjects to interior or corner allocations with suggestive language.[14] Screen shots of the instructions are presented in Appendix 1.6.5, which were read aloud and projected on a screen.

Subjects' decision screens displayed a dynamic calendar and a series of nine

[14]Though, we cannot be sure if the language led subjects towards or away from specific allocations, subjects were not shy about either type of allocation. Roughly 70% of responses are at corners, but only 36 of 97 subjects made zero interior allocations. See Section 4.4 for further detail.

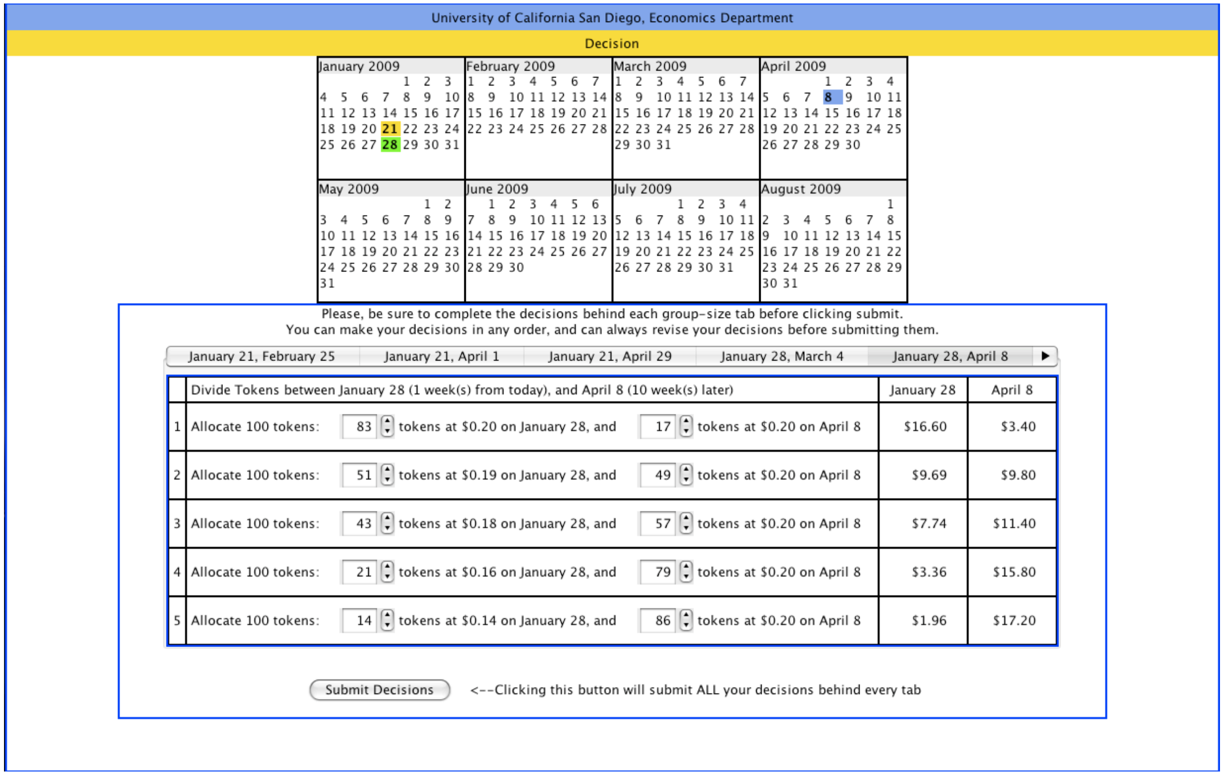

Figure 1.1: Sample Decision Screen

"decision tabs." These decision tabs corresponded to the nine choice sets described above, one tab for each (t, k) combination. Subjects could respond to the decision tabs in any order they wished. Each decision tab had five budget decisions presented in order of increasing interest rate and then in order of increasing budget.[15] An image of the decision screen is presented in Figure 1.2.2.

For each decision, individuals were told how many tokens they were to allocate (always 100), the sooner token value a_t, and the later token value a_{t+k}.[16] As each budget decision was being made, the calendar in the subjects' screen highlighted the experiment date (in yellow), the sooner date t (in green), and the later date $t + k$ (in blue). This allowed subjects to visualize the delay length for a given decision.[17]

Background Consumption and DMPL

In addition to the CTB experiment, we implemented a series of three MPLs and two HL risk price list tasks (the components of the DMPL). The MPLs featured the (t, k) combinations: $(t = 0, k = 35)$, $(t = 0, k = 98)$, $(t = 35, k = 35)$. The MPLs can be used to create alternate measures of both discounting and present bias for comparison. The HL risk price lists were designed to elicit risk aversion or utility function curvature over \$20 and \$25, respectively.[18]

At the end of the computer-based CTB experiment, subjects were administered a questionnaire. Importantly, subjects were asked how much they spend in a typical week. The average response was \$49.32 per week or \$7.05 per day of "background consumption." This figure is used later in our analysis (see Section 1.4.1).

[15]For a disussion of order effects and presenting choices by increasing interest rate, see Harrison et al. (2005).

[16]Individuals were not told the gross interest rate, $(1+r)$. However, in a companion questionnaire individuals were asked several numeracy questions, including one on compound interest. Roughly 70% or respondents were able to correctly answer a standard compound interest question. The level of numeracy in the sample suggests that the majority would be able to calculate at least the interest rate over the delay, k.

[17]Because t and k were multiples of 7, all dates were described by the number of weeks (e.g., $t = 7, k = 35$ was described as "1 week from today" and "5 weeks later"). Note, also, that allocation amounts were initially blank on the decision screen and subjects used up and down arrows to make choices.

[18]The MPLs and HLs could also be chosen at random for payment. For directions and the price list tasks see Appendix Section 1.6.6.

1.3 Parameter Estimation with the CTB

Given assumptions on the functional form of utility and the nature of discounting, the CTB provides a natural context in which to jointly estimate (and test hypotheses of) time preferences, present bias, and curvature of the utility function. To begin, we posit a time separable CRRA utility function discounted via the quasi-hyperbolic β-δ discounting function (Strotz, 1956; Phelps and Pollak, 1968; Laibson, 1997),

$$U(c_t, c_{t+k}) = (c_t - \omega_1)^{\alpha} + \beta\delta^k(c_{t+k} - \omega_2)^{\alpha}, \tag{1.3}$$

where δ is the one period discount factor and β is the present bias parameter. The quasi-hyperbolic form elegantly captures the notion of present-biased time preferences and nests the exponential discounting when $\beta = 1$. A value $\beta < 1$ indicates present bias and when $t > 0$ present bias does not influence choice. The values c_t and c_{t+k} are experimental earnings and α is the CRRA curvature parameter.[19] The CRRA utility function is frequently estimated in experimental studies on both time and risk preferences and also used as the benchmark utility formulation across many fields of economics. The terms ω_1 and ω_2 are additional utility parameters which could be interpreted as classic Stone-Geary consumption minima, intertemporal reference points, or background consumption. For example, such utility parameters are used in Andersen et al. (2008), where experimental earnings are added to background consumption, B, such that $\omega_1 = \omega_2 = -B$. The parameter, B, is not estimated in their specification, but set to 118 Danish Kroner, the average value of daily consumption in Denmark in 2003, around \$25 US in 2009. Appendix Table 1.5 provides comparisons using various given values of ω_1 and ω_2.

Maximizing (2.2) subject to the future value budget (1.2) yields the tangency condition

$$\frac{c_t - \omega_1}{c_{t+k} - \omega_2} = \left\{ \begin{array}{lll} (\beta\delta^k(1+r))^{(\frac{1}{\alpha-1})} & if & t = 0 \\ (\delta^k(1+r))^{(\frac{1}{\alpha-1})} & if & t > 0 \end{array} \right\}, \tag{1.4}$$

[19] This power utility formulation for CRRA is often used in experimental contexts and differs slightly from CRRA utility formulated as $c^{1-\rho}/1-\rho$, with ρ being the coefficient of relative risk aversion. In our utility formulation the coefficient of relative risk aversion is $1 - \alpha$.

and the intertemporal formulation of a Stone-Geary linear demand for c_t,

$$c_t = \left\{ \begin{array}{ll} \frac{1}{1+(1+r)(\beta\delta^k(1+r))^{(\frac{1}{\alpha-1})}}]\omega_1 + [\frac{(\beta\delta^k(1+r))^{(\frac{1}{\alpha-1})}}{1+(1+r)(\beta\delta^k(1+r))^{(\frac{1}{\alpha-1})}}](m-\omega_2) & if \quad t=0 \\ \frac{1}{1+(1+r)(\delta^k(1+r))^{(\frac{1}{\alpha-1})}}]\omega_1 + [\frac{(\delta^k(1+r))^{(\frac{1}{\alpha-1})}}{1+(1+r)(\delta^k(1+r))^{(\frac{1}{\alpha-1})}}](m-\omega_2) & if \quad t>0 \end{array} \right\}. \quad (1.5)$$

1.3.1 Estimation of Intertemporal Preferences

Notice the parameters (β, δ, α) and the data (r, k, t) enter into the tangency condition of (2.2) and the demand function of (2.1) in a non-linear fashion. Naturally, if $\alpha = 1$, only corner solutions are obtained. We discuss estimation of the parameters β, δ, α, ω_1 and ω_2 when $\alpha < 1$, and recognize that corner solutions may indeed arise in the data.[20] We motivate two regression techniques, each with their benefits and weaknesses.

The first technique estimates (2.1) and the parameters β, δ, α, ω_1 and ω_2 using non-linear least squares. Appendix Section 1.6.1 provides the details of the estimator. The strength of this methodology is that it estimates the Stone-Geary parameters ω_1 and ω_2. Its weakness is that it cannot account for the censored data issues inherent to potential corner solutions without additional distributional assumptions.[21]

For the second technique, we consider the tangency condition of (2.2). If we assume ω_1 and ω_2 are (non-estimated) known values, we can take logs to obtain

$$ln(\frac{c_t - \omega_1}{c_{t+k} - \omega_2}) = \left\{ \begin{array}{ll} (\frac{ln\ \beta}{\alpha-1}) + (\frac{ln\ \delta}{\alpha-1}) \cdot k + (\frac{1}{\alpha-1}) \cdot ln(1+r) & if \quad t=0 \\ (\frac{ln\ \delta}{\alpha-1}) \cdot k + (\frac{1}{\alpha-1}) \cdot ln(1+r) & if \quad t>0 \end{array} \right\},$$

[20]With the employed utility formulation and $\alpha < 1$, corner solutions can be predicted provided ω_1 and $\omega_2 < 0$. As discussed in Section 4.4, corner solutions are frequent. Appendix Tables 1.9 and 1.10 provide individual estimates and demonstrate that for the motivated regression techniques, individuals with only corner solutions have estimated values of $\alpha = 0.999$, while individuals with more interior solutions are estimated to have more utility function curvature. This gives support to the employed regression techniques for identifying utility function curvature and near linear preferences. Indeed, estimated curvature is found to correlate strongly with the discussed bias in MPL-based discounting estimates. See Sub-section 1.4.2 for details.

[21]However, with such an assumption we could reduce the sum of squared residuals to the solution function (2.1) recognizing that c_t will be censored in the interval $[0, m/(1+r)]$. Details of an NLS estimator of (2.1) adapted for censoring are provided in Appendix Section 1.6.1 and discussed in Section 4.4. We thank an anonymous referee for this very helpful suggestion.

which is linear in the in the data, k and $ln(1+r)$, and reduces to,

$$ln(\frac{c_t - \omega_1}{c_{t+k} - \omega_2}) = (\frac{ln\ \beta}{\alpha - 1}) \cdot \mathbf{1}_{t=0} + (\frac{ln\ \delta}{\alpha - 1}) \cdot k + (\frac{1}{\alpha - 1}) \cdot ln(1+r),$$

where $\mathbf{1}_{t=0}$ is an indicator for the time period $t = 0$. Given an additive error structure, such a linear equation is easily estimated, with parameter estimates for δ, β, and α obtained via nonlinear combinations of coefficient estimates. The weakness of estimation based on the tangency condition of (2.2) is that it requires first that the background parameters ω_1 and ω_2 be known, and second that the consumption ratio $(c_t - \omega_1 / c_{t+k} - \omega_2)$ be strictly positive, such that the log transform is well-defined. The strength, however, is that censoring issues are easily addressed. Two-limit tobit maximum likelihood regressions can be implemented to account for corner solutions (Wooldridge, 2002). Appendix 1.6.1 provides details.

Of additional interest in the present analysis is robustness to alternate functional forms for utility.[22] A leading alternative utility formulation, constant absolute risk aversion (CARA) utility is also easily estimable in the CTB environment. Indeed, because of the exponential form background parameters drop out of the marginal condition if $\omega_1 = \omega_2$. The marginal condition can be written

$$exp(-\rho(c_t - c_{t+k})) = \left\{ \begin{array}{lll} \beta\delta^k \cdot (1+r) & if & t = 0 \\ \delta^k \cdot (1+r) & if & t > 0 \end{array} \right\},$$

where ρ represents the coefficient of absolute risk aversion in the utility formulation $u(c_t) = -exp(-\rho c_t)$. Taking logs and rearranging, this is linear in the data $\mathbf{1}_{t=0}$, k, and $ln(1+r)$, reducing to

$$c_t - c_{t+k} = (\frac{ln\ \beta}{-\rho}) \cdot \mathbf{1}_{t=0} + (\frac{ln\ \delta}{-\rho}) \cdot k + (\frac{1}{-\rho}) \cdot ln(1+r). \tag{1.6}$$

Both this tangency condition and the solution function,

$$c_t = (\frac{ln\ \beta}{-\rho}) \cdot \frac{\mathbf{1}_{t=0}}{2+r} + (\frac{ln\ \delta}{-\rho}) \cdot \frac{k}{2+r} + (\frac{1}{-\rho}) \cdot \frac{ln(1+r)}{2+r} + \frac{m}{2+r}, \tag{1.7}$$

[22] We thank an anonymous referee for helpful suggestions related to estimating this alternate functional form.

can be easily estimated via similar Two-limit tobit maximum likelihood regression techniques. Appendix 1.6.1 provides further detail. A CARA specification eliminates the need to estimate additional utility parameters and is easily handled with standard estimation techniques, but does not readily allow for comparison with prior CRRA estimates and different background assumptions. Given that each estimation strategy has its relative strengths, we provide all estimates and discuss any differences in our analysis.

1.4 Experimental Results

The results are presented in two sub-sections. First, we present aggregate CTB data and provide estimates of aggregate discounting, present bias and curvature. Second, we explore individual level results, estimating preference parameters and comparing the results within-subject to parameters obtained from DMPL methodology.

1.4.1 Aggregate Analysis

We identify experimental allocations as solutions to standard intertemporal optimization problems. These solutions are functions of our parameters of interest (discounting and curvature), and experimentally varied parameters (interest rates and delay lengths). Our experimental results should mirror this functional relationship. In Figure 4.2 we plot the mean number of tokens chosen earlier against the gross interest rate, $(1 + r)$, of each CTB decision. We plot separate points

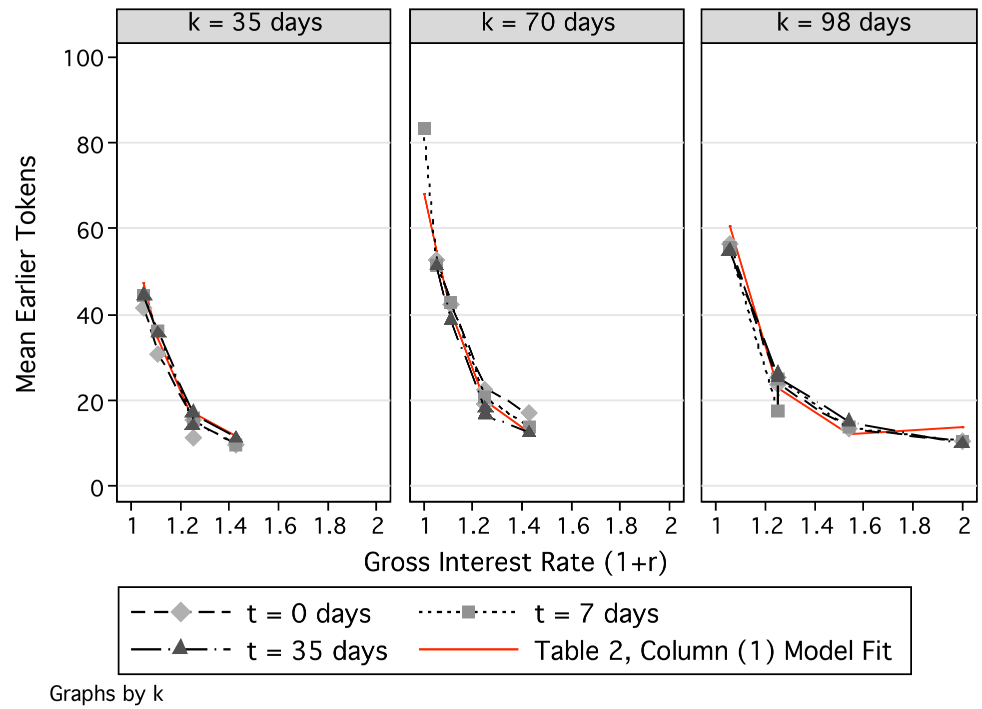

Figure 1.2: Mean Experimental Responses Over Time

for the three experimental values of t ($t = 0$, 7, 35 days), and separate graphs for the three experimental values of k ($k = 35$, 70, 98 days). At each delay length, the number of tokens allocated to the earlier payment declines monotonically with the interest rate; and at comparable gross interest rates, the number of tokens allocated earlier increases with delay.

Evidence for present bias or hyperbolic discounting would be observed in Figure 4.2 as the mean level of tokens allocated earlier being substantially higher when $t = 0$ compared to $t = 7$ or 35. Instead, we observe that the mean number of earlier tokens at each interest rate is roughly constant across t.

Notice that Figure 4.2 also reveals that choices respond to both changing interest rates and delay lengths in predicted way.[23] Masked by these aggregate results, however, is important individual heterogeneity. Roughly 37 percent of subjects (36 of 97) have no interior choices in 45 convex budgets, consistent with linear preferences.[24] Additionally, for the remaining 61 subjects, in any given decision, an average of approximately 50% of responses are found at corners. In the following section we discuss estimation of aggregate preferences following the estimation procedures discussed in Section 1.3.1 that can and cannot account for such corner solutions. In Section 1.4.2, we discuss heterogeneity and provide individual estimates.

[23]Additionally, there is support for a homothetic utility function as the mean number of earlier tokens does not change appreciably with increased income. This aggregate result masks individual-level heterogeneity. Some subjects violate strict income monotonicity, by decreasing either c_t or c_{t+k} in response to an income increase. In eight experimental budget expansions, 72 of 97 subjects make two or fewer such monotonicity violations for c_t and 89 of 97 subjects make two or fewer violations for c_{t+k}. Such violations may be a consequence of natural subject error as on average individuals would have to adjust their responses by only 1.67 later tokens (valued at $0.42) to be consistent with income monotonicity. Other potential errors are also small. For instance, the data generally satisfy the law of demand. For ($t = 7,\ k = 70$) only one subject had a strictly upward sloping demand curve, and 8 of 97 subjects had some increase in demand of c_t in response to increased interest rate. This can be compared to an extreme form of non-monotonic demand, multiple-switching in standard MPL experiments. Around 10 percent of subjects feature multiple switch points in price list experiments (Holt and Laury, 2002; Meier and Sprenger, 2010) and as many as 50 percent in some cases (Jacobson and Petrie, 2009). As well, there is support for positive discounting. For example, between the 1st, 6th and 11th budgets in Table 1, $(t = 0,\ k = 35,\ 70,\ 98), (1 + r) = 1.05$, only one subject strictly decreased her allocation to the earlier payment in response to the delay increase.

[24]See Appendix Tables 1.9 and 1.10 for individual censoring details and estimates.

Estimating Aggregate Preferences

Table 1.2 presents estimates of aggregate preference parameters. In column (1), the annual discount rate, present bias parameter, CRRA utility function curvature and $\hat{\omega}_1$ and $\hat{\omega}_2$ are estimated by non-linear least squares on solution function (2.1) with clustered standard errors.

Column (1) indicates, first, the aggregate annual discount rate is estimated at 0.300 (s.e. 0.064). This discount rate is lower than those estimated by most other researchers.[25]

Second, aggregate curvature is precisely estimated at $\hat{\alpha} = 0.920$ (s.e. = 0.006), significantly different from 1 ($F_{1,96} = 155.18,\ p < .01$), but far closer to linear utility than estimated from the DMPL approach employing HL risk measures or other experimental estimates of risk aversion. For comparison, using DMPL methodology with Danish subjects, Andersen et al. (2008) find a CRRA curvature parameter of 0.259. When allowing for this curvature and setting both ω_1 and ω_2 equal to minus average daily spending in Denmark, Andersen et al. (2008) find a discount rate of 0.101. When assuming linear utility, they obtain a discount rate of 0.251.

The third, and most prominent finding is that, echoing Figure 4.2, we find no evidence of present bias. That is, $\hat{\beta}$ is estimated to be 1.004 (s.e. = .002). The hypothesis of no present bias, $\beta = 1$, is marginally rejected ($F_{1,96} = 2.82,\ p < .10$), with the favored alternative being future bias, $\beta > 1$. Obtaining a precisely estimated $\hat{\beta}$ so close to 1 is of specific interest. The general finding in both monetary and non-monetary experiments and aggregate analyses is of substantial present bias (Frederick et al., 2002), with a suggested value for β of around 0.7 (Laibson et al., 2003). Figure 4.2 also provides model fits corresponding to Table 1.2, column (1) $t = 35$ days, demonstrating that the estimated time consistent preferences closely fit the aggregate data. However, the R^2 value indicates that substantial variation remains unexplained, potentially related to individual heterogeneity. Individual analyses are presented in Section 1.4.2

[25] Similar results are obtained when adapting the NLS criterion function for censoring. See Appendix Table 1.4. Notable exceptions of similarly low discount rates include Coller and Williams (1999); Harrison et al. (2002, 2005) which all assume linear preferences and Andersen et al. (2008), employing the DMPL technique.

Table 1.2: Discounting and Curvature Parameter Estimates

Method:	(1) NLS	(2) NLS	(3) NLS	(4) Tobit	(5) NLS	(6) Tobit	(7) Tobit	(8) Tobit
Annual Discount Rate	0.300	0.377	0.371	0.324	0.246	0.275	0.254	0.335
	(0.064)	(0.087)	(0.091)	(0.173)	(0.128)	(0.162)	(0.159)	(0.136)
Present Bias: $\hat{\beta}$	1.004	1.006	1.007	1.023	1.026	1.026	1.028	1.017
	(0.002)	(0.006)	(0.006)	(0.010)	(0.008)	(0.010)	(0.010)	(0.008)
CRRA Curvature: $\hat{\alpha}$	0.920	0.921	0.897	0.977	0.706	0.873		
	(0.006)	(0.006)	(0.009)	(0.004)	(0.017)	(0.018)		
CARA Curvature: $\hat{\rho}$							0.008	0.007
							(0.001)	(0.001)
$\hat{\omega}_1$	1.368							
	(0.275)							
$\hat{\omega}_2$	-0.085							
	(1.581)							
$\hat{\omega}_1 = \hat{\omega}_2$		1.350	0	-0.01	-7.046	-7.046	-	-
		(0.278)	-	-	-	-	-	-
R^2 / LL	0.4911	0.4908	0.4871	-7642.74	0.4499	-5277.56	-8864.52	-7772.91
# Observations	4365	4365	4365	4365	4365	4365	4365	4365
# Uncensored	-	-	-	1329	-	1329	1329	1329
# Clusters	97	97	97	97	97	97	97	97

Notes: NLS and two-limit tobit ML estimators. Column (1): Unrestricted CRRA regression of equation (5). Column (2): CRRA regression of equation (5) with restriction $\omega_1 = \omega_2$. Columns (3) and (4): CRRA regressions of equations (5) and (4), respectively with restriction $\omega_1 = \omega_2 = 0$. Columns (5) and (6): CRRA regressions of equations (5) and (4), respectively with restriction $\omega_1 = \omega_2 = -7.046$ (the negative of average reported daily spending). Columns (7) and (8): CARA regressions of equations (6) and (7), respectively. Clustered standard errors in parentheses. Annual discount rate calculated as $(1/\hat{\delta})^{365} - 1$. Standard errors calculated via the delta method.

The finding of no aggregate present bias is at striking odds with a body of experimental results in both economics and psychology. Reconciling our findings with others is an important issue. A potential explanation is associated with our experimental methodology. First, experimental evidence suggests that present bias may be conflated with subjects' assessment of the risk of receiving payments (Halevy, 2008).[26] Keren and Roelofsma (1995) and Weber and Chapman (2005) find in two of three experiments that when applying increasing levels of risk to both present and future payments, present bias decreases to some degree. Our experimental methodology is designed to eliminate differential risk between sooner and later payments. Indeed, in

[26]Indeed, this is the motivating argument for experimental front-end delays. See, for example, Harrison et al. (2002, 2005).

Andreoni and Sprenger (2010a) we show that when differential payment risk is exogenously added back into the decision environment, a hyperbolic pattern of discounting appears.

Though eliminating differential payment reliability represents one possible explanation for our findings, many others exist. Principal among these explanations is that present bias is a visceral response only activated when sooner rewards are actually immediate. For example, dynamic inconsistency is shown to manifest itself in immediate choices over healthy and unhealthy snacks (Read and van Leeuwen, 1998), juice drinks (McClure et al., 2007) and more immediate monetary rewards (McClure et al., 2004).[27] In order to equate transaction costs over sooner and later payments we were unable to provide truly immediate rewards. Viewed in this light, our findings represent a potential bound on present bias. With delays of a few hours in between decision and reward receipt, present bias may be effectively eliminated. A second explanation is that monetary payments should perhaps not elicit present bias to the same extent as more tempting primary goods. Though the body of experimental evidence on present bias has used monetary payments, and high correlations are obtained across primary and monetary intertemporal rewards (Reuben et al., 2008), the underlying psychological models are very clearly focused on the temptation of consumption utility and not on monetary rewards (Laibson, 1997; O'Donoghue and Rabin, 1999; Gul and Pesendorfer, 2001). A third explanation is that unstudied elements of the CTB presentation encourage dynamic inconsistency. We explore this possibility in sub-section 1.4.2 by comparing CTB present bias with MPL present bias. MPL-identified present bias is substantially lower than previously obtained and correlates significantly with that found in CTBs at the individual level, suggesting that aspects of payment mechanism and not CTB presentation limit present bias in our context. It must also be recognized that our findings are one study among many, and further research is necessary before firm conclusions can be drawn.

[27] In McClure et al. (2004), immediate monetary rewards were received via e-mail in the form of Amazon gift certificates directly after the experiment.

The Effect of Setting ω_1 and ω_2 from Consumption Data and Alternative Utility Forms

Extra-experimental consumption poses an important challenge for studies of time preferences. While experimenters are able to vary experimental payments, subjects make choices over consumption streams including both experimental payments and non-experimental consumption. It is assumed that individuals do not adjust their non-experimental consumption. That is, ω_1 and ω_2 are taken as non-estimated, fixed parameters. Prior research has set these to zero or fixed $-\omega_1$ and $-\omega_2$ to match the average value of daily consumption (Andersen et al., 2008).

In column (1) of Table 1.2, we report estimates of both Stone-Geary parameters $\hat{\omega}_1$ and $\hat{\omega}_2$. The hypothesis that $\omega_1 = \omega_2$ is not rejected ($F_{1,96} = 0.87,\ p = 0.35$). In column (2) we report estimates of an identical NLS procedure with the restriction that $\omega_1 = \omega_2$ and obtain very similar results. This suggests the restriction that $\omega_1 = \omega_2$ is not costly.

Columns (3) through (6) of Table 1.2 examine whether the results are influenced by procedures that fix rather than estimate ω_1 and ω_2. Additionally, fixed values of ω_1 and ω_2 allow us to easily compare results across the estimators motivated in Section 1.3.1. We estimate non-linear least squares regressions identical to columns (1) and (2) and impose varying restrictions on the values of ω_1 and ω_2. We also provide two-limit tobit maximum likelihood regressions accounting for corner solution censoring, corresponding to the same restrictions.

In columns (3) and (4), the imposed restriction is $\omega_1 = \omega_2 = 0$.[28] In columns (5) and (6), we restrict $\omega_1 = \omega_2 = -7.05$, based on a post-experiment questionnaire which elicited average daily consumption of our subjects to be \$7.05.

Some differences in estimated parameters are obtained across econometric techniques. In particular, curvature is less pronounced when accounting for the censored nature of the data, as should be expected. Across econometric techniques, estimated preference parameters are found to be sensitive to the choice of background parameters. Both the estimated discount rate and $\hat{\alpha}$ decrease appreciably as the restricted value of the ω parameters moves from 0 to -7.05. The present bias parameter

[28] In column (4), the restriction is $\omega_1 = \omega_2 = -0.01$, such that the log consumption ratio $log(c_t - \omega_1/c_{t+k} - \omega_2)$ is well-defined.

$\hat{\beta}$ varies in a tight range.[29] These results suggest that the method of determining the ω parameters is potentially of great relevance. In Appendix Table 1.5, we demonstrate the effect of changing the values of ω_1 and ω_2 on estimated preference parameters for both NLS and tobit estimators. The results indicate substantial sensitivity of estimated parameters (particularly curvature) to increasingly negative values of ω_1 and ω_2. Corresponding R^2 and likelihood values diminish accordingly.

As discussed in Section 1.3.1, background parameters are eliminated from estimation under CARA utility if $\omega_1 = \omega_2$. As such, utility and discounting estimates based on CARA utility will not suffer from the same sensitivity to background assumptions as CRRA estimates. In columns (7) and (8) of Table 1.2 we provide two-limit Tobit CARA estimates based on (1.6) and (1.7). Virtually identical discounting and present bias parameters are estimated under this alternative functional form and coefficients of absolute risk aversion of $\hat{\rho}$ between 0.007 and 0.008 are obtained. Notable from these estimate as well as the CRRA estimates is the limited utility function curvature estimated from CTB responses. Taken as a measure of risk aversion, for a 50%-50% gamble over \$20 and \$0, our CARA column (7) and CRRA column (3) estimates indicate certainty equivalents of \$9.60 and \$9.23, respectively. These values are far from the often-found extreme experimental risk aversion and requires further research on the relationship between risk and time preferences. This work is begun in Andreoni and Sprenger (2010a).

1.4.2 Individual Analysis

Table 1.3 presents estimates of discounting, present bias and curvature parameters at the individual level. For each subject, we estimate the parameters of equation (2.1). To limit the number of estimated parameters and facilitate comparison with DMPL methodology, we restrict $\omega_1 = \omega_2 = 0$. The parameters $\hat{\beta}$, $\hat{\delta}$, and CRRA curvature parameter $\hat{\alpha}$ are estimated by non-linear least squares as in Table 1.2, column (3).[30] As robustness tests we first conduct estimation restricting $\omega_1 = \omega_2$

[29] Similar results are obtained when adapting the NLS criterion function for censoring. See Appendix Table 1.4.

[30] We opt for the NLS estimator to accommodate the restriction $\omega_1 = \omega_2 = 0$. Additionally, the motivated tobit estimators require a sufficient number of non-censored interior solutions for estimation. Given that 36 of 97 subjects have no interior solutions, consistent with linear preferences,

at various levels and, second, we allow ω_1 and ω_2 to equal minus self-reported daily consumption. Additionally, we provide tobit and OLS estimates. Obtained values are similar to Table 1.3 and reported in Appendix Tables 1.6 through 1.8.

Time preferences and curvature parameters are estimable for 86 of 97 subjects.[31] The results are broadly consistent with those estimated at the aggregate level. The median estimated annual discount rate is 0.41, close to the aggregate values obtained in Table 1.2. Echoing the aggregate results, individual present bias is limited as the median estimated $\hat{\beta}$ is 1.001. The median estimated $\hat{\alpha}$ is 0.967, suggesting that individual curvature, like aggregate curvature, is limited. In addition to median values, Table 1.3 reports the 5th-95th percentile range for individual estimates of the annual discount rate, $\hat{\delta}$, $\hat{\beta}$, and $\hat{\alpha}$ along with the minimum and maximum values estimated. For the majority of subjects the employed estimation strategy generates reasonable parameter estimates. However, extreme observations do exist. Figure 4.7, Panel A presents histograms of individual curvature and discounting estimates from the CTB methodology. The histograms demonstrate that a large proportion of subjects have low discount rates, limited present bias and limited utility function curvature. Estimation results for all subjects are in Appendix Tables 1.9 and 1.10.

Table 1.3: Individual Discounting, Present Bias and Curvature Parameter Estimates

	N	Median	5th Percentile	95th Percentile	Min	Max
Annual Discount Rate	86	0.4076	-0.1784	5.618	-0.9949	35.3555
Daily Discount Factor: $\hat{\delta}$	86	0.9991	0.9948	1.0005	0.9902	1.0146
Present Bias: $\hat{\beta}$	86	1.0011	0.9121	1.1075	0.7681	1.3241
CRRA Curvature: $\hat{\alpha}$	86	0.9665	0.7076	0.9997	-0.1331	0.9998

Notes: NLS estimators with restriction $\omega_1 = \omega_2 = 0$ as in Table 1.2, column (3).

this condition would not be met for a number of experimental subjects. See Appendix 1.6.1 for details.

[31] We do not study the 11 remaining subjects. Eight of these subjects had zero variance in their experimental responses, allocating the same number of sooner tokens in each choice set. Estimation convergence is not achieved for two subjects and the last remaining subject gave an identical pattern of sooner token choices in every choice set: 4 tokens in the first decision, 3 in the second, 2 in the third, 1 in the fourth and 0 in the fifth.

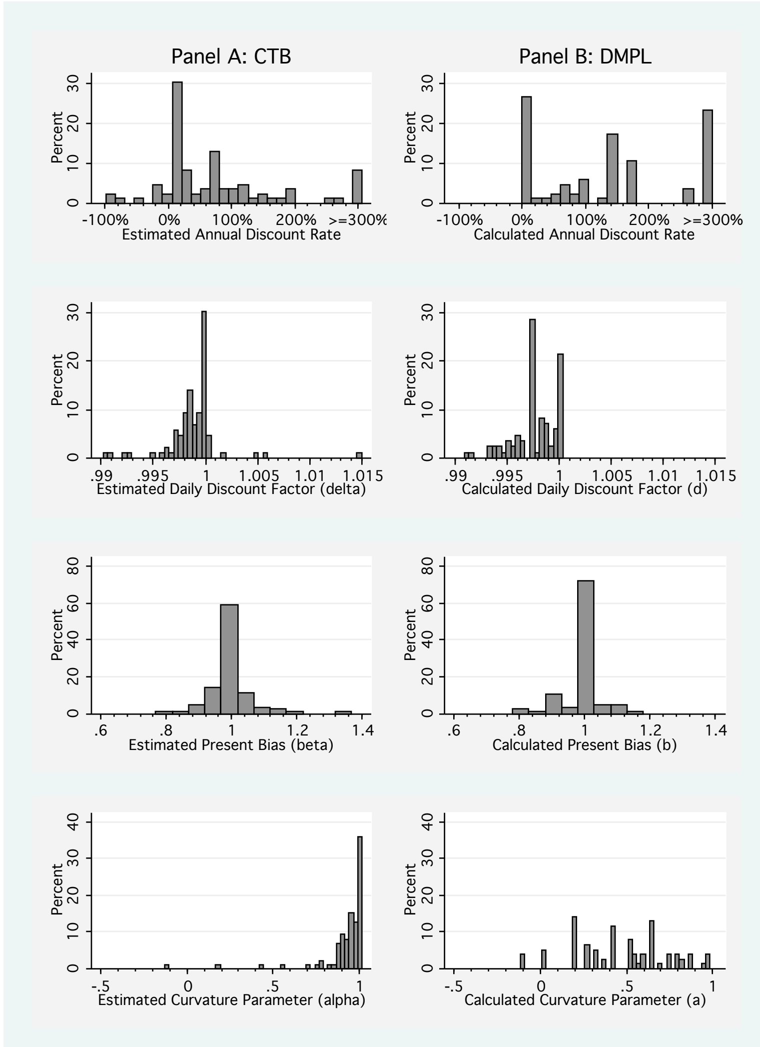

Figure 1.3: Histograms of CTB Estimates and DMPL Calculations

Correlation Between CTB Parameter Estimates and DMPL Calculations

For completeness, we compare individual discounting and curvature parameter estimates from the CTB to those calculated from DMPL methodology. Three standard time multiple price lists and two HL risk price lists were administered to all subjects. From the three price lists, we calculate daily discount factors following standard practice.[32] Given a switching point, X, a later payment, Y, and a delay

[32]MPL switch points yield an interval of the individual discount factor (Coller and Williams, 1999), which is easily accounted for with interval regression techniques (Coller and Williams, 1999; Harrison et al., 2002). However, common practice for calculation takes one point in the interval (see,

length, k, in a price list, l, we calculate the daily discount factor as $d_l = (X/Y)^{1/k}$. This is equivalent to positing a linear utility function and background $\omega_1 = \omega_2 = 0$. We examine the average of the three measures, $d = 1/3 \cdot (d_1 + d_2 + d_3)$. From the two HL risk price lists, we calculate curvature parameters also following standard practice.[33] Given a switching probability pair, $(p, 1-p)$, and two HL lotteries, A and B, in a specific price list l we take the value a_l that equates the CRRA expected utility of lottery A and lottery B. We take the midpoint of the interval in which this value lies as the calculated curvature parameter, a_l. We examine the average value, $a = 1/2 \cdot (a_1 + a_2)$. In both MPLs and HLs, individuals must exhibit a unique switching point to have a calculable discount factor or curvature parameter.

Of the subjects for whom we estimate $\hat{\delta}$, 84 of 86 have a calculable discount factor, d. The median value implies an annual discount rate of 137 percent, which replicates the very high observed discount rates in MPL experiments assuming linear utility. We can also identify present bias in the MPLs by the standard methodology of comparing the $(t, k) = (0, 35)$ MPL to the $(t, k) = (35, 35)$ MPL. Fourteen of 84 subjects (16.7%) are classified as present-biased, $(d_{(t=0,k=35)} < d_{(t=35,k=35)})$, while the median present bias parameter, b, is 1.[34] For comparison, using similar MPL methods, Ashraf et al. (2006), Dohmen et al. (2006), and Meier and Sprenger (2010) find around 30-35% of subjects to be present-biased and a substantially smaller percentage to be future-biased. In contrast, using closely controlled payments and the CTB method, Gine et al. (2010) find limited aggregate present bias and almost equal appearances of present and future bias.[35] This further supports the notion that our unique payment methods resulted in fewer instances of apparent present bias. Of the subjects for

for example Ashraf et al., 2006; Burks et al., 2009; Meier and Sprenger, 2010). We choose the point of the interval that makes subjects appear the *most* patient.

