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by

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Abstract: Experimental and behavioral economists, as well as psychologists, commonly assume conditional independence of choices when constructing likelihood functions for structural estimation. I test this assumption using data from a new experiment designed for this purpose. Within the limits of the experiment's identifying restriction and designed power to detect deviations from conditional independence, conditional independence is not rejected. In naturally occurring data, concerns about violations of conditional independence are certainly proper and well-taken (for well-known reasons). However, when an experimenter employs contemporary state-of-the-art experimental mechanisms and designs, the current evidence suggests that conditional independence is an acceptable assumption for analyzing data so generated.

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1. Introduction

Conditional independence of observations is a common requirement for well-behaved estimation with relatively simple estimators, such as simple maximum likelihood without any dynamic relationship between observations now and observations in the past. For naturally occurring data (such as most survey data), there is a well-founded concern that sequences of decisions made by the same observational unit are probably not conditionally independent of one another, and much econometric innovation addresses this problem.

Are similar concerns appropriate for laboratory data arising from a sequence of decisions made by the same subject in an individual choice experiment? This is an interesting and, to my knowledge, unexplored question. Much innovation of laboratory methods, based closely on <u>decision-theoretic</u> notions of independence, has taken place over the last forty years. The decision-theoretic design of laboratory mechanisms, such as the <u>random problem selection</u> or RPS mechanism, proceeds from decision theoretic independence axioms of various kinds. When an experimenter employs such mechanisms, she means to make a choice now "independent" of decision problems her subject has already encountered in the laboratory session, but in a decision-theoretic sense of the word "independence."

A long history of experimental work (beginning perhaps with Starmer and Sugden 1991 and continuing through Brown and Healy 2018) examines these decision-theoretic senses of independence (or as Brown and Healy wish to frame this, the <u>statewise monotonicity axiom</u> discussed by Azrieli et al. 2018). In statistical terms, that long experimental literature focused on the behavior of <u>marginal</u> choice probabilities within a mechanism, asking whether the presence or absence of other decision problems (within the mechanism) affected observed choice proportions in a given decision problem.

The econometric and statistical sense of the term "conditional independence" concerns conditional, not marginal, choice probabilities. Yet decision-theoretic axioms such as the <u>compound independence axiom</u> or CIA of expected utility and other theories, or the statewise monotonicity axiom, do suggest that, in sequences of decision problems embedded within the RPS mechanism, a choice now should not only be independent of previous decision problems but also independent of previous choices. Therefore I ask whether or not conditional independence (in its econometric and statistical sense) appears to be satisfied in a state-of-the-art decision making experiment. Such experiments employ the RPS mechanism as well as other features that seem necessary to obtain an empirical version of decision theoretic independence (which I define shortly and call behavioral incentive compatibility or BIC).

Related work by Hey and Lee (2005a, 2005b) and Hey and Zhou (2014) tests whether subjects appear to be optimizing one grand function of all decisions across all or some trials (a sufficient condition for conditional dependence) and those tests suggest that subjects are not doing that. But conditional dependence could arise from other sources such as autocorrelated random preference parameter processes. The tests of Hey and Lee, and Hey and Zhou, also depend on assumed structural models of risk preference. The test I conduct here will depend on an identifying restriction, but make no assumptions concerning any specific underlying preference structure.

Within the limits of the experiment's identifying restriction and designed power to detect deviations from conditional independence, conditional independence is not rejected. A substantial number of scholars may breathe a sigh of relief at this since it has been very common practice to assume conditional independence when constructing likelihood functions for the estimation and analysis of structural preferences from laboratory data (e.g. Hey and Orme 1994; Loomes et al. 2002; Andersen et al. 2008; Rieskamp 2008; Wilcox 2008, 2011). My experimental results here suggest this has not been mistaken practice.

2. Definition of an experiment and features of contemporary state-of-the-art experiments

Here an experiment $\mathcal{E} = \langle \Omega_1^i, \Omega_2^i, \dots, \Omega_J^i \rangle$ means a sequence of trials $j \ = \{1,2,\dots,J\}$ where each subject $i \in \{1,2,\dots,I\}$ chooses from a basic pair $\Omega_j^i = \{R_j^i, S_j^i\}$ of lotteries. Let $c_j^i = 1$ if subject i chooses R_j^i from Ω_j^i and $c_j^i = 0$ if she chooses S_j^i from Ω_j^i. A lottery R_j means a one-stage probability distribution (r_{lj}, r_{mj}, r_{hj}) over a vector (l_j, m_j, h_j) of three possible money outcomes $z \in \mathbb{R}^+$ where $l_j < m_j < h_j$. A one-stage probability distribution is a probability measure of three exhaustive and mutually exclusive events, determined by one (and only one) simple random device such as a single throw of a six-sided die (as employed in my experiment). This rules out resolution of uncertainty by means of a sequence of two or more simple random devices (it rules out multi-stage probability distributions). By a basic pair $\Omega_j = \{R_j, S_j\}$ of lotteries, I mean a pair where neither lottery first-order stochastically dominates the other. Henceforth a pair always means a basic pair.

Within each pair $\Omega_j = \{R_j, S_j\}$, R_j is relatively risky compared to the relatively safe S_j, meaning $s_{mj} > r_{mj}, r_{lj} > s_{lj}$, and $r_{hj} > s_{hj}$: R_j has higher probabilities of the low and high outcomes l_j and h_j, while S_j has a higher probability of the middle outcome m_j. This conventional terminology is only descriptive (it carries no normative implication).

In an experiment, each page (in the case of a physical booklet presentation) or each screen (in the case of a computer presentation) presents exactly one pair: Call this feature separated decisions or SED. An experiment also features the random problem selection or RPS mechanism to motivate subjects without creating unwanted portfolio or wealth effects across the trial sequence. After all J choices have been made by subject i, a random device selects just one trial j^* (every trial has an equal J^{-1} chance of selection). Then subject i plays out only her chosen lottery in trial j^* using a second random device, and this is her sole payment from her choices. Subjects may also receive a fixed payment simply for showing up on time for an experiment but this is not connected to the choices they make.

Under either the compound independence axiom (CIA) of expected utility and other theories (Segal 1990), the isolation effect of prospect theory (Kahneman and Tversky 1979), or the statewise monotonicity axiom defined by Azrieli et al. (2018), experiments featuring RPS should achieve what I call behavioral incentive compatibility or BIC. Consider a J pair experiment $\mathcal{E} = \langle \Omega_1^i, \Omega_2^i, \dots, \Omega_j^i, \dots, \Omega_J^i \rangle$ and a one pair experiment $\mathcal{E}^\circ = \langle \Omega_{1^\circ}^i \rangle$ where $\Omega_{1^\circ}^i \equiv \Omega_j^i$ are the same pair: BIC holds iff $P(R_j^i) = P(R_{1^\circ}^i)$, where $P(R_j^i) \equiv P(c_j^i = 1)$ is the marginal probability that subject i chooses R_j^i from Ω_j^i. Put differently, BIC holds when the choice probability $P(R_j^i)$ in a J pair experiment equals the choice probability $P(R_{1^\circ}^i)$ in an experiment presenting only that pair.

Current evidence fails to reject BIC when all alternatives are lotteries in basic pairs and the experiment features both SED and RPS (Brown and Healy 2018 show this and discuss past evidence). Baltussen et al. 2012 show that BIC can fail when trials are not choices from lottery pairs (in particular, where each trial is a sequence of decisions in a multi-stage risky choice game); and both Harrison and Swarthout (2014) and Cox et al. (2015) show that BIC can fail without SED. Therefore my new experiment features lotteries in basic pairs, SED, and RPS, which I regard as the current "state-of-the-art" for obtaining BIC.

