

# Reduction of Compound Lotteries with Objective Probabilities: Theory and Evidence

by

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## ABSTRACT.

The reduction of compound lotteries axiom (ROCL) has assumed a central role in the evaluation of behavior towards risk and uncertainty. We present experimental evidence on its validity in the domain of objective probabilities. Our experiment explicitly recognizes the impact that the random lottery incentive mechanism payment procedure may have on preferences, and so we collect data using both “1-in-1” and “1-in-K” payment procedures, where  $K > 1$ . Our battery of lottery pairs includes simple one-stage lotteries, two-stages compound lotteries, and their actuarially-equivalent one-stage lotteries. We find *violations of ROCL* and that behavior is better characterized by a source-dependent version of the Rank-Dependent Utility model rather than Expected Utility Theory. However, *violations of ROCL* occur only in the treatment in which many choices are given to each subject and the random lottery incentive mechanism, used as payment protocol, itself induces a “double compounding” by design. We do *not infer any violations of ROCL* when subjects are only given one decision to make. These results are supported by both non-parametric analysis of choice patterns as well as structural estimation of latent preferences. We conclude that payment protocols can create distortions in experimental tests of ROCL.

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The reduction of compound lotteries axiom (ROCL) has assumed a central role in the evaluation of behavior towards risk, uncertainty and ambiguity. We present experimental evidence on its validity in domains defined over objective probabilities, where the tests are as clean as they can possibly be.<sup>1</sup> Even in this setting, one has to pay close attention to the experimental payment protocols used and their interaction with the experimental task, so that one does not inadvertently assume precisely what one is trying to test. We show that the behavior is indeed consistent with ROCL when one “cleans the lab test tubes” to the utmost, but that behavior is inconsistent with ROCL when one uses common payment procedures that require that one implicitly assume the validity of ROCL.

We therefore show that a fundamental methodological problem with tests of the ROCL assumption is that one cannot use incentives for decision makers that rely on the validity of ROCL. This means, in effect, that experiments must be conducted in which a subject has one, and only one, choice.<sup>2</sup> Apart from the expense and time of collecting data at such a pace, this also means that all evaluations have to be on a between-subjects basis, in turn implying the necessity of modeling assumptions about heterogeneity in behavior.

In sections 1 and 2 we define the theory and experimental tasks used to examine ROCL in the context of objective probabilities. In section 3 we present evidence from our experiment. We find *violations of ROCL*, and observed behavior is better characterized by the Rank-Dependent Utility model (RDU) rather than Expected Utility Theory (EUT). However, *violations of ROCL* only occur when *many* choices are given to each subject and the random lottery incentive mechanism (RLIM) is used as the

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<sup>1</sup> The validity of ROCL over objective probabilities has also been identified as a potential indicator of attitudes towards uncertainty and ambiguity. Smith [1969] conjectured that people might have similar, source-dependent preferences over compound lotteries defined over objective probabilities *and* over ambiguous lotteries where the probabilities are not well-defined. Halevy [2007] provides experimental evidence that attitudes towards ambiguity and compound objective lotteries are indeed tightly associated.

<sup>2</sup> One alternative is to present the decision maker with several tasks at once and evaluate the portfolio chosen, or to present the decision maker with several tasks in sequence and account for wealth effects. Neither is attractive, since they each raise a number of (fascinating) theoretical confounds to the interpretation of observed behavior. One uninteresting alternative is not to pay the decision maker for the outcomes of the task.

payment protocol. We do *not infer any violations of ROCL* when subjects are only given *one* decision to make. Section 4 draws conclusions for modeling, experimental design, and inference about decision making.

## 1. Theory

We start with a statement of some basic axioms used in models of decision-making under risk, and then discuss their implications for the experimental design. Our primary conclusion is the existence of an interaction of experimental payment protocols and the validity of the ROCL axiom. To understand how one can design theoretically clean tests of ROCL that do not run into confounds, we must state the axioms precisely.

### *A. Basic Axioms*

Following Segal [1988][1990][1992], we distinguish between three axioms. In words, the **Reduction of Compound Lotteries** axiom states that a decision-maker is indifferent between a compound lottery and the actuarially-equivalent simple lottery in which the probabilities of the two stages of the compound lottery have been multiplied out. To use the language of Samuelson [1952; p.671], the former generates a *compound income-probability-situation*, and the latter defines an *associated income-probability-situation*, and that “...only algebra, not human behavior, is involved in this definition.”

To state this more explicitly, with notation to be used to state all axioms, let X, Y and Z denote simple lotteries, A and B denote compound lotteries,  $\succ$  express strict preference, and  $\sim$  express indifference. Then the ROCL axiom says that  $A \sim X$  if the probabilities and prizes in X are the actuarially-equivalent probabilities and prizes from A. Thus if A is the compound lottery that pays “double or nothing” from the outcome of the lottery that pays \$10 if a coin flip is a head and \$2 if the coin flip is a tail, then X would be the lottery that pays \$20 with probability  $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$ , \$4 with

probability  $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$ , and nothing with probability  $\frac{1}{2}$ . From an observational perspective, one must see choices between compound lotteries and actuarially-equivalent simple lotteries to test ROCL.

The **Compound Independence Axiom** (CIA) states that two compound lotteries, each formed from a simple lottery by adding a positive common lottery with the same probability, will exhibit the same preference ordering as the simple lotteries. This is a statement that the ordering of the two constructed compound lotteries will be the same as the ordering of the different simple lotteries that distinguish the compound lotteries, provided that the common prize in the compound lotteries is the same and has the same (compound lottery) probability. It says nothing about how the compound lotteries are to be evaluated, and in particular *it does not assume ROCL*. It only restricts the preference *ordering* of the two constructed compound lotteries to match the preference *ordering* of the original simple lotteries.

The CIA says that if A is the compound lottery giving the simple lottery X with probability  $\alpha$  and the simple lottery Z with probability  $(1-\alpha)$ , and B is the compound lottery giving the simple lottery Y with probability  $\alpha$  and the simple lottery Z with probability  $(1-\alpha)$ , then  $A \succ B$  iff  $X \succ Y \forall \alpha \in (0,1)$ . The construction of the two compound lotteries A and B has the “independence axiom” cadence of the common prize Z with a common probability  $(1-\alpha)$ , but the implication is only that the ordering of the compound and constituent simple lotteries are the same.<sup>3</sup>

Finally, the **Mixture Independence Axiom** (MIA) says that the preference ordering of two simple lotteries must be the same as the actuarially-equivalent simple lottery formed by adding a common outcome in a compound lottery of each of the simple lotteries, where the common outcome

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<sup>3</sup> For example, Segal [1992; p.170] defines the CIA by assuming that the second-stage lotteries are replaced by their certainty-equivalent, “throwing away” information about the second-stage probabilities before one examines the first-stage probabilities at all. Hence one cannot then *define* the actuarially-equivalent simple lottery, by construction, since the informational bridge to that calculation has been burnt. The certainty-equivalent could have been generated by any model of decision making under risk, such as RDU or Prospect Theory.

has the same value and the same (compound lottery) probability. So stated, it is clear that the MIA strengthens the CIA by making a definite statement that the constructed compound lotteries are to be evaluated in a way that is ROCL-consistent. Construction of the compound lottery in the MIA is actually implicit: the axiom only makes observable statements about two pairs of simple lotteries. To restate Samuelson's point about the definition of ROCL, the experimenter testing the MIA could have constructed the associated income-probability-situation without knowing the risk preferences of the individual (although the experimenter would need to know how to multiply).

The MIA says that  $X \succ Y$  iff the actuarially-equivalent simple lottery of  $\alpha X + (1-\alpha)Z$  is strictly preferred to the actuarially-equivalent simple lottery of  $\alpha Y + (1-\alpha)Z$ ,  $\forall \alpha \in (0,1)$ . The verbose language used to state the axiom makes it clear that MIA embeds ROCL into the usual independence axiom construction with a common prize  $Z$  and a common probability  $(1-\alpha)$  for that prize.

The reason these three axioms are important is that the failure of MIA does not imply the failure of CIA and ROCL. It does imply the failure of one or the other, but it is far from obvious which one. Indeed, one could imagine some individuals or task domains where only CIA might fail, only ROCL might fail, or both might fail. Because specific types of failures of ROCL lie at the heart of many important models of decision-making under uncertainty and ambiguity, it is critical to keep the axioms distinct as a theoretical *and* experimental matter.

### *B. Payment Protocols and Experimental Design*

The choice of the payment protocol is critical to test ROCL. The most popular payment protocol for individual choice experiments assumes the validity of the CIA. This payment protocol is called the **Random Lottery Incentive Mechanism** (RLIM). It entails the subject making  $K$  choices and then one of the  $K$  choices is selected at random to be played out. Typically, and without loss of generality, assume that the selection of the  $k$ -th task to be played out uses a uniform distribution over the

K tasks. Since the other  $K-1$  tasks will generate a payoff of zero, the payment protocol can be seen as a compound lottery that assigns probability  $\alpha = 1/k$  to the selected task and  $(1-\alpha) = (1-(1/k))$  to the other  $K-1$  tasks as a whole. If the experiment consists of binary choices between simple lotteries X and Y, then immediately the RLIM can be seen to entail an application of the CIA, where  $Z = U(\$0)$  and  $(1-\alpha) = (1-(1/k))$ , for the utility function  $U(\cdot)$ . Hence, under the CIA, the preference ordering of X and Y is independent of all of the choices in the other tasks (Holt [1986]).

If the K objects of choice in the experiment include any compound lotteries directly or indirectly, then the RLIM requires the stronger MIA instead of just the CIA. Indeed, this was the setting for the classic discussions of Holt [1986], Karni and Safra [1987] and Segal [1988] on the interaction of the independence axiom with the RLIM, which were motivated by the “preference reversal” findings of Grether and Plott [1979]. In those experiments the elicitation procedure for the certainty-equivalents of simple lotteries was, itself, a compound lottery. Hence the validity of the incentives for this design required both CIA and ROCL, hence MIA. Holt [1986] and Karni and Safra [1987] showed that if CIA was violated, but ROCL and transitivity was assumed, one might still observe choices that suggest “preference reversals.” Segal [1988] showed that if ROCL was violated, but CIA and transitivity was assumed, that one might also still observe choices that suggest “preference reversals.” Again, the only reason that ROCL was implicated in these discussions is because the experimental task implicitly included choices over compound lotteries. In our experiment, we consider choices over simple lotteries and compound lotteries in one of our treatments, so the validity of RLIM in the latter rests on the validity of the CIA and ROCL.

The need to assume the CIA or MIA can be avoided by setting  $K=1$  and asking each subject to answer one binary choice task for payment, as advocated by Cox, Sadiraj and Schmidt [2011] and Harrison and Swarthout [2012]. Unfortunately, this comes at the cost of another assumption: the assumption that risk preferences across subjects are the same. This is a strong assumption, obviously,

and one that leads to inferential tradeoffs in terms of the “power” of tests relying on randomization that will vary with sample size. Sadly, plausible estimates of the degree of heterogeneity in the typical population imply massive sample sizes for reasonable power, well beyond those of most experiments.

The assumption of homogeneous preferences can be diluted, however, by changing it to a conditional form: that risk preferences are homogeneous conditional on a finite set of observable characteristics. Although this sounds like an econometric assumption, and it certainly has statistical implications, it is as much a matter of (operationally meaningful) theory as formal statements of the CIA, ROCL and MIA.

## 2. Experiment

### *A. Lottery Parameters*

We designed our battery of lotteries to allow for specific types of comparisons needed for testing ROCL. Beginning with a given *simple* (S) lottery and *compound* (C) lottery, we next create an *actuarially-equivalent* (AE) lottery from the C lottery, and then we construct three pairs of lotteries: a S-C pair, a S-AE pair, and an AE-C pair. By repeating this process 15 times, we create a battery of lotteries consisting of 15 S-C pairs shown in Table A2, 15 S-AE pairs shown in Table A3, and 10 AE-C pairs<sup>4</sup> shown in Table A4. Appendix A explains the logic behind the selection of these lotteries.

Figure 1 displays the coverage of lotteries in the Marschak-Machina triangle, covering all of the contexts used. Probabilities were drawn from 0,  $\frac{1}{4}$ ,  $\frac{1}{2}$ ,  $\frac{3}{4}$  and 1, and the final prizes from \$0, \$10, \$20, \$35 and \$70. We use the familiar “Double or Nothing” (DON) procedure for creating compound lotteries. So, the first-stage prizes displayed in a compound lottery were drawn from \$5, \$10, \$17.50 and \$35, and then the second-stage DON procedure yields the set of final prizes given above.

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<sup>4</sup> The lottery battery contains only 10 AE-C lottery pairs because some of the 15 S-C lottery pairs shared the same compound lottery.



The majority of our compound lotteries use a conditional version of DON in the sense that the initial lottery will trigger the double or nothing option that the subject will face only if a particular outcome is realized in the initial lottery. For example, consider the compound lottery formed by an initial lottery that pays \$10 and \$20 with equal probability and a subsequent DON lottery if the outcome of the initial lottery is \$10, implying a payoff of \$20 or \$0 with equal chance *if the DON stage is reached*. If the initial lottery outcome is \$20, there is no subsequent DON stage. The right panel of Figure 2 shows a tree representation of this compound lottery where the initial lottery is depicted in the first stage and the DON lottery is depicted in the second stage of the compound lottery. The left panel of Figure 2 shows the corresponding actuarially-equivalent simple lottery which offers \$20 with probability  $\frac{3}{4}$  and \$0 with probability  $\frac{1}{4}$ .

The conditional DON lottery allows us to obtain good coverage in terms of prizes and probabilities and to maintain simple random processes for the initial lottery and the DON lottery. One can construct a myriad of compound lotteries with only two components: (1) initial lotteries that pay two outcomes with 50:50 odds or pay a given stake with certainty; and (2) a conditional DON which pays double a predetermined amount with 50% probability or nothing with equal chance. Using only the unconditional DON option would impose an *a priori* restriction on the coverage within the Marschak-Machina triangle.

### *B. Experimental Procedures*

We implement two between-subjects treatments. We call one treatment “Pay 1-in-1” (1-in-1) and the other “Pay 1-in-40” (1-in-40). Table 1 summarizes our experimental design and the sample size of subjects and choices in each treatment.

In the 1-in-1 treatment, each subject faces a single choice over two lotteries. The lottery pair presented to each subject is randomly selected from the battery of 40 lottery pairs. The lottery chosen

by the subject is then played out and the subject receives the realized monetary outcome. There are no other rewarded tasks, before or after a subject's binary choice, that affect the outcome. Further, there is no other activity that may contribute to learning about decision making in this context.

In the 1-in-40 treatment, each subject faces choices over all 40 lottery pairs, with the order of the pairs randomly shuffled for each subject. After all choices have been made, one choice is randomly selected for payment using the RLIM, with each choice having a 1-in-40 chance of being selected. The selected choice is then played out and the subject receives the realized monetary outcome, again with no other rewarded tasks. This treatment is potentially different from the 1-in-1 treatment in the absence of ROCL, since the RLIM induces a compound lottery consisting of a 1-in-40 chance for each of the 40 chosen lotteries to be selected for payment.

The general procedures during an experiment session were as follows. Upon arrival at the laboratory, each subject drew a number from a box which determined random seating position within the laboratory. After being seated and signing the informed consent document, subjects were given printed instructions and allowed sufficient time to read these instructions<sup>5</sup>. Once subjects had finished reading the instructions, an experimenter at the front of the room read aloud the instructions, word for word. Then the randomizing devices<sup>6</sup> were explained and projected onto the front screen and three large flat-screen televisions spread throughout the laboratory. The subjects were then presented with lottery choices, followed by a demographic questionnaire that did not affect final payoffs. Next, each subject was approached by an experimenter who provided dice for the subject to roll and determine her own payoff. If a DON stage was reached, a subject would flip a U.S. quarter dollar coin to determine the final outcome of the lottery. Finally, subjects left the laboratory and were privately paid their earnings: a

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<sup>5</sup> Appendix A provides complete subject instructions.

<sup>6</sup> Only physical randomizing devices were used, and these devices were demonstrated prior to any decisions. In the 1-in-40 treatment, two 10-sided dice were rolled by each subject until a number between 1 and 40 came up to select the relevant choice for payment. Subjects in both treatments would roll the two 10-sided dice (a second roll in the case of the 1-in-40 treatment) to determine the outcome of the chosen lottery.

\$7.50 participation payment in addition to the monetary outcome of the realized lottery.

We used software to present lotteries to subjects and record their choices. Figure 3 shows an example of the subject display of an AE-C lottery pair.<sup>7</sup> The pie chart on the right of Figure 3 displays the first and second stages of the compound lottery as an initial lottery that has a DON stage identified by text. The pie chart on the left of Figure 3 shows the paired AE lottery. Figure 4 shows an example of the subject display of a S-C lottery pair, and Figure 5 shows an example of the subject display of a S-AE lottery pair.

### *C. Evaluation of Hypotheses*

The 1-in-40 treatment adds an additional layer of compounding of choices that subjects do not face in the 1-in-1 treatment. If subjects in both treatments have the same risk preferences and behavior is consistent with ROCL, we should see the same pattern of decisions for comparable lottery pairs across the two treatments in spite of the additional layer of compounding in the 1-in-40 treatment. The same pattern should also be observed as one characterizes heterogeneity of individual preferences towards risk, although these inferences depend on the validity of the manner in which heterogeneity is modeled.

Nothing here assumes that behavior is characterized by EUT. The validity of EUT requires both ROCL and CIA. So when we say that risk preferences should be the same in the two treatments under ROCL, these are simply statements about the Arrow-Pratt risk premium, and not about how that is decomposed into explanations that rely on diminishing marginal utility or probability weighting. For instance, the Rank-Dependant Utility model assumes ROCL but the risk premium of a compound

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<sup>7</sup> Decision screens were presented to subjects in color. Black borders were added to each pie slice in Figures 3, 4 and 5 to facilitate black-and-white viewing. In black and white these displays might make it appear that the \$0 prize was shown in bold and the others not. This is an illusion; all prizes and probabilities were displayed equally, but with distinct colors.

lottery depends both on aversion towards variation in prizes (utility function) and attitudes towards probabilities. We later analyze the decomposition of the risk premium as well as the nature of any violation of ROCL.

Our method of evaluation is twofold. First, we use non-parametric tests to evaluate the choice patterns of subjects. Our experimental design allows us to evaluate ROCL using choice patterns in two ways: we examine choice patterns in AE-C lottery pairs where ROCL predicts *indifference*, and we examine choice patterns across the linked S-C and S-AE lottery pairs where ROCL predicts *consistent choices*.

For our second method of evaluation of ROCL, we estimate structural models of risk preferences and test if the risk preference parameters depend on whether a C or an AE lottery is being evaluated. This method does not assume EUT, and indeed we allow non-EUT specifications. We specify a source-dependent form of utility and probability weighting, and test for violations of ROCL by determining if the subjects evaluate simple and compound lotteries differently.

