NBER WORKING PAPER SERIES

CONTROLLING FOR COMPROMISE EFFECTS DEBIASES ESTIMATES OF PREFERENCE PARAMETERS

Jonathan P. Beauchamp
Daniel J. Benjamin
Christopher F. Chabris
David I. Laibson

Working Paper 21792
http://www.nber.org/papers/w21792

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2015

We thank Helga Fehr-Duda, Philipp Koellinger, Kevin McCabe, Ted O'Donoghue, Matthew Rabin, and Charlie Sprenger for helpful comments. For research assistance, we are grateful to Jonathan Cohen, Brice Cooke, Jaesun Lee, and especially Brendan Price and Alexandra Roulet. Research reported in this publication was supported by the National Institute on Aging of the National Institutes of Health under Award Numbers R01AG021650 and P01AG005842. The content is the sole responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health or of the National Bureau of Economic Research. Beauchamp thanks the Pershing Square Fund for Research on the Foundations of Human Behavior, the Social Sciences and Humanities Research Council of Canada, and the Trudeau Foundation for financial support.

Controlling for Compromise Effects Debiases Estimates of Preference Parameters
Jonathan P. Beauchamp, Daniel J. Benjamin, Christopher F. Chabris, and David I. Laibson
NBER Working Paper No. 21792
December 2015
JEL No. B49,D03,D14,D83,G11

ABSTRACT

The compromise effect—a tendency to choose options close to the "middle" of a choice set—has been shown to confound measurement of preferences. In an experiment with 550 participants, we study risk preferences elicited with Multiple Price Lists. Following prior work, we manipulate the compromise effect by varying the middle options of each Multiple Price List and find that measured risk-preference estimates are sensitive to this change in the choice set. To eliminate this bias, we incorporate context effects directly into a structural econometric model. We show that this method generates robust estimates of preference parameters.

Jonathan P. Beauchamp
Department of Economics
Harvard University
Cambridge, MA 02138
beauchamp.jonathan@gmail.com

Daniel J. Benjamin
Center for Economics and Social Research
University of Southern California
635 Downie Way, Suite 312
Los Angeles, CA 90089-3332
and NBER
daniel.benjamin@gmail.com

Christopher F. Chabris
Union College
807 Union Street
Schenectady, NY 12308
chabrisc@union.edu

David I. Laibson
Department of Economics
Littauer M-12
Harvard University
Cambridge, MA 02138
and NBER
dlaibson@gmail.com

Supplementary materials available at http://www.nber.org/papers/w21792:
- online appendix

1 Introduction

Psychologists and economists have identified many ways in which people's choices are influenced by contextual features of choice sets. One of the best documented examples is the compromise effect: a tendency to choose the "middle" option (Simonson 1989).[1] Such an effect has been shown to confound measurement of risk preferences elicited using Multiple Price Lists (Birnbaum 1992, Stewart, Chater, Stott, and Reimers 2003, Harrison, Lau, Rutström, and Sullivan 2005, Andersen, Harrison, Lau, and Rutström 2006, Harrison, Lau, and Rutström 2007, Harrison, List, and Towe 2007). We propose and estimate an econometric model that allows us to disentangle the compromise effect from risk preferences.[2]

We apply our framework to data we collected in a laboratory experiment with 550 participants. In this experiment, we elicit risk preferences using the Multiple Price List (MPL) method. Using participants' choices, we estimate the parameters of (cumulative) prospect theory. Prospect theory is one of the most commonly used models of risk preferences (e.g., Tversky and Kahneman 1992, Wakker 2010, Bruhin et al 2010), and MPLs are the most common method of eliciting preferences (e.g., Holt and Laury 2002, Harrison et al 2007, Andersen et al 2008).

Consistent with the earlier work cited above (e.g., Harrison et al., 2005), we find that subjects tend to choose a switchpoint biased toward the *middle* of the MPL—i.e., a compromise effect. To understand the potential role of such a compromise effect, consider the following example. The screenshot below is drawn from our own experiment and is typical of MPL experiments. In this example, a participant is asked to make seven binary choices. Each of the seven choices is between a gamble (in this case, a 10% chance of gaining \$100 and a 90% chance of gaining \$50) and a sure-thing alternative. The gamble doesn't change across the seven rows, while the alternative varies from high to low.

[1] For a discussion of microfoundations for the compromise effect, see Kamenica (2008). Extending his framework to our experiment, participants might believe that the experimenter has designed the choices so that it is optimal for the typical participant to switch toward the middle rows. If participants let others' preferences serve as a guide to how they should choose (perhaps because of cognitive costs associated with making a fully deliberative choice), participants might tilt their choices toward switching toward the middle rows. Alternatively, participants may not have thought-through beliefs about the attractiveness of the middle option but may follow a heuristic of biasing choices that way, perhaps because this heuristic generally leads to reasonable choices at low cognitive cost.

[2] Throughout, we assume that stable, "deep" preferences exist. An alternative (psychological) tradition views preferences as "constructed" and inherently unstable from one choice to the next (e.g., Slovic, 1995).

A gamble gives you a 10% chance of gaining $100 and a 90% chance of gaining $50 instead.

Would you rather…

(a)	Take the gamble	OR	Gain $57.00
(b)	Take the gamble	OR	Gain $56.90
(c)	Take the gamble	OR	Gain $56.70
(d)	Take the gamble	OR	Gain $56.40
(e)	Take the gamble	OR	Gain $55.90
(f)	Take the gamble	OR	Gain $55.00
(g)	Take the gamble	OR	Gain $53.60

If this subject were to display a strong compromise effect, she would act as if she were indifferent between the gamble and the sure-thing in the middle row, row (d). Such indifference would imply that she is risk seeking because the gamble has a lower expected value than the sure thing in row (d). A sufficiently strong compromise effect can distort revealed risk preferences—in this case, leading a participant who is otherwise risk-averse to make risk-seeking choices.

To econometrically model the compromise effect, we augment a discrete-choice model with additional parameters that represent a penalty for choosing a switchpoint further from the middle. The compromise-effect parameters are identified in our data because we experimentally vary the middle options. Following prior work (Birnbaum 1992, Stewart, Chater, Stott, and Reimers 2003, Harrison, Lau, Rutström, and Sullivan 2005, Andersen, Harrison, Lau, and Rutström 2006, Harrison, Lau, and Rutström 2007, and Harrison, List, and Towe 2007), we use *scale manipulations*, in which we hold the lowest and highest alternatives of the MPL fixed and manipulate the locations of the five intermediate outcomes within the scale. For example, compare the screenshot above to the screenshot that follows, which has new alternatives in rows (b) through (e), although rows (a) and (f) are the same. With respect to this second Multiple Price List, an agent who acts as if the middle option, row (d), is her indifference point would be judged to be risk averse.

A gamble gives you a 10% chance of gaining $100 and a 90% chance of gaining $50 instead.

Would you rather…

(a)	Take the gamble	OR	Gain $57.00
(b)	Take the gamble	OR	Gain $55.60
(c)	Take the gamble	OR	Gain $54.70
(d)	Take the gamble	OR	Gain $54.20
(e)	Take the gamble	OR	Gain $53.90
(f)	Take the gamble	OR	Gain $53.70
(g)	Take the gamble	OR	Gain $53.60

In our experiment, each participant is exposed to one of five different scale treatment conditions.

Our ex-ante hypotheses focus on two key prospect-theory parameters: curvature γ (which captures risk aversion over gains and risk seeking over losses) and loss aversion λ (which captures the degree to which people dislike losses more than they like gains).[3] Our analysis yields three main findings. First, when we estimate the prospect-theory parameters while including additional parameters to capture the compromise effect, our estimates of γ and λ are robust across the five scale treatment conditions. (When estimating the model with the questions from all parts of the experiment together, our estimates are $\hat{\gamma} = 0.24$ and $\hat{\lambda} = 1.31$, which falls within the range of estimates in the existing literature.[4]) The robustness of these preference-parameter estimates suggest that they are not biased by the compromise effect.

Second, our estimates of the compromise-effect parameters replicate the findings from earlier work that participants have a bias toward choosing a switchpoint in the middle rows of the MPL (e.g., Harrison et al., 2005; see other references above). Moreover, our quantitative estimates indicate that the bias is sizeable; we estimate that the attractiveness of the middle rows relative to the extreme rows represents 17%-23% of the prospects' monetary value.

Third, when we estimate the prospect-theory model *without* controlling for the compromise effect, the scale manipulations have a very powerful effect on the preference-parameter estimates. In particular, the compromise effect is strong enough to cause us to estimate either risk seeking (as predicted by prospect theory) or risk aversion (the opposite of what is predicted by prospect theory) in the loss domain, depending on the scale manipulations. The compromise effect can also make behavior look as if there is no loss aversion.

In addition to the scale manipulations described above, we also study the effect of telling experimental participants the expected value of the risky prospects. Ex ante, we hypothesized that this manipulation would anchor the participants on the expected value (as in Tversky and Kahneman's 1974 anchoring experiments) thereby nudging their preferences toward risk neutrality. Surprisingly to us, however, in no case do we find evidence that this expected-value manipulation

[3] We predicted that our scaling manipulations would not substantially change the estimated parameters of the probability weighting function, because the prospects all have "probability-flipped" variants: i.e., for each MPL featuring a prospect with probability p of monetary outcome x_H and probability $1-p$ of monetary outcome x_L, the experiment includes another MPL featuring a probability-flipped prospect with probability $1-p$ of outcome x_H and probability p of outcome x_L. Scaling manipulations will have (approximately) offsetting effects with respect to the probability weighting function for these two probability-flipped prospects.

[4] Our estimate of λ falls towards the lower end of the range of existing estimates.

is quantitatively important. In our data, providing expected value information does not affect measured risk aversion nor measured loss aversion.

This paper differs from earlier work demonstrating the effects of scaling manipulations by estimating a model that explicitly accounts for the compromise effect and enables us to separately estimate it from risk preferences. Moreover, our sample is larger than those used in earlier work, which allows us to precisely estimate the scale treatment effects. In addition, because we pose gambles involving losses as well as gambles involving gains, we can study the effect of scale manipulations not only on risk aversion over gains, but also on risk aversion over losses and on loss aversion.

Like us, Lichtenstein, Slovic, and Zink (1969) and Montgomery and Adelbratt (1982) both conclude that there is no effect on risk aversion of telling participants the expected values of gambles. But in both papers, the data are difficult to interpret because there is a small, statistically insignificant tendency for participants to behave more risk-neutrally when they are told the expected value, and the authors' conclusions are based primarily on the fact that most participants self-report not using the expected value information. On the other hand, Harrison and Rutström (2008) *do* find that providing expected value information significantly decreases risk aversion. The difference may arise because the prospects in their experiment are relatively complex, each involving four possible outcomes (vs. one or two in our experiment); we speculate that participants may rely more on the expected value information when the prospects are more complex.

A limitation of our experiment is that only one out of its four parts is incentivized (Part A, which involves only prospects with monetary gains). Reassuringly, our main results hold when we restrict attention to the incentivized data.

The rest of the paper is organized as follows. In Section 2, we discuss our experimental design. In Section 3, we propose a model that accounts for both compromise effects and prospect theory, and we describe our estimation strategy. In Section 4, we list and discuss the five formal hypotheses that we test. In Section 5, we report the results of the estimation of our model, and we test the robustness of its estimates to the scale manipulations. Section 6 parallels Section 5 but examines the prospect-theory model without controls for compromise effects. Section 7 estimates the economic magnitude and importance of compromise effects. Section 8 discusses the results of our expected value manipulation. Section 9 concludes.