3. Purpose of the new experiment

When we write probabilities $P(R_j^i)$ of choice events to build likelihood functions for preference estimation, these obviously condition on offered pairs. The simplest model $P(R_j^i) = f^i(\Omega_j^i)$ <u>only</u> conditions on the offered pair and the subject. This <u>conditional independence</u> assumption greatly simplifies construction of the likelihood of a choice sequence $c^i = (c_1^i, c_2^i, \dots, c_J^i)$ and minimizes the number of parameters to be estimated. Behavioral economists (and psychologists) widely make this assumption for likelihood-based analysis of choice sequences (e.g. Hey and Orme 1994; Loomes et al. 2002; Andersen et al. 2008; Rieskamp 2008; Wilcox 2008, 2011). In general choices may be <u>conditionally dependent</u>: True choice probabilities would then be $P(R_j^i) = g^i(\Omega_j^i, c_{j-1}^i, c_{j-2}^i, \dots, c_1^i) \not\equiv f^i(\Omega_j^i)$. Here, I test the null hypothesis of conditional independence against an alternative hypothesis of restricted conditional dependence $P(R_j^i) = g^i(\Omega_j^i, c_{j-1}^i)$ that informs my experimental design, power planning (detailed in the Appendix), and data analysis.

Henceforth I suppress explicit conditioning on Ω_j^i, taking it as implicit that all choice probabilities are conditioned on the offered pair. Thus $P(R_j^i | R_{j-1}^i)$ will mean $g^i(\Omega_j^i, 1)$ and $P(R_j^i | S_{j-1}^i)$ will mean $g^i(\Omega_j^i, 0)$, while $P(R_j^i)$ written without any condition will mean the marginal probability that $c_j^i = 1$ given that subject i chooses from pair Ω_j^i.

4. Design of this experiment

Let t and $\tau \in \{1,2, \dots ,50\}$ index two sequences of 50 choice pairs, the <u>t sequence</u> (with pairs indexed by t) and the <u>τ sequence</u> (with pairs indexed by τ). The design presents each subject with these two sequences, for $J = 100$ total choice pairs. The order of presentation of the t and τ sequences is varied across subjects: Let $\mathcal{O}_1$ and $\mathcal{O}_2$ denote sets of subjects i

who received the t sequence or τ sequence first, respectively. The two sequences are separated by a short unpaid survey (as described below, the survey just gives subjects a short break between the sequences; responses to survey questions are of no interest here).

Both sequences contain 12 target pairs $\mathcal{T} = \{10,13,16,19,22,25,28,31,34,37,40,43\}$ in exactly the same place within each sequence, for a test and (fifty pairs later) a retest of choice from each target pair. Target pairs are identical across the two sequences. For example target pairs $t = 10$ and $\tau = 10$ are exactly the same choice pair.

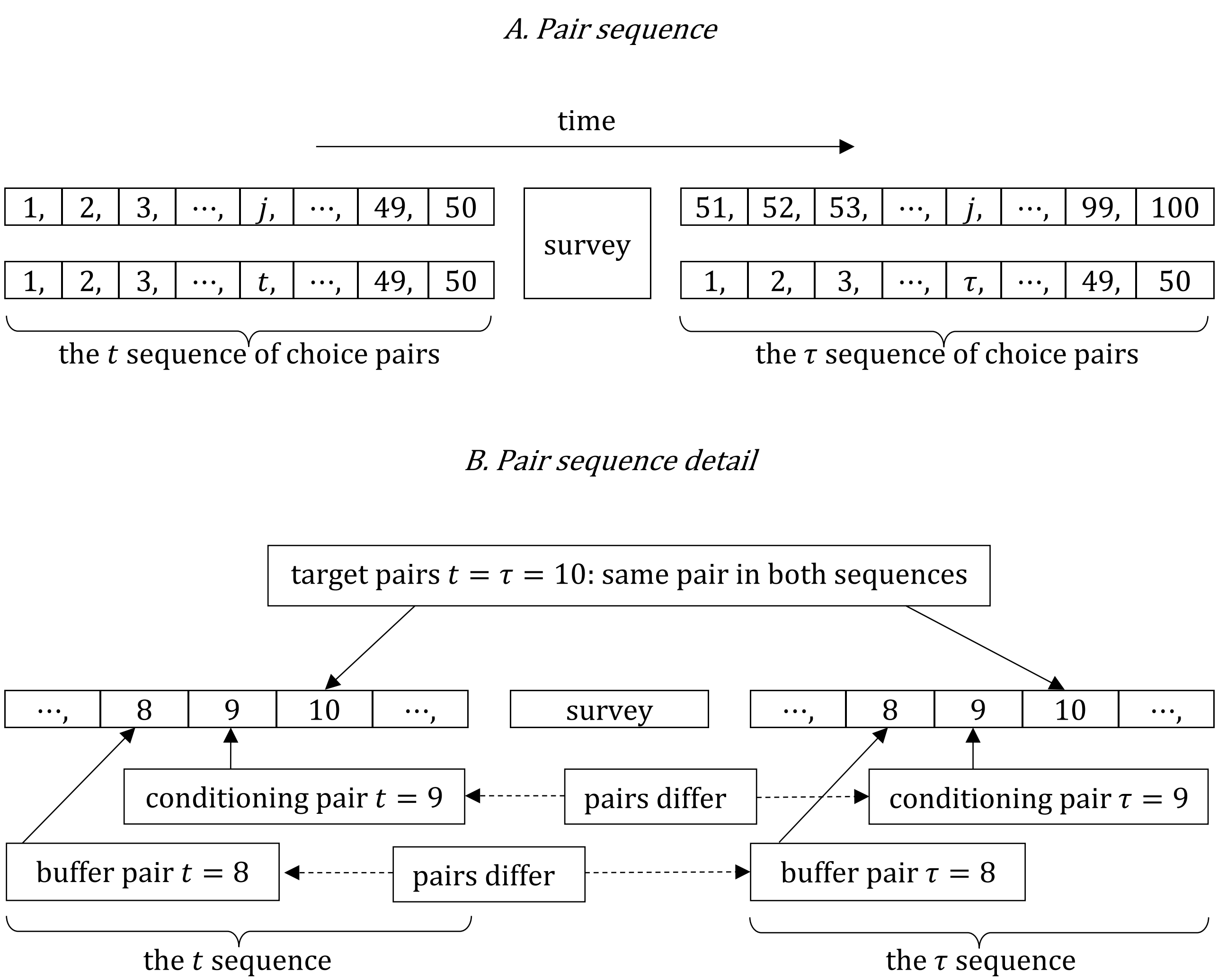
Conditioning pairs $\{9,12,15,18,21,24,27,30,33,36,39,42\}$ immediately precede each target pair. These pairs differ across the t and τ sequences. For example, conditioning pairs $t = 9$ and $\tau = 9$ (presented just before the common target pair $t = \tau = 10$) are different choice pairs: In pair $t = 9$, R_t is more attractive than S_t (call this a high conditioning pair) for most subjects, while in pair $\tau = 9$ S_τ is more attractive than R_τ(call this a low conditioning pair) for most subjects. This manipulation makes it likely that any subject comes to the two presentations of identical target pair $t = \tau = 10$ with two different choice histories (different choices at $t = \tau = 9$). Similarly for each $t = \tau \in \mathcal{T}$ a high conditioning pair immediately precedes t or τ while a low conditioning pair immediately precedes the other matched target pair. Table 1 shows that this manipulation was largely successful.