In both of our methods of evaluation of ROCL, we use data from the 1-in-1 treatment and the 1-in-40 treatment which uses RLIM as the payment protocol. Of course, analysis of the data from the 1-in-40 treatment requires us to assume that CIA holds. However, by also analyzing choices from the 1-in-1 treatment we can test if the RLIM itself creates distortions that could be confounded with violations of ROCL, and in fact this turns out to be critical to the validity of ROCL.

#### *D. Different Sample Sizes*

One difference in the treatments is that every subject in the 1-in-K task makes K times the number of choices of each subject in the 1-in-1 task. This is tautological, from our design, but might raise concerns when estimating treatment effects. We conduct some simple statistical and econometric

checks to address these concerns.<sup>8</sup>

The simplest way to check on this, to be certain, is to sample *from* the sample of 1-in-K choices to match the sample of 1-in-1 choices. Each bootstrap simulation draws one choice at random from the K choices for each subject in the 1-in-K treatment, estimates a structural model of the treatment effect for those data and the 1-in-1 choices, and then repeats that process. Since the specific lottery pair assigned to subjects in the 1-in-K treatment was randomized in order, we can simply bootstrap from each of the 40 choices in sequence. In addition, we add in 460 further bootstrap replications in which one choice of the 1-in-K subjects are sampled at random. The distribution of  $p$ -values over these 500 bootstrap draws will then reveal if the treatment effect is significant or not, without any concerns about differential sample sizes.

### 3. Evidence

We evaluate the evidence by examining choice patterns and estimating preferences. Each approach has strengths and weaknesses. Evaluating choice patterns has the advantage of remaining agnostic about the particular model of decision making under risk. However, it has the disadvantage of not using all information embedded in the difference between the two lotteries. If one assumes an RDU model for illustration, it is intuitively clear that a deviation from RDU maximization should be more serious if the RDU difference is large than when it is minuscule. Simply counting the number of violations of predicted choice patterns, and ignoring the size of the deviation, ignores this information. Of course, to use that information one has to make some assumptions about what determines the probability of any predicted choice, and hence offer a metric for comparing the importance of deviations from risk neutrality.

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<sup>8</sup> We certainly allow for correlated errors within the choices by each individual in the 1-in-K tasks, which is a separate statistical issue.

A structural model of behavior, again using RDU for example, allows a more rigorous use of information on the size of errors from the perspective of the null hypothesis that ROCL is valid. For example, choices that are inconsistent with the null hypothesis but that involve statistically insignificant errors from the perspective of that hypothesis are not treated with the same weight as statistically significant errors.

An additional advantage of a structural model is that it is relatively easy to extend it to allow for varying degrees of heterogeneity of preferences, which is critical for between-subject tests with 1-in-1 data unless one is willing to maintain the unattractive assumption of homogeneous risk preferences. Given the importance of our treatment in which we study just one choice per subject, this ability to compare behavior from pooled choices across subjects, while still conditioning on some differences in subjects, is essential.

Again, we see these two ways of evaluating results as complementary. This is true even when they all come to the same “bottom line” conclusion. As it happens, they do not come to the same conclusion here, making it all the more important to understand why they are complementary.

#### *A. Evidence from Choice Patterns*

To analyze choice patterns, we test two hypotheses predicted by ROCL: 1. indifference between a compound lottery and an actuarially-equivalent simple lottery; and 2. consistency of choices when compound lottery is replaced by its actuarially-equivalent simple lottery.<sup>9</sup> This section summarizes our

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<sup>9</sup> We use an indifference test of ROCL since it is a natural one given that the definition of ROCL itself requires the indifference between a compound lottery and its actuarially-equivalent lottery. However, it is difficult to identify empirically when a subject is truly indifferent between two options. For example, when indifferent between two options, a subject might have the heuristic of flipping a coin to choose one or the other, or he could even systematically choose the option that is first presented to him from left to right or from top to bottom. In our non-parametric test of indifference we assume that subjects that display indifference flip a “mental” coin to choose between the two lotteries, and thus we should observe the compound lottery and its actuarially-equivalent lottery being chosen with equal proportions when pooling 1-in-1 choices across subjects. Given these potential issues with the indifference test, we also have a consistency test that yield clear predictions of behavior. Only 10 of the 40 lottery pairs in our battery of lotteries is of the

hypothesis tests of choice patterns. More detailed discussion of our statistical tests are presented in Appendix D.

Beginning with the indifference hypothesis, we do *not* find statistical evidence to reject the ROCL prediction of indifference in the 1-in-1 treatment, although we *do* find statistical evidence of violations of ROCL in the 1-in-40 treatment. Table D1 presents choice data for all AE-C lottery pairs in the 1-in-1 treatment. Roughly 59% of subjects chose the C lottery, and a Fisher Exact test fails to reject the hypothesis of indifferent (i.e., equiprobable) choices ( $p$ -value = 0.342). Further, Table D2 reports an individual Binomial test of equiprobable choices for each AE-C pair in the 1-in-1 treatment, and every  $p$ -value is insignificant at any reasonable level of confidence.<sup>10</sup> In contrast, Table D3 presents a Cochran Q test of the AE-C choices in the 1-in-40 treatment, and equiprobable choice is resoundingly rejected ( $p$ -value < 0.0001).

Moving to the consistency hypothesis, we consider the following scenario. Suppose a subject is presented with a given S-C lottery pair, and further assume that she prefers the C lottery over the S lottery. If the subject satisfies ROCL and is separately presented with a second pair of lotteries consisting of the same S lottery and the AE lottery of the previously-presented C lottery, then she would prefer and should choose the AE lottery. Similarly, of course, if she instead prefers the S lottery when presented separately with a given S-C lottery pair, then she should choose the S lottery when presented with the corresponding S-AE lottery pair. Therefore, ROCL is violated if we observe unequal proportions of choosing the S lottery across an S-C pair and the linked S-AE pair.

We do *not* find evidence in the choice data to reject the consistency hypothesis in the 1-in-1 treatment, while we *do* find evidence of violations of ROCL in the 1-in-40 treatment. Table D4 presents

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compound-actuarially.equivalent type.

<sup>10</sup> Evidently, it is possible that we fail to reject the null hypothesis given that we have a small sample size in the indifference test of the 1-in-1 case. However, we also test ROCL with the consistency test in which the sample size is bigger and with the structural estimation where we pool responses from both the consistency and the indifference tests.

a Fisher Exact test for each of the comparisons in the 1-in-1 treatment with sufficient data for the test. Only 1 of the 11 tests results in statistically significant evidence of inconsistency. Further, we conduct a Cochran-Mantel-Haenszel test to jointly evaluate whether choices in the 1-in-1 treatment are consistent over all linked pairs, and we conclude consistency ( $p$ -value = 0.122). In contrast, Table D5 presents results of the Cochran Q test coupled with the Bonferroni-Dunn correction procedure to evaluate consistency of choices in the 1-in-40 treatment, and we see statistically significant evidence of inconsistent choices.

We find consistent evidence from the choice patterns, whether we look at predictions of indifference or predictions of consistent choice. The evidence implies a failure of ROCL for binary choice when one embeds these choices in a payment protocol that assumes a further level of compounding. In effect, ROCL survives when there is only one level of compounding (in the 1-in-1 treatment), and fails when there is “double compounding” (in the 1-in-40 treatment).

### *B. Estimated Risk Preferences*

We estimate risk preferences assuming a RDU model of decision-making under risk. The key issue is whether the structural risk parameters of the model differ when subjects evaluate simple lotteries or compound lotteries.

Assuming a Constant Relative Risk Aversion (CRRA) utility function, we allow the CRRA parameter  $r$  for simple lotteries and the parameter  $(r + rc)$  for compound lotteries, where  $rc$  captures the additive effect of evaluating a compound lottery. Hence the decision maker employs the utility function

$$\begin{aligned} U(x \mid \text{simple lottery}) &= x^{(1-r)}/(1-r) && (1) \\ U(x \mid \text{compound lottery}) &= x^{(1-r-rc)}/(1-r-rc) && (1') \end{aligned}$$

The RDU model extends the EUT model by allowing for decision weights on lottery outcomes. Decision weights are calculated for each lottery outcome, using differences between rank-ordered cumulative probabilities generated from a probability weighting function. We adopt the simple “power”



probability weighting function proposed by Quiggin [1982], with curvature parameter  $\gamma$  for simple lotteries and  $\gamma+\gamma c$  for compound lotteries:

$$\begin{aligned}\omega(p \mid \text{simple lottery}) &= p^\gamma & (2) \\ \omega(p \mid \text{compound lottery}) &= p^{\gamma+\gamma c} & (2')\end{aligned}$$

EUT is the special case in which  $\gamma = \gamma + \gamma c = 1$ . Under RDU the hypothesis of source-independence, which is consistent with ROCL, is that  $\gamma c = 0$  and  $rc = 0$ . We also consider the inverse-S probability weighting function given by:

$$\begin{aligned}\omega(p \mid \text{simple lottery}) &= p^\gamma / (p^\gamma + (1-p)^\gamma)^{1/\gamma} & (3) \\ \omega(p \mid \text{compound lottery}) &= p^{\gamma+\gamma c} / (p^{\gamma+\gamma c} + (1-p)^{\gamma+\gamma c})^{1/(\gamma+\gamma c)} & (3')\end{aligned}$$

We undertake non-nested specification tests to evaluate which probability weighting function is the best.

Specifying preferences in this manner provides us with a structural test for ROCL under an EUT source-dependent model, since the EUT source-dependent model is a special case of the RDU source-dependent model when there is no probability weighting. In this case, if  $rc = 0$  then compound lotteries are evaluated identically to simple lotteries, which is consistent with ROCL. However, if  $rc \neq 0$ , as conjectured by Smith [1969] for objective and subjective compound lotteries, then decision-makers violate ROCL in a certain source-dependent manner, where the “source” here is whether the lottery is simple or compound.<sup>11</sup> As stressed by Smith [1969],  $rc \neq 0$  for *subjective* lotteries provides a direct explanation for the Ellsberg Paradox, but is much more readily tested on the domain of objective lotteries. Of course, the linear specification  $r + rc$  is a parametric convenience, but the obvious one to examine initially.

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<sup>11</sup> Abdellaoui, Baillon, Placido and Wakker [2011] conclude that different probability weighting functions are used when subjects face risky processes with known probabilities and uncertain processes with subjective processes. They call this “source dependence,” where the notion of a source is relatively easy to identify in the context of an artefactual laboratory experiment, and hence provides the tightest test of this proposition. Harrison [2011] shows that their conclusions are an artefact of estimation procedures that do not take account of sampling errors. A correct statistical analysis that does account for sampling errors provides no evidence for source dependence using their data. Of course, failure to reject a null hypothesis could just be due to samples that are too small.

One of the reasons for wanting to estimate a structural econometric model is to have some controls for heterogeneity of preferences. We include the effects of allowing a series of binary demographic variables on a linear specification for each structural parameter: **female** is 1 for women, and 0 otherwise; **senior** is 1 for whether that was the current stage of undergraduate education, and 0 otherwise; **white** is 1 based on self-reported ethnic status; and **gpaHI** is 1 for those reporting a cumulative GPA between 3.25 and 4.0 (at least half A's and B's), and 0 otherwise. Thus the structural parameters  $r$ ,  $r_c$ ,  $\gamma$  and  $\gamma_c$  are each estimated as a linear function of a constant and these observable characteristics.

The complete econometric model is otherwise conventional, and written out in detail in Appendix E.

The results are consistent with those obtained from the choice patterns. In general we find that the EUT model is rejected in favor of the RDU model, whether one allows source-dependence or not. For that reason we will focus our evaluation of hypotheses on the RDU source-dependent model, with allowance for observable heterogeneity of risk preferences.<sup>12</sup> We can reject the hypothesis of source-dependence with the 1-in-1 data, hence we again find support for ROCL in that setting. Figure 6 shows the estimated point estimates for utility and probability weighting functions, conditional on either the Power or Inverse-S specifications, with both performing comparably from an explanatory perspective. Formal hypothesis tests do *not* allow us to reject the hypothesis of the same risk preferences for simple and compound lotteries, with  $p$ -values of 0.72 and 0.95 for the Power and Inverse-S specifications respectively.<sup>13</sup>

However, when we turn to the 1-in-40 data we estimate very different risk preferences for

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<sup>12</sup> One noteworthy result is that if we incorrectly assumed an EUT source-dependent model we would *reject* the ROCL assumption in the 1-in-1 treatment, with a  $p$ -value of 0.008. Of course, that assumption is invalid, demonstrating the importance of finding the correct specification of decision-making under risk.

<sup>13</sup> We draw the same qualitative conclusion if we assume homogeneous preferences, with  $p$ -values of 0.28 and 0.76 respectively.

simple and compound lotteries. Figure 7 shows the estimated point estimates, and these are statistically significantly different with  $p$ -values less than 0.001. These estimates imply that an average subject exhibits different diminishing marginal utility and different probability weighting depending on whether he is evaluating a simple versus compound lottery. To give an order of magnitude to this difference, consider the AE-C pair #39. Assume a hypothetical subject characterized by the average parameters estimated for the source-dependent RDU model.<sup>14</sup> Assuming the Power (Inverse-S) probability weighting function, this subject would attach a certainty equivalent of \$29.6 (\$31.3) to the C lottery and of \$34 (\$37.1) to its corresponding AE lottery, a 14.7% (18.5%) difference.<sup>15</sup>

Finally, we undertake 500 bootstrap simulations from the 1-in-40 data to check if using just one observation from the 40 choices of each subject makes any difference to our conclusions. As explained earlier, each bootstrap simulation draws one choice at random from the 40 that each subject in the 1-in-40 treatment made, estimates the implied test of ROCL using those 62 selected choices, and calculates the  $p$ -value for the test of ROCL. We use the RDU model for this purpose, since it generalizes the EUT model and is a better characterization for these data. Figure 8 collects the resulting bootstrap distributions, and confirms that our conclusions are not an artefact of different sample sizes in the 1-in-1 and 1-in-40 treatments.

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<sup>14</sup> In these calculations we use the estimates of the homogenous preferences specification described in Appendix E. The results are virtually the same if we used the heterogeneous preferences specification and the unconditional average of utility and weighting function parameter estimates.

<sup>15</sup> The certainty equivalents are calculated, as usual, as the certain amount of money that makes an individual indifferent between receiving this certain amount and playing the lottery. We evaluate these certainty equivalents using the utility function for simple lotteries, since a sure amount of money is a simple single-stage lottery with no risk. Finally, the RDU of the AE and the C lotteries are estimated by using the utility and probability weighting functions for simple lotteries and for compound lotteries, respectively.

## 5. Conclusions

Because of the attention paid to violations of the Independence Axiom, it is noteworthy that early formal concerns with the possibility of a “utility or disutility for gambling” centered around the Reduction of Compound Lotteries (ROCL) axiom.<sup>16</sup> Von Neumann and Morgenstern [1953, p. 28] commented on the possibility of allowing for a (dis)utility of gambling component in their preference representation:

Do not our postulates introduce, in some oblique way, the hypotheses which bring in the mathematical expectation [of utility]? More specifically: May there not exist in an individual a (positive or negative) utility of the mere act of ‘taking a chance,’ of gambling, which the use of the mathematical expectation obliterates? How did our axioms (3:A)-(3:C) get around this possibility? As far as we can see, our postulates (3:A)-(3:C) do not attempt to avoid it. Even the one that gets closest to excluding the ‘utility of gambling’ - (3:C:b) - seems to be plausible and legitimate - unless a much more refined system of psychology is used than the one now available for the purposes of economics [...] Since (3:A)-(3:C) secure that the necessary construction [of utility] can be carried out, concepts like a ‘specific utility of gambling’ cannot be formulated free of contradiction on this level.

On the very last page of their *magnus opus*, von Neumann and Morgenstern [1953; p. 632] propose that if their postulate (3:C:b), which is the ROCL, is relaxed, one could indeed allow for a specific utility for the act of gambling:

It seems probable, that the really critical group of axioms is (3:C) - or, more specifically, the axiom (3:C:b). This axiom expresses the combination rule for multiple chance alternatives, and it is plausible, that a specific utility or disutility of gambling can only exist if this simple combination rule is abandoned. Some change of the system [of axioms] (3:A)-(3:B), at any rate involving the abandonment or at least a radical modification of (3:C:b), may perhaps lead to a mathematically complete and satisfactory calculus of utilities which allows for the possibility of a specific utility or disutility of gambling. It is hoped that a way will be found to achieve this, but the mathematical

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<sup>16</sup> The issue of the (dis)utility of gambling goes back at least as far as Pascal, who argued in his *Pensées* that “people distinguish between the pleasure or displeasure of chance (uncertainty) and the objective evaluation of the worth of the gamble from the perspective of its consequences” (see Luce and Marley [2000; p. 102]). Referring to the ability of bets to elicit beliefs, Ramsey [1926] claims that “[t]his method I regard as fundamentally sound; but it suffers from being insufficiently general, and from being necessarily inexact. It is inexact partly [...] because the person may have a special eagerness or reluctance to bet, because he either enjoys or dislikes excitement or for any other reason, e.g. to make a book. The difficulty is like that of separating two different cooperating forces” (from the reprint in Kyburg and Smokler [1964; p. 73]).

difficulties seem to be considerable.

Thus, the relaxation of ROCL opens the door to the possibility of having a distinct (dis)utility for the act of gambling on compound lotteries with objective probabilities. This implies that people would have preferences over compound lotteries that differ from preferences over single-stage lotteries.

Our primary goal is to test this hypothesis for objective probabilities. Our conclusions are influenced by the experiment payment protocols used and the assumptions about how to characterize risk attitudes and heterogeneity across subjects. We find evidence of violations of ROCL, but only when subjects are presented with “double compounding” choices in which the binary choices involve compound lotteries *and* the payment protocol is itself a compound lottery. When subjects are only presented with one binary choice, and there is no additional compounding required by the payment protocol, behavior is consistent with ROCL. These results are obtained consistently whether we use non-parametric statistics to analyze choice patterns or structural econometrics to estimate preferences.

We do not test any specific theories to explain why ROCL does well in one setting compared to the other. Several can be conjectured. One is that subjects in the 1-in-K environment see themselves as facing one foreground risk that is well specified at choice  $k$ , and view the remaining  $K-k$  choices as akin to a “background risk.” This is plausible since they do not know the specific risks, but could guess at their general form. Even in the case of zero-mean background risks, positive and negative effects on foreground risk aversion can be predicted (Eeckhoudt, Gollier and Schlesinger [1996], Gollier and Pratt [1996] and Quiggin [2003]). Another possible explanation is that subjects are attempting to form a portfolio in the 1-in-K case, whereas this is not possible by construction in the 1-in-1 case. Again, this explanation is complicated by the fact that subjects typically do not know the specific lotteries to come. Finally, there could just be “learning effects” over time as subjects gain experience, or exhibit fatigue, with evaluating lotteries; or simply, that there is a threshold level of compounding above which subjects have trouble satisfying ROCL due to a cognitive inability to reduce compound probabilities.

