Measuring and Controlling for the Compromise Effect When Estimating Risk Preference Parameters*

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Abstract

The compromise effect arises when being close to the "middle" of a choice set makes an option more appealing. The compromise effect poses conceptual and practical problems for economic research: by influencing choices, it can bias researchers' inferences about preference parameters. To study this bias, we conduct an experiment with 550 participants who made choices over lotteries from multiple price lists (MPLs). Following prior work, we manipulate the compromise effect to influence choices by varying the middle options of each MPL. We then estimate risk preferences using a discrete-choice model without a compromise effect embedded in the model. As anticipated, the resulting risk preference parameter estimates are not robust, changing as the compromise effect is manipulated. To disentangle risk preference parameters from the compromise effect and to measure the strength of the compromise effect, we augment

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our discrete-choice model with additional parameters that represent a rising penalty for expressing an indifference point further from the middle of the ordered MPL. Using this method, we estimate an economically significant magnitude for the compromise effect and generate robust estimates of risk preference parameters that are no longer sensitive to compromise-effect manipulations.

Keywords: compromise effect, cumulative prospect theory, loss aversion, risk preferences

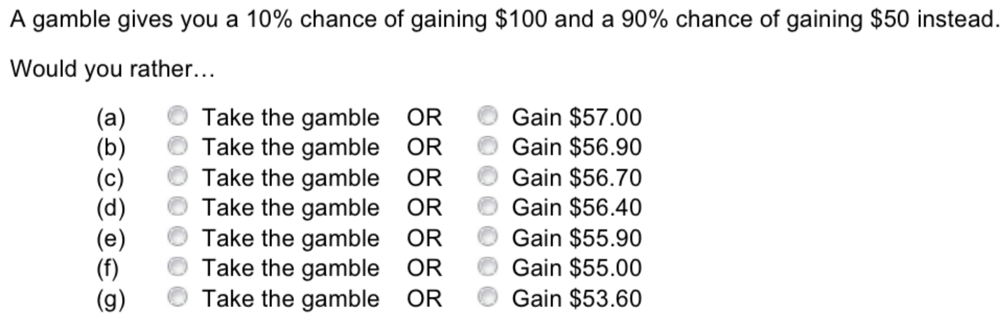
JEL Classification: B49, D03, D14, D83, G11

1 Introduction

The *compromise effect* arises when options in a choice set can be ordered on common dimensions or attributes (such as price, quantity, size, or intensity), and decision makers have a propensity to select the options in the "middle" of the choice set. In short, the compromise effect is a bias toward the middle option. For example, suppose a group of respondents were asked whether they wanted a free nature hike of either 1 mile or 4 miles. Now suppose that a different, otherwise identical group were asked whether they preferred a free nature hike of 1, 4, or 7 miles. A strong compromise effect would lead to a *greater* fraction of respondents choosing 4 miles in the second choice set (see Simonson 1989 for a closely related empirical result and Kamenica 2008 for a discussion of microfoundations).

The compromise effect poses conceptual and practical problems for economic research. By influencing choices, the compromise effect can bias researchers' inferences about other economic parameters. In this paper, we propose and estimate an econometric model that disentangles and separately measures *both* the compromise effect and other parameters of interest. To demonstrate our approach, we conduct a laboratory experiment with 550 participants in which we elicit risk preferences using multiple price lists (MPLs). We study this context because, despite the limitations of the MPL procedure (e.g., Freeman, Halevy and Kneeland 2019), it is among the most commonly used methods to elicit preferences in the economics literature (e.g., Tversky and Kahneman 1992, Holt and Laury 2002, Harrison, List, and Towe 2007, Andersen, Harrison, Lau, and Rutström 2008) and because the compromise effect has been carefully and robustly documented already in the context of inferring risk preferences using an MPL (Birnbaum 1992, Harrison, Lau, Rutström, and Sullivan 2005, Andersen, Harrison, Lau, and Rutström 2006, Harrison, Lau, and Rutström 2007).

The screenshot shown in Figure 1 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 and a sure-thing alternative. The gamble doesn't change across the seven rows, while the sure-thing alternative varies from high to low.


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

Would you rather…

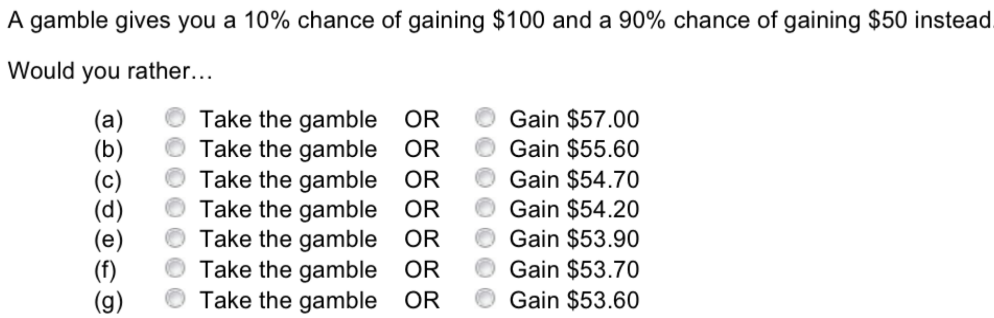
(a)	Take the gamble	OR	Gain $57.00
(b)	Take the gamble	OR	Gain $56.90
(c)	Take the gamble	OR	Gain $56.70
(d)	Take the gamble	OR	Gain $56.40
(e)	Take the gamble	OR	Gain $55.90
(f)	Take the gamble	OR	Gain $55.00
(g)	Take the gamble	OR	Gain $53.60

FIGURE 1. Screenshot from the experiment.

A subject who displayed a very strong compromise effect would act as if she were indifferent between the gamble and the sure-thing in row (d), which is the middle row. Such indifference would imply that she is risk seeking because the gamble has a lower expected value than the sure thing in row (d). In this example, a strong compromise effect would lead a participant who may otherwise be risk averse to make risk-seeking choices.[1]

Following prior work (Birnbaum 1992, Harrison, Lau, Rutström, and Sullivan 2005, Andersen, Harrison, Lau, and Rutström 2006, Harrison, Lau, and Rutström 2007, and Harrison, List, and Towe 2007), we experimentally vary the middle option using scale manipulations. Specifically, we hold the lowest and highest rows of the MPL fixed and manipulate the locations of the five intermediate outcomes within the scale. For example, compare the screenshot in Figure 1 to the one in Figure 2, which has new alternatives in rows (b) through (f), although rows (a) and (g) are the same. With respect to this second MPL, an agent who acts as if the middle option, row (d), is her indifference point would be judged to be risk averse.