Table 1: Choice percentages (of 204 subjects) in conditioning pairs

high conditioning pairs meant to induce choice of R (risky)		low conditioning pairs meant to induce choice of S (safe)	
pair index	percentage R (risky)	pair index	percentage S (safe)
$t = 9$	81.37	$t = 12$	87.75
$t = 15$	79.90	$t = 18$	83.82
$t = 21$	90.20	$t = 24$	92.65
$t = 27$	84.80	$t = 30$	90.69
$t = 33$	90.69	$t = 36$	99.02
$t = 39$	78.92	$t = 42$	87.75
$\tau = 12$	88.24	$\tau = 9$	68.63
$\tau = 18$	92.65	$\tau = 15$	77.45
$\tau = 24$	92.16	$\tau = 21$	95.10
$\tau = 30$	88.73	$\tau = 27$	65.20
$\tau = 36$	91.67	$\tau = 33$	87.75
$\tau = 42$	87.75	$\tau = 39$	86.76
average	87.26	average	85.21

Figure 1 illustrates the overall trial sequence in the experiment. Notice the additional presence of buffer pairs which serve several design purposes. First, a buffer pair separates each pair of a conditioning and target pair from the next such pair of pairs (see panel B of Figure 1). Second, both the t and τ sequences begin and end with seven buffer pairs. This gives subjects a (short) warm-up prior to presentation of pairs of conditioning and target pairs and additionally keeps these away from the ends of sequences (when subjects might begin relaxing their concentration). Appendix Table A1 lists all of the choice pairs, and this online supplement contains screen prints of the experiment's computerized instructions.

Figure 1: The experiment sequence for the subjects $i \in \mathcal{O}_1$ (receiving the t sequence first).



5. Hypotheses and data analysis

In eqs. 1, 2, 3 and 4 below I index pairs by locations $j = k = m \in \mathcal{T}$, with exactly one of j or k in the t sequence and the other in the τ sequence. That is, both j and k are the same target pair location m, one in the t sequence and the other in the τ sequence and, for the time being, which is which remains unspecified. The experimental design implies that one of j or k follows a high conditioning pair while the other follows a low conditioning pair. With all this in mind, conditionally independent and identically distributed trials imply that

$$(1) \qquad P\left(R_j^i \cap R_{j-1}^i\right)/P\left(R_{j-1}^i\right) \equiv P\left(R_j^i \middle| R_{j-1}^i\right) = P\left(R_k^i | S_{k-1}^i\right) \equiv P\left(R_k^i \cap S_{k-1}^i\right)/P\left(S_{k-1}^i\right).$$

Rearrange the left-most and right-most terms of eq. 1 to get the null hypothesis

$$(2) \qquad H_0\text{: } P\left(R_j^i \cap R_{j-1}^i\right)P\left(S_{k-1}^i\right) - P\left(R_k^i \cap S_{k-1}^i\right)P\left(R_{j-1}^i\right) = 0.$$

To test this null, define these twelve data-derived within-subject differences for each subject i:

$$(3) \qquad y_m^i = \mathbf{1}\left(c_j^i = 1 \cap c_{j-1}^i = 1\right) \cdot \mathbf{1}\left(c_{k-1}^i = 0\right) - \mathbf{1}\left(c_k^i = 1 \cap c_{k-1}^i = 0\right) \cdot \mathbf{1}\left(c_{j-1}^i = 1\right).$$

Adopt the <u>indexing convention</u> that, when it is possible to do so, the target pair indices j and k are assigned to the t and τ sequences so that $c_{j-1}^i = 1$ and $c_{k-1}^i = 0$. (Notice that whenever this is not possible, $y_m^i = 0$ regardless of the assignment of those indices.) The design's conditioning pair features are meant to make $\left(R_{j-1}^i \cap S_{k-1}^i\right)$ a likely event in the data for $j = k = m \in \mathcal{T}$. Table 2 shows the experiment's joint distributions of safe and risky choices in pairs of high and low conditioning pairs: The sum of the off-diagonal cells in these tables give the percent of subjects for whom j and k can be assigned such that events $\left(R_{j-1}^i \cap S_{k-1}^i\right)$ occur and shows that these are common in the data, as intended.

Table 2: Empirical joint distribution of choices in high and low conditioning pairs (percentages of 204 subjects).

		low conditioning pair choice					
		safe	risky	safe	risky	safe	risky
		$t = \tau = 9$		$t = \tau = 12$		$t = \tau = 15$	
	safe	11.76	6.86	9.80	1.96	15.20	4.90
	risky	56.86	24.51	77.94	10.29	62.25	17.65
		$t = \tau = 18$		$t = \tau = 21$		$t = \tau = 24$	
	safe	7.35	0.00	9.80	0.00	7.35	0.49
high conditioning pair choice	risky	76.47	16.18	85.29	4.90	85.29	6.86
		$t = \tau = 27$		$t = \tau = 30$		$t = \tau = 33$	
	safe	14.71	0.49	9.80	1.47	8.33	0.98
	risky	50.49	34.31	80.88	7.84	79.41	11.27
		$t = \tau = 36$		$t = \tau = 39$		$t = \tau = 42$	
	safe	8.33	0.00	18.14	2.94	10.78	1.47
	risky	90.69	0.98	68.63	10.29	76.96	10.78

To know the expected value of each y_m^i, I need an identifying restriction:

<u>Identifying Restriction</u>: $R_j^i \cap R_{j-1}^i$ and S_{k-1}^i are conditionally independent, and $R_k^i \cap S_{k-1}^i$ and R_{j-1}^i are conditionally independent.

This identifying restriction is implied by both the null and alternative hypotheses. Beyond the specifics of the null and alternative hypotheses, the restriction requires that at a remove of fifty trials there is no dependence between the test and the retest of the same target pair and the conditioning pairs preceding them. The design's survey break between the t and τ sequences is meant to enhance the plausibility of this "no memory" assumption between the two sequences. Under this assumed restriction,

(4) $$E[y_m^i] = P(R_j^i \cap R_{j-1}^i)P(S_{k-1}^i) - P(R_k^i \cap S_{k-1}^i)P(R_{j-1}^i).$$

Therefore, defining the observation from each subject i as $y^i = \frac{1}{12}\sum_{m\in\mathcal{T}} y_m^i$, a one-sample test against a zero location of the y^i tests the null of eq. 2 against the alternative of conditional dependence.

Given the construction of y^i detailed above (especially the indexing convention), nonzero values of y^i are evidence favoring one of two alternatives. When $y^i > 0$, relatively risky choices are <u>more</u> common when preceded by a relatively risky choice than when preceded by a relatively safe choice: On average we observe <u>persistence</u> of the choices of subject i. When $y^i < 0$, relatively risky choices are <u>less</u> common when preceded by a relatively risky choice than when preceded by a relatively safe choice: On average we observe <u>alternation</u> of the choices by subject i.

A simple one-parameter <u>odds ratio model</u> of conditional dependence (e.g. Lipsitz et al. 1991; Carey et al. 1993) captures both possibilities (persistence or alternation) and this model motivated the experimental design and informed my power analysis of the design. The Appendix contains that power analysis, which is for a two-tailed t-test against the null hypothesis of eq. 2, at a size of 5%, given effect sizes described in the Appendix. To obtain power of 90%, the analysis recommends a sample size of 200 subjects. The actual sample size is 204 subjects i, with half in the $\mathcal{O}_1$ pair ordering and the other half in the $\mathcal{O}_2$ ordering.