2 Experiment

2.1 Design

Throughout the experiment, we employ the Multiple Price List (MPL) elicitation method (Holt and Laury, 2002).[5] At the top of each computer screen, a *fixed prospect* is presented. The fixed prospect is usually a non-degenerate lottery; it is "fixed" in the sense that it is an option in all of the binary choices on that screen. (The fixed prospect changes *across* screens.) On each screen, seven binary choices are listed below the fixed prospect. Each binary choice is made between the fixed prospect (at the top of the screen) and what we refer to as an *alternative* (or *alternative prospect*). The alternatives vary within a screen, with one alternative for each of the seven binary choices. In some (but not all) cases, the alternatives are sure things. Screenshots of the experiment are shown in the Introduction as well as in the Appendix.

Our set-up for eliciting risk preferences is standard. Indeed, we designed many details of our experiment—such as giving participants choices between a fixed prospect and seven alternatives—to closely follow Tversky and Kahneman's (1992; henceforth T&K) experiment in their paper that introduced Cumulative Prospect Theory (CPT). Moreover, our set of fixed prospects is identical to the set used by T&K. Further mimicking T&K's procedure, our computer program enforces consistency in the participants' choices by requiring participants to respond monotonically to the seven choices on the screen.[6] Our algorithm for generating the seven alternatives is explained in Section 2.2 and in the Online Appendix, where we also list the complete set of fixed prospects and alternatives.[7]

[5] While our procedure is a Multiple Price List according to conventional usage of the term, it is not the same as Holt and Laury's (2002) procedure. Holt and Laury offer their participants choices between a fixed gamble and a set of alternative gambles that vary in the probability of the good outcome. In contrast, as illustrated by the screenshots in the Introduction, our alternatives are sure-things (not gambles). Accordingly, across the rows we vary the value of the sure-thing alternative.

[6] More precisely, participants have to select only two circles: the one corresponding to the worst alternative outcome they prefer to the fixed prospect and the one corresponding to the fixed prospect in the following row. An auto-fill feature of the computer program fills in the other circles.

This procedure is a version of the "Switching MPL" (or "sMPL") design discussed by Andersen et al. (2006), in which participants are asked to choose at which row they want to switch.

[7] Our procedure differs from T&K's in three important ways. First, our algorithm for generating the seven alternative outcomes necessarily differs from theirs because theirs is described in too little detail to exactly imitate it (and the actual values are not reported). Second, while their gambles were all hypothetical, our "Part A" gambles (discussed below) were incentivized. Third, for each screen, T&K implement a two-step procedure for identifying risk preferences: after finding the point at which participants switch from preferring the alternative outcomes to preferring the fixed prospect, they have the participant make choices between the fixed prospect and a second set of seven alternative outcomes, linearly spaced between a value 25% higher than the lowest amount accepted in the

Each participant faces a total of 64 screens in the experiment, each of which contains seven choices between a fixed prospect and alternatives. There are four types of screens that differ from each other in the kinds of prospects and alternatives they present. To make it easier for participants to correctly understand the choices we are presenting to them, we divide the experiment into four sequential parts (each with its own instruction screen), with each part containing a single type of fixed prospect and a single type of alternative. The order of the screens is randomized within each part, with half the participants completing the screens in one order, and the other half completing the screens in the reverse order.

In Part A, the fixed prospects are in the gain domain, and the alternatives are sure gains (as in the example screens in the Introduction). There are 28 fixed prospects that differ both in probabilities and money amounts, which range from \$0 to \$400. The seven alternatives for each fixed prospect range from the fixed prospect's certainty equivalent for a CRRA expected-utility-maximizer with CRRA parameter $\gamma = 0.99$ to the certainty equivalent for $\gamma = -1$ (which is risk seeking).[8] Because the range of estimates of γ in the literature falls well within this interval (Booij, van Praag, and Kullen, 2010), the interval likely covers the relevant range of alternatives for the participants. Each participant is told that there is a 1/6 chance that one of his or her choices in Part A will be randomly selected and implemented for real stakes at the end of the experiment. The expected payout for a risk-neutral participant who rolls a 6 is about \$100. The remaining parts of the experiments involve hypothetical stakes.

In Part B, the fixed prospects now have outcomes in the loss domain, and the alternatives are sure losses. The 28 prospects and alternatives in Part B are identical to those in Part A but with all dollar amounts multiplied by -1.

Parts C and D depart somewhat from the baseline format of our experiment, in that the alternatives are now risky prospects rather than sure things.[9] Moreover, in Part C, the fixed prospect is the degenerate prospect of a sure thing of \$0 and is not listed at the top of each screen.

first set and a value 25% lower than the highest amount rejected. We avoid this two-step procedure (which Harrison, Lau and Rutström, 2007, call an "Iterative Multiple Price List"), partly because it takes more experimental time to implement and partly because it is generally not incentive compatible. Specifically, the experimental participant may have an incentive to distort his/her indifference point for the alternative outcome in the first stage toward a higher monetary value in order to have high monetary values for the alternative outcomes in the second stage.

[8] We use $\gamma = 0.99$, rather than $\gamma = 1$, to generate our lowest alternative outcomes because $\gamma = 1$ corresponds to log utility and implies a certainty equivalent of \$0 for any prospect with a chance of a \$0 outcome, regardless of how small the probability of that \$0 outcome is.

[9] K&T designed Parts C and D primarily to measure loss aversion.

The seven alternatives on each of the four screens in Part C are mixed prospects that have a 50% chance of a loss and 50% chance of a gain. For example, one of the screens in Part C is:

A gamble gives you a 50% chance of losing $150 and …

(a)	… a 50% chance of gaining $0.00 instead.	Take the gamble	OR	Don't take the gamble
(b)	… a 50% chance of gaining $14.90 instead.	Take the gamble	OR	Don't take the gamble
(c)	… a 50% chance of gaining $39.60 instead.	Take the gamble	OR	Don't take the gamble
(d)	… a 50% chance of gaining $80.60 instead.	Take the gamble	OR	Don't take the gamble
(e)	… a 50% chance of gaining $148.80 instead.	Take the gamble	OR	Don't take the gamble
(f)	… a 50% chance of gaining $262.00 instead.	Take the gamble	OR	Don't take the gamble
(g)	… a 50% chance of gaining $450.00 instead.	Take the gamble	OR	Don't take the gamble

On any given screen, the amount of the possible loss is fixed, and the seven mixed prospects involve different amounts of the possible gain. Part C has four screens, each with a different loss amount: $25, $50, $100, and $150.

Part D also comprises four screens, each containing choices between a fixed 50%-50% risky prospect and seven alternative 50%-50% risky prospects. On two of the four screens, both the fixed prospect and the alternatives are mixed prospects, i.e., one possible outcome is a gain and the other is a loss, as in the following:

Gamble 1 gives you a 50% chance of losing $50 and a 50% chance of gaining $150

Gamble 2 gives you a 50% chance of losing $125 and …

(a)	… a 50% chance of gaining $375.00 instead.	Take gamble 1	OR	Take gamble 2
(b)	… a 50% chance of gaining $356.30 instead.	Take gamble 1	OR	Take gamble 2
(c)	… a 50% chance of gaining $332.50 instead.	Take gamble 1	OR	Take gamble 2
(d)	… a 50% chance of gaining $302.00 instead.	Take gamble 1	OR	Take gamble 2
(e)	… a 50% chance of gaining $263.10 instead.	Take gamble 1	OR	Take gamble 2
(f)	… a 50% chance of gaining $213.40 instead.	Take gamble 1	OR	Take gamble 2
(g)	… a 50% chance of gaining $150.00 instead.	Take gamble 1	OR	Take gamble 2

On the other two screens, the fixed and the alternative prospects involve only gains.[10] On any given screen, one of the two possible realizations of the alternative prospect is fixed, and the seven choices on the screen involve different amounts of the other possible realization of that prospect. For each screen in Parts C and D, the alternative prospects range from the amount that would make an individual with linear utility, no probability distortion, and loss insensitivity ($\lambda = 0$)

[10] K&T designed these two screens as placebo tests for loss aversion; we therefore do not use the data from these screens in our estimation.

indifferent to the fixed prospect to the amount that would make an individual with loss aversion $\lambda = 3$ indifferent.

After Parts A-D, participants complete a brief questionnaire that asks age, race, educational background, standardized test scores, ZIP code of permanent residence, and parents' income (if the participant is a student) or own income (if not a student). It also asks a few self-reported behavioral questions, including general willingness to take risks and frequency of gambling.

2.2 Treatments

As detailed below, the experiment has a 5×2 design, with five "Pull" treatments, which vary the set of alternatives, crossed with two "EV" treatments, which vary whether the expected value of the prospects is displayed or not. Each participant is randomly assigned to one of the ten treatment cells and remains in this cell for all screens and all parts (A-D) of the experiment.

The Pull treatments allow us to assess whether compromise effects impact measured risk and loss preferences. The five treatments are identical in the set of fixed prospects and in the first and seventh alternative on each screen but differ from each other in the intermediate (the second through sixth) alternatives. For instance, in Part A for the illustrative fixed prospect above in the screenshots in the Introduction—a 10% chance of gaining \$100 and a 90% chance of gaining \$50—the alternatives (a) through (g) are shown in the positive half of Figure 1 for all five Pull treatments.

The five treatments are labeled Pull -2, Pull -1, Pull 0, Pull 1, and Pull 2. In the Pull 0 treatment, the alternatives are evenly spaced, aside from rounding to the nearest \$0.10, from the low amount of \$53.60 to the high amount of \$57.00. In the Pull 1 and the Pull 2 treatments, the alternatives are more densely concentrated at the monetary amounts closer to zero. These treatments are designed to resemble T&K's experiment, in which the second through sixth alternatives are "logarithmically spaced between the extreme outcomes of the prospect" (T&K, p. 305). Conversely, in the Pull -1 and Pull -2 treatments, the alternatives are more densely concentrated at the monetary amounts farther from zero. Pull 2 and Pull -2 are more skewed than Pull 1 and Pull -1. We refer to the different treatments as "Pulls" to convey the intuition that they pull the distributions of the alternatives toward zero (for the positive Pulls) or away from zero (for the negative Pulls).