[33]HL switch points yield an interval of the individual curvature parameter (Holt and Laury, 2002), which can be accounted for with either interval regression techniques or alternative estimators (Harrison et al., 2005). However, common practice for calculation takes one point in the interval or alternatively the number of lottery A choices (see, for example Dohmen et al., 2005; Holt and Laury, 2002).

[34]Present bias b, is calculated as $(d_{(t=0,k=35)}/d_{(t=35,k=35)})^{35}$. Nine subjects are classified as future-biased $(d_{(t=0,k=35)} > d_{(t=35,k=35)})$ and 61 are classified as dynamically consistent $(d_{(t=0,k=35)} = d_{(t=35,k=35)})$

[35]Additionally, Gine et al. (2010) allow individuals to revise prior choices. Present bias, as measured in CTBs, predicts present-biased revisions. This gives support to the CTB methodology for being able to both measure individual preferences and predict important choice.

whom we estimate $\hat{\alpha}$, 77 of 86 have a calculable curvature parameter, a. The median value is 0.513 indicating substantial utility curvature.

Figure 4.7, Panel B provides histograms of these calculations for comparison with CTB estimates. Figure 4.7 shows that present bias is found to be similar across elicitation techniques. Discount rates and curvature, however, differ substantially. Time and risk price lists yield systematically higher discount rates and utility function curvature than CTB estimates. As in Andersen et al. (2008), correcting for curvature from the HL risk measures yields lower discounting estimates. Performing such an exercise, we obtain a median discount rate estimate of 33 percent per year. However, such a correction may be misguided given the wide difference between HL risk measures and the CTB estimates. This motivates careful examination of the correlation of obtained preference parameters across elicitation methods.

Figure 4.3 plots calculated DMPL and estimated CTB parameters against each other. In Panel A the calculated discount factor, d, is plotted against the estimated parameter, $\hat{\delta}$, along with an estimated regression line and 45 degree line. Panel B is similar for a and $\hat{\alpha}$. No panel is presented for b and β, because of the sheer volume of responses near to $(b, \hat{\beta}) = (1, 1)$. However, estimated present bias from CTB methodology, $\hat{\beta}$, and calculated present bias from MPL methodology b are significantly correlated ($\rho = 0.255,\ p < 0.05$) as are $\hat{\beta}$ and the frequently-used categorical variable classifying present-biased (1) , dynamically consistent (0) and future biased (-1) subjects, ($\rho = -0.274,\ p < 0.05$). The correlation between between DMPL and CTB present bias further suggests that payment methods as

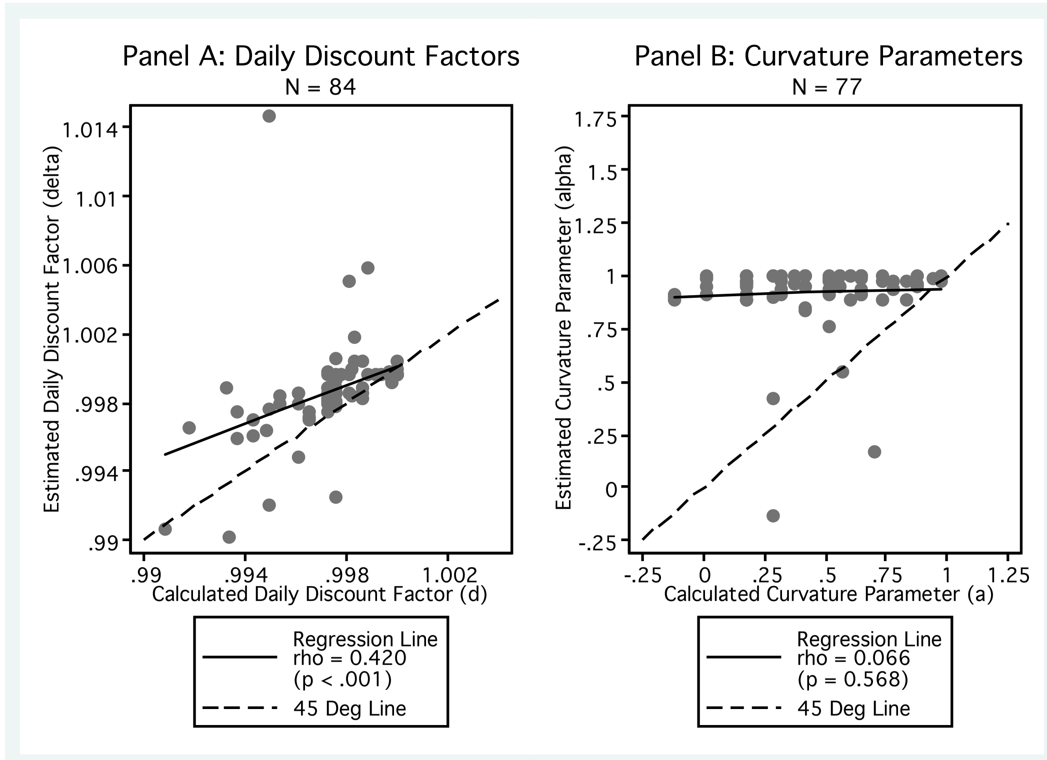

Figure 1.4: Comparison of CTB Estimates and DMPL Calculations

opposed to CTB presentation led to less apparent present bias.

Panel A of Figure 4.3 shows a high degree of correlation between MPL calculated and CTB estimated discount factors ($\rho = 0.420,\ p < 0.001$). However, most of the data lies above the 45 degree line, consistent with standard arguments that, under concave utility, discount factors calculated from price lists alone will be downwards-biased. Additionally, we can examine the difference, $\hat{\delta} - d$, as a measure of price list-induced bias. Interestingly, this discounting bias measure is negatively correlated with CTB estimated curvature, $\hat{\alpha}$, ($\rho = -0.743,\ p < 0.001$). Subjects who are closer to linear utility will have less biased MPL-calculated discount factors. This indicates that, though biased, standard MPLs do yield useful measures of time preference and that the bias attenuates with utility function curvature as theoretically predicted. Importantly, HL measured curvature does not correlate with the bias ($\rho = -0.092,\ p = 0.431$).

The lack of correlation between HL curvature and price list-induced discounting bias is not surprising. It is generated by the apparent zero correlation in Panel B of Figure 4.3 between HL calculated curvature, a, and CTB estimated curvature $\hat{\alpha}$ ($\rho = 0.066,\ p = 0.568$). This is interesting because, under CRRA utility, the two elicitation methodologies ostensibly measure the same utility construct. Not only is the level of curvature inconsistent between the two, but also the correlation is remarkably low. Additionally, HL curvature cannot account for the bias induced in MPL discounting experiments. These findings suggest that the practice of using HL *risk* experiments to identify and correct for curvature in *discounting* may be problematic.

As we obtain different parameter estimates across CTB and DMPL methodologies, a natural question arises as to which is better for eliciting time preferences. Though the individual analyses suggest the CTB estimates are more reasonable and can better explain the curvature-induced bias in MPL discount factors, more research must be conducted before firm conclusions can be drawn. Additionally, recent work from Noor (2009, 2011) demonstrates that an alternate experimental methodology fixing monetary payments and having delay length be the object of choice can, under certain regularity conditions, elicit discounting functions. This is in contrast to most experimental designs such as both CTB and MPL where time-dated rewards, with varying delay lengths and monetary values, are the object of choice. Though this new methodology has not been widely implemented, it should be tested and related to both CTB and DMPL techniques in order to both better understand the new mechanism and potentially understand which of the common time-dated rewards methodologies yields more consistent measures.

1.5 Conclusion

MPLs and other experimental methods frequently produce high estimates of annual discount rates at odds with non-laboratory measures. A possible bias of MPLs is the imposition of linear preferences, generating upwards-biased discount rate estimates if utility is actually concave. Solutions to this bias to date have relied on Double Multiple Price List methodology: identifying time preferences with MPLs and utility function curvature with HL risk measures.

We propose a single simple instrument that identifies discounting and utility function curvature, that we call Convex Time Budgets. Allocations in Convex Time Budgets are viewed as solutions to standard intertemporal optimization problems with convex choice sets. Given assumptions on functional form, discounting and curvature parameters are estimable. Additionally, tests of present-biased time preferences are easily implemented.

In a computer-based experiment with 97 subjects, we show that CTBs precisely identify discounting and curvature parameters at both the aggregate and individual levels. Across specifications, we find an aggregate discount rate of around 30% per year, substantially lower than most experimental estimates. Linear utility is rejected econometrically, though we find less utility function curvature than obtained with DMPL methodology or most studies using HL risk measures. Additionally, we find no evidence of present bias.

When examining individual estimates, we find that MPL-elicited discount rates, though upwards-biased, do correlate with CTB estimates. HL risk measures, however, are found to be virtually uncorrelated with CTB estimated utility function curvature.

These findings raise several natural and important questions. First, why did we find no evidence of present bias, while so many other studies using cash rewards do find present bias? The most likely answer, it appears to us, lies in the unique steps we took to equate the costs and risks associated with sooner and later payments. This is surely the most consequential aspect of our findings, and as such invites rigorous replication and testing.

Second, why do we find substantial differences between CTB estimates and those obtained with DMPL methodology? In particular, why is the curvature over time obtained from CTBs so different from and uncorrelated with the curvature over risk obtained from HL measures. Why can't HL risk measures account for MPL-induced bias in discounting? At a minimum, these results indicate that using risk experiments to identify curvature in discounting may be problematic. They also suggest that future research is necessary on the interactions between risk and time. Particular attention should be given to investigating the link between payment risk and present bias. We begin this investigation in Andreoni and Sprenger (2010a).

1.6 Appendix

1.6.1 Estimating Preference Parameters

Nonlinear Least Squares

Let there be N experimental subjects and P CTB budgets. Assume that each subject j makes her $c_{t_{ij}}$, $i = 1, 2, ..., P$, decisions according to (2.1) but that these decisions are made with some mean-zero, potentially correlated error. That is let

$$g(m,r,k,t;\beta,\delta,\alpha,\omega_1,\omega_2) = \left\{ \begin{array}{lll} \frac{1}{1+(1+r)(\beta\delta^k(1+r))^{(\frac{1}{\alpha-1})}}]\omega_1 + [\frac{(\beta\delta^k(1+r))^{(\frac{1}{\alpha-1})}}{1+(1+r)(\beta\delta^k(1+r))^{(\frac{1}{\alpha-1})}}](m-\omega_2) & if & t=0 \\ \frac{1}{1+(1+r)(\delta^k(1+r))^{(\frac{1}{\alpha-1})}}]\omega_1 + [\frac{(\delta^k(1+r))^{(\frac{1}{\alpha-1})}}{1+(1+r)(\delta^k(1+r))^{(\frac{1}{\alpha-1})}}](m-\omega_2) & if & t>0 \end{array} \right\},$$

then

$$c_{t_{ij}} = g(m,r,k,t;\beta,\delta,\alpha,\omega_1,\omega_2) + e_{ij}.$$

Stacking the P observations for individual j, we have

$$\mathbf{c_{t_j}} = \mathbf{g}(m,r,k,t;\beta,\delta,\alpha,\omega_1,\omega_2) + \mathbf{e_j}.$$

The vector $\mathbf{e_j}$ is zero in expectation with variance covariance matrix $\mathbf{V_j}$, a $(P \times P)$ matrix, allowing for arbitrary correlation in the errors e_{ij}. We stack over the N experimental subjects to obtain

$$\mathbf{c_t} = \mathbf{g}(m, r, k, t; \beta, \delta, \alpha, \omega_1, \omega_2) + \mathbf{e}.$$

We assume that the terms e_{ij} may be correlated within individuals but that the errors are uncorrelated across individuals, $E(\mathbf{e_j'}\mathbf{e_g}) = 0$ for $j \neq g$. And so $\mathbf{e}$ is zero in expectation with covariance matrix $\mathbf{\Omega}$, a block diagonal $(NP \times NP)$ matrix of clusters, with individual covariance matrices, $\mathbf{V_j}$.

We define the usual criterion function $S(m, r, k; \beta, \delta, \alpha, \omega_1, \omega_2)$ as the sum of squared residuals,

$$S(m, r, k, t; \beta, \delta, \alpha, \omega_1, \omega_2) = \sum_{j=1}^{N}\sum_{i=1}^{P}(c_{t_{ij}} - g(m, r, k, t; \beta, \delta, \alpha, \omega_1, \omega_2))^2,$$

and minimize $S(\cdot)$ using non-linear least squares with standard errors clustered on the individual level to obtain $\hat{\beta}$, $\hat{\delta}$, $\hat{\alpha}$, $\hat{\omega}_1$ and $\hat{\omega}_2$. NLS procedures permitting the estimation of preference parameters at the aggregate or individual level are implemented in many standard econometrics packages (in our case, *Stata*). Additionally, an estimate of the annual discount rate can be calculated as $(1/\hat{\delta})^{365} - 1$ with standard error obtained via the delta method. $\hat{\mathbf{\Omega}}$ is estimated as the individual-level clustered error covariance matrix. Given additional assumptions on the individual covariance matrix $\mathbf{V_j}$, such as diagonal or block-diagonal, individual parameter estimates can also be obtained via the same estimation procedure.

It is important to recognize the strengths and weaknesses of such an NLS preference estimator. Background parameters ω_1 and ω_2 can be estimated as opposed to assumed, which is an advantage. A potential disadvantage is that the NLS estimator is not well-suited to the censored data issues inherent to potential corner solutions without additional assumptions.

The NLS estimator can be adapted to account for possible corner solutions by adapting the criterion function and making additional distributional assumptions. Let c_t^* be a latent variable for period t allocation that follows

$c_t^* = g(m, r, k, t; \beta, \delta, \alpha, \omega_1, \omega_2) + \epsilon$. We observe $c_t = 0$ if $c_t^* \leq 0$, $c_t = m/1 + r$ if $c_t^* \geq m/1+r$ and $c_t = c_t^*$ otherwise. As discussed in Wooldridge (2002) Chapter 16, c_t^* here does not have an interpretation, but the latent variable vocabulary and associated censored techniques are applicable to corner solution applications. Borrowing from Greene (2003) Chapter 22, assume that ϵ is continuous random variable, with density $f(\epsilon)$ and distribution $F(\epsilon)$, that ϵ is orthogonal to the data (m, r, k, t) and has mean 0 and variance σ^2. Then the expectation

$$E[c_t|m, r, k, t] = P[c_t^* \leq 0|m, r, k, t] \cdot 0 + P[c_t^* \geq \frac{m}{1+r}|m, r, k, t] \cdot \frac{m}{1+r} + P[0 < c_t^* < \frac{m}{1+r}] \cdot E[c_t^*|0 < c_t^* < \frac{m}{1+r}|m, r, k, t]$$

can be rewritten

$$E[c_t|m, r, k, t] = F_l \cdot 0 + (1 - F_h) \cdot \frac{m}{1+r} + (F_h - F_l) \cdot E[c_t^*|0 < c_t^* < \frac{m}{1+r}|m, r, k, t],$$

where $F_h = F(\frac{m/(1+r)-g(\cdot)}{\sigma})$ and $F_l = F(\frac{0-g(\cdot)}{\sigma})$. A distributional assumption is imposed on ϵ to provide functional form. In particular ϵ is taken to follow a normal distribution. This provides the following form,

$$E[c_t|m, r, k, t] = \Phi_l \cdot 0 + (1 - \Phi_h) \cdot \frac{m}{1+r} + (\Phi_h - \Phi_l) \cdot (g(\cdot) + (\frac{\phi_l - \phi_h}{\Phi_h - \Phi_l})\sigma),$$

with $\Phi(\cdot)$ and $\phi(\cdot)$ representing the standard normal distribution and density, respectively.

We introduce $\tilde{g}(m, r, k, t; \beta, \delta, \alpha, \omega_1, \omega_2, \sigma) = \Phi_l \cdot 0 + (1 - \Phi_h) \cdot \frac{m}{1+r} + (\Phi_h - \Phi_l) \cdot (g(\cdot) + (\frac{\phi_l - \phi_h}{\Phi_h - \Phi_l})\sigma)$, with $g(\cdot)$ defined as before. This motivates a new criterion function

$$\tilde{S}(m, r, k, t; \beta, \delta, \alpha, \omega_1, \omega_2) = \sum_{j=1}^{N} \sum_{i=1}^{P} (c_{t_{ij}} - \tilde{g}(m, r, k, t; \beta, \delta, \alpha, \omega_1, \omega_2, \sigma))^2, \qquad (1.8)$$

which is minimized using non-linear least squares with standard errors clustered on the individual level. Estimates are discussed in the text and presented in Appendix Table 1.4.

Censored Regression Techniques

Next we consider more standard censored regression techniques that can address corner solution issues. We consider the tangency condition of (2.2). If we assume ω_1 and ω_2 are non-estimated, known values, we can take logs to obtain

$$ln(\frac{c_t - \omega_1}{c_{t+k} - \omega_2}) = \left\{ \begin{array}{lll} (\frac{ln\ \beta}{\alpha-1}) + (\frac{ln\ \delta}{\alpha-1}) \cdot k + (\frac{1}{\alpha-1}) \cdot ln(1+r) & if & t=0 \\ (\frac{ln\ \delta}{\alpha-1}) \cdot k + (\frac{1}{\alpha-1}) \cdot ln(1+r) & if & t>0 \end{array} \right\},$$

which is linear in the in the data k and $ln(1+r)$, and reduces to,

$$ln(\frac{c_t - \omega_1}{c_{t+k} - \omega_2}) = (\frac{ln\ \beta}{\alpha - 1}) \cdot \mathbf{1}_{t=0} + (\frac{ln\ \delta}{\alpha - 1}) \cdot k + (\frac{1}{\alpha - 1}) \cdot ln(1+r),$$

where $\mathbf{1}_{t=0}$ is an indicator for the time period $t = 0$.

Let there be N experimental subjects and P CTB budgets. Assume that each subject j makes her $c_{t_{ij}}$, $i = 1, 2, ..., P$, decisions according to the above log-linearized relationship but that these decisions are made with some additive mean-zero, potentially correlated error. That is,

$$ln(\frac{c_t - \omega_1}{c_{t+k} - \omega_2})_{ij} = (\frac{ln\ \beta}{\alpha - 1}) \cdot \mathbf{1}_{t=0} + (\frac{ln\ \delta}{\alpha - 1}) \cdot k + (\frac{1}{\alpha - 1}) \cdot ln(1+r) + e_{ij},$$

Stacking the P observations for individual j, we have

$$\mathbf{ln}(\frac{\mathbf{c_t} - \omega_1}{\mathbf{c_{t+k}} - \omega_2})_\mathbf{j} = (\frac{ln\ \beta}{\alpha - 1}) \cdot \mathbf{1}_{t=0} + (\frac{ln\ \delta}{\alpha - 1}) \cdot \mathbf{k} + (\frac{1}{\alpha - 1}) \cdot \mathbf{ln(1+r)} + \mathbf{e_j}$$

The vector $\mathbf{e_j}$ is zero in expectation with variance covariance matrix $\mathbf{V_j}$, a $(P \times P)$ matrix, allowing for arbitrary correlation in the errors e_{ij}. We stack over the N experimental subjects to obtain

$$\mathbf{ln}(\frac{\mathbf{c_t} - \omega_1}{\mathbf{c_{t+k}} - \omega_2}) = (\frac{ln\ \beta}{\alpha - 1}) \cdot \mathbf{1}_{t=0} + (\frac{ln\ \delta}{\alpha - 1}) \cdot \mathbf{k} + (\frac{1}{\alpha - 1}) \cdot \mathbf{ln(1+r)} + \mathbf{e}$$

We assume that the terms e_{ij} may be correlated within individuals but that the errors are uncorrelated across individuals, $E(\mathbf{e_j'e_g}) = 0$ for $j \neq g$. And so $\mathbf{e}$ is zero in expectation with covariance matrix $\mathbf{\Omega}$, a block diagonal $(NP \times NP)$ matrix of clusters, with individual covariance matrices, $\mathbf{V_j}$.

The above linear model is easily estimated with ordinary least squares. However the log consumption ratio is censored by corner solution responses,

$$ln(\frac{c_t - \omega_1}{c_{t+k} - \omega_2}) \in [ln(\frac{0 - \omega_1}{c_{t+k} - \omega_2}), ln(\frac{c_t - \omega_1}{0 - \omega_2})],$$

motivating censored regression techniques such as the two-limit tobit model more appropriate. Wooldridge (2002) presents corner solutions as the primary motivation for two-limit tobit regression techniques and Chapter 16, Problem 16.3 corresponds closely to the above. Parameters can be estimated via the two-limit tobit regression.

$$\mathbf{ln}(\frac{\mathbf{c_t} - \omega_\mathbf{1}}{\mathbf{c_{t+k}} - \omega_\mathbf{2}}) = \gamma_1 \cdot \mathbf{1}_{t=0} + \gamma_2 \cdot \mathbf{k} + \gamma_3 \cdot \mathbf{ln}(\mathbf{1} + \mathbf{r}) + \mathbf{e}$$

With parameters of interest recovered via the non-linear combinations

$$\hat{\alpha} = \frac{1}{\hat{\gamma}_3} + 1 \ ; \ \hat{\delta} = exp(\frac{\hat{\gamma}_2}{\hat{\gamma}_3}) \ ; \ \hat{\beta} = exp(\frac{\hat{\gamma}_1}{\hat{\gamma}_3}),$$

and standard errors obtained via the delta method. Additionally, an estimate of the annual discount rate can be calculated as $(1/\hat{\delta})^{365} - 1$ with standard error obtained via the delta method. $\hat{\mathbf{\Omega}}$ is estimated as the individual-level clustered error covariance matrix.

Given additional assumptions on the individual covariance matrix $\mathbf{V_j}$, such as diagonal or block-diagonal as well as a sufficient number of non-censored observations (one less than the number of parameters), individual parameter estimates can also be obtained via the same estimation procedure.

Censored regression techniques are helpful in addressing the critical issues of corner solutions. However, there are disadvantages to the technique. First, the values ω_1 and ω_2 must be assumed rather than estimated from the data. Second, the consumption ratio $(\frac{c_t - \omega_1}{c_{t+k} - \omega_2})$ must be strictly positive such that the log consumption ratio is well defined. This restricts the values of ω_1 and ω_2 to be strictly negative.

Under alternative preference models, the difficulty of background parameters is eliminated. Consider for example constant absolute risk aversion utility, $u(c_t) = -exp(-\rho(c_t - \omega_1)) = -exp(-\rho c_t) \cdot exp(\rho \omega_1)$. Under this CARA parameterization and

$\omega_1 = \omega_2$, the background parameters drop out of the marginal condition such that the tangency can be written

$$exp(-\rho(c_t - c_{t+k})) = \left\{ \begin{array}{lll} \beta\delta^k \cdot (1+r) & if & t=0 \\ \delta^k \cdot (1+r) & if & t>0 \end{array} \right\}.$$

Taking logs and rearranging, this is linear in the data $\mathbf{1}_{t=0}$, k, and $ln(1+r)$, reducing to

$$c_t - c_{t+k} = (\frac{ln\ \beta}{-\rho}) \cdot \mathbf{1}_{t=0} + (\frac{ln\ \delta}{-\rho}) \cdot k + (\frac{1}{-\rho}) \cdot ln(1+r).$$

This can again be estimated with censored regression techniques and parameters of interest recovered as before. Additionally, the solution function,

$$c_t = (\frac{ln\ \beta}{-\rho}) \cdot \frac{\mathbf{1}_{t=0}}{2+r} + (\frac{ln\ \delta}{-\rho}) \cdot \frac{k}{2+r} + (\frac{1}{-\rho}) \cdot \frac{ln(1+r)}{2+r} + \frac{m}{2+r},$$

can also be estimated with censored regression techniques with the coefficient on the nuisance term $\frac{m}{2+r}$ constrained to be 1. As the strategies employed for these censored CARA regressions are virtually identical to those just discussed for CRRA utility, further matrix notation is unnecessary.

1.6.2 About Arbitrage

A relevant issue with monetary incentives in time preference experiments, as opposed to experiments using primary consumption as rewards, is that, in theory, monetary payments should be subject to extra-lab arbitrage opportunities. Subjects who can borrow (save) at external interest rates inferior (superior) to the rates offered in the lab should arbitrage the lab by taking the later (sooner) experimental payment. As such, discount rates measured using monetary incentives should collapse to the interval of external borrowing and savings interest rates and present bias should be observed only if liquidity positions or interest rates are expected to change. In the CTB context, this arbitrage argument also implies that subjects should *never* choose intermediate allocations unless they are liquidity constrained.[36] Furthermore,

[36] If an arbitrage opportunity exists, the lab offered budget set is inferior to the extra-lab budget set everywhere except one corner solution. This corner should be the chosen allocation. Liquidity

for ‘secondary’ rewards, such as money, it is possible that there could be less of a visceral temptation for immediate gratification than for ‘primary’ rewards that can be immediately consumed. As a result, one might expect limited present bias when monetary incentives are used.

Contrary to the arbitrage argument, others have shown that experimentally elicited discount rates are generally not measured in a tight interval near market rates (Coller and Williams, 1999; Harrison et al., 2002); they are not remarkably sensitive to the provision of external rate information or to the elaboration of arbitrage opportunities (Coller and Williams, 1999); and they are uncorrelated with credit constraints (Meier and Sprenger, 2010). In our CTB environment, a sizeable proportion of chosen allocations are intermediate (30.4% of all responses, average of 13.7 per subject) and the number of intermediate allocations is uncorrelated with individual liquidity proxies such as credit-card holdership ($\rho = -0.049,\ p = 0.641$) and bank account holdership ($\rho = -0.096,\ p = 0.362$).

Despite the fact that money is not a primary reward, monetary experiments do generate evidence of present-biased preferences (Dohmen et al., 2006; Meier and Sprenger, 2010). Of further interest is the finding by McClure et al. (2004, 2007) that discounting and present bias over primary and monetary rewards have very similar neural images. As well, discount factors elicited over primary and monetary rewards correlate highly at the individual level (Reuben et al., 2008). The fact that we find significant but limited utility function curvature is therefore consistent with the evidence of strict convexity of preferences in the presence of arbitrage.

constraints could yield intermediate allocations if individuals are unable to move resources through time outside of the lab and desire smooth consumption streams. Additionally intermediate allocations could be obtained if the lab-offered rate lay in between borrowing and savings rates. Cubitt and Read (2007) provide substantial discussion on the limits of the preference information that can be obtained from intertemporal choice experiments.

1.6.3 Additional Aggregate Estimates

In this appendix we provide two table of additional aggregate estimates. Table 1.4 provides NLS estimates adapted for censoring as described in Appendix Section 1.6.1 with the normalization $\sigma = 1$. Table 1.5 demonstrates the sensitivity of estimates to alternate assumptions on background parameters ω_1 and ω_2 with NLS and Two-Limit Tobit estimates.

Table 1.4: Discounting and Curvature Estimates

	(1)	(2)	(3)	(4)
Method:	NLS	NLS	NLS	NLS
Annual Discount Rate	0.297	0.377	0.374	0.371
	(0.063)	(0.087)	(0.027)	(0.027)
Present Bias: $\hat{\beta}$	1.007	1.006	1.007	1.006
	(0.005)	(0.006)	(0.006)	(0.006)
CRRA Curvature: $\hat{\alpha}$	0.919	0.921	0.899	0.810
	(0.006)	(0.006)	(0.004)	(0.006)
$\hat{\omega}_1$	1.340			
	(0.297)			
$\hat{\omega}_2$	-0.083			
	(1.580)			
$\hat{\omega}_1 = \hat{\omega}_2$		1.321	0	-7.046
		(0.302)	-	-
R^2	0.2396	0.2393	0.2355	0.2231
# Observations	4365	4365	4365	4365
# Clusters	97	97	97	97

Notes: NLS estimators of equation (1.8) accounting for censoring. Column (1): Unrestricted CRRA regression. Column (2): CRRA regression with restriction $\omega_1 = \omega_2$. Column (3) CRRA regression with restriction with restriction $\omega_1 = \omega_2 = 0$. Column (4): CRRA regression with restriction $\omega_1 = \omega_2 = -7.046$ (the negative of average reported daily spending). Clustered standard errors in parentheses. Annual discount rate calculated as $(1/\hat{\delta})^{365} - 1$. Standard errors calculated via the delta method.

Table 1.5: Background Consumption, Parameter Estimates and Goodness of Fit

	NLS Estimates				*Two-Limit Tobit Estimates*			
$\omega_1 = \omega_2$	Discount Rate (s.e.)	$\hat{\beta}$ (s.e)	$\hat{\alpha}$ (s.e.)	R^2	Discount Rate (s.e.)	$\hat{\beta}$ (s.e)	$\hat{\alpha}$ (s.e.)	Log-Likelihood
-25	.151	1.04	.24	.433	.264	1.027	.711	-4173.8
	(.151)	(.01)	(.045)		(.16)	(.01)	(.041)	
-20	.159	1.039	.361	.434	.266	1.027	.754	-4393.04
	(.149)	(.009)	(.037)		(.16)	(.01)	(.035)	
-15	.175	1.037	.487	.437	.268	1.027	.799	-4660.35
	(.145)	(.009)	(.03)		(.161)	(.01)	(.029)	
-14	.18	1.036	.513	.438	.269	1.027	.808	-4721.82
	(.144)	(.009)	(.028)		(.161)	(.01)	(.028)	
-13	.186	1.035	.539	.439	.27	1.027	.817	-4786.7
	(.142)	(.009)	(.027)		(.161)	(.01)	(.026)	
-12	.192	1.034	.566	.44	.27	1.027	.826	-4855.43
	(.141)	(.009)	(.025)		(.161)	(.01)	(.025)	
-11	.2	1.033	.593	.441	.271	1.027	.835	-4928.58
	(.139)	(.009)	(.024)		(.161)	(.01)	(.024)	
-10	.209	1.032	.621	.443	.272	1.027	.845	-5006.81
	(.137)	(.008)	(.022)		(.161)	(.01)	(.022)	
-9	.22	1.03	.649	.445	.273	1.027	.854	-5091.02
	(.134)	(.008)	(.02)		(.161)	(.01)	(.021)	
-8	.232	1.028	.678	.447	.274	1.026	.864	-5182.36
	(.131)	(.008)	(.019)		(.162)	(.01)	(.02)	
-7	.246	1.026	.707	.45	.275	1.026	.874	-5282.39
	(.127)	(.008)	(.017)		(.162)	(.01)	(.018)	
-6	.263	1.023	.737	.453	.277	1.026	.884	-5393.3
	(.123)	(.008)	(.016)		(.162)	(.01)	(.017)	
-5	.282	1.02	.767	.458	.279	1.026	.894	-5518.36
	(.118)	(.007)	(.014)		(.162)	(.01)	(.015)	
-4	.302	1.017	.796	.463	.281	1.026	.904	-5662.8
	(.113)	(.007)	(.013)		(.163)	(.01)	(.014)	
-3	.323	1.014	.824	.468	.284	1.026	.916	-5835.85
	(.107)	(.007)	(.012)		(.163)	(.01)	(.012)	
-2	.342	1.011	.851	.475	.288	1.026	.928	-6056.91
	(.101)	(.006)	(.01)		(.164)	(.01)	(.01)	
-1	.359	1.009	.875	.481	.295	1.025	.943	-6382.19
	(.095)	(.006)	(.009)		(.166)	(.01)	(.008)	

Notes: NLS and two-limit tobit estimators with restriction $\omega_1 = \omega_2$ equal to first column as in Table 1.2. 4365 observations (1329 uncensored) for each row. Clustered standard errors in parentheses. Annual discount rate calculated as $(1/\hat{\delta})^{365} - 1$, standard errors calculated via the delta method.

1.6.4 Additional Individual Estimates

In this appendix we provide three summary tables and two tables of individual estimates of additional individual level estimates with alternative specifications and estimators. All three tables are in the form of Table 1.3. In 1.6 we impose the restriction $\omega_1 = \omega_2 = -7.05$, minus average daily background consumption, and provide NLS estimates. In 1.7, we impose the same restriction and provide tobit estimators. For individuals with one or fewer interior solutions, we estimate via OLS as the tobit requires at least two uncensored observations for estimation. See Appendix Section 1.6.1 for details. In 1.8 we impose the restriction $\omega_1 = \omega_2 = -B$, where B corresponds to the subject's own self-reported daily background consumption, and provide NLS estimates for responders. The number of subjects for whom estimation is achieved is also reported and varies across tables. Tables 1.9 and 1.10 provide NLS estimates for each subject with $\omega_1 = \omega_2 = 0$ as in Table 1.2, column (3) and discussed in the text.

Table 1.6: Individual Discounting, Present Bias and Curvature Parameter Estimates

	N	Median	5th Percentile	95th Percentile	Min	Max
Annual Discount Rate	88	.4277	-.8715	5.6481	-1	55.4768
Daily Discount Factor: $\hat{\delta}$	88	.999	.9948	1.0056	.989	1.031
Present Bias: $\hat{\beta}$	88	1.0285	.8963	1.1566	.8016	1.1961
CRRA Curvature: $\hat{\alpha}$	88	.7536	.1293	.8977	-3.273	.9052

Notes: NLS estimators with restriction $\omega_1 = \omega_2 = -7.05$.

Table 1.7: Individual Discounting, Present Bias and Curvature Parameter Estimates

	N	Median	5th Percentile	95th Percentile	Min	Max
Annual Discount Rate	84	.3923	-.9868	7.9005	-1	42.9775
Daily Discount Factor: $\hat{\delta}$	84	.9991	.994	1.0119	.9897	1.4535
Present Bias: $\hat{\beta}$	84	1.0238	.9102	1.3384	.8426	5.7041
CRRA Curvature: $\hat{\alpha}$	84	.7836	-.0838	.9846	-50.4261	.9916

Notes: Tobit and OLS (for subjects with one or fewer uncensored observations) estimators with restriction $\omega_1 = \omega_2 = -7.05$.

Table 1.8: Individual Discounting, Present Bias and Curvature Parameter Estimates

	N	Median	5th Percentile	95th Percentile	Min	Max
Annual Discount Rate	82	.3734	-.9169	3.7477	-.9989	80.6357
Daily Discount Factor: $\hat{\delta}$	82	.9991	.9957	1.0068	.988	1.0187
Present Bias: $\hat{\beta}$	82	1.0087	.905	1.2156	.8208	1.2223
CRRA Curvature: $\hat{\alpha}$	82	.7987	-.0155	.9859	-.6922	.9955

Notes: NLS estimators with restriction $\omega_1 = \omega_2 = -B$, the subject's own self-reported daily background consumption. Reporters only.

Table 1.9: Individual Estimates 1

Subject #	Annual Rate	$\hat{\beta}$	$\hat{\alpha}$	Interior	Proportion of Responses Zero Tokens Sooner	All Tokens Sooner
1	.123	.958	.984	.4	.56	.04
2	.73	1.054	1	.16	.64	.2
3	.931	.988	.986	0	.71	.29
4	.55	1.017	.935	.6	.27	.13
5	.117	1.001	.999	0	.98	.02
6	.117	1.001	.999	0	.98	.02
7	.339	1.02	.979	.18	.78	.04
8	1.906	1	.911	.13	.44	.42
9	.117	1.001	.999	0	.98	.02
10	.	.	.	0	1	0
11	.735	.931	1	.07	.62	.31
12	1.966	.979	.955	.13	.38	.49
13	.496	1.027	.993	.51	.4	.09
14	.	.	.	0	.22	.78
15	.965	.993	.98	0	.69	.31
16	.305	.994	.916	.51	.49	0
17	.723	.938	.996	0	.71	.29
18	14.452	1.107	.951	.31	.09	.6
19	1.318	1.105	.885	.84	.11	.04
20	-.16	.904	.956	.16	.84	0
21	1.592	.984	.952	.13	.49	.38
22	5.618	.971	.772	.13	.2	.67
23	.707	.999	1	0	.8	.2
24	.117	1.001	.999	0	.98	.02
25	.117	1.001	.999	0	.98	.02
26	.117	1.001	.999	0	.98	.02
27	1.145	.993	.975	.07	.56	.38
28	2.742	.994	.933	.42	.22	.36
29	.	.	.	1	0	0
30	.676	1.043	.906	.64	.29	.07
31	.144	1.015	.966	.33	.64	.02
32	.73	.973	.963	.49	.42	.09
33	.788	1.002	.954	0	.73	.27
34	17.243	.912	.927	.18	.04	.78
35	.	.	.	0	0	1
36	.117	1.001	.999	0	.98	.02
37	.736	1.006	.997	.07	.71	.22
38	-.837	.852	.167	1	0	0
39	1.134	1.131	.887	.98	0	.02
40	.117	1.001	.999	0	.98	.02
41	1.81	.911	.885	.6	.04	.36
42	1.186	.967	.933	.58	.2	.22
43	.899	.975	.935	.18	.6	.22
44	.257	.979	1	0	.89	.11
45	.1	1.033	.89	.96	.04	0
46	-.995	.999	-.133	1	0	0
47	.476	1.078	.975	.22	.73	.04
48	.	.	.	0	1	0
49	1.545	1.062	.953	.36	.33	.31
50	.116	.94	.997	0	.89	.11

Table 1.10: Individual Estimates 2

Subject #	Annual Rate	$\hat{\beta}$	$\hat{\alpha}$	Interior	Proportion of Responses: Zero Tokens Sooner	Proportion of Responses: All Tokens Sooner
51	29.583	1.138	.918	.13	0	.87
52	.	.	.	.04	.76	.2
53	2.536	1.191	.847	.71	.09	.2
54	.219	1.003	.976	.16	.82	.02
55	.169	.975	.968	.09	.87	.04
56	.744	.916	.95	.16	.56	.29
57	-.144	1.042	.944	.38	.62	0
58	.306	1.01	.999	0	.91	.09
59	-.88	.974	.771	.98	.02	0
60	3.462	.768	.915	.11	.2	.69
61	1.511	.957	.904	.89	0	.11
62	-.123	1.037	.419	1	0	0
63	.513	.992	.761	1	0	0
64	.732	.949	1	.16	.62	.22
65	.126	1	.993	.69	.29	.02
66	1.073	.957	.834	.91	.04	.04
67	.291	1.003	.951	.36	.6	.04
68	.117	1.001	.999	0	.98	.02
69	.117	1.001	.999	0	.98	.02
70	3.225	.959	.89	.71	0	.29
71	.117	1.001	.999	0	.98	.02
72	35.356	1.324	.991	0	.22	.78
73	.117	1.001	.999	0	.98	.02
74	.117	1.001	.999	0	.98	.02
75	.109	1.059	.884	.42	.58	0
76	-.474	1.003	.708	1	0	0
77	.117	1.001	.999	0	.98	.02
78	0	1.003	.999	.02	.98	0
79	.	.	.	0	1	0
80	-.178	.982	.913	.47	.53	0
81	.834	1.009	.907	.56	.38	.07
82	.219	.986	.543	1	0	0
83	.117	1.001	.999	0	.98	.02
84	.	.	.	.8	.2	0
85	-.001	1.007	.973	.87	.13	0
86	.117	1.001	.999	0	.98	.02
87	.	.	.	0	0	1
88	1.206	.959	.972	.49	.22	.29
89	.117	1.001	.999	0	.98	.02
90	1.954	.935	.905	.38	.16	.47
91	.732	1.027	.943	.62	.33	.04
92	.999	.986	.967	.36	.49	.16
93	.	.	.	0	1	0
94	.117	1.001	.999	0	.98	.02
95	.117	1.001	.999	0	.98	.02
96	.555	1.051	.938	.76	.22	.02
97	.	.	.	0	.64	.36

1.6.5 Welcome Text and Payment Explanation

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)

You must live in:

- XXX College.
- XXX College AND have a student mail box number between 92XXXX and 92XXXX
- XXX College AND have a student mail box number between 92XXXX through 92XXXX.