[1] We say that a participant is risk averse (risk seeking) when her certainty equivalent for a gamble is less (more) than the gamble's expected value.


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

FIGURE 2. Screenshot from an alternative scale treatment condition of the experiment.

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

To econometrically disentangle other economic parameters from the compromise effect, and to measure the strength of the compromise effect, we augment a discrete-choice model with additional parameters that represent a (rising) penalty for expressing an indifference point further from the middle of the ordered MPL. Our approach of incorporating the compromise effect into the econometric model is different from including treatment-condition indicators as controls. Simply controlling for the treatment condition would not identify risk preferences because the compromise effect influences choices in *every* treatment condition (i.e., there is no compromise-effect-free treatment condition).

The risk preferences we study in the current paper are prospect-theoretic preferences over risky lotteries (e.g., Tversky and Kahneman 1992, Wakker 2010, Bruhin, Fehr-Duda, and Epper 2010). Our *ex ante* hypotheses focus on two parameters: utility curvature γ (which captures concavity in the gain domain and convexity in the loss domain) and loss aversion λ (which captures the degree to which people dislike losses more than they like gains).[2] Our analysis yields three main findings.

First, our estimates of the compromise-effect parameters replicate the findings from earlier work that participants have a bias toward expressing indifference closer to the middle rows of the MPL (e.g., Harrison, Lau, Rutström, and Sullivan 2005; see other references above). Moreover, our quantitative estimates indicate that the bias is economically significant; we estimate that the attractiveness of the middle rows relative to the extreme rows represents 17%-23% of the prospects'

[2] As we discuss below, we also find that the compromise effect influences the probability weighting function.

monetary value.

Second, when we estimate the prospect-theory model without controls for the compromise effect, the scale manipulations have a very powerful effect on the (mis-) estimated preference parameters. In particular, the compromise effect is strong enough to cause us to estimate either loss-domain convexity of the utility function (as predicted by prospect theory) or loss-domain concavity (the opposite of what is predicted by prospect theory), depending on the scale manipulations. The compromise effect is also strong enough that, when manipulated, it can make behavior look as if there is essentially no loss aversion (see the results for the Pull 2 treatment below).

Third, 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. The robustness of these preference-parameter estimates implies that they are not biased by the compromise effect. (When estimating the model pooling all of our experimental data, our estimates are $\hat{\gamma} = 0.24$ and $\hat{\lambda} = 1.31$, which fall within the range of estimates in the existing literature, albeit with $\hat{\lambda}$ toward the lower end of the range in the literature.)

In addition to the scale manipulations described above, we also study the effect of explicitly telling experimental participants the expected value of the risky prospects. We hypothesized that this manipulation would anchor the participants on the expected value, thereby nudging their preferences toward risk neutrality. However, we find that expected value information does not affect measured utility curvature nor measured loss aversion.

This paper contributes to the literature on the compromise effect by estimating a model that explicitly accounts for the compromise effect and enables us to separately estimate it from risk preferences. Our sample is substantially larger than those used in earlier work, which allows us to precisely estimate the effects of the scale manipulations. Moreover, because we pose gambles involving losses as well as gambles involving gains, we can study the effect of scale manipulations not only on utility-function concavity in the gain domain, but also on curvature in the loss domain and on loss aversion. In addition, we provide estimates of the economic magnitude and importance of the compromise effect relative to the prospects' monetary value, and we examine the demographic correlates of the parameters in our econometric model. A limitation of our experiment is that only one out of its four parts (which involves 28 of the 62 sets of choices we analyze) is incentivized. Reassuringly, all of our results still 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 describe our econometric discrete-choice model, which incorporates the compromise effect. 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 with the compromise effect, and we test the robustness of the estimates to the scale manipulations. Section 6 parallels Section 5 but examines the model without controls for the compromise effect. Section 7 estimates the economic magnitude and importance of the compromise effect in our data. Sections 8 briefly analyzes the demographic correlates of the main parameters of our econometric model (including γ, λ, and parameters that capture the compromise effect). Section 9 briefly discusses the results of our expected value manipulation. Section 10 compares our results to other findings in the literature on the estimation of risk preferences. Section 11 concludes by discussing possible directions for future work.

2 Experiment

2.1 Design

Throughout the experiment, we employ the Multiple Price List (MPL) elicitation method (Tversky and Kahneman 1992, Holt and Laury 2002). At the top of each computer screen, a *fixed prospect* is presented (except for the screens in Part C of the experiment – see below). 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 Online Appendix, and the original instructions of the experiment are shown in the Online Appendix. 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.

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.[3]

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. 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. The other circles are auto-filled. This procedure is a version of the "Switching MPL" (or "sMPL", see Andersen, Harrison, Lau, and Rutström 2006). This procedure may reduce participant fatigue and produces clean data, but it might have the unintended effect of biasing participants to select a row near the middle and thus may exaggerate the compromise effect.[4]

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

[3] Our procedure differs from T&K's in three ways. First, T&K do not report the actual values they used. Second, while their gambles were all hypothetical, our "Part A" gambles are incentivized. Third, for each screen, T&K implement a two-step procedure: after finding the point at which participants switch from preferring the alternative outcomes to preferring the fixed prospect, 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 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") to maintain incentive compatibility.

[4] To mitigate this possible unintended effect, our experiment's instructions avoid words like "switch" and "middle". Instead, the instructions stated the following on a practice screen which the participants had to complete at the beginning of the experiment (see Online Appendix Section 12): "the site will automatically fill in the answers to certain questions based on the answers you have already provided. For instance, if you indicate that you would prefer to gain $126 over picking a ball from the bag, the site will assume that you would also prefer to gain $135 over picking a ball from the bag, and it will answer that question for you." (By contrast, Andersen, Harrison, Lau, and Rutström's (2006) Switching MPL asks subjects to choose which row they want to switch at.)

(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 $\gamma = -1$ (i.e., convex utility).[5] Because the range of estimates of γ in the literature falls well within this interval (Booij, van Praag, and van de Kuilen, 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.[6]

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. 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. 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 shown in Figure 3. 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:

[5] We use $\gamma = 0.99$ because $\gamma = 1$ corresponds to log utility and implies a certainty equivalent of \$0 for any prospect with a chance of a \$0 outcome.

