Testing Independence Conditions in the Presence of Errors and Splitting Effects[*]

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Abstract

This paper presents experimental tests of several independence conditions implied by expected utility and alternative models. We perform a repeated choice experiment and fit an error model that allows us to discriminate between true violations of independence and those that can be attributed to errors. In order to investigate the role of event splitting effects, we present each choice problem not only in coalesced form (as in many previous studies) but also in split form. It turns out previously reported violations of independence and splitting effects remain significant even when controlling for errors. However, splitting effects have a substantial influence on tests of independence conditions. When choices are presented in canonical split form, in which probabilities on corresponding probability-consequence ranked branches are equal, violations of the properties tested could be reduced to insignificance or even reversed.

Key words: Independence axiom, splitting effects, coalescing, errors, experiment

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1 Introduction

Many studies concluded that expected utility (EU) theory fails to provide an accurate description of peoples´ behavior in several choice problems under risk. Choices apparently violate the crucial independence axiom as shown by the famous paradoxes of Allais (1953). Such violations led to development of numerous alternative theories (e.g. rank-dependent utility, disappointment and regret models, prospect theory, etc.), which aim to provide a more realistic account of actual choice behavior (see surveys by Abdellaoui, 2009; Birnbaum, 2008; Luce, 2000; Starmer, 2000; Sugden, 2004; Schmidt, 2004). Most of these new theories rely on independence assumptions that are weaker than assumptions of EU. Experimental tests of these weakened independence conditions revealed systematic violations, which rule out not only EU but also rank-dependent utility models (Wakker, Erev, and Weber, 1994; Wu, 1994; Birnbaum and Navarrete, 1998; Birnbaum, 2004b, 2005a, 2005b, 2008).

Other studies have shown that people are not perfectly consistent when choosing between risky lotteries (see e.g. Camerer, 1989; Starmer and Sugden, 1989; Harless and Camerer, 1994; Hey and Orme, 1994); that is, in repeated choice problems they may choose one option in the first round and choose the other option in the second one. Such preference reversals to the same problem suggest that choices involve a stochastic component. Nowadays one very intensively discussed questions in decision theory is how to model this stochastic component adequately (e.g., see Birnbaum, 2013; Gul & Pesendorfer, 2006; Blavatskyy, 2007, 2008, 2011, 2012; Conte, Hey, and Moffat, 2011; Hey, Morone, and Schmidt, 2007; Wilcox, 2008, 2011; Harrison and Rutström, 2009; etc.)

Is it possible that one can explain systematic violations of EU by proper modeling of the error component of choice? Hey (1995) concluded: "It may be the case that these further explorations may alter the conclusion to which I am increasingly being drawn: that one can explain experimental analyses of decision making under risk better (and simpler) as EU plus noise – rather than through some higher level functional – as long as one specifies the noise appropriately." Certain of the reported violations of EU might be at least partly caused by errors instead of being intrinsic violations (Blavatskyy, 2006; Sopher and Gigliotti, 1993, Birnbaum and Schmidt, 2008) for violations of transitivity, and Schmidt and Hey (2004), Butler and Loomes (2007, 2009), and Berg, Dickhaut, and Rietz (2010) for preference reversals.

Schmidt and Neugebauer (2007) considered only cases where subjects chose the same option in a choice problem three times in row. Provided that the probability of errors is not too high, such repeated choices more likely reflect "true preferences" since it is rather improbable that a subject makes the same error three times. It turns out that in these cases the incidence of violations of independence decreases substantially.

The goal of the present paper is to provide a more systematic analysis to estimate the incidence of true violations as opposed to those that might be attributed to error. We perform a repeated choice experiment and fit an error model that is neutral with respect to violations of any independence condition. This model allows us to discriminate precisely which portion of violations can be attributed to errors and which part should be considered as "real" violations. Note that such an analysis is not possible with a model of EU plus error term (e.g., as used by Schmidt and Neugebauer, 2007) since that model assumes that true preferences are governed by EU and, thus, satisfy the independence axiom, coalescing, and transitivity. The model we use in the present paper, in contrast, assumes only that there is a true choice probability and an error rate (which can be different for each choice problem); it does not assume transitivity, independence, or coalescing (implications of EU we aim to test).

A further systematic deviation from EU [and in fact also from many of alternatives to EU such as rank-dependent utility (RDU), rank- and sign-dependent utility (RSDU), and cumulative prospect theory (CPT)] is provided by violations of coalescing (splitting effects). For example, coalescing assumes that gamble G = ($50, 0.1; $50, 0.1; $0, 0.8) is equivalent to G' = ($50, 0.2; $0, 0.8). A *branch* of a gamble is a probability-consequence or event-consequence pair that is distinct in the presentation to the participants. In this case, G is a three-branch gamble, and G' is the two-branch coalesced form of G. A splitting effect is said to occur if, for example, G is preferred to a gamble, F, and yet F is preferred to the gamble G', apart from random error.

There exists abundant evidence that splitting an event with a good consequence increases the attractiveness of a lottery in comparison to other lotteries (Starmer and Sugden, 1993; Birnbaum and Navarrete, 1998 Humphrey 1995, 2001).[1] According to configural weighting models, splitting the branch leading to the lowest consequence can also lower the evaluation of a gamble, even when that worst consequence is positive (Birnbaum, 2007; 2008).