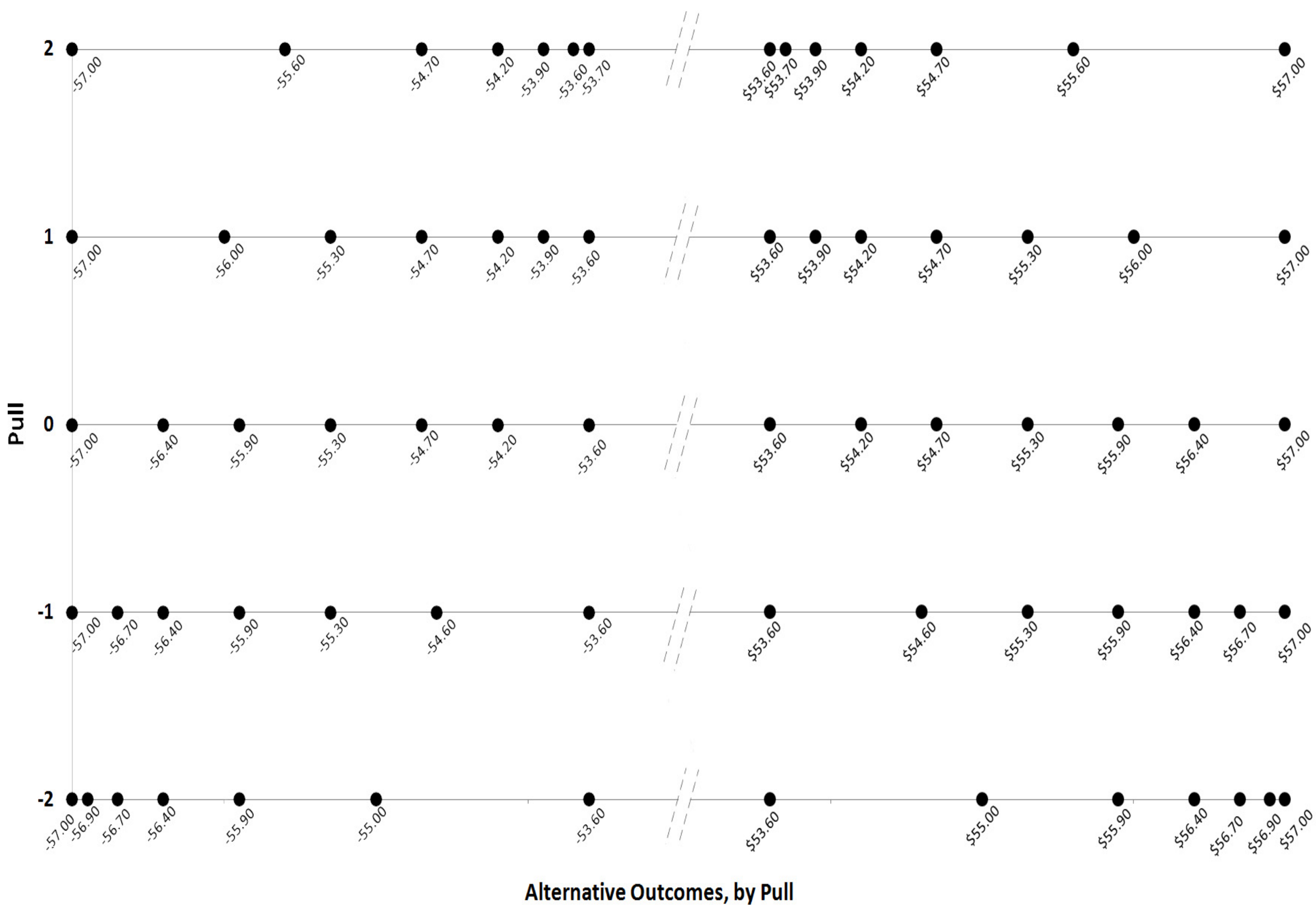


FIGURE 1. Alternative outcomes by Pull treatment for example screens. The right side of the figure shows alternative outcomes by Pull treatment for an example screen from Part A with a fixed prospect offering a 10% chance of gaining \$100 and a 90% chance of gaining \$50. The left side of the figure shows alternative outcomes by Pull treatment for an example screen from Part B with a fixed prospect offering a 10% chance of losing \$100 and a 90% chance of losing \$50.

Analogously, in Parts C and D, Pull 1 and Pull 2 pull the distribution of the varying amounts of the alternative prospect on each screen toward zero, and Pull -1 and Pull -2 do the opposite. The Online Appendix describes the precise algorithm we use to determine the second through sixth alternatives and shows the complete set of fixed prospects and alternatives for each Pull treatment and for each part of the experiment.

The EV treatments differ in whether or not we inform participants about the expected values of the prospects. Because we anticipated that many participants would be unfamiliar with the concept of expected value, simple language is used in the "EV treatment" to describe it. For instance, in Part A, the following appears below the fixed prospect at the top of the screen: "On average, you would gain $55 from taking this gamble."

2.3 Procedures and Sample

The experiment was run online from March 11 to March 20, 2010. Our sample was drawn from the Harvard Business School Computer Lab for Experimental Research's (CLER) online subject pool database. This database contains several thousand participants nationwide who are available to participate in online studies. Participants had to be at least 18 years old, eligible to receive payment in the U.S., and off Harvard University's regular payroll. They are mainly recruited through flyer postings around neighboring campuses.

At the launch of the experiment, the CLER lab posted a description to advertise the experiment to the members of the online subject pool database. Any member of the pool could then participate until a sample size of 550 was reached. Each participant was pseudo-randomly assigned to one Pull and to one EV treatment to ensure that our treatments were well-balanced. A total of 521 participants completed all four parts of the experiment. The mean response time for the participants who completed the experiment in less than one hour was 32 minutes.[11]

In addition to the above-described incentive payment for Part A, participants were paid a total of $5 if they began the experiment; $7 if they completed Part A; $9 if they completed Parts A and B; $11 if they completed Parts A, B, and C; and $15 if they completed all four parts of the experiment.

[11] Participants were allowed to complete the experiment in more than one session, so response times were larger than 24 hours for some. Of the 497 participants for whom we have response time data, 405 took less than one hour.

3 Model and Estimation

3.1 Baseline CPT Model

For prospect $P = (x_H, p_H; x_L, p_L)$ with probability p_H of monetary outcome x_H and probability $p_L = 1 - p_H$ of monetary outcome x_L, we assume that utility has the form:

$$U(P) = \left\{ \begin{array}{ll} \omega(p_H) \cdot u\left(x_H\right) + (1 - \omega(p_H)) \cdot u\left(x_L\right) & \text{if } 0 < x_L < x_H \\ -\omega(p_L) \cdot \lambda \cdot u\left(-x_L\right) - (1 - \omega(p_L)) \cdot \lambda \cdot u\left(-x_H\right) & \text{if } x_L < x_H < 0 \\ \omega(p_H) \cdot u\left(x_H\right) - \omega(p_L) \cdot \lambda \cdot u\left(-x_L\right) & \text{if } x_L < 0 < x_H \end{array} \right\}, \tag{1}$$

where $\omega(\cdot)$ is the cumulative probability weighting function and satisfies $\omega(0) = 0$ and $\omega(1) = 1$, $u(\cdot)$ is the Bernoulli utility function and satisfies $u(0) = 0$, and λ is the coefficient of loss aversion. We assume that $u(\cdot)$ takes the CRRA (a.k.a. "power utility") form, $u(x) = \frac{x^{1-\gamma}}{1-\gamma}$, as is standard in the literature on CPT (e.g., Trepel, Fox, and Poldrack, 2005; T&K).

We use the Prelec (1998) probability weighting function:

$$\omega(p) = \exp(-\beta(-\log(p))^{\alpha}),$$

where α, $\beta > 0$. The α and β parameters regulate the curvature and the elevation of $\omega(p)$, respectively.[12]

3.2 Modeling Compromise Effects

We model compromise effects by assuming that, in addition to their baseline CPT preferences, participants suffer a loss in utility from choosing a switchpoint farther from the middle row on the screen. Formally, recall that on each screen q of the experiment, a participant makes choices between a fixed prospect, denoted P_{qf}, and seven alternatives presented in decreasing order of monetary payoff, denoted P_{q1}, P_{q2}, ..., P_{q7}.[13] Following Hey and Orme (1994), we use a Fechner error specification and assume that on any screen q, the participant chooses P_{qi} over P_{qf} if and

[12] As a robustness check, we estimated the model with T&K's probability weighting function (with the data from all parts of the experiment): $\omega(p) = p^{\alpha}/(p^{\alpha} + (1-p)^{\alpha})^{\frac{1}{\alpha}}$. The results presented below are robust to the use of this alternative function (see the Online Appendix for details).

[13] In Part C, the alternative prospects are presented in increasing order of monetary payoff. We ignore this subtlety here for expositional purposes.

only if

$$(2) \qquad \frac{U(P_{qi})}{\sigma_q} + c_i + \varepsilon_{qA} > \frac{U(P_{qf})}{\sigma_q} + \varepsilon_{qf} \quad \Longleftrightarrow \quad \varepsilon_q < \frac{U(P_{qi}) - U(P_{qf})}{\sigma_q} + c_i,$$

where c_i is a constant that depends on the row i in which the alternative P_{qi} appears, σ_q is parameter to regulate the relative importance of the utility function vs. the other arguments, and ε_{qf}, ε_{qA}, and ε_q are preference shocks that vary across (but not within) screens. We assume that $\varepsilon_{qf} - \varepsilon_{qA} \equiv \varepsilon_q \sim N(0,1)$. We refer to c_i as the *compromise effect* of row i, and we assume that $\Sigma_{i=1}^{7} c_i = 0$.[14]

3.3 Estimation

We estimate the model via Maximum Likelihood Estimation, pooling participants together and clustering the standard errors at the participant level. We impose the parameter restriction $\gamma < 1$.[15] We simplify the estimation in two ways. First, we reduce the number of σ_q parameters by assuming that σ_q is identical for screens involving prospects of similar magnitudes.[16] Second, we assume that c_i takes the quadratic functional form $c_i = \pi_0 + \pi_1 \cdot i + \pi_2 \cdot i^2$.[17] With this functional form, the constraint $\Sigma_{i=1}^{7} c_i = 0$ implies a linear restriction among the parameters, $\pi_0 = -4\pi_1 - 20\pi_2$, so we estimate the two parameters π_1 and π_2.

[14] This assumption implies that compromise effects do not on average bias participants towards selecting either the alternative or the fixed prospect across the rows of a screen.

[15] Fifteen of the 28 fixed prospects in Part A have a chance of yielding \$0 (and likewise for Part B). $\gamma \geq 1$ would imply extremely risk-averse behavior with these 15 prospects, such that any positive alternative sure outcome would always be preferred with probability 1. Every participant in the experiment made choices ruling out such extreme risk aversion, except for one participant. That participant picked the alternative sure outcome in every single choice in Part A. (As discussed below, we excluded from the estimation participants for whom the MLE did not converge when estimated using only their data. This participant's data were excluded as a result.)

[16] More precisely, for Part A, we estimate a σ_q parameter for each of five groups of screens. Screens are grouped together based on the expected utility of their fixed prospects; the latter is calculated based on the parameter estimates reported by Fehr-Duda and Epper (2012, Table 3) for their representative sample. We thus estimate $\sigma_{A,0-25}$, $\sigma_{A,25-50}$, $\sigma_{A,50-75}$, $\sigma_{A,75-100}$, $\sigma_{A,100+}$, where $\sigma_{A,L-H}$ is for screens with a fixed prospect whose expected value is between L and H. For Part B, we proceed analogously. We also estimate $\sigma_{C,\text{small}}$ and $\sigma_{C,\text{big}}$ for the two smaller and the two larger prospects of Part C, respectively, and σ_D for the two prospects of Part D.

We also attempted to estimate the model with a different σ_q parameter for each screen. The results are robust to that specification when the data from Part A only are used. However, we encountered convergence problems when estimating the model with the data from all parts of the experiment (because of the very large number of parameters estimated simultaneously) and with the data from Part B (because the MLE maximization algorithm pushed the σ_q parameter for one of the screens toward infinity).

[17] An alternative would have been to assume a linear specification for c_i, but that would have constrained c_i in the middle row to equal zero—a feature of the results that bears directly on whether participants have a tendency to switch in the middle row, as discussed below. The quadratic specification is more flexible and does not impose this constraint.