[6] As many researchers have shown (e.g., Harrison Johnson McInnes Rutström 2005; Holt and Laury 2002), real stakes sometimes change the results of an experiment, as compared with hypothetical stakes. We use real stakes in the gain domain (Part A). When we ask participants to make decisions that involve both the gain and the loss domain or just the loss domain in Parts B-D, we use hypothetical stakes because of the ethical problems associated with making experimental participants bear real losses (as opposed to the pseudo-loss of losing an experimental endowment). We emphasize that the main results are robust to using only Part A (gains) questions, where we do use real stakes.

$25, $50, $100, and $150.

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

FIGURE 3. Screenshot from Part C of the experiment.

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 Figure 4.

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

FIGURE 4. Screenshot from Part D of the experiment.

On the other two screens, the fixed and the alternative prospects involve only gains. 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 ($\lambda = 0$) 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 the compromise effect impacts measured risk and loss preferences. The five treatments are identical in the set of fixed prospects and in the first and seventh alternatives 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 5 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 intermediate 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 intermediate 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 intermediate alternatives toward zero (for the positive Pulls) or away from zero (for the negative Pulls).

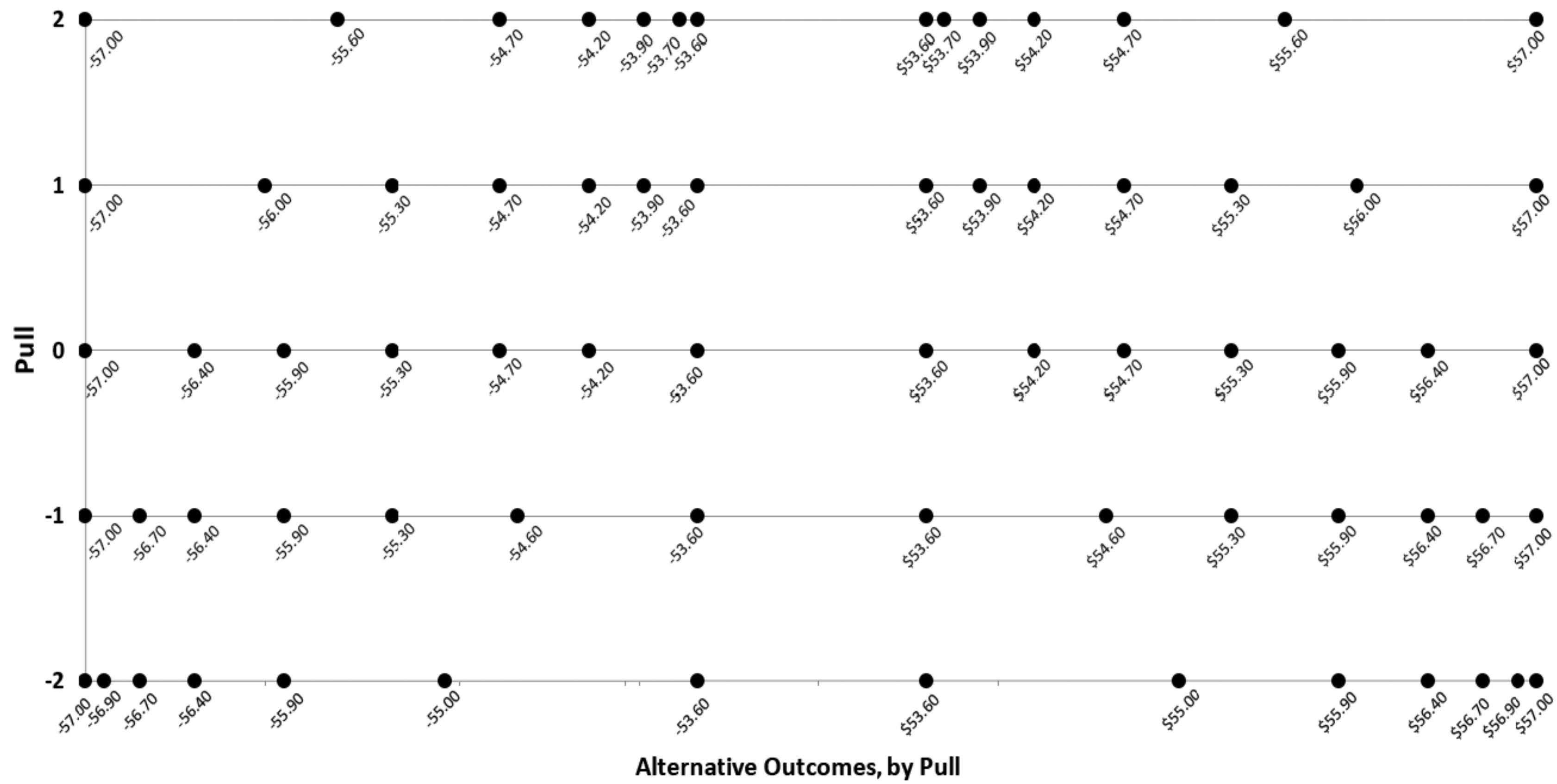


FIGURE 5. Alternative outcomes by Pull treatment for example screens. The right side of the figure shows alternative outcomes by Pull treatment for an example screen from Part A with a fixed prospect offering a 10% chance of gaining $100 and a 90% chance of gaining $50. The left side of the figure shows 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 intermediate 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 to describe it in the EV treatment. 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 not on Harvard University's regular payroll. At the time we ran the experiment, members of the CLER online subject pool database were 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.[7]

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

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

experiment.

2.4 Summary Statistics of the Raw Data from the Experiment

Online Appendix Section 3 includes figures that show the percentage of choices where the safe option was chosen, by Pull and EV treatments, separately for Parts A, B, C, and D of the experiment. These figures give a first impression of the data we collected in our experiment, but caution is warranted in interpreting them because the different Pull treatments involve different sets of choices, and the raw data are thus not directly comparable across treatments.

3 Model and Estimation

3.1 Baseline CPT Model

We assume that participants' risk preferences can be modeled according to CPT. 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:

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

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., Fox and Poldrack 2014; T&K).

We use the Prelec (1998) probability weighting function:

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

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

3.2 Modeling the Compromise Effect

We model the compromise effect by assuming that, in addition to their 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}.[8] 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 only if

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

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_{qAlt}, and ε_q are preference shocks that vary across (but not within) screens. We assume that $\varepsilon_{qf} - \varepsilon_{qAlt} \equiv \varepsilon_q \sim N(0,1)$. We refer to c_i as the *parameter for the compromise effect of row* i, and we assume that $\Sigma_{i=1}^{7} c_i = 0$, implying no bias on average toward selecting either the alternative or the fixed prospect. In other words, the constraint implies that this set of parameters does not have an average effect (summing across all rows in the MPL) on the preference between the alternative and the fixed prospect.