The above construction of the null hypothesis and the observation y^i for testing it assumes not only conditional independence but identically distributed trials of target pair choices across the t and τ sequences. The design's balanced variation of presentation order of the t and τ sequences should offset any <u>simple drift</u> toward either more risky or more safe choices as trials progress. However, simple drift is a finding of some experiments (e.g. Hey and Orme 1994; Ballinger and Wilcox 1997; Loomes and Sugden 1998) so to check for it define the observation

(5) $$x^i = \frac{1}{12}\left[\mathbf{1}(i\epsilon\mathcal{O}_1)\sum_{t=\tau\in\mathcal{T}}(c_t^i - c_\tau^i) + \mathbf{1}(i\epsilon\mathcal{O}_2)\sum_{t=\tau\in\mathcal{T}}(c_\tau^i - c_t^i)\right],$$

which is just the difference between observed risky choices of subject i in her first and second trials of target pairs. Figure 2 displays the empirical cumulative distribution function of x^i across the experiment's 204 subjects. The sample mean of x^i and that mean's

Figure 2: Empirical cumulative distribution function of x^i across 204 subjects.

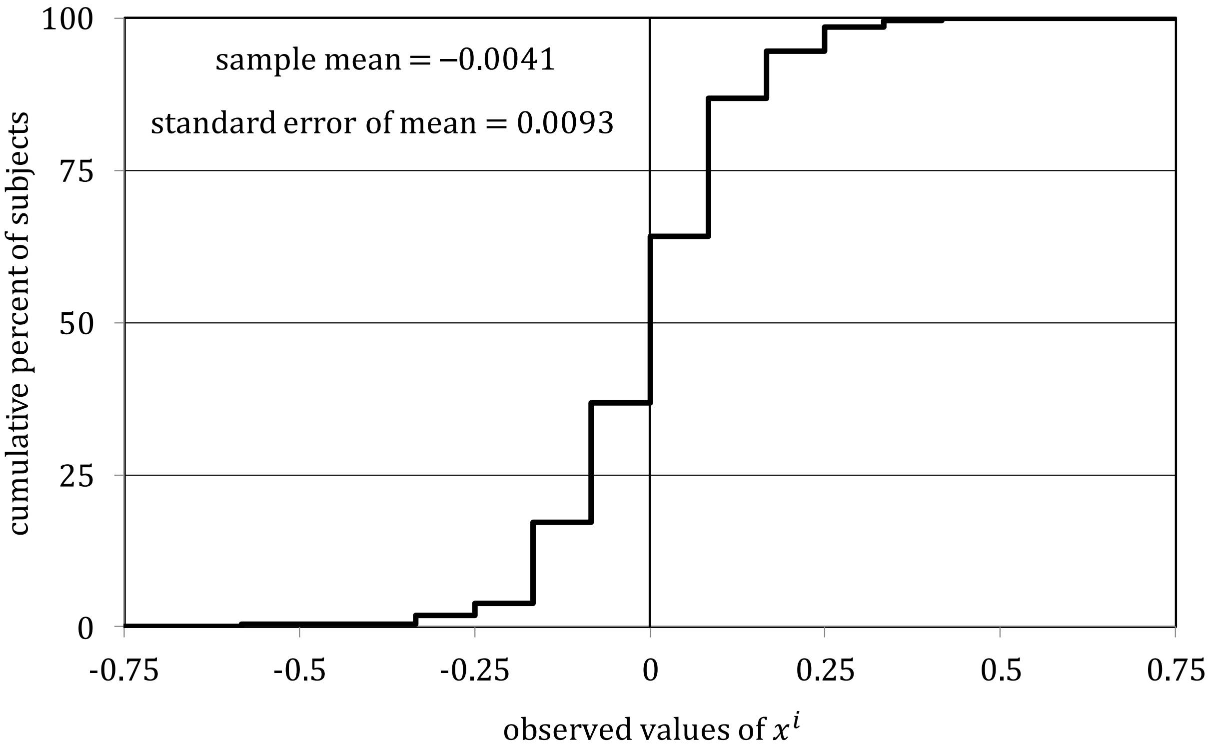
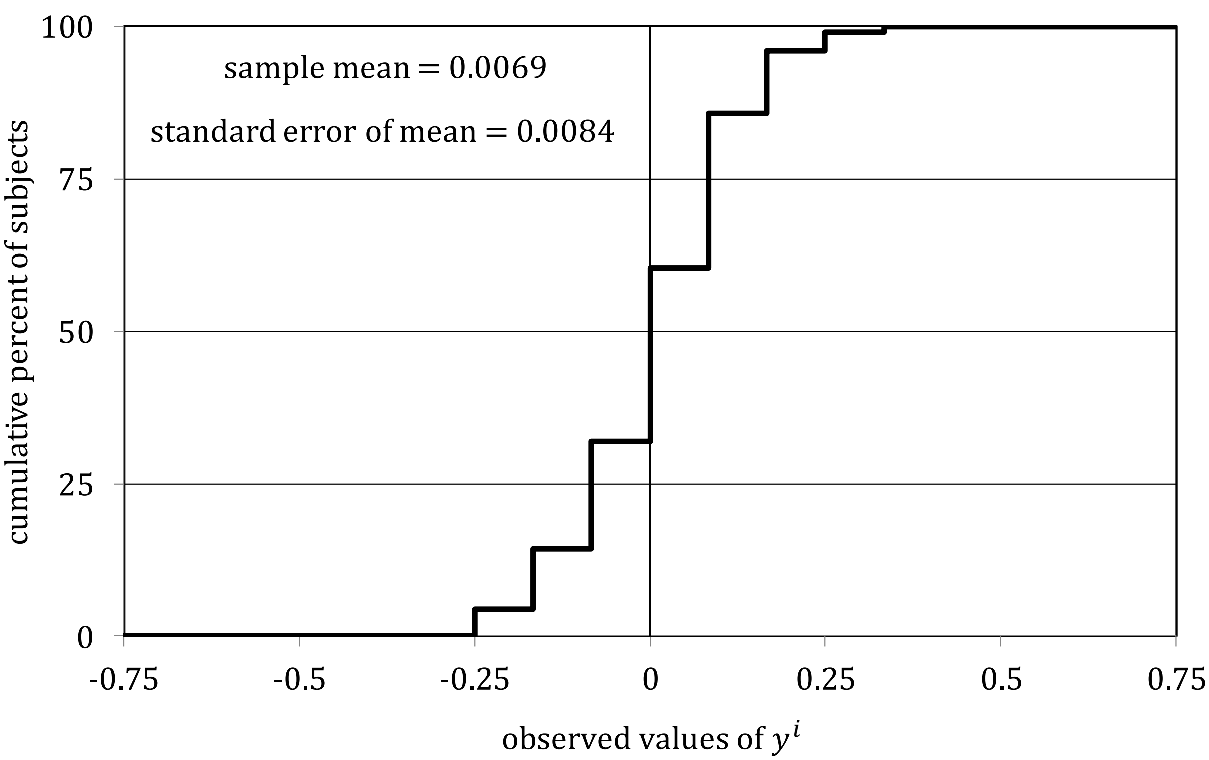


Figure 3. Empirical cumulative distribution function of y^i across 204 subjects.



standard error are –0.0041 and 0.0093, respectively, suggesting an absence of significant simple drift in the new experiment.

Figure 3 displays the empirical cumulative distribution function of y^i across the experiment's 204 subjects. The sample mean of y^i and that mean's standard error are 0.0069 and 0.0084, respectively, yielding a t-statistic with absolute value less than one, so there is no significant violation of conditional independence in the new experiment. The statistic is positive, suggesting that if there is any conditional dependence here, it is perhaps a bit of persistence of choice.