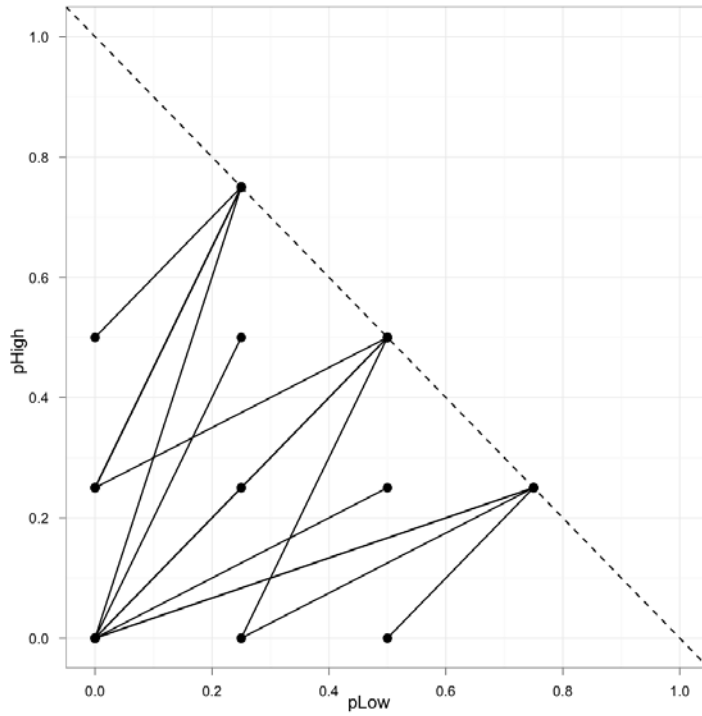
These explanations, and others, could be examined with extended designs. For example, one could test other values of  $K$ . We considered  $K=40$  since this is a plausible level for studies estimating risk attitudes and testing the axioms of EUT, yet in different settings a smaller or larger  $K$  is of interest.<sup>17</sup>

ROCL is central to the evaluation of behavior towards risk, uncertainty and ambiguity. We present experimental evidence on the validity of ROCL in a specific domain defined over objective probabilities. We caution against any experimental evaluation of ROCL over subjective beliefs that assumes no interaction with the payment protocol.

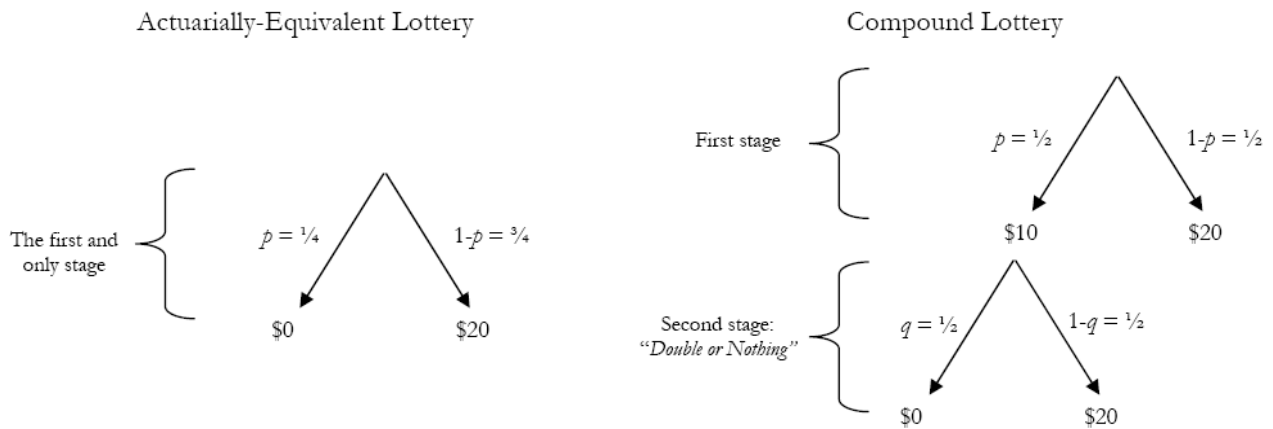
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<sup>17</sup> For instance, the popular Holt and Laury [2002] method for eliciting risk attitudes uses  $K=10$ . Harrison and Swarthout [2012] show that behavior over 1-in-2 choices differs from behavior over 1-in-1 choices, although they did not test the interaction of the ROCL axiom with those payment protocols.

**Figure 1: Probability Coverage of Battery of 40 Lotteries Pairs**



**Figure 2: Tree Representation of a Compound Lottery and its Corresponding Actuarially-Equivalent Simple Lottery**



**Table 1: Experimental Design**

Treatment	Subjects	Choices
1. Pay-1-in-1	133	133
2. Pay-1-in-40	62	2480

**Figure 3: Choices Over Compound and Actuarially-Equivalent Lotteries**

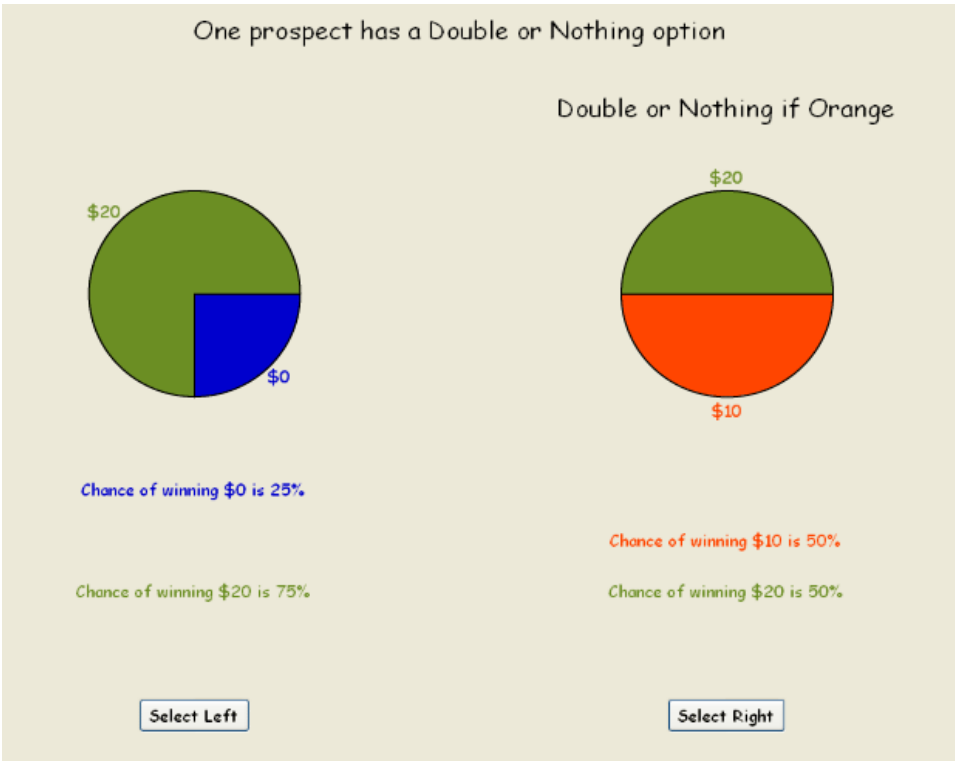




Figure 4: Choices Over Simple and Compound Lotteries

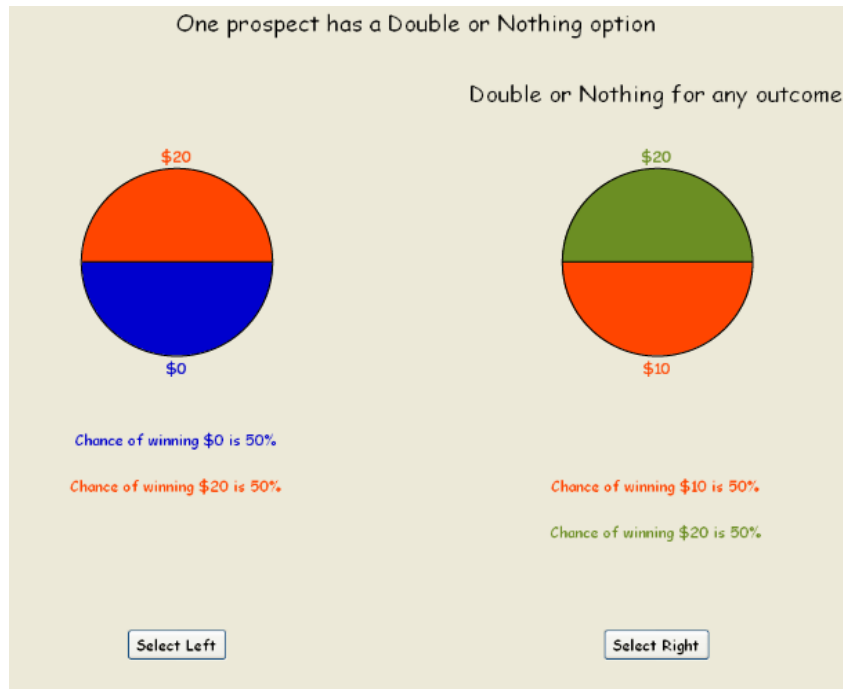


Figure 5: Choices Over Simple and Actuarially-Equivalent Lotteries

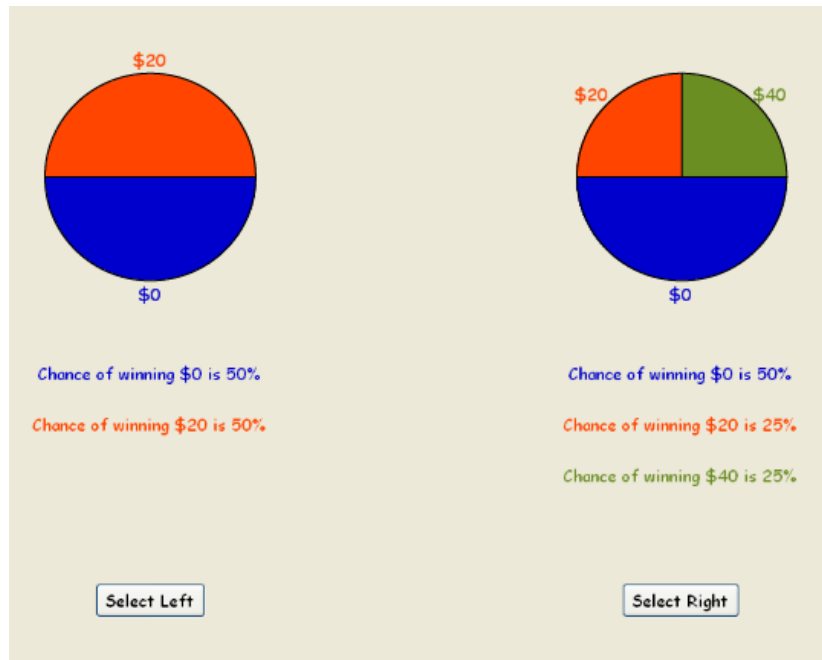


Figure 6: Estimated RDU Models for 1-in-1 Data

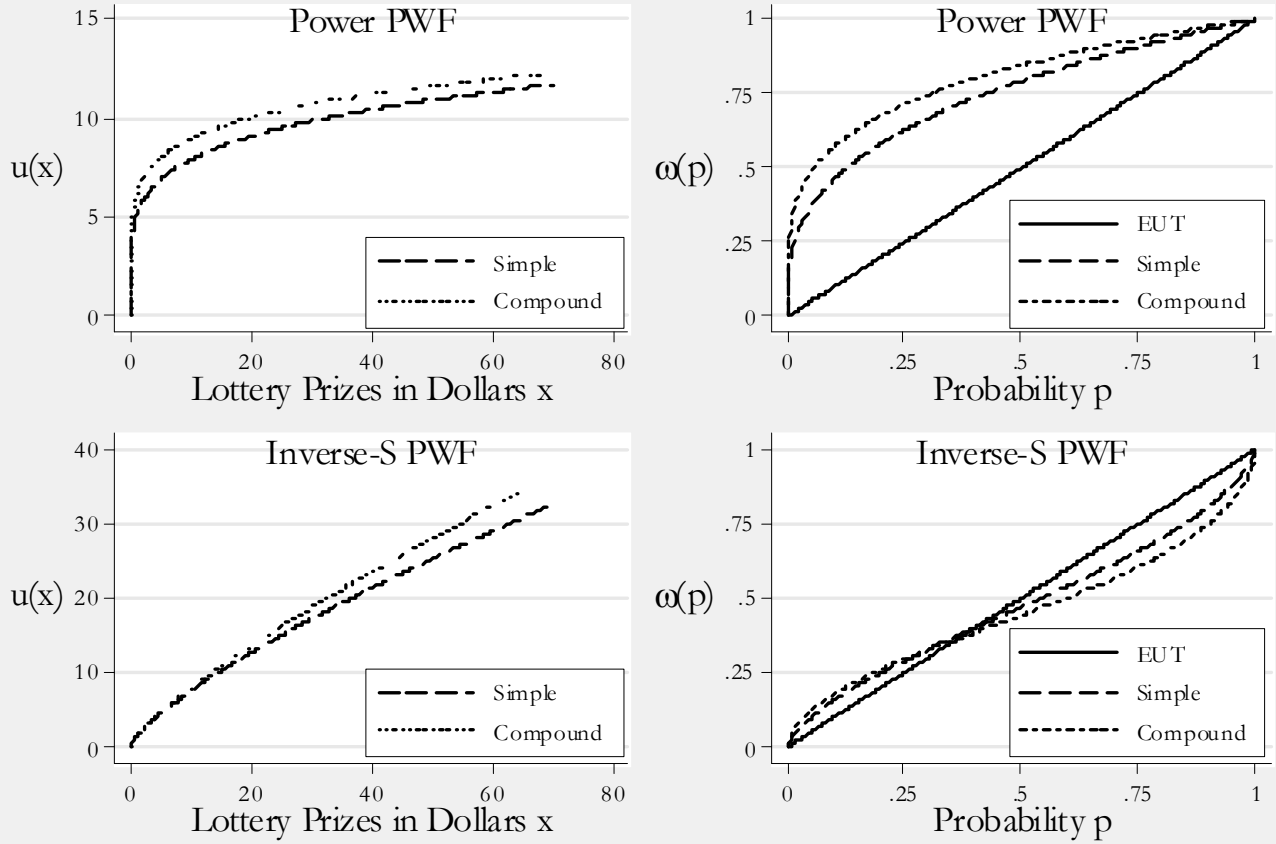
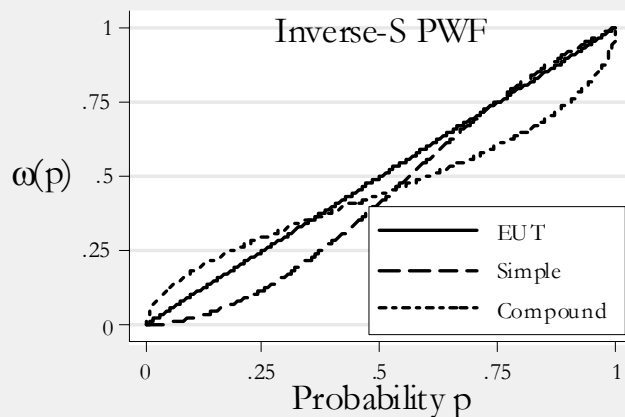
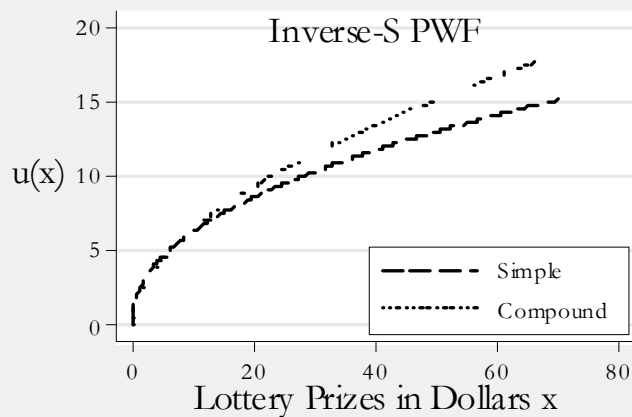
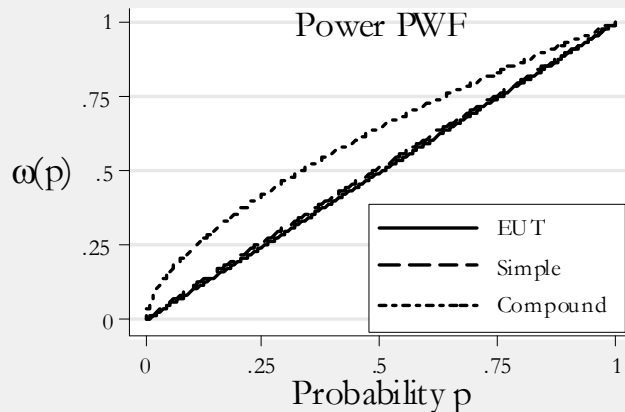
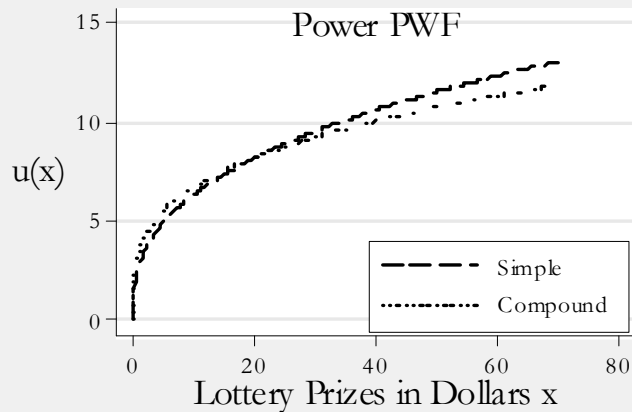
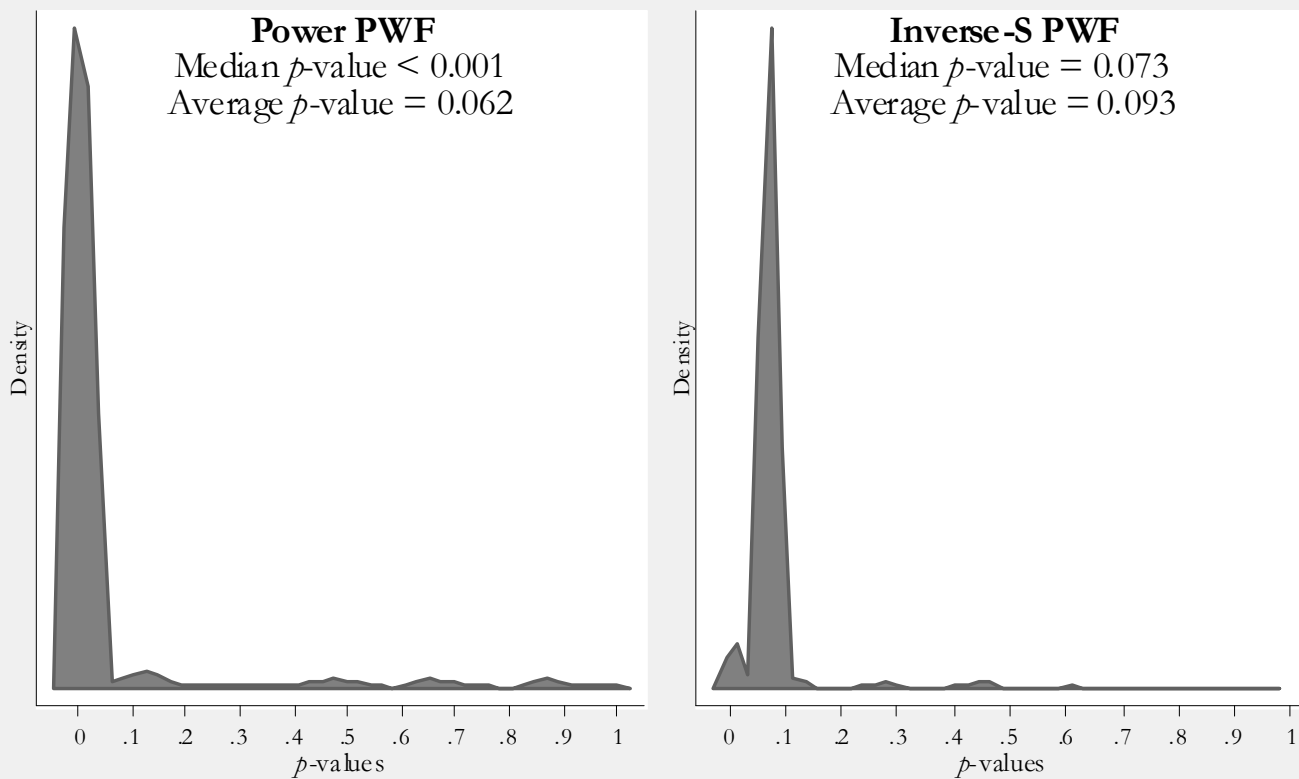