Our estimation strategy jointly estimates three sets of parameters: (i) the prospect theory preference parameters for loss aversion, λ, utility curvature, γ, and the form of the probability weighting function, $\{\alpha, \beta\}$; (ii) a vector of row-by-row compromise effect parameters, $\{c_i\}_{i=1}^{7}$; and (iii) the scaling parameters, σ_q, that scale utility differences for each screen, q. From our perspective, the scaling parameters are nuisance parameters. The incorporation of the (varying) scaling parameters partially addresses the critique of random utility models identified by Wilcox (2011) and Apesteguia and Ballester (2018). Our use of varying scaling parameters follows the spirit of the recommendations of Wilcox (2011). The solution of Apesteguia and Ballester (2018) – stochastic preferences parameters – could also be incorporated into our framework, though it would involve substantial computational hurdles because we have four preference parameters.

[8] In Part C, the alternative prospects are presented in increasing order of monetary payoff.

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 of the 28 fixed prospects in Part A have a chance of yielding \$0 (and likewise for Part B). Accordingly, $\gamma \geq 1$ would imply that any strictly positive alternative sure outcome would be preferred with probability 1. Every participant in the experiment made choices ruling out such extreme risk aversion, except for one participant.[9]

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.[10] Second, we assume that c_i takes the quadratic functional form $c_i = \pi_0 + \pi_1 \cdot i + \pi_2 \cdot i^2$. 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.

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.[11] To do so, we assume that γ, α, β are the same in the gain and loss domains. Note that γ captures concavity of the utility function in the gain domain and convexity 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 participants for whom the MLE algorithm does not converge (after 500 iterations) when the CPT model 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.

[9] 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 among the data that were excluded as a result of this.

[10] 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). We 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 fixed prospects of Part C, respectively, and σ_D for the two fixed prospects of the two screens of Part D we use.

[11] 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 primarily identify $\hat{\lambda}$. When we refer to "all screens from Parts A-D," we mean all screens excluding these two.

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 $\Pr{}_{q,7,-} \equiv \Phi((U(P_{q7}) - U(P_{qf}))/\sigma_q + c_7)$. 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:

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

where, for notational simplicity, we write $\Pr_{q,0,1}$ for $\Pr_{q,-,1}$ and $\Pr_{q,7,8}$ for $\Pr_{q,7,-}$. The likelihood function for all the participants pooled together is $\Pi_{p \epsilon P} L_p$, where P is the set of participants.

3.4 Robustness checks

In addition to the baseline CPT model described above (with CRRA utility and the Prelec (1998) probability weighting function), we estimated three additional models: (1) the CPT model with CRRA utility but with T&K's probability weighting function: $\omega(p) = p^{\alpha}/(p^{\alpha} + (1-p)^{\alpha})^{\frac{1}{\alpha}}$; (2) the CPT model with the Prelec probability weighting function, but with CARA (a.k.a. "exponential") utility (Köbberling and Wakker 2005), $u(x) = \frac{1-e^{-\alpha^{+}_{\text{expo}} x}}{\alpha^{+}_{\text{expo}}}$ if $x \geq 0$, $u(-x) = \frac{1-e^{-\alpha^{-}_{\text{expo}} |x|}}{\alpha^{-}_{\text{expo}}}$ if $x < 0$; and (3) the CPT model with the Prelec probability weighting function, but with expo-power utility (Saha 1993), $u(x) = \frac{1-e^{-\alpha_{\text{e-p}} x^{1-\gamma_{\text{e-p}}}}}{\alpha_{\text{e-p}}}$. The results presented below in Sections 5 and 6 are robust to the use of these alternative models (see the Online Appendix for details).

3.5 Identification With and Without the Pull Treatments

Our five Pull treatments are designed to identify the effect of the compromise effect on measured risk preferences. However, even without the Pull treatments, generic risk aversion experiments will be able to identify the compromise effect parameters. To gain intuition for this fact, consider a MPL experiment in which each screen features a different level of risk aversion (i.e., different values for γ, λ, α, and β) that elicits indifference at the middle row of the MPL. Accordingly, *measured* risk aversion will vary across screens (unless the researcher takes account of the compromise effect). Hence, the compromise effect will be identified as long as the level of risk aversion that elicits indifference in the middle row varies across MPL screens and the compromise-effect parameters are included in the model. Because the compromise effect parameters would be identified even without within-subject variation in the Pull treatment, our data could also be used to identify the compromise effects at the level of each individual participant, but those estimates would be less precise than the representative agent estimates on which we focus in this paper.

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 the right-hand-side column of the MPL), and a negative value of c_i implies a bias in favor of choosing the fixed prospects (which is in the left-hand-side column of the MPL). So Hypothesis 1 implies a switch point that is biased toward the middle row of the MPL.

Thus, we hypothesize that the compromise effect will cause utility concavity in the gain domain (as assessed in Part A) to be systematically increased across the range of treatments from Pull -2 to Pull 2 (in the model without the compromise effect).[12] For instance, consider the two example screenshots from the Introduction. The first screenshot illustrates the Pull -2 treatment. Since the intermediate alternatives are shifted away from zero, the compromise effect induces participants to choose an indifference point that is farther from zero, thereby implying relatively low utility concavity. In contrast, in the Pull 2 treatment, illustrated in the second screenshot, the intermediate alternatives are shifted closer to zero. The compromise effect causes participants to choose an indifference point that is closer to zero, thereby implying relatively high utility concavity.

The hypothesized effect of the Pull treatments on measured utility *convexity* in the loss domain is analogous. Moving across the range of treatments from Pull -2 to Pull 2 is now hypothesized to raise estimated utility convexity. 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 relatively low utility convexity. 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 relatively high utility convexity.

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 the compromise effect influences estimates of utility curvature γ and loss aversion λ in the traditional CPT model. In Section 3.2 above, we introduced a model that incorporates parameters for the compromise effect. If that model is properly specified, we would expect the bias induced by the compromise effect to disappear and the estimates of utility curvature γ and loss aversion γ to be similar across Pull treatments. In summary, we hypothesize:

Hypothesis 2.a: Estimates of utility curvature (γ, γ^+, γ^-) from our model with the compromise effect will not vary in Pull.