6. Conclusions

It appears that when an experimenter uses state-of-the-art experimental mechanisms and features in an individual choice experiment, conditional independence of observed choices is an acceptable assumption. To my knowledge, the new experiment reported here is the first direct test of conditional independence, though the tests reported by Hey and Lee (2005a, 2005b) and Hey and Zhou (2014) certainly weigh in favor of conditional independence as well. And perhaps this does not need emphasis, but neither my evidence nor that of Hey and his co-authors says anything at all about other decision experiments where choice problems are not basic pairs of one-stage lotteries, or RPS and SED are not features of the experiment. Nor does this evidence say anything about other sorts of experiments such as multiperiod games or markets. Other scholars could investigate the status of conditional independence in these other kinds of experiments.

My data analysis and experimental design depended on two things: First, no drift in choice probabilities across the two trials of my target choice pairs (which appears to be empirically acceptable); and second, an identifying restriction—in essence that at a remove of about fifty intervening trials there is no conditional dependence. I have no test of that assumption, but believe it is defensible. The two oldest facts from human memory research are the primacy and recency effects. The recency effect suggests that if there is any conditional dependence, we should probably expect to detect it in recently past choices (say one or two trials ago) rather than at a remove of fifty trials past. The primacy effect is that the earliest events or stimuli in a sequence are more likely to be remembered. My experimental design pads the front end of each choice sequence (the earliest trials, most exposed to any primacy effect) with seven buffer pairs not used in my test. However, I accept that there is room for doubt about my identifying restriction.

Behavioral econometricians and psychometricians frequently assume conditional independence when they construct their likelihood functions for structural estimation of preferences from discrete choice sequences observed in the lab. They may take some comfort from my results—assuming, of course, that their experiment employs RPS, and SED, and that their subjects' choices are from pairs of one-stage lotteries. For the rest, we await new experiments testing conditional independence in other experimental situations.

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Appendix

An odds ratio model (Lipsitz et al. 1991; Carey et al. 1993) of restricted conditional dependence guided my power analysis for designing the experiment:

<u>Constant odds ratio of four joint probabilities</u> parameterized by the constant $\gamma > 0$:

(A1) $\gamma = P\left(R_j^i \cap R_{j-1}^i\right) \cdot P\left(S_j^i \cap S_{j-1}^i\right) / \left[P\left(R_j^i \cap S_{j-1}^i\right) \cdot P\left(S_j^i \cap R_{j-1}^i\right)\right] > 0.$

<u>The four joint probabilities add up to unity</u> (probability theory identity):

(A2) $1 = P\left(R_j^i \cap R_{j-1}^i\right) + P\left(S_j^i \cap S_{j-1}^i\right) + P\left(R_j^i \cap S_{j-1}^i\right) + P\left(S_j^i \cap R_{j-1}^i\right).$

<u>Pairs of joint probabilities add up to marginal probabilities</u> (probability theory identities):

(A3) $P\left(R_j^i\right) = P\left(R_j^i \cap R_{j-1}^i\right) + P\left(R_j^i \cap S_{j-1}^i\right)$ and
(A4) $P\left(R_{j-1}^i\right) = P\left(R_j^i \cap R_{j-1}^i\right) + P\left(S_j^i \cap R_{j-1}^i\right).$

With given values of γ, $P\left(R_j^i\right)$, and $P\left(R_{j-1}^i\right)$ in hand, Eqs. A1 to A4 imply the following quadratic equation in $P\left(R_j^i \cap R_{j-1}^i\right)$:

(A5) $(\gamma - 1)\left[P\left(R_j^i \cap R_{j-1}^i\right)\right]^2 + \alpha_j^i P\left(R_j^i \cap R_{j-1}^i\right) + \beta_j^i = 0,$ where
$\alpha_j^i = (1-\gamma)\left[P\left(R_j^i\right) + P\left(R_{j-1}^i\right)\right] - 1$ and $\beta_j^i = \gamma P\left(R_j^i\right) \cdot P\left(R_{j-1}^i\right).$

When $\gamma \neq 1$, the quadratic formula gives roots of this equation. Only one root is well-behaved in the sense that the solution $P\left(R_j^i \cap R_{j-1}^i\right)$ is always in $[0,1]$ $\forall\, \gamma \neq 1$): It is

(A6) $P\left(R_j^i \cap R_{j-1}^i\right) = -0.5 \cdot \left(\alpha_j^i + \left[\left(\alpha_j^i\right)^2 - 4(\gamma - 1)\beta_j^i\right]^{0.5}\right)(\gamma - 1)^{-1}\ \forall\, \gamma \neq 1,$ and
$P\left(R_j^i \cap R_{j-1}^i\right) = P\left(R_j^i\right) \cdot P\left(R_{j-1}^i\right)$ for $\gamma = 1.$

The solution from eq. A6 allows a sequential solution for the other three joint probabilities using eqs. A2, A3 and A4:

(A7) $P(R_j^i \cap S_{j-1}^i) = P(R_j^i) - P(R_j^i \cap R_{j-1}^i)$,

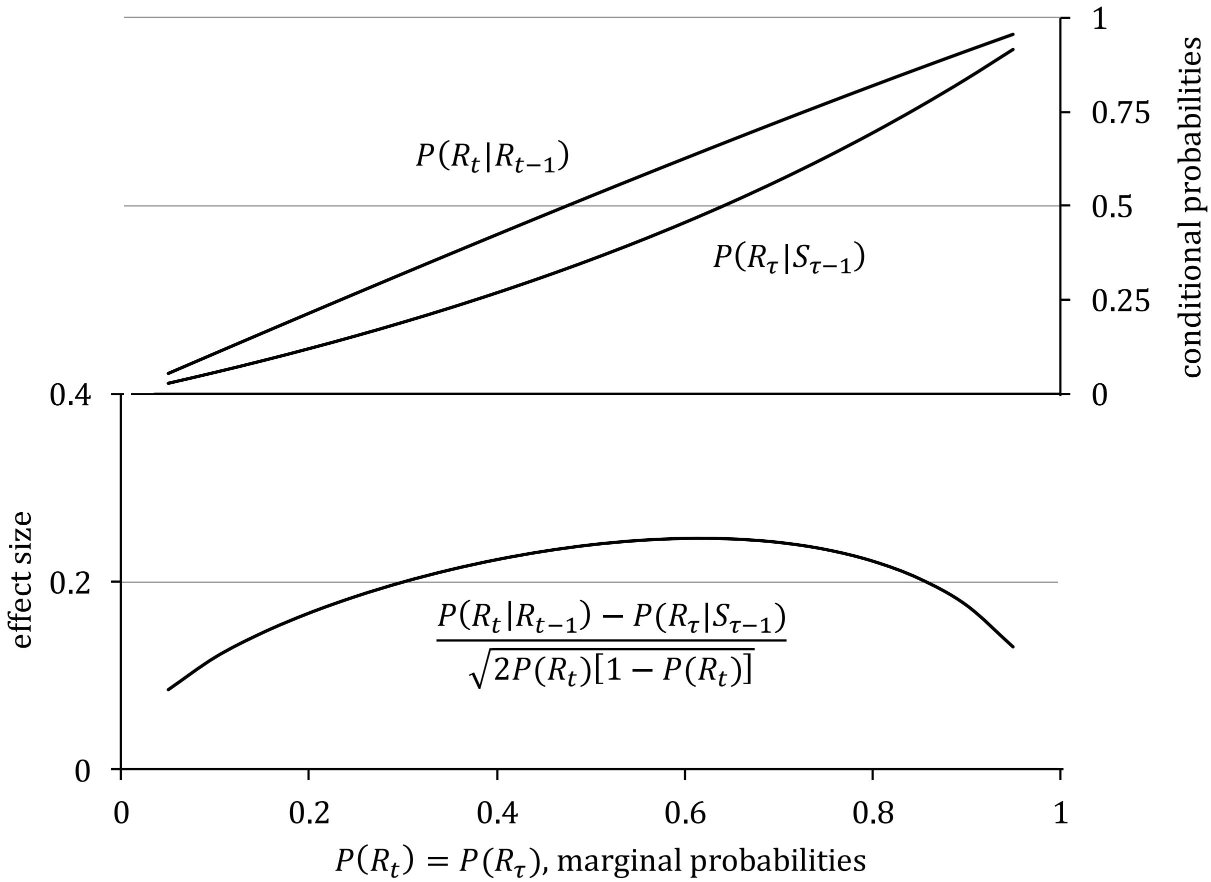
(A8) $P(S_j^i \cap R_{j-1}^i) = P(R_{j-1}^i) - P(R_j^i \cap R_{j-1}^i)$, and