Figure 7: Estimated RDU Models for 1-in-40 Data



# Figure 8: Bootstrap Evaluation of Different Sample Sizes

500 bootstrap evaluations using the RDU  
source-dependent model assuming homogeneous preferences



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## Appendix A: Parameters

To construct our battery of 40 lottery pairs, we used several criteria to choose the compound lotteries and their actuarially-equivalent lotteries used in our experiments:

1. The lottery compounding task should be as simple as possible. The instructions used by Halevy [2007] are a model in this respect, with careful picture illustrations of the manner in which the stages would be drawn. We wanted to avoid having physical displays, since we had many lotteries. We also wanted to be able to have the computer interface vary the order for us on a between-subject basis, so we opted for a simpler procedure that was as comparable as possible in terms of information as our simple lottery choice interface.
2. The lottery pairs should offer reasonable coverage of the Marschak-Machina (MM) triangle and prizes.
3. There should be choices/chords that assume parallel indifference curves, as expected under EUT, but the slope of the indifference curve should vary, so that the battery of lotteries can be used to test for a wide range of risk attitudes under the EUT null hypothesis (this criteria was employed for the construction of the basic 69 simple lotteries).
4. There should be a number of compound lotteries with their actuarially-equivalent counterparts in the interior of the triangle. Experimental evidence suggests that people tend to comply with the implications of EUT in the interior of the triangle and to violate it on the borders (Conlisk [1989], Camerer [1992], Harless [1992], Gigliotti and Sopher [1993] and Starmer [2000]).
5. We were careful to choose lottery pairs with stakes and expected payoff per individual that are comparable to those in the original battery of 69 simple lotteries, since these had been used extensively in other samples from this population.

Our starting point was the battery of 69 lotteries in Table A1 used in Harrison and Swarthout [2012], which in turn were derived from Wilcox [2010]. The lotteries were originally designed in part to satisfy the second and third criteria given above. Our strategy was then to “reverse engineer” the initial lotteries needed to obtain compound lotteries that would yield actuarially-equivalent prospects which already existed in the set of 69 pairs. For instance, the first pair in our battery of 40 lotteries was derived from pair 4 in the battery of 69 (contrast pair 1 in Table A2 with pair 4 in Table A1). We want the distribution of the “risky” lottery in the latter pair to be the actuarially-equivalent prospect of our compound lottery. To achieve this, we have an initial lottery that pays \$10 and \$0 with 50% probability each, and offering “Double or Nothing” if the outcome of the latter prospect is \$10. Hence it offers equal chances of \$20 or \$0 if the DON stage is reached. The \$5 stake was changed to \$0 because DON requires this prize to be among the possible outcomes of the compound lotteries.<sup>18</sup> The actuarially-equivalent lottery of this compound prospect pays \$0 with 75% probability and \$20 with 25% probability, which is precisely the risky lottery in pair 4 of the default battery of 69 pairs. Except for the compound lottery in pair 10 in our set of lotteries, the actuarially-equivalent lotteries play the role of the “risky” lotteries.

Figure A1 shows the coverage of these lottery pairs in terms of the Marschak-Machina

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<sup>18</sup> We contemplated using “double or \$5,” but this did not have the familiarity of DON.

triangle. Each prize context defines a different triangle, but the patterns of choice overlap considerably. Figure A1 shows that there are many choices/chords that assume parallel indifference curves, as expected under EUT, but that the slope of the indifference curve can vary, so that the tests of EUT have reasonable power for a wide range of risk attitudes under the EUT null hypothesis (Loomes and Sugden [1998] and Harrison, Johnson, McInnes and Rutström [2007]). These lotteries also contain a number of pairs in which the “EUT-safe” lottery has a *higher* EV than the “EUT-risky” lottery: this is designed deliberately to evaluate the extent of risk premia deriving from probability pessimism rather than diminishing marginal utility.

The majority of our compound lotteries use a conditional version of the DON device because it allows to obtain good coverage of prizes and probabilities and keeps the compounding representation simple. As noted in the text, one can construct diverse compound lotteries with only two simple components: initial lotteries that either pay two outcomes with 50:50 odds or pay a given stake with certainty, and a conditional DON which pays double a predetermined amount with 50% probability or nothing with equal chance.

In our design, if the subject has to play the DON option she will toss a coin to decide if she gets double the stated amount. One could use randomization devices that allow for probability distributions different from these 50:50 odds, but we want to keep the lottery compounding simple and familiar. Therefore, if one commits to 50:50 odds in the DON option, using exclusively unconditional DON will only allow one to generate compound lotteries with actuarially-equivalent prospects that assign 50% chance to getting nothing. For instance, suppose a compound prospect with an initial lottery that pays positive amounts  $\$X$  and  $\$Y$  with probability  $p$  and  $(1-p)$ , respectively, and offers DON for any outcome. The corresponding actuarially-equivalent lottery pays  $\$2X$ ,  $\$2Y$  and  $\$0$  with probabilities  $p/2$ ,  $(1-p)/2$  and  $1/2$ , respectively.

The original 69 pairs use 10 contexts defined by three outcomes drawn from  $\$5$ ,  $\$10$ ,  $\$20$ ,  $\$35$  and  $\$70$ . For example, the first context consists of prospects defined over prizes  $\$5$ ,  $\$10$  and  $\$20$ , and the tenth context consists of lotteries defined over stakes  $\$20$ ,  $\$35$  and  $\$70$ . As a result of using the DON device, we have to introduce  $\$0$  to the set of stakes from which the contexts are drawn. However, some of the initial lotteries used prizes in contexts different from the ones used for final prizes, so that we could ensure that the stakes for the compounded lottery matched those of the simple lotteries. For example, pair 3 in Table A2 is defined over a context with stakes  $\$0$ ,  $\$10$  and  $\$35$ . The compound lottery of this pair offers an initial lottery that pays  $\$5$  and  $\$17.50$  with 50% chance each and a DON option for any outcome. This allows us to have as final prizes  $\$0$ ,  $\$10$  and  $\$35$ .

Our battery of 40 lotteries uses 6 of the original 10 contexts, but substitute the  $\$5$  stake for  $\$0$ . We do not use the other 4 contexts: for them to be distinct from our 6 contexts they would have to have 4 outcomes, the original 3 outcomes plus the  $\$0$  stake required by the DON option. We chose to use only compound lotteries with no more than 3 final outcomes, which in turn requires initial lotteries with no more than 2 outcomes. Accordingly, the initial lotteries of compound prospects are defined over distributions that offer either 50:50 odds of getting any of 2 outcomes or certainty of getting a particular outcome which makes our design simple. It is worth noting that there are compound lotteries composed of initial prospects that offer an amount  $\$X$  with 100% probability and a DON option that pays  $\$2X$  and  $\$0$  with 50% chance each (see pairs 5, 6 and 14 in



Table A2 and pairs 34 and 40 in Table A4). By including this type of “trivial” compound lottery, we provide the basis for ROCL to be tested in its simplest form.

Finally, we included compound lotteries with actuarially-equivalent counterparts in the interior and on the border of the MM triangle, since previous experimental evidence suggests that this is relevant to test the implications of EUT. Pairs 3, 7, 10, 11, 32, 35 and 38 have compound lotteries with their actuarially-equivalent lotteries in the interior of the triangle.

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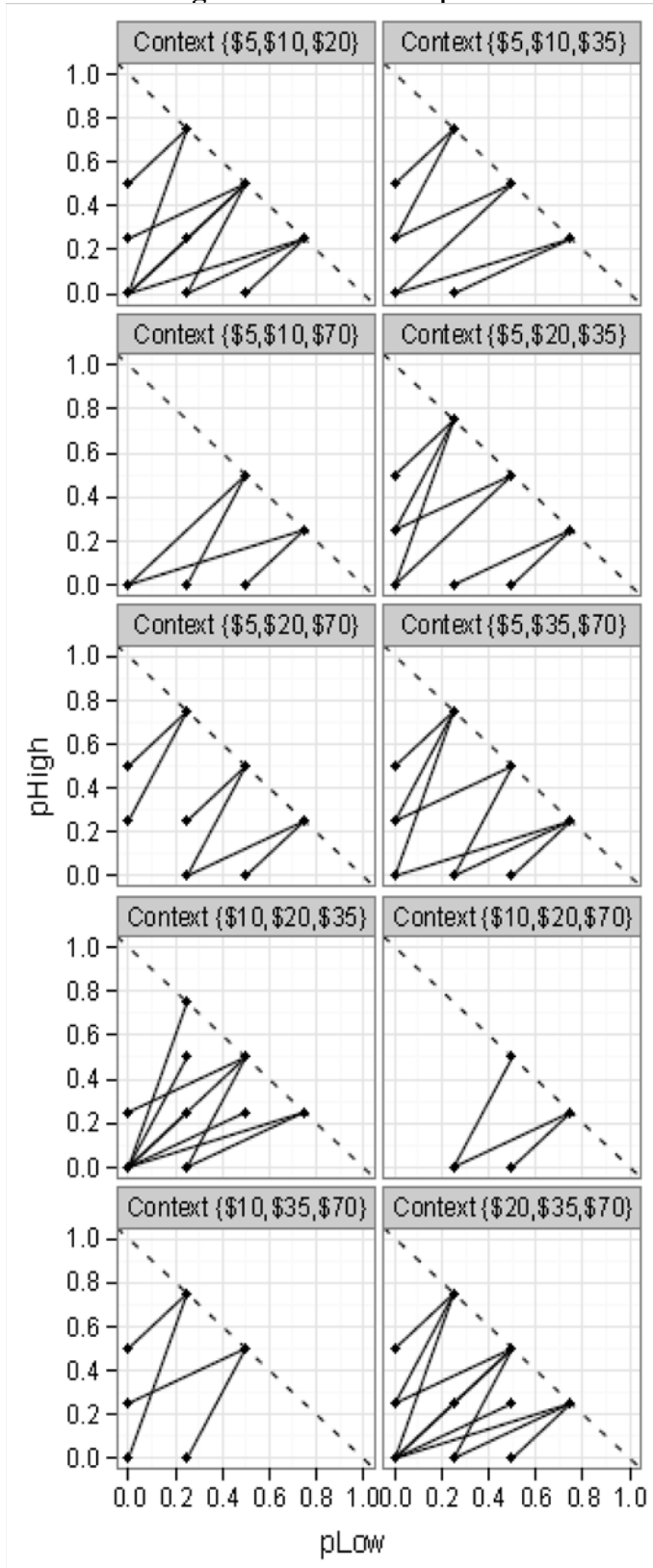
**Table A1: Default Simple Lotteries**

Pair	Context	Prizes			“Safe” Lottery Probabilities			“Risky” Lottery Probabilities			EV Safe	EV Risky
		Low	Middle	High	Low	Middle	High	Low	Middle	High		
1	1	\$5	\$10	\$20	0	1	0	0.25	0	0.75	\$10.00	\$16.25
2	1	\$5	\$10	\$20	0.25	0.75	0	0.5	0	0.5	\$8.75	\$12.50
3	1	\$5	\$10	\$20	0	1	0	0.5	0	0.5	\$10.00	\$12.50
4	1	\$5	\$10	\$20	0.5	0.5	0	0.75	0	0.25	\$7.50	\$8.75
5	1	\$5	\$10	\$20	0	1	0	0.25	0.5	0.25	\$10.00	\$11.25
6	1	\$5	\$10	\$20	0.25	0.5	0.25	0.5	0	0.5	\$11.25	\$12.50
7	1	\$5	\$10	\$20	0	0.5	0.5	0.25	0	0.75	\$15.00	\$16.25
8	1	\$5	\$10	\$20	0	0.75	0.25	0.5	0	0.5	\$12.50	\$12.50
9	1	\$5	\$10	\$20	0.25	0.75	0	0.75	0	0.25	\$8.75	\$8.75
10	1	\$5	\$10	\$20	0	1	0	0.75	0	0.25	\$10.00	\$8.75
11	2	\$5	\$10	\$35	0	1	0	0.5	0	0.5	\$10.00	\$20.00
12	2	\$5	\$10	\$35	0	0.75	0.25	0.25	0	0.75	\$16.25	\$27.50
13	2	\$5	\$10	\$35	0.25	0.75	0	0.75	0	0.25	\$8.75	\$12.50
14	2	\$5	\$10	\$35	0	0.5	0.5	0.25	0	0.75	\$22.50	\$27.50
15	2	\$5	\$10	\$35	0	0.75	0.25	0.5	0	0.5	\$16.25	\$20.00
16	2	\$5	\$10	\$35	0	1	0	0.75	0	0.25	\$10.00	\$12.50
17	3	\$5	\$10	\$70	0.25	0.75	0	0.5	0	0.5	\$8.75	\$37.50
18	3	\$5	\$10	\$70	0	1	0	0.5	0	0.5	\$10.00	\$37.50
19	3	\$5	\$10	\$70	0.5	0.5	0	0.75	0	0.25	\$7.50	\$21.25
20	3	\$5	\$10	\$70	0	1	0	0.75	0	0.25	\$10.00	\$21.25
21	4	\$5	\$20	\$35	0	1	0	0.25	0	0.75	\$20.00	\$27.50
22	4	\$5	\$20	\$35	0	0.75	0.25	0.25	0	0.75	\$23.75	\$27.50
23	4	\$5	\$20	\$35	0	0.5	0.5	0.25	0	0.75	\$27.50	\$27.50

24	4	\$5	\$20	\$35	0	1	0	0.5	0	0.5	\$20.00	\$20.00
25	4	\$5	\$20	\$35	0.5	0.5	0	0.75	0	0.25	\$12.50	\$12.50
26	4	\$5	\$20	\$35	0	0.75	0.25	0.5	0	0.5	\$23.75	\$20.00
27	4	\$5	\$20	\$35	0.25	0.75	0	0.75	0	0.25	\$16.25	\$12.50
28	5	\$5	\$20	\$70	0.25	0.75	0	0.5	0	0.5	\$16.25	\$37.50
29	5	\$5	\$20	\$70	0	0.75	0.25	0.25	0	0.75	\$32.50	\$53.75
30	5	\$5	\$20	\$70	0.5	0.5	0	0.75	0	0.25	\$12.50	\$21.25
31	5	\$5	\$20	\$70	0.25	0.5	0.25	0.5	0	0.5	\$28.75	\$37.50
32	5	\$5	\$20	\$70	0.25	0.75	0	0.75	0	0.25	\$16.25	\$21.25
33	5	\$5	\$20	\$70	0	0.5	0.5	0.25	0	0.75	\$45.00	\$53.75
34	6	\$5	\$35	\$70	0	1	0	0.25	0	0.75	\$35.00	\$53.75
35	6	\$5	\$35	\$70	0.25	0.75	0	0.5	0	0.5	\$27.50	\$37.50
36	6	\$5	\$35	\$70	0	0.75	0.25	0.25	0	0.75	\$43.75	\$53.75
37	6	\$5	\$35	\$70	0.5	0.5	0	0.75	0	0.25	\$20.00	\$21.25
38	6	\$5	\$35	\$70	0	0.5	0.5	0.25	0	0.75	\$52.50	\$53.75
39	6	\$5	\$35	\$70	0	0.75	0.25	0.5	0	0.5	\$43.75	\$37.50
40	6	\$5	\$35	\$70	0.25	0.75	0	0.75	0	0.25	\$27.50	\$21.25
41	6	\$5	\$35	\$70	0	1	0	0.75	0	0.25	\$35.00	\$21.25
42	7	\$10	\$20	\$35	0	1	0	0.25	0	0.75	\$20.00	\$28.75
43	7	\$10	\$20	\$35	0.25	0.75	0	0.5	0	0.5	\$17.50	\$22.50
44	7	\$10	\$20	\$35	0	1	0	0.25	0.25	0.5	\$20.00	\$25.00
45	7	\$10	\$20	\$35	0	1	0	0.5	0	0.5	\$20.00	\$22.50
46	7	\$10	\$20	\$35	0	1	0	0.25	0.5	0.25	\$20.00	\$21.25
47	7	\$10	\$20	\$35	0	0.75	0.25	0.5	0	0.5	\$23.75	\$22.50
48	7	\$10	\$20	\$35	0	1	0	0.5	0.25	0.25	\$20.00	\$18.75
49	7	\$10	\$20	\$35	0.25	0.75	0	0.75	0	0.25	\$17.50	\$16.25
50	7	\$10	\$20	\$35	0	1	0	0.75	0	0.25	\$20.00	\$16.25