[12] We note that in CPT, risk preferences are determined by a combination of the utility function and the probability weighting function, and therefore there is no one-to-one mapping from risk preferences to utility curvature. However, our *ex ante* hypotheses concerned utility curvature γ and loss aversion λ only (not the probability weighting function). As we discuss below in Section 6, we find in our data that the compromise effect also influences the probability weighting function.

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

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

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

5 Estimating the Compromise Effect and Risk Preferences Jointly

We begin by estimating our model with the compromise effect. 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 focus on the other parameters (α, β, and the σ_q parameters) because we did not have *ex ante* hypotheses for these, but we briefly discuss the results for α and β in Section 6 below and 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 $\hat{\gamma}^+ = 0.448$. The estimate of γ^- is significantly smaller than 0 at the 5% level, indicating concavity of the utility function 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, consistent with some loss aversion, albeit less than usually assumed. Except for the notably small estimate of γ^-, our parameter estimates (including those for the probability weighting function parameters) are broadly in line with existing estimates in the literature. We compare our estimates to the literature in Section 10.

<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 to interpret.[13]

5.1 Estimating the Compromise Effect

We now proceed to test *Hypothesis 1*, which predicts that the parameters for the compromise effect 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. Figure 6 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 is 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 statistically distinguishable from zero: the p-value of the Wald test is less than 1×10^{-10}. These results strongly support Hypothesis 1 and are robust to restricting the data to the incentivized Part A only. We note that the compromise effect is weaker when estimated with the data from Part A versus the data from Part B. This may suggest that the compromise effect is stronger in the loss domain; alternatively, participant fatigue and the lack of incentives in Part B could have led to reduced participant attention and to a stronger compromise effect.

[13] Wakker (2010, section 9.6) highlights two concerns when $u(\cdot)$ takes the CRRA form 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 make estimates of λ sensitive to exactly which prospects are used in the experiment. As previously mentioned, in the Online Appendix we report estimates of a robustness check where we assume CARA utility and different utility curvature parameters in the gain and loss domains.

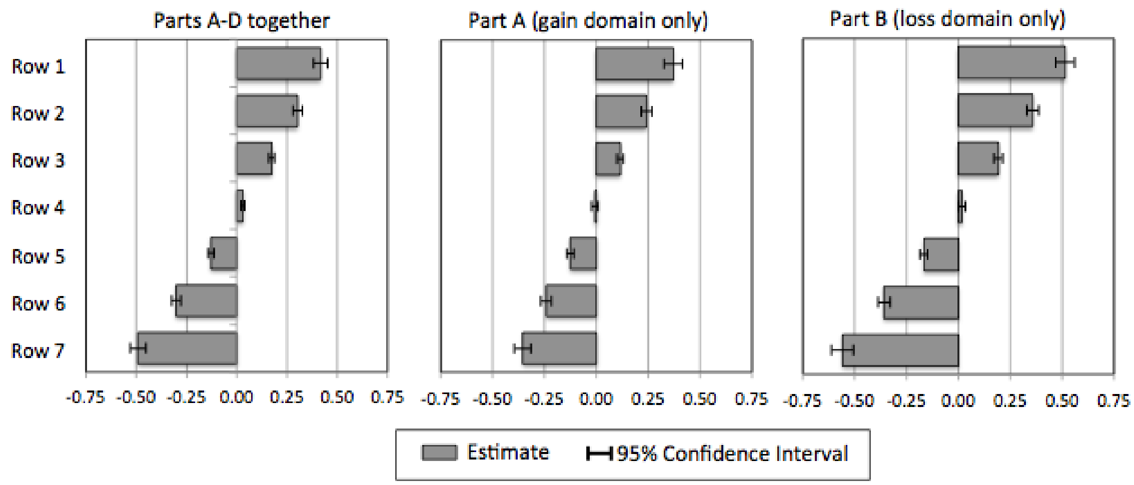


FIGURE 6. Implied estimates of the parameters for the compromise effect c_i as a function of the row i in which a choice appears. In the estimation, we parameterize the parameters for the compromise effect with the quadratic functional form $c_i = \pi_0 + \pi_1 \cdot i + \pi_2 \cdot i^2$, $\Sigma_{i=1}^{7} c_i = 0$, 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 $var(\hat{c}_i) \approx (i-4)^2 var(\hat{\pi}_1) + (i^2-20)^2 var(\hat{\pi}_2)$ (assuming $cov(\hat{\pi}_1, \hat{\pi}_2) \approx 0$).

5.2 Robustness of the Preference-Parameter Estimates from Joint Estimation

To test Hypotheses 2a and 2b, we begin by estimating the model with the compromise effect separately in the subsamples corresponding to each of the five Pull treatments. Figure 7 shows estimates of γ, γ^+ and γ^-, with 95% confidence intervals, for each subsample. Figure 8 shows estimates of λ.

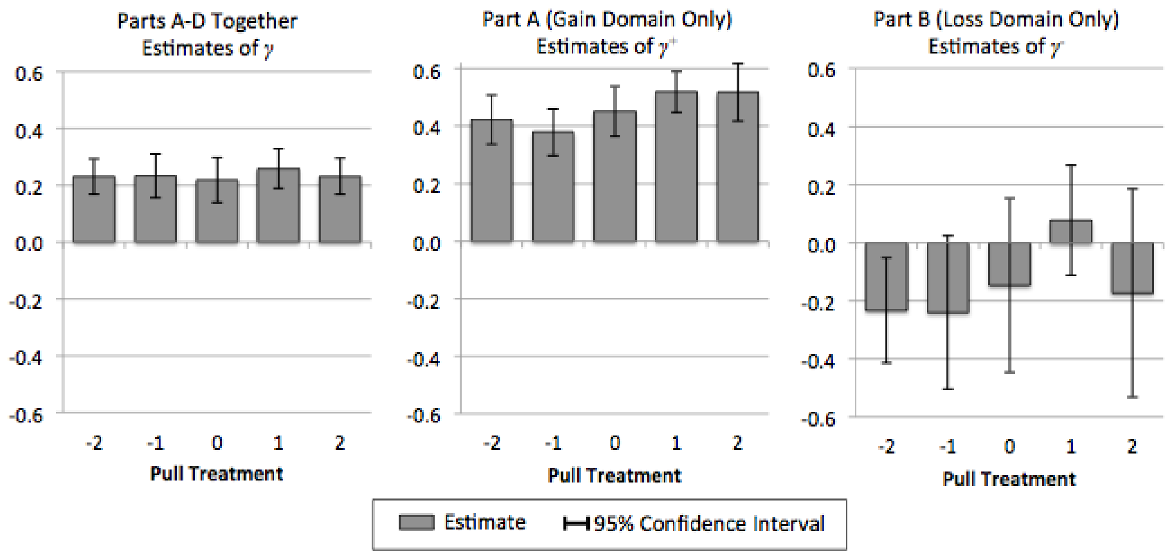


FIGURE 7. Estimates of γ, γ^+, and γ^- by Pull treatment, from the model with the compromise effect. The negative estimates of γ^- for Part B reflect concavity of the utility function in the loss domain, unlike what CPT predicts. (γ is not estimated for Parts C and D only because these parts have few 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 do likewise for α, β, and all the σ_q parameters, and we test whether the ϕ parameters are equal to zero.[14]