For each specification, we produce three sets of estimates. First, we estimate γ, α, and β (and the other parameters) with data from all screens from Parts A-D.[18] To do so, we assume that γ, α, β are the same in the gain and loss domains. Note that γ is then the coefficient of relative risk aversion in the gain domain and the coefficient of relative risk *seeking* in the loss domain. Second, we estimate γ^+, α^+, and β^+ (and the other parameters) with data from Part A only (which only includes questions in the gain domain and is incentivized). Lastly, we estimate γ^-, α^-, and β^- (and the other parameters) with data from Part B only (which only includes questions in the loss domain).

We exclude from the estimation data from participants for whom the MLE algorithm does not converge[19] when the CPT model without compromise effects is estimated separately for each participant with data from Parts A-D. We identified 28 such participants out of a total of 521 participants who completed all parts of the experiment, and most of them had haphazard response patterns.

To derive a likelihood function, first recall that the experimental procedure constrained participants to behave consistently: if a participant chooses P_{qi} over P_{qf} for some $i > 1$, then the participant chooses P_{qj} over P_{qf} for all $j < i$. Hence the probability that the participant switches from choosing the alternative when the alternative is P_{qi} to choosing the fixed prospect when the alternative is $P_{q(i+1)}$ is

$$\begin{aligned}
\Pr{}_{q,i,i+1} &\equiv \Pr\left(\text{participant switches between } P_{qi} \text{ and } P_{q(i+1)}\right) \\
&= \Pr\left(\frac{U(P_{q(i+1)}) - U(P_{qf})}{\sigma_q} + c_{i+1} < \varepsilon_q < \frac{U(P_{qi}) - U(P_{qf})}{\sigma_q} + c_i\right) \\
&= \Phi\left(\frac{U(P_{qi}) - U(P_{qf})}{\sigma_q} + c_i\right) - \Phi\left(\frac{U(P_{q(i+1)}) - U(P_{qf})}{\sigma_q} + c_{i+1}\right),
\end{aligned}$$

where $\Phi(\cdot)$ is the CDF of a standard normal random variable; the probability that the participant always chooses the fixed prospect is $\Pr{}_{q,-,1} \equiv 1 - \Phi((U(P_{q1}) - U(P_{qf}))/\sigma_q + c_1)$; and the probability that the participant always chooses the alternative over the fixed prospect is $\Pr{}_{q,7,-} \equiv \Phi((U(P_{q7}) -$

[18] We drop the two screens of Part D that involve only positive outcomes (designed by T&K as placebo tests for loss aversion) so that Parts C and D can be understood as primarily identifying $\hat{\lambda}$. Data from those two screens are excluded from all estimations. Here and from now on, whenever we refer to "all screens from Parts A-D," we mean all screens excluding these two.

[19] To be precise, we exclude participants for whom the relative change in the coefficient vector from one iteration to the next is still greater than 1×10^{-4} after 500 iterations of the MLE algorithm.

$U(P_{qf}))/\sigma_q + c_7)$. We assume that ε_q is drawn *i.i.d.* for each screen q in the set of screens, Q, faced by a participant.

Thus, the likelihood function for any given participant p is:[20]

$$L_p = \prod_{q \in Q} \prod_{i=0,1,\ldots,7} (\Pr{}_{q,i,i+1})^{1\{p \text{ switches between } P_{qi} \text{ and } P_{q,i+1}\}}.$$

The likelihood function for all the participants pooled together is $\Pi_{p \epsilon P} L_p$, where P is the set of participants.

4 Hypotheses

Having defined the model, we now articulate a number of hypotheses that we will test empirically by estimating the model with the data from the experiment. Drawing on prior work (see the Introduction for discussion), our starting point is the hypothesis that participants will be biased toward switching close to the middle of the seven rows in the Multiple Price List.

Hypothesis 1: Estimates of c_i will reveal a compromise effect. Specifically, $\hat{c}_i$ will be positive in the top rows, close to zero in the middle rows, and negative in the bottom rows, decreasing monotonically from the first to the last row.

Note that a positive value of c_i implies a bias in favor of choosing the alternative (which is in column 2 of the MPL), and a negative value of c_i implies a bias in favor of choosing the fixed prospects (which is in column 1 of the MPL). So Hypothesis 1 implies a switch point that is biased toward the middle row of the MPL.

Thus, compromise effects imply that measured risk aversion in the gain domain, as assessed in Part A, will be systematically increased across the range of treatments from Pull -2 to Pull 2 (in the model without compromise effects).[21] For instance, consider the two example screenshots from the Introduction. The first screenshot illustrates the Pull -2 treatment. Since the intermediate

[20] For notational simplicity, we write $\Pr_{q,0,1}$ for $\Pr_{q,-,1}$ and $\Pr_{q,7,8}$ for $\Pr_{q,7,-}$.

[21] The foregoing is accurate in the context of CRRA preferences with no probability weighting function. However, we estimate CPT preferences, which include a probability weighting function. Nonetheless, we believe that the logic underlying the following hypotheses is robust to the inclusion of reasonable specifications of a probability weighting function in the model.

alternatives are shifted away from zero, compromise effects induce participants to choose an indifference point that is farther from zero, thereby implying a relatively low level of risk aversion. In contrast, in the Pull 2 treatment, illustrated in the second screenshot, the intermediate alternatives are shifted closer to zero. Compromise effects cause participants to choose an indifference point that is closer to zero, thereby implying a relatively high level of risk aversion.

The hypothesized effect of the Pull treatments on measured risk *seeking* in the loss domain is analogous. Moving across the range of treatments from Pull -2 to Pull 2 is now hypothesized to raise estimated risk seeking. For example, consider a fixed prospect that has outcomes in the loss domain. In the Pull -2 treatment, the intermediate alternatives are all negative and shifted away from zero, coaxing participants to choose an indifference point that is farther from zero, thereby implying a relatively low level of risk seeking. By contrast, in the Pull 2 treatment, the intermediate alternatives are all negative and shifted relatively close to zero, coaxing participants to choose an indifference point that is closer to zero, thereby implying a relatively high level of risk seeking.

Similar considerations imply that moving across the range of treatments from Pull -2 to Pull 2 is predicted to reduce the level of estimated loss aversion.

We thus hypothesize that compromise effects affect estimates of risk aversion and loss aversion in the traditional CPT model. In Section 3.2 above, we introduced a model that incorporates compromise effects. If that model is properly specified, we would expect the bias induced by compromise effects to disappear and the estimates of risk aversion and loss aversion to be similar across Pull treatments. In summary, we hypothesize:

Hypothesis 2.a: Estimates of relative risk aversion in the gain domain (γ, γ^+) and relative risk seeking in the loss domain (γ, γ^-) from our model with compromise effects will not vary in Pull.

Hypothesis 2.b: Estimates of loss aversion (λ) from our model with compromise effects will not vary in Pull.

Hypothesis 3.a: Estimates of γ, γ^+, and γ^- from the model *without* compromise effects will be increasing in Pull.

Hypothesis 3.b: Estimates of λ from the model *without* compromise effects will be decreasing in Pull.

5 Estimating Compromise Effects and Risk Preferences Jointly

We begin by estimating our model with compromise effects. We focus our attention on the curvature parameter γ and the loss aversion parameter λ because our ex ante hypotheses are about these parameters. We do not interpret the results for the other parameters (α, β, and the σ_q parameters) because we did not have ex ante hypotheses, but we report the estimates for all parameters in the Online Appendix.

Table 1 shows the estimates for our parameters of interest. The estimates of γ (obtained from the data from all parts together), γ^+ (obtained from the data from Part A only), and γ^- (obtained from the data from Part B only) differ substantially from one another, ranging from $\hat{\gamma}^- = -0.106$ to $\hat{\gamma}^+ = 0.448$. These estimates are broadly in line with existing estimates in the literature, although the estimate of γ^- is below what is typically found.[22] Indeed, the estimate of γ^- is significantly smaller than 0 at the 5% level, indicating risk *aversion* in the loss domain, which is the opposite of what CPT predicts.

The estimate of λ (obtained from the data from all parts together) is 1.311, matching the lower range of loss aversion estimates in the literature.[23] The estimates of the probability weighting function parameters, $\hat{\alpha}$ and $\hat{\beta}$, are broadly in accord with findings from prior work.[24]

<INSERT TABLE 1 ABOUT HERE>

The sizeable difference between the estimates in Parts A and B suggests that the assumption that γ, α, and β are the same in the gain and loss domains is unsupported by the data. We nonetheless maintain this assumption when estimating the model with the data from all parts of the experiment because we are interested in studying $\hat{\lambda}$, and as Wakker (2010) points out, assuming different parameters in the gain and loss domains makes the loss aversion parameter more difficult

[22] Booij, van Praag, and Kullen's (2010) Table 1 reviews existing experimental estimates. Translated into the CRRA functional form we estimate, the range of existing parameter estimates is $\hat{\gamma}^+ \in [-0.01, .0.78]$ in the gain domain and $\hat{\gamma}^- \in [-0.06, 0.39]$ in the loss domain.

[23] Though T&K estimate λ to be 2.25, there is still no consensus about the value of λ in the literature. Among the papers reviewed by Abdellaoui, Bleichrodt, and Paraschiv (2007, Tables 1 and 5), the range of loss aversion estimates is $\hat{\lambda} \in [0.74, 8.27]$, and among the papers reviewed by Booij, van Praag, and Kuilen (2010, Table 1), the range is $\hat{\lambda} \in [1.07, 2.61]$.

[24] Our estimates of α range from 0.564 to 0.690 and our estimates of β range from 0.858 to 1.471. Booij, van Praag, and Kuilen's (2010) Table 1 only lists three studies that estimated the two-parameter Prelec (1998) functional form, and they only did so for prospects in the gain domain. The ranges of estimates are $\hat{\alpha}^+ \in [0.53, 1.05]$ and $\hat{\beta}^+ \in [1.08, 2.12]$. Hence, our $\hat{\beta}^+$ estimate (obtained from the data from Part A only) falls below the lower end of the range.

to interpret.[25]

5.1 Estimating Compromise Effects

We now proceed to test *Hypothesis 1*, which predicts that the compromise effects c_i will be positive in the top rows, close to zero in the middle rows, and negative in the bottom rows, and will decrease from the first to the last row.

The estimated c_i's are calculated from the estimates of π_1 and π_2, and their standard errors and confidence intervals are calculated using the delta method. Figure 2 shows the estimated c_i for each row i (the numerical values are listed in the Online Appendix). As can be seen, the estimated c_i's decline from row 1 (where c_1 is large and positive) to row 7 (where c_7 is large and negative), and c_4 is always relatively small (in fact, it not significantly different from 0 at the 5% level when estimated with the data from Part A or Part B only). These results indicate that participants tend to switch from choosing the alternative to choosing the fixed prospect toward the middle row. Furthermore, the estimates of the π_1 and π_2 parameters reported in Table 1 are highly jointly significant: the p-value of the Wald test is less than 1×10^{-10}. These results strongly support Hypothesis 1 and are robust to restricting to the data to the incentivized Part A only.

[25] Wakker (2010, section 9.6) highlights two serious concerns with assuming different parameters in the gain and loss domains if $u^+(x) = \frac{x^{1-\gamma^+}}{1-\gamma^+}$ and $u^-(x) = \frac{x^{1-\gamma^-}}{1-\gamma^-}$ and $\gamma^+ \neq \gamma^-$. First, the ratio of disutility from a sure loss of x to utility from a sure gain of x, $\frac{-\lambda u^-(-x)}{u^+(x)}$, is *not* uniformly equal to λ but instead depends on the value of x. Second, for any λ, there exists a range of x values for which this ratio is actually *smaller* than 1, which is the opposite of loss aversion. These problems can make estimates of λ especially sensitive to exactly which prospects are used in the experiment.