51	8	\$10	\$20	\$70	0.25	0.75	0	0.5	0	0.5	\$17.50	\$40.00
52	8	\$10	\$20	\$70	0.5	0.5	0	0.75	0	0.25	\$15.00	\$25.00
53	8	\$10	\$20	\$70	0.25	0.75	0	0.75	0	0.25	\$17.50	\$25.00
54	9	\$10	\$35	\$70	0	1	0	0.25	0	0.75	\$35.00	\$55.00
55	9	\$10	\$35	\$70	0.25	0.75	0	0.5	0	0.5	\$28.75	\$40.00
56	9	\$10	\$35	\$70	0	0.5	0.5	0.25	0	0.75	\$52.50	\$55.00
57	9	\$10	\$35	\$70	0	0.75	0.25	0.5	0	0.5	\$43.75	\$40.00
58	10	\$20	\$35	\$70	0	1	0	0.25	0	0.75	\$35.00	\$57.50
59	10	\$20	\$35	\$70	0.25	0.75	0	0.5	0	0.5	\$31.25	\$45.00
60	10	\$20	\$35	\$70	0	0.75	0.25	0.25	0	0.75	\$43.75	\$57.50
61	10	\$20	\$35	\$70	0	1	0	0.5	0	0.5	\$35.00	\$45.00
62	10	\$20	\$35	\$70	0.5	0.5	0	0.75	0	0.25	\$27.50	\$32.50
63	10	\$20	\$35	\$70	0	1	0	0.25	0.5	0.25	\$35.00	\$40.00
64	10	\$20	\$35	\$70	0.25	0.5	0.25	0.5	0	0.5	\$40.00	\$45.00
65	10	\$20	\$35	\$70	0	0.5	0.5	0.25	0	0.75	\$52.50	\$57.50
66	10	\$20	\$35	\$70	0	1	0	0.5	0.25	0.25	\$35.00	\$36.25
67	10	\$20	\$35	\$70	0.25	0.75	0	0.75	0	0.25	\$31.25	\$32.50
68	10	\$20	\$35	\$70	0	0.75	0.25	0.5	0	0.5	\$43.75	\$45.00
69	10	\$20	\$35	\$70	0	1	0	0.75	0	0.25	\$35.00	\$32.50

Figure A1: Default Simple Lotteries



**Table A2: Simple Lotteries vs. Compound Lotteries (Pairs 1-15)**

Pair	Final Prizes				Simple Lottery			Compound Lottery							EV Simple	EV Compound
	Context	Low	Middle	High	Probabilities			Initial Lottery Prizes			Initial Lottery Probabilities			"Double or Nothing" option		
					Low	Middle	High	Low	Middle	High	Low	Middle	High			
1	1	\$0	\$10	\$20	0.5	0.5	0	\$0	\$10	\$20	0.5	0.5	0	DON if middle	\$5.00	\$5.00
2	1	\$0	\$10	\$20	0	1	0	\$0	\$10	\$20	0.5	0.5	0	DON if middle	\$10.00	\$5.00
3	2	\$0	\$10	\$35	0	1	0	\$0	\$5	\$17.50	0	0.5	0.5	DON for any outcome DON if middle	\$10.00	\$11.25
	2	\$0	\$10	\$35	0.25	0.75	0	\$0	\$17.50	\$35	0.5	0.5	0		\$7.50	\$8.75
5	3	\$0	\$10	\$70	0.25	0.75	0	\$0	\$35	\$70	0	1	0	DON for any outcome	\$7.50	\$35.00
6	3	\$0	\$10	\$70	0	1	0	\$0	\$35	\$70	0	1	0	DON for any outcome	\$10.00	\$35.00
7	4	\$0	\$20	\$35	0	1	0	\$0	\$10	\$35	0	0.5	0.5	DON if middle	\$20.00	\$22.50
8	4	\$0	\$20	\$35	0	0.75	0.25	\$0	\$17.50	\$35	0	0.5	0.5	DON if middle	\$23.75	\$8.75
9	5	\$0	\$20	\$70	0	0.5	0.5	\$0	\$35	\$70	0	0.5	0.5	DON if middle	\$45.00	\$52.50
10	5	\$0	\$20	\$70	0.5	0	0.5	\$0	\$20	\$35	0	0.5	0.5	DON if high	\$35.00	\$27.50
11	5	\$0	\$20	\$70	0	1	0	\$0	\$20	\$35	0	0.5	0.5	DON if high	\$20.00	\$27.50
12	5	\$0	\$20	\$70	0	0.75	0.25	\$0	\$35	\$70	0	0.5	0.5	DON if middle	\$32.50	\$52.50
13	6	\$0	\$35	\$70	0	1	0	\$0	\$35	\$70	0	0.5	0.5	DON if middle	\$35.00	\$52.50
14	6	\$0	\$35	\$70	0	0.75	0.25	\$0	\$35	\$70	0	1	0	DON for any outcome	\$43.75	\$35.00
15	6	\$0	\$35	\$70	0	0.75	0.25	\$0	\$35	\$70	0	0.5	0.5	DON if middle	\$43.75	\$52.50

**Table A3: Simple Lotteries vs. Actuarially-Equivalent Lotteries (Pairs 16-30)**

Pair	Context	Final Prizes			Simple Lottery Probabilities			Actuarially-Equivalent Lottery Probabilities			EV Simple	EV Actuarially-Equivalent
		Low	Middle	High	Low	Middle	High	Low	Middle	High		
16	1	\$0	\$10	\$20	0.5	0.5	0	0.75	0	0.25	\$5.00	\$5.00
17	1	\$0	\$10	\$20	0	1	0	0.75	0	0.25	\$10.00	\$5.00
18	2	\$0	\$10	\$35	0	1	0	0.5	0.25	0.25	\$10.00	\$11.25
19	2	\$0	\$10	\$35	0.25	0.75	0	0.75	0	0.25	\$7.50	\$8.75
20	3	\$0	\$10	\$70	0.25	0.75	0	0.5	0	0.5	\$7.50	\$35.00
21	3	\$0	\$10	\$70	0	1	0	0.5	0	0.5	\$10.00	\$35.00
22	4	\$0	\$20	\$35	0	1	0	0.25	0.25	0.5	\$20.00	\$22.50
23	4	\$0	\$20	\$35	0	0.75	0.25	0.25	0	0.75	\$23.75	\$8.75
24	5	\$0	\$20	\$70	0	0.5	0.5	0.25	0	0.75	\$45.00	\$52.50
25	5	\$0	\$20	\$70	0.5	0	0.5	0.25	0.5	0.25	\$35.00	\$27.50
26	5	\$0	\$20	\$70	0	1	0	0.25	0.5	0.25	\$20.00	\$27.50
27	5	\$0	\$20	\$70	0	0.75	0.25	0.25	0	0.75	\$32.50	\$52.50
28	6	\$0	\$35	\$70	0	1	0	0.25	0	0.75	\$35.00	\$52.50
29	6	\$0	\$35	\$70	0	0.75	0.25	0.5	0	0.5	\$43.75	\$35.00
30	6	\$0	\$35	\$70	0	0.75	0.25	0.25	0	0.75	\$43.75	\$52.50

**Table A4: Actuarially-Equivalent Lotteries vs. Compound Lotteries (Pairs 31-40)**

Pair	Final Prizes				Actuarially-Equivalent Lottery Probabilities			Compound Lottery							EV	EV
	Context	Low	Middle	High	Low	Middle	High	Initial Lottery Prizes			Initial Lottery Probabilities			"Double or Nothing" option	Actuarially-Equivalent	Compound
								Low	Middle	High	Low	Middle	High			
31	1	\$0	\$10	\$20	0.75	0	0.25	\$0	\$10	\$20	0.5	0.5	0	DON if middle	\$5.00	\$5.00
32	2	\$0	\$10	\$35	0.5	0.25	0.25	\$0	\$5	\$17.50	0	0.5	0.5	DON for any outcome	\$11.25	\$11.25
33	2	\$0	\$10	\$35	0.75	0	0.25	\$0	\$17.50	\$35	0.5	0.5	0	DON if middle	\$8.75	\$8.75
34	3	\$0	\$10	\$70	0.5	0	0.5	\$0	\$35	\$70	0	1	0	DON for any outcome	\$35.00	\$35.00
35	4	\$0	\$20	\$35	0.25	0.25	0.5	\$0	\$10	\$35	0	0.5	0.5	DON if middle	\$22.50	\$22.50
36	4	\$0	\$20	\$35	0.25	0	0.75	\$0	\$17.50	\$35	0	0.5	0.5	DON if middle	\$8.75	\$8.75
37	5	\$0	\$20	\$70	0.25	0	0.75	\$0	\$35	\$70	0	0.5	0.5	DON if middle	\$52.50	\$52.50
38	5	\$0	\$20	\$70	0.25	0.5	0.25	\$0	\$20	\$35	0	0.5	0.5	DON if high	\$27.50	\$27.50
39	6	\$0	\$35	\$70	0.25	0	0.75	\$0	\$35	\$70	0	0.5	0.5	DON if middle	\$52.50	\$52.50
40	6	\$0	\$35	\$70	0.5	0	0.5	\$0	\$35	\$70	0	1	0	DON for any outcome	\$35.00	\$35.00



## Appendix B: Instructions (WORKING PAPER)

### B.1. Instructions for Treatment 1-in-1

1p

#### Choices Over Risky Prospects

This is a task where you will choose between prospects with varying prizes and chances of winning. You will be presented with one pair of prospects where you will choose one of them. You should choose the prospect you prefer to play. You will actually get the chance to play the prospect you choose, and you will be paid according to the outcome of that prospect, so you should think carefully about which prospect you prefer.

Here is an example of what the computer display of a pair of prospects might look like.



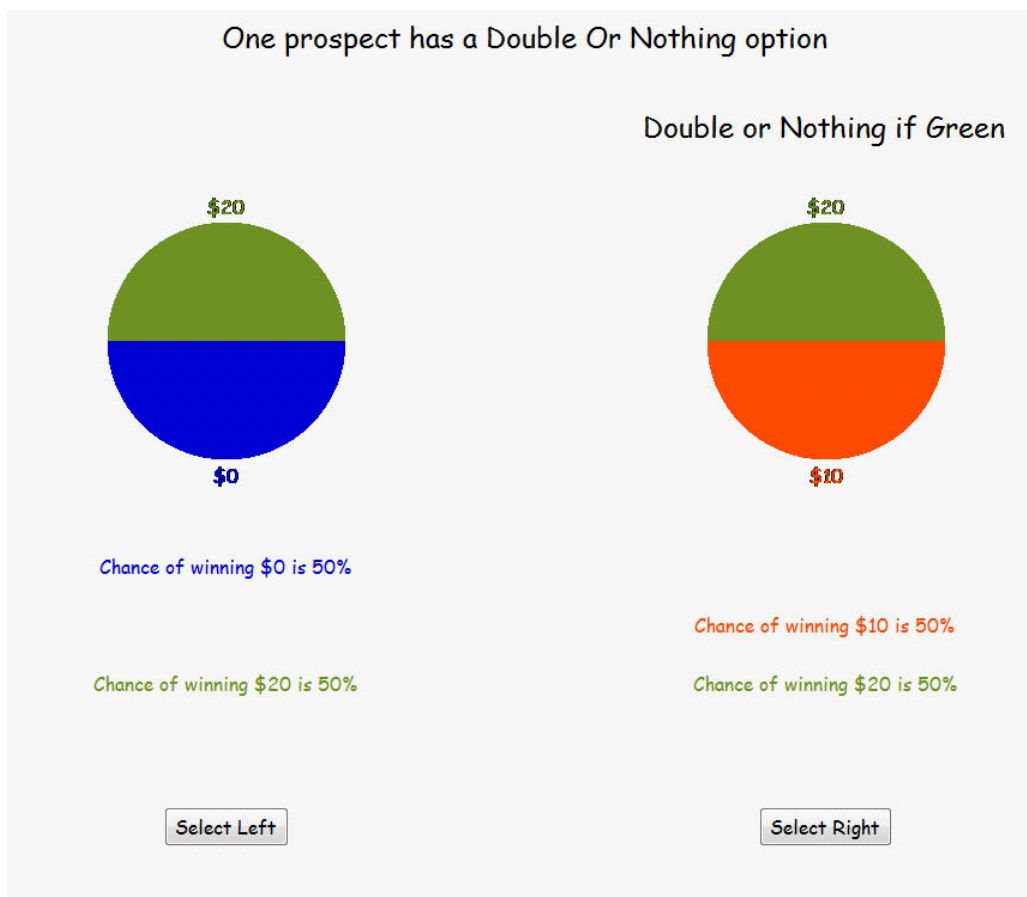
The outcome of the prospects will be determined by the draw of a random number between 1 and 100. Each number between, and including, 1 and 100 is equally likely to occur. In fact, you will be able to draw the number yourself using two 10-sided dice.

In the above example the left prospect pays five dollars (\$5) if the number drawn is between 1 and 40, and pays fifteen dollars (\$15) if the number is between 41 and 100. The blue color in the pie

chart corresponds to 40% of the area and illustrates the chances that the number drawn will be between 1 and 40 and your prize will be \$5. The orange area in the pie chart corresponds to 60% of the area and illustrates the chances that the number drawn will be between 41 and 100 and your prize will be \$15.

Now look at the pie in the chart on the right. It pays five dollars (\$5) if the number drawn is between 1 and 50, ten dollars (\$10) if the number is between 51 and 90, and fifteen dollars (\$15) if the number is between 91 and 100. As with the prospect on the left, the pie slices represent the fraction of the possible numbers which yield each payoff. For example, the size of the \$15 pie slice is 10% of the total pie.

You could also get a pair of prospects in which one of the prospects will give you the chance to play “Double or Nothing.” For instance, the right prospect in the following screen image pays “Double or Nothing” if the Green area is selected, which happens if the number drawn is between 51 and 100. The right pie chart indicates that if the number is between 1 and 50 you get \$10. However, if



the number is between 51 and 100 a coin will be tossed to determine if you get double the amount. If it comes up Heads you get \$40, otherwise you get nothing. The prizes listed underneath each pie refer to the amounts before any “Double or Nothing” coin toss.

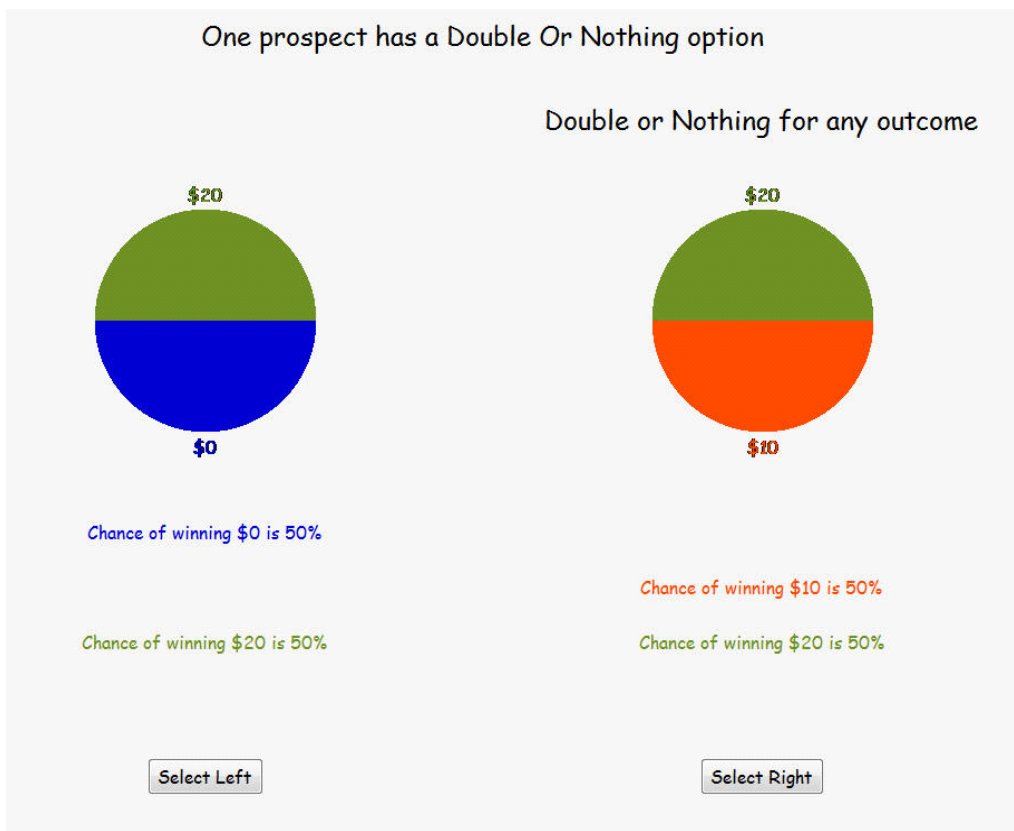
The pair of prospects you choose from is shown on a screen on the computer. On that screen, you should indicate which prospect you prefer to play by clicking on one of the buttons beneath the prospects.

After you have made your choice, raise your hand and an experimenter will come over. It is certain that your one choice will be played out for real. You will roll the two ten-sided dice to determine the outcome of the prospect you chose, and if necessary you will then toss a coin to determine if you get “Double or Nothing.”

For instance, suppose you picked the prospect on the left in the last example. If the random number was 37, you would win \$0; if it was 93, you would get \$20.

If you picked the prospect on the right and drew the number 37, you would get \$10; if it was 93, you would have to toss a coin to determine if you get “Double or Nothing.” If the coin comes up Heads then you get \$40. However, if it comes up Tails you get nothing from your chosen prospect.

It is also possible that you will be given a prospect in which there is a “Double or Nothing” option no matter what the outcome of the random number. The screen image below illustrates this possibility.



Therefore, your payoff is determined by three things:

- by which prospect you selected, the left or the right;
- by the outcome of that prospect when you roll the two 10-sided dice; and
- by the outcome of a coin toss if the chosen prospect outcome is of the “Double or Nothing” type.

Which prospects you prefer is a matter of personal taste. The people next to you may be presented with a different prospect, and may have different preferences, so their responses should not matter to you. Please work silently, and make your choices by thinking carefully about the prospect you are presented with.

All payoffs are in cash, and are in addition to the \$7.50 show-up fee that you receive just for being here. The only other task today is for you to answer some demographic questions. Your answers to those questions will not affect your payoffs.

### Choices Over Risky Prospects

This is a task where you will choose between prospects with varying prizes and chances of winning. You will be presented with a series of pairs of prospects where you will choose one of them. There are 40 pairs in the series. For each pair of prospects, you should choose the prospect you prefer to play. You will actually get the chance to play **one** of the prospects you choose, and you will be paid according to the outcome of that prospect, so you should think carefully about which prospect you prefer.

Here is an example of what the computer display of such a pair of prospects might look like.



The outcome of the prospects will be determined by the draw of a random number between 1 and 100. Each number between, and including, 1 and 100 is equally likely to occur. In fact, you will be able to draw the number yourself using two 10-sided dice.