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. This means that, 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.

Payment Explanation

Earning Money

To begin, you will be given a $10 thank-you payment, just for participating in this study! You will receive this thank-you payment in two equally sized payments of $5 each. The two $5 payments will come to you at two different times. These times will be determined in the way described below.

In this study, you will make 47 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 today, and as late as the last week of classes in the Spring Quarter, or possibly two other dates in between. Once all 47 decisions have been made, we will randomly select one of the 47 decisions as the decision-that-counts. 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 thank you 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. That includes payments that you receive today as well as payments you may receive at later dates. On the scheduled day of payment, a check will be placed for delivery in campus mail services by Professor Andreoni and his assistants. *By special arrangement, campus mail services has guaranteed delivery of 100% of your payments on the same 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.

You have been assigned a participant number. This will be linked to your personal information in order to complete payment. After all payments have been made, only the participant number will remain in the data set.

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.

1.6.6 Multiple Price Lists and Holt Laury Risk Price Lists

NAME: ______________________________ PID:____________________

How It Works:

In the following sheets you are asked to choose between smaller payments closer to today and larger payments further in the future. For each row, choose one payment: either the smaller, sooner payment or the larger, later payment. There are 22 decisions in total. Each decision has a number from 1 to 22.

NUMBERS 1 THROUGH 7: Decide between payment today and payment in five weeks
NUMBERS 8 THROUGH 15: Decide between payment today and payment in fourteen weeks
NUMBERS 16 THROUGH 22: Decide between payment in five weeks and payment in ten weeks

This sheet represents one of the 47 choices you make in the experiment. If the number 47 is drawn, this sheet will determine your payoffs. If the number 47 is drawn, a second number will also be drawn from 1 to 22. This will determine which decision (from 1 to 22) on the sheet is the decision-that-counts. The payment you choose (either sooner or later) in the decision that counts will be added to either your earlier $5 thank-you payment or your later $5 thank-you payment.

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

TODAY VS. FIVE WEEKS FROM TODAY

WHAT WILL YOU DO IF YOU GET A NUMBER BETWEEN 1 AND 7?

Decide for **each** possible number if you would like the smaller payment for sure **today** or the larger payment for sure in **five weeks**? Please answer for each possible number (1) through (7) by filling in one box for each possible number.

Example: *If you prefer $19 today in Question 1 mark as follows:* ☑ *$19* ***today*** *or* ☐ *$20 in* ***five weeks***
If you prefer $20 in five weeks in Question 1, mark as follows: ☐ *$19* ***today*** *or* ☑ *$20 in* ***five weeks***

If you get number (1):	Would you like to receive ☐ $19 **today**	or	☐ $20 in **five weeks**
If you get number (2):	Would you like to receive ☐ $18 **today**	or	☐ $20 in **five weeks**
If you get number (3):	Would you like to receive ☐ $16 **today**	or	☐ $20 in **five weeks**
If you get number (4):	Would you like to receive ☐ $14 **today**	or	☐ $20 in **five weeks**
If you get number (5):	Would you like to receive ☐ $11 **today**	or	☐ $20 in **five weeks**
If you get number (6):	Would you like to receive ☐ $8 **today**	or	☐ $20 in **five weeks**
If you get number (7):	Would you like to receive ☐ $5 **today**	or	☐ $20 in **five weeks**

TODAY VS. FOURTEEN WEEKS FROM TODAY

WHAT WILL YOU DO IF YOU GET A NUMBER BETWEEN 8 AND 15?

Decide for **each** possible number if you would like the smaller payment for sure **today** or the larger payment for sure in **fourteen weeks**? Please answer for each possible number (8) through (15) by filling in one box for each possible number.

Example: If you prefer $19 today in Question 8 mark as follows: ☑ *$19* ***today*** *or* ☐ *$20 in* ***14 weeks***
If you prefer $20 in fourteen weeks in Question 8, mark as follows: ☐ *$19* ***today*** *or* ☑ *$20 in* ***14 weeks***

If you get number (8):	Would you like to receive	☐ $20 **today**	or ☐ $20 in **fourteen weeks**
If you get number (9):	Would you like to receive	☐ $19 **today**	or ☐ $20 in **fourteen weeks**
If you get number (10):	Would you like to receive	☐ $18 **today**	or ☐ $20 in **fourteen weeks**
If you get number (11):	Would you like to receive	☐ $16 **today**	or ☐ $20 in **fourteen weeks**
If you get number (12):	Would you like to receive	☐ $13 **today**	or ☐ $20 in **fourteen weeks**
If you get number (13):	Would you like to receive	☐ $10 **today**	or ☐ $20 in **fourteen weeks**
If you get number (14):	Would you like to receive	☐ $7 **today**	or ☐ $20 in **fourteen weeks**
If you get number (15):	Would you like to receive	☐ $4 **today**	or ☐ $20 in **fourteen weeks**

FIVE WEEKS FROM TODAY VS. TEN WEEKS FROM TODAY

WHAT WILL YOU DO IF YOU GET A NUMBER BETWEEN 16 AND 22?

Decide for **each** possible number if you would like the smaller payment for sure in **five weeks** or the larger payment for sure in **ten weeks**? Please answer for each possible number (16) through (22) by filling in one box for each possible number.

Example: If you prefer $19 in four weeks in Question 16 mark as follows: ☑ $19 ***in 5 weeks or*** *☐ $20 in* ***10 weeks***
If you prefer $20 in ten weeks in Question 16, mark as follows: ☐ $19 ***in 5 weeks*** *or ☑ $20 in* ***10 weeks***

If you get number (16): Would you like to receive	☐ $19 **in five weeks**	or ☐ $20 in **ten weeks**
If you get number (17): Would you like to receive	☐ $18 **in five weeks**	or ☐ $20 in **ten weeks**
If you get number (18): Would you like to receive	☐ $16 **in five weeks**	or ☐ $20 in **ten weeks**
If you get number (19): Would you like to receive	☐ $14 **in five weeks**	or ☐ $20 in **ten weeks**
If you get number (20): Would you like to receive	☐ $11 **in five weeks**	or ☐ $20 in **ten weeks**
If you get number (21): Would you like to receive	☐ $8 **in five weeks**	or ☐ $20 in **ten weeks**
If you get number (22): Would you like to receive	☐ $5 **in five weeks**	or ☐ $20 in **ten weeks**

Decision	Option A							Option B					
		If the die reads	you receive	and	If the die reads	you receive			If the die reads	you receive	and	If the die reads	you receive
1	☐	1	10.39		2-10	8.31		☐	1	20		2-10	0.52
2	☐	1-2	10.39		3-10	8.31		☐	1-2	20		3-10	0.52
3	☐	1-3	10.39		4-10	8.31		☐	1-3	20		4-10	0.52
4	☐	1-4	10.39		5-10	8.31		☐	1-4	20		5-10	0.52
5	☐	1-5	10.39		6-10	8.31		☐	1-5	20		6-10	0.52
6	☐	1-6	10.39		7-10	8.31		☐	1-6	20		7-10	0.52
7	☐	1-7	10.39		8-10	8.31		☐	1-7	20		8-10	0.52
8	☐	1-8	10.39		9-10	8.31		☐	1-8	20		9-10	0.52
9	☐	1-9	10.39		10	8.31		☐	1-9	20		10	0.52
10	☐	1-10	10.39		-	8.31		☐	1-10	20		-	0.52

Decision	Option A							Option B					
		If the die reads	you receive	and	If the die reads	you receive			If the die reads	you receive	and	If the die reads	you receive
11	☐	1	13.89		2-10	5.56		☐	1	25		2-10	0.28
12	☐	1-2	13.89		3-10	5.56		☐	1-2	25		3-10	0.28
13	☐	1-3	13.89		4-10	5.56		☐	1-3	25		4-10	0.28
14	☐	1-4	13.89		5-10	5.56		☐	1-4	25		5-10	0.28
15	☐	1-5	13.89		6-10	5.56		☐	1-5	25		6-10	0.28
16	☐	1-6	13.89		7-10	5.56		☐	1-6	25		7-10	0.28
17	☐	1-7	13.89		8-10	5.56		☐	1-7	25		8-10	0.28
18	☐	1-8	13.89		9-10	5.56		☐	1-8	25		9-10	0.28
19	☐	1-9	13.89		10	5.56		☐	1-9	25		10	0.28
20	☐	1-10	13.89		-	5.56		☐	1-10	25		-	0.28

1.7 Acknowledgement

Professor James Andreoni is a co-author on this work and it has been prepared for publication.

Chapter 2

Risk Preferences Are Not Time Preferences

Abstract

Risk and time are intertwined. The present is known while the future is inherently risky. This observation problematizes the study of time preferences as non-expected utility models of risk preferences can generate behavior that is observationally equivalent to hyperbolic time discounting. In risky intertemporal experiments we document robust violations of discounted expected utility. Importantly, these violations are further inconsistent with leading non-expected utility models such as prospect theory and models with preferences for the resolution of uncertainty. Our results have potentially important implications for understanding dynamically inconsistent preferences.

2.1 Introduction

Research on decision making under uncertainty has a long tradition. A core of tools designed to explore risky decisions has evolved, leading to the expected utility (EU) framework.[1] There are, however, a number of well-documented departures from EU such as the Allais (1953b) common consequence and common ratio paradoxes.

[1] Ellingsen (1994) provides a thorough history of the developments building towards expected utility theory and its cardinal representation.

An organizing principle behind the body of violations of expected utility 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.

Certainty and uncertainty are combined in intertemporal decisions. The present is known and certain, while the future is inherently risky, and the far future may be riskier still. This observation problematizes the study of pure time preference. Behaviors identified as dynamically inconsistent time preferences, such as diminishing impatience, may instead be generated by non-EU boundary effects.[2]

The discounted expected utility (DEU) model is the standard approach to addressing risky intertemporal decision-making. Interestingly, there are relatively few noted violations of the expected utility aspect of the DEU model.[3] An implication of the DEU model is that intertemporal allocations should depend *only* on relative intertemporal risk. For example, if sooner consumption will be realized 50% of the time and later consumption will be realized 50% of the time, intertemporal allocations should be identical to a situation where all consumption is risk-free. This is an intertemporal statement of the common ratio property of expected utility, and can be further applied to ecologically relevant situations where present rewards are certain and future rewards are risky.

In an experiment with 80 undergraduate subjects at the University of California, San Diego, we test intertemporal common ratio predictions using Convex Time Budgets (CTBs) under varying risk conditions (Andreoni and Sprenger, 2009). In CTBs, individuals are asked to allocate a budget of experimental tokens to sooner and later payments. Unlike multiple price lists (Coller and Williams, 1999; Harrison

[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).

[3]Loewenstein and Thaler (1989) and Loewenstein and Prelec (1992) document a number of anomalies in the *discounting* aspect of discounted utility models. The only evidence of intertemporal violations of EU we are aware of are Baucells and Heukamp (2009) and Gneezy et al. (2006) who show that temporal delay can generate behavior akin to the classic common ratio effect and that the so-called 'uncertainty effect' is present for hypothetical intertemporal decisions, respectively.

et al., 2002), which require linear utility for identification of time preferences, CTBs allow both precise identification of utility parameters and tests of structural discounting assumptions (Andreoni and Sprenger, 2009; Gine et al., 2010).[4] Critical to any study of time preferences is the close control over and minimization of payment risk. This is, to our knowledge, the first incentivized study to systematically vary payment risk for intertemporal decisions with the CTB or any other experimental methodology.

We implement CTBs in two baseline risk conditions: 1) A risk-free condition where all payments, both sooner and later, will be paid 100% of the time; and 2) a risky condition where, independently, sooner and later payments will be paid only 50% of the time. All uncertainty was resolved immediately after the allocation decisions were made, for both sooner and later payments. Additionally, mechanisms were in place to guarantee delivery of experimental payments once such resolution was made. Under the standard DEU model, CTB allocations in the two conditions should yield identical choices. The pattern of results we find clearly violates DEU, and is further inconsistent with non-EU concepts such as probability weighting (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992; Tversky and Fox, 1995), temporally dependent probability weighting (Halevy, 2008), and resolution-timing preferences (Kreps and Porteus, 1978; Chew and Epstein, 1989; Epstein and Zin, 1989). We document substantial DEU violations at both the group and individual level. Indeed, 85% of subjects are found to violate common ratio predictions and do so in more than 80% of opportunities.

We examine four critical additional conditions with differential risk, but common ratios of probabilities. In the first such condition the sooner payment is paid 100% of the time while the later payment is paid only 80% of the time. This is compared to a common ratio counterpart where the sooner payment is paid 50% of the time while the later payment is paid only 40% of the time. We document substantial violations of common ratio predictions favoring the sooner 100% payment. We mirror this design with conditions where the later payment has the higher probability. There we document substantial violations of common ratio predictions favoring the *later* 100% payment. The data are organized systematically at both the group and

[4]Alternate strategies for accounting for utility function curvature are discussed in Frederick et al. (2002) and implemented in Andersen et al. (2008).

individual level. Subjects who violate common ratio in the baseline 100%-100% and 50%-50% conditions are more likely to violate in the four additional conditions.

Our results reject DEU, prospect theory, and resolution timing models when certainty is present. However, when certainty is not present behavior closely mirrors DEU predictions. Interestingly, this is close to the initial intuition for the Allais paradox. Allais (1953b, 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. Such an argument may help to explain the frequent experimental finding of present-biased preferences (Frederick et al., 2002). That is, certainty, not intrinsic temptation, may lead present payments to be disproportionately preferred. This view has been argued in prior explorations of present-bias and prospect theory (Halevy, 2008), and is implied in the recognized dynamic inconsistency of non-EU models (Green, 1987; Machina, 1989). However, as our results are inconsistent with prospect theory, they point to a different mechanism. Though elaboration of this mechanism 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]

The paper proceeds as follows: Section 4.2 presents a conceptual development, building to testable hypotheses of intertemporal decision making in risky and certain situations. Section 4.3 describes our experimental design. Section 4.4 presents results and Section 2.5 is a discussion and conclusion.

2.2 Conceptual Background

To motivate our experimental design, we briefly analyze decision problems for discounted expected utility, resolution timing preferences, and prospect theory. When utility is time separable and stationary, the standard DEU model is written,

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

[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.

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. Under 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 available 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. The formulations of Kreps and Porteus (1978) and Chew and Epstein (1989), and the primary classes discussed by Epstein and Zin (1989) will reduce to standard expected utility. 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).[6]

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') if $p_1/p_2 = p_1'/p_2'$.

A leading alternative to expected utility that may be relevant in intertemporal choice is prospect theory probability weighting (Kahneman and Tversky, 1979;

[6]Not all of the classes of recursive utility models discussed in 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.

Tversky and Kahneman, 1992). 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 $p_1 = p_2$ and $p_1' = p_2'$, and a common ratio of objective probabilities is held, $p_1/p_2 = p_1'/p_2'$, then $\pi(p_1)/\pi(p_2) = \pi(p_1')/\pi(p_2') = 1$ 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.

A discussed 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 some temporally dependent function $g(p, t) : [0, 1] \times \Re^+ \to [0, 1]$ where t represents the time at which payments will be made. Provided freedom to pick the 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.[7]

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).

[7]Halevy (2008) gives the example of $g(p, t) = g(p^t)$ with $g(0) = 0; g(1) = 1$.

2.3 Experimental Design

In order to explore the development of Section 4.2 related to uncertain and certain intertemporal consumption, an experiment using Convex Time Budgets (CTB) (Andreoni and Sprenger, 2009) 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 $t + k$, $k > 0$.[8] 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.

2.3.1 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.[9] 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

[8] 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. Andreoni and Sprenger (2009) provide detailed discussion in this vein.

[9] 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).

percent, implying annual interest rates of between 0 and 2116.6 percent (compounded quarterly). Table 2.1 shows the token values, gross interest rates, standardized daily interest rates and corresponding annual interest rates for the basic CTB budgets.

Table 2.1: 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

The basic CTB decisions described above were implemented in a total of six risk conditions. Let p_1 and p_2 be the probabilities that payment would be made for the sooner and later payments, 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 at the end of the experiment to determine whether the payment would be sent or not. Hence, p_1 and p_2 were immediately known, independent, and subjects were told that different random numbers would determine their sooner and later payments.[10]

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 4.2, DEU, resolution timing 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% payment in the third condition. These conditions map to ecologically relevant decisions where sooner payments are certain and

[10]See Appendix 2.6.3 for the payment instructions provided to subjects.

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 resolution timing 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 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 4.2, subjects should respond to changes in relative risk, allocating smaller amounts to sooner payments when relative risk is low.

2.3.2 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 minimize payment risk once uncertainty was resolved and equate transaction costs over time.

Recruitment and Experimental Payments

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. We recruited 80 undergraduate students fitting this criterion.

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% delivery of mail, minimizing payment risk. Subjects were fully informed of the method of payment.[11]

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.[12] 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. This randomization device introduces a compound lottery to the decision environment, which does not change the common ratio predictions for DEU. However, the payment mechanism does add complication to the decision environment and eliminates experimental certainty. Subjects were told to treat each decision as if it were to determine their payments.[13] The results of Section 4.4, suggest that individuals do still treat 100% differently than other probabiltiies.

[11]See Appendix 2.6.2 for the information provided to subjects.

[12]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 $\$X$.

[13]See Appendix 2.6.3 for text.

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 4.2 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.

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!

Figure 2.1: Sample Decision Sheet

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.

2.4 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 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.

2.4.1 Behavior Under Certainty and Uncertainty

Section 4.2 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 3.2.1 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, resolution timing 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

[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 $(0.5, 0.5), (0.5, 0.4), (0.4, 0.5), (1, 1), (1, 0.8), (0.8, 1)$.

difference is found to be highly significant: $F_{14,79} = 6.07,\ p < .001$.[15]

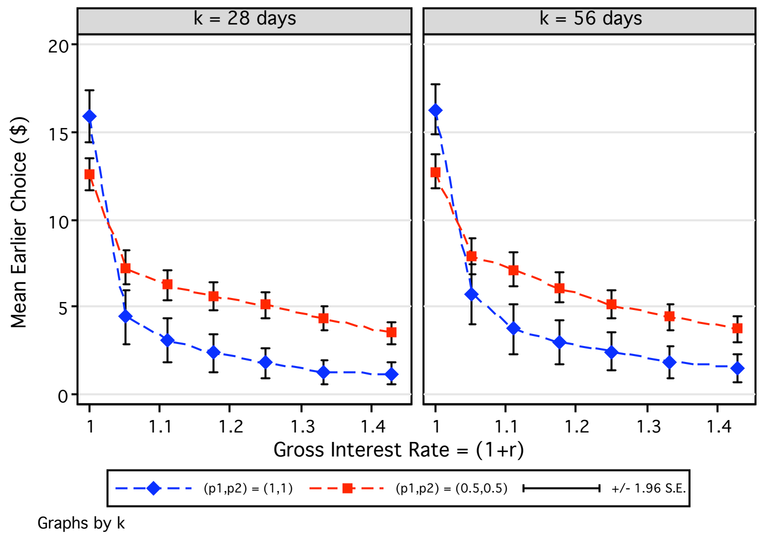

Figure 2.2: Behavior Under Certainty and Uncertainty

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% chance sooner payment would be sent *and* 50% chance later payment would be sent, in red. $t = 7$ *days* in all cases, $k \in \{28, 56\}$ *days*. Error bars represent 95% confidence intervals, taken as $+/- 1.96$ standard errors of the mean. Test of H_0 : Equality across conditions: $F_{14,79} = 6.07,\ p < .001$.

The data follow an interesting pattern. Behavior in both $(p_1, p_2) = (1, 1)$ and $(0.5, 0.5)$ conditions respect increasing interest rates. Allocations to sooner payments decrease as interest rates rise. At the lowest interest rate, c_t allocations are substantially higher in the $(1, 1)$ condition. However, 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 uncertainty is immediately resolved,

[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 2.3 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 (2009) 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.

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% of opportunities, an average of 11.38 ($s.d. = 3.99$) times. Figure 4.7, Panel A presents a histogram of $count_i$, each subject's number of violations across conditions $(p_1, p_2) = (1, 1)$ and

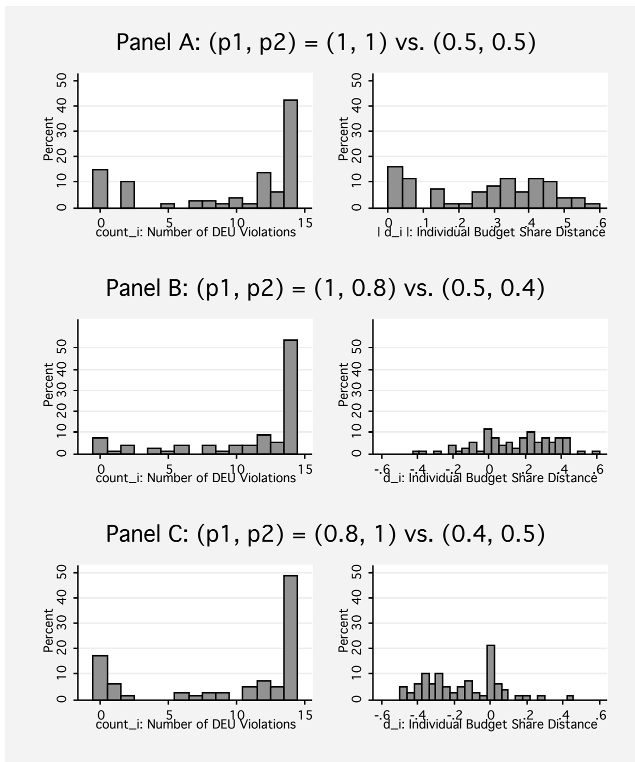

Figure 2.3: Individual Behavior Under Certainty and Uncertainty

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.

$(0.5, 0.5)$. More than 40% 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. As 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 absolute value as the difference may be positive and negative, following the aggregate results.[17] The mean value of $|d_i|$ is 0.27 ($s.d. = 0.18$), indicating that individual violations are substantial, around 27% of the budget share. Indeed 63.8% of the sample (51/80) exhibit $|d_i| > 0.2$, indicating that violations are not produced by simple random response error.

2.4.2 Behavior with Differential Risk

In this sub-section we explore behavior in four conditions with differential risk. First, we discuss violations in common ratio situations where only one payment is certain. Second, we examine our three experimental conditions where all payments are uncertain and document behavior consistent with discounted expected utility.

A Preference for Certainty

Figure 2.4.2 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 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]

[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. A disproportionate 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 $(p_1, p_2) = (0.4, 0.5)$.

[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),

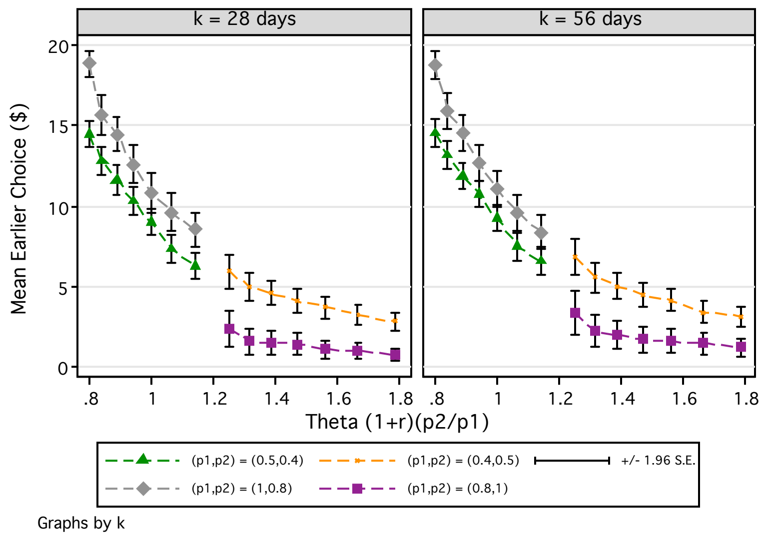

Figure 2.4: A Preference for Certainty

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% 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 $(p_1, p_2) = (0.4, 0.5)$: $F_{14,79} = 5.46$, $p < .001$.

Figure 4.7, 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% 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% 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 2.4.2 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. $(p_1, p_2) = (0.4, 0.5)$, ($\rho = -0.47,\ p < 0.01$). Table 2.2 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.

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 probability weighting (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992;

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 2.3 for regression.

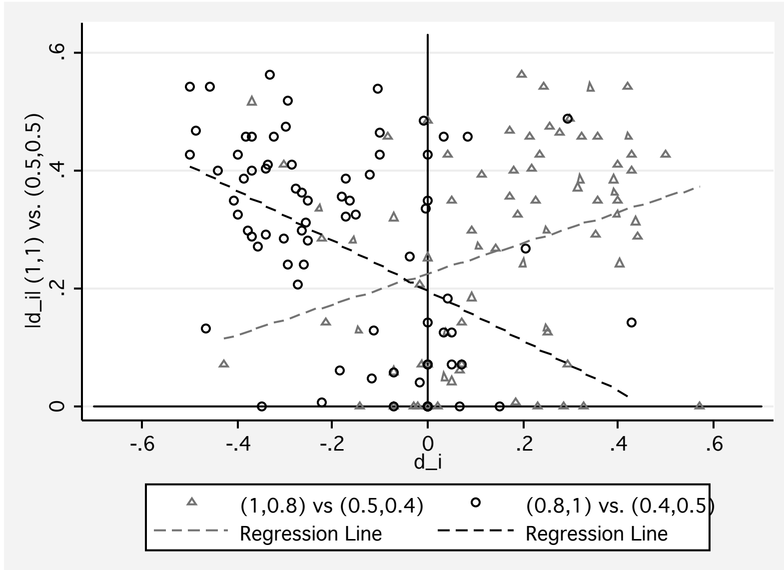

Figure 2.5: Violation Behavior Across Conditions

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 2.2 for more details.

Tversky and Fox, 1995; Wu and Gonzalez, 1996; Prelec, 1998; Gonzalez and Wu, 1999; Halevy, 2008). Recognizing that violations correlate highly across contexts that can and cannot be explained by such 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 building upon this design is required analyzing prospect theory predictions in atemporal choices before conclusions can be drawn as to the general validity of the model. This work is initiated in Andreoni and Sprenger (2010b).

Second, though the results suggest that prospect theory may not be the final account of dynamic inconsistency, certainty may play a critical role in generating such behavior. 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

Table 2.2: Individual Violation Correlation Table

		$count_i$ (1, 1) vs. (0.5, 0.5)	$count_i$ (1, 0.8) vs. (0.5, 0.4)	$count_i$ (0.8, 1) vs. (0.4, 0.5)	$\|d_i\|$ (1, 1) vs. (0.5, 0.5)	d_i (1, 0.8) vs. (0.5, 0.4)	d_i (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			
$\|d_i\|$	(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*: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

and individual level. 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.

When All Choices Are Uncertain

Figure 2.4.2 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 pre-

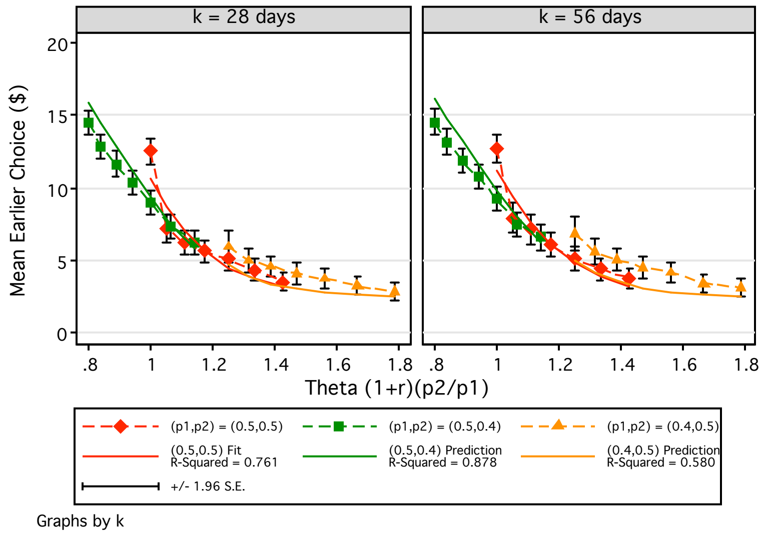

Figure 2.6: Aggregate Behavior Under Uncertainty

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% 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 2.4, column (6).

dictions 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 2.4.2. 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 is generally higher for lower gross interest rates.[21] Second, out of sample

[19] Appendix 4.4.2 describes the estimation procedure, the methodology for which was developed in Andreoni and Sprenger (2009). Appendix Table 2.4, 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% 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 (2009), where payment risk was minimized and no experimental variation of risk was implemented.

[20] As discussed in Section 4.2, 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.

[21] This pattern of allocations is obtained for all sessions and for all orders indicating no presence

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 2.4.2 demonstrates that in situations where all payments are risky, the results are 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]

2.5 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 relative to common ratio when it is available, but behave approximately as discounted expected utility maximizers away from certainty.

Our results have substantial implications for intertemporal decision theory. In 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

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 2.6.1) 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%.

[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).

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), 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, 1953b). Allais (1953b, 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) and may help researchers to understand how and why present bias and other discounting phenomena are manifested.

2.6 Appendix

2.6.1 Appendix Tables

Table 2.3: 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.

Estimating Preference Parameters

In this appendix we discuss with structural estimation of intertemporal preference parameters. Given structural assumptions, the design allows us to estimate utility parameters, following methodology developed in Andreoni and Sprenger (2009). 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

$$c_t^*(\theta, t, k, 1+r, m) = \frac{[1 - (\theta\delta^k)^{\frac{1}{\alpha-1}}]}{[1 + (1+r)(\theta\delta^k)^{\frac{1}{\alpha-1}}]}\omega + \frac{[(\theta\delta^k)^{\frac{1}{\alpha-1}}]}{[1 + (1+r)(\theta\delta^k)^{\frac{1}{\alpha-1}}]}m. \tag{2.1}$$

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 2.4 presents discounting and curvature parameters estimated from the

[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 (2009) 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 (2009) who show that accounting for censoring issues has little influence on estimates.

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

Table 2.4: 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)
$\text{Rate}_{(1,1)}$		0.281	0.276		0.282	
		(0.036)	(0.039)		(0.036)	
$\text{Rate}_{(0.5,0.5)}$		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% per year ($F_{1,79} = 0.69,\ p = 0.41$).[25] In the certain condition, $(p_1, p_2) = (1, 1)$, we

[25]For comparison, using similar methodology without uncertainty Andreoni and Sprenger (2009)

find almost linear utility while in the uncertain condition, $(p_1, p_2) = (0.5, 0.5)$, we estimate 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 2.4, 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 2.6.1 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 2.6.1 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

find aggregate discount rate between 25-35% 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.

for the $(p_1, p_2) = (0.5, 0.5)$ data, the R^2 value reduces to 0.629.

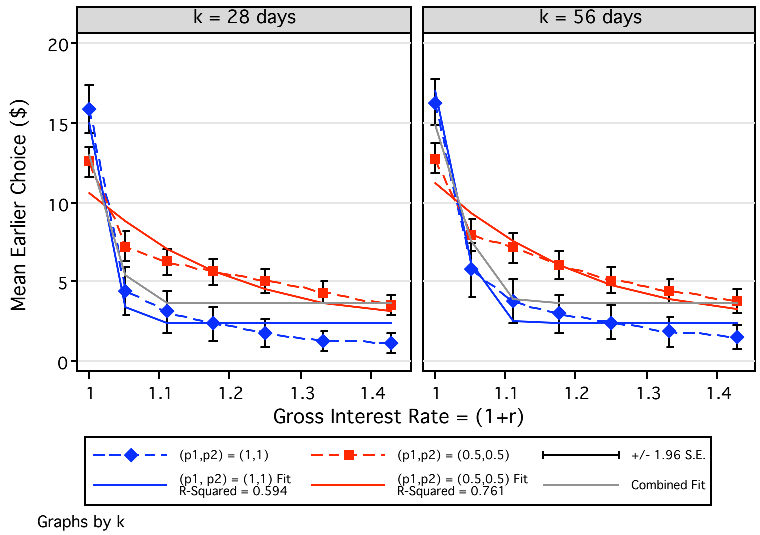

Figure 2.7: Behavior Under Certainty and Uncertainty

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% chance sooner payment would be sent *and* 50% chance later payment would be sent, in red. $t = 7$ *days* in all cases, $k \in \{28, 56\}$ *days*. Error bars represent 95% confidence intervals, taken as $+/-1.96$ standard errors of the mean. Test of H_0 : Equality across conditions: $F_{14,79} = 6.07$, $p < .001$.

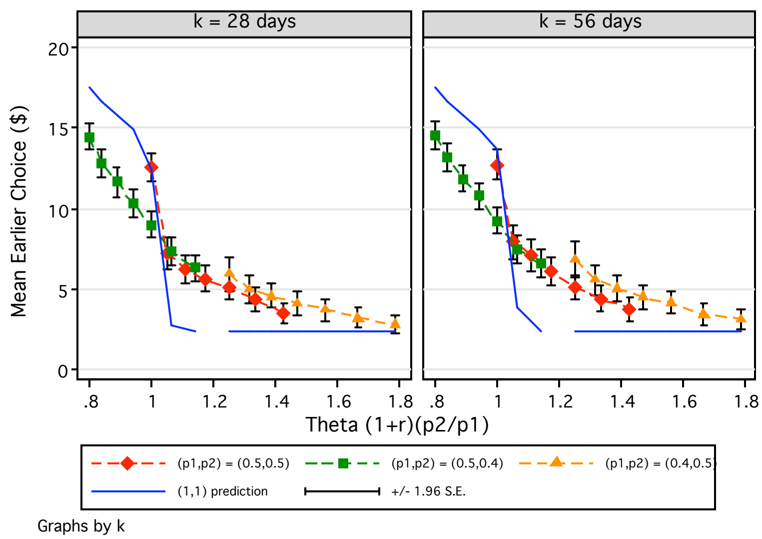

Figure 2.8: Behavior Under Uncertainty with Predictions Based on Certainty

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% 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 2.4, column (6).

2.6.2 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.

2.6.3 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.

2.7 Acknowledgement

Professor James Andreoni is a co-author on this work and it has been prepared for publication.

Chapter 3

Uncertainty Equivalents: Testing the Limits of the Independence Axiom

Abstract

We show that a novel experimental device, the uncertainty equivalent, provides a direct test of linearity-in-probability for decision-making under objective uncertainty. In a within-subject experiment with both uncertainty and certainty equivalents we demonstrate that the expected utility model performs remarkably well away from certainty, but breaks down near certainty. In particular, violations of both the independence axiom and stochastic dominance are obtained at probability one. This indicates that individuals may have a disproportionate preference for certainty, as assumed in models of disappointment aversion or u-v preferences, and is notably inconsistent with standard notions of prospect theory probability weighting. We unify these results by showing that a preference for certainty means that certainty equivalents will lead to specification error when used to estimate preferences for risk alone. Using certainty equivalents we reproduce the misspecification, leading to S-shaped probability weighting and, moreover, show that the error is largely driven by subjects with the strongest preferences for certainty in uncertainty equivalent tasks.

3.1 Introduction

The theory of Expected Utility (EU) is among the most elegant and esthetically pleasing results in all of economics. It shows that if a preference ordering over a given set of gambles is complete, transitive, continuous, and, in addition, it satisfies the independence axiom, then utility is linear in objective probabilities.[1] The idea that a gamble's utility *could* be represented by the mathematical expectation of its utility outcomes dates to the St. Petersburg Paradox (Bernouilli, 1738). The idea that a gamble's utility was necessarily such an expectation if independence and the other axioms were satisfied became clear only in the 1950's (Samuelson, 1952, 1953).[2]

Two parallel research tracks have developed in the study of decision-making under uncertainty with respect to the independence axiom. The first has taken linearity-in-probability as given and attempted to measure attitudes towards uncertainty using experimental methods. Subjects are asked to choose between gambles (Holt and Laury, 2002) or provide certainty equivalents for gambles (Birnbaum, 1992; Kachelmeier and Shehata, 1992). Using the EU formulation and functional form assumptions for utility, such as constant relative risk aversion, preference parameters are calculated or estimated. Harrison and Rutstrom (2008) provide a detailed summary of both the experimental methods and estimation exercises associated with this literature.

The second track has focused on identifying violations of independence.[3] Principal among these violations are the common consequence and common ratio paradoxes initially documented by Allais (1953b) and frequently reproduced in laboratory

[1]Subjective Expected Utility is not discussed in this paper. All results will pertain only to objective probabilities.

[2]The independence axiom is closely related to the Savage (1954) 'sure-thing principle' for subjective expected utility (Samuelson, 1952). Expected utility became known as von Neumann-Morgenstern (vNM) preferences after the publication of von Neumann and Morgenstern (1944). Independence, however, was not among the discussed axioms, but rather implicitly assumed. Samuelson (1952, 1953) discusses the resulting confusion and his suspicion of an implicit assumption of independence in the vNM treatment. Samuelson's suspicion was then confirmed in a note by Malinvaud (1952). For an excellent discussion of the history of the independence axiom, see Fishburn and Wakker (1995).