(A9) $P(S_j^i \cap S_{j-1}^i) = 1 - P(R_j^i \cap R_{j-1}^i) - P(R_j^i \cap S_{j-1}^i) - P(S_j^i \cap R_{j-1}^i)$.

In turn, with given values of $P(R_j^i)$ and $P(R_{j-1}^i)$ in hand, Eqs. A6 and A7 then give solutions for the key conditional probabilities $P(R_j^i|R_{j-1}^i)$ and $P(R_j^i|S_{j-1}^i)$ given any value of γ one wishes to specify as an interesting alternative hypothesis. The upper panels of Figures A1 and A2 graph these conditional probabilities for a $t = \tau \in \mathcal{T}$ target pair where $t-1$ is a high conditioning pair with $P(R_{t-1}^i) = 0.85$ and $\tau - 1$ is a low conditioning pair with $P(S_{\tau-1}^i) = 0.85$ (approximately reflecting the average results for conditioning pairs shown in Table 1). Figure A1 assumes that $\gamma = 2$ yielding <u>persistence</u> so that $P(R_t^i|R_{t-1}^i) - P(R_\tau^i|S_{\tau-1}^i) > 0$ at any common marginal probability $P(R_t^i) = P(R_\tau^i)$ (shown on the horizontal axis). Figure A2 instead assumes that $\gamma = 0.5$ yielding <u>alternation</u> so that $P(R_\tau^i|S_{\tau-1}^i) - P(R_t^i|R_{t-1}^i) > 0$ at any common marginal probability $P(R_t^i) = P(R_\tau^i)$.

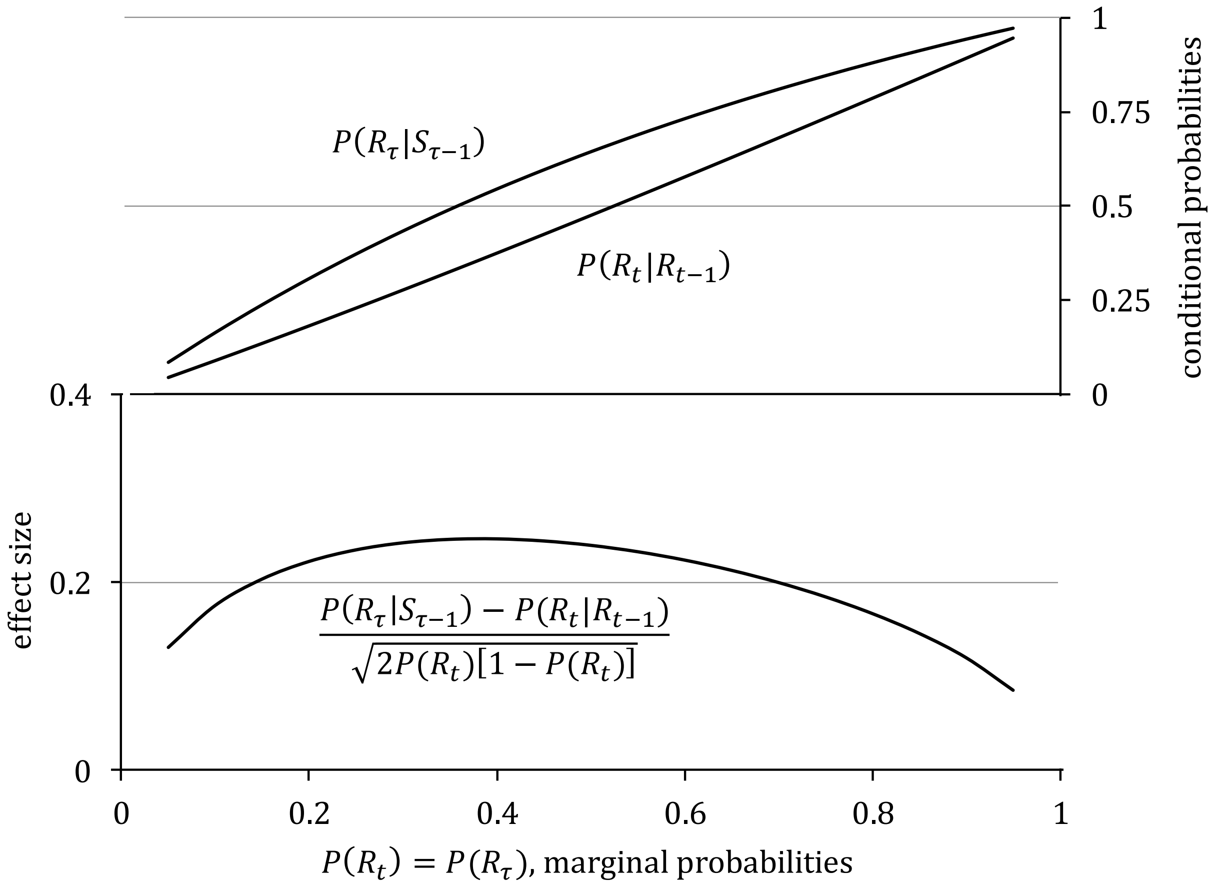
The lower panels of Figures A1 and A2 graph corresponding <u>effect sizes</u>. For example, to draw the lower panel of Figure A1, one divides the difference $P(R_t^i|R_{t-1}^i) - P(R_\tau^i|S_{\tau-1}^i)$ under the alternative hypothesis $\gamma = 2$ by the standard deviation $\left(2P(R_t^i)[1-P(R_t^i)]\right)^{0.5}$ of that difference under the null hypothesis that $P(R_t^i|R_{t-1}^i) = P(R_\tau^i|S_{\tau-1}^i) = P(R_t^i)$ (which is $\gamma = 1$). The figures reveal that these effect sizes are on the small side. Cohen's (1988) convention for these kinds of effect sizes calls 0.2 and 0.5 <u>small</u> and <u>medium</u> effect sizes, and those in the figures never quite reach 0.25 regardless of the common marginal probability $P(R_t^i) = P(R_\tau^i)$. This is one reason for the repeated measurement of the design (that is, why there are twelve pairs of target and conditioning pairs in each sequence, providing twelve values y_m^i which are then averaged within each subject to yield overall observations y^i for each subject i).

Figure A1. Conditional probabilities and effect size implied by $P\left(R_{t-1}^{i}\right) = P\left(S_{\tau-1}^{i}\right) = 0.85$ and persistence ($\gamma = 2$).