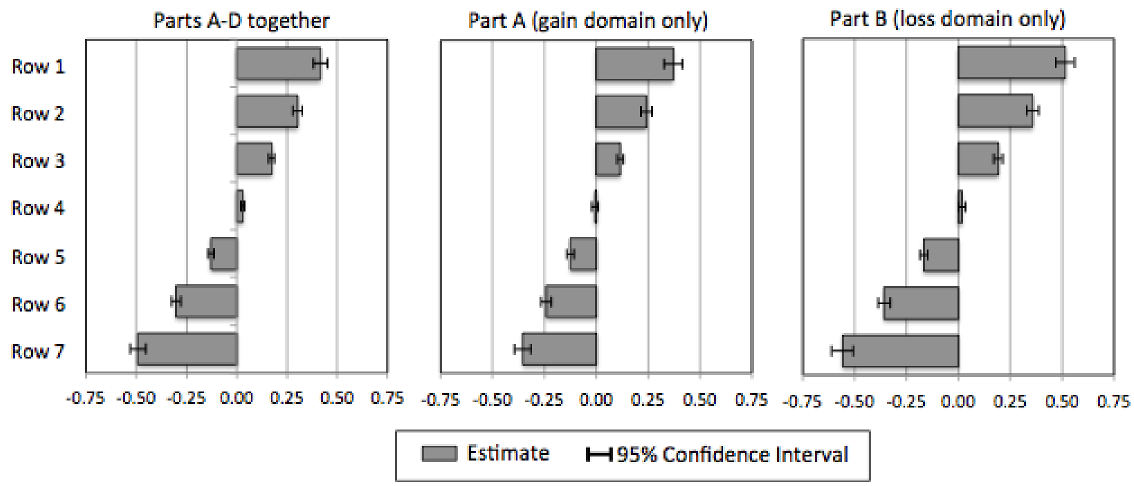


FIGURE 2. Implied estimates of the compromise effects c_i as a function of the row i in which a choice appears. The standard errors and confidence intervals are obtained with the delta method. In the estimation, we parameterize the compromise effects with the quadratic functional form $c_i = \pi_0 + \pi_1 \cdot i + \pi_2 \cdot i^2$, $\Sigma_{i=1}^{7} c_i = 0$, which is equivalent to $c_i = \pi_1 \cdot (i-4) + \pi_2 \cdot (i^2 - 20)$. Note that the confidence intervals are smaller around the middle rows because, by the delta method (assuming $cov(\hat{\pi}_1, \hat{\pi}_2) \approx 0$), $var(\hat{c}_i) \approx (i-4)^2 var(\hat{\pi}_1) + (i^2-20)^2 var(\hat{\pi}_2)$.

5.2 Robustness of the Preference-Parameter Estimates from Joint Estimation

To test Hypotheses 2a and 2b, we begin by estimating the model with compromise effects separately in the subsamples corresponding to each of the five Pull treatments.[26] Figure 3 shows estimates of γ, γ^+ and γ^-, with 95% confidence intervals, for each subsample. Figure 4 shows estimates of λ.

[26] Though this may not at first be obvious, compromise effects are identified in these subsamples because the tendency to switch toward the middle row of the MPL is a feature of the data that is distinguishable from CPT preferences, even within each subsample.

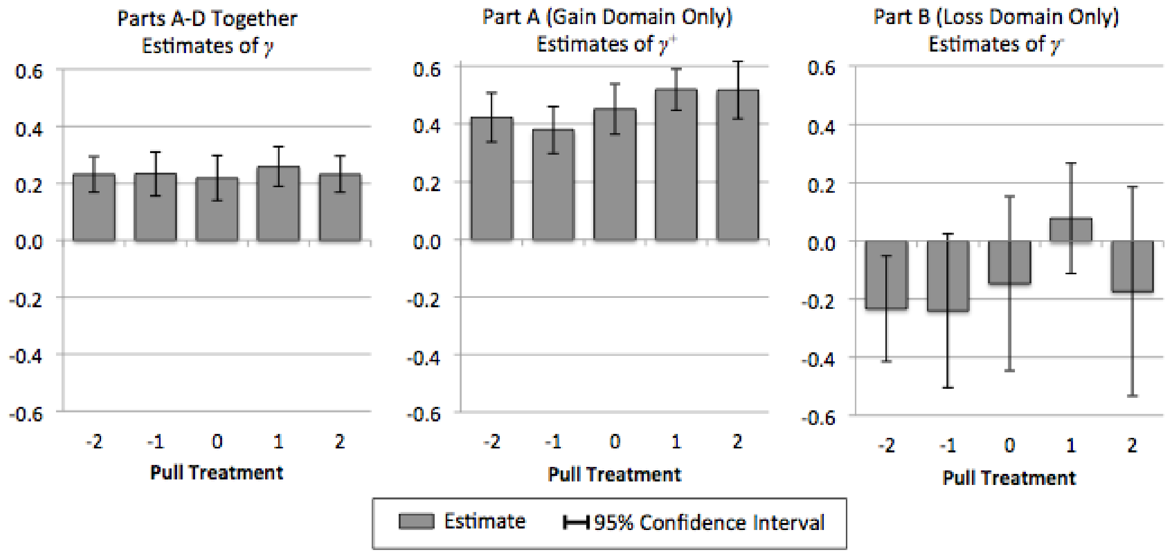


FIGURE 3. Estimates of γ, γ^+, and γ^- by Pull treatment, from the CPT model with compromise effects. The negative estimates of γ^- for Part B reflect risk aversion in the loss domain, unlike what CPT predicts. (γ is not estimated for Parts C and D only because these parts have few questions.)

As can be seen, the estimates of γ, γ^+, γ^-, and λ do not differ substantially across Pull treatments, consistent with Hypotheses 2a and 2b. To formally test for equality across treatments, we estimate the model with all parameters specified as linear functions of the Pull variable and of a dummy that indicates if the participant was in the EV treatment. In other words, we substitute γ in the utility function in (1) by $\gamma = \gamma_0 + \phi_1^\gamma \cdot Pull + \phi_2^\gamma \cdot EV$, λ by $\lambda = \lambda_0 + \phi_1^\lambda \cdot Pull + \phi_2^\lambda \cdot EV$, and likewise for α, β, and all the σ_q parameters, and we test whether the ϕ parameters are equal to zero.[27]

[27] As can be seen from Figures 5 and 6 below, which present estimates of γ, γ^+, γ^-, and λ by Pull treatment from the CPT model without compromise effects, a linear specification for the Pull variable is a reasonable approximation.

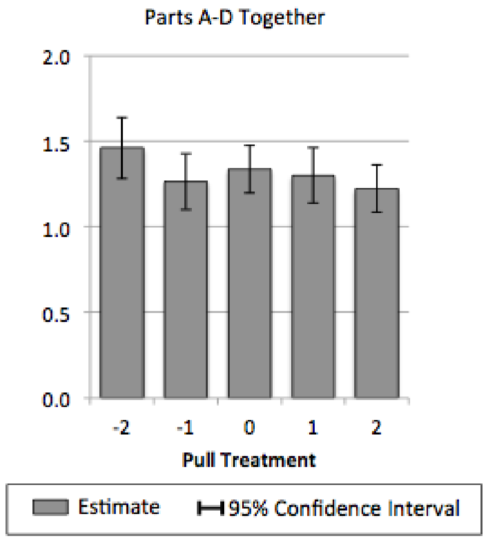


FIGURE 4. Estimates of λ by Pull treatment from the CPT model with compromise effects, for Parts A-D together. (λ cannot be estimated for Part A only or Part B only because the questions in these parts are all in the gain or loss domains. We do not estimate λ for Parts C and D only because these parts have few questions.)

Table 2 shows the results. The three estimates of ϕ_1^γ are all close to zero, and none is statistically distinguishable from zero (including the estimate from the incentivized Part A). We interpret these estimates as providing more formal support for Hypothesis 2a. In contrast, the estimate of ϕ_1^λ is significantly different from zero at the 10% level, and its sign is consistent with what one would expect from the Pull manipulation, which suggests that our model with compromise effects does not perfectly control for these effects. As we will see below, however, this estimate of ϕ_1^λ is much smaller than the one obtained from the CPT model when we do not control for compromise effects, indicating that our model with compromise effects substantially reduces the bias due to these effects.

<INSERT TABLE 2 ABOUT HERE>

Taken together, we interpret the evidence as strongly supportive of Hypothesis 2a and also broadly supportive of Hypothesis 2b. In other words, our model (2) yields robust estimates of the CPT parameters γ and λ, both when estimated in the sample of all participants and within the subsamples corresponding to each of the five Pull treatments.

6 Biases in Estimated Risk Preferences when Compromise Effects Are Omitted from the Model

We now proceed to estimate the CPT model without compromise effects, the version of the model usually estimated by economists. As above, we focus our attention on γ and λ; results for all parameters are presented in the Online Appendix.

Table 3 shows the estimates for selected parameters. The estimates of γ, γ^+ and γ^- are all smaller in magnitude than those from the model with compromise effects (2), indicating less curvature in the utility function. The estimate of γ^- is not significantly different from 0 anymore, consistent with a linear utility function in the loss domain. The estimate of λ is not significantly different from its value when estimated in the model that includes compromise effects.[28]

<INSERT TABLE 3 ABOUT HERE>

The parameter estimates all fall within the range of existing estimates in the literature (except for $\hat{\beta}^+$, which falls slightly below the range).

To test Hypotheses 3a and 3b, we proceed analogously as above and estimate the model without compromise effects separately in the subsamples corresponding to each of the five Pull treatments. As can be seen from Figures 5 and 6, the estimates differ substantially across Pull treatments. As predicted by Hypotheses 3a and 3b, $\hat{\gamma}$, $\hat{\gamma}^+$ and $\hat{\gamma}^-$ are increasing in Pull and $\hat{\lambda}$ is decreasing in Pull. Comparing Figures 5 and 6 to Figures 3 and 4, it is clear that failing to control for compromise effects when estimating the model separately for each treatment introduces a sizeable bias in the estimates of γ and λ.

As can be seen in the right panel of Figure 5, the Pull treatment manipulation of compromise effects is strong enough to generate estimates of γ^- that are either significantly smaller than 0 (Pull -2) or significantly larger than 0 (Pull 2). Furthermore, as can be seen from Figure 6, the

[28] We note that the parameter estimates in Tables 1 and 3 are in fact not too dissimilar, but we believe this similarity is coincidental and this would not be the case had we used different Pull treatments in the experiment.

Pull treatment manipulation of compromise effects causes estimates of λ to vary from 1.059 (Pull 2) to 1.746 (Pull -2). The former estimate is not significantly different from 1 at the 10% level, suggesting that compromise effects can create the appearance of no loss aversion.