[14] The statistical power to test the pairwise differences in our parameter estimates (for each discrete step in the Pull treatment) is limited. Accordingly, we test *Hypothesis* 3.a and *Hypothesis* 3.b by estimating a linear model. Figures 9 and 10 imply that a linear specification is a good approximation.

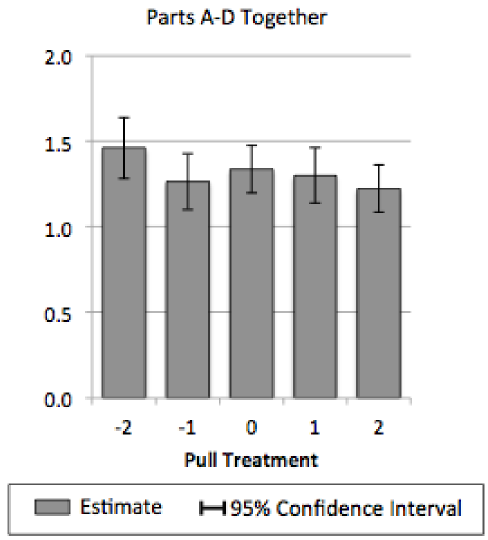


FIGURE 8. Estimates of λ by Pull treatment from the model with the compromise effect, for Parts A-D together. (λ cannot be estimated for Part A only or Part B only because the questions in these parts are all in the gain or loss domains. We do not estimate λ for Parts C and D only because these parts 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. 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 the compromise effect does not perfectly control for this effect. As we will see below, however, this estimate of ϕ_1^λ is much smaller than the one obtained from the model without the compromise effect, indicating that our model with the compromise effect substantially reduces the bias due to this effect.

<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 the Compromise Effect Is Omitted from the Model

We now proceed to estimate the CPT model without the compromise effect, 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 the compromise effect (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 with the compromise effect.

<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 the compromise effect separately in the subsamples corresponding to each of the five Pull treatments. As can be seen from Figures 9 and 10, 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 9 and 10 to Figures 7 and 8, it is clear that failing to control for the compromise effect 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 9, the Pull treatment manipulation of the compromise effect 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 10, the Pull treatment manipulation of the compromise effect 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 the compromise effect can create the appearance of no loss aversion.

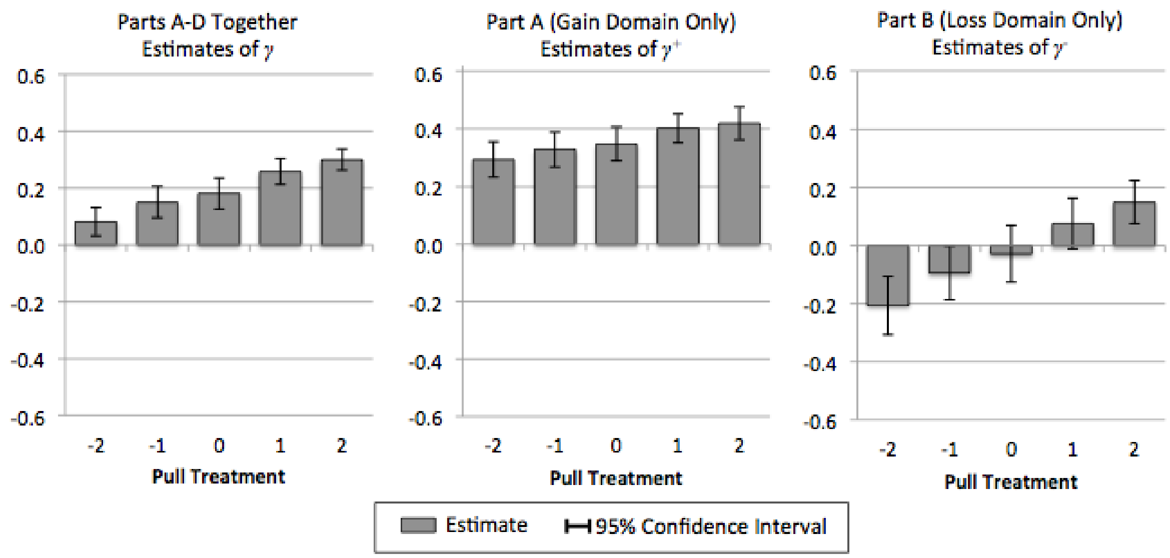


FIGURE 9. Estimates of γ, γ^+, and γ^- by Pull treatment, from the model without the compromise effect. This figure is analogous to Figure 7, except that the estimated model does not control for the compromise effect.

As above, we formally test the impact of the compromise effect 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 $\hat{\gamma}$, 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).

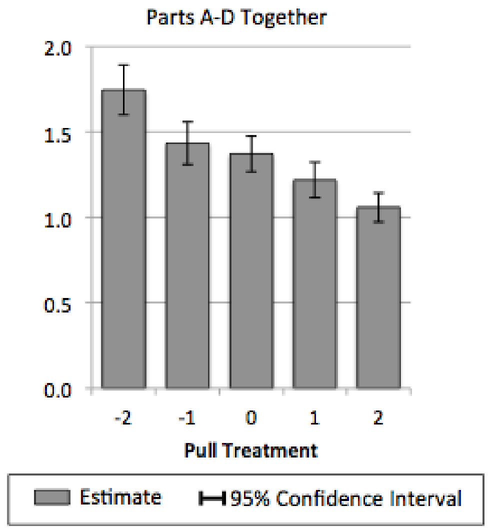


FIGURE 10. Estimates of λ by Pull treatment from the model without the compromise effect, for Parts A-D together. This figure is analogous to Figure 8, except that the estimated model does not control for the compromise effect.

<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 the compromise effect.