The figures also reveal an asymmetry relevant to the experimental design. Under the alternative hypothesis of persistence ($\gamma = 2$) the range of marginal probabilities achieving effect sizes of at least 0.2 is about 0.30 to 0.85. But under the alternative hypothesis of alternation ($\gamma = 0.5$), the range of marginal probabilities achieving effect sizes of at least 0.2 is about 0.15 to 0.70. The compromise range most useful for both alternative hypotheses is to (try to) choose target pairs with marginal probabilities in a range from about 0.30 to 0.70. On the other hand, some marginal probabilities outside this range are among those most useful for estimation of preferences (Manski and McFadden 1981; Kanninen 2002). In this design, I attempted to choose target pairs which, on the basis of past results with the population I sample from (more on this appears presently), would have population mean probabilities falling across most of the unit interval. Half of the

Figure A2. Conditional probabilities and effect size implied by $P\left(R_{t-1}^{i}\right) = P\left(S_{\tau-1}^{i}\right) = 0.85$ and alternation ($\gamma = 0.5$).



twelve target pair tests and retests fall within the range from 0.30 to 0.70 mentioned above, with the other half more extreme.

An estimate of the distributions of marginal and conditional choice probabilities in the population I sample from helps with selecting a reasonable sample size. I have a previous experiment with a sample of 501 undergraduate subjects from my university, each choosing from 72 lottery pairs on the outcome range \$8 to \$48, using a 4-sided die as the chance device. This unpublished experiment was completed in January 2010 in collaboration with the late John Dickhaut. Using this data and assuming conditional independence, I estimated a random parameters Rank Dependent Utility or RDU model (Quiggin 1982, 1993). RDU is essentially the same as Tversky and Kahneman's (1992) Cumulative Prospect Theory limited to lotteries over gains. This yields an estimated distribution of preference parameter vectors θ in the population of likely subjects at my university.

With this estimation completed, I draw 1000 simulated subjects indexed by $n \in \{1,2, \dots ,1000\}$ from my estimated distribution of RDU parameters, and for design planning purposes I regard these 1000 simulated subjects as "the population" I sample from when I run an experiment. Each simulated subject is a vector $\theta^n = (\kappa^n, \mu^n, \omega^n, \lambda^n)$ of four probabilistic RDU model parameters described below.

The parameter $\kappa^n \in \mathbb{R}$ is utility curvature in this HARA utility function:

(A10) $\quad u(z|\kappa^s) = (1-\kappa^n)^{-1}\left[-1 + (1+z)^{(1-\kappa^n)}\right]$ for $\kappa^n \neq 1$, $\ln(1+z)$ for $\kappa^n = 1$.

The parameters $\mu^n \in (0,1)$ and $\omega^n \in (0,\infty)$ are elevation and curvature parameters of this "Beta" weighting function:

(A11) $\quad w(G|\mu^n, \omega^n) = B(G|a^n, b^n)$ where $a^n = \mu^n\omega^n$, $b^n = (1-\mu^n)\omega^n$,
G is decumulative probability in a lottery, and
$B(x|a^n, b^n)$ is the cumulative distribution function of the Beta distribution.

The parameter $\lambda^n \in (0,\infty)$ is a precision or sensitivity parameter of the probabilistic RDU model of choice I use in the random parameters estimation.

The RDU model of marginal probabilities is then

(A12) $\quad P\left(R_j^n|\theta^n\right) = \Lambda\left(\lambda^n \Delta RDU_j^n\right)$ where $\Delta RDU_j^n = RDU\left(R_j^n\right) - RDU(S_j^n)$,
$\Lambda(x) = [1+\exp(x)]^{-1}$ is the logistic cumulative distribution function,
$RDU\left(R_j^n\right) = \pi_{hj}(\mu^n, \omega^n) + \pi_{mj}(\mu^n, \omega^n)v_j(m_j|\kappa^n)$,
$\pi_{hj}(\mu^s, \omega^s) = w\left(r_{hj}\middle|\mu^n, \omega^n\right)$, $\pi_{mj}(\mu^n, \omega^n) = w\left(r_{hj} + r_{mj}\middle|\mu^n, \omega^n\right) - w\left(r_{hj}\middle|\mu^n, \omega^n\right)$,
and $v_j\left(m_j|\kappa^n\right) = \left[u\left(m_j|\kappa^n\right) - u\left(l_j|\kappa^n\right)\right]/\left[u\left(h_j|\kappa^n\right) - u\left(l_j|\kappa^n\right)\right]$.

This specification of marginal RDU choice probabilities employs the contextual utility probabilistic choice model of Wilcox (2008, 2011) which is appropriate for three-outcome pairs of lotteries. These marginal probabilities, calculated for all $J = 100$ pairs in the design for each of the 1000 simulated subjects n, are the choice probabilities under the null hypothesis $\gamma = 1$. Conditional choice probabilities may be calculated from them by way of eqs. A6 and A7 for any assumed value of $\gamma \neq 1$, providing choice probabilities under any alternative hypothesis.

Monte Carlo simulation can check the size of potential test statistics using the marginal probabilities (i.e. those that apply when the null hypothesis $\gamma = 1$ is true) as true choice probabilities. I draw 10,000 samples, each with $N = 200$ simulated subjects, from my population of simulated subjects. For each of those simulated subjects, I draw 100 Bernoulli variates c_j^n based on their marginal choice probability as given by eq. A12. Then y^n may be computed for each of the 200 simulated subjects in each sample, and then one may compute (in each sample) the p-values of test statistics against the null hypothesis in eq. 2. For a nominal size of 5%, the actual size of t-tests, signed-rank tests, and sign tests from this Monte Carlo simulation are 5.06%, 5.15% and 4.12%, respectively. As far as size goes, both the t-tests and the signed-rank tests look quite good, whereas the sign tests appear to be somewhat conservative.

Monte Carlo simulation can also check the power of potential test statistics, at various sample sizes, using the conditional probabilities (i.e. those that apply when the alternative hypotheses with $\gamma \neq 1$ are true) as true choice probabilities. I draw 10,000 samples, each with $N = 200$ simulated subjects, from my population of simulated subjects. For each of those simulated subjects, eq. A12 is first used to compute marginal probabilities, and then eqs. A6 and A7 are used to convert these into conditional probabilities with some $\gamma \neq 1$. I draw 100 Bernoulli variates c_j^n based on those conditional choice probabilities and the previous draw at $j - 1$ (each draw c_{j-1}^n determines the conditional choice probability used to draw c_j^n). Then y^n may be computed for each of the 200 simulated subjects in each sample, and then one may compute (in each sample) the p-values of test statistics against the null hypothesis in eq. 2.

When $\gamma = 2$ (the value of γ I specify for the alternative hypothesis of persistence), at a nominal size of 5% and with $N = 200$ simulated subjects per sample, t-tests, signed-rank tests, and sign tests reject the null hypothesis in 89.71%, 89.20% and 81.20% of the 10,000 samples, respectively. These power figures show that both the t-tests and the signed-rank tests get very close to 90% power with $N = 200$, whereas the sign tests are noticeably less powerful than that. The alternative hypothesis of alternation (I specify $\gamma = 0.5$ for this) produces very similar results. The t-tests, signed-rank tests, and sign tests reject the null hypothesis in 90.37%, 90.13% and 81.78% of the 10,000 samples, respectively. Again, both the t-tests and the signed-rank tests get very close to 90% power with $N = 200$, whereas the sign tests are noticeably less powerful than that.

I made the same calculations above for progressively larger samples (beginning at $N = 100$ and stepping this up in increments of 10) until the sample size produced roughly 90% power for both $\gamma = 2$ and $\gamma = 0.5$, which first occurs at $N = 200$. This is how the sample size was chosen.