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[1] For similar evidence of splitting effects in other contexts than choice under uncertainty see e.g. Weber, Eisenführ and von Winterfeldt (1988) and Bateman et al. (1997).

While Birnbaum and Navarrete employed splitting effects in order to generate substantial violations of first-order stochastic dominance, the papers of Humphrey note that splitting effects may have contributed to previously reported violations of transitivity (see also Birnbaum & Schmidt, 2008). It may well be the case that splitting effects are the main cause of violations of independence conditions, as in the Allais paradoxes (Birnbaum, 2004a).

Birnbaum (2004a) noted that the Allais common consequence problem can be decomposed into three simpler properties: coalescing (no splitting effects), transitivity of preference, and restricted branch independence. If a person satisfies these three properties, she would not show the paradoxical violation of EU. Restricted branch independence is a weaker form of independence that holds that when the number of branches is the same in both gambles being compared, and the probabilities of corresponding branches are equal. With these restrictions, the value of a common consequence is assumed to have no effect. For example, with three branch gambles, restricted branch independence holds that $S = (x, p; y, q; z, 1 - p - q)$ preferred to $R = (x', p; y', q; z, 1 - p - q)$ if and only if $S' = (x, p; y, q; z', 1 - p - q)$ preferred to $R' = (x', p; y', q; z', 1 - p - q)$. Note that the common consequence is z in the first choice and z', in the second choice. Presenting Allais common consequence problems in split form can convert them to tests of restricted branch independence.

For example, consider the choice between $S^* = (\$40, .2; \$2, .8)$ and $R^* = (\$98, .1; \$2, .9)$. The *canonical split form* of this choice is the form in which probabilities on corresponding ranked branches are equal and the number of branches is minimal: $S = (\$40, .1; \$40, .1; \$2, .8)$ versus $R = (\$98, .1; \$2, .1; \$2, .8)$. Coalescing and transitivity imply that a person should choose S over R iff she chooses S^* over R^*. According to restricted branch independence, a person chooses S over R iff she chooses $S' = (\$40, .1; \$40, .1; \$98, .8)$ over $R' = (\$98, .1; \$2, .1; \$98, .8)$. By coalescing and transitivity, this holds iff $(\$40, .2; \$98, .8)$ is chosen over $(\$98, .9; \$2, .1)$. CPT satisfies coalescing (Birnbaum & Navarrete, 1998), so we can test not only EU but also CPT by testing such pairs of choices in both coalesced and canonical split form.

This paper is organized as follows. The next section presents error models and discusses the issue of testing properties such as independence in the presence of errors. Section 3 presents our experimental design and method while section 4 reports the results. Sections 5 and 6 present a corroborating experiment that confirms and extends our results. Discussion and concluding observations appear in the last section.

2 Errors and Violations of Independence

This section shows that random errors can generate data that appear to provide systematic violations of EU. This section shows how the true and error model can be used to separate estimation of error from the model to be tested. Consider a simple variant of the common ratio effect.
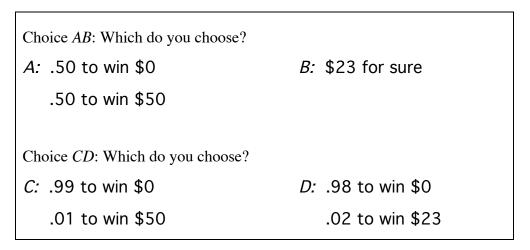

Choice *AB*: Which do you choose?

A: .50 to win $0   *B:* $23 for sure

.50 to win $50

Choice *CD*: Which do you choose?

C: .99 to win $0   *D:* .98 to win $0

.01 to win $50   .02 to win $23

Figure 1: A Test of Common Ratio Independence

According to EU theory, a person should prefer *A* over *B* if and only if that person prefers *C* over *D* because EU(*C*) > EU(*B*) if and only if EU(*C*) > EU(*D*). Proof: EU(A) = 0.5*u(50)* + 0.5*u*(0) > *u*(20) iff 0.01*u(50)* + 0.99*u(0)* > 0.02*u(20)* + 0.98*u*(0). There are four possible response patterns in this experiment, *AC*, *AD*, *BC*, and *BD*, where e.g. *AC* represents preference for *A* in the first choice and *C* in the second choice. The response patterns *AC* and *BD* are consistent with EU while the other two patterns violate the independence axiom of EU. Suppose we obtained data as follows from 100 participants:

	C	D
A	9	9
B	33	49

Table 1: Hypothetical Frequencies of Response Patterns

In this case 33 people switched from *B* to *C*, whereas only 9 reversed preferences in the opposite pattern. The conventional statistical test (test of correlated proportions) is significant, $z = 3.7$, which is usually taken as evidence that EU theory can be rejected (e.g., Conlisk, 1989).

Can such results occur by random errors? As has been previously noted, yes (e.g., Birnbaum, 2004b; Loomes, 2005). Suppose for example a subject chooses *A* over *B* iff *EU(A) - EU(B)* + ε > 0 and chooses *C* and *D* iff *EU(C) - EU(D)* + ε′ > 0 where ε and ε′ are normally distributed, independent random variables with *E(