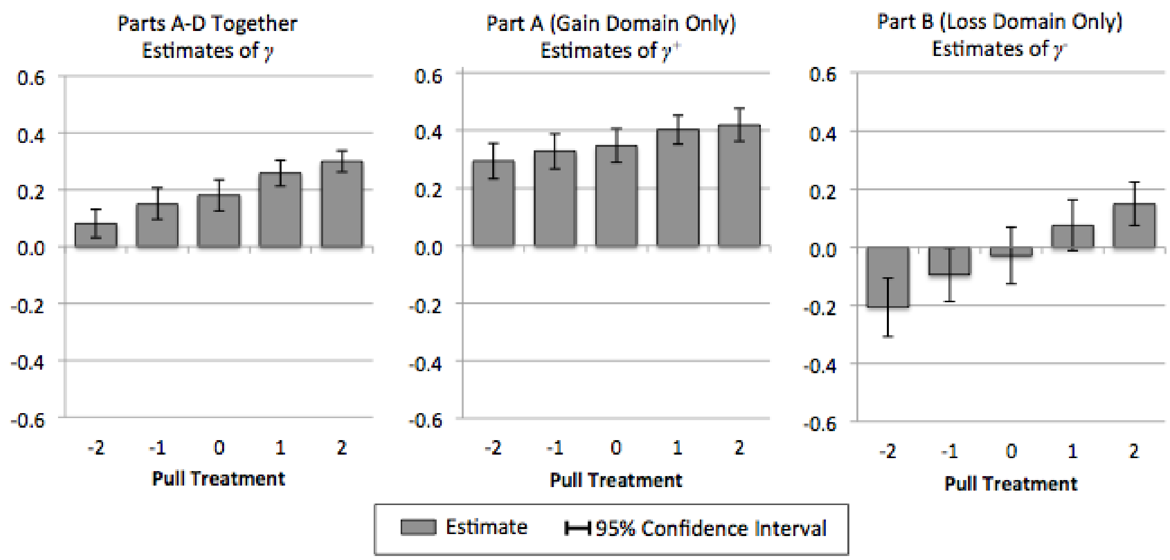


FIGURE 5. Estimates of γ, γ^+, and γ^- by Pull treatment, from the CPT model without compromise effects. This figure is analogous to Figure 3, except that the estimated model does not control for compromise effects.

As above, we formally test the impact of compromise effects by specifying all parameters as linear functions of the Pull variable and of a dummy that indicates if the participant was in the EV treatment. The results are presented in Table 4. $\hat{\phi}_1^{\gamma}$ is significant at the 1% level and positive in all three columns (including in the column corresponding to the incentivized Part A), providing formal support for Hypothesis 3a. The implied differences between the estimates in the Pull -2 and the Pull 2 treatments are sizeable: for γ, the implied difference is 0.168 (4×0.042), and for $\hat{\gamma}^-$, the corresponding figure is 0.252 (4×0.063). $\hat{\phi}_1^{\lambda}$ is highly statistically significant and negative, thus supporting Hypothesis 3b. The implied difference between $\hat{\lambda}$ in the Pull -2 and the Pull 2 treatments is 0.588 (4×0.147).

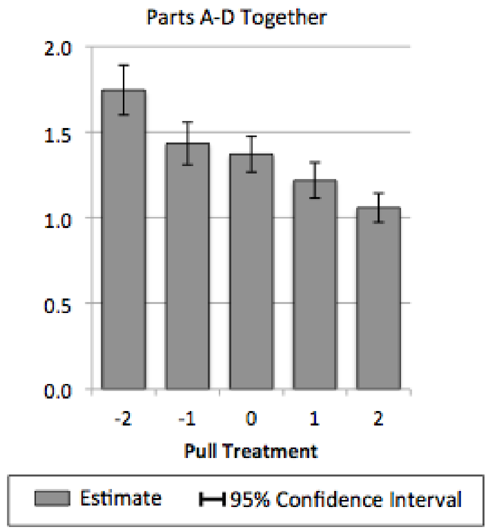


FIGURE 6. Estimates of λ by Pull treatment from the CPT model without compromise effects, for Parts A-D together. This figure is analogous to Figure 4, except that the estimated model does not control for compromise effects.

<INSERT TABLE 4 ABOUT HERE>

The evidence thus strongly supports Hypotheses 3a and 3b and suggests that many existing results based on experiments using the MPL elicitation method may be severely biased due to compromise effects.

7 How Large are Compromise Effects?

Having demonstrated that compromise effects can have a significant impact on choice in a MPL setting, we now obtain a rough estimate of their importance relative to the prospects' monetary outcomes.

To do so, we make an assumption that we show in the next paragraph is justified empirically: the magnitude of the compromise effects and of the preference shocks scales linearly with the expected utilities of the prospects on a screen. Formally, we assume that there is a constant $\Delta > 0$ such that for all screens q,

$$\sigma_q = \Delta \cdot |U(P_{qf})|, \tag{3}$$

where (as defined in Section 3.2) the parameter σ_q regulates the relative importance of utility vs. the other arguments (compromise effects and shocks), and $U(P_{qf})$ is the expected utility of the fixed prospect on screen q. Thus, for the prospects from Part A (which are all in the gain domain, allowing us to ignore the absolute value sign), we can substitute $\Delta \cdot U(P_{qf})$ for σ_q in Equation (2) of our model. It follows that a participant will prefer the alternative P_{qi} over the fixed prospect P_{qf} in row i of screen q if and only if

$$\begin{aligned} & U(P_{qi}) - U\left(P_{qf}\right) + \Delta \cdot c_i \cdot U(P_{qf}) > \sigma_q \varepsilon_q \\ \iff \quad & U(P_{qi}) - U\left(\theta_i \cdot P_{qf}\right) > \sigma_q \varepsilon_q, \end{aligned}$$

where $(1 + \theta_i) = (1 - \Delta c_i)^{\frac{1}{1-\gamma}}$. For the prospects from Part B, a similar equivalence holds, but with $(1 + \theta_i) = (1 + \Delta c_i)^{\frac{1}{1-\gamma}}$. Therefore, our assumption enables us to quantify the influence of a compromise effect c_i as the factor $(1 + \theta_i)$ by which the screen's fixed prospect would have to be multiplied to have the same effect on choice. Equivalently, θ_i is the magnitude of the compromise effect measured in terms of a fraction of monetary value of the screen's fixed prospect (with a negative value meaning that the compromise effect makes the fixed prospect less likely to be chosen).

We now assess our assumption in equation (3) empirically. Recall from Section 3.3 that, to estimate our models, we group screens together that have similar expected values of their fixed prospects and estimate a common $\hat{\sigma}_q$ for each group. Defining (and slightly abusing) some notation, let $\hat{U}(P_{\tilde{q}f})$ denote the expected utility of the fixed prospect on screen $\tilde{q}$ calculated using the model parameters estimated from the specification that includes compromise effects; and let $\hat{E}_{\tilde{q}\in q}[|\hat{U}(P_{\tilde{q}f})|]$ denote the mean of the absolute values of these $\hat{U}(P_{\tilde{q}f})$'s across all the screens $\tilde{q}$ in group q. (Because the screens in a group have similar $\hat{U}(P_{\tilde{q}f})$'s, each $\hat{U}(P_{\tilde{q}f})$ has roughly the same magnitude as the group mean.) The empirical counterpart to equation (3) would be a multiplicative relationship between $\hat{\sigma}_q$ and $\hat{E}_{\tilde{q}\in q}[|\hat{U}(P_{\tilde{q}f})|]$ that is the same across different groups q. Figure 7 illustrates this relationship in our data. As can be seen, for the three sets of estimation results (Parts A-D together, Part A, and Part B), $\hat{\sigma}_q$ indeed appears to be reasonably well approximated as a multiplicative constant times $\hat{E}_{\tilde{q}\in q}[|\hat{U}(P_{\tilde{q}f})|]$. Moreover, the multiplicative constant $\hat{\Delta}$ is nearly the same across the three sets of results, ranging from 0.32 to 0.36.[29]

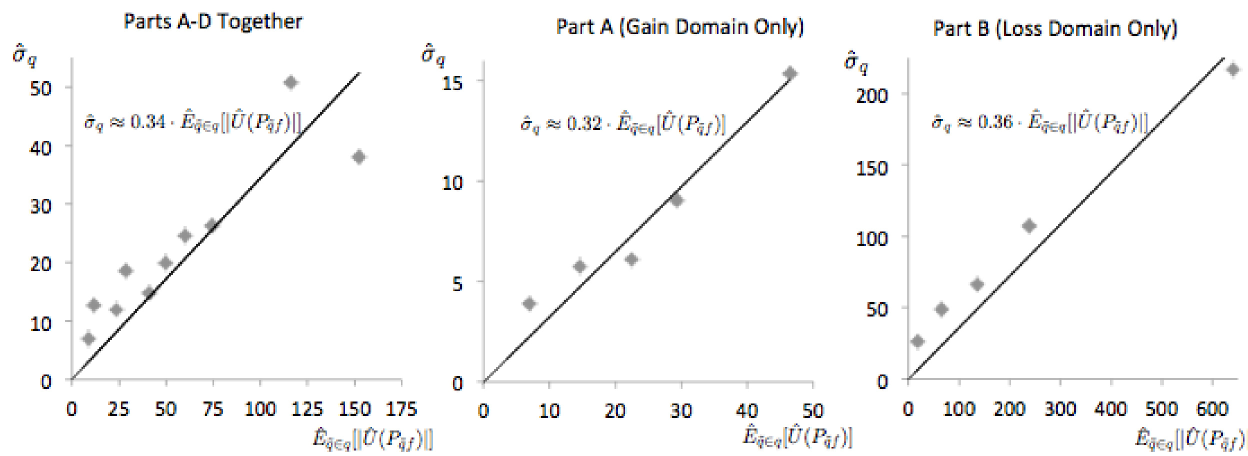


FIGURE 7. Relationship between $\hat{\sigma}_q$ and the expected utility of a screen's fixed prospect. See text for details.

Using the estimated $\hat{\Delta}$ for each of the three sets of results, Table 5 presents estimates of the strength of the compromise effect, $\hat{\theta}_i$, for each row i on a screen (because this is meant to be an approximation, we omit standard errors).

<INSERT TABLE 5 ABOUT HERE>

Our estimates of the strength of the compromise effects in a screen's first and last rows (where their impact is largest) range in magnitude from ~17% to ~23% of the monetary value of the screen's fixed prospect. We interpret such magnitudes as non-trivial.

[29] In OLS regressions of $\hat{\sigma}_q$ on a constant and $\hat{E}_{\tilde{q}\in q}[|\hat{U}(P_{\tilde{q}f})|]$, the intercept is significantly larger than 0 but relatively small for all three sets of estimation results. For the estimates of $\hat{\Delta}$ that we report here, we set the intercept equal to 0.

8 Effect of Displaying the Gambles' Expected Values on Estimated Risk Preferences

Displaying the expected value may anchor the participants on the expected value (Tversky and Kahneman, 1974) and make their preferences more risk neutral.[30] We therefore hypothesize that (1) $\hat{\gamma}^+$ and $\hat{\gamma}^-$ will shift toward 0 in the EV treatment, and (2) $\hat{\lambda}$ will shift toward 1 in the EV treatment.

Online Appendix Figures 1-4 show estimates of $\hat{\gamma}$ and $\hat{\lambda}$ for the subsamples corresponding to two EV treatments, with 95% confidence intervals. Displaying the expected value does not appear to affect estimated risk or loss aversion. In addition, none of the estimates of $\hat{\phi}_2^{\gamma}$ and of $\hat{\phi}_2^{\lambda}$ in Table 4 are statistically distinguishable from zero. Thus, like Lichtenstein, Slovic, and Zink (1969) and Montgomery and Adelbratt (1982) but unlike Harrison and Rutström (2008), we do not find support for the hypothesis that the EV treatment shifts $\hat{\gamma}^+$ and $\hat{\gamma}^-$ toward 0 and $\hat{\lambda}$ toward 1. As discussed in the introduction, a key difference between our experiement and Harrison and Rutström's (2008) is that the prospects in the latter are more complex, involving four possible outcomes. It is possible that participants intuitvely estimate the prospects' expected values in our experiment but are not able to accurately do so in Harrison and Rutström's experiment, and that providing expected value information is therefore redundant in our experiment but not in theirs.