Although our *ex ante* hypotheses focused on estimated utility curvature $\hat{\gamma}$ and loss aversion $\hat{\lambda}$, we note that the compromise effect also influences on the estimated probability weighting function parameters $\hat{\alpha}$ and $\hat{\beta}$ in the model without the compromise effect. In the gain domain, a higher Pull is associated with α and β estimates that imply lower certainty equivalents (i.e., higher measured risk aversion) for most gambles, which reinforces the risk-aversion-increasing effects we find from a higher Pull on utility curvature γ.[15] This influence is absent or strongly attenuated in the model

[15] In the model without the compromise effect, $\hat{\beta}$ tends to increase in Pull. In the gain domain, this implies that Pull reduces the elevation of the probability weighting function, and thus lowers the probability weight assigned to the outcome with the higher payoff. In this way, Pull reduces the certainty equivalent for the gamble. As for $\hat{\alpha}$, it decreases in Pull. In the gain domain, a lower α means a lower probability weight for the outcome with the higher payoff if that outcome's probability exceeds $1/e = 0.368$. Since most gambles in the experiment satisfy that condition,

with the compromise effect.

7 How Large is the Compromise Effect?

Having demonstrated that the compromise effect can have a significant impact on choice in a MPL setting, we now obtain a rough estimate of its 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 effect and of the preference shocks scales linearly with the 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 the parameter σ_q (as defined in Section 3.2) regulates the relative importance of utility vs. the other parameters for the compromise effect and shocks, and $U(P_{qf})$ is the 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

$$\begin{aligned} & U(P_{qi}) - U\left(P_{qf}\right) + \Delta \cdot c_i \cdot U(P_{qf}) > \sigma_q \varepsilon_q \\ \iff \; & U(P_{qi}) - U\left((1+\theta_i) \cdot P_{qf}\right) > \sigma_q \varepsilon_q, \end{aligned}$$

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

Pull decreases $\hat{\alpha}$, which in turn implies a lower certainty equivalent for most gambles. Online Appendix Section 4 reports the complete set of estimates of the parameterized model with and without the compromise effect.

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 utility of the fixed prospect on screen $\tilde{q}$ calculated using the model parameters estimated from the specification with the compromise effect; 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 11 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.[16]

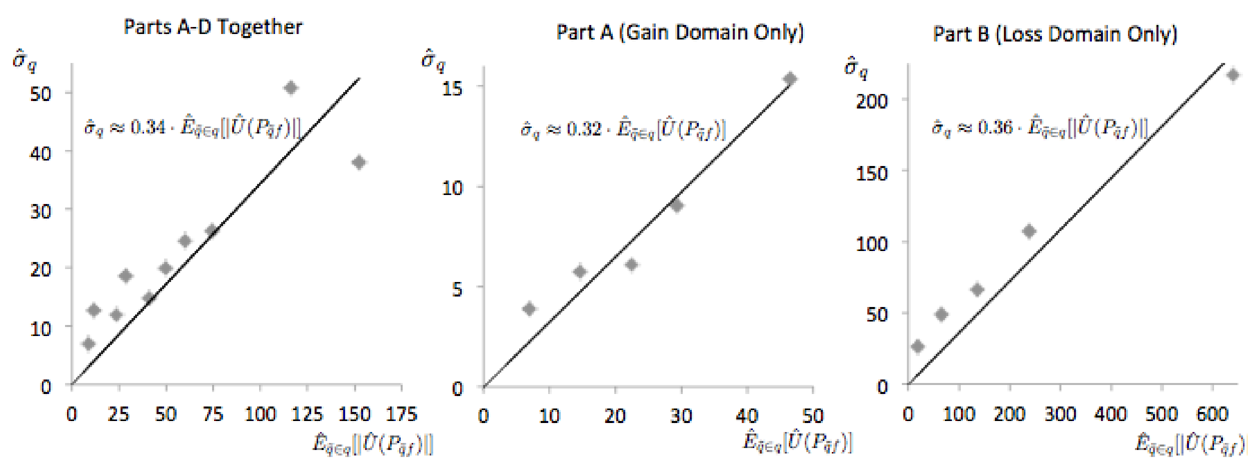


FIGURE 11. Relationship between $\hat{\sigma}_q$ and the utility of a screen's fixed prospect. See text for details.

Using the estimated $\hat{\Delta}$ for each of the three sets of results, Table 5 presents estimates of the

[16] 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 economically small in all cases. For the estimates of $\hat{\Delta}$ reported here, we use a 0 intercept.

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 effect 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.

8 Demographic Correlates of the CPT Model Parameters and of the Parameters that Capture the Compromise Effect

A large literature seeks to estimate the demographic correlates of economic preferences and decision making (e.g., Beauchamp, Cesarini, and Johannesson 2017, Benjamin, Brown, and Shapiro 2013, Booij, van Praag, and van de Kuilen 2010, Dohmen, Falk, Huffman, and Sunde 2010). The data we collected in our experiment, which include a number of demographic variables, allow us to contribute to this literature by analyzing the demographic correlates of the four key parameters of the CPT model (γ, λ, α, β) and of the two model parameters that capture the compromise effect (π_1, π_2). In our baseline demographic specification, we estimate our CPT model with the compromise effect using data from Parts A-D together, with these six key model parameters specified as linear functions of a constant, age, sex, a dummy variable indicating whether one has a college degree, SAT Math score, the log of one's parents' combined annual income, as well as dummy variables to control for race.

We also estimated several additional specifications to verify the robustness of the results from our baseline demographic specification. First, we estimated the baseline demographic specification again, but using data from Part A only, and then using data from Part B only. Second, we estimated a specification akin to the baseline demographic specification using data from Parts A-D together, but with CARA (a.k.a. "exponential") utility (Köbberling and Wakker 2005). As a third robustness check, we employed a two-step procedure in which we first estimated our baseline CPT model with the compromise effect separately for each participant, and then regressed each estimated parameter of interest on the demographic variables; to reduce the number of parameters and thereby improve the frequency of convergence in the first step of that procedure, we assume that σ_q is identical across all screens (for each experimental participant).