[3]This second line of research began contemporaneously with the recognition of the importance of the independence axiom. Indeed Allais' presentation of Allais (1953a) was in the same session as Samuelson's presentation of Samuelson (1953) and the day after Savage's presentation of Savage (1953) at the Colloque Internationale d'Econométrie in Paris in May of 1952.

studies (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992).

There is now an extensive catalogue of similar violations of EU (Camerer, 1992; Harless and Camerer, 1994; Starmer, 2000). These violations are used to motivate new theoretical and experimental exercises. The most important associated development is Cumulative Prospect Theory's (CPT) inverted S-shaped non-linear probability weighting (Kahneman and Tversky, 1979; Quiggin, 1982; Tversky and Kahneman, 1992; Tversky and Fox, 1995). In a series of experiments eliciting certainty equivalents for gambles, Tversky and Kahneman (1992) and Tversky and Fox (1995) estimate utility parameters demonstrating that high probabilities are down-weighted and low probabilities are up-weighted. The probability weighting phenomenon has become an established feature in decision research. Identifying the general S-shape of the weighting function and determining its parameter values has received significant attention both theoretically and in experiments (Wu and Gonzalez, 1996; Prelec, 1998; Gonzalez and Wu, 1999; Abdellaoui, 2000). Figure 3.1 illustrates the general observation of down-weighting of high probabilities and up-weighting of low probabilities. Based upon the strength of these findings researchers have developed new methodology for eliciting risk preferences such as the 'trade-off' method (Wakker and Deneffe, 1996) that is robust to non-linear probability weighting.

Interestingly, there are few direct tests of the independence axiom's most critical implication: linearity-in-probabilities of the expected utility function. Experimental tests of probability distortion such as Tversky and Kahneman (1992) and Tversky and Fox (1995) are not separate from functional form assumptions.[4] Furthermore, if the independence axiom is assumed for identification of utility parameters (Holt and Laury, 2002) or if elicitation methodology is designed to difference out probability weights (Wakker and Deneffe, 1996; Booij and van de Kuilen, 2009), then the axiom is untestable. This is not to say that one cannot test EU via violations (Allais, 1953b), calibrational arguments (Rabin, 2000a,b), or goodness-of-fit comparisons (Camerer, 1992; Hey and Orme, 1994; Harless and Camerer, 1994). These tests clearly demonstrate that the independence axiom can fail to hold, but the conclusions do not speak

[4] This observation is made by Abdellaoui (2000). Notable exceptions are the non-parametric probability weighting estimates of Gonzalez and Wu (1999); Bleichrodt and Pinto (2000) and Abdellaoui (2000) which find support for non-linearity-in-probabilities (see sub-section 3.4.2 for discussion).

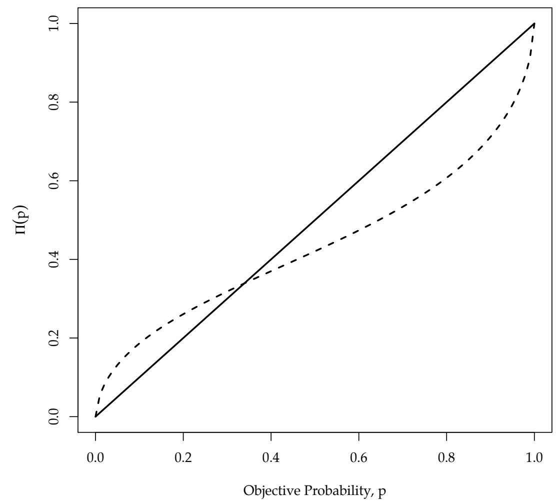

Figure 3.1: Standard Probability Weighting

Note: The general S-shaped probability weighting finding is illustrated of up-weighting of low probabilities and down-weighting of high probabilities. The plotted function is $\pi(p) = p^\gamma/(p^\gamma + (1-p)^\gamma)^{1/\gamma}$ with $\gamma = 0.61$ as found by Tversky and Kahneman (1992).

directly to the often-suggested alternative interpretation of non-linearity in probabilities.

In this paper, we provide a direct test of linearity-in-probabilities that uncovers when independence holds, how it fails, and the nature of violations. We reintroduce an experimental method, which we call the *uncertainty equivalent*. Whereas a certainty equivalent identifies the certain amount that generates indifference to a given gamble, the uncertainty equivalent identifies the probability mixture over the gamble's best outcome and zero that generates indifference. For example, consider a $(p, 1-p)$ gamble over \$10 and \$30, $(p; 10, 30)$. The uncertainty equivalent identifies the $(q, 1-q)$ gamble over \$30 and \$0, $(q; 30, 0)$, that generates indifference.[5] Independence implies a linear relationship between p and q.

The uncertainty equivalent draws its motivation from the initial proofs of

[5]We recognize that it is a slight abuse of traditional notation to have the probability refer to the lower outcome in the given gamble and the higher outcome in the uncertainty equivalent. It does, however, ease explication to have p refer to the probability of the low value and q refer to the probability of the high value.

expected utility, where the cardinal index for a gamble is derived as the probability mixture over the best and worst options in the space of gambles. Such derivations are provided in most textbook treatments of expected utility.[6]

The uncertainty equivalent can also be used to inform the discussion of a variety of non-EU preference models including S-shaped probability weighting, expectations based reference-dependence such as disappointment aversion (Bell, 1985; Loomes and Sugden, 1986; Gul, 1991; Koszegi and Rabin, 2006, 2007)[7], and 'u-v' preferences (Neilson, 1992; Schmidt, 1998; Diecidue et al., 2004).[8] Though these models are often motivated by violations of independence and the Allais (1953b) paradox, they have both divergent psychological accounts of the phenomenon and divergent predictions in the uncertainty equivalent environment. Probability weighting explains the Allais paradox with non-linear probability distortions, disappointment aversion relies instead on reference-dependence around an expectations-based reference point, and 'u-v' preferences rely on a direct preference for certainty. Importantly, in the uncertainty equivalent environment these different models of preferences have different predictions as to the relationship between gambles and their uncertainty equivalents, and these predictions can generally be examined without relying on functional form assumptions for utility.

We conducted a within-subject experiment with 76 undergraduates at the University of California, San Diego, using both uncertainty equivalents and standard certainty equivalents. We demonstrate four important results. First, using uncertainty equivalents the independence axiom performs well away from certainty, where proba-

[6]See, e.g. Varian (1992). Our research has uncovered that methods like our uncertainty equivalent were discussed in Farquhar's (1984) excellent survey of utility assessment methods and, to our knowledge, were implemented experimentally in only one study of nine subjects using hypothetical monetary rewards (McCord and de Neufville, 1986), and a number of medical questionnaires (Magat et al., 1996; Oliver, 2005, 2007; Bleichrodt et al., 2007).

[7]We include the Koszegi and Rabin (2006, 2007) model in the broad class of expectations-based reference dependence as the model's predictions will closely resemble those of standard disappointment aversion in the present context as well as most other experimental environments (Ericson and Fuster, 2009; Gill and Prowse, 2010; Abeler et al., Forthcoming). For specific evidence distinguishing Koszegi and Rabin (2006, 2007) preferences from disappointment aversion, see Sprenger (2010).

[8]'u-v' preferences are less well-known than the other preference models. For a discussion of the early history of u-v preferences, see Schoemaker (1982). These models capture the intuition of Allais (1953b) that when options are far from certain, individuals act effectively as EU maximizers but, when certainty is available, it is disproportionately preferred. The u-v model differs in important ways from extreme or even discontinuous probability weighting and prior experiments have demonstrated these differences (Andreoni and Sprenger, 2010a).

bilities are found to be weighted approximately linearly. Second, linearity breaks down as probabilities approach 1. The nature of the violation is contrary to S-shaped probability weighting. Third, we document that 38 percent of subjects violate stochastic dominance at certainty, providing a within-subject example of the recently debated ‘uncertainty effect’ (Gneezy et al., 2006; Rydval et al., 2009; Keren and Willemsen, 2008; Simonsohn, 2009). Such violations are a prediction of both the u-v model and some formulations of disappointment aversion, and are indicative of a disproportionate preference for certainty. Fourth, in the certainty equivalents experiments, subjects show both small stakes risk aversion and apparent S-shaped probability weighting, reproducing prior findings. These phenomena are largely driven by subjects who display a disproportionate preference for certainty by violating stochastic dominance in uncertainty equivalents. This suggests that extreme experimental risk aversion and probability weighting may be artifacts of a disproportionate preference for certainty in traditional experimental methodology.

Our findings have critical implications for research on risk attitudes and have applications to a variety of economic problems. The results demonstrate that experimental measures of risk attitudes and EU violations are dramatically influenced by the presence of certainty. In uncertainty equivalents we find no support for S-shaped probability weighting, but rather evidence for a disproportionate preference for certainty. Conversely, in standard certainty equivalents these same subjects exhibit S-shaped probability distortions. Additionally, the disproportionate preference for certainty has predictive power for the extent of apparent probability weighting. We put these findings in context by noting that certainty has long been known to play a special role in decision making. The original Allais (1953b) paradoxes drew attention to certainty being disproportionately preferred. And, violations of EU are documented predominantly when certainty is involved (Conlisk, 1989; Camerer, 1992; Harless and Camerer, 1994; Starmer, 2000). Recognizing that certainty may be disproportionately preferred gives a reason, perhaps, to expect apparent non-EU behavior in certainty-based analyses: certainty effects are built into the experimental design. This further suggests that future empirical and theoretical work should take specific preferences for certainty into account when modeling decision-making.

The paper continues as follows. Section 3.2 discusses the uncertainty equivalent methodology and develops empirical predictions based on different preference models. Section 3 presents experimental design details. Section 4 presents results and Section 5 concludes.

3.2 The Uncertainty Equivalent

Consider a lottery $(p; X, Y)$ which provides \$$X$ with probability p and \$$Y >$ \$$X$ with probability $1-p$. A certainty equivalent task elicits the certain amount, \$$C$, that is indifferent to this gamble. The uncertainty equivalent elicits the q-gamble over \$Y and \$0, $(q; Y, 0)$, that is indifferent to this gamble. Take for example a 50%-50% gamble paying either \$10 or \$30. The uncertainty equivalent is the q-gamble over \$30 and \$0 that generates indifference.

Under standard preference models, a more risk averse individual will, for a given gamble, have a lower certainty equivalent, C, and a higher uncertainty equivalent, q. A powerful distinction of the uncertainty equivalent is, however, that it is well-suited to identifying alternative preference models such as S-shaped probability weighting (where the non-linearity of the weighting function can be recovered directly), disappointment aversion and u-v preferences. If preferences under certainty differ from those under uncertainty as in both disappointment aversion and u-v models, then certainty equivalent methodology assuming a single utility function is misspecified.[9] Risk preferences or probability weights are not identified separately from differential preferences over certainty and uncertainty. Figure 3.2 demonstrates the difficulty with certainty equivalents relative to uncertainty equivalents in the case of u-v preferences where $v(\cdot)$ with certainty differs from $u(\cdot)$ with uncertainty.

3.2.1 Empirical Predictions

We present empirical predictions in the uncertainty equivalent environment for expected utility, S-shaped probability weighting, disappointment aversion, and u-v

[9]In the u-v model, the differential preferences over certainty and uncertainty are delivered via a discontinuity. In disappointment aversion, choice under uncertainty involves disappointment and so an extra utility parameter not present in choice under certainty. See sub-sections 3.2.1 and 3.2.1 for further discussion.

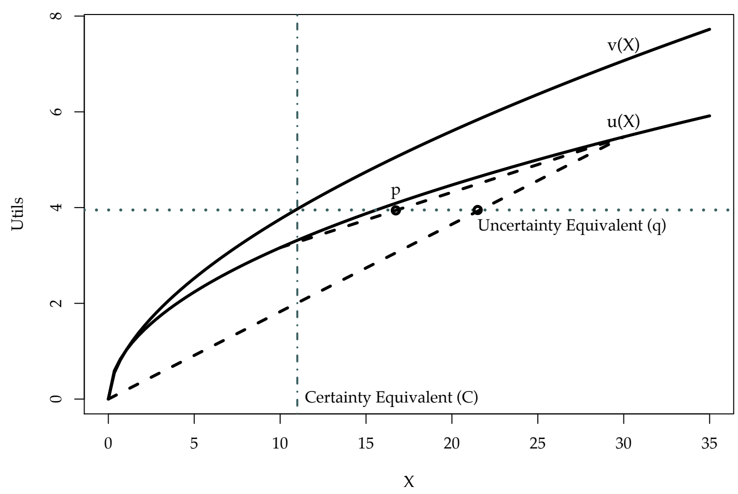

Figure 3.2: The Uncertainty Equivalent

Note: Illustration of uncertainty equivalent, $(q; 30, 0)$, for the lottery $(p; 10, 30)$. Certainty equivalent, C, demonstrated for u-v preferences with a disproportionate preference for certainty.

preferences.[10] Unlike experimental contexts that require functional form assumptions for model identification, the uncertainty equivalent can generally provide tests of utility specifications based on the relationship between p and q without appeal to specific functional form for utility.[11]

Expected Utility

Expected utility's independence axiom makes a critical prediction of behavior in the uncertainty equivalent. Take a p chance of $\$X$ and a $1-p$ chance of a larger payment $\$Y > \$X$. The uncertainty equivalent of this prospect is the value q

[10]This is, of course, a limited list of the set of potentially testable decision models. For example, we do not discuss ambiguity aversion or the anticipatory utility specifications of Kreps and Porteus (1978) and Epstein and Zin (1989) as experimental uncertainty was resolved directly at the end of the experimental sessions. These specifications generally reduce to expected utility when uncertainty is resolved immediately.

[11]One environment where this is not the case is our discussion of disappointment aversion as probabilities enter directly into the utility function in the formation of the referent. See sub-section 3.2.1 for detail.

satisfying

$$p \cdot u(X) + (1-p) \cdot u(Y) = q \cdot u(Y) + (1-q) \cdot u(0).$$

Assuming u(0) = 0, $u(Y) > u(X)$, and letting $\theta = u(X)/u(Y) < 1$, then

$$q = p \cdot \frac{u(X)}{u(Y)} + 1 - p = 1 - p \cdot (1-\theta),$$

and

$$\frac{dq}{dp} = \frac{u(X)}{u(Y)} - 1 = -(1-\theta) < 0.$$

Thus, expected utility generates a negative *linear* relationship between the probability p of $\$X$ and the probability q of $\$Y$. This is an easily testable prediction.

Cumulative Prospect Theory Probability Weighting

Under Cumulative Prospect Theory, probabilities are weighted by the non-linear function $\pi(p)$. One popular functional form is the one parameter function used in Tversky and Kahneman (1992)[12], $\pi(p) = p^{\gamma}/(p^{\gamma} + (1-p)^{\gamma})^{1/\gamma}$, $0 < \gamma < 1$. This inverted S-shaped function, as with others used in the literature, has the property that $\pi'(p)$ approaches infinity as p approaches 0 or 1. Probability weights are imposed on the higher of the two utility values.[13]

Under this CPT formulation, the uncertainty equivalent indifference condition is

$$(1 - \pi(1-p)) \cdot u(X) + \pi(1-p) \cdot u(Y) = \pi(q) \cdot u(Y) + (1 - \pi(q)) \cdot u(0).$$

Again letting $u(0) = 0$ and $\theta = u(X)/u(Y) < 1$,

$$(1 - \pi(1-p)) \cdot \theta + \pi(1-p) = \pi(q).$$

[12] Tversky and Fox (1995) and Gonzalez and Wu (1999) employ a similar two parameter $\pi(p)$ function. See Prelec (1998) for alternative specifications.

[13] This formulation is assumed for binary gambles over strictly positive outcomes in Kahneman and Tversky (1979) and for all gambles in Tversky and Kahneman (1992). We abstract away from prospect theory's fixed reference point formulation as static reference points do not alter the analysis. Changing reference points, as in disappointment aversion, are discussed in sub-section 3.2.1.

This implicitly defines q as a function of p, yielding

$$\frac{dq}{dp} = -\frac{\pi'(1-p)}{\pi'(q)} \cdot [1-\theta] < 0.$$

As with expected utility, q and p are negatively related. Contrary to expected utility, the rate of change, dq/dp, depends on both p and q. Importantly, as p approaches 1, $\pi'(1-p)$ approaches infinity and, provided finite $\pi'(q)$, the slope dq/dp becomes increasingly negative.[14] This result presents a clearly testable alternative to expected utility. The argument need not rest on the derivatives of the probability weighting function. Any modified S-shaped weighting function featuring up-weighting of low probabilities and down-weighting of high probabilities will share the characteristic that the relationship between q and p will become more negative as p approaches 1.[15] Comparing gambles to their uncertainty equivalents is an ideal way to test for S-shaped probability weighting as the non-linearity of the weighting function can be measured directly.

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[14] It is difficult to create a general statement for concavity based upon the second derivative d^2q/dp^2, as the second derivatives of the weighting function can be positive or negative depending on the concavity or convexity of the S-shaped distortion. The second derivative is

$$\frac{d^2q}{dp^2} = \frac{\pi''(1-p)\cdot[1-\theta]\cdot\pi'(q) + \pi''(q)\frac{dq}{dp}\cdot\pi'(1-p)\cdot[1-\theta]}{\pi'(q)^2}.$$

For p near 1, the sign is partially determined by $\pi''(q)$ which may be negative or positive. For S-shaped weighting, $\pi''(1-p)$ will be negative in the concave region of low probabilities, and dq/dp will be negative from the development above. If q lies in the convex weighting region, such that $\pi''(q) > 0$, then $d^2q/dp^2 < 0$ and the relationship is concave and may remain so with $\pi''(q) < 0$. Consensus puts the concave region between probability 0 and around 1/3 (Tversky and Kahneman, 1992; Tversky and Fox, 1995; Prelec, 1998). As will be seen, the uncertainty equivalents for $p = 1$ lie substantially above 1/3 for all of our experimental conditions such that a concave relationship between p and q would be expected.

[15] This would be the case for virtually all functional forms and parameter values discussed in Prelec (1998) and for functions respecting condition (A) of the Quiggin (1982) weighting function. Take p close to 1 and $(1-p)$ close to zero, $u(Y)$ will be up-weighted and $u(X)$ will be down-weighted on the left hand side of the above indifference condition. In order to compensate for the up-weighting of the good outcome on the left hand side, q on the right hand side must be high. At $p = 1$, the up-weighting of $u(Y)$ disappears precipitously and so q decreases precipitously to maintain indifference.

Disappointment Aversion

Disappointment aversion refers to a general class of reference-dependent models with expectations-based reference points. In disappointment aversion, a gamble's outcomes are evaluated relative to the gamble's EU certainty equivalent (Bell, 1985; Loomes and Sugden, 1986; Gul, 1991). Recent research on expectations-based reference dependence extends the notion of reference points to reference distributions (Koszegi and Rabin, 2006, 2007). In the environment described in this paper, models with reference distributions and models with reference points generate very similar predictions. For simplicity, we present the analysis in terms of expectations-based reference points.[16]

Consider a p chance of $\$X$ and a $1-p$ chance of a larger payment $\$Y > \$X$. The EU certainty equivalent of this prospect is the value, C_p, satisfying

$$p \cdot u(X) + (1-p) \cdot u(Y) = u(C_p).$$

Taking C_p as the reference point, the reference-dependent utility of the p-gamble is

$$U_p = p \cdot \tilde{u}(X|C_p) + (1-p) \cdot \tilde{u}(Y|C_p)$$

where $\tilde{u}(\cdot|C_p)$ is the reference-dependent utility function with a reference point at C_p.

We assume a standard specification for $u(\cdot|C_p)$ (Bell, 1985; Loomes and Sugden, 1986),

$$\tilde{u}(z|C_p) = u(z) + \mu(u(z) - u(C_p)),$$

where the function $u(z)$ represents consumption utility for some outcome, z, and $\mu(\cdot)$ represents disappointment-elation utility relative to the referent, C_p. Several simplifying assumptions are made. Following Koszegi and Rabin (2006, 2007) we

[16] For analysis focusing on the distinction between Koszegi and Rabin (2006, 2007) preferences and other models of disappointment aversion as well as discussion of the different equilibrium and utility maximization concepts across the models, see Sprenger (2010).

assume a piecewise-linear disappointment-elation function,

$$\mu(u(z)-u(C_p)) = \left\{ \begin{array}{lll} \eta\cdot(u(z)-u(C_p)) & if & u(z)-u(C_p)\geq 0 \\ \eta\cdot\lambda\cdot(u(z)-u(C_p)) & if & u(z)-u(C_p)<0 \end{array} \right\},$$

where the utility parameter with $\lambda > 1$ indicates disappointment aversion. For simplicity and to aid the exposition, $\eta = 1$ is assumed. Under these specifications, the utility of the $(p; X, Y)$ gamble can be written as

$$U_p = p\cdot[u(X)+\lambda\cdot(u(X)-u(C_p))] + (1-p)\cdot[u(Y)+1\cdot(u(Y)-u(C_p))].$$

Replacing $u(C_p) = p\cdot u(X) + (1-p)\cdot u(Y)$, this becomes

$$U_p = [p + p\cdot(1-p)\cdot(\lambda-1)]\cdot u(X) + [(1-p) - p\cdot(1-p)\cdot(\lambda-1)]\cdot u(Y).$$

Note that this implies that disappointment aversion is another version of a probability weighted utility function with weighting function

$$\tilde{\pi}(1-p) = (1-p) - p\cdot(1-p)\cdot(\lambda-1).$$

In addition $\tilde{\pi}(1-p) \leq 1-p$ if $\lambda > 1$, and $\tilde{\pi}(1-p)$ is a convex function.[17] Hence disappointment aversion is equivalent to a specific form of probability weighting that is not S-shaped, but rather downweights all probabilities, between 0 and 1. Loomes and Sugden (1986) and Gul (1991) provide similar demonstrations that disappointment aversion is observationally equivalent to down-weighting of all probabilities. As such, disappointment aversion will not generate the same relationship between given $(p; X, Y)$ gambles and their uncertainty equivalents, $(q; Y, 0)$, as predicted by S-shaped probability weighting. Instead of a concave shape, the relationship between p and q will be predicted to be convex.

Following identical logic to that of the previous section, setting up the same

[17] $\tilde{\pi}(1-p)$ describes a parabola with a critical point at $1-p = (\lambda-2)/(2\lambda-2)$.

uncertainty equivalent indifference relation and simplifying, we again find

$$\frac{dq}{dp} = -\frac{\tilde{\pi}'(1-p)}{\tilde{\pi}'(q)} \cdot [1-\theta] \,.$$

Because the weighting function, $\tilde{\pi}(\cdot)$, is convex, one can easily check that the second derivative, $\frac{d^2q}{dp^2}$, is greater than zero, implying that disappointment aversion predicts a convex relationship between p and q for $\lambda > 1$.[18]

Of additional interest is that as p approaches 1, $\tilde{\pi}'(1-p)$ approaches $2-\lambda$ under our formulation. For sufficiently disappointment averse individuals, $\lambda > 2$, the relationship between p and q, dq/dp, will become positive as p approaches 1, provided $\tilde{\pi}'(q) > 0$. This is an important prediction of disappointment aversion in the uncertainty equivalent environment. A positive relationship between p and q near certainty implies violations of first order stochastic dominance as certainty is approached. The uncertainty equivalent, q, acts as a utility index of the offered p-gamble. Given two offered gambles, p and p', with $p > p'$, and two associated

[18]Note that $\tilde{\pi}(q) \geq \tilde{\pi}(1-p)$ is implied from the above indifference condition, and, for a weakly increasing $\tilde{\pi}(\cdot)$, $q \geq 1-p$. Convexity implies $\tilde{\pi}'(q) \geq \tilde{\pi}'(1-p)$. For the employed specification $\tilde{\pi}'(1-p) = 2-\lambda+2(1-p)(\lambda-1)$ and $\tilde{\pi}''(\cdot)$ is a constant, such that $\tilde{\pi}''(1-p) = \tilde{\pi}''(q) = 2(\lambda-1)$. This second derivative is positive under the assumption $\lambda > 1$. Hence, the sign of

$$\frac{d^2q}{dp^2} = \frac{\tilde{\pi}''(1-p)\cdot[1-\theta]\cdot\tilde{\pi}'(q) + \tilde{\pi}''(q)\frac{dq}{dp}\cdot\tilde{\pi}'(1-p)\cdot[1-\theta]}{\tilde{\pi}'(q)^2}.$$

depends on the sign of

$$\tilde{\pi}'(q) + \frac{dq}{dp}\cdot\tilde{\pi}'(1-p).$$

Plugging in for dq/dp

$$\tilde{\pi}'(q) - \frac{\tilde{\pi}'(1-p)}{\tilde{\pi}'(q)}\cdot[1-\theta]\cdot\tilde{\pi}'(1-p),$$

and dividing by $\tilde{\pi}'(q)$ we obtain

$$1 - \frac{\tilde{\pi}'(1-p)}{\tilde{\pi}'(q)}\cdot[1-\theta]\cdot\frac{\tilde{\pi}'(1-p)}{\tilde{\pi}'(q)}.$$

Because convexity of $\tilde{\pi}(\cdot)$ and $q \geq (1-p)$ implies $\tilde{\pi}'(q) \geq \tilde{\pi}'(1-p)$, $\tilde{\pi}'(1-p)/\tilde{\pi}'(q) \leq 1$. Additionally $1-\theta < 1$, by the assumption of monotonicity. The second term is therefore a multiplication of three terms that are less than or equal to 1 and one concludes

$$1 - \frac{\tilde{\pi}'(1-p)}{\tilde{\pi}'(q)}\cdot[1-\theta]\cdot\frac{\tilde{\pi}'(1-p)}{\tilde{\pi}'(q)} > 0,$$

$d^2q/dp^2 > 0$, the relationship is convex.

uncertainty equivalents, q and q', a subject violates first order stochastic dominance if $q > q'$ as this indirectly reveals a preference for a gamble with higher probability of a lower prize.

A sufficiently disappointment averse individual will disproportionately prefer certainty as the specter of disappointment is eliminated at certainty. Hence certainty of a low outcome may be preferred to a near-certain dominating gamble with the possibility of disappointment.

Importantly, disappointment averse models are often constructed with assumptions guaranteeing that the underlying preferences satisfy stochastic dominance, taking violations of stochastic dominance as a disqualifying feature of a model of behavior (Loomes and Sugden, 1986; Gul, 1991). However, the models of Bell (1985) and (Koszegi and Rabin, 2006, 2007) do not feature such assumptions. Hence, testing for violations of stochastic dominance tests the constraints imposed by different approaches to expectations-based reference dependence.

u-*v* Preferences

The u-v model (Neilson, 1992; Schmidt, 1998; Diecidue et al., 2004) is designed to capture Allais' (1953b) intuition of a disproportionate preference for security in the 'neighborhood of certainty.' Let $u(X)$ be the utility of \$$X$ with uncertainty and $v(X)$ be the utility of \$$X$ with certainty. Assume $v(X) > u(X)$ for \$$X > 0$. Under such u-v preferences, p and q will have a linear relationship away from $p = 1$. At $p = 1$, the discontinuity in utility introduces a discontinuity in the relationship between p and q. At $p = 1$, the q that solves the indifference condition

$$v(X) = q \cdot u(Y)$$

will be

$$q = \frac{v(X)}{u(Y)} > \frac{u(X)}{u(Y)}.$$

With the u-v specification, q will be linearly decreasing in p and then discontinuously *increase* at $p = 1$.

Importantly, if the neighborhood of certainty is understood to begin at probabilities less than one, the discontinuity may appear below $p = 1$. Instead of a discontinuity at certainty, however, one could imagine a less rigid model of preferences where the relationship between p and q is continuous but non-linear as p approaches 1. Distinguishing between such a representation and disappointment aversion would be virtually impossible. In this sense, discontinuous u-v preferences could act as a simple, tractable representation of disappointment averse decision-making without making appeals to an expectations-based reference point. All uncertainty entails disappointment and so lower utility. This is similar in spirit to the β-δ representation of time preferences to approximate hyperbolic discounting.

Similar to some versions of disappointment aversion, the u-v preference model violates first order stochastic dominance if there exists a disproportionate preference for certainty, $v(X) > u(X)$. Certainty of a small payment will be preferred to near certain gambles paying this small amount with sufficiently high probability and something larger with low probability.

Though such a property is viewed as a weakness of the u-v preference model (Diecidue et al., 2004), we again take the view that violations of stochastic dominance are actually an implication of the model that can be easily tested in the uncertainty equivalent environment.

3.2.2 Summary

Figure 3.2.1 presents the theoretical predictions of the four discussed models of decision-making under uncertainty. Importantly, the uncertainty equivalent environment provides separation between the models. Under expected utility, q should be a linear function of p. Under S-shaped probability weighting q should be a concave function of p with the relationship growing more negative as p approaches 1. Under disappointment aversion q should be a convex function of p, perhaps with sharper convexity as p approaches 1, and with indirect violations of stochastic dominance. Under u-v preferences, q should be a linear function of p until certainty, with convexity appearing near certainty and being associated with indirect violations of stochastic dominance.

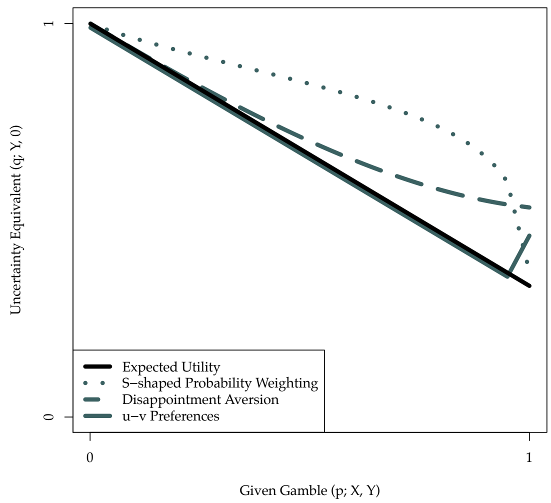

Figure 3.3: Empirical Predictions

Note: Empirical predictions of the relationship between given gambles, $(p; X, Y)$, and uncertainty equivalents $(q; Y, 0)$ for Expected Utility, S-shaped CPT probability weighting, disappointment aversion, and u-v preferences. A linear prediction is obtained for EU, a concave relationship for S-shaped CPT probability weighting, and a convex relationship for disappointment aversion. For u-v preferences a linear negative relationship between $(p; X, Y)$ and $(q; Y, 0)$ is obtained for $p < 1$, with a discontinuous increase in $(q; Y, 0)$ at certainty, $p = 1$.

3.3 Experimental Design

Eight uncertainty equivalents were implemented with probabilities $p \in \{0.05, 0.10, 0.25, 0.50, 0.75, 0.90, 0.95, 1\}$ in three different payment sets, $(X, Y) \in \{(10, 30), (30, 50), (10, 50)\}$, yielding 24 total uncertainty equivalents. The experiment was conducted with paper-and-pencil and each payment set (X, Y) was presented as a packet of 8 pages. The uncertainty equivalents were presented in increasing order from $p = 0.05$ to $p = 1$ in a single packet.

On each page, subjects were informed that they would be making a series of decisions between two options: Option A and Option B. Option A was a p chance of receiving $\$X$ and a $1 - p$ chance of receiving $\$Y$.[19] Option A remained the same for every decision on the page. Option B varied in steps from a 5 percent chance of

[19]All probabilities in the experiment were presented as a $p \times 100$ chance in 100.

receiving $\$Y$ and a 95 percent chance of receiving \$0 to a 99 percent chance of receiving $\$Y$ and a 1 percent chance of receiving \$0. A sample decision task is presented in Figure 3.3. In this price list style experiment, the row at which a subject switches from preferring Option A to Option B indicates the range of values within which the uncertainty equivalent, q, lies.

	Option A			*or*	Option B		
	Chance of \$10	Chance of \$30			Chance of \$0	Chance of \$30	
	50 in 100	50 in 100	☑	*or*	100 in 100	0 in 100	☐
1)	50 in 100	50 in 100	☐	*or*	95 in 100	5 in 100	☐
2)	50 in 100	50 in 100	☐	*or*	90 in 100	10 in 100	☐
3)	50 in 100	50 in 100	☐	*or*	85 in 100	15 in 100	☐
4)	50 in 100	50 in 100	☐	*or*	80 in 100	20 in 100	☐
5)	50 in 100	50 in 100	☐	*or*	75 in 100	25 in 100	☐
6)	50 in 100	50 in 100	☐	*or*	70 in 100	30 in 100	☐
7)	50 in 100	50 in 100	☐	*or*	65 in 100	35 in 100	☐
8)	50 in 100	50 in 100	☐	*or*	60 in 100	40 in 100	☐
9)	50 in 100	50 in 100	☐	*or*	55 in 100	45 in 100	☐
10)	50 in 100	50 in 100	☐	*or*	50 in 100	50 in 100	☐
11)	50 in 100	50 in 100	☐	*or*	45 in 100	55 in 100	☐
12)	50 in 100	50 in 100	☐	*or*	40 in 100	60 in 100	☐
13)	50 in 100	50 in 100	☐	*or*	35 in 100	65 in 100	☐
14)	50 in 100	50 in 100	☐	*or*	30 in 100	70 in 100	☐
15)	50 in 100	50 in 100	☐	*or*	25 in 100	75 in 100	☐
16)	50 in 100	50 in 100	☐	*or*	20 in 100	80 in 100	☐
17)	50 in 100	50 in 100	☐	*or*	15 in 100	85 in 100	☐
18)	50 in 100	50 in 100	☐	*or*	10 in 100	90 in 100	☐
19)	50 in 100	50 in 100	☐	*or*	5 in 100	95 in 100	☐
20)	50 in 100	50 in 100	☐	*or*	1 in 100	99 in 100	☐
	50 in 100	50 in 100	☐	*or*	0 in 100	100 in 100	☑

Figure 3.4: Sample Uncertainty Equivalent Task

Note: Sample uncertainty equivalent task for $(p; X, Y) = (0.5, 10, 30)$ eliciting $(q; 30, 0)$.

Generally, in price list experiments a non-negligible proportion of individuals switch from preferring Option A to Option B and then switch back. Around 10 percent of subjects feature multiple switch points in similar price list experiments (Holt and Laury, 2002; Meier and Sprenger, 2010) and as many as 50 percent in some cases (Jacobson and Petrie, 2009). Because such multiple switch points are difficult to rationalize and may indicate subject confusion, some researchers mechanically enforce single switch points.[20] Instead, we augmented the standard price list with a simple framing device designed to clarify the decision process. In particular, we added a line

[20] See Harrison et al. (2005) for discussion.

to both the top and bottom of each price list in which the choices were clear, and illustrated this by checking the obvious best option. The top line shows that each p-gamble is preferred to a 100 percent chance of receiving \$0 while the bottom line shows that a 100 percent chance of receiving \$$Y$ is preferred to each p-gamble.[21] These pre-checked gambles were not available for payment, but were used to clarify the decision task without leading the subjects. Since the economist is primarily interested in the price list method as a means of measuring a single choice – the switching point – it seemed natural to include language to this end. Hence, in directions subjects were told "Most people begin by preferring Option A and then switch to Option B, so one way to view this task is to determine the best row to switch from Option A to Option B." This greatly reduced the volume of multiple switching to less than 1 percent of total responses.[22]

In order to provide an incentive for truthful revelation of uncertainty equivalents, subjects were randomly paid one of their choices in cash at the end of the experimental session.[23] Seventy-six subjects were recruited from the undergraduate population at University of California, San Diego. The experiment lasted about one hour and average earnings were \$24.50, including a \$5 minimum payment.

3.3.1 Additional Risk Preference Measures

In addition to the uncertainty equivalents discussed above, subjects were also administered two Holt and Laury (2002) risk measures over payment values of \$10 and \$30 as well as 7 standard certainty equivalents tasks with p gambles over \$30 from the set $p \in \{0.05, 0.10, 0.25, 0.50, 0.75, 0.90, 0.95, 1\}$. The certainty equivalents probabilities were chosen to be identical to those used in the original probability weighting experiments of Tversky and Kahneman (1992) and Tversky and Fox (1995). These additional measures were also designed in price list style with similar language

[21] This methodology is close to the clarifying instructions from the original Holt and Laury (2002) where subjects were described a 10 lottery choice task and random payment mechanism and then told, "In fact, for Decision 10 in the bottom row, the die will not be needed since each option pays the highest payoff for sure, so your choice here is between 200 pennies or 385 pennies."

[22] Observations with multiple switch points were removed from analysis and are noted.

[23] Please see the instructions in the Appendix for payment information provided to subjects.

to the uncertainty equivalents and could also be chosen for payment.[24] Examples of these additional risk measures are provided in the appendix. Two orders of the tasks were implemented: 1) UE, HL, CE and 2) CE, HL, UE to examine order effects.[25]

3.4 Results

The analysis is presented in two sub-sections. First, we present data from uncertainty equivalents and provide simple likelihood ratio tests of competing models of risk preferences. We find that expected utility performs well away from certainty, but that a disproportionate preference for certainty is displayed when p approaches 1. Indeed this disproportionate preference yields individual violations of stochastic dominance when p is close to certainty.

Second, we analyze behavior in certainty equivalents experiments. The certainty equivalents data indicate the presence of both small stakes risk aversion and prospect theory probability weighting, reproducing previous findings. Importantly, we use behavior in the uncertainty equivalents to predict behavior in the certainty equivalents. Individuals who violate stochastic dominance in uncertainty equivalents are more likely to exhibit small-stakes risk aversion and non-linear probability weighting in certainty equivalents.

3.4.1 Uncertainty Equivalents and Tests of Linearity

Subjects made 24 uncertainty equivalent decisions in three (X, Y) payment sets. A summary of the data is presented in Figure 3.4.1. In order to provide estimates of the mean uncertainty equivalent as well as appropriate standard errors for each experimental probability, we first estimate non-parametric interval regressions

[24]Multiple switching was again greatly reduced relative to prior studies to less than 1 percent of responses. Observations with multiple switch points were removed from analysis and are noted. As will be seen, results of the CE task reproduce the results of others. This increases our confidence that our innovations with respect to the price lists did not result in biased or peculiar measurement of behavior.

[25]Though we used the HL task primarily as a buffer between certainty and uncertainty equivalents, a high degree of correlation is obtained across elicitation techniques. As the paper is already long, correlations with HL data are discussed primarily in footnotes.

(Stewart, 1983).[26] The interval response of the uncertainty equivalent, q, is regressed on indicators for all probability and payment-set interactions with standard errors clustered on the subject level. For ease of interpretation we calculate the relevant coefficients as linear combinations of interaction terms and present these in Table 4.1, Panel A. In Figure 3.4.1, the corresponding mean uncertainty equivalent, q, is graphed versus the experimental values of p for each payment set.