9 Conclusion

In this paper, we estimate an econometric model that explicitly takes into account the compromise effect. We are thus able to disentangle the compromise effect from risk preference parameters. When we estimate this model, we find that our risk-preference estimates are robust: the inferred risk parameters essentially do not change with exogenous manipulations of the choice set. *Without* controls for the compromise effect, however, we replicate the finding from prior work (e.g., Harrison et al., 2005) that risk-preference estimates are sensitive to exogeneous manipulations of the choice set.

[30] Furthermore, to the extent that risk aversion over small-stakes prospects and loss aversion are due to cognitive errors in comprehending the value of a prospect, displaying information that is useful for assessing a prospect's value—such as its expected value—may decrease small-stakes risk aversion and loss aversion. Motivated by that hypothesis, Benjamin, Brown, and Shapiro (2013) examined a similar manipulation.

As in T&K, our estimation of the prospect-theory parameters has assumed that the reference point is the participant's status-quo wealth. Köszegi and Rabin (2006, 2007) have argued that the assumption that the reference point is the participant's (possibly stochastic) expectation of wealth provides a better explanation of risk-taking behavior in a variety of contexts. Could a version of prospect theory in which the reference point reflects a participant's expectations explain why the manipulations of the choice set influence the estimated preference parameters (when we do not include controls for the compromise effect)? This question poses a challenging research program. Modeling the reference point as an expectation would not merely make the reference point depend on the alternative options in the *current* choice problem but also on the sequence of choice problems that have been faced already, as well as the experimental instructions. Existing work provides little guidance on modeling these complex relationships, and many ad hoc assumptions would be needed.[31]

Our analysis also raises the question of why loss averson estimates vary across published studies. Wakker (2010, p.265) concludes that "loss aversion is volatile and depends much on framing, and [Tversky and Kahneman's (1992) loss aversion estimate] $\lambda = 2.25$ cannot have the status of a universal constant." Estimates of loss aversion range from 0.74 to 8.27 in one pair of reviews: Booij, van Praag, and Kullenís (2010, Table 1) and Abdellaoui, Bleichrodt, and Paraschiv (2007, Tables 1 and 5). Identifying the aspects of this variation that can be explained by choice-set effects constitutes a potentially fruitful future research program.

A limitation of our paper is that the compromise-effect parameter values we estimate are specific to our experimental setting, and thus cannot be extrapolated to other settings. On the other hand, the methodology we demonstrate—jointly estimating the compromise effect and preference parameters—can be generalized in at least two useful directions.

First, the compromise-effect controls that we propose here can be used not only to improve the robustness of estimates of risk preference parameters, but also of parameter estimates for any

[31] One simple but extreme possibility that has been explored (Sprenger, forthcoming) is that the fixed prospect in the current choice problem's price list pins down a participant's expectation. We note that since the fixed prospect was held constant across our scale manipulations, the scale effects we find could not be explained by a version of prospect theory with this model of reference point formation.

Song (2015) examines a natural implementation of the reference-point-as-expectations model in an experiment in which participants experience a binary gamble before facing a single Multiple Price List. He finds evidence that the reference point is determined partially by expectations set by the earlier gamble. This evidence, however, does not bear on whether the reference-point-as-expectations theory could also explain the compromise effects that we explore.

other preferences elicited using MPLs, including time and other-regarding preferences. Second, our estimation method can be generalized beyond compromise effects to other types of context effects (for instance, a weak preference for items that happen to come early in a list of alternatives or at the end of the list, an effect that is observed on ballots; see, e.g., Koppell and Steen 2004).

Our paper also raises several other questions for future research. Most importantly, are there alternative elicitation methods, such as binary choice experiments (without MPL), that are more robust? Or are these alternative methods biased by other types of context effects?

10 REFERENCES

Abdellaoui, Mohammed, Han Bleichrodt, and Corina Paraschiv. 2007. "Loss Aversion Under Prospect Theory: A Parameter-Free Measurement." *Management Science*, 53(10): 1659–1674.

Andersen, Steffen, Glenn W. Harrison, Morten I. Lau, and Elisabet E. Rutström. 2006. "Elicitation Using Multiple Price List Formats." *Experimental Economics*, 9(4): 383–405.

Andersen, Steffen, Glenn W. Harrison, Morten I. Lau, and Elisabet E. Rutström. 2008. "Eliciting Risk and Time Preferences." *Econometrica*, 76(3): 583–618.

Benjamin, Daniel J., Sebastian A. Brown, and Jesse M. Shaprio. 2013. "Who is 'Behavioral'? Cognitive Ability and Anomalous Preferences." *Journal of the European Economic Association*, 11(6): 1231-1255.

Birnbaum, Michael H. 1992. "Violations of Monotonicity and Contextual Effects in Choice-Based Certainty Equivalents." *Psychological Science*, 3(5): 310–314.

Booij, Adam S., Bernard M.S. van Praag, and Gijs van den Kullen. 2010. "A parametric analysis of prospect theory's functionals for the general population." *Theory and Decision*, 68: 115–148.

Bruhin, Adrian, Helga Fehr-Duda, and Thomas Epper. 2010. "Risk and Rationality: Uncovering Heterogeneity in Probability Distortion." *Econometrica*, 78(4): 1375–1412.

Fehr-Duda, Helga and Thomas Epper. 2012. "Probability and Risk: Foundations and Economic Implications of Probability-Dependent Risk Preferences." *Annual Review of Economics*, 4:567-593.

Harrison, Glenn W., Morten I. Lau, Elisabet E. Rutström, and Melonie B. Sullivan. 2005. "Eliciting Risk and Time Preferences Using Field Experiments: Some Methodological Issues." In *Field experiments in economics (Research in Experimental Economics, Volume 10)*, ed. J. Carpenter, G. Harrison, and J. List, 125–218. Emerald Group Publishing Limited.

Harrison, Glenn W., Morten I. Lau, and Elisabet E. Rutström. 2007. "Estimating Risk Attitudes in Denmark: a Field Experiment." *Scandinavian Journal of Economics*, 109(2): 341–368.

Harrison, Glenn W., John A. List, and Charles Towe. 2007. "Naturally Occurring Preferences and Exogenous Laboratory Experiments: A Case Study of Risk Aversion." *Econometrica*, 75(2): 433–458.

Harrison, Glenn W. and Elisabet E. Rutström. 2008. "Risk Aversion in the Laboratory." In *Risk Aversion in Experiments (Research in Experimental Economics, Volume 12)*, ed. J. Cox, G. Harrison, 41–196. Emerald Group Publishing Limited.

Hey, John D., and Chris Orme. 1994. "Investigating Generalizations of Expected Utility Theory Using Experimental Data." *Econometrica*, 62(6): 1291-1326.

Holt, Charles A., and Susan K. Laury. 2002. "Risk Aversion and Incentive Effects." *American Economic Review*, 92(5); 1644–1655.

Kahneman, Daniel, and Amos Tversky. 1979. "Prospect Theory: An Analysis of Decision Under Risk." *Econometrica*, 47(2): 263–291.

Kamenica, Emir. 2008. "Contextual Inference in Markets: On the Informational Content of Product Lines." *American Economic Review*, 98(5): 2127–2149.

Koppell, Jonathan G. S., and Jennifer A. Steen. 2004. "The Effects of Ballot Position on Election Outcomes." *Journal of Politics*, 66(1): 267-281.

Köszegi, Botond, and Matthew Rabin. 2006. "A Model of Reference-Dependent Preferences." *Quarterly Journal of Economics*, 121(4): 1133-1165.

Köszegi, Botond, and Matthew Rabin. 2007. "Reference-Dependent Risk Attitudes." *American Economic Review*, 97(4): 1047-1073.

Lichtenstein, Sara, Paul Slovic, and Donald Zink. 1969. "The Effect of Instruction in Expected Value Optimality of Gambling Decisions." *Journal of Experimental Psychology*, 79(2): 236–240.

Montgomery, Henry, and Thomas Adelbratt. 1982. "Gambling Decisions and Information About Expected Value." *Organizational Behavior and Human Performance*, 29: 39–57.

Prelec, Drazen. 1998. "The Probability Weighting Function." *Econometrica*, 66(3): 497–527.

Simonson, Itamar. 1989. "Choice Based on Reasons: The Case of Attraction and Compromise Effects." *Journal of Consumer Research*, 16(2): 158-174.

Slovic, Paul. 1995. "The Construction of Preference." *American Psychologist*, 50(5): 364-371.

Song, Changcheng. 2015. "An Experiment on Reference Points and Expectations." Working Paper.

Sprenger, Charles. Forthcoming. "An Endowment Effect for Risk: Experimental Tests of Stochastic Reference Points." *Journal of Political Economy.*

Stewert, Neil, Nick Chater, Henry P. Stott, and Stian Reimers. 2003. "Prospect Relativity: How Choice Options Influence Decision Under Risk." *Journal of Experimental Psychology: General*, 132(1): 23–46.

Trepel, Christopher, Craig R. Fox, and Russel A. Poldrack. 2005. "Prospect Theory on the Brain? Toward a Cognitive Neuroscience of Decision Under Risk." *Cognitive Brain Research*, 23(1): 34–50.

Tversky, Amos, and Daniel Kahneman. 1974. "Judgment under Uncertainty: Heuristics and Biases." *Science*, 185(4157): 1124–1131.

Tversky, Amos, and Daniel Kahneman. 1992. "Advances in Prospect Theory: Cumulative Representation of Uncertainty." *Journal of Risk and uncertainty*, 5(4): 297–323.

Wakker, Peter P. 2010. *Prospect Theory: For Risk and Ambiguity.* Cambridge University Press, Cambridge, UK.

Table 1. ML Estimates of Selected Parameters in the Model with Compromise Effects

	Parts A-D Together	Part A (Gain Domain Only)	Part B (Loss Domain Only)
γ, γ^+, γ^-	0.242***	0.448***	-0.106**
	(0.016)	(0.020)	(0.043)
λ	1.311***		
	(0.034)		
α, α^+, α^-	0.619***	0.564***	0.690***
	(0.015)	(0.015)	(0.022)
β, β^+, β^-	1.119***	0.858***	1.471***
	(0.025)	(0.033)	(0.061)
π_1	-0.091***	-0.134***	-0.144***
	(0.012)	(0.018)	(0.018)
π_2	-0.008***	0.002	-0.004*
	(0.001)	(0.002)	(0.002)
Log-likelihood	-55,379	-23,915	-25,400
Wald test for π_1, π_2	$p < 1 \times 10^{-10}$	$p < 1 \times 10^{-10}$	$p < 1 \times 10^{-10}$
Parameters	19	10	10
Individuals	493	493	493
Observations	30,566	13,804	13,804

NOTE: Standard errors are clustered by participant. The log-likelihood statistic is for the model without clustering. The Wald test is for the joint significance of π_1 and π_2.
* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 2. ML Estimates of Selected Parameters in the Parameterized Model with Compromise Effects