Two main results stand out across the baseline and robustness specifications. First, higher SAT Math scores are associated with lower γ—i.e., with lower utility concavity in the gain domain and lower convexity (or higher concavity) in the loss domain. This result is consistent with the existing literature on the association between cognitive ability and risk preferences (see Dohmen, Falk, Huffman, and Sunde 2018 for a review of the literature), although it has been argued that this association is driven by the fact that measurement noise may be higher for individuals with lower cognitive ability (Andersson, Holm, Tyran, and Wengström 2016). The second result that stands out is that higher SAT Math scores are associated with *higher* loss aversion (λ). This result, although robust across our specifications, is surprising given that previous research has found that education is negatively associated with loss aversion (Booij, van Praag, and van de Kuilen 2010, Gächter, Johnson, Herrmann 2007, Hjorth and Fosgerau 2011). Aside from these two results, the associations between the other covariates and parameters were not statistically distinguishable from zero or were not robust across specifications.

The Online Appendix reports estimates of the baseline demographic specification and provides additional details. We note that one limitation of this analysis is that our sample of experimental participants was not selected to be representative of the population.

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

We designed our experiment not only to test our *ex ante* hypotheses about the compromise effect, but also to test our *ex ante* hypothesis that providing the expected value of the gamble to participants would make observed preferences more risk neutral. We reasoned that displaying expected value may anchor the participants on the expected value (Tversky and Kahneman, 1974) or simplify comprehension of the gamble (Benjamin, Brown, and Shapiro, 2013). If so, then whether or not the expected value is salient may be another factor that varies across risk-taking environments that may help explain variation in risk-taking behavior. Put in terms of our parameter estimates, we 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 10.1 and 10.2 show estimates of $\hat{\gamma}$ and $\hat{\lambda}$ for the subsamples corresponding to the two EV treatments, with 95% confidence intervals. Displaying the expected value does not appear to affect estimated risk preferences 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. A difference between our experiment 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 intuitively 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.

10 Discussion

In this paper, we estimate an econometric model that explicitly takes into account the compromise effect and thus disentangles it from risk preference parameters. The resulting risk-preference estimates are robust: the inferred risk parameters essentially do not change with exogenous manipulations of the compromise effect. Without parameters for the compromise effect, however, we replicate the finding from prior work that risk-preference-parameter estimates are sensitive to exogenous manipulations of the compromise effect.

How do our "debiased" preference-parameter estimates (from Table 1) compare to those from the literature? For utility curvature in the gain and loss domains, we respectively estimate $\hat{\gamma}^{+} = 0.448$ (i.e., gain domain concavity) and $\hat{\gamma}^{-} = -0.106$ (i.e., loss domain *concavity*, which contradicts the CPT prediction of loss domain *convexity*). Booij, van Praag, and van de Kuilen'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. For loss aversion, we estimate $\widehat{\lambda} = 1.311$. Although T&K estimated λ to be 2.25, the literature contains a broad range of estimates: among the papers reviewed by Abdellaoui, Bleichrodt, and Paraschiv (2007, Tables 1 and 5), $\hat{\lambda} \in [0.74, 8.27]$, and among those reviewed by Booij, van Praag, and van de Kuilen (2010, Table 1), $\hat{\lambda} \in [1.07, 2.61]$. Finally, our estimates of the two-parameter Prelec (1998) probability-weighting parameters are in the ranges $\widehat{\alpha} \in [0.564, 0.690]$ and $\widehat{\beta} \in [0.858, 1.471]$. Booij, van Praag, and van de Kuilen's (2010) Table 1 only lists three studies that estimated this 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 $\hat{\beta}^+ \in [1.08, 2.12]$. Fox and Poldrack's (2014) Table A.3 also lists three studies that estimated the two-parameter Prelec (1998) functional form for prospects in the gain domain. The ranges of estimates are $\hat{\alpha}^+ \in [0.62, 1.15]$ and $\hat{\beta}^+ \in [1.00, 1.58]$. Overall, then, our parameter estimates are broadly in line with existing estimates in the literature, except that some of our estimates of the probability weighting parameter β^+ fall below the range of estimates in the literature and our negatively signed estimate of γ^- implies concavity in the loss domain, which is only occassionally observed experimentally and is the opposite of CPT's prediction.

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 parameters 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.[17]

[17] Sprenger (2015) assumes that the fixed prospect in each binary choice pins down a participant's reference point. Because the fixed prospect was held constant across our scale manipulations, this approach can't explain the effects we find.

11 Future Work and Extensions

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. For example, our experiment includes 64 MPL's, which may induce fatigue among experimental participants, potentially explaining why the compromise effect strengthens from Part A to Part B.

Future work should explore at least four different directions. First, the existence of an economically significant compromise effect should lead experimenters to design experiments that minimize the influence of compromise effects. For example, would it be methodologically superior to randomize the order of the rows on each screen of an MPL? This proposal would trade off a smaller compromise effect with a greater cognitive burden on experimental participants.

Second, when compromise effects *are* likely to emerge in a particular experimental design, then the experimenters should consider including enough treatments and enough participants to be able to econometrically disentangle the compromise effects from other economic parameters of interest. Doing this efficiently – i.e., with an optimized number of treatment arms and participants – is an open econometric challenge that we anticipate future research will address. In principle, the methodology we have demonstrated—jointly estimating the compromise effect and preference parameters—is general and can be applied and extended to other domains. In practice, it is a challenge to undertake this extension in a way that is efficient.

Third, we should extend these methods to other settings where existing designs have been influenced by compromise effects and where new 'compromise-free' analysis will improve our understanding of behavior.

Fourth, the same econometric procedure we implement here—estimating a discrete-choice model that includes additional parameters that capture location in the choice set—could also be applied to measure and control for other types of context effects, such as a tendency to choose items that happen to come at the beginning of a list of alternatives (e.g., as in election ballots; e.g., Koppell and Steen 2004).

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

	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. These are tests of the null hypothesis that the coefficient is zero. However, with respect to all parameters (except π_1 and π_2) the natural null hypothesis is not equality to 0. For instance, λ is the loss aversion parameter, so the hypothesis of local linearity is $\lambda = 1$. We reject this restriction: the t-stat is $(1.311 - 1)/0.034 = 9.15$.

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

		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^-	α_0	0.556***	0.505***	0.617***
		(0.019)	(0.018)	(0.027)
β, β^+, β^-	β_0	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. These are tests of the null hypothesis that the coefficient is zero.

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

	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.
* significant at 10% level; ** significant at 5% level; *** significant at 1% level. These are tests of the null hypothesis that the coefficient is zero.

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

		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^-	α_0	0.535***	0.497***	0.577***
		(0.012)	(0.014)	(0.016)
β, β^+, β^-	β_0	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. These are tests of the null hypothesis that the coefficient is zero.

Table 5. Implied Impact of the Compromise Effect Expressed as a Fraction 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.