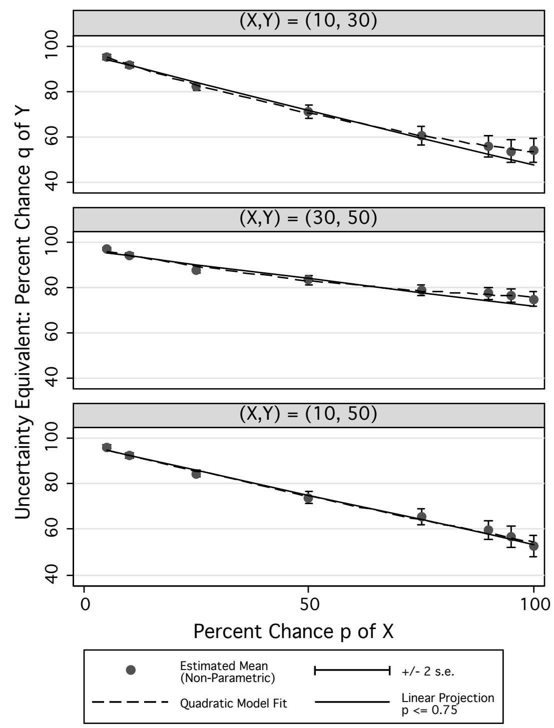

Figure 3.5: Uncertainty Equivalent Responses

Note: Figure presents uncertainty equivalent, $(q; Y, 0)$, corresponding to Table 4.1, Panel A for each given gamble, $(p; X, Y)$, of the experiment. The dashed black line represents the quadratic model fit of Table 4.1, Panel B. The solid black line corresponds to a linear projection based upon data from $p \leq 0.75$, indicating the degree to which the data adhere to the expected utility prediction of linearity away from certainty.

Figure 3.4.1 graphs the responsiveness of uncertainty equivalents to experimental parameters, p, X, and Y.[27] We first consider the prediction from expected

[26] Identical results are obtained when using OLS and the midpoint of the interval.

[27] Uncertainty equivalents correlate significantly with the number of safe choices chosen in the

utility of a linear relationship between p and q. Figure 3.4.1 provides a projection to certainty based on a linear interval regression of q on p for $p \leq 0.75$. Though the data adhere closely to this linear fit away from certainty, in the $(X, Y) = (10, 30)$ and $(30, 50)$ conditions, the slope dq/dp becomes appreciably more shallow as p approaches 1. Indeed in the $(X, Y) = (10, 30)$ condition, mean behavior exhibits a violation of stochastic dominance at certainty as the q for $p = 1$ is slightly above that of $p = 0.95$. In the $(X, Y) = (10, 50)$ condition, the relationship between p and q appears virtually linear throughout.[28]

To explore the apparent non-linearity near $p = 1$, Table 4.1, Panels B and C present estimates of the relationship between q and p. Panel B estimates interval regressions assuming a quadratic relationship, and Panel C assumes a linear relationship. Based on the arguments presented above, expected utility predicts a negligible square term, S-shaped probability weighting predicts a negative square term, and disappointment aversion and u-v preferences predict a positive square term. Panel B reveals positive square terms that are statistically significant in two of the conditions, $(X, Y) = (10, 30)$ and $(30, 50)$.[29]

The parametric specifications of Panels B and C can be compared to the non-parametric specification presented in Panel A via simple likelihood ratio chi-square tests. Neither the quadratic nor the linear specification can be rejected relative to the fully non-parametric model: $\chi^2(15)_{A,B} = 8.23$, $(p = 0.91)$; $\chi^2(18)_{A,C} = 23.66$, $(p = 0.17)$. However, the linear specification of Panel C can be rejected relative to the parsimonious quadratic specification of Panel B, $\chi^2(3)_{B,C} = 15.43$, $(p < 0.01)$. We reject expected utility's linear prediction in favor of a convex relationship between p

Holt-Laury risk tasks. For example, for $p = 0.5$ the individual correlations between the uncertainty equivalent q and the number of safe choices, S_{10}, in the \$10 HL task are $\rho_{q(10,30),S_{10}} = 0.52$ $(p < 0.01)$, $\rho_{q(30,50),S_{10}} = 0.38$ $(p < 0.01)$, and $\rho_{q(10,50),S_{10}} = 0.54$ $(p < 0.01)$. The individual correlations between the uncertainty equivalent, q, and the number of safe choices, S_{30}, in the \$30 HL task are $\rho_{q(10,30),S_{30}} = 0.54$ $(p < 0.01)$, $\rho_{q(30,50),S_{30}} = 0.45$ $(p < 0.01)$, and $\rho_{q(10,50),S_{30}} = 0.67$ $(p < 0.01)$. The correlation between the number of safe choices in the HL tasks is also high,$\rho_{S_{10},S_{30}} = 0.72$ $(p < 0.01)$. These results demonstrate consistency across elicitation techniques as higher elicited q and a higher number of safe HL choices both indicate more risk aversion.

[28]See Section 3.4.1 for discussion.

[29]One can also interpret the coefficients on $p \times 100$ as a measure of utility function curvature at $p = 0$ where $dq/dp = -(1 - u(X)/u(Y))$. Under risk neutrality, this coefficient should be -0.66 for $(X, Y) = (10, 30)$, -0.4 for $(X, Y) = (30, 50)$ and -0.8 for $(X, Y) = (10, 50)$. Though the estimates in the $(X, Y) = (10, 30)$ and $(30, 50)$ conditions are close to the risk neutral prediction, the $(X, Y) = (10, 50)$ condition differs substantially from risk neutrality.

Table 3.1: Estimates of the Relationship Between q and p

	(1) $(X,Y) = (\$10,\$30)$	(2) $(X,Y) = (\$30,\$50)$	(3) $(X,Y) = (\$10,\$50)$
Dependent Variable: Interval Response of Uncertainty Equivalent ($q \times 100$)			
Panel A: Non-Parametric Estimates			
$p \times 100 = 10$	-3.623***	-2.575***	-3.869***
	(0.291)	(0.321)	(0.413)
$p \times 100 = 25$	-13.270***	-8.867***	-11.840***
	(0.719)	(0.716)	(0.748)
$p \times 100 = 50$	-24.119***	-13.486***	-22.282***
	(1.476)	(0.916)	(1.293)
$p \times 100 = 75$	-34.575***	-17.790***	-30.769***
	(2.109)	(1.226)	(1.777)
$p \times 100 = 90$	-39.316***	-19.171***	-36.463***
	(2.445)	(1.305)	(2.190)
$p \times 100 = 95$	-41.491***	-20.164***	-39.721***
	(2.635)	(1.411)	(2.425)
$p \times 100 = 100$	-41.219***	-21.747***	-43.800***
	(2.626)	(1.536)	(2.454)
Constant	95.298***	96.822***	96.230***
	(0.628)	(0.290)	(0.497)
Log-Likelihood = -4498.66			
AIC = -9047.32, BIC = 9185.02			
Panel B: Quadratic Estimates			
$p \times 100$	-0.660***	-0.376***	-0.482***
	(0.060)	(0.035)	(0.047)
$(p \times 100)^2$	0.002***	0.002***	0.001
	(0.001)	(0.000)	(0.000)
Constant	98.125***	97.855***	97.440***
	(0.885)	(0.436)	(0.642)
Log-Likelihood = -4502.77			
AIC = -9025.55, BIC = 9080.63			
Panel C: Linear Estimates			
$p \times 100$	-0.435***	-0.209***	-0.428***
	(0.027)	(0.016)	(0.027)
Constant	95.091***	95.603***	96.718***
	(0.678)	(0.512)	(0.714)
Log-Likelihood = -4510.49			
AIC = -9034.98, BIC = 9073.54			

Notes: Coefficients from single interval regression for each panel (Stewart, 1983) with 1823 observations. Standard errors clustered at the subject level in parentheses. 76 clusters. The regressions feature 1823 observations because one individual had a multiple switch point in one uncertainty equivalent in the $(X,Y) = (\$10,\$50)$ condition. *Level of significance*: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

and q. In comparisons of information criteria, the quadratic specification is preferred by the Akaike Information Criteria (AIC) and the linear specification is preferred by the Bayesian Information Criteria (BIC).

Our results are important for evaluating linearity-in-probabilities, and for understanding the robustness of the standard probability weighting phenomenon. The data indicate that expected utility performs well away from certainty where the data adhere closely to linearity. However, the data deviate from linearity as p approaches 1, generating a convex relationship between p and q. This is a strong and significant rejection of the S-shaped probability weighting model. The finding is notable as the uncertainty equivalent paradigm is only a small deviation from standard certainty equivalents, where probability weighting has often been demonstrated. In Section 3.4.2, we unify these results by showing that these same subjects demonstrate apparent probability weighting in certainty equivalents.

While the data reject S-shaped probability weighting, both disappointment aversion and u-v preferences predict the obtained convex relationship between p and q with sharpened convexity at $p = 1$. The difference between the models arises in that u-v preferences predicts a strictly linear relationship away from certainty while disappointment aversion predicts convexity throughout. Though the data adhere closely to linearity for $p \leq 0.75$ in Figure 3.4.1, significant positive square terms are obtained for $p \leq 0.75$ in regressions corresponding to Table 4.1, and the the linear specification can be rejected relative to the quadratic specification of Panel B, $\chi^2(3) = 20.07$, $(p < 0.01)$. Supporting disappointment aversion, we reject linearity for probabilities away from certainty. However, linearity does provide surprisingly good model fit in this region allowing EU to adequately explain the data away from certainty.

We interpret the analysis of this subsection as being most consistent with disappointment aversion, though u-v could also provide a parsimonious explanation. The implied presence of a disproportionate preference for certainty in these models can, in theory, predict violations of first order stochastic dominance as p approaches 1.[30] In the next section we explore this prediction of violations of stochastic dominance

[30] Hints of this disproportionate preference exist in the prior literature. McCord and de Neufville (1986), with nine experimental subjects and a related construct they called a lottery equivalent,

near to certainty.

Violations of Stochastic Dominance

A substantial portion of our subjects violate first order stochastic dominance. These violations are organized close to certainty in a manner that is consistent with disappointment aversion and u-v preferences, indicating a disproportionate preference for certainty. Dominance violations are identified when a subject reports a higher q for a given p relative to his or her previous response q' for a smaller p'. For example, revealing a q of 0.5 for $p = 1$ would be an indirect violation of stochastic dominance if an individual had previously revealed a q' of 0.4 for $p' = 0.95$.

Each individual has 84 opportunities to violate first order stochastic dominance in such a way.[31] We can identify the percentage of choices violating stochastic dominance at the individual level and so develop an individual violation rate. To begin, away from certainty, violations of stochastic dominance are few, averaging only 4.3% ($s.d. = 6.4\%$). In the 21 cases per subject when certainty, $p = 1$, is involved, the individual violation rate increases significantly to 9.7% (15.8%), ($t = 3.88,\ p < 0.001$). When examining only the three comparisons of $p = 1$ to $p' = 0.95$, the individual violation rate increases further to 17.5% (25.8%), ($t = 3.95,\ p < 0.001$). Additionally, 38 percent (29 of 76) of subjects demonstrate at least one violation of stochastic dominance when comparing $p = 1$ to $p' = 0.95$. This finding suggests that violations of stochastic dominance are prevalent and tend to be localized close to certainty as allowed by disappointment aversion and predicted by u-v preferences. We identify individuals who feature violations of stochastic dominance between $p = 1$ and

document no systemaric difference in utilities elicited below probability 1, but that elicited utility at probability one was "consistently above and to the right of the other functions" [p. 60]. Bleichrodt et al. (2007) use five methods of utility elicitation for health outcomes including certainty equivalents and lottery equivalents. Expected utility was found to perform poorly in decisions involving certainty, but well in comparisons involving only uncertain prospects. Additionally the utilities elicited with certainty were generally above those elicited with uncertainty. Though these results and other certainty effects are often argued to be supportive of S-shaped probability weighting, careful consideration of our results suggests otherwise.

[31] Identifying violations in this way recognizes the interval nature of the data as it is determined by price list switching points. We consider violations within each payment set (X, Y). With 8 probabilities in each set, seven comparison can be made for $p = 1 : p' \in \{0.95, 0.9, 0.75, 0.5, 0.25, 0.1, 0.05\}$. Six comparisons can be made for $p = 0.95$ and so on, leading to 28 comparisons for each payment set and 84 within-set comparisons of this form.

$p' = 0.95$ as *Certainty Preferring*, and organize much of our further discussion around the behavior of subjects with and without this disproportionate preference for certainty. The remaining 62 percent of subjects without a disproportionate preference for certainty are classified as *Certainty Neutral*.[32]

Figure 3.4.1 reproduces Figure 3.4.1, but splits the sample by certainty preference. First, this shows the roughly 60% of subjects that are classified as Certainty Neutral demonstrate a linear relationship between q and p throughout. In estimates corresponding to Table 4.1, Panel B, negligible and insignificant square terms are obtained and quadratic and linear specifications cannot be distinguished ($\chi^2(3)_{B,C} = 0.69,\ p = 0.88$).[33] These data show that without specific individuals who exhibit a disproportionate preference for certainty, expected utility organizes the data extremely well. This finding of linearity is additionally important because eliminating the convexity of Certainty Preferring individuals should, in principle, give S-shaped probability weighting's concave prediction the best opportunity to be revealed.

Second, the mean uncertainty equivalents in Figure 3.4.1, Panels A and B coincide away from certainty and decline linearly with p. However, the uncertainty equivalents for subjects with a disproportionate preference for certainty peel away as certainty is approached. Third, for Certainty Preferring subjects aggregate violations of stochastic dominance are less pronounced in the $(X, Y) = (10, 50)$ condition. Andreoni and Sprenger (2009c) discuss experimental conditions when violations of stochastic dominance are more or less likely to be observed in experimental data and demonstrate that for one natural specification with differential curvature one would expect less pronounced violations of stochastic dominance as experimental stakes diverge in value.[34]

[32]This is not a complete taxonomy of types as one could imagine a classification for *Certainty Averse*. A full axiomatic development of Certainty Preferent, Neutral and Averse is left for future work and the present classifications are consistent with violation and non-violation of stochastic dominance between $p = 1$ and $p' = 0.95$. There were no session or order effects obtained for stochastic dominance violation rates or categorization of certainty preference. Certainty Preferring individuals are also more likely to violate stochastic dominance away from certainty. Their violation rate away from certainty is 8.2% (7.5%) versus 1.9% (4.1%) for Certainty Neutral subjects, ($t = 4.70,\ p < 0.001$). This, however, is largely driven by violations close to certainty.

[33]See Appendix Tables 3.2 and 3.3 for full estimates.

[34]The Andreoni and Sprenger (2009c) specification is of u-v preferences with $v(x) = x^{\alpha},\ u(x) = x^{\alpha-\beta}$ with $\beta < \alpha < 1$. The differential curvature causes less pronounced violations of stochastic dominance when stakes differ substantially.

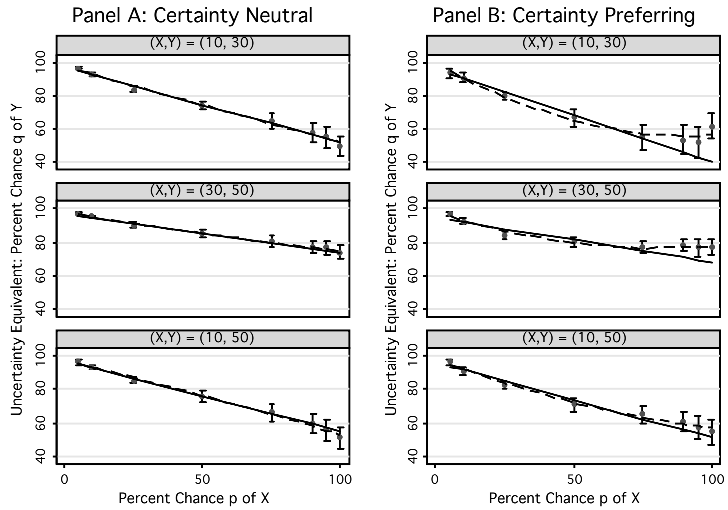

Figure 3.6: Uncertainty Equivalents and Certainty Preference

Note: Figure presents estimated uncertainty equivalent, $(q; Y, 0)$, for each given gamble, $(p; X, Y)$, of the experiment split by certainty preference, following methodology from Table 4.1, Panel A. Dashed black line represents the quadratic model fit following methodology from Table 4.1, Panel B. The solid black line corresponds to a linear projection based upon data from $p \leq 0.75$, indicating the degree to which the data adhere to the expected utility prediction of linearity away from certainty. See Appendix Tables 3.2 and 3.3 for estimates.

Documenting *within-subject* violations of stochastic dominance using uncertainty equivalents is potentially of great interest. The observed violations are predicted by both the *u-v* model and specific versions of disappointment aversion (Bell, 1985; Koszegi and Rabin, 2006, 2007) and represent a within-subject example of the hotly debated 'uncertainty effect.' Gneezy et al. (2006) discuss between-subject results indicating that a 50%-50% gamble over book-store gift certificates is valued less than the certainty of the gamble's worst outcome. Though the effect was reproduced in Simonsohn (2009), other work has challenged these results (Keren and Willemsen, 2008; Rydval et al., 2009). While Gneezy et al. (2006) do not find within-subject examples of the uncertainty effect, Sonsino (2008) finds a similar within-subject effect in the Internet auction bidding behavior of around 30% of individuals. Additionally, the uncertainty effect was thought not to be present for monetary payments (Gneezy et al., 2006). Our findings may help to inform the debate on the uncertainty effect and its robustness to the monetary domain. Additionally, our results may also help to identify the source of the uncertainty effect: a disproportionate preference for certainty. Indeed, this view is hypothesized by Gneezy et al. (2006), who suggest that "an individual posed with a lottery that involves equal chance at a $50 and $100 gift certicate might code this lottery as a $75 gift certicate plus some risk. She might then assign a value to a $75 gift certicate (say $35), and then reduce this amount (to say $15) to account for the uncertainty."[p. 1291]

The original uncertainty effect of preferring a 50%-50% gamble's worst outcome over the gamble itself requires an intense preference for certainty. In our environment, the behavior could be generated if the elicited q at certainty was higher than those elicited for $p \in \{0.95, 0.9, 0.75, 0.5\}$. That is, subjects would have an individual violation rate in the 21 experimental comparisons involving certainty of greater than 0.5. Figure 3.4.1 presents a histogram of *Violation Rate*, the stochastic dominance violation rate for the 21 comparisons involving certainty.[35] In our subsequent analysis we will use Violation Rate as a continuous measure of the intensity of the disproportionate preference for certainty.[36] Notable from Figure 3.4.1 is that there is heterogeneity in the intensity of Violation Rate and that only in the extreme is it high enough to be clearly suggestive of preferring a 50%-50% gamble's worst outcome over the gamble itself.

It is important to note that the violations of stochastic dominance that we document are indirect measures of violation. We hypothesize that violations of stochastic dominance would be less prevalent in direct preference rankings of gambles with a dominance relation. Of course, such direct violations of dominance may be eliminated from the u-v preference model via editing arguments (Neilson, 1992) and are excluded by assumption in some models of disappointment aversion (Gul, 1991; Loomes and Sugden, 1986). However, it must be recognized that both models in some form pre-

[35] A small minority of Certainty Neutral subjects have non-zero violation rates, as their elicited q at certainty is higher than that of some lower probability. The average certainty Violation Rate (0.069) for the 19% of Certainty Neutral subjects (9 of 47) with positive Violation Rate values is about same as their average violation rate away from certainty (0.060). For Certainty Preferring subjects, the average certainty Violation Rate (0.235) is about three times their violation rate away from certainty (0.082).

[36] We recognize that this is a rough measure of intensity of certainty preference in the sense that individuals could have a non-monotonic relationship between p and q away from certainty. However, given the low dominance violation rates away from certainty, this is not extremely problematic.

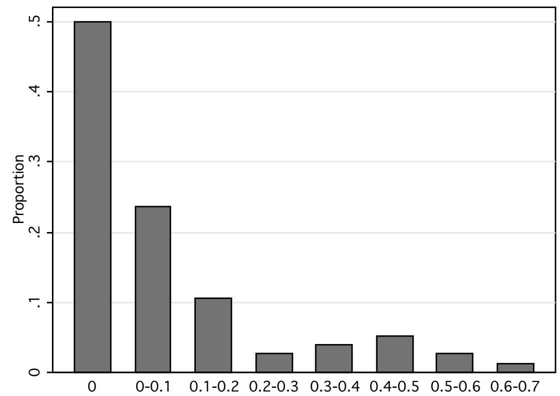

Figure 3.7: Histogram of Violation Rate

Note: Figure presents a histogram of Violation Rate calculating the fraction of violations of stochastic dominance in 21 experimental comparisons involving $p = 1$.

dict violations of dominance in all contexts. Because one would not predict violations in direct comparison, the present results could potentially be viewed as errors or mistakes in decision-making influenced by frames and experimental methods.

Though we believe the presence of dominance violations can be influenced by frames, this is likely true for the presence of many decision phenomena. In the following section we present data from certainty equivalents demonstrating that one cannot likely consider near-certainty dominance violations as an error and probability weighting as a true preference. The two phenomena correlate highly at the individual level.

3.4.2 Certainty Equivalents Data

In this section we explore behavior in standard certainty equivalents. Seven certainty equivalents tasks with p gambles over \$30 and \$0 from the set $p \in \{0.05, 0.10, 0.25, 0.50, 0.75, 0.90, 0.95\}$ were administered, following the probabilities used in the original probability weighting experiments of Tversky and Kahneman (1992) and Tversky and Fox (1995). The analysis also follows closely the presentation and non-linear estimation techniques of Tversky and Kahneman (1992) and Tversky and Fox (1995).

As noted in Section 3.2, certainty equivalent analysis estimating risk aversion or probability weighting parameters that assumes a single utility function is flawed if there exists a disproportionate preference for certainty. If a differential preference for certainty exists, then risk aversion or probability weights are not identified separately from the specific preference for certainty. As such, extreme small-stakes risk aversion or non-linear probability weighting may be apparent when none actually exists.[37] We first document small stakes risk aversion and apparent probability weighting in our data and then correlate these phenomena with the violations of dominance measured in Section 3.4.1.

Risk Aversion and Probability Weighting

The identification of probability weighting and small-stakes risk aversion from certainty equivalents data normally relies on a range of experimental probabilities from near zero to near one. Probability weighting is initially supported if, for fixed stakes, subjects appear risk loving at low probabilities and risk averse at higher probabilities.[38] Small-stakes risk aversion would be viewed as the risk aversion aspect of this phenomenon.

Figure 3.4.2 presents a summary of the obtained certainty equivalents.[39] As in Section 3.4.1, in order to obtain appropriate estimates of the mean and standard errors, we first conducted an interval regression of the certainty equivalent, C, on indicators for the experimental probabilities. Corresponding estimates are provided in Appendix Table 3.4, Column (1). Following Tversky and Kahneman (1992), the data are presented relative to a benchmark of risk neutrality such that, for a linear utility function, Figure 3.4.2 directly reveals the probability weighting function, $\pi(p)$. The data show evidence of both small stakes risk aversion and non-linear probability

[37]Diecidue et al. (2004) discuss potential functional forms that could deliver both apparent up-weighting of low probabilities and down-weighting of high probabilities in certainty equivalents. One possibility is the u-v parameterization discussed in Andreoni and Sprenger (2009c) with differential curvature, $v(x) = x^{\alpha}$, $u(x) = x^{\alpha-\beta}$ with $\beta < \alpha < 1$, which produces both up-weighting of (very) low probabilities and down-weighting of high probabilities.

[38]Because certainty equivalent responses are determined by both utility function curvature and probability weighting, even risk aversion at low probabilities could be consistent with probability weighting provided risk aversion was increasing in probability.

[39]Figure 3.4.2 excludes one subject with multiple switching in one task. Identical aggregate results are obtained with the inclusion of this subject. However, we cannot estimate probability weighting at the individual level for this subject.

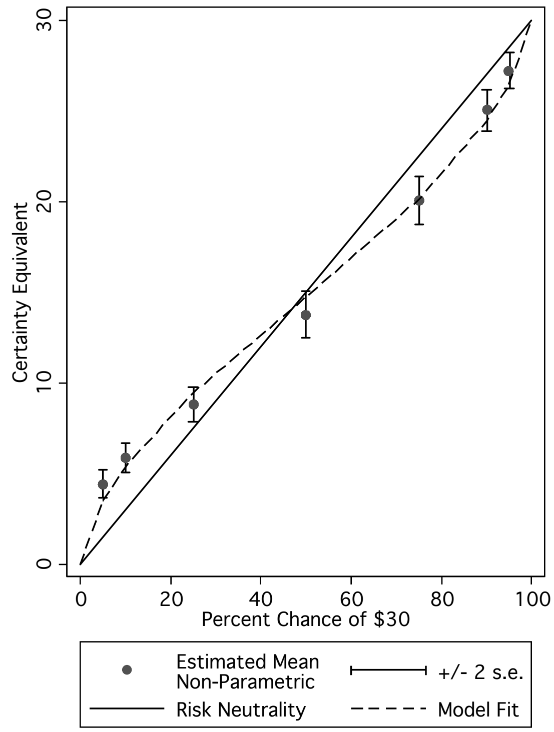

Figure 3.8: Certainty Equivalent Responses

Note: Mean certainty equivalent response. Solid line corresponds to risk neutrality. Dashed line corresponds to fitted values from non-linear least squares regression (1).

weighting. Subjects are significantly risk loving at low probabilities and significantly risk averse at intermediate and high probabilities. These findings are in stark contrast to those obtained in the uncertainty equivalents discussed in Section 3.4.1. Whereas in uncertainty equivalents we obtain no support for S-shaped probability weighting, in certainty equivalents we reproduce the probability weighting results generally found.[40]

Tversky and Kahneman (1992) and Tversky and Fox (1995) obtain probability weighting parameters from certainty equivalents data by parameterizing both the

[40] Certainty equivalents correlate significantly with the number of safe choices in the Holt-Laury risk tasks. For example, for $p = 0.5$ the individual correlations between the midpoint certainty equivalent, C, and the number of safe choices, S_{10} and S_{30}, in the HL tasks are $\rho_{C,S_{10}} = -0.24$ ($p < 0.05$) and $\rho_{C,S_{30}} = -0.24$ ($p < 0.05$). These results demonstrate consistency across elicitation techniques as a lower certainty equivalent and a higher number of safe HL choices both indicate more risk aversion. Additionally, the certainty equivalents correlate significantly with uncertainty equivalents. For example, for $p = 0.5$ the individual correlations between the midpoint certainty equivalent, C, and the midpoint of the uncertainty equivalent, q, are $\rho_{C,q(10,30)} = -0.24$ ($p < 0.05$), $\rho_{C,q(30,50)} = -0.25$ ($p < 0.05$), and $\rho_{C,q(10,50)} = -0.24$ ($p < 0.05$).

utility and probability weighting functions and assuming the indifference condition

$$u(C) = \pi(p) \cdot u(30)$$

is met for each observation. We follow the parameterization of Tversky and Kahneman (1992) with power utility, $u(X) = X^{\alpha}$, and the one-parameter weighting function $\pi(p) = p^{\gamma}/(p^{\gamma} + (1-p)^{\gamma})^{1/\gamma}$.[41] Lower γ corresponds to more intense probability weighting. The parameters $\hat{\gamma}$ and $\hat{\alpha}$ are then estimated as the values that minimize the sum of squared residuals of the non-linear regression equation

$$C = [p^{\gamma}/(p^{\gamma} + (1-p)^{\gamma})^{1/\gamma} \times 30^{\alpha}]^{1/\alpha} + \epsilon. \tag{3.1}$$

When conducting such analysis on our aggregate data with standard errors clustered on the subject level, we obtain $\hat{\alpha} = 1.07$ (0.05) and $\hat{\gamma} = 0.73$ (0.03).[42] The hypothesis of linear utility, $\alpha = 1$, is not rejected, ($F_{1,74} = 2.18$, $p = 0.15$), while linearity in probability, $\gamma = 1$, is rejected at all conventional levels, ($F_{1,74} = 106.36$, $p < 0.01$). The model fit is presented as the dashed line in Figure 3.4.2. The obtained probability weighting estimate compares favorably with the Tversky and Kahneman (1992) estimate of $\hat{\gamma} = 0.61$ and other one-parameter estimates such as Wu and Gonzalez (1996) who estimate $\hat{\gamma} = 0.71$.

[41] Tversky and Fox (1995) use power utility with curvature fixed at $\alpha = 0.88$ from Tversky and Kahneman (1992) and a two parameter $\pi(\cdot)$ function.

[42] For this analysis we estimate using the interval midpoint as the value of C, and note that the dependent variable is measured with error.

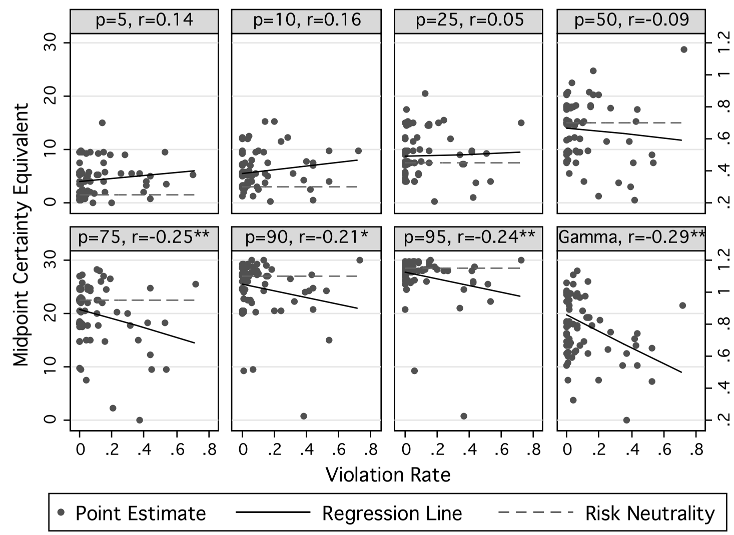

Figure 3.9: Certainty Equivalents (C), Probability Weighting ($\hat{\gamma}$), and Violation Rate

Note: Correlations (r) for 75 of 76 subjects. One subject with multiple switching in one certainty equivalent task not included. One percent jitter added to scatter plots. Two observations with Violation Rate = 0 and $\hat{\gamma} \geq 2$ not included in scatter plots, but included in regression line and reported correlation. *Level of significance*: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The presence of small stakes risk aversion and probability weighting in the aggregate certainty equivalents data is intriguing. These phenomena, as observed in certainty equivalents, may be conflated with a disproportionate preference for certainty. In order to test this claim, we present simple correlations in Figure 3.4.2. For each experimental probability, we correlate certainty equivalents with the intensity of certainty preference, Violation Rate, from the uncertainty equivalents. A horizontal benchmark of each gamble's expected value is also provided. The correlations suggest that the intensity of the disproportionate preference for certainty plays an important role for measured risk attitudes. Significant negative correlations between Violation Rate and certainty equivalents are obtained, primarily at higher probabilities. Insignificant positive correlations are obtained at lower probabilities. These results indicate that subjects with a more intense preference for certainty, as measured by Violation Rate, display significantly more small stakes risk aversion. These results are confirmed in regression, and we reject the null hypothesis that Violation Rate has no influence on certainty equivalent responses, ($\chi^2(7) = 18.06,\ p < 0.01$). Appendix Table 3.4, Column (5) provides the detail.

For subjects with a more intense preference for certainty, the significant increase in risk aversion at high probabilities and the slight increase in risk loving at low probabilities introduces more non-linearity into their measured probability weighting functions. Figure 3.4.2 also presents the correlation between individual probability weighting, $\hat{\gamma}$, estimated from (1) and the intensity of certainty preference, Violation Rate.[43] The degree to which individuals disproportionately prefer certainty predicts the degree of certainty equivalent-elicited probability weighting, ($\rho = -0.29,\ p = 0.011$).[44] This gives support to the claim that a disproportionate preference for certainty is conflated with non-linear probability weighting in standard

[43] Following the aggregate estimate, $\alpha = 1$ is assumed for the individual estimates.

[44] Additionally, the indicator for Certainty Preferent correlates with certainty equivalent response. See Appendix Table 3.4, Column (3). Eleven subjects exhibited non-monotonic relationships between experimental probabilities and elicited certainty equivalents. Though no systematic pattern was observed, such behavior is another example of violating stochastic dominance in decisions involving certainty. Non-monotonicity correlates significantly with being Certainty Preferent, ($\rho = 0.44,\ p < 0.01$). Eliminating these individuals leaves the results qualitatively unchanged. Certainty equivalent behavior remains supportive of probability weighting and elicited probability weights remain significantly correlated with Violation Rate, ($\rho = -0.29,\ p < 0.05$). Appendix Table 3.4, Columns (2), (4) and (6) provide estimates.

certainty-based experiments.[45]

It is critically important to contrast our results demonstrating the absence of probability weighting in uncertainty equivalents, its presence in certainty equivalents, and its relationship to violations of stochastic dominance with the body of results that find support for S-shaped probability weighting. Above we have discussed why standard estimation exercises assuming functional form for utility or probability weighting cannot be used to directly test linearity-in-probabilities. However, there exist a number of studies parametrically investigating probability weighting in decisions without certainty (Tanaka et al., 2010; Booij et al., 2010). These parametric estimates indicate that S-shaped probability weighting may be observed in decisions without certainty and clearly points to the need for future research. Additionally, attention must be given to the 'parameter-free' elicitation techniques that find non-parametric support for non-linear probability weights (Gonzalez and Wu, 1999; Abdellaoui, 2000; Bleichrodt and Pinto, 2000). Importantly, both Gonzalez and Wu (1999) and Abdellaoui (2000) make use of certainty equivalents or a number of certain outcomes to identify probability weights, a technique that is misspecified if there exists a specific preference for certainty. Bleichrodt and Pinto (2000) do not use certainty equivalents techniques, but their experiment is designed not to elicit preferences over monetary payments, but rather over hypothetical life years. It is not clear the extent to which such findings apply to incentivized elicitation procedures over money.[46]

[45] As a robustness test, we repeat the analysis with an alternate functional form Prelec (1998), $\pi(p) = exp(-(-lnp)^{\gamma})$, and obtain a correlation between $\hat{\gamma}$ and Violation Rate of $\rho = -0.25$ $p < 0.05$. Additionally, Appendix Tables 3.5 and 3.6 present results from the classification of risk attitudes based on the interval of the certainty equivalent response. These data demonstrate that Certainty Preferring subjects are more likely to be classified as risk loving at low probabilities and are more likely to be classified as risk averse at higher probabilities. Additionally, Certainty Neutral subjects are more likely to be risk neutral at higher probabilities.

[46] Abdellaoui (2000), Bleichrodt and Pinto (2000), Booij and van de Kuilen (2009) and Booij et al. (2010) share a two-stage elicitation procedure which 'chains' responses in order to obtain utility or probability weighting values. Such chained procedures are common to the 'trade-off' (Wakker and Deneffe, 1996) method of utility assessment. A discussed problem with these chained methods is that errors propagate through the experiment.

3.5 Conclusion

Volumes of research exists exploring both the implications and violations of the independence axiom. Surprisingly, little research exists directly testing the most critical result of the independence axiom: linearity-in-probabilities of the Expected Utility (EU) function. We present an experimental device that easily generates such a direct test, the *uncertainty equivalent*. Uncertainty equivalents not only provide tests of expected utility's linearity-in-probabilities, but also provide separation between competing alternative preference models such as Cumulative Prospect Theory's (CPT) inverted S-shaped probability weighting (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), expectations-based reference dependence (Bell, 1985; Loomes and Sugden, 1986; Gul, 1991; Koszegi and Rabin, 2006, 2007), and the u-v preference model (Neilson, 1992; Schmidt, 1998; Diecidue et al., 2004).

In a within-subject experiment with both uncertainty equivalents and standard certainty equivalent methodology we demonstrate four important results. First, independence performs well away from certainty where probabilities are found to be weighted nearly linearly. Second, independence breaks down close to certainty. The nature of the violation is contrary to standard S-shaped probability weighting and consistent with other alternative models such as disappointment aversion and u-v preferences, which both feature a disproportionate preference for certainty. Third, nearly 40% of experimental subjects indirectly violate first order stochastic dominance as probabilities approach 1. These violations are a necessary prediction of the u-v model and are accommodated in some versions of disappointment aversion. Fourth, in certainty equivalents experiments, apparent S-shaped probability weighting and small stakes risk aversion phenomena are observed, closely reproducing prior findings. However, these phenomena are driven by individuals who exhibit a disproportionate preference for certainty in uncertainty equivalents by violating stochastic dominance.

By far the most central result of this research is the demonstration that a parsimonious model of a disproportionate preference for certainty can be extremely powerful in explaining and unifying a gamut of economic behavior, from apparent S-shaped probability weighting to violations of first order stochastic dominance.

Our findings have critical implications for research on risk attitudes and have applications to a variety of economic problems. The results demonstrate that experimental measures of risk attitudes and EU violations are dramatically influenced by the presence of certainty. Since the work of Allais (1953b) certainty has been known to play a special role in decision-making and in generating non-EU behavior. Our results indicate that a specific preference for certainty may be the key element in producing such behavior. This suggests that empirical work should take great care to separate certainty preferences from other phenomena under investigation. Additionally, theoretical research should take seriously models with specific preferences for certainty and their implications in the study of decision-making under uncertainty.