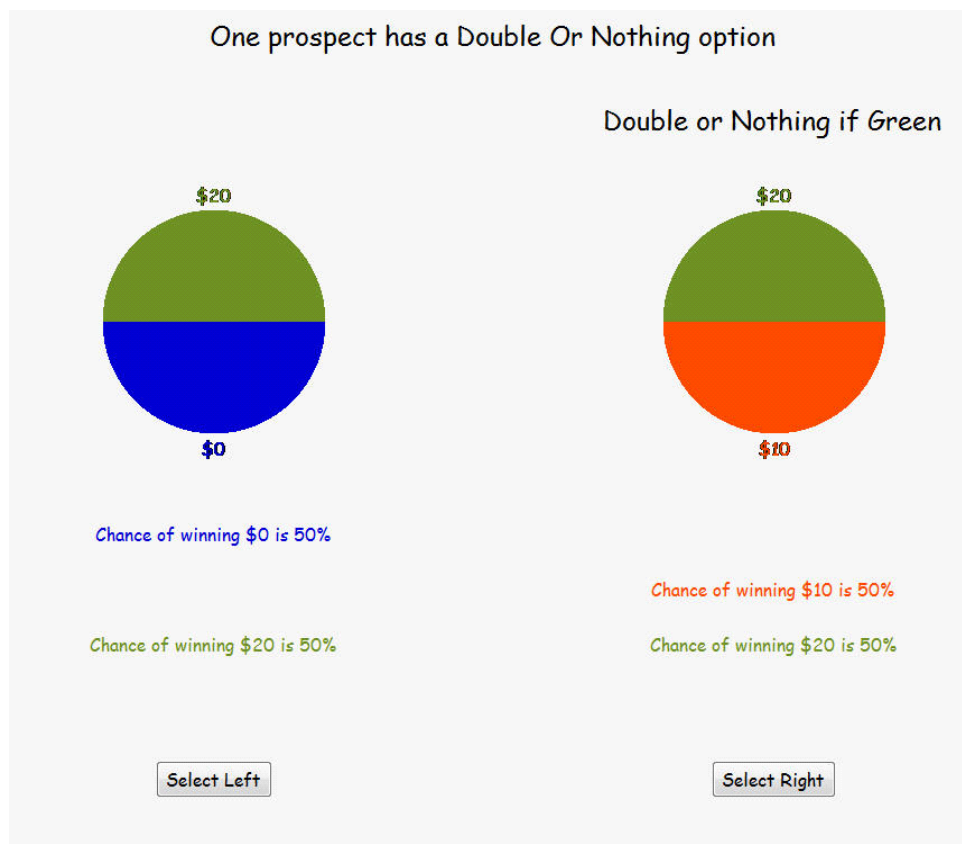
In the above example the left prospect pays five dollars (\$5) if the number drawn is between 1 and 40, and pays fifteen dollars (\$15) if the number is between 41 and 100. The blue color in the pie

chart corresponds to 40% of the area and illustrates the chances that the number drawn will be between 1 and 40 and your prize will be \$5. The orange area in the pie chart corresponds to 60% of the area and illustrates the chances that the number drawn will be between 41 and 100 and your prize will be \$15.

Now look at the pie in the chart on the right. It pays five dollars (\$5) if the number drawn is between 1 and 50, ten dollars (\$10) if the number is between 51 and 90, and fifteen dollars (\$15) if the number is between 91 and 100. As with the prospect on the left, the pie slices represent the fraction of the possible numbers which yield each payoff. For example, the size of the \$15 pie slice is 10% of the total pie.

Each pair of prospects is shown on a separate screen on the computer. On each screen, you should indicate which prospect you prefer to play by clicking on one of the buttons beneath the prospects.

You could also get a pair of prospects in which one of the prospects will give you the chance to play “Double or Nothing.” For instance, the right prospect in the following screen image pays “Double or Nothing” if the Green area is selected, which happens if the number drawn is between 51 and 100. The right pie chart indicates that if the number is between 1 and 50 you get \$10. However, if the number is between 51 and 100 a coin will be tossed to determine if you get double the amount. If it comes up Heads you get \$40, otherwise you get nothing. The prizes listed underneath each pie refer



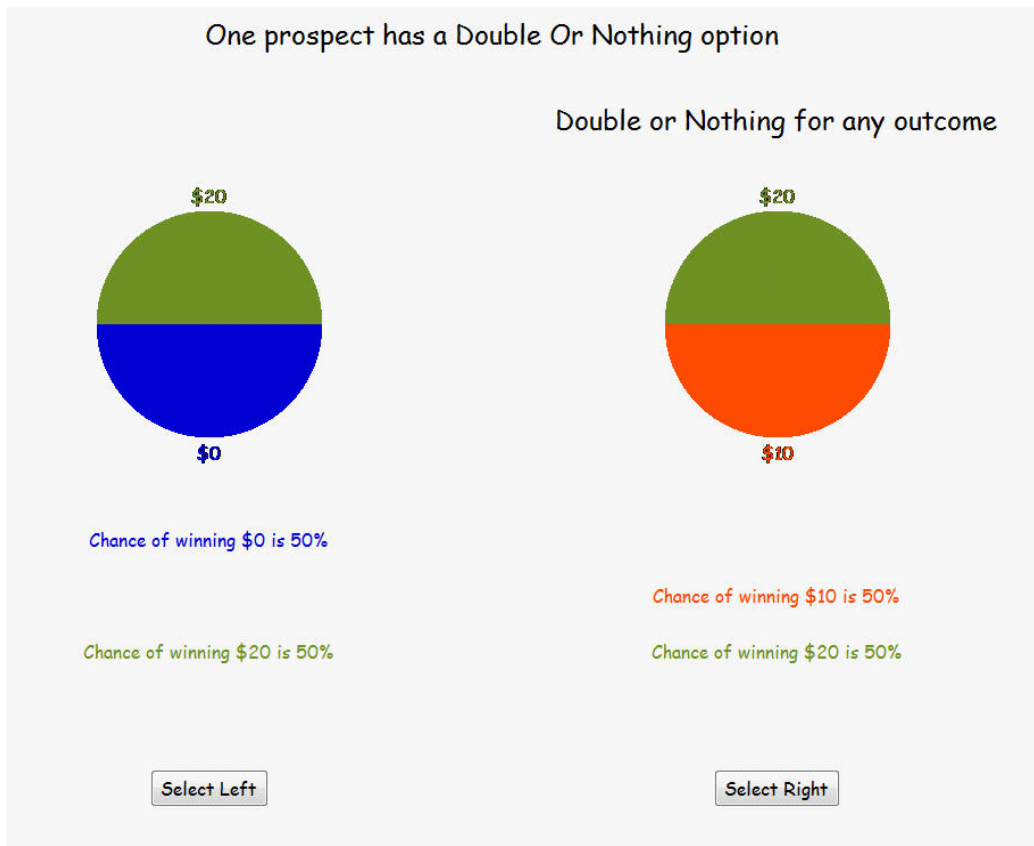
to the amounts before any “Double or Nothing” coin toss.

After you have worked through all of the 40 pairs of prospects, raise your hand and an experimenter will come over. You will then roll two 10-sided dice until a number between 1 and 40 comes up to determine which pair of prospects will be played out. Since there is a chance that any of your 40 choices could be played out for real, you should approach each pair of prospects as if it is the one that you will play out. Finally, you will roll the two ten-sided dice to determine the outcome of the prospect you chose, and if necessary you will then toss a coin to determine if you get "Double or Nothing."

For instance, suppose you picked the prospect on the left in the last example. If the random number was 37, you would win \$0; if it was 93, you would get \$20.

If you picked the prospect on the right and drew the number 37, you would get \$10; if it was 93, you would have to toss a coin to determine if you get "Double or Nothing." If the coin comes up Heads then you get \$40. However, if it comes up Tails you get nothing from your chosen prospect.

It is also possible that you will be given a prospect in which there is a "Double or Nothing" option no matter what the outcome of the random number. The screen image below illustrates this possibility.



Therefore, your payoff is determined by four things:

- by which prospect you selected, the left or the right, for each of these 40 pairs;
- by which prospect pair is chosen to be played out in the series of 40 such pairs using the two 10-sided dice;
- by the outcome of that prospect when you roll the two 10-sided dice; and
- by the outcome of a coin toss if the chosen prospect outcome is of the “Double or Nothing” type.

Which prospects you prefer is a matter of personal taste. The people next to you may be presented with different prospects, and may have different preferences, so their responses should not matter to you. Please work silently, and make your choices by thinking carefully about each prospect.

All payoffs are in cash, and are in addition to the \$7.50 show-up fee that you receive just for being here. The only other task today is for you to answer some demographic questions. Your answers to those questions will not affect your payoffs



## Appendix C: Related Literature (WORKING PAPER)

Cubitt, Starmer and Sugden [1998a] studied the Reduction of Compound Lotteries Axiom (ROCL) in a 1-in-1 design that gave each subject one and only one problem for real stakes and was conceived to test principles of dynamic choice. Also, Starmer and Sugden [1991], Beattie and Loomes [1997] and Cubitt, Starmer and Sugden [1998] have studied the Random Lottery Incentive Method (RLIM), and as a by-product have tested the ROCL axiom. We focus on the results related to the ROCL axiom: Harrison and Swarthout [2012] review the results related to RLIM.

Cubitt, Starmer and Sugden [1998a] gave to one group of subjects one problem that involved compound lotteries and gave to another group the reduced compound version of the same problem. If ROCL is satisfied, one should see the same pattern of choice in both groups. They cannot find statistically significant violations of ROCL in their design.

Starmer and Sugden [1991] gave their subjects two pairs of lotteries that were designed to test “common consequence” violations of EUT. In each pair  $i$  there was a risky ( $R_i$ ) option and a safe ( $S_i$ ) option. They recruited 160 subjects that were divided into four groups of equal number. Two groups faced one of the two pairs in 1-in-1 treatments, while the other two groups were given both pairs to make a choice over using the RLIM to choose the pair for final payoff. We focus on the latter two groups since RLIM induces four possible compound lotteries: i) (0.5,  $R_1$ ; 0.5,  $R_2$ ), ii) (0.5,  $R_1$ ; 0.5,  $S_2$ ), iii) (0.5,  $R_2$ ; 0.5,  $S_1$ ) and iv) (0.5,  $S_1$ ; 0.5,  $S_2$ ). The lottery parameters were chosen to make compound lotteries ii) and iii) have equal actuarially-equivalent prospects.

They hypothesize that if a *reduction principle* holds, and if any of the induced compound lotteries ii) and iii) above is preferred by a subject, then the other one must be preferred.<sup>19</sup> The rejection of this hypothesis is a violation of ROCL, since this axiom implies that two compound lotteries with the same actuarially-equivalent prospects should be equally preferred. Therefore, the null hypothesis in Starmer and Sugden [1991; p. 976] is that “the choice between these two responses to be made at random; as a result, these responses should have the same expected frequency.” From the 80 subjects that faced the 1-in-2 treatments, 32.5% of the individuals chose (0.5,  $R_1$ ; 0.5,  $S_2$ ) and 15% chose (0.5,  $R_2$ ; 0.5,  $S_1$ ), thus Starmer and Sugden reject the null hypothesis of equal frequency in choices based on a one-tail test with a binomial distribution and  $p$ -value=0.017. This pattern is very similar in each of the 1-in-2 treatments; in one of them the proportions are 30% and 15%, whereas in the other they are 35% and 15%. A two-sided Fisher Exact test yields a  $p$ -value of 0.934, which suggest that these patterns of choices are very similar in both 1-in-2 treatments. Therefore, there is no statistical evidence to support ROCL in their experiment.

Beattie and Loomes [1997] examined 4 lottery choice tasks. The first 3 tasks involved a binary choice between two lotteries, and the fourth task involved the subject selecting one of four possible lotteries, two of which were compound lotteries.<sup>20</sup> They recruited 289 subjects that were randomly

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<sup>19</sup> Following Holt [1986], Starmer and Sugden [1991, p. 972] define the reduction principle to be when “compound lotteries are reduced to simple ones by the calculus of probabilities and that choices are determined by the subject’s preferences over such reduced lotteries.” This is what we call ROCL, in *addition* to some axioms that are maintained for present purpose.

<sup>20</sup> Beattie and Loomes [1997] use nine prospects: A= (0.2, £15; 0.8, £0), B= (0.25, £10; 0.75, £0), C= (0.8, £0; 0.2, £30), D= (0.8, £5; 0.2, £0), E=(0.8, £15; 0.2, £0), F = (1, £10), G=(1, £4), H=(0.5,£10; 0.5,£0),

assigned to six groups. The first group faced a hypothetical treatment and was paid a flat fee for completing all four tasks. The second group was given a 1-in-4 treatment, and each of the other four groups faced one of the four tasks in 1-in-1 treatments. Sample sizes were 49 for the hypothetical treatment and the 1-in-4 treatment, and a total of 191 in the four 1-in-1 treatments.

Beattie and Loomes [1997; p. 164] find that “there is no support for the idea that two problems involving the same ‘reduced form’ alternatives – and therefore involving the same difference between expected values – will be treated equivalently.” On this basis, their Question 3 in the 1-in-4 treatment would be actuarially-equivalent to their Question 1 in the 1-in-1 treatment. They found that the pattern of choices in both treatments are so different “that a chi-square test rejects with a very great confidence ( $p < .001$ ) the hypothesis that they are treated equivalently” (p. 164). The  $p$ -value  $< 0.001$  of the Fisher Exact test provides further support for this violation of ROCL.

Their Question 4 is a task that is similar to the method developed by Binswanger [1980]: subjects are offered an ordered set of choices that increase the average payoff while increasing variance. The difference with the Binswanger procedure is that two of the four choices involved compound lotteries: one paid a given amount of money if two Heads in a row were flipped, and the other paid a higher amount if three Heads in a row were flipped. For responses in Question 4, Beattie and Loomes [1997; p.162]

...conjecture that the REAL [1-in-1] treatment might stimulate the greatest effort to picture the full sequential process [of coin flipping in the compound prospects] and, as a part of that, to anticipate feelings at each stage in the sequence; whereas the HYPO [hypothetical] treatment would be most conducive to thinking of the alternatives in their reduced form as a set of simple lotteries... The RPSP [1-in-4 treatment] might then, both formally and psychologically, represent an intermediate position, making the process less readily imaginable by adding a further stage (the random selection of the problem) to the beginning of the sequence, and reducing but not eliminating the incentive to expend the necessary imaginative effort.

On this basis, they predict that, when answering Question 4, subjects in the hypothetical treatment are more likely to think in *reduced form probability distributions*. Beattie and Loomes consider that this might enhance the salience of the high-payoff option, and thus the compound lotteries are expected to be chosen more frequently in the hypothetical treatment than in the 1-in-1 and 1-in-4 treatments.

Beattie and Loomes [1997; p.165] found support for these conjectures: Their subjects tend to choose the compound lotteries more often in the hypothetical treatment than in the ones with economic incentives (i.e., 1-in-1 and 1-in-4 treatments). They found that under the hypothetical treatment more than 1 in 3 of the sample opted for the compound lotteries; this proportion was reduced in the 1-in-4 treatment to just over 1 in 5; and in the 1-in-1 treatment the proportion fell to 1

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I=(£25 if two Heads in a row are flipped; otherwise nothing) and J=(£62.50 if three Heads in a row are flipped; otherwise nothing). Questions 1 through 3 are binary choices that offer, respectively, A or B, C or D and E or F. In Question 4, the subject must choose the prospect that she prefers the most among G, H, I or J. Options I and J are compound lotteries with 2 and 3 stages, respectively.

in 12. A chi-square test rejects ( $p$ -value  $< 0.01$ ) the hypothesis that there is no difference in patterns across treatments. The Fisher Exact test is consistent with this result.<sup>21</sup>

Cubitt, Starmer and Sugden [1998] use common consequence and common ratio pairs of pairs in three experiments. We focus in the first two since the third experiment has no treatments relevant to test ROCL. In the first experiment they compare 1-in-1 choices with 1-in-2 choices. Their comparison rests on subjects not having extreme risk-loving preferences over the pairs of lotteries in the 1-in-2 treatment designed to capture this behavior. Given that this *a priori* assumption is true, and it is generally supported by their data, the lottery pairs in each of the 1-in-2 treatments were chosen to generate compound prospects with actuarially-equivalent lotteries equal to the prospects in each of the 1-in-1 treatments.

If ROCL is satisfied, the distribution of responses between risky and safe lotteries should be the same in both treatments. The  $p$ -value from the Fisher Exact test in one of the 1-in-1 and 1-in-2 treatment comparisons<sup>22</sup> is 0.14, which suggests that ROCL is most likely violated.<sup>23</sup>

Similarly, in the second experiment the 1-in-2 treatment induced compound lotteries with actuarially-equivalent prospects equal to the lottery choices in one of their 1-in-1 treatment. In the latter, 52% of the 46 subjects chose the risky lottery, whereas 38% of the 53 subjects in the 1-in-2 treatment chose the risky prospect. These choice patterns suggest that ROCL does not hold in the second experiment.<sup>24</sup>

Bernasconi and Loomes [1992] devise exactly the right sorts of tests of ROCL with objective probabilities, motivated we were by the theoretical claims of Segal [1987] about the role of ROCL with respect to the modeling of ambiguity aversion. Unfortunately their experiments were hypothetical, and conducted with pseudo-volunteers from classes. Although the authors claim that “... we can confidently say that we found no obvious signs of widespread confusion, misunderstanding or carelessness,” there is no way to ascertain *a priori* the reliability of the responses.

Abdellaoui, Klibanoff and Placido [2011] elicit certainty equivalents (CE) from simple risk and compound risk lotteries on a within-subjects basis. Each subject made a number of binary choices to provide a certainty-equivalent interval for each of 32 gambles, with one selected at the end to play out

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<sup>21</sup> We test the similarity between treatments of the proportions of subjects that chose each of the four prospects in Question 4. The two-sided Fisher Exact test applied to the hypothetical and the 1-in-1 treatments rejects the hypothesis of no difference in choice patterns ( $p$ -value = 0.001). The  $p$ -value for the comparison of the same four choices between the hypothetical and the 1-in-4 treatments is 0.473.

<sup>22</sup> Groups 1.1 and 1.3 in their notation.

<sup>23</sup> The proportions of subjects that chose the risky prospect in the other 1-in-1 and 1-in-2 treatments (groups 1.2 and 1.4 in their notation) are close: 50% and 55%, respectively. However, we cannot perform the Fisher Exact test for this 1-in-1 and 1-in-2 comparison, since the compound lotteries induced by the 1-in-2 treatment have actuarially-equivalent prospects equal to the ones in the 1-in-1 treatment only if the subjects do not exhibit extreme risk-loving preferences. Since 8% of the subjects in this 1-in-2 treatment exhibited risk-loving preferences, one cannot perform the Fisher test because this contaminates the comparison between the compound lotteries and their actuarially-equivalent counterparts.

<sup>24</sup> Since one of the subjects in the 1-in-2 treatment (group 2.3) exhibited risk-loving preferences, we cannot perform the Fisher Exact test for the reasons explained earlier.

for money. Thus their design *assumes* the RLIM procedure which we evaluate in our design, and which we find to interact with inferences about ROCL over objective probabilities. They also included some ambiguous lotteries within the 32 gambles, to test whether attitudes towards compound objective risk, as compared to simple objective risk, were correlated with attitudes towards ambiguity.