		Parts A-D Together	Part A (Gain Domain Only)	Part B (Loss Domain Only)
γ, γ^+, γ^-	γ_0	0.206***	0.423***	-0.118**
		(0.026)	(0.028)	(0.052)
	ϕ_1^γ	0.008	0.011	-0.032
		(0.017)	(0.018)	(0.026)
	ϕ_2^γ	0.058*	0.033	0.002
		(0.035)	(0.039)	(0.067)
λ	λ_0	1.271***		
		(0.053)		
	ϕ_1^λ	-0.053*		
		(0.029)		
	ϕ_2^λ	0.075		
		(0.074)		
α, α^+, α^-		0.556***	0.505***	0.617***
		(0.019)	(0.018)	(0.027)
β, β^+, β^-		1.190***	0.911***	1.524***
		(0.037)	(0.048)	(0.086)
π_1		-0.090***	-0.139***	-0.142***
		(0.012)	(0.018)	(0.018)
π_2		-0.008***	0.002	-0.005**
		(0.001)	(0.002)	(0.002)
Log-likelihood		-55,225	-23,839	-25,343
Wald test for π_1, π_2		$p < 1 \times 10^{-10}$	$p < 1 \times 10^{-10}$	$p < 1 \times 10^{-10}$
Parameters		53	26	26
Individuals		493	493	493
Observations		30,566	13,804	13,804

NOTE: Standard errors are clustered by participant. The log-likelihood statistic is for the model without clustering. The Wald test is for the joint significance of π_1 and π_2.
* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 3. ML Estimates of Selected Parameters in Model Without Compromise Effects

	Parts A-D Together	Part A (Gain Domain Only)	Part B (Loss Domain Only)
γ, γ^{+}, γ^{-}	0.203***	0.363***	-0.010
	(0.012)	(0.014)	(0.022)
λ	1.337***		
	(0.027)		
α, α^{+}, α^{-}	0.574***	0.538***	0.615***
	(0.010)	(0.011)	(0.013)
β, β^{+}, β^{-}	1.123***	0.958***	1.296***
	(0.016)	(0.020)	(0.030)
Log-likelihood	-59,957	-25,604	-28,141
Parameters	17	8	8
Individuals	493	493	493
Observations	30,566	13,804	13,804

NOTE: Standard errors are clustered by participant. The log-likelihood statistic is for the model without clustering. The Wald test is for the joint significance of π_0, π_1, and π_2.
* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 4. ML Estimates of Selected Parameters in the Parameterized Model Without Compromise Effects

		Parts A-D Together	Part A (Gain Domain Only)	Part B (Loss Domain Only)
γ, γ^+, γ^-	γ_0	0.196***	0.353***	-0.003
		(0.016)	(0.018)	(0.026)
	ϕ_1^γ	0.042***	0.041***	0.063***
		(0.009)	(0.012)	(0.012)
	ϕ_2^γ	0.001	0.003	-0.022
		(0.023)	(0.029)	(0.030)
λ	λ_0	1.318***		
		(0.040)		
	ϕ_1^λ	-0.147***		
		(0.022)		
	ϕ_2^λ	0.086		
		(0.059)		
α, α^+, α^-		0.535***	0.497***	0.577***
		(0.012)	(0.014)	(0.016)
β, β^+, β^-		1.143***	0.980***	1.305***
		(0.022)	(0.028)	(0.037)
Log-likelihood		-59,427	-25,406	-27,852
Parameters		51	24	24
Individuals		493	493	493
Observations		30,566	13,804	13,804

NOTE: Standard errors are clustered by participant. The log-likelihood statistic is for the model without clustering.
* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 5. Implied Impact of Compromise Effects Expressed as a Percentage of the Monetary Value of a Screen's Fixed Prospect ($\hat{\theta}_i$)

	Parts A-D Together		Part A (Gain	Part B (Loss
	Prospects from Part A	Prospects from Part B	Domain Only)	Domain only)
Row 1	-0.18	0.19	-0.20	0.17
Row 2	-0.13	0.14	-0.14	0.12
Row 3	-0.08	0.08	-0.07	0.06
Row 4	-0.01	0.01	0.00	0.01
Row 5	0.06	-0.06	0.07	-0.05
Row 6	0.14	-0.13	0.14	-0.12
Row 7	0.23	-0.21	0.22	-0.18

NOTE: As explained in the text, these figures are approximate.

11 APPENDIX: Screenshots of the Experiment

Screenshots of a randomly selected screen from each part of the experiment are shown below for a participant in the Pull -1 and EV treatments. Each scenario appears on a separate screen in the experiment.

This study consists of a total of 64 scenarios, divided into four parts. You have completed 7 of the 64 scenarios.

Part A: Scenario 8 of 28

For each of questions (a) to (g), please mark your preferred option.

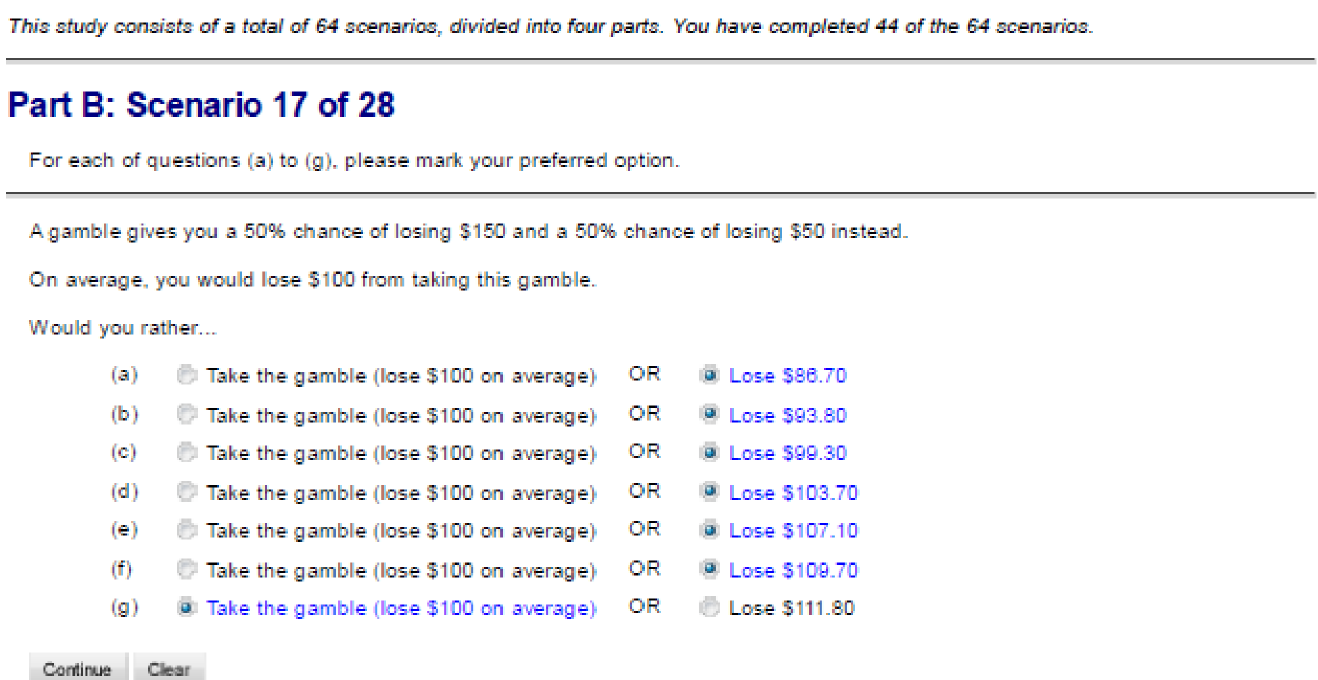
A gamble gives you a 75% chance of gaining $200 and a 25% chance of gaining $100 instead.

On average, you would gain $175 from taking this gamble.

Would you rather...

(a)	Take the gamble (gain $175 on average)	OR	Gain $180.30
(b)	Take the gamble (gain $175 on average)	OR	Gain $179.30
(c)	Take the gamble (gain $175 on average)	OR	Gain $178.00
(d)	Take the gamble (gain $175 on average)	OR	Gain $176.40
(e)	Take the gamble (gain $175 on average)	OR	Gain $174.30
(f)	Take the gamble (gain $175 on average)	OR	Gain $171.60
(g)	Take the gamble (gain $175 on average)	OR	Gain $168.30

Continue Clear



This study consists of a total of 64 scenarios, divided into four parts. You have completed 44 of the 64 scenarios.

Part B: Scenario 17 of 28

For each of questions (a) to (g), please mark your preferred option.

A gamble gives you a 50% chance of losing $150 and a 50% chance of losing $50 instead.

On average, you would lose $100 from taking this gamble.

Would you rather...

(a)	Take the gamble (lose $100 on average)	OR	Lose $86.70
(b)	Take the gamble (lose $100 on average)	OR	Lose $93.80
(c)	Take the gamble (lose $100 on average)	OR	Lose $99.30
(d)	Take the gamble (lose $100 on average)	OR	Lose $103.70
(e)	Take the gamble (lose $100 on average)	OR	Lose $107.10
(f)	Take the gamble (lose $100 on average)	OR	Lose $109.70
(g)	Take the gamble (lose $100 on average)	OR	Lose $111.80

Continue Clear

This study consists of a total of 64 scenarios, divided into four parts. You have completed 56 of the 64 scenarios.

Part C: Scenario 1 of 4

For each of questions (a) to (g), please mark your preferred option.

A gamble gives you a 50% chance of losing $50 and ...

(a)	... a 50% chance of gaining $0.00 instead.	Take the gamble (lose $25.00 on average)	OR	Don't take the gamble
(b)	... a 50% chance of gaining $42.30 instead.	Take the gamble (lose $3.85 on average)	OR	Don't take the gamble
(c)	... a 50% chance of gaining $75.40 instead.	Take the gamble (gain $12.70 on average)	OR	Don't take the gamble
(d)	... a 50% chance of gaining $101.30 instead.	Take the gamble (gain $25.65 on average)	OR	Don't take the gamble
(e)	... a 50% chance of gaining $121.60 instead.	Take the gamble (gain $35.80 on average)	OR	Don't take the gamble
(f)	... a 50% chance of gaining $137.50 instead.	Take the gamble (gain $43.75 on average)	OR	Don't take the gamble
(g)	... a 50% chance of gaining $150.00 instead.	Take the gamble (gain $50.00 on average)	OR	Don't take the gamble

Continue Clear

This study consists of a total of 64 scenarios, divided into four parts. You have completed 62 of the 64 scenarios.

Part D: Scenario 3 of 4

For each of questions (a) to (g), please mark your preferred option.

Gamble 1 gives you a 50% chance of losing $50 and a 50% chance of gaining $150.

Gamble 2 gives you a 50% chance of losing $125 and ...

(a)	... a 50% chance of gaining $375.00 instead.	Take gamble 1 (gain $50 on average)	OR	Take gamble 2 (gain $125.00 on average)
(b)	... a 50% chance of gaining $356.30 instead.	Take gamble 1 (gain $50 on average)	OR	Take gamble 2 (gain $115.65 on average)
(c)	... a 50% chance of gaining $332.50 instead.	Take gamble 1 (gain $50 on average)	OR	Take gamble 2 (gain $103.75 on average)
(d)	... a 50% chance of gaining $302.00 instead.	Take gamble 1 (gain $50 on average)	OR	Take gamble 2 (gain $88.50 on average)
(e)	... a 50% chance of gaining $263.10 instead.	Take gamble 1 (gain $50 on average)	OR	Take gamble 2 (gain $69.05 on average)
(f)	... a 50% chance of gaining $213.40 instead.	Take gamble 1 (gain $50 on average)	OR	Take gamble 2 (gain $44.20 on average)
(g)	... a 50% chance of gaining $150.00 instead.	Take gamble 1 (gain $50 on average)	OR	Take gamble 2 (gain $12.50 on average)

Continue Clear