3.6 Appendix

3.6.1 Additional Estimates

Table 3.2: Relationship Between q and p for Certainty Neutral Subjects

	(1) $(X,Y)=(\$10,\$30)$	(2) $(X,Y)=(\$30,\$50)$	(3) $(X,Y)=(\$10,\$50)$
Dependent Variable: Interval Response of Uncertainty Equivalent $(q\times 100)$			
Panel A: Non-Parametric Estimates			
$p\times 100=10$	-3.832***	-1.708***	-3.161***
	(0.361)	(0.349)	(0.322)
$p\times 100=25$	-12.928***	-6.782***	-10.750***
	(0.767)	(0.739)	(0.974)
$p\times 100=50$	-22.492***	-11.804***	-20.306***
	(1.426)	(1.211)	(1.631)
$p\times 100=75$	-32.058***	-16.635***	-30.306***
	(2.216)	(1.685)	(2.462)
$p\times 100=90$	-38.760***	-19.613***	-37.080***
	(2.725)	(1.875)	(3.070)
$p\times 100=95$	-41.526***	-20.348***	-40.412***
	(3.005)	(1.855)	(3.233)
$p\times 100=100$	-46.951***	-23.220***	-45.199***
	(3.083)	(2.021)	(3.168)
Constant	96.367***	97.037***	96.210***
	(0.407)	(0.289)	(0.687)
Log-Likelihood = -2760.30			
Panel B: Quadratic Estimates			
$p\times 100$	-0.471***	-0.276***	-0.393***
	(0.043)	(0.041)	(0.055)
$(p\times 100)^2$	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Constant	97.313***	97.905***	97.132***
	(0.582)	(0.465)	(0.804)
Log-Likelihood = −2764.70			
Panel C: Linear Estimates			
$p\times 100$	-0.454***	-0.226***	-0.445***
	(0.033)	(0.022)	(0.036)
Constan	97.081***	97.227***	97.832***
	(0.486)	(0.528)	(0.861)
Log-Likelihood = -2765.05			

Notes: Coefficients from single interval regression for each panel (Stewart, 1983) with 1127 observations. Standard errors clustered at the subject level in parentheses. 47 clusters. The regressions feature 1127 observations because one individual had a multiple switch point in one uncertainty equivalent in the $(X,Y)=(\$10,\$50)$ condition. *Level of significance*: $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$

Table 3.3: Relationship Between q and p for Certainty Preferring Subjects

	(1) $(X,Y)=(\$10,\$30)$	(2) $(X,Y)=(\$30,\$50)$	(3) $(X,Y)=(\$10,\$50)$
Dependent Variable: Interval Response of Uncertainty Equivalent ($q \times 100$)			
Panel A: Non-Parametric Estimates			
$p \times 100 = 10$	-3.282***	-3.984***	-5.019***
	(0.492)	(0.534)	(0.918)
$p \times 100 = 25$	-13.823***	-12.249***	-13.605***
	(1.433)	(1.218)	(1.104)
$p \times 100 = 50$	-26.754***	-16.214***	-25.487***
	(3.079)	(1.247)	(2.016)
$p \times 100 = 75$	-38.651***	-19.663***	-31.521***
	(4.145)	(1.670)	(2.450)
$p \times 100 = 90$	-40.215***	-18.456***	-35.487***
	(4.706)	(1.599)	(2.947)
$p \times 100 = 95$	-41.432***	-19.864***	-38.602***
	(4.969)	(2.191)	(3.656)
$p \times 100 = 100$	-31.933***	-19.361***	-41.533***
	(4.255)	(2.316)	(3.906)
Constant	93.565***	96.473***	96.263***
	(1.469)	(0.600)	(0.691)
Log-Likelihood = -1712.51			
Panel B: Quadratic Estimates			
$p \times 100$	-0.967***	-0.538***	-0.625***
	(0.121)	(0.052)	(0.081)
$(p \times 100)^2$	0.005***	0.003***	0.002**
	(0.001)	(0.001)	(0.001)
Constant	99.440***	97.774***	97.941***
	(2.120)	(0.872)	(1.074)
Log-Likelihood = –1719.41			
Panel C: Linear Estimates			
$p \times 100$	-0.406***	-0.182***	-0.401***
	(0.048)	(0.024)	(0.039)
$(p \times 100)^2$	91.865***	92.967***	94.914***
	(1.414)	(0.835)	(1.192)
Log-Likelihood = -1736.33			

Notes: Coefficients from single interval regression for each panel (Stewart, 1983) with 696 observations. Standard errors clustered at the subject level in parentheses. 29 clusters.

Level of significance: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.4: Relationship between C and p

	Dependent Variable: Interval Response of Certainty Equivalent (C)					
	(1)	(2)	(3)	(4)	(5)	(6)
$p \times 100 = 10$	1.456***	1.517***	1.270***	1.246***	1.380***	1.550***
	(0.220)	(0.174)	(0.166)	(0.167)	(0.203)	(0.177)
$p \times 100 = 25$	4.378***	4.380***	4.275***	4.215***	4.542***	4.786***
	(0.333)	(0.358)	(0.375)	(0.379)	(0.373)	(0.371)
$p \times 100 = 50$	9.339***	9.688***	9.749***	9.858***	9.921***	10.613***
	(0.632)	(0.588)	(0.643)	(0.649)	(0.721)	(0.610)
$p \times 100 = 75$	15.595***	16.159***	16.226***	16.404***	16.744***	17.350***
	(0.668)	(0.682)	(0.771)	(0.774)	(0.710)	(0.681)
$p \times 100 = 90$	20.593***	21.448***	21.345***	21.632***	21.488***	22.004***
	(0.625)	(0.560)	(0.719)	(0.680)	(0.666)	(0.628)
$p \times 100 = 95$	22.785***	23.412***	23.660***	23.572***	23.688***	23.771***
	(0.601)	(0.537)	(0.661)	(0.670)	(0.583)	(0.598)
Certainty Preferring (=1)			1.578**	1.127		
			(0.768)	(0.862)		
Certainty Preferring (=1), $p \times 100 = 10$			0.484	0.917**		
			(0.528)	(0.429)		
Certainty Preferring (=1), $p \times 100 = 25$			0.267	0.556		
			(0.728)	(0.888)		
Certainty Preferring (=1), $p \times 100 = 50$			-1.065	-0.581		
			(1.420)	(1.404)		
Certainty Preferring (=1), $p \times 100 = 75$			-1.633	-0.823		
			(1.412)	(1.561)		
Certainty Preferring (=1), $p \times 100 = 90$			-1.945	-0.619		
			(1.309)	(1.174)		
Certainty Preferring (=1), $p \times 100 = 95$			-2.268*	-0.539		
			(1.273)	(1.087)		
Violation Rate					2.792	2.719
					(1.896)	(3.028)
Violation Rate, $p \times 100 = 10$					0.769	-0.474
					(1.448)	(1.430)
Violation Rate, $p \times 100 = 25$					-1.682	-5.676***
					(2.718)	(2.185)
Violation Rate, $p \times 100 = 50$					-5.893	-12.871***
					(6.776)	(3.732)
Violation Rate, $p \times 100 = 75$					-11.643**	-16.613***
					(5.327)	(3.165)
Violation Rate, $p \times 100 = 90$					-9.102*	-7.769*
					(5.033)	(4.345)
Violation Rate, $p \times 100 = 95$					-9.178*	-4.993
					(4.726)	(3.772)
Constant	4.421***	4.209***	3.812***	3.875***	4.146***	4.013***
	(0.376)	(0.391)	(0.448)	(0.453)	(0.427)	(0.435)
Log-Likelihood	-1335.677	-1085.103	-1330.755	-1081.072	-1326.645	-1072.665
# Observations	525	448	525	448	525	448
# Clusters	75	64	75	64	75	64

Notes: Coefficients from interval regressions (Stewart, 1983). Standard errors clustered at the subject level in parentheses. Columns (2), (4), (6) restrict the sample to individuals with a monotonic relationship between C and p. *Level of significance*: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table 3.5: Risk Attitudes in Certainty Equivalents

Panel A: All Subjects (N=75)				
			Proportion	
p	N	Risk Averse	Risk Neutral	Risk Loving
0.05	75	0.12	0.28	0.60
0.10	75	0.09	0.25	0.65
0.25	75	0.23	0.33	0.44
0.50	75	0.40	0.27	0.33
0.75	75	0.49	0.23	0.28
0.90	75	0.47	0.23	0.31
0.95	75	0.28	0.48	0.24
Panel B: Certainty Preferring (N=29)				
			Proportion	
p	N	Risk Averse	Risk Neutral	Risk Loving
0.05	29	0.07	0.21	0.72
0.10	29	0.03	0.10	0.86
0.25	29	0.17	0.24	0.59
0.50	29	0.45	0.10	0.45
0.75	29	0.52	0.07	0.41
0.90	29	0.48	0.17	0.34
0.95	29	0.31	0.34	0.34
Panel C: Certainty Neutral (N=46)				
			Proportion	
p	N	Risk Averse	Risk Neutral	Risk Loving
0.05	46	0.15	0.33	0.52
0.10	46	0.13	0.35	0.52
0.25	46	0.26	0.39	0.35
0.50	46	0.37	0.37	0.26
0.75	46	0.48	0.33	0.20
0.90	46	0.46	0.26	0.28
0.95	46	0.26	0.57	0.17

Notes: Table reports classification of risk averse, neutral and loving based on interval of certainty equivalent response for 75 of 76 subjects. One subject with multiple switching in one task is eliminated.

Table 3.6: Risk Aversion and Risk Loving in Certainty Equivalents

	All p		$p \leq 0.25$		$p > 0.25$	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Risk Averse, Neutral or Loving Classification						
Risk Loving						
Certainty Preferring (=1)	1.261***		1.129**		1.336***	
	(0.381)		(0.445)		(0.469)	
Violation Rate		4.236***		5.046**		3.418**
		(1.349)		(2.131)		(1.685)
$p \times 100$	-0.013***	-0.013***	-0.032**	-0.030**	-0.016*	-0.015*
	(0.004)	(0.004)	(0.014)	(0.015)	(0.008)	(0.008)
Constant	0.471	0.578**	0.672*	0.692**	0.703	0.911
	(0.296)	(0.275)	(0.359)	(0.343)	(0.713)	(0.690)
Risk Averse						
Certainty Preferring (=1)	0.667*		-0.045		0.920**	
	(0.389)		(0.653)		(0.441)	
Violation Rate		4.614***		3.987		4.628***
		(1.226)		(2.543)		(1.392)
$p \times 100$	0.010**	0.010**	0.029	0.028	-0.013*	-0.013*
	(0.004)	(0.004)	(0.021)	(0.021)	(0.008)	(0.008)
Constant	-0.756**	-0.935**	-1.107**	-1.359**	1.060	0.978
	(0.362)	(0.366)	(0.551)	(0.548)	(0.656)	(0.671)
# Observations	525	525	225	225	300	300
# Clusters	75	75	75	75	75	75
Log-Likelihood	-529.355	-528.915	-205.289	-205.384	-315.141	-314.776

Notes: Coefficients of multinomial logit regressions. Dependent variable: classification of risk averse, neutral and loving based on interval of certainty equivalent response for 75 of 76 subjects. One subject with multiple switching in one task is eliminated. Reference category: risk neutrality. Clustered standard errors in parentheses. *Level of significance*: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

3.6.2 Experimental Instructions

Hello and Welcome.

ELIGIBILITY FOR THIS STUDY: To be in this study, you must be a UCSD student. There are no other requirements. The study will be completely anonymous. We will not collect your name, student PID or any other identifying information. You have been assigned a participant number and it is on the note card in front of you. This number will be used throughout the study. Please inform us if you do not know or cannot read your participant number.

EARNING MONEY:

To begin, you will be given a $5 minimum payment. This $5 is yours. Whatever you earn from the study today will be added to this minimum payment. All payments will be made in cash at the end of the study today.

In this study you will make choices between two options. The first option will always be called OPTION A. The second option will always be called OPTION B. In each decision, all you have to do is decide whether you prefer OPTION A or OPTION B. These decisions will be made in 5 separate blocks of tasks. Each block of tasks is slightly different, and so new instructions will be read at the beginning of each task block.

Once all of the decision tasks have been completed, we will randomly select one decision as the decision-that-counts. If you preferred OPTION A, then OPTION A would be implemented. If you preferred OPTION B, then OPTION B would be implemented.

Throughout the tasks, either OPTION A, OPTION B or both will involve chance. You will be fully informed of the chance involved for every decision. Once we know which is the decision-that-counts, and whether you prefer OPTION A or OPTION B, we will then determine the value of your payments.

For example, OPTION A could be a 75 in 100 chance of receiving $10 and a 25 in 100 chance of receiving $30. This might be compared to OPTION B of a 50 in 100 chance of receiving $30 and a 50 in 100 chance of receiving nothing. Imagine for a moment which one you would prefer. You have been provided with a calculator to help you in your decisions.

If this was chosen as the decision-that-counts, and you preferred OPTION A, we would then randomly choose a number from 1 to 100. This will be done by throwing two ten-sided die: one for the tens digit and one for the ones digit (0-0 will be 100). If the chosen number was between 1 and 75 (inclusive) you would receive \$10 (+5 minimum payment) = \$15. If the number was between 76 and 100 (inclusive) you would receive \$30 (+5 minimum payment) = \$35. If, instead, you preferred OPTION B, we would again randomly choose a number from 1 to 100. If the chosen number was between 1 and 50 (inclusive) you'd receive \$0 (+5 minimum payment) = \$5. If the number was between 51 and 100 (inclusive) you'd receive \$30 (+5 minimum payment) = \$35.

In a moment we will begin the first task.

3.6.3 Sample Uncertainty Equivalent

On this page you will make a series of decisions between two uncertain options. Option A will be a 5 in 100 chance of \$10 and a 95 in 100 chance of \$30. Option B will vary across decisions. Initially, Option B will be a 95 in 100 chance of \$0 and a 5 in 100 chance of \$30. As you proceed down the rows, Option B will change. The chance of receiving \$30 will increase, while the chance of receiving \$0 will decrease.

For each row, all you have to do is decide whether you prefer Option A or Option B.

	Option A			or	Option B		
	Chance of \$10	Chance of \$30			Chance of \$0	Chance of \$30	
	5 in 100	95 in 100	☑	or	100 in 100	0 in 100	☐
1)	5 in 100	95 in 100	☐	or	95 in 100	5 in 100	☐
2)	5 in 100	95 in 100	☐	or	90 in 100	10 in 100	☐
3)	5 in 100	95 in 100	☐	or	85 in 100	15 in 100	☐
4)	5 in 100	95 in 100	☐	or	80 in 100	20 in 100	☐
5)	5 in 100	95 in 100	☐	or	75 in 100	25 in 100	☐
6)	5 in 100	95 in 100	☐	or	70 in 100	30 in 100	☐
7)	5 in 100	95 in 100	☐	or	65 in 100	35 in 100	☐
8)	5 in 100	95 in 100	☐	or	60 in 100	40 in 100	☐
9)	5 in 100	95 in 100	☐	or	55 in 100	45 in 100	☐
10)	5 in 100	95 in 100	☐	or	50 in 100	50 in 100	☐
11)	5 in 100	95 in 100	☐	or	45 in 100	55 in 100	☐
12)	5 in 100	95 in 100	☐	or	40 in 100	60 in 100	☐
13)	5 in 100	95 in 100	☐	or	35 in 100	65 in 100	☐
14)	5 in 100	95 in 100	☐	or	30 in 100	70 in 100	☐
15)	5 in 100	95 in 100	☐	or	25 in 100	75 in 100	☐
16)	5 in 100	95 in 100	☐	or	20 in 100	80 in 100	☐
17)	5 in 100	95 in 100	☐	or	15 in 100	85 in 100	☐
18)	5 in 100	95 in 100	☐	or	10 in 100	90 in 100	☐
19)	5 in 100	95 in 100	☐	or	5 in 100	95 in 100	☐
20)	5 in 100	95 in 100	☐	or	1 in 100	99 in 100	☐
	5 in 100	95 in 100	☐	or	0 in 100	100 in 100	☑

3.6.4 Sample Holt-Laury Task

On this page you will make a series of decisions between two uncertain options. Option A involves payments of \$5.20 and \$4.15. Option B involves payments of \$10 and \$0.26. As you proceed, both Option A and Option B will change. For Option A, the chance of receiving \$5.20 will increase and the chance of receiving \$4.15 will decrease. For Option B, the chance of receiving \$10 will increase, while the chance of receiving \$0.26 will decrease.

For each row, all you have to do is decide whether you prefer Option A or Option B. Indicate your preference, by checking the corresponding box.

	Option A			*or*	Option B		
	Chance of $5.20	Chance of $4.15			Chance of $10	Chance of $0.26	
	0 in 100	100 in 100	☑	*or*	0 in 100	100 in 100	☐
1)	10 in 100	90 in 100	☐	*or*	10 in 100	90 in 100	☐
2)	20 in 100	80 in 100	☐	*or*	20 in 100	80 in 100	☐
3)	30 in 100	70 in 100	☐	*or*	30 in 100	70 in 100	☐
4)	40 in 100	60 in 100	☐	*or*	40 in 100	60 in 100	☐
5)	50 in 100	50 in 100	☐	*or*	50 in 100	50 in 100	☐
6)	60 in 100	40 in 100	☐	*or*	60 in 100	40 in 100	☐
7)	70 in 100	30 in 100	☐	*or*	70 in 100	30 in 100	☐
8)	80 in 100	20 in 100	☐	*or*	80 in 100	20 in 100	☐
9)	90 in 100	10 in 100	☐	*or*	90 in 100	10 in 100	☐
	100 in 100	0 in 100	☐	*or*	100 in 100	0 in 100	☑

3.6.5 Sample Certainty Equivalents

On this page you will make a series of decisions between two options. Option A will be a 50 in 100 chance of $30 and a 50 in 100 chance of $0. Option B will vary across decisions. Initially, Option B will be a $0.50 for sure. As you proceed down the rows, Option B will change. The sure amount will increase.

For each row, all you have to do is decide whether you prefer Option A or Option B.

	Option A			*or*	Option B	
	Chance of $30	Chance of $0			Sure Amount	
	50 in 100	50 in 100	☑	*or*	$0.00 for sure	☐
1)	50 in 100	50 in 100	☐	*or*	$0.50 for sure	☐
2)	50 in 100	50 in 100	☐	*or*	$1.00 for sure	☐
3)	50 in 100	50 in 100	☐	*or*	$1.50 for sure	☐
4)	50 in 100	50 in 100	☐	*or*	$2.50 for sure	☐
5)	50 in 100	50 in 100	☐	*or*	$3.50 for sure	☐
6)	50 in 100	50 in 100	☐	*or*	$4.50 for sure	☐
7)	50 in 100	50 in 100	☐	*or*	$6.50 for sure	☐
8)	50 in 100	50 in 100	☐	*or*	$8.50 for sure	☐
9)	50 in 100	50 in 100	☐	*or*	$10.50 for sure	☐
10)	50 in 100	50 in 100	☐	*or*	$13.50 for sure	☐
11)	50 in 100	50 in 100	☐	*or*	$16.50 for sure	☐
12)	50 in 100	50 in 100	☐	*or*	$19.50 for sure	☐
13)	50 in 100	50 in 100	☐	*or*	$21.50 for sure	☐
14)	50 in 100	50 in 100	☐	*or*	$23.50 for sure	☐
15)	50 in 100	50 in 100	☐	*or*	$25.50 for sure	☐
16)	50 in 100	50 in 100	☐	*or*	$26.50 for sure	☐
17)	50 in 100	50 in 100	☐	*or*	$27.50 for sure	☐
18)	50 in 100	50 in 100	☐	*or*	$28.50 for sure	☐
19)	50 in 100	50 in 100	☐	*or*	$29.00 for sure	☐
20)	50 in 100	50 in 100	☐	*or*	$29.50 for sure	☐
	50 in 100	50 in 100	☐	*or*	$30.00 for sure	☑

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Chapter 4

An Endowment Effect for Risk: Experimental Tests of Stochastic Reference Points

Abstract

The endowment effect has been widely documented. Recent models of reference dependent preferences indicate that expectations play a prominent role in the presence of the phenomenon. A subset of such expectations-based models predicts an endowment effect for risk when reference points change from certain to stochastic. In two purposefully simple risk preference experiments, eliminating often-discussed confounds, I demonstrate both between and within-subjects an endowment effect for risky gambles. While subjects are virtually risk neutral when choosing between fixed gambles and changing certain amounts, a high degree of risk aversion is displayed when choosing between fixed amounts and changing gambles. These results provide needed separation between expectations-based reference-dependent models, allow for evaluation of recent theoretical extensions, and may help to close a long-standing debate in decision science on inconsistency between probability and certainty equivalent methodology for utility elicitation.

4.1 Introduction

The endowment effect refers to the frequent finding in both experimental and survey research that willingness to pay (*WTP*) for a given object is generally lower than willingness to accept (*WTA*) for the same good.[1] Though standard economics argues the two values should be equal apart from income effects, differences between *WTA* and *WTP* have been documented across a variety of contexts from public services and environmental protection to private goods and hunting licences (Thaler, 1980; Knetsch and Sinden, 1984; Brookshire and Coursey, 1987; Coursey et al., 1987; Knetsch, 1989; Kahneman et al., 1990; Harbaugh et al., 2001). Horowitz and McConnell (2002) provide a survey of 50 studies and find a median ratio of mean *WTA* to mean *WTP* of 2.6.

The endowment effect has been cited as a key example of loss aversion relative to a reference point (Knetsch et al., 2001). Reference-dependent preferences with disproportionate treatment of losses predicts sizable differences between *WTA* and *WTP*. If losses are felt more severely than commensurate gains, paying for a good one does not own involves incurring monetary loss, reducing *WTP*. Meanwhile, giving up a good one does own involves incurring physical loss, increasing *WTA*. The preference structure of loss aversion drives a wedge between the two values, resulting in $WTA > WTP$.

Theoretical models of reference-dependent preferences with asymmetric treatment of losses originated in the prospect theory work of Kahneman and Tversky (1979). These models have gained traction, rationalizing not only the endowment effect, but also a number of other important anomalies from labor market decisions (Camerer et al., 1997; Goette and Fehr, 2007), to consumer behavior (Hardie et al., 1993; Sydnor, Forthcoming), and finance (Odean, 1998; Barberis and Huang, 2001; Barberis et al., 2001), among others.

Critical to reference-dependent models is the determination of the reference point around which losses and gains are encoded. Originally, the reference point was left undetermined, taken to be the status quo, current level of assets, or a level of

[1] Though I will refer to WTP and WTA as exchanging money for goods, these terms can also be thought of as willingness to exchange goods for goods such as mugs for pens and vice versa.

aspiration or expectation (Kahneman and Tversky, 1979). Indeed, the freedom of the reference point may be the reason why reference dependence is able to rationalize such a large amount of behavior. Model extensions have added discipline. Particular attention has been given to expectations-based mechanisms for the determination of fixed reference points in models of Disappointment Aversion (DA) (Bell, 1985; Loomes and Sugden, 1986; Gul, 1991), or for the determination of stochastic reference distributions in the more recent models of Koszegi and Rabin (2006, 2007) (KR).[2] In DA the referent is modeled as the expected utility certainty equivalent of a gamble, while in the KR model the referent is the full distribution of expected outcomes.

The DA and KR models provide coherent structure for the determination of reference points, and have found support in a number of studies. A recent body of field and laboratory evidence has highlighted the importance of expectations for reference-dependent behavior (Post et al., 2008; Ericson and Fuster, 2009; Gill and Prowse, 2010; Pope and Schweitzer, Forthcoming; Crawford and Meng, Forthcoming; Abeler et al., Forthcoming; Card and Dahl, Forthcoming). Additionally, Koszegi and Rabin (2006) and Knetsch and Wong (2009) argue that a sensible account of expectations may help to organize the discussion of the conditions under which the endowment effect is observed in standard exchange experiments (Plott and Zeiler, 2005, 2007).[3]

Though the accumulated data do demonstrate the importance of expectations for reference dependence, the data is generally consistent with either DA or the KR model and is often presented as such. That is, the body of evidence is unable to distinguish between the DA and KR models. Achieving such a distinction is critical for evaluating applications of the two models in a variety of settings where their

[2]Disappointment Aversion can refer to a number of different classes of models. I focus primarily on Bell (1985) and Loomes and Sugden (1986) who capture disappointment aversion in functional form by fixing the referent as the certainty equivalent of a given gamble and develop a reference-dependent disappointment-elation function around this point. Shalev (2000) provides a similar functional form in a loss-averse game-theoretic context with the reference point fixed at a gamble's certainty equivalent. Though similar in spirit to these models, Gul (1991) provides a distinct axiomatic foundation for disappointment aversion relaxing the independence axiom. The resulting representation's functional form is similar to prospect theory probability weighting (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992) with disappointment aversion making a particular global restriction on the shape of the probability weighting function (Abdellaoui and Bleichrodt, 2007).

[3]Expectations of exchange may also help organize results such as documented differences in endowment-effect behavior between experienced traders and novices List (2003, 2004).

predictions differ. Such settings include, but are not limited to theoretical extensions, experimental anomalies, financial decision making and marketing.

This paper presents evidence from two experiments focused on identifying a particular prediction of the KR model which is not shared with disappointment aversion: an endowment effect for risk. The KR model predicts that when risk is expected, and therefore the referent is stochastic, behavior will be different from when risk is unexpected and the referent is certain. In particular, when the referent is stochastic, and an individual is offered a certain amount, the KR model predicts near risk neutrality. Conversely, when the referent is a fixed certain amount, and an individual is offered a gamble, the KR model predicts risk aversion. Hence, the KR model features an endowment effect for risk. Disappointment aversion makes no such asymmetric prediction as to the relationship between risk attitudes and reference points, because gambles are always evaluated relative to a fixed referent, the gamble's certainty equivalent.[4]

Prior studies have provided only limited evidence on the critical KR prediction of an endowment effect for risk. Knetsch and Sinden (1984) demonstrate that a higher proportion of individuals are willing to pay $2 to keep a lottery ticket with unknown odds of winning around $50, than to accept $2 to give up the same lottery ticket if they already possess it. Kachelmeier and Shehata (1992) show that *WTA* for a 50%-50% gamble over $20 is significantly larger than subsequent *WTP* out of experimental earnings for the same gamble.

Though intriguing, these studies and others on the endowment effect suffer from potential experimental confounds. Plott and Zeiler (2005, 2007) discuss a variety of issues. In particular, they argue that when providing subjects with actual endowments via language, visual cues or physical cues, subjects may view the endowment as a gift and be unwilling to part with it. When using neutral language and elicitation procedures based on the Becker et al. (1964) mechanism, Plott and Zeiler (2005) document virtually no difference between *WTA* and *WTP* for university-branded mugs.[5]

[4]See Koszegi and Rabin (2007) for discussion.

[5]Plott and Zeiler (2005) discuss data from a series of small-scale paid practice lottery conditions, which they argue were contaminated by subject misunderstanding and order effects. Recently these data have been called into question as potentially demonstrating an endowment effect for small-stakes lotteries (Isoni et al., Forthcoming). However, the debate remains unresolved as to whether subject misunderstanding of the Becker et al. (1964) mechanism or other aspects of the experimental

Plott and Zeiler (2007) demonstrate, among other things, the extent to which the endowment effect could be related to subjects' interpretation of gift-giving. The authors increase and reduce emphasis on gifts and document corresponding increases and decreases in willingness to trade endowed mugs for pens, and vice versa.

Given the potential confounds of prior experimental methods, it is important to move away from the domain of physical endowments and ownership-related language. I present between and within-subjects results from simple, neutrally-worded experiments conducted with undergraduate students at the University of California, San Diego. In the primary experiment, 136 subjects were separated into two groups. Half of subjects were asked a series of certainty equivalents for given gambles. In each decision the gamble was fixed while the certain amount was changed. The other half were asked a series of probability equivalents for given certain amounts. In each decision the certain amount was fixed while the gamble probabilities were changed. In a second study, portions of the data collected for Andreoni and Sprenger (2010b) with an additional 76 subjects and a similar, within-subjects design are presented.

The results are striking. Both between and within-subjects virtual risk neutrality is obtained in the certainty equivalents data, while significant risk aversion is obtained in probability equivalents. In the primary study, subjects randomly assigned to probability equivalent conditions are between three and four times more likely to exhibit risk aversion than subjects assigned to certainty equivalent conditions. This result is maintained when controlling for socio-demographic characteristics, numeracy, cognitive ability and self-reported risk attitudes.

The between-subjects design of the primary study is complemented with additional uncertainty equivalents (McCord and de Neufville, 1986; Magat et al., 1996; Oliver, 2005, 2007; Andreoni and Sprenger, 2010b). Uncertainty equivalents ask subjects to choose between a given gamble and alternate gambles outside of the given gamble's outcome support. The KR preference model predicts risk aversion in this domain and risk neutrality in the inverse. That is, individuals should be risk averse when endowed with a gamble $(p; y, x)$, $y > x > 0$ and trading for gambles $(q; y, 0)$, but should be risk neutral when endowed with a gamble $(p; y, 0)$ and trading for gambles

procedure are the primary factors (Plott and Zeiler, Forthcoming).

$(q; y, x)$.[6] These predictions are generally supported.

Finding evidence of an endowment effect for risk, particularly in a neutral environment like that presented in these studies, provides support for the KR preference model. Unlike prior work demonstrating the importance of expectations for reference points, these results are able to distinguish between KR preferences and other expectations-based models such as disappointment aversion. Gaining separation between these models is an important experimental step and necessary for evaluating theoretical developments that depend critically on the stochasticity of the referent (Koszegi and Rabin, 2006, 2007; Heidhues and Koszegi, 2008; Koszegi and Rabin, 2009). Additionally, the distinction between the KR and DA models is important in a variety of applied settings where the two models make different predictions. In particular, the DA model predicts first order risk aversion in the sense of Segal and Spivak (1990) over all gambles, while the KR model predicts first order risk aversion only when risk is unexpected.[7] Applications include financial decisions where first order risk aversion is argued to influence stock market participation (Haliassos and Bertaut, 1995; Barberis et al., 2006) and returns (Epstein and Zin, 1990; Barberis and Huang, 2001); insurance purchasing where first order risk aversion potentially influences contract choice (Sydnor, Forthcoming), and decision science where researchers have long debated the inconsistency between probability equivalent and certainty equivalent methods for utility assessment (Hershey et al., 1982; McCord and de Neufville, 1985, 1986; Hershey and Schoemaker, 1985; Schoemaker, 1990).

In addition to providing techniques for modeling stochastic referents, Koszegi and Rabin (2006, 2007) propose a refinement of their model, the Preferred Personal Equilibrium (PPE), in which the referent is revealed by choice behavior. The PPE refinement predicts identical risk attitudes across the experimental conditions. Since the findings reject disappointment aversion, which predicts the same pattern, it necessarily rejects this refinement. A more likely non-PPE candidate for organizing the behavior is that the referent is established as the fixed element in a given series of decisions, which was always presented first. Koszegi and Rabin (2006) provide intuition in

[6]Standard theories and disappointment aversion again predict no difference in risk preference across this changing experimental environment.

[7]By unexpected I mean when risky outcomes lie outside the support of the referent. This is the case when the referent is fixed and when the referent is stochastic but outcomes are more variable.

this direction suggesting "a person's reference point is her probabilistic beliefs about the relevant consumption outcome held between the time she first focused on the decision determining the outcome and shortly before consumption occurs"[p. 1141]. "First focus" may plausibly be drawn to the fixed, first element in a series of decisions and the intuition is in line with both the psychological literature on "cognitive reference points" (Rosch, 1974)[8] and evidence from multi-person domains where behavior is organized around initial reactions to experimental environments (Camerer et al., 2004; Costa-Gomes and Crawford, 2006; Crawford and Iriberri, 2007; Costa-Gomes et al., 2009). The potential sensitivity of expectations-based referents to minor contextual changes has implications for both economic agents, such as marketers, and experimental methodology.

The paper proceeds as follows. Section 4.2 presents conceptual considerations for thinking about certainty and probability equivalents in standard theories, reference-dependent theories and the KR model. Section 4.3 presents experimental design and Section 4.4 presents results. Section 4.5 provides interpretation and discusses future avenues of research and Section 4.6 is a conclusion.

4.2 Conceptual Considerations

In this section several models of risk preferences are discussed. With one exception, the models predict equivalence of risk attitudes across certainty equivalents and probability equivalents. The exception is the KR model, which predicts an endowment effect for risk.

Consider expected utility. Any complete, transitive, continuous preference ordering over lotteries that also satisfies the independence axiom will be represented by a standard expected utility function, $v(\cdot)$, that is linear in probabilities. Under such preferences, a certainty equivalent for a given gamble will be established by a simple indifference condition. Take a binary p gamble over two positive values, y and

[8] Rosch (1974) describes a cognitive reference point as the stimulus "which other stimuli are seen 'in relation to'"[p. 532]. In the present studies this relationship is achieved by asking subjects to make repeated choices between the fixed decision element and changing alternatives.

$x \leq y$, $(p; y, x)$, and some certain amount, c, satisfying the indifference condition

$$v(c) = p \cdot v(y) + (1 - p) \cdot v(x).$$

Under expected utility, it will not matter whether risk preferences are elicited via the certainty equivalent, c, or the probability equivalent, p; the elicited level of risk aversion, or the curvature of $v(\cdot)$, should be identical. There should be no endowment effect for risk.

A similar argument can be made for reference-dependent prospect theory which establishes loss-averse utility levels relative to some fixed referent and relaxes the independence axiom's implied linearity in probability (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992; Tversky and Fox, 1995; Wu and Gonzalez, 1996; Prelec, 1998; Gonzalez and Wu, 1999; Abdellaoui, 2000; Bleichrodt and Pinto, 2000). Let $u(\cdot|r)$ represent loss-averse utility given some fixed referent, r. The cumulative prospect theory indifference condition is

$$u(c|r) = \pi(p) \cdot u(y|r) + (1 - \pi(p)) \cdot u(x|r),$$

where $\pi(\cdot)$ represents some arbitrary non-linear probability weighting function. Under such a utility formulation, certainty and probability equivalents again yield identical risk attitudes as the reference point is fixed at some known value.

Extensions to reference-dependent preferences have attempted to explain behavior by establishing what the reference point should actually be. Models of disappointment aversion fix the prospect theory reference point via expectations as a gamble's expected utility certainty equivalent (Bell, 1985; Loomes and Sugden, 1986). Disappointment aversion's fixed referent does not change the predicted equivalence of risk preferences across probability and certainty equivalents as gambles are always evaluated relative to their certainty equivalents. In effect, disappointment aversion selects r in the above indifference condition as the expected utility certainty equivalent, $p \cdot v(y) + (1 - p) \cdot v(x)$, and selects a linear probability weighting function.[9]

[9] See Bell (1985); Loomes and Sugden (1986) for the exact functional forms.

4.2.1 KR Preferences

The KR model builds upon standard reference-dependent preferences in two important ways. First, similar to disappointment aversion, the referent is expectations-based, and second, the referent may be stochastic. Together these innovations imply that behavior when risk is expected, and therefore the referent is stochastic, will be substantially different from when risk is unexpected, and the referent is certain. In particular, KR preferences as presented below predict risk neutrality in specific cases where the referent is stochastic and risk aversion in cases where the the referent is certain.

Let r represent the referent potentially drawn according to measure G. Let x be a consumption outcome potentially drawn according to measure F. Then the KR utility formulation is

$$U(F|G) = \iint u(x|r)dG(r)dF(x)$$

with

$$u(x|r) = m(x) + \mu(m(x) - m(r)).$$

The function $m(\cdot)$ represents consumption utility and $\mu(\cdot)$ represents gain-loss utility relative to the referent, r. Several simplifying assumptions are made. First, following Koszegi and Rabin (2006, 2007) small stakes decisions are considered such that consumption utility, $m(\cdot)$, can plausibly be taken as approximately linear, and a piecewise-linear gain-loss utility function is adopted,

$$\mu(y) = \left\{ \begin{array}{lll} \eta \cdot y & if & y \geq 0 \\ \eta \cdot \lambda \cdot y & if & y < 0 \end{array} \right\},$$

where the utility parameter λ represents the degree of loss aversion. For simplicity and to aid the exposition, $\eta = 1$ is assumed, and only binary lotteries are considered such that G and F will be binomial distributions summarized by probability values p and q, respectively.

Consider two cases, first where the referent is certain and consumption outcomes are stochastic, and second where the referent is stochastic and consumption outcomes are certain. The above KR model predicts risk averse behavior in the first case and risk neutrality in the second. This is illustrated next.

Probability Equivalent: Certain Referent, Binary Consumption Gamble

Consider a referent, r, and a binary consumption gamble with outcomes $x_1 \geq r$ with probability q and $x_2 \leq r$ with probability $1-q$.[10] Write the KR utility as

$$U(F|r) = q \cdot u(x_1|r) + (1-q) \cdot u(x_2|r).$$

The first term refers to the chance of expecting r as the referent and obtaining x_1 as the consumption outcome. The second term is similar for expecting r and obtaining x_2. If $x_1 \geq r > x_2$, the KR model predicts loss aversion to be present in the second term. Under the assumptions above, this becomes

$$U(F|r) = q \cdot [x_1 + 1 \cdot (x_1 - r)] + (1-q) \cdot [x_2 + \lambda \cdot (x_2 - r)]. \tag{4.2}$$

Compare this to the utility of the certain amount, $U(r|r) = r$. The lottery will be preferred to the certain referent if $U(F|r) > U(r|r)$ and the indifference point, or *probability equivalent*, will be obtained for some F^*, with corresponding probability q^*, such that $U(F^*|r) = U(r|r)$,

$$r = q^* \cdot [x_1 + 1 \cdot (x_1 - r)] + (1-q^*) \cdot [x_2 + \lambda \cdot (x_2 - r)];$$

$$q^* = \frac{r - x_2 - \lambda \cdot (x_2 - r)}{[x_1 - x_2] + [1 \cdot (x_1 - r) - \lambda \cdot (x_2 - r)]}. \tag{4.3}$$

The interpretation of the relationship between risk aversion elicited as q^* and loss aversion, λ, is straightforward. For an individual who is not loss averse, $\lambda = 1$, $q^* = (r - x_2)/(x_1 - x_2)$. This equates the expected value of the probability

[10] I assume $x_2 \geq 0$ and that at least one of the inequalities is strict such that consumption gamble is non-degenerate.

equivalent and the referent value, $r = q^* \cdot x_1 + (1 - q^*) \cdot x_2$. Risk neutral behavior is exhibited by individuals who are not loss averse.

For loss averse individuals with $\lambda > 1$, $q^* > (r - x_2)/(x_1 - x_2)$ for $x_1 > r > x_2 \geq 0$. The gamble F^* will have higher expected value than r. Figure 4.1, Panel A illustrates the decision for a loss averse individual. Additionally $dq^*/d\lambda > 0$ for $x_1 > r > x_2 \geq 0$, such that probability equivalents are increasing in the degree of loss aversion.[11] If endowed with a fixed amount in a probability equivalent task and trading for a gamble, a loss-averse individual will appear risk averse.

Note that as x_1 approaches r, then q^* approaches 1, and as x_2 approaches r, then q^* approaches 0. Hence q^* will accord with the risk neutral level, $(r - x_2)/(x_1 - x_2)$, at the limits $x_1 = r$ and $x_2 = r$. This implies a hump shaped deviation between q^* and the risk neutral level of q if $\lambda > 1$.