Focusing on their results for simple and compound objective risk, they compare certainty equivalents over the 64 subjects for tasks with the same expected value (EV). They further break the comparisons down into one set of tasks for which the winning probability was 1/12, another set of tasks for which it was 6/12, and a third set of tasks for which it was 11/12. Within each set the EV was the same: €4.17, €25 and €45.83. Thus any inferences about the effect of the probability of winning on the risk premium are perfectly confounded by inferences about the effect of the scale of the prizes. They report (p.9) that subjects were, on average, risk-seeking for low probabilities, risk-neutral for probabilities of one-half, and risk-averse for high probabilities. Of course, this is consistent with increasing relative risk aversion for prizes deriving solely from the utility function, and need not be due to risk premia induced by probability weighting.

The critical test is whether one observed differences in behavior between the CE for simple lotteries and the CE for compound lotteries. For the 1/12 probability, they report (Table 3, p.10) an average CE of 7.81 with a standard deviation of 5.22 for the simple lottery, and an average CE of 6.78 with a standard deviation of 5.12 for one of the representative compound risks (called “hypergeometric compound risk” by them). In this case the corresponding *t*-test, assuming unequal variances, has a *t* statistic of 1.13 and a two-sided *p*-value on the null hypothesis of no difference in means equal to 0.26. So one would not conclude that there is any statistically significant difference. The same tests for the other four compound risk types of the probability 1/12 result in *p*-values of 0.06 (“compound risk high”), 0.07 (“compound risk high with explicit degenerate urn”), 0.29 (“compound risk low”) and 0.32 (“compound risk low with explicit degenerate urn”). So 2 of the 5 tests have *p*-values that might be viewed as indicating statistically significant differences in risk premia between simple and compound lotteries. The correct test would recognize that these are matched samples, since the same subject gave the CE in each case; we do not have access to the raw data at the individual level to conduct these tests. The correct test would also recognize that the choices across the pairs are not independent, so that the five *p*-values reported above are not independently generated.

For the set of compound lotteries with a winning probability of 6/12, the *p*-values for these tests are 0.13, 0.91, 0.03, 0.82, 0.33, 0.72 and 0.22. The first 2 tests refer to “diverse uniform compound risk” and “degenerate uniform compound risk,” respectively, and the remaining 5 tests are the same as for the winning probability of 1/12. Only 1 of the 7 tests have a *p*-value approaching statistical significance.

Abdellaoui, Klibanoff and Placido [2011] draw conclusions about the validity of ROCL for objective risks from the tabulations of CE differences in their Table 4. These tabulations show, for each comparison type, the fraction of the 64 subjects that had a CE for the simple risk below the CE for the compound risk, the fraction that had *exactly the same CE*, and the fraction that has a CE for the simple risk above the CE for the compound risk. They refer to the middle fraction as those exhibiting behavior consistent with ROCL, but obviously this ignores the usual sampling variability one expects to see in such behavior. They report ROCL-consistent fractions that are generally between 20% and

26%, but it is hard to know how to interpret those fractions in any statistically meaningful way. This is unfortunate, since the inferences they draw from them correspond to our own when one assumes RLIM.

Finally, for the set of compound lotteries with a winning probability of 11/12, the  $p$ -values for these tests are 0.19, <0.001, 0.002, <0.001, and 0.01 respectively. The type of compound risk in each case is the same as for the 1/12 case. Here we see much more significant evidence of differences in risk attitudes, with the evidence in all cases in favor of there being a larger risk premium for the compound risk than for the EV-comparable simple risk.

Kaivanto and Kroll [2012] undertake a strange experiment in which subjects are asked to choose between two ways of generating a 1/10 probability of winning €100 or nothing. The first way is to roll a 100-sided die, and receive the prize if the same two digits appear (i.e., 11, 22, 33, etc.). the other way is to roll a 10-sided die twice, where the first roll generates the first digit and the second roll generates the second digit. The first randomization method is interpreted as a simple lottery, and the second randomization method as a compound lottery. The majority of subjects preferred the 100-sided randomization method. It is not at all apparent why one would view this as a test of ROCL in any interesting sense.

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## Appendix D: Nonparametric Tests (WORKING PAPER)

### *D1. Choice Patterns Where ROCL Predicts Indifferent Choices*

Analysis of Data from the 1-in-1 Treatment. We use a generalized version of the Fisher Exact test to jointly test the null hypothesis that the proportion of subjects who chose the C lottery over the AE lottery in each of the AE-C lottery pairs are the same, as well as the Binomial Probability test to evaluate our null hypothesis of equiprobable choice in each of the AE-C lottery pairs.

We do not observe statistically significant violations of the ROCL indifference prediction in the 1-in-1 treatment. Roughly 59% of the subjects (19 out of 32 subjects) presented with a AE-C lottery pair chose the C lottery and we cannot reject that this proportion is statistically different from 50%. Table D1 presents the generalized Fisher Exact test for all AE-C lottery pair choices, and the test's  $p$ -value of 0.342 provides no support for the hypothesis of violations of the ROCL indifference prediction. We see from this test that the proportions are the same across pairs. We now use the Binomial Probability test to see if the proportions are different from 50%. Pooling over all AE-C lottery pairs we find a  $p$ -value of 18.9% for the null hypothesis that subjects chose the C and the AE lotteries in equal proportions. Table D2 shows the Binomial Probability test applied individually to each of the AE-C lottery pairs for which we have observations. We cannot reject the null hypothesis that subjects chose the C and the AE lotteries in equal proportions, as all  $p$ -values are insignificant at any reasonable level of confidence.

Analysis of Data from the 1-in-40 Treatment The strategy to test the ROCL prediction of indifference in this treatment is different from the one used in the 1-in-1 treatment, given the repeated measures we have for each subject in the 1-in-40 treatment. We now use the Cochran Q test to evaluate whether the proportion of subjects who choose the C lottery is the same in each of the 10 AE-C lottery pairs.<sup>25</sup> A significant difference of proportions identified by this test is sufficient to reject the null prediction of indifference.<sup>26</sup> Of course, an insignificant difference of proportions would require us to additionally verify that the common proportion across pairs the pairs is indeed 50% before we fail to reject the null hypothesis of indifference.

We observe an overall violation of the ROCL indifference prediction in the 1-in-40 treatment. Table D3 reports the results of the Cochran Q test, as well as summary statistics of the information used to conduct the test. The Cochran Q test yields a  $p$ -value of less than 0.0001, which strongly suggests rejection of the null hypothesis of equiprobable proportions. We conclude that, for at least for one of the AE-C lottery pairs, the proportion of subjects who chose the C lottery is not equal to 50%. This result is a violation of ROCL and we cannot claim that subjects satisfy ROCL and choose at random in all of the 10 AE-C lottery pairs in the 1-in-40 treatment.

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<sup>25</sup> The Binomial Probability test is inappropriate in this setting, as it assumes independent observations. Obviously, observations are not independent when each subject makes 40 choices.

<sup>26</sup> For example, suppose there were only 2 AE-C lottery pairs. If the Cochran Q test finds a significant difference, we conclude that the proportion of subjects choosing the C lottery is not the same in the two lottery pairs. Therefore, even if the proportion for one of the pairs was truly equal to 50%, the test result would imply that the other proportion is not statistically equal to 50%, and thus indifference fails.

## *D2. Choice Patterns Where ROCL Predicts Consistent Choices*

Suppose a subject is presented with a given S-C lottery pair, and further assume that she prefers the C lottery over the S lottery. If the subject satisfies ROCL and is separately presented with a second pair of lotteries consisting of the same S lottery and the AE lottery of the previously-presented C lottery, then she would prefer and should choose the AE lottery. Similarly, of course, if she instead prefers the S lottery when presented separately with a given S-C lottery pair, then she should choose the S lottery when presented with the corresponding S-AE lottery pair.

Recall that each of the 15 S-C lottery pairs in Table A2 has a corresponding S-AE pair in Table A3. Therefore, we can construct 15 comparisons of lottery pairs that constitute 15 consistency tests of ROCL. In the 1-in-40 treatment we again must assume that the RLIM is incentive compatible, and we again use data from the 1-in-1 treatment to control for possible confounds created by the RLIM. We must now assume homogeneity in risk preferences for the analysis of behavior in the 1-in-1 treatment, since we are making across-subject comparisons. However, in the next section we will present econometric analysis which allows for heterogeneity in risk preferences and test if a violation of the homogeneity assumption is confounded with a violation of ROCL.

Our hypothesis is that a given subject chooses the S lottery when presented with the S-C lottery pair if and only if the same subject also chooses the S lottery when presented with the corresponding S-AE lottery pair.<sup>27</sup> Therefore, ROCL is satisfied if we observe that the proportion of subjects who choose the S lottery when presented with a S-C pair is equal to the proportion of subjects who choose the S lottery when presented with the corresponding S-AE pair. Conversely, ROCL is violated if we observe unequal proportions of choosing the S lottery across a S-C pair and linked S-AE pair. We do not find evidence to reject the consistency in patterns implied by ROCL in the 1-in-1 treatment, while we do find evidence of violations of ROCL in the 1-in-40 treatment.

Analysis of Data from the 1-in-1 Treatment We use the Cochran-Mantel-Haenszel (CMH) test to test the joint hypothesis that in all of the 15 paired comparisons, subjects choose in the same proportion the S lottery when presented with the S-C lottery pair and its linked S-AE lottery pair (see Mantel and Haenszel [1959]).<sup>28</sup> If the CMH test rejects the null hypothesis, then we interpret this as evidence of overall ROCL-inconsistent observed behavior. We also use the Fisher Exact test to evaluate individually the consistency predicted by ROCL in each of the 15 linked comparisons of S-C pairs and S-AE pairs for which we have enough data to conduct the test.

We cannot reject the ROCL consistency prediction. The CMH test does not reject the joint null hypothesis that the proportion of subjects chose the S lottery when they were presented with any given S-C pair is equal to the proportion of subjects that chose the S lottery when they were presented

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<sup>27</sup> Notice that this is equivalent to stating the null hypothesis using the C and AE lotteries. We chose to work with the S lottery for simplicity.

<sup>28</sup> The proportion of subjects who choose the S lottery when presented with a S-C pair, or its paired S-AE lottery pair, has to be equal within each paired comparison, but can differ across comparisons. More formally, the CMH test evaluates the null hypothesis that the odds ratio of each of the 15 contingency tables constructed from the 15 paired comparisons are jointly equal to 1.



with the corresponding S-AE pair. The  $\chi^2$ -statistic for the CMH test with the continuity correction<sup>29</sup> is equal to 2.393 with a corresponding  $p$ -value of 0.122. Similarly, the Fisher Exact tests presented in Table D4 show only in one comparison the  $p$ -value is less than 0.05. Therefore, we cannot reject the ROCL consistency prediction in the 1-in-1 treatment. However, as we mentioned previously, this conclusion relies on the assumption of homogeneity in preferences which we can control for in the econometric models.

Analysis of Data from the 1-in-40 Treatment We use the Cochran Q test coupled with the Bonferroni-Dunn (B-D) correction procedure<sup>30</sup> to test the hypothesis that subjects choose the S lottery in the same proportion when presented with linked S-C and S-AE lottery pairs. The B-D procedure takes into account repeated comparisons and allows us to maintain a familywise error rate across the 15 paired comparisons of S-C and S-AE lottery pairs.

We find evidence to reject the ROCL consistency prediction. Table D5 shows the results of the B-D method<sup>31</sup> for each of the 15 paired comparisons. Table D5 provides evidence that with a 5% familywise error rate, subjects choose the S lottery in different proportions across linked S-C lottery pairs and S-AE lottery pairs in two comparisons: Pair 1 vs. Pair 16 and Pair 3 vs. Pair 18. This implies that the ROCL prediction of consistency is rejected in 2 of our 15 consistency comparisons.

We are also interested in studying the patterns of violations of ROCL. A pattern inconsistent with ROCL would be subjects choosing the S lottery when presented with a given S-C lottery pair, but switching to prefer the AE lottery when presented with the matched S-AE pair. We construct  $2 \times 2$  contingency tables that show the number of subjects in any given matched pair who exhibit each of the four possible choice patterns: (i) always choosing the S lottery; (ii) choosing the S lottery when presented with a S-C pair and switching to prefer the AE lottery when presented with the matched S-AE pair; (iii) choosing the C lottery when presented with a S-C pair and switching to prefer the S lottery when presented with the matched S-AE; and (iv) choosing the C lottery when presented with the S-C lottery and preferring the AE lottery when presented with the matched S-AE.

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<sup>29</sup> We follow Li, Simon and Gart [1979] and use the continuity correction to avoid possible misleading conclusions from the test in small samples.

<sup>30</sup> The B-D method is a *post-hoc* procedure that is conducted after calculating the Cochran Q test. The first step is to conduct the Cochran Q test to evaluate the null hypothesis that the proportions of individuals who choose the S lottery is the same in all 15 S-C and 15 S-AE linked lottery pairs. If this null is rejected the B-D method involves calculating a critical value  $d$  that takes into account all the information of the 30 lottery pairs. The B-D method allows us to test the statistical significance of the observed difference between proportions of subjects who choose the S lottery in any given paired comparison. Define  $p_1$  as the proportion of subjects who choose the S lottery when presented with a given S-AE lottery pair. Similarly, define  $p_2$  as the proportion of subjects who chose the S lottery in the paired S-C lottery pair. The B-D method rejects the null hypothesis that  $p_1 = p_2$  if  $|p_1 - p_2| > d$ . In this case we would conclude that the observed difference is statistically significant. This is a more powerful test than conducting individual tests for each paired comparison because the critical value  $d$  takes into account the information of all 15 comparisons. See Sheskin [2004; p. 871] for further details of the B-D method.

<sup>31</sup> The Cochran Q test rejected its statistical null hypothesis  $\chi^2$  statistic 448.55, 29 degrees of freedom and  $p$ -value < 0.0001.

Since we have paired observations, we use the McNemar test to evaluate the null hypothesis of equiprobable occurrences of discordant choice patterns (ii) and (iii) within each set of matched pairs. We find a statistically significant difference in the number of (ii) and (iii) choice patterns within 4 of the 15 matched pairs. Table D6 reports the exact  $p$ -values for the McNemar test. The McNemar test results in  $p$ -values less than 0.05 in four comparisons: Pair 1 vs. Pair 16, Pair 3 vs. Pair 18, Pair 10 vs. Pair 25 and Pair 13 vs. Pair 28.<sup>32</sup> Moreover, the odds ratios of the McNemar tests suggest that the predominant switching pattern is choice pattern (iii): subjects tend to switch from the S lottery in the S-AE pair to the C lottery in the S-C pair. The detailed contingency tables for these 4 matched pairs show that the number of choices consistent with pattern (iii) is considerably greater than the number of choices consistent with (ii).

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<sup>32</sup> These violations of ROCL are also supported by the B-D procedure if the familywise error rate is set to 10%.

**Table D1: Generalized Fisher Exact Test on the Actuarially-Equivalent Lottery vs. Compound Lottery Pairs**  
Treatment: 1-in-1

Fisher Exact  $p$ -value = 0.342

AE-C Lottery Pair	Observed # of choices of AE lotteries	Observed # of choices of C lotteries	Total
31	0	1	1
32	0	3	3
33	2	5	7
36	4	1	5
37	1	1	2
38	2	1	3
39	1	4	5
40	3	3	6
Total	13	19	32

Note: due to the randomization assignment of lottery pairs to subjects, there were no observations for pairs 34 and 35.

**Table D2: Binomial Probability Tests on Actuarially-Equivalent Lottery vs. Compound Lottery Pairs**  
Treatment: 1-in-1

AE-C Lottery Pair	Total # of observations	Observed # of choices of C lotteries	Observed proportion of choices of C lotteries (p)	<i>p</i> -value for $H_0: p = 0.5$
32	3	3	1	0.25
33	7	5	0.714	0.453
36	5	1	0.2	0.375
37	2	1	0.5	1
38	3	1	0.333	1
39	5	4	0.8	0.375
40	6	3	0.5	1

Note: due to the randomization assignment of lottery pairs to subjects there were no observations for pairs 34 and 35 and only 1 observation for pair 31.

**Table D3: Cochran Q Test on the Actuarially-Equivalent Lottery vs. Compound Lottery Pairs**

Treatment: 1-in-40

Cochran's  $\chi^2$  statistic (9 d.f) = 86.090

*p*-value < 0.0001

Data	
AE-C Lottery Pair	Observed # of choices of C lotteries (out of 62 observations)
31	48
32	46
33	34
34	26
35	24
36	18
37	34
38	35
39	49
40	28

**Table D4: Fisher Exact Test on Matched Simple-Compound and Simple-Actuarially-Equivalent Pairs**

Treatment: 1-in-1

Comparison	Total # of subjects in		Proportion of subjects that chose the S lottery in the S-AE pair ( $\pi_1$ )	Proportion of subjects that chose the S lottery in the S-C pair ( $\pi_2$ )	<i>p</i> -value for $H_0: \pi_1 = \pi_2$
	S-AE Pair	S-C Pair			
Pair 1 vs. Pair 16	3	2	1	0.5	0.4
Pair 3 vs. Pair 18	6	2	0.5	0	0.464
Pair 5 vs. Pair 20	1	2	0	1	0.333
Pair 6 vs. Pair 21	3	4	0.67	0.5	1
Pair 7 vs. Pair 22	4	9	1	0.56	0.228
Pair 8 vs. Pair 23	3	4	0.33	0.5	1
Pair 9 vs. Pair 24	3	6	0	0.83	0.048
Pair 11 vs. Pair 26	5	9	0.6	0.56	1
Pair 12 vs. Pair 27	5	2	0.8	1	1
Pair 13 vs. Pair 28	4	1	0.5	1	1
Pair 15 vs. Pair 30	3	1	1	0	0.25

Note: due to the randomization assignment of lottery pairs to subjects, the table only shows the Fisher Exact test for 11 S-AE/S-C comparisons for which there are sufficient data to conduct the test.

**Table D5: Bonferroni-Dunn Method on Matched Simple-Compound and Simple-Actuarially-Equivalent Pairs**

Treatment: 1-in-40

Matching	Proportion of subjects that chose the S lottery in the S-AE pair ( $p_1$ )	Proportion of subjects that chose the S lottery in the S-C pair ( $p_2$ )	$ p_1 - p_2 $
Pair 1 vs. Pair 16	0.871	0.387	0.484
Pair 2 vs. Pair 17	0.984	0.952	0.032
Pair 3 vs. Pair 18	0.887	0.629	0.258
Pair 4 vs. Pair 19	0.226	0.21	0.016
Pair 5 vs. Pair 20	0.403	0.290	0.113
Pair 6 vs. Pair 21	0.742	0.661	0.081
Pair 7 vs. Pair 22	0.677	0.548	0.129
Pair 8 vs. Pair 23	0.548	0.548	0
Pair 9 vs. Pair 24	0.258	0.306	0.048
Pair 10 vs. Pair 25	0.919	0.774	0.145
Pair 11 vs. Pair 26	0.581	0.613	0.032
Pair 12 vs. Pair 27	0.565	0.645	0.081
Pair 13 vs. Pair 28	0.871	0.726	0.145
Pair 14 vs. Pair 29	0.742	0.677	0.065
Pair 15 vs. Pair 30	0.387	0.419	0.032

Note: the test rejects the null hypothesis of  $p_1=p_2$  if  $|p_1-p_2| > d$ . The calculation of the critical value  $d$  requires that one first define *ex ante* a familywise Type I error rate ( $\alpha_{FW}$ ). For  $\alpha_{FW} = 10\%$  the corresponding critical value is 0.133, and for  $\alpha_{FW} = 5\%$  the critical value is 0.159.