Table A1: The lottery pairs.

the t sequence pairs											the τ sequence pairs										
t	l	m	h	r_l	r_m	r_h	s_l	s_m	s_h	pair type	τ	l	m	h	r_l	r_m	r_h	s_l	s_m	s_h	pair type
1	18	23	58	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{5}{6}$	$\frac{1}{6}$	buff	1	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{2}{3}$	0	buff
2	8	18	58	$\frac{1}{2}$	0	$\frac{1}{2}$	0	$\frac{2}{3}$	$\frac{1}{3}$	buff	2	8	18	23	$\frac{1}{2}$	0	$\frac{1}{2}$	0	1	0	buff
3	8	18	58	$\frac{1}{6}$	0	$\frac{5}{6}$	0	1	0	buff	3	8	18	58	$\frac{1}{6}$	0	$\frac{5}{6}$	0	1	0	buff
4	8	18	23	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{2}{3}$	0	buff	4	8	18	58	$\frac{5}{6}$	0	$\frac{1}{6}$	$\frac{1}{6}$	$\frac{5}{6}$	0	buff
5	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	0	$\frac{5}{6}$	$\frac{1}{6}$	buff	5	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	0	$\frac{5}{6}$	$\frac{1}{6}$	buff
6	8	18	23	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{6}$	$\frac{5}{6}$	0	buff	6	8	18	23	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{1}{2}$	$\frac{1}{2}$	buff
7	18	23	58	$\frac{5}{6}$	0	$\frac{1}{6}$	0	1	0	buff	7	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	buff
8	8	18	58	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	0	1	0	buff	8	8	18	58	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{1}{2}$	$\frac{1}{2}$	buff
9	18	23	58	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	0	1	0	high	9	8	18	58	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{1}{2}$	$\frac{1}{2}$	low
10	8	18	23	$\frac{1}{6}$	0	$\frac{5}{6}$	0	1	0	targ	10	8	18	23	$\frac{1}{6}$	0	$\frac{5}{6}$	0	1	0	targ
11	8	18	23	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{6}$	$\frac{1}{6}$	$\frac{5}{6}$	0	buff	11	8	18	23	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	0	1	0	buff
12	8	18	23	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	low	12	8	18	58	$\frac{1}{3}$	0	$\frac{2}{3}$	0	1	0	high
13	8	18	58	$\frac{2}{3}$	0	$\frac{1}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	targ	13	8	18	58	$\frac{2}{3}$	0	$\frac{1}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	targ
14	18	23	58	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{1}{2}$	$\frac{1}{2}$	buff	14	8	18	58	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	0	1	0	buff
15	18	23	58	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{6}$	$\frac{1}{6}$	$\frac{5}{6}$	0	high	15	8	18	23	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{1}{3}$	$\frac{2}{3}$	low

Table A1: The lottery pairs (continued).

the t sequence pairs											the τ sequence pairs										
t	l	m	h	r_l	r_m	r_h	s_l	s_m	s_h	pair type	τ	l	m	h	r_l	r_m	r_h	s_l	s_m	s_h	pair type
16	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	0	$\frac{2}{3}$	$\frac{1}{3}$	targ	16	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	0	$\frac{2}{3}$	$\frac{1}{3}$	targ
17	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	buff	17	8	18	23	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{5}{6}$	$\frac{1}{6}$	buff
18	8	18	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	low	18	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	high
19	8	18	58	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	targ	19	8	18	58	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	targ
20	8	18	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{2}{3}$	0	buff	20	18	23	58	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	buff
21	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	high	21	8	18	23	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{6}$	0	1	0	low
22	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	0	1	0	targ	22	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	0	1	0	targ
23	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	buff	23	8	18	58	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	buff
24	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	low	24	18	23	58	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	0	1	0	high
25	8	18	58	$\frac{5}{6}$	0	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{2}$	0	targ	25	8	18	58	$\frac{5}{6}$	0	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{2}$	0	targ
26	8	18	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	buff	26	8	18	58	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{1}{3}$	$\frac{2}{3}$	buff
27	8	18	58	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{5}{6}$	$\frac{1}{6}$	high	27	8	18	23	$\frac{5}{6}$	0	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{2}$	0	low
28	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	targ	28	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	targ
29	8	18	58	$\frac{5}{6}$	0	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{3}$	0	buff	29	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{6}$	buff
30	8	18	23	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	low	30	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	0	1	0	high

Table A1: The lottery pairs (continued).

the t sequence pairs											the τ sequence pairs										
t	l	m	h	r_l	r_m	r_h	s_l	s_m	s_h	pair type	τ	l	m	h	r_l	r_m	r_h	s_l	s_m	s_h	pair type
31	8	18	23	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{2}{3}$	0	targ	31	8	18	23	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{2}{3}$	0	targ
32	8	18	58	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{6}$	0	1	0	buff	32	8	18	58	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	buff
33	18	23	58	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	high	33	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{2}{3}$	$\frac{1}{3}$	low
34	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	targ	34	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	targ
35	8	18	58	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	buff	35	8	18	58	$\frac{1}{2}$	0	$\frac{1}{2}$	0	1	0	buff
36	8	18	23	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{6}$	low	36	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{6}$	high
37	8	18	58	$\frac{5}{6}$	0	$\frac{1}{6}$	0	1	0	targ	37	8	18	58	$\frac{5}{6}$	0	$\frac{1}{6}$	0	1	0	targ
38	8	18	58	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{2}{3}$	$\frac{1}{3}$	buff	38	8	18	23	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{2}$	0	buff
39	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{2}{3}$	0	high	39	8	18	23	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	low
40	18	23	58	$\frac{5}{6}$	0	$\frac{1}{6}$	0	1	0	targ	40	18	23	58	$\frac{5}{6}$	0	$\frac{1}{6}$	0	1	0	targ
41	18	23	58	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	buff	41	18	23	58	$\frac{5}{6}$	0	$\frac{1}{6}$	$\frac{1}{6}$	$\frac{5}{6}$	0	buff
42	8	18	23	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	low	42	18	23	58	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{1}{2}$	$\frac{1}{3}$	high
43	8	18	58	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{2}{3}$	0	targ	43	8	18	58	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{2}{3}$	0	targ
44	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{2}$	0	buff	44	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{2}$	0	buff
45	18	23	58	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{6}$	0	1	0	buff	45	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	0	1	0	buff

Table A1: The lottery pairs (continued).

the t sequence pairs											the τ sequence pairs										
t	l	m	h	r_l	r_m	r_h	s_l	s_m	s_h	pair type	τ	l	m	h	r_l	r_m	r_h	s_l	s_m	s_h	pair type
46	8	18	58	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{5}{6}$	0	buff	46	18	23	58	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{1}{2}$	$\frac{1}{2}$	buff
47	8	18	23	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{1}{2}$	$\frac{1}{2}$	buff	47	18	23	58	$\frac{2}{3}$	0	$\frac{1}{3}$	0	$\frac{5}{6}$	$\frac{1}{6}$	buff
48	8	18	58	$\frac{1}{3}$	0	$\frac{2}{3}$	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{6}$	buff	48	8	18	23	$\frac{1}{6}$	0	$\frac{5}{6}$	0	1	0	buff
49	8	18	58	$\frac{1}{6}$	0	$\frac{5}{6}$	0	$\frac{2}{3}$	$\frac{1}{3}$	buff	49	18	23	58	$\frac{1}{3}$	0	$\frac{2}{3}$	0	$\frac{2}{3}$	$\frac{1}{3}$	buff
50	8	18	58	$\frac{2}{3}$	0	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{2}$	0	buff	50	18	23	58	$\frac{5}{6}$	0	$\frac{1}{6}$	$\frac{2}{3}$	$\frac{1}{3}$	0	buff