[11] The derivative $dq^*/d\lambda = \frac{-(x_2 - r)\cdot(2x_1 - 2_r)}{[x_1 - x_2 + 1\cdot(x_1 - r) - \lambda\cdot(x_2 - r)]^2} > 0$ for $x_1 > r > x_2 \geq 0$.

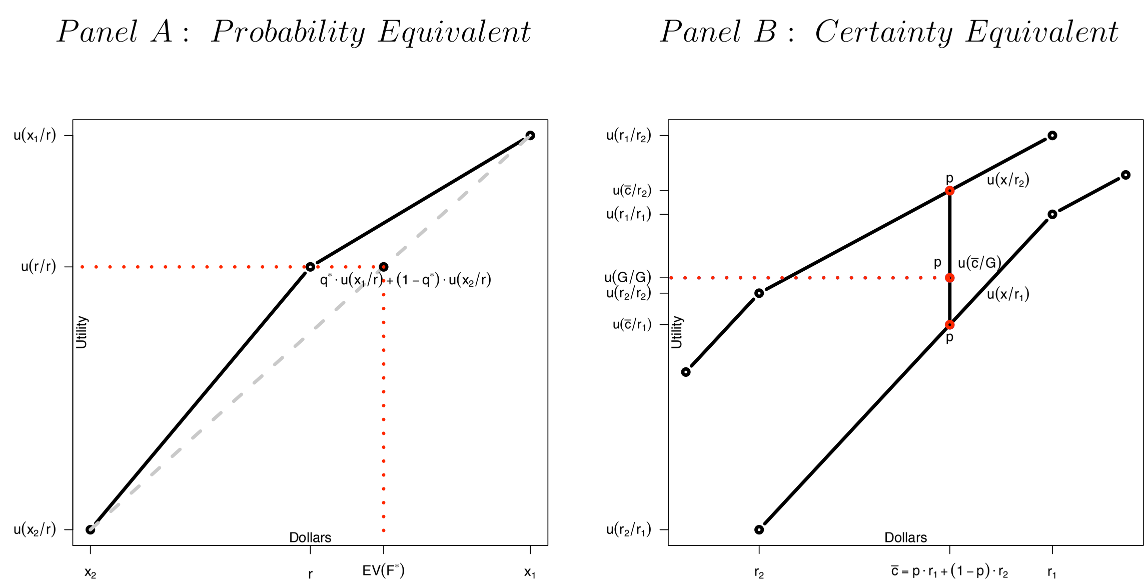

Figure 4.1: KR Probability and Certainty Equivalents

Note: The figure illustrates probability and certainty equivalents under KR preferences. For probability equivalents in Panel A, the KR model predicts apparent risk averse behavior as the expected value of the probability equivalent gamble, $EV(F^*)$, is greater than the referent, r. For certainty equivalents in Panel B, KR predicts risk neutrality at all levels of loss aversion as the utility of the expected value of a gamble, $u(\bar{c}|G)$, is equal to the value of the gamble, $u(G|G)$.

Certainty Equivalent: Binary Referent Gamble, Certain Consumption

Now consider a binary referent gamble and the prospect of certain consumption. Let r_1 be the referent with probability p and $r_2 \le r_1$ be the referent with probability $1-p$. The utility of the binary referent gamble is

$$U(G|G) = p \cdot p \cdot u(r_1|r_1) + (1-p) \cdot (1-p) \cdot u(r_2|r_2) + p \cdot (1-p) \cdot u(r_1|r_2) + p \cdot (1-p) \cdot u(r_2|r_1).$$

The first term refers to the chance of expecting r_1 as the referent and obtaining r_1 as the consumption outcome. The second term is similar for r_2. The third term refers to the chance of expecting r_2 as the referent and obtaining r_1 as the consumption outcome. The fourth term refers to the chance of expecting r_1 as the referent and obtaining r_2 as the consumption outcome. With $r_1 \ge r_2$, the KR model predicts loss aversion to be present in the fourth term. Under the assumed utility formulation this reduces to

$$U(G|G) = p^2 \cdot r_1 + (1-p)^2 \cdot r_2 + p \cdot (1-p) \cdot [r_1 + 1 \cdot (r_1 - r_2)] + p \cdot (1-p) \cdot [r_2 + \lambda \cdot (r_2 - r_1)];$$

$$U(G|G) = p \cdot r_1 + (1-p) \cdot r_2 + p \cdot (1-p) \cdot [1 \cdot (r_1 - r_2) + \lambda \cdot (r_2 - r_1)].$$

Given this stochastic referent, consider the utility of a certain outcome, x, with $r_1 \ge x \ge r_2$,

$$U(x|G) = p \cdot u(x|r_1) + (1-p) \cdot u(x|r_2),$$

$$U(x|G) = x + p \cdot [\lambda \cdot (x - r_1)] + (1-p) \cdot [1 \cdot (x - r_2)].$$

The indifference point, or *certainty equivalent* c, is obtained for $U(c|G) = u(G|G)$,

$$p \cdot r_1 + (1-p) \cdot r_2 + p \cdot (1-p) \cdot [1 \cdot (r_1 - r_2) + \lambda \cdot (r_2 - r_1)] = c + p \cdot [\lambda \cdot (c - r_1)] + (1-p) \cdot [1 \cdot (c - r_2)].$$

To demonstrate that individuals will be risk neutral in certainty equivalent decisions, one need only establish the expected value as the risk neutral benchmark, $\bar{c} = p \cdot r_1 + (1-p) \cdot r_2$. Substituting $c = \bar{c}$ in the right hand side of the above equation, one obtains

$$p \cdot r_1 + (1-p) \cdot r_2 + p \cdot [\lambda(p \cdot r_1 + (1-p) \cdot r_2 - r_1)] + (1-p) \cdot [1(p \cdot r_1 + (1-p) \cdot r_2 - r_2)],$$

which reduces to

$$p \cdot r_1 + (1-p) \cdot r_2 + p \cdot (1-p) \cdot [1(r_1 - r_2) + \lambda(r_2 - r_1)],$$

and is identical to the left hand side of the above equation. Hence, indifference occurs at the risk neutral benchmark, $c = \bar{c} = p \cdot r_1 + (1-p) \cdot r_2$. Figure 4.1, Panel B illustrates the certainty equivalent of a gamble as the gamble's expected value, $\bar{c}$. If endowed with a gamble in a certainty equivalent task and trading for a fixed amount, a loss-averse individual will appear risk neutral, regardless of the level of loss aversion. This is in contrast to probability equivalents where loss-averse individuals will appear risk averse.

Equilibrium Behavior

Koszegi and Rabin (2006, 2007) present a rational expectations equilibrium concept, the Unacclimating Personal Equilibrium (UPE), in which consumption outcomes correspond to expectations. The objective of the UPE concept is to represent the notion that rational individuals will only expect consumption outcomes that they will definitely consume given the expectation of said consumption outcomes. To select among the potential multiplicity of such equilibria, the KR model features a refinement, the Preferred Personal Equilibrium (PPE). The PPE concept maintains that the UPE with the highest ex-ante expected utility is selected.[12]

The development above demonstrates that KR preferences may allow for a difference in elicited risk behavior between certainty equivalents and probability equivalents. However, this difference is not predicted under PPE. The probability equivalent,

[12] Another equilibrium concept in Koszegi and Rabin (2006, 2007) is the Choice-acclimating Personal Equilibrium (CPE) which applies to decisions made far in advance of the resolution of uncertainty. In the present context CPE and PPE have similar implications, as both are based on the coincidence of referent and consumption outcomes.

$U(F^*|r)$, and the certainty equivalent, $U(c|G)$, are not UPE values as the referent and consumption outcomes do not coincide.

If the referent is revealed in choice behavior, then when an individual is observed accepting some gamble, F^*, over some fixed amount, r, the PPE concept establishes only that $U(F^*|F^*) > U(r|r)$. That is, $(F^*|F^*)$ provided the higher ex-ante expected utility. If $U(F^*|F^*) > U(r|r)$ is the PPE revealed preference in a probability equivalent, then it cannot be that the opposite is revealed in a certainty equivalent. Under PPE, the KR model predicts no difference between certainty equivalents and probability equivalents. However, equilibrium behavior may be a challenging requirement. Individuals may naively change their referent in accordance with changes in contextual variables. Koszegi and Rabin (2006) provide intuitive support for such naivete suggesting that the referent is established as the probabilistic beliefs held at the moment of "first focus" on a decision. To the extent that first focus is drawn to different aspects of decisions, one might expect very similar decisions in theory to induce different probabilistic referents in practice. The experimental design is indeed predicated on the notion that minor changes in experimental context, particularly what element is fixed and presented first in a decision environment, can effectively change the perceived referent.

4.3 Experimental Design

Motivated by the conceptual development above, a primary between-subject two condition experiment was designed. A secondary within-subjects design with similar methods and data from Andreoni and Sprenger (2010b) is discussed in Section 4.4.3. In Condition 1, subjects completed two series of probability equivalents tasks. The tasks were designed in price-list style with 21 decision rows in each task. Each decision row was a choice between 'Option A', a certain amount, and 'Option B', an uncertain gamble. The certain Option A was fixed for each task, as were the gamble outcomes. The probability of receiving the gamble's good outcome increased from 0% to 100% as subjects proceeded through the task. In Condition 1.1, subjects completed 8 tasks with fixed certain amounts chosen from $\{\$6, \$8, \$10, \$14, \$17, \$20, \$23, \$26\}$

and gambles over \$30 and \$0. In Condition 1.2, subjects completed 6 tasks with fixed certain amounts chosen from $\{\$12, \$14, \$17, \$20, \$23, \$26\}$ and gambles over \$30 and \$10. Most subjects began each task by preferring Option A and then switched to Option B such that the probability at which a subject switches from Option A to Option B provides bounds for their probability equivalent. Figure 4.2, Panel A features a sample probability equivalent task. If the fixed Option A element in each task is perceived as the referent, the KR model predicts risk aversion in these probability equivalents.

In Condition 2, subjects completed two series of certainty equivalents tasks. The tasks were similarly designed in price-list style with 22 decision rows in each task. Each decision row was a choice between 'Option A', a gamble, and 'Option B', a certain amount. The Option A gamble was fixed for each task. The certain amount increased as subjects proceeded down the task. In Condition 2.1, subjects completed 7 tasks with gamble outcomes of \$30 and \$0, probabilities chosen from $\{0.05, 0.10, 0.25, 0.50, 0.75, 0.90, 0.95\}$, and certain amounts ranging from \$0 to \$30. In Condition 2.2 subjects completed a further 7 tasks with gamble outcomes of \$30

Panel A: Probability Equivalent

	Option A			Option B		
	Certain Payment of $10		*or*	Chance of $0	Chance of $30	
1)	$10	☐	*or*	100 in 100	0 in 100	☐
2)	$10	☐	*or*	95 in 100	5 in 100	☐
3)	$10	☐	*or*	90 in 100	10 in 100	☐
4)	$10	☐	*or*	85 in 100	15 in 100	☐
5)	$10	☐	*or*	80 in 100	20 in 100	☐
6)	$10	☐	*or*	75 in 100	25 in 100	☐
7)	$10	☐	*or*	70 in 100	30 in 100	☐
8)	$10	☐	*or*	65 in 100	35 in 100	☐
9)	$10	☐	*or*	60 in 100	40 in 100	☐
10)	$10	☐	*or*	55 in 100	45 in 100	☐
11)	$10	☐	*or*	50 in 100	50 in 100	☐
12)	$10	☐	*or*	45 in 100	55 in 100	☐
13)	$10	☐	*or*	40 in 100	60 in 100	☐
14)	$10	☐	*or*	35 in 100	65 in 100	☐
15)	$10	☐	*or*	30 in 100	70 in 100	☐
16)	$10	☐	*or*	25 in 100	75 in 100	☐
17)	$10	☐	*or*	20 in 100	80 in 100	☐
18)	$10	☐	*or*	15 in 100	85 in 100	☐
19)	$10	☐	*or*	10 in 100	90 in 100	☐
20)	$10	☐	*or*	5 in 100	95 in 100	☐
21)	$10	☐	*or*	0 in 100	100 in 100	☐

Panel B: Certainty Equivalent

	Option A			*or*	Option B	
	Chance of $0	Chance of $30			Certain Amount	
1)	50 in 100	50 in 100	☐	*or*	$0.00 with certainty	☐
2)	50 in 100	50 in 100	☐	*or*	$0.50 with certainty	☐
3)	50 in 100	50 in 100	☐	*or*	$1.00 with certainty	☐
4)	50 in 100	50 in 100	☐	*or*	$1.50 with certainty	☐
5)	50 in 100	50 in 100	☐	*or*	$2.50 with certainty	☐
6)	50 in 100	50 in 100	☐	*or*	$3.50 with certainty	☐
7)	50 in 100	50 in 100	☐	*or*	$4.50 with certainty	☐
8)	50 in 100	50 in 100	☐	*or*	$6.50 with certainty	☐
9)	50 in 100	50 in 100	☐	*or*	$8.50 with certainty	☐
10)	50 in 100	50 in 100	☐	*or*	$10.50 with certainty	☐
11)	50 in 100	50 in 100	☐	*or*	$13.50 with certainty	☐
12)	50 in 100	50 in 100	☐	*or*	$16.50 with certainty	☐
13)	50 in 100	50 in 100	☐	*or*	$19.50 with certainty	☐
14)	50 in 100	50 in 100	☐	*or*	$21.50 with certainty	☐
15)	50 in 100	50 in 100	☐	*or*	$23.50 with certainty	☐
16)	50 in 100	50 in 100	☐	*or*	$25.50 with certainty	☐
17)	50 in 100	50 in 100	☐	*or*	$26.50 with certainty	☐
18)	50 in 100	50 in 100	☐	*or*	$27.50 with certainty	☐
19)	50 in 100	50 in 100	☐	*or*	$28.50 with certainty	☐
20)	50 in 100	50 in 100	☐	*or*	$29.00 with certainty	☐
21)	50 in 100	50 in 100	☐	*or*	$29.50 with certainty	☐
22)	50 in 100	50 in 100	☐	*or*	$30.00 with certainty	☐

Figure 4.2: Probability and Certainty Equivalents (Conditions 1.1 and 2.1)

and \$10, probabilities chosen from $\{0.05, 0.10, 0.25, 0.50, 0.75, 0.90, 0.95\}$, and certain amounts ranging from \$10 to \$30. Most subjects began each task by preferring Option A and then switched to Option B such that the certain value at which a subject switched from Option A to Option B provides bounds for their certainty equivalent. Figure 4.2, Panel B features a sample certainty equivalent task. If the fixed Option A element in each task is perceived as the referent, the KR model predicts risk neutrality in these certainty equivalents.

4.3.1 Additional Measures

The probability and certainty equivalents tasks of Conditions 1.1, 1.2, 2.1 and 2.2 provide a simple comparison of elicited risk attitudes. This design is complemented with a third set of tasks for which the KR model can also predict experimental differences. Condition 1.3, completed by subjects assigned to Condition 1, was a series of 8 uncertainty equivalent tasks with 21 decision rows in each task. Option A was a fixed gamble over \$30 and \$10 with probabilities chosen from $\{0.00, 0.05, 0.10, 0.25, 0.50, 0.75, 0.90, 0.95\}$. Option B was a changing gamble over \$30 and \$0. Condition 2.3, completed by subjects assigned to Condition 2, was a series of inverted uncertainty equivalent tasks with 21 decision rows in each task. Option A was a fixed gamble over \$30 and \$0 with probabilities chosen from $\{0.35, 0.40, 0.50, 0.60, 0.75, 0.85, 0.90, 0.95\}$. Option B was a changing gamble over \$30 and \$10. Figure 4.3, Panels A and B provide a sample uncertainty equivalent and the inverse.

The KR preference model can predict a marked difference in elicited risk attitudes across Conditions 1.3 and 2.3 if the referent is perceived as the fixed element in each task. The KR model predicts a particular shape of quadratically declining risk aversion in Condition 1.3, the standard uncertainty equivalent. The reason is that at the lowest probability, 0, the task is identical to a probability equivalent of

Panel A : Uncertainty Equivalent

	Option A			*or*	Option B		
	Chance of $10	Chance of $30			Chance of $0	Chance of $30	
1)	50 in 100	50 in 100	☐	*or*	100 in 100	0 in 100	☐
2)	50 in 100	50 in 100	☐	*or*	95 in 100	5 in 100	☐
3)	50 in 100	50 in 100	☐	*or*	90 in 100	10 in 100	☐
4)	50 in 100	50 in 100	☐	*or*	85 in 100	15 in 100	☐
5)	50 in 100	50 in 100	☐	*or*	80 in 100	20 in 100	☐
6)	50 in 100	50 in 100	☐	*or*	75 in 100	25 in 100	☐
7)	50 in 100	50 in 100	☐	*or*	70 in 100	30 in 100	☐
8)	50 in 100	50 in 100	☐	*or*	65 in 100	35 in 100	☐
9)	50 in 100	50 in 100	☐	*or*	60 in 100	40 in 100	☐
10)	50 in 100	50 in 100	☐	*or*	55 in 100	45 in 100	☐
11)	50 in 100	50 in 100	☐	*or*	50 in 100	50 in 100	☐
12)	50 in 100	50 in 100	☐	*or*	45 in 100	55 in 100	☐
13)	50 in 100	50 in 100	☐	*or*	40 in 100	60 in 100	☐
14)	50 in 100	50 in 100	☐	*or*	35 in 100	65 in 100	☐
15)	50 in 100	50 in 100	☐	*or*	30 in 100	70 in 100	☐
16)	50 in 100	50 in 100	☐	*or*	25 in 100	75 in 100	☐
17)	50 in 100	50 in 100	☐	*or*	20 in 100	80 in 100	☐
18)	50 in 100	50 in 100	☐	*or*	15 in 100	85 in 100	☐
19)	50 in 100	50 in 100	☐	*or*	10 in 100	90 in 100	☐
20)	50 in 100	50 in 100	☐	*or*	5 in 100	95 in 100	☐
21)	50 in 100	50 in 100	☐	*or*	0 in 100	100 in 100	☐

Panel B : Inverted Uncertainty Equivalent

	Option A			*or*	Option B		
	Chance of $0	Chance of $30			Chance of $10	Chance of $30	
1)	50 in 100	50 in 100	☐	*or*	100 in 100	0 in 100	☐
2)	50 in 100	50 in 100	☐	*or*	95 in 100	5 in 100	☐
3)	50 in 100	50 in 100	☐	*or*	90 in 100	10 in 100	☐
4)	50 in 100	50 in 100	☐	*or*	85 in 100	15 in 100	☐
5)	50 in 100	50 in 100	☐	*or*	80 in 100	20 in 100	☐
6)	50 in 100	50 in 100	☐	*or*	75 in 100	25 in 100	☐
7)	50 in 100	50 in 100	☐	*or*	70 in 100	30 in 100	☐
8)	50 in 100	50 in 100	☐	*or*	65 in 100	35 in 100	☐
9)	50 in 100	50 in 100	☐	*or*	60 in 100	40 in 100	☐
10)	50 in 100	50 in 100	☐	*or*	55 in 100	45 in 100	☐
11)	50 in 100	50 in 100	☐	*or*	50 in 100	50 in 100	☐
12)	50 in 100	50 in 100	☐	*or*	45 in 100	55 in 100	☐
13)	50 in 100	50 in 100	☐	*or*	40 in 100	60 in 100	☐
14)	50 in 100	50 in 100	☐	*or*	35 in 100	65 in 100	☐
15)	50 in 100	50 in 100	☐	*or*	30 in 100	70 in 100	☐
16)	50 in 100	50 in 100	☐	*or*	25 in 100	75 in 100	☐
17)	50 in 100	50 in 100	☐	*or*	20 in 100	80 in 100	☐
18)	50 in 100	50 in 100	☐	*or*	15 in 100	85 in 100	☐
19)	50 in 100	50 in 100	☐	*or*	10 in 100	90 in 100	☐
20)	50 in 100	50 in 100	☐	*or*	5 in 100	95 in 100	☐
21)	50 in 100	50 in 100	☐	*or*	0 in 100	100 in 100	☐

Figure 4.3: Uncertainty Equivalents (Conditions 1.3 and 2.3)

\$10 for sure. As discussed in Section 4.2.1, risk aversion is predicted. From there, both the referent and the outcomes are stochastic such that the uncertainty equivalent $(q; 30, 0)$ for a given gamble $(p; 30, 10)$ will be a convex function of p related to the squared probability, p^2.

The deviation from linearity depends on the degree of loss aversion λ and Appendix Section 4.7.1 provides the mathematical detail. This is in contrast to the prediction of expected utility where q should be a linear function p. This is also in contrast to cumulative prospect theory probability weighting (Tversky and Kahneman, 1992) where q is predicted to be a concave function of p.[13]

Interestingly, the KR model predicts risk neutrality in Condition 2.3, the inverted uncertainty equivalent. The logic is as follows: having prospective gamble outcomes inside of the support of the referent gamble is similar to having a perturbed certainty equivalent task. Just as risk neutrality is predicted in certainty equivalents, the KR model also predicts risk neutrality in the inverted uncertainty equivalents. Appendix Section 4.7.1 again provides the mathematical detail.

4.3.2 Design Details

In order to eliminate often-discussed confounds (Plott and Zeiler, 2005, 2007), neutral language such as 'Option A' and 'Option B' was used throughout. Subjects were never told that they were trading nor was exchange ever mentioned in the instructions. Subjects were told,

In each task you are asked to make a series of decisions between two options: Option A and Option B. In each task Option A will be fixed while Option B will vary. For example, ... [EXAMPLE].... For each row all you have to do is decide whether you prefer Option A or Option B.

The full instructions are provided as Appendix Section 4.7.3. In each condition, the decisions were blocked into tasks corresponding to the three sub-conditions

[13]See Andreoni and Sprenger (2010b) for a discussion of uncertainty equivalents and their value in separating between competing models of risk preferences.

discussed above. New instructions were read at the beginning of each task block explaining the new procedures and encouraging subjects to take each decision carefully. Subjects were provided with calculators should they wish to use them in making their decisions.

Two orders of the tasks were used in each condition to examine order effects: X.1, X.3, X.2 and X.2, X.3, X.1. The uncertainty equivalents were left in the middle as a buffer between the more similar tasks. No order effects were observed. In addition to varying the order, an attempt was also made to manipulate slightly the physical representation of Option A in each decision. This was done for around half of subjects by stapling miniature copies of the appropriate number of bills, or bills with appropriate percentages at the top of each decision sheet. The stapling was done such that subjects would be forced to hold the representation of Option A in order to make the first few decisions. Though I imagined that this nuance might influence the degree of attachment to Option A, it had virtually no effect.[14] A total of 136 subjects participated in the study across 10 experimental sessions. Table 4.1 provides the dates, times, orders and details of all sessions.

Table 4.1: Experimental Sessions

Number	Date	Time	Condition	Order	Representation	# Obs
1	May 11, 2010	12:00 pm	1	(1) X.1, X.3, X.2	No	10
2	May 11, 2010	2:30 pm	1	(2) X.2, X.3, X.1	No	12
3	May 12, 2010	12:00 pm	2	(1) X.1, X.3, X.2	No	19
4	May 12, 2010	2:30 pm	2	(2) X.2, X.3, X.1	No	16
5	May 18, 2010	12:00 pm	1	(1) X.1, X.3, X.2	No	11
6	May 18, 2010	2:30 pm	1	(2) X.2, X.3, X.1	No	6
7	May 25, 2010	12:00 pm	1	(1) X.1, X.3, X.2	Yes	15
8	May 25, 2010	2:30 pm	1	(2) X.2, X.3, X.1	Yes	16
9	May 26, 2010	12:00 pm	2	(1) X.1, X.3, X.2	Yes	15
10	May 26, 2010	2:30 pm	2	(2) X.2, X.3, X.1	Yes	16
Total						136

Notes: 'Representation' refers to whether or not Option A was physically represented by stapling miniature bills or bills and percentages to the decision sheet.

In order to provide incentive for truthful revelation of preferences, subjects

[14] Andreoni and Sprenger (2010b) use uncertainty equivalents to test expected utility and investigate violations of first order stochastic dominance near to certainty. In the non-representation treatments for Condition 1.3 the findings are reproduced. However, in the representation treatments for Condition 1.3, stochastic dominance violations at certainty are reduced to zero. See Sections 4.4.2 and 4.4.3 for discussion.

were randomly paid for one of their choices.[15] The instructions fully described the payment procedure and the mechanism for carrying out randomization of payments, two ten-sided die. The randomization was described in independent terms. That is, mention was made of rolling die first for Option A and then for Option B and an example was given. Subjects earned, on average, \$23 from the study including a \$5 minimum payment that was added to all experimental earnings.

4.4 Results

The results are presented in three sub-sections. The first sub-section provides a brief summary of the elicited risk attitudes across the two conditions and non-parametric tests demonstrating risk aversion in the probability equivalent tasks and virtual risk neutrality in the certainty equivalent tasks. Second, motivated by these non-parametric results, the KR utility model is estimated and compelling out-of-sample predictions for uncertainty equivalent tasks at both the aggregate and individual level are provided. The third sub-section is devoted to discussing within-subjects results with data from Andreoni and Sprenger (2010b), which also demonstrate significant differences in elicited risk preferences between certainty and probability equivalent techniques.

4.4.1 Risk Attitudes

Of the 136 individuals who participated in the primary experiment, 70 individuals participated in Condition 1 and 66 participated in Condition 2. As in most price-list style experiments, a number of subjects switch from Option A to Option B and then back to Option A.[16] Three subjects (4.3%) in Condition 1 and eleven

[15]This randomization device introduces a compound lottery to the decision environment as each individual made around 440 choices over their 22 tasks. Reduction of compound lotteries does not change the general equivalence predictions for standard expected utility, prospect theory and disappointment aversion discussed above. However, to the extent that the compound lottery changes perceived referents, the randomization introduces complications into the KR analysis as it creates a potential link between choices and referents across tasks. See Section 4.4.1 for further discussion.

[16]Around 10 percent of subjects feature multiple switch points in similar price-list experiments (Holt and Laury, 2002; Meier and Sprenger, 2010), and as many as 50 percent in some cases (Jacobson and Petrie, 2009). Because such multiple switch points are difficult to rationalize and may indicate

subjects (16.7%) in Condition 2 featured multiple switch points in at least one task. The majority of multiple switching occurred in Condition 2.3, indicating that this task may have been confusing to subjects.[17] Attention is given to the 122 subjects who had unique switch points in all 22 decision tasks.[18] This results in 1474 individual decisions in Condition 1 and 1210 decisions in Condition 2. Of these 2684 total decision tasks, in a small percentage (0.60%) the subject preferred Option A for all rows and in a larger percentage (4.14%) the subject preferred Option B for all rows. These responses provide only one-sided bounds on the interval of the subject's response. The other bound is imputed via top and bottom-coding accordingly.[19]

A variety of demographic, cognitive and attitudinal data were collected after the study was concluded in order to provide a simple balancing test. Table 4.2 compares data across experimental conditions for survey respondents.[20] Though some differences do exist, particularly in academic year, subjects were broadly balanced on observable characteristics, simple numeracy and cognitive ability scores, and subjectively reported risk attitudes.[21] An omnibus test from the logit regression of condition assignment on all survey variables for 111 of 122 individuals with complete survey data does not reject the null hypothesis of equal demographic, cognitive and attitudinal characteristics across conditions ($\chi^2 = 14.1,\ p = 0.12$). Because the randomization

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subject confusion, researchers often exclude such observations or mechanically enforce single switch points. See Harrison et al. (2005) for discussion.

[17]Five of 11 multiple switchers in Condition 2, had multiple switching in only Condition 2.3, one had multiple switching in Conditions 2.1 and 2.3, two had multiple switching in Conditions 2.2 and 2.3, and three had multiple switching in all three subconditions.

[18]All results are maintained when including multiple switchers and taking their first switch point as their choice. See Appendix Table 4.4 for details.

[19]For example if an individual chose Option A at all rows in a probability equivalent including when Option B was a 100% chance of getting $30, I topcode the interval as $[100, 100]$. Virtually all of the bottom-coded responses, 100 of 111, were decisions in Condition 2.3 where the bottom-coded choice would be preferring $10 with certainty to a given gamble over $0 and $30. No bottom-coded responses arose in Condition 1 or 2.1 where the lowest Option B outcome was $0 with certainty.

[20]111 of 122 subjects completed all survey elements. 60 of 67 subjects in Condition 1 and 51 of 55 subjects in Condition 2 provided complete survey responses. Non-response is unrelated to condition as Condition 1 accounts for 54-55 percent of the data in both the respondent and full samples.

[21]Numeracy is measured with a six question exam related to simple math skills such as division and compound interest previously validated in a number of large and representative samples (Lusardi and Mitchell, 2007; Banks and Oldfield, 2007; Gerardi et al., 2010). Cognitive ability is measured with the three question Cognitive Reflection Test introduced and validated in Frederick (2005). Subjective risk attitudes are measured on a 7 point scale with the question,"How willing are you to take risks in general on a scale from 1 (unwilling) to 7 (fully prepared)" previously validated in a large representative sample (Dohmen et al., 2005).

is at the session level, this helps to ensure that accidental selection issues are not driving the experimental results. Additional within-subjects results unaffected by selection are provided in Sub-Section 4.4.3, along with demonstrations of robustness to controlling for demographic differences.

Table 4.2: Summary Statistics and Balancing Test

		Total N = 122	Condition 1 N = 67	Condition 2 N = 55		
Variable	# Obs	Mean (s.d)	Mean (s.d)	Mean (s.d.)	*t*-statistic	*p*-value
Male (=1)	119	0.46 (0.50)	0.42 (0.50)	0.52 (0.50)	1.12	(p=0.27)
Academic Year	122	2.63 (1.08)	2.45 (1.05)	2.85 (1.08)	2.10	(p=0.04)
Grade Point Average	120	3.20 (.42)	3.25 (.44)	3.15 (.39)	-1.26	(p=0.21)
English 1^{st} Language (=1)	122	0.56 (0.50)	0.51 (0.50)	0.62 (0.49)	1.22	(p=0.22)
Smoker (=1)	122	0.04 (0.20)	0.03 (0.17)	0.05 (0.23)	0.68	(p=0.50)
Weekly Spending ($)	122	89.68 (89.49)	85.00 (68.63)	95.38 (110.12)	0.64	(p=0.53)
Risk Attitudes (1-7)	122	3.84 (1.19)	3.70 (1.22)	4.00 (1.14)	1.39	(p=0.17)
Cognitive Ability Score (1-3)	117	1.79 (1.05)	1.86 (1.08)	1.70 (1.01)	-0.83	(p=0.41)
Numeracy Score (1-6)	120	5.75 (0.54)	5.78 (0.52)	5.71 (0.57)	-0.76	(p=0.45)
		Omnibus $\chi^2 = 14.1$, $(p = 0.12)$				

Notes: Summary statistics for 122 subjects with unique switch points in all 22 decision tasks. # Obs refers to the number of responses to each question. Omnibus χ^2 test statistic corresponding to the null hypothesis of zero slopes in logit regression with 111 subjects with complete survey data of condition assignment on all survey variables with robust standard errors.

I begin by investigating behavior in Conditions 1.1, 1.2, 2.1 and 2.2. With the exception of the KR preference model, all discussed theories predict experimental equivalence across these conditions. That is, elicited risk attitudes should be identical whether one asks the probability equivalent of a given certain amount or the certainty equivalent of a given gamble. Figure 4.4 presents median data for the 122 individuals with unique switch points along with a dashed black line corresponding to risk neutrality. The experimentally controlled parameter is presented on the horizontal axis and the median subject response is presented on the vertical axis.

Apparent from the median data is the systematic difference in elicited risk attitudes between certainty and probability equivalents. When fixing a stochastic gamble and trading for increasing certain amounts in Conditions 2.1 and 2.2, subjects display virtual risk neutrality. When fixing a certain amount and trading for increasing gambles in Conditions 1.1 and 1.2 subjects display risk aversion.

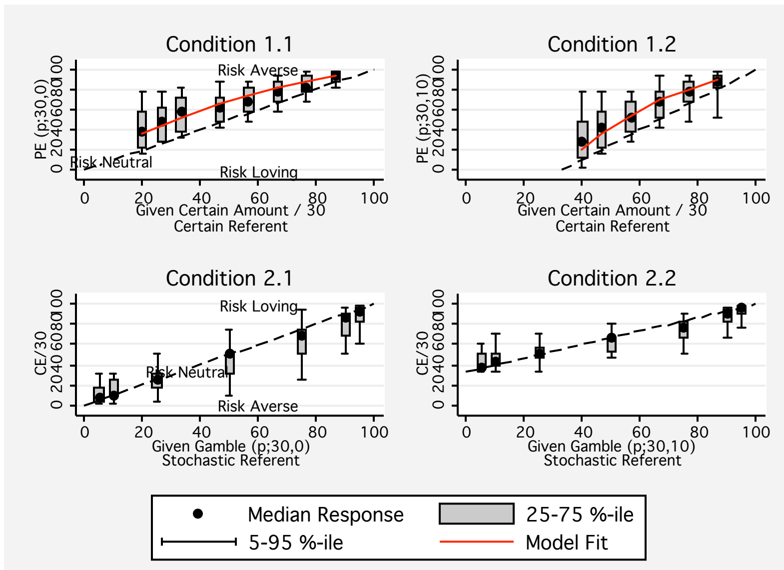

Figure 4.4: Conditions 1.1, 1.2, 2.1, and 2.2 Responses
Note: Median data from 122 experimental subjects with unique switching points in all 22 decision tasks. Dashed black line corresponds to risk neutrality. Solid red line for Conditions 1.1 and 1.2 corresponds to KR model fit with $\hat{\lambda} = 3.4$. The KR model predicts risk aversion for probability equivalents in Conditions 1.1 and 1.2 and risk neutrality for certainty equivalents in Conditions 2.1 and 2.2.

For each experimental task, decisions are classified as being risk neutral, risk averse or risk loving. These classifications recognize the interval nature of the data. For example, a decision is coded as risk neutral if the risk neutral response lies in the interval generated by the subject's switch point. Figure 4.5, Panel A presents these classifications. Whereas the distributions of risk averse, neutral and loving responses are somewhat even in the certainty equivalents of Condition 2, the majority of responses are risk averse in the probability equivalents of Condition 1. Proportionately nearly twice as many responses are classified as risk averse in probability equivalents relative to certainty equivalents. As this may be a strict classification of responses,

Figure 4.5, Panel B extends the interval of the switch point to $+/-$ one choice. By this wider interval measure the majority of the data in Condition 1 remains risk averse, while the majority of the data in Condition 2 is now classified as risk neutral.

Table 4.3 presents ordered logit regressions for the classification of responses of Figure 4.5, Panel A with standard errors clustered on the individual level. The dependent variable is *Risk Attitude*, which takes the value -1 for a risk loving classification, 0 for risk neutrality, and +1 for a risk averse response. The natural order of *Risk Attitude* corresponds to increasing risk aversion. These regressions control for condition and the variable *Risk Neutral Response*. *Risk Neutral Response* is coded from 0 to 100 and expresses in percentage terms the dashed line of risk neutrality in Figure 4.4. That is, *Risk Neutral Response* is either the given certain amount's risk neutral probability equivalent (in Condition 1), or the given gamble's expected value divided by 30 (in Condition 2). This helps to control for experimental variation that might be related to elicited risk attitudes under non-EU preference models such as non-linear probability weighting. Certain specifications additionally control for order and representation effects as well as the collected demographic and attitudinal characteristics for individuals who responded in full to the post-study survey. Across specifications, subjects in Condition 1 are significantly more likely to have risk averse responses. Odds ratios for being classified as risk averse relative to risk neutral or risk loving are provided in brackets. Subjects randomly assigned to Condition 1 are between three and four times more likely to exhibit risk aversion than those assigned to Condition 2.[22]

These simple tests indicate an endowment effect for risk. In certainty equivalents tasks, subjects are generally risk neutral. In probability equivalents tasks, subjects are generally risk averse. Standard expected utility, prospect theory and disappointment aversion all predict experimental equivalence across these two environments. The data are potentially consistent with the KR model, with its possibility of a stochastic reference distribution. However, the obtained data are not directly consistent with the refined PPE concept, which would also predict identical behavior

[22]Results are maintained with the inclusion of multiple switchers. Additionally, no interactions for order or representation effects were obtained. Appendix Table 4.4 provides these additional regressions.

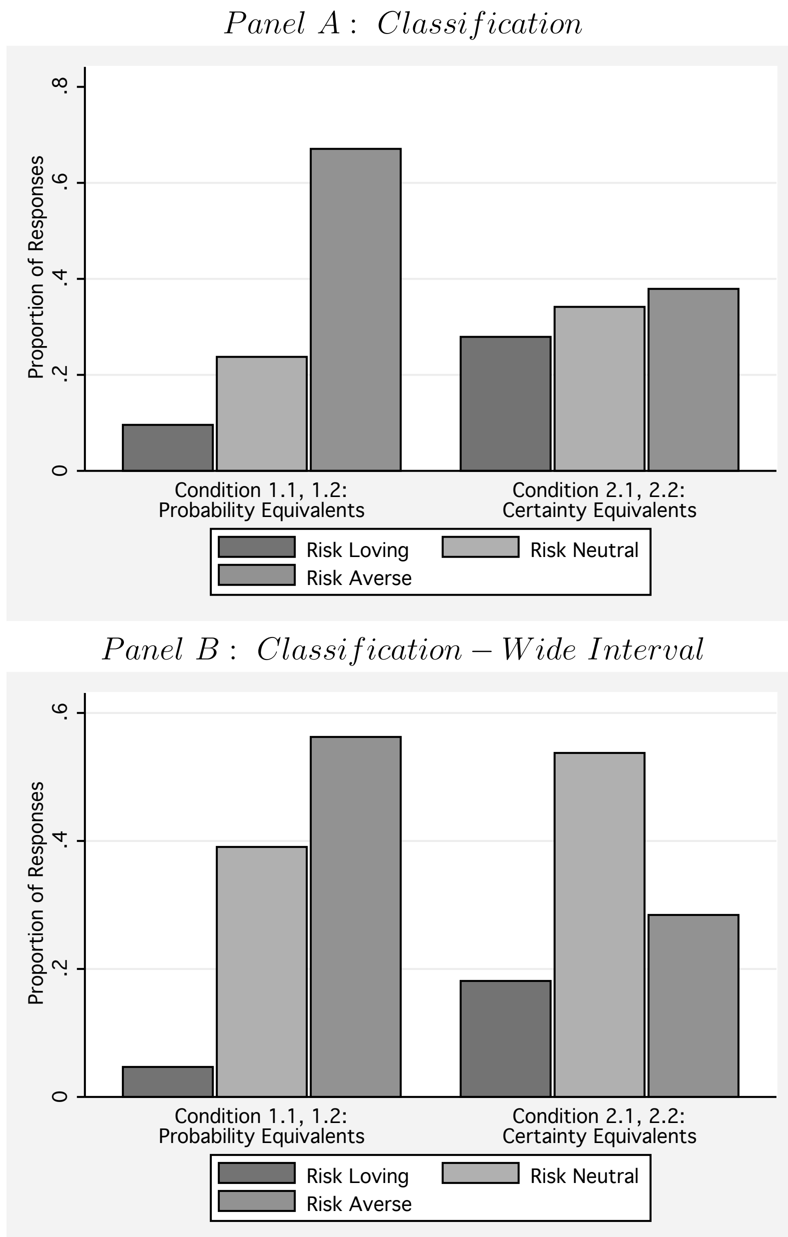

Figure 4.5: Conditions 1.1, 1.2, 2.1, and 2.2 Classifications

Note: The figure presents classifications of responses from 122 experimental subjects with unique switching points in all 22 decision tasks. The KR model predicts risk aversion for probability equivalents in Conditions 1.1 and 1.2 and risk neutrality for certainty equivalents in Conditions 2.1 and 2.2. Panel A provides the classifications based on the interval of a subject's switch point. Panel B provides classifications based on a wider interval of the switch point +/− one choice.