**Table D6: McNemar Test on Matched Simple-Compound  
and Simple-Actuarially-Equivalent Pairs**  
Treatment: 1-in-40

Matching	Exact $p$ -value	Odds Ratio
Pair 1 vs. Pair 16	<0.0001	0.0625
Pair 2 vs. Pair 17	0.625	0.3333
Pair 3 vs. Pair 18	0.0001	0.0588
Pair 4 vs. Pair 19	1	0.8571
Pair 5 vs. Pair 20	0.1671	0.4615
Pair 6 vs. Pair 21	0.3323	0.5454
Pair 7 vs. Pair 22	0.1516	0.5
Pair 8 vs. Pair 23	1	1
Pair 9 vs. Pair 24	0.6072	0.6667
Pair 10 vs. Pair 25	0.0352	0.25
Pair 11 vs. Pair 26	0.8238	1.222
Pair 12 vs. Pair 27	0.4049	1.555
Pair 13 vs. Pair 28	0.0117	0.1
Pair 14 vs. Pair 29	0.5034	0.6667
Pair 15 vs. Pair 30	0.8388	1.1818



## Appendix E: Structural Econometric Analysis (WORKING PAPER)

Assume that utility of income is defined by

$$U(x) = x^{(1-r)}/(1-r) \quad (E1)$$

where  $x$  is the lottery prize and  $r \neq 1$  is a parameter to be estimated. For  $r=1$  assume  $U(x)=\ln(x)$  if needed. Thus  $r$  is the coefficient of CRRA:  $r=0$  corresponds to risk neutrality,  $r<0$  to risk loving, and  $r>0$  to risk aversion. Let there be  $J$  possible outcomes in a lottery, and denote outcome  $j \in J$  as  $x_j$ . Under EUT the probabilities for each outcome  $x_j$ ,  $p(x_j)$ , are those that are induced by the experimenter, so expected utility is simply the probability weighted utility of each outcome in each lottery  $i$ :

$$EU_i = \sum_{j=1}^J [ p(x_j) \times U(x_j) ]. \quad (E2)$$

The EU for each lottery pair is calculated for a candidate estimate of  $r$ , and the index

$$\nabla EU = EU_R - EU_L \quad (E3)$$

is calculated, where  $EU_L$  is the “left” lottery and  $EU_R$  is the “right” lottery as presented to subjects. This latent index, based on latent preferences, is then linked to observed choices using a standard cumulative normal distribution function  $\Phi(\nabla EU)$ . This “probit” function takes any argument between  $\pm\infty$  and transforms it into a number between 0 and 1. Thus we have the probit link function,

$$\text{prob}(\text{choose lottery R}) = \Phi(\nabla EU) \quad (E4)$$

Even though this “link function” is common in econometrics texts, it forms the critical statistical link between observed binary choices, the latent structure generating the index  $\nabla EU$ , and the probability of that index being observed. The index defined by (E3) is linked to the observed choices by specifying that the R lottery is chosen when  $\Phi(\nabla EU) > 1/2$ , which is implied by (E4).

The likelihood of the observed responses, conditional on the EUT and CRRA specifications being true, depends on the estimates of  $r$  given the above statistical specification and the observed choices. The “statistical specification” here includes assuming some functional form for the cumulative density function (CDF). The conditional log-likelihood is then

$$\ln L(r; y, \mathbf{X}) = \sum_i [ (\ln \Phi(\nabla EU)) \times \mathbf{I}(y_i = 1) + (\ln (1-\Phi(\nabla EU))) \times \mathbf{I}(y_i = -1) ] \quad (E5)$$

where  $\mathbf{I}(\cdot)$  is the indicator function,  $y_i = 1(-1)$  denotes the choice of the right (left) lottery in risk aversion task  $i$ , and  $\mathbf{X}$  is a vector of individual characteristics reflecting age, sex, race, and so on.

Harrison and Rutström [2008; Appendix F] review procedures that can be used to estimate structural models of this kind, as well as more complex non-EUT models, with the goal of illustrating how to write explicit maximum likelihood (ML) routines that are specific to different structural choice models. It is a simple matter to correct for multiple responses from the same subject (“clustering”), if needed.

It is also a simple matter to generalize this ML analysis to allow the core parameter  $r$  to be a linear function of observable characteristics of the individual or task. We extend the model to be  $r = r_0 + R \times \mathbf{X}$ , where  $r_0$  is a fixed parameter and  $R$  is a vector of effects associated with each characteristic in the variable vector  $\mathbf{X}$ . In effect, the unconditional model assumes  $r = r_0$  and estimates  $r_0$ . This extension significantly enhances the attraction of structural ML estimation, particularly for responses pooled over different subjects and treatments, since one can condition estimates on observable characteristics of the task or subject.

In our case we also extend the structural parameter to take on different values for the lotteries presented as compound lotteries. That is, (E1) applies to the evaluation of utility for all simple lotteries and a different CRRA risk aversion coefficient  $r + rc$  applies to compound lotteries, where  $rc$  captures the additive effect of evaluating a compound lottery. Hence, for compound lotteries, the decision maker employs the utility function

$$U(x \mid \text{compound lottery}) = x^{(1-r-rc)} / (1-r-rc) \quad (\text{E1}')$$

instead of (E1), and we would restate (E1) as

$$U(x \mid \text{simple lottery}) = x^{(1-r)} / (1-r) \quad (\text{E1}'')$$

for completeness. Specifying preferences in this manner provide us with a structural test for ROCL. If  $rc = 0$  then this implies that compound lotteries are evaluated identically to simple lotteries, which is consistent with ROCL. However, if  $rc \neq 0$ , as conjectured by Smith [1969] for objective and subjective compound lotteries, then, decision-makers violate ROCL in a certain source-dependent manner, where the “source” here is whether the lottery is simple or compound.<sup>33</sup> As stressed by Smith [1969],  $rc \neq 0$  for *subjective* lotteries provides a direct explanation for the Ellsberg Paradox, but is much more readily tested on the domain of objective lotteries. Of course, the linear specification  $r + rc$  is a parametric convenience, but the obvious one to examine initially.

An important extension of the core model is to allow for subjects to make some *behavioral* errors. The notion of error is one that has already been encountered in the form of the statistical assumption that the probability of choosing a lottery is not 1 when the EU of that lottery exceeds the EU of the other lottery. This assumption is clear in the use of a non-degenerate link function between the latent index  $\nabla EU$  and the probability of picking a specific lottery as given in (E4). If there were no errors from the perspective of EUT, this function would be a step function: zero for all values of  $\nabla EU < 0$ , anywhere between 0 and 1 for  $\nabla EU = 0$ , and 1 for all values of  $\nabla EU > 0$ .

We employ the error specification originally due to Fechner and popularized by Hey and Orme [1994]. This error specification posits the latent index

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<sup>33</sup> Abdellaoui, Baillon, Placido and Wakker [2011] conclude that different probability weighting functions are used when subjects face risky processes with known probabilities and uncertain processes with subjective processes. They call this “source dependence,” where the notion of a source is relatively easy to identify in the context of an artefactual laboratory experiment, and hence provides the tightest test of this proposition. Harrison [2011] shows that their conclusions are an artefact of estimation procedures that do not take account of sampling errors. A correct statistical analysis that does account for sampling errors provides no evidence for source dependence using their data. Of course, failure to reject a null hypothesis could just be due to samples that are too small.

$$\nabla EU = (EU_R - EU_L)/\mu \quad (E3')$$

instead of (E3), where  $\mu$  is a structural “noise parameter” used to allow some errors from the perspective of the deterministic EUT model. This is just one of several different types of error story that could be used, and Wilcox [2008] provides a masterful review of the implications of the alternatives.<sup>34</sup> As  $\mu \rightarrow 0$  this specification collapses to the deterministic choice EUT model, where the choice is strictly determined by the EU of the two lotteries; but as  $\mu$  gets larger and larger the choice essentially becomes random. When  $\mu=1$  this specification collapses to (E3), where the probability of picking one lottery is given by the ratio of the EU of one lottery to the sum of the EU of both lotteries. Thus  $\mu$  can be viewed as a parameter that flattens out the link functions as it gets larger.

An important contribution to the characterization of behavioral errors is the “contextual error” specification proposed by Wilcox [2011]. It is designed to allow robust inferences about the primitive “more stochastically risk averse than,” and posits the latent index

$$\nabla EU = ((EU_R - EU_L)/v)/\mu \quad (E3'')$$

instead of (E3'), where  $v$  is a new, normalizing term for each lottery pair L and R. The normalizing term  $v$  is defined as the maximum utility over all prizes in this lottery pair minus the minimum utility over all prizes in this lottery pair. The value of  $v$  varies, in principle, from lottery choice pair to lottery choice pair: hence it is said to be “contextual.” If the pair contains a compound and a simple lottery, and the subject exhibits different utility functions for each, then the normalizing term  $v$  can potentially depend on the utility used over compound lotteries *and* on the one used for simple lotteries. For the Fechner specification, dividing by  $v$  ensures that the *normalized* EU difference  $[(EU_R - EU_L)/v]$  remains in the unit interval for each lottery pair. The term  $v$  does not need to be estimated in addition to the utility function parameters and the parameter for the behavioral error term, since it is given by the data and the assumed values of those estimated parameters.

The specification employed here is the source-dependent CRRA utility function from (E1') and (E1''), the Fechner error specification using contextual utility from (3''), and the link function using the normal CDF from (E4). The log-likelihood is then

$$\ln L(r, rc, \mu; y, \mathbf{X}) = \sum_i [ (\ln \Phi(\nabla EU)) \times \mathbf{I}(y_i = 1) + (\ln (1 - \Phi(\nabla EU))) \times \mathbf{I}(y_i = -1) ] \quad (E5'')$$

and the parameters to be estimated are  $r$ ,  $rc$  and  $\mu$  given observed data on the binary choices  $y$  and the lottery parameters in  $\mathbf{X}$ .

It is possible to consider more flexible utility functions than the CRRA specification in (E1), but that is not essential for present purposes. We do, however, consider extensions of the EUT model to allow for rank-dependent decision-making under Rank-Dependent Utility (RDU) models.

The RDU model extends the EUT model by allowing for decision weights on lottery outcomes. The specification of the utility function is the same parametric specification (E1') and

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<sup>34</sup> Some specifications place the error at the final choice between one lottery or after the subject has decided which one has the higher expected utility; some place the error earlier, on the comparison of preferences leading to the choice; and some place the error even earlier, on the determination of the expected utility of each lottery.

(E1'') considered for source-dependent EUT. To calculate decision weights under RDU one replaces expected utility defined by (E2) with RDU

$$RDU_i = \sum_{j=1, J} [ w(p(M_i)) \times U(M_i) ] = \sum_{j=1, J} [ w_j \times U(M_i) ] \quad (E2')$$

where

$$w_j = \omega(p_i + \dots + p_j) - \omega(p_{i+1} + \dots + p_j) \quad (E6a)$$

for  $j=1, \dots, J-1$ , and

$$w_j = \omega(p_j) \quad (E6b)$$

for  $j=J$ , with the subscript  $j$  ranking outcomes from worst to best, and  $\omega(\cdot)$  is some probability weighting function.

We adopt the simple “power” probability weighting function proposed by Quiggin [1982], with curvature parameter  $\gamma$ :

$$\omega(p) = p^\gamma \quad (E7)$$

So  $\gamma \neq 1$  is consistent with a deviation from the conventional EUT representation. Convexity of the probability weighting function is said to reflect “pessimism” and generates, if one assumes for simplicity a linear utility function, a risk premium since  $\omega(p) < p \quad \forall p$  and hence the “RDU EV” weighted by  $\omega(p)$  instead of  $p$  has to be less than the EV weighted by  $p$ . The rest of the ML specification for the RDU model is identical to the specification for the EUT model, but with different parameters to estimate.

It is obvious that one can extend the probability weighting specification to be source-dependent, just as we did for the utility function. Hence we extend (E7) to be

$$\omega(p \mid \text{compound lottery}) = p^{\gamma+\gamma c} \quad (E7')$$

for compound lotteries, and

$$\omega(p \mid \text{simple lottery}) = p^\gamma \quad (E7'')$$

for simple lotteries. The hypothesis of source-independence, which is consistent with ROCL, in this case is that  $\gamma c = 0$  and  $\gamma r = 0$ . We also estimate our models using the inverse-S probability weighting function given by:

$$\omega(p) = p^\gamma / (p^\gamma + (1-p)^\gamma)^{1/\gamma} \quad (E8)$$

The source-dependent specification trivially allows for the inverse-S probability weighting function as follows

$$\omega(p \mid \text{compound lottery}) = p^{\gamma+\gamma c} / (p^{\gamma+\gamma c} + (1-p)^{\gamma+\gamma c})^{1/(\gamma+\gamma c)} \quad (E8')$$

for compound lotteries, and

$$\omega(p \mid \text{simple lottery}) = p^\gamma / (p^\gamma + (1-p)^\gamma)^{1/\gamma} \quad (\text{E8''})$$

for simple lotteries.

#### Analysis of Data from the 1-in-1 Treatment

Assuming homogeneity in preferences, we find that the EUT source-dependent model is rejected in favor of the RDU source-dependent model. Conditional on the RDU source-dependent model we find *no violations of ROCL*. Table E1 shows the parameter estimates of the models we consider. Panel A shows the estimates for the model assuming a power weighting function: the estimates for parameters  $\tau$ ,  $\tau_c$ ,  $\gamma$  and  $\gamma_c$  are 0.62, 0.14, 0.77 and -0.20, respectively. A test of the joint null hypothesis that  $\tau_c = \gamma_c = 0$  results in a  $p$ -value of 0.25. The same test results in a  $p$ -value of 0.76 for the case of the inverse-S weighting function. A test of the joint hypothesis that  $\gamma=1$  and  $\gamma_c=0$  results in a  $p$ -value less than 0.001, whichever probability weighting function is assumed. Thus we reject the hypothesis of that these data are best characterized by the EUT source-dependent model, and conclude that the evidence favors the RDU source-dependent model.

#### Analysis of Data from the 1-in-40 Treatment.

Assuming homogeneity in preferences, we find that the EUT source-dependent model is rejected in favor of the RDU source-dependent model. Conditional on the RDU source-dependent model we find *violations of ROCL*. Table E2 shows estimates for the parameters of the models of interest. A statistical test for the joint null hypothesis that  $\tau_c = \gamma_c=0$  results in a  $p$ -value less than 0.001 whether or not we assume the Power or Inverse-S probability weighting functions. Finally, a test for the joint hypothesis that  $\gamma=1$  and  $\gamma_c=0$  results in a  $p$ -value less than 0.001, whichever probability weighting function is assumed. Thus we reject the hypothesis of that these data are best characterized by the EUT source-dependent model, and conclude that the evidence favors the RDU source-dependent model.

### **Additional References**

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**Table E1: Estimates of Source-Dependent Models Allowing with 1-in-1 Data**

Data from the 1-in-1 treatment (133 observations from 133 subjects  $\times$  1 choice).  
Estimates from the Fechner error parameter omitted.

A. Source-Dependent RDU with Power Weighting Function (LL=-81.82)

	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]	
r	.6182909	.0669022	9.24	0.000	.487165	.7494169
rc	.1410226	.0922965	1.53	0.127	-.0398752	.3219204
$\gamma$	.7699848	.193429	3.98	0.000	.3908709	1.149099
$\gamma c$	-.1955013	.1819329	-1.07	0.283	-.5520832	.1610806

( $H_0$ :  $rc = \gamma c = 0$ ;  $p$ -value = 0.2848)

( $H_0$ :  $\gamma c = 0$  and  $\gamma = 1$ ;  $p$ -value < 0.001)

B. Source-Dependent RDU with Inverted-S Weighting Function (LL=-81.36)

	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]	
r	.4209517	.1168385	3.60	0.000	.1919524	.649951
rc	-.0927005	.1540249	-0.60	0.547	-.3945838	.2091829
$\gamma$	.6768297	.1122897	6.03	0.000	.4567461	.8969134
$\gamma c$	-.1470867	.2065464	-0.71	0.476	-.5519103	.2577369

( $H_0$ :  $rc = \gamma c = 0$ ;  $p$ -value = 0.7634)

( $H_0$ :  $\gamma c = 0$  and  $\gamma = 1$ ;  $p$ -value = 0.0088)

C. Source-Dependent Version of EUT (LL=-84.24)

	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]	
r	.5799425	.073165	7.93	0.000	.4365416	.7233433
rc	.2718091	.1027934	2.64	0.008	.0703378	.4732804

**Table E2: Estimates of Source-Dependent Models with 1-in-40 Data**

Data from the 1-in-40 treatment (2480 observations from 62 subjects  $\times$  40 choices).  
Estimates from the Fechner error parameter omitted.

A. Source-Dependent RDU with Power Weighting Function (LL=-1460.57)

	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]	
r	.5670508	.028896	19.62	0.000	.5104156	.623686
rc	.1090157	.0175052	6.23	0.000	.0747062	.1433252
$\gamma$ c	1.088315	.080862	13.46	0.000	.9298286	1.246802
$\gamma$ c	-.3957716	.0628587	-6.30	0.000	-.5189724	-.2725707

(Ho: rc =  $\gamma$ c = 0;  $p$ -value<0.0001)  
(Ho:  $\gamma$ c = 0 and  $\gamma$  = 1;  $p$ -value < 0.0001)

B. Source-Dependent RDU with Inverted-S Weighting Function (LL=-1462.80)

	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]	
r	.5594736	.0271264	20.62	0.000	.5063069	.6126404
rc	-.0567367	.0410043	-1.38	0.166	-.1371036	.0236301
$\gamma$	1.655437	.1490058	11.11	0.000	1.363391	1.947483
$\gamma$ c	-.9713041	.1425871	-6.81	0.000	-1.25077	-.6918386

(Ho: rc =  $\gamma$ c = 0;  $p$ -value<0.0001)  
(Ho:  $\gamma$ c = 0 and  $\gamma$  = 1;  $p$ -value < 0.0001)

C. Version of EUT (LL=-1512.45)

	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]	
r	.6200649	.0143099	43.33	0.000	.592018	.6481118
rc	.2238311	.0193132	11.59	0.000	.185978	.2616843