Table 4.3: Probability and Certainty Equivalent Risk Attitude Regressions

	(1)	(2)	(3)
Dependent Variable:		*Risk Attitude Classification*	
Probability Equivalents: Condition 1 (=1)	1.330***	1.329***	1.172***
	(0.225)	(0.224)	(0.241)
	[3.782]	[3.778]	[3.230]
Risk Neutral Response	0.008**	0.008**	0.008**
	(0.003)	(0.003)	(0.003)
Male (=1)			-0.087
			(0.236)
Academic Year			-0.223*
			(0.094)
Grade Point Average			0.275
			(0.266)
English 1^{st} Language (=1)			-0.368
			(0.248)
Smoker (=1)			-0.323
			(0.502)
Weekly Spending ($)			0.001
			(0.001)
Risk Attitudes (1-7)			-0.128
			(0.107)
Cognitive Ability Score (1-3)			-0.052
			(0.110)
Numeracy Score (1-6)			0.014
			(0.248)
Order 2 (=1)		0.074	-0.008
		(0.214)	(0.218)
Representation (=1)		0.023	0.063
		(0.215)	(0.218)
Constant 1	-0.517*	-0.471	-0.959
	(0.220)	(0.270)	(1.467)
Constant 2	0.993***	1.040***	0.559
	(0.215)	(0.252)	(1.464)
# Observations	1708	1708	1554
# Clusters	122	122	111
Log-Likelihood	-1609.620	-1609.275	-1443.404

Notes: Coefficients from ordered logit of *Risk Attitude* classification on control variables, measured from probability and certainty equivalents of Conditions 1.1, 1.2, 2.1, and 2.2. *Risk Attitude* takes the value -1 for risk loving, 0 for risk neutral, and +1 for risk averse. Standard errors clustered on the individual level in parentheses. Odds ratios for Condition 1 in brackets, calculated as the exponentiated coefficient. Column (3) features data from 111 subjects who also completed the post-study survey. *Level of significance*: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

across conditions.

In applying the equilibrium concepts from KR, I consider some form of narrow bracketing within a given row of a choice task. That is, the subject considers a choice in a given row between Option A, representing some fixed amount or gamble, G, and Option B, representing some fixed amount or gamble, F. As discussed in Section 4.2.1, choosing Option A over Option B therefore implies the PPE relation $U(G|G) > U(F|F)$. However in the KR model there is some ambiguity in the bracketing of the referent. It is possible, for instance, to consider the referent to be the distribution induced by all choices in the task, or even all choices in the entire experiment. Such a specification could potentially revive PPE as a viable organization of the data. However solving for a PPE in these cases is computationally intensive and a somewhat implausible calculation on the part of subjects. The narrow bracketing used in this analysis is a direct application of the KR equilibrium in choices between lotteries.

Equilibrium behavior even in its simplest form may be a stringent requirement for experimental subjects. A body of evidence from strategic environments argues against equilibrium logic in the laboratory (Camerer et al., 2004; Costa-Gomes and Crawford, 2006; Crawford and Iriberri, 2007; Costa-Gomes et al., 2009). Resulting process models such as level-k thinking are argued to be organized around initial reactions to experimental environments. Koszegi and Rabin (2006) provide a similar indication, suggesting that referents are established as probabilistic beliefs held at the moment an individual first focused on a decision. In our environment, subjects may first focus their thinking on the fixed element in a given series of decisions. If so, then the referent may sensibly change across the conditions of our experiment. In certainty equivalents the referent will be stochastic, while in probability equivalents the referent will be certain. See Section 4.5 for further discussion.

4.4.2 Estimating KR Preferences

Under the assumption that subjects organize their thinking around the fixed element in a series of decisions, the KR model with exogenously manipulated referents rationalizes the data. Importantly, such a model is easily implemented econometrically. The KR model motivated above is described by one key parameter, λ, the degree of loss aversion, which can be estimated at either the group or individual level via non-linear least squares.

Using the data from probability equivalent Conditions 1.1 and 1.2, the midpoint of the interval implied by a subject's switch point is taken as the value q^* in equation (2). Equation (2) is then estimated via non-linear least squares with standard errors clustered on the subject level. The aggregate estimate is $\hat{\lambda} = 3.41$ ($s.e. = 0.34$). The null hypothesis of zero loss aversion, $\lambda = 1$, is rejected $F_{1,66} = 49.06,\ p < 0.01$. This value of loss aversion is consistent with loss aversion estimates from other contexts (Tversky and Kahneman, 1992; Gill and Prowse, 2010; Pope and Schweitzer, Forthcoming) and is closely in line with the often discussed benchmark of losses being felt twice as severely as gains, $\lambda = 3,\ \eta = 1$ (Koszegi and Rabin, 2006, 2007).[23] Figure 4.4 presents predicted values from this aggregate regression as the solid red line for Conditions 1.1 and 1.2. The aggregate data matches well the fitted model's predicted hump-shaped deviation from risk neutrality. Of course, the KR preference model predicts risk neutrality in Conditions 2.1 and 2.2.

In order to evaluate the predictive validity of the KR preference model, it can be tested out of sample with alternative segments of the data. As noted above, the KR preference model with a stochastic referent predicts risk neutrality in Condition 2.3 and predicts a particular shape of quadratically declining risk aversion in Condition 1.3. Figure 4.6 presents data from these conditions as well as out-of-sample predictions for KR model with the estimated $\hat{\lambda} = 3.4$. Though the KR prediction of risk neutrality breaks down at the intermediate probabilities of Condition 2.3, in Condition 1.3, the out-of-sample prediction closely matches aggregate behavior.[24]

[23] The functional form of Tversky and Kahneman (1992) does not feature consumption utility and so the loss aversion estimate of $\hat{\lambda} = 2.25$ in their paper is a direct measure of losses being felt twice as severely as gains.

[24] Additionally, Condition 1.3 reproduces the general shape and level of the uncertainty equivalents discussed in Andreoni and Sprenger (2010b) demonstrating a slightly convex relationship between given gambles and their uncertainty equivalents. However, Andreoni and Sprenger (2010b) document the convexity becoming sharper as the given gamble approaches certainty, and this result is not present in the data. Minor differences in experimental detail may account for the difference at $p = 0$ between the present results and Andreoni and Sprenger (2010b) including a different number and order of tasks and slightly changed tasks. The Andreoni and Sprenger (2010b) price lists were designed with decision aids of checked top and bottom rows. The task used in Condition 1.3 was not. More importantly, however, appears to be the presence of the physical representation of Option A. The sharpened convexity at $p = 0$ in Andreoni and Sprenger (2010b) is driven by individuals who violate first order stochastic dominance close to certainty. They document individual dominance violation rates between $p = 0$ and $p = 0.05$ of around 17.5 percent across three tasks. When Option A is not physically represented, a similar violation rate of 13.5 percent is found. However, when Option A is physically represented, zero violations of stochastic dominance at certainty are observed. The effect of physical representation on the proportion of individuals violating stochastic dominance at certainty is significant ($z = 2.09,\ p < 0.05$).

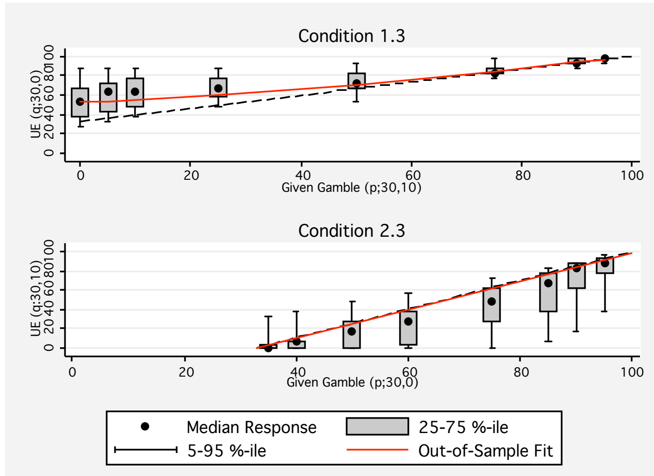

Figure 4.6: Conditions 1.3 and 2.3

Note: Median data from 122 experimental subjects with unique switching points in all 22 decision tasks. Dashed black line corresponds to risk neutrality. Solid red lines correspond to out-of-sample KR model prediction with $\hat{\lambda} = 3.4$ as estimated in Conditions 1.1 and 1.2.

In addition to aggregate out-of-sample predictions, for the 67 subjects in Condition 1, individual analyses can be conducted. Using the data from Conditions 1.1 and 1.2, the degree of loss aversion, λ_i, can be estimated following equation (2). These individual estimates, $\hat{\lambda}_i$, can be correlated with behavior in Condition 1.3. As discussed in Section 4.3.1, the deviation from risk neutrality is predicted to increase with λ_i.

The log deviation from risk neutrality, *Log Deviation*, is measured for each individual by taking the log difference between the area under the linear interpolation of their responses to Condition 1.3 and the area under the dashed risk neutral response line. Hence, *Log Deviation* would take the value 0 for risk neutrality, a positive value for risk aversion and a negative value for risk loving. Figure 4.7, Panels A and B provide histograms of the individual estimates of loss aversion $\hat{\lambda}_i$ and *Log Deviation*. The median $\hat{\lambda}_i$ is 3.6, echoing the aggregate result. The median *Log Deviation* is 0.15, indicating a deviation towards risk aversion in Condition 1.3. Figure 4.7, Panel C correlates loss aversion, $\hat{\lambda}_i$, estimated from Conditions 1.1 and 1.2 with the *Log Deviation* calculated from Condition 1.3.[25] Individuals who are more loss averse in

[25]Three subjects with extreme values, $\hat{\lambda}_i > 20$ or *Log Deviation* < -0.5, are not included in Figure

Conditions 1.1 and 1.2, deviate more from risk neutrality in Condition 1.3. The correlation is significant at all conventional levels ($\rho = 0.48,\ p < 0.01$). Additionally, individuals with estimated $\hat{\lambda}_i$ close to 1 have calculated *Log Deviation* close to the risk neutral level of 0. This indicates both experimental consistency across conditions and the predictive validity of loss aversion estimates at the individual level.

4.4.3 Secondary Study Within-Subjects Design

The results to here have been from a primary between-subjects design. Though the data demonstrate a sizeable endowment effect for risk and the KR preference model is able to organize the results, questions may naturally arise about the robustness of the phenomenon to issues of selection. In this section, I discuss portions of the data obtained for Andreoni and Sprenger (2010b), a within-subjects study of 76 subjects designed primarily with uncertainty equivalents similar to Condition 1.3 and certainty equivalents similar to Condition 2.1.[26] Two Holt and Laury (2002) risk tasks were implemented as a buffer between uncertainty equivalents and certainty equivalents in Andreoni and Sprenger (2010b) and decisions were collected between conditions.

Importantly, the uncertainty equivalent for $p = 0$ is identical to a probability equivalent. Subjects are asked to provide the gamble probability over \$30 and \$0 that makes them indifferent to a 100% chance of receiving \$10. This single probability equivalent can be compared to the subject's own range of certainty equivalents. One finds the gamble probability over \$30 and \$0 at which the certainty equivalent is revealed to be \$10. Under the assumption that the referent is perceived as the fixed element in a task, comparison of this gamble probability to the revealed probability equivalent of \$10 gives a within-subjects measure of the endowment effect for risk.

For certainty equivalents tasks, the probability of winning \$30 that yields a certainty equivalent of \$10 is identified by finding the smallest probability, p, at which the certainty equivalent is higher than \$10, the largest probability, p', at which the

4.7, Panel C for space, though their data is used in analysis and presented correlations.

[26] Some minor experimental differences exist such as the use of pre-checked boxes as decision aids, differing orders and no representation in Andreoni and Sprenger (2010b). See Section 4.4.2 and Andreoni and Sprenger (2010b) for more details.

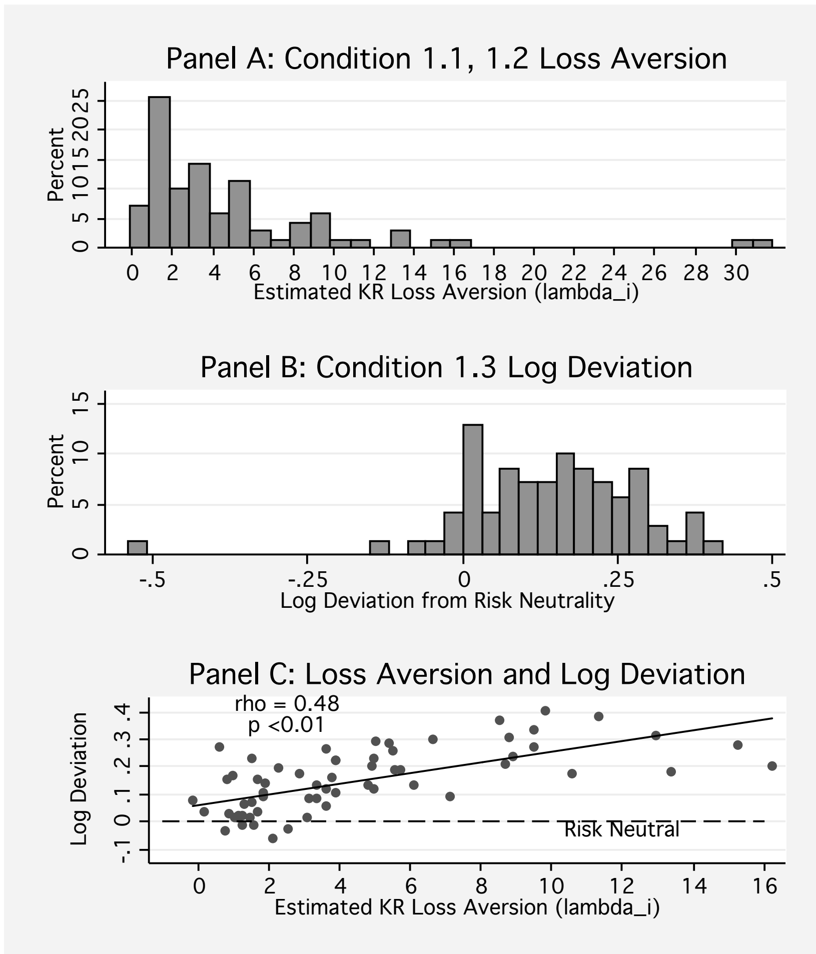

Figure 4.7: Loss Aversion ($\hat{\lambda}_i$) and *Log Deviation* From Risk Neutrality

Note: Individual loss aversion estimate $\hat{\lambda}_i$ of equation (2) using data from Conditions 1.1 and 1.2. *Log Deviation* from risk neutrality calculated from Condition 1.3. Three subjects with extreme values, $\hat{\lambda}_i > 20$ or *Log Deviation* < -0.5 , are not included in Panel C for space, but included in reported correlation.

certainty equivalent is smaller than \$10, and taking the average $\overline{p} = (p + p')/2$.[27] The average value of $\overline{p}$ is 0.306 ($s.d.$ 0.210). The average $(q; 30, 0)$ probability equivalent of \$10 for sure is 0.541 (0.220). The difference is significant at all conventional levels ($t = 7.16,\ p < 0.001$).[28] Additionally, these values are remarkably consistent with the between-subjects evidence documented in Sections 4.4.1 and 4.4.2 where subjects are close to risk neutral in certainty equivalents and reveal average probability equivalents for \$10 with certainty of 0.547 (0.189) in Condition 1.3 and 0.546 (0.185) in Condition 1.1.

The within-subjects results demonstrate an endowment effect for risk. Subjects are close to risk neutral when revealing the gamble for which the certainty equivalent is \$10. The same subjects are risk averse when revealing the probability equivalent gamble for \$10. The KR model with the standard values of $\lambda = 3,\ \eta = 1$ and the assumption that the referent is perceived as the fixed element of a task would predict \$10 certainty equivalents to arise at a 33% chance of winning \$30 and probability equivalents of \$10 for sure to arise at a 50% chance of winning \$30. The within-subjects data are therefore consistent with the KR preference model with standard values and those obtained in the between-subjects estimates of Section 4.4.2.

4.5 Discussion

The obtained results are supportive of the KR preference model. Unlike prior work demonstrating the importance of expectations for reference points, these results are able to distinguish between KR preferences and other expectations-based models such as disappointment aversion. Gaining separation between these models is an important experimental step and necessary for evaluating theoretical developments that depend critically on the stochasticity of the referent (Koszegi and Rabin, 2006, 2007; Heidhues and Koszegi, 2008; Koszegi and Rabin, 2009). Additionally, the distinction

[27] There are 13 cases where p' is not observed because the individuals' certainty equivalents always exceeded \$10. There is one case where p is not observed because the individual's certainty equivalent never exceeded \$10. In these cases the non-missing value is used. Eliminating these observations does not influence the result.

[28] The difference remains significant when comparing $(q; 30, 0)$ to either p ($t = 4.32,\ p < 0.001$), or p' ($t = 8.57,\ p < 0.001$) for individuals with non-missing values of p and p', respectively.

between the KR and DA models is important in a variety of applied settings where the two models make different predictions. In particular, the DA model predicts first order risk aversion in the sense of Segal and Spivak (1990) over all gambles, while the KR model predicts first order risk aversion only when risk is unexpected. When outcomes lie within the support of expectations, risk neutrality is predicted. Several applications in finance, insurance purchasing and decision science can be considered.

First, in finance first-order risk aversion is argued to influence stock market participation (Haliassos and Bertaut, 1995; Barberis et al., 2006) and returns (Epstein and Zin, 1990; Barberis and Huang, 2001). The KR model indicates that individuals will accept fair bets within expectations, but grow more risk averse as outcomes exceed expectations. As such, the KR model would predict first-order risk aversion, with corresponding participation and returns effects, only when potential outcomes lie outside prior expectations. In contrast, under the DA model individuals are first-order risk averse over all gambles, never accepting a fair investment bet.[29]

Second, a comparison can be made for insurance purchasing where first order risk aversion potentially influences contract choice (Sydnor, Forthcoming). First-order risk aversion predicts a desire for full insurance even when insurance is not actuarially fair (Segal and Spivak, 1990). Hence, a disappointment averse consumer may fully insure under positive insurance profits. The KR model predicts insurance only for potential outcomes outside of expectations. As such, the KR model gives a suggestion as to what gambles will be insured and how they relate to expectations.[30] This is in contrast to disappointment aversion where full insurance is desired for all gambles. Future work should explore these different predictions of the KR and DA models in real-world financial decision-making and experimental settings.

Third, a gap between KR and DA models is apparent in decision science where researchers have long debated the inconsistency between probability equivalent and

[29]Additionally, there is no natural way to model an outcome being outside of expectations in disappointment aversion. All gambles are evaluated relative to their own expected utility certainty equivalents.

[30]Of course, risks can lie outside of expectations by endogenously constructing a fixed referent and risks can coincide with expectations by constructing a stochastic referent. It may be UPE to insure and have a fixed referent as well as not insure and have a stochastic referent. Hence the necessity of the PPE concept to select among such UPE choices. Koszegi and Rabin (2007) provide a detailed discussion of the relationship between insurance purchasing and expectations under the KR preference model.

certainty equivalent methods for utility assessment (Hershey et al., 1982; McCord and de Neufville, 1985, 1986; Hershey and Schoemaker, 1985; Schoemaker, 1990). The general finding is that probability equivalents yield more risk aversion. This difference in elicited utility is predicted by the KR model but not DA. Though there are some hints in the literature that such experimental inconsistency may be due to a "response mode bias," where probability equivalent tasks are reframed as mixed gambles with gains and losses (Hershey et al., 1982; Hershey and Schoemaker, 1985), many other explanations for the inconsistency have been proposed (McCord and de Neufville, 1985; Schoemaker and Hershey, 1992; Schoemaker, 1993). The present results and use of the KR model help to resolve this long-standing issue. If referents are changed from stochastic to certain as the environment moves from a certainty to a probability equivalent, then the specific KR structure of preferences and not some idiosyncratic bias organizes this long-debated inconsistency in utility elicitiation.

Though the findings are supportive of the KR model and provide direction for further analysis, a distinction must be made between the results presented and the equilibrium predictions of the KR model. Koszegi and Rabin (2006, 2007) present a rational expectations equilibrium concept, the Unacclimating Personal Equilibrium (UPE), in which consumption outcomes correspond to expectations. To select among the potential multiplicity of equilibria, the Preferred Personal Equilibrium (PPE) concept is introduced. The Preferred Personal Equilibrium (PPE) concept of KR requires the coincidence of expectations and behavior. Under the PPE refinement, the KR model, similar to disappointment aversion, predicts no endowment effect for risk. Because the data reject disappointment aversion, they necessarily reject the PPE refinement.

PPE logic may be a stringent requirement for experimental subjects. Koszegi and Rabin (2006) suggest that "a person's reference point is her probabilistic beliefs about the relevant consumption outcome held between the time she first focused on the decision determining the outcome and shortly before consumption occurs"[p. 1141]. "First focus" in the present studies is plausibly drawn to the fixed element in a given series of decisions. Hence the referent may be established as this fixed element and deviated from when alternatives become sufficiently attractive. It is likely that the different fixed elements induced different stochastic referents as the the data are systematically organized by the KR model under this assumption.

The non-PPE finding is in line with a body of evidence from strategic environments arguing against equilibrium logic and organizing behavior with initial reactions to decision environments (Camerer et al., 2004; Costa-Gomes and Crawford, 2006; Crawford and Iriberri, 2007; Costa-Gomes et al., 2009). Additionally it suggests that expectations-based referents may be quickly changed by context. This supports field findings such as Pope and Schweitzer (Forthcoming) and Post et al. (2008) where referents and risk taking behavior change with both minor contextual shifts such as field performance on specific holes in golf tournaments and major contextual variables such as unwon large-value prizes in sequential game shows.

The potential sensitivity of expectations-based referents to contextual changes has implications for both economic agents and experimental methodology. First, from a methodological perspective, if fixed elements can serve as referents, slightly changed choice environments may induce very different behavior. This is of particular importance for the experimental measurement of preferences and willingness to pay where similar techniques are used and resulting estimates are given economic significance. Second, if expectations-based referents can be manipulated via simple framing devices without physical endowments, then scope exists for marketers and policy-makers to deeply influence behavior with menus alone.

4.6 Conclusion

Reference-dependent preferences with loss-aversion relative to a reference point has been widely adopted in both theoretical and empirical research, rationalizing not only endowment effect behavior but a host of other anomalies from labor supply, to consumer behavior, to finance. Critical to such reference-dependent models is the determination of the referent around which losses and gains are encoded. Though initially the referent was left a virtual free parameter, extensions to reference dependence have added discipline. Attention has focused on expectations-based mechanisms for the determination of fixed reference points in models of Disappointment Aversion (DA) (Bell, 1985; Loomes and Sugden, 1986; Gul, 1991), or for the determination of stochastic reference distributions in the models of Koszegi and Rabin (2006, 2007) (KR).

A body of field and laboratory evidence has highlighted the importance of expectations for reference-dependent behavior consistent with these expectation-based models. Though accumulated data does demonstrate the importance of expectations for reference dependence, the data is generally consistent with either the DA or the KR model. That is, prior evidence cannot distinguish between the two models.

This paper presents evidence from two experiments focused on identifying a particular prediction of the KR model which is not shared with DA: an endowment effect for risk. The endowment effect for risk is closely related to the potential stochasticity of the referent. When the referent is stochastic, and an individual is offered a certain amount, the KR model predicts risk neutrality. Conversely, when the referent is a fixed certain amount, and an individual is offered a gamble, the KR model predicts risk aversion. Disappointment aversion makes no such prediction of differential behavior as gambles are always evaluated relative to a fixed referent, the gamble's expected utility certainty equivalent.

To date, little evidence exists exploring the KR implication of an endowment effect for risk. In purposefully simple risk preference experiments, eliminating often-discussed confounds, I test both between- and within-subjects for an endowment effect for risky gambles. In the primary study discussed, half of subjects make probability equivalent choices between fixed certain amounts and changing gambles. Half of subjects make certainty equivalent choices between fixed gambles and changing certain amounts. Importantly, both standard models and most reference-dependent models including disappointment aversion predict equivalence of risk attitudes across the experimental conditions. One exception is the KR preference model. Under the assumption that the fixed element in a series of decisions serves as the referent, the KR model predicts that subjects will be risk averse in probability equivalents and risk neutral in certainty equivalents.

Both between- and within-subjects the data indicate an endowment effect for risk. In the primary study, subjects are between three and four times more likely to be risk averse if randomly faced with a probability equivalent as opposed to a certainty equivalent. In a secondary, within-subjects study, the phenomenon is also observed.

Under the assumption that the experimental variation changes the perceived referent, the KR preference model organizes the data at both the aggregate and individual level and both between and within-subjects.

Finding evidence of an endowment effect for risk, particularly in a neutral environment, provides critical support for the KR preference model. Unlike prior work demonstrating the importance of expectations for reference points, these results clearly distinguish between KR preferences and disappointment aversion. Providing separation between these competing accounts of expectations-based reference dependence represents an important experimental step and a necessary contribution for evaluating theory models and applications that rely specifically on stochastic referents. Demonstrating methodology for distinguishing between the expectations-based reference-dependent preferences is additionally important as the different models provide notably different accounts of financial decision-making, insurance purchasing behavior, and long-standing anomalies in decision science. Future work should test these competing accounts, particularly in the domain of financial behavior, with both field and laboratory evidence.

4.7 Appendix

4.7.1 KR Preferences and Uncertainty Equivalents

I demonstrate two results: 1) that the KR preference models predicts quadratically declining risk aversion in uncertainty equivalents where individuals are endowed with gambles $(p; y, x)$, $y > x > 0$ and trading for gambles $(q; y, 0)$; and 2) that the KR preference model predicts risk neutrality in inverted uncertainty equivalents where individuals are endowed with gambles $(p; y, 0)$ and trading for gambles $(q; y, x)$.

Uncertainty Equivalents

Uncertainty equivalents feature a binary referent gamble, G, summarized by the probability p. Let r_1 be the referent with probability p and $r_2 < r_1$ be the referent with probability $1-p$. Given the linear KR model, the utility of the referent is

$$U(G|G) = p \cdot r_1 + (1-p) \cdot r_2 + p \cdot (1-p) \cdot [1 \cdot (r_1 - r_2) + \lambda \cdot (r_2 - r_1)].$$

Now, I consider a binary consumption gamble, F, summarized by the probability q. Let $x_1 = r_1$ be the gamble outcome with probability q and $x_2 < r_2$ be the gamble outcome with probability $1-q$. I write the KR utility as

$$U(F|G) = q \cdot p \cdot x_1 + (1-q) \cdot p \cdot [x_2 + \lambda \cdot (x_2 - r_1)] + q \cdot (1-p) \cdot [x_1 + 1 \cdot (x_1 - r_2)] + (1-q) \cdot (1-p) \cdot [x_2 + \lambda \cdot (x_2 - r_2)].$$

For simplicity I carry out the uncertainty equivalent replacements, $x_1 = r_1$ and $x_2 = 0$ such that this reduces to

$$U(F|G) = q \cdot \{r_1 + (1-p) \cdot [r_1 - r_2] + p \cdot \lambda \cdot r_1 + (1-p) \cdot \lambda \cdot r_2\} - \lambda \cdot p \cdot r_1 - \lambda \cdot (1-p) \cdot r_2.$$

The uncertainty equivalent will be the consumption gamble, F^*, with corresponding probability q^* satisfying $U(F^*|G) = U(G|G)$.

$$p \cdot r_1 + (1-p) \cdot r_2 + p \cdot (1-p) \cdot [1 \cdot (r_1 - r_2) + \lambda \cdot (r_2 - r_1)] =$$
$$q^* \cdot \{r_1 + (1-p) \cdot [r_1 - r_2] + p \cdot \lambda \cdot r_1 + (1-p) \cdot \lambda \cdot r_2\} - \lambda \cdot p \cdot r_1 - \lambda \cdot (1-p) \cdot r_2$$

$$q^* = \frac{(1+\lambda) \cdot p \cdot r_1 + (1+\lambda) \cdot (1-p) \cdot r_2 + (1-\lambda) \cdot p \cdot (1-p) \cdot (r_1 - r_2)}{\{r_1 + (1-p) \cdot [r_1 - r_2] + p \cdot \lambda \cdot r_1 + (1-p) \cdot \lambda \cdot r_2\}} \qquad (4.4)$$

The uncertainty equivalent q^* can be evaluated at two critical points, $p = 0$ and $p = 1$. At $p = 0$,

$$q_0^* = \frac{(1+\lambda) \cdot r_2}{2r_1 + (\lambda - 1) \cdot r_2}.$$

This is simply the probability equivalent for r_2 with certainty when $x_2 = 0$. To see this, I compare to the probability equivalent of equation (2) with $r = r_2$, $x_2 = 0$, and

$x_1 = r_1$,

$$q^* = \frac{r - x_2 - \lambda \cdot (x_2 - r)}{[x_1 - x_2] + [1 \cdot (x_1 - r) - \lambda \cdot (x_2 - r)]},$$

$$q^* = q_0^* = \frac{(1+\lambda) \cdot r_2}{2r_1 + (\lambda - 1) \cdot r_2}.$$

As in the development of 4.2.1, I predict risk aversion for loss averse individuals, $\lambda > 1$, at $p = 0$.

At intermediate probabilities, the uncertainty equivalent follows equation (3) above which depends upon the squared probability term $p \cdot (1-p)$. The function $q^*(p)$ can be traced to demonstrate quadratically declining risk aversion. One such trace is provided as the out of sample prediction in Figure 4.4. Risk neutrality is slowly approached such that at $p = 1$, $q^* = 1$. I evaluate q^* at $p = 1$, and show

$$q_1^* = \frac{(1+\lambda) \cdot r_1}{(1+\lambda) \cdot r_1} = 1.$$

Inverted Uncertainty Equivalents

Inverted uncertainty equivalents, like uncertainty equivalents feature a binary referent gamble, G, summarized by the probability p. Let r_1 be the referent with probability p and $r_2 < r_1$ be the referent with probability $1-p$. Given the linear KR model, the utility of the referent is

$$U(G|G) = p \cdot r_1 + (1-p) \cdot r_2 + p \cdot (1-p) \cdot [1 \cdot (r_1 - r_2) + \lambda \cdot (r_2 - r_1)].$$

For simplicity I carry out the inverted uncertainty equivalent replacement, $r_2 = 0$ such that this reduces to

$$U(G|G) = p \cdot r_1 + p \cdot (1-p) \cdot (1-\lambda) \cdot r_1,$$

$$U(G|G) = p \cdot r_1 \cdot [1 + (1-p) \cdot (1-\lambda)],$$

$$U(G|G) = p \cdot r_1 \cdot [2 + p \cdot \lambda - p - \lambda].$$

It will be convenient to write this as

$$U(G|G) = p \cdot r_1 \cdot [2 + p \cdot \lambda - p] - p \cdot \lambda \cdot r_1.$$

Now, I consider a binary consumption gamble, F, summarized by the probability q. Let $x_1 = r_1$ be the gamble outcome with probability q and $x_2 > r_2$ be the gamble outcome with probability $1 - q$. I write the KR utility as

$$U(F|G) = q \cdot p \cdot x_1 + (1-q) \cdot p \cdot [x_2 + \lambda \cdot (x_2 - r_1)] + q \cdot (1-p) \cdot [x_1 + 1 \cdot (x_1 - r_2)] + (1-q) \cdot (1-p) \cdot [x_2 + 1 \cdot (x_2 - r_2)]$$

and note that only one term features loss aversion λ as $x_2 > r_2$. I carry out the replacements $r_2 = 0$ and $x_1 = r_1$ such that this reduces to

$$U(F|G) = q \cdot p \cdot r_1 + (1-q) \cdot p \cdot [x_2 + \lambda \cdot (x_2 - r_1)] + q \cdot (1-p) \cdot 2r_1 + (1-q) \cdot (1-p) \cdot 2x_2,$$

$$U(F|G) = [q \cdot r_1 + (1-q) \cdot x_2] \cdot [2 + p \cdot \lambda - p] - p \cdot \lambda \cdot r_1.$$

The inverted uncertainty equivalent will be the consumption gamble, F^*, with corresponding probability q^* satisfying $U(F^*|G) = U(G|G)$,

$$p \cdot r_1 \cdot [2 + p \cdot \lambda - p] - p \cdot \lambda \cdot r_1 = [q^* \cdot r_1 + (1-q^*) \cdot x_2] \cdot [2 + p \cdot \lambda - p] - p \cdot \lambda \cdot r_1;$$

$$p \cdot r_1 = q^* \cdot r_1 + (1-q^*) \cdot x_2.$$

Note that the left hand side corresponds to the expected value of the referent gamble with $r_2 = 0$. The right hand side corresponds to the expected value of the consumption gamble with $x_1 = r_1$. The inverted uncertainty equivalent reveals where the gamble expected values are equal and so, independent of loss aversion, λ, risk neutrality is expected.

4.7.2 Appendix Tables

Table 4.4: Additional Risk Attitude Regressions

	(1)	(2)	(3)
Dependent Variable:	*Risk Attitude Classification*		
Panel A: Including Multiple Switchers			
Condition 1 (=1)	1.303***	1.304***	1.128***
	(0.206)	(0.207)	(0.223)
Risk Neutral Response	Yes	Yes	Yes
Demographic Controls	No	No	Yes
Order and Representation	No	Yes	Yes
# Observations	1904	1904	1750
# Clusters	136	136	125
Panel B: All Treatment Interactions			
Condition 1 (=1)	1.330***	1.784***	1.450**
	(0.225)	(0.409)	(0.540)
Condition 1 (=1) x Order 2 (=0) x Rep (=1)		-0.402	-0.153
		(0.613)	(0.684)
Condition 1 (=1) x Order 2 (=1) x Rep (=0)		-0.709	-0.200
		(0.584)	(0.746)
Condition 1 (=1) x Order 2 (=1) x Rep (=1)		-0.738	-0.634
		(0.593)	(0.686)
Condition 1 (=0) x Order 2 (=0) x Rep (=1)		0.050	-0.114
		(0.377)	(0.394)
Condition 1 (=0) x Order 2 (=1) x Rep (=0)		0.271	-0.178
		(0.351)	(0.435)
Condition 1 (=0) x Order 2 (=1) x Rep (=1)		0.443	0.326
		(0.287)	(0.321)
Condition 1 (=0)	-	-	-
Risk Neutral Response	Yes	Yes	Yes
Demographic Controls	No	No	Yes
# Observations	1708	1708	1554
# Clusters	122	122	111

Notes: Coefficients from ordered logit of *Risk Attitude* classification on control variables, measured from probability and certainty equivalents of Conditions 1.1, 1.2, 2.1, and 2.2. *Risk Attitude* takes the value -1 for risk loving, 0 for risk neutral, and +1 for risk averse. Standard errors clustered on the individual level in parentheses. Demographics, Order and Representation correspond to variables in Table 4.3.
Level of significance: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

4.7.3 Experimental Materials

Instructions

Hello and Welcome.

ELIGIBILITY FOR THIS STUDY: To be in this study, you must be a UCSD student. There are no other requirements. The study will be completely anonymous. We will not collect your name, student PID or any other identifying information. You have been assigned a participant number and it is on the note card in front of you. This number will be used throughout the study. Please inform us if you do not know or cannot read your participant number.

EARNING MONEY: To begin, you will be given a $5 minimum payment. This $5 is yours. Whatever you earn from the study today will be added to this minimum payment. All payments will be made in cash at the end of the study today.

In this study you will make decisions between two options. The first option will always be called OPTION A. The second option will always be called OPTION B. Each decision you make is a choice. For each decision, all you have to do is decide whether you prefer OPTION A or OPTION B.

These decisions will be made in 3 separate blocks of tasks. Each block of tasks is slightly different, and so new instructions will be read at the beginning of each task block.

Once all of the decision tasks have been completed, we will randomly select one decision as the decision-that-counts. Each decision has an equal chance of being the decision-that-counts. If you preferred OPTION A, then OPTION A would be implemented. If you preferred OPTION B, then OPTION B would be implemented.

Throughout the tasks, either OPTION A, OPTION B or both will involve chance. You will be fully informed of the chance involved for every decision. Once we know which is the decision-that-counts, and whether you prefer OPTION A or OPTION B, we will then determine the value of your payments.

For example, OPTION A could be a 75 in 100 chance of receiving $10 and a 25 in 100 chance of receiving $30. This might be compared to OPTION B of a 50 in 100 chance of receiving $30 and a 50 in 100 chance of receiving nothing. Imagine for a moment which one you would prefer. You have been provided with a calculator should you like to use it in making your decisions.

If this was chosen as the decision-that-counts, and you preferred OPTION A, we would then randomly choose a number from 1 to 100. This would be done by throwing two ten-sided die: one for the tens digit and one for the ones digit (0-0 will be 100). If the chosen number was between 1 and 75 (inclusive) you would receive $10 (+5 minimum payment) = $15. If the number was between 76 and 100 (inclusive) you would receive $30 (+5 minimum payment) = $35. If, instead, you preferred OPTION B, we would again randomly choose a number from 1 to 100. If the chosen number was between 1 and 50 (inclusive) you'd receive \$0 (+5 minimum payment) = \$5. If the number was between 51 and 100 (inclusive) you'd receive \$30 (+5 minimum payment) = \$35.

In a moment we will begin the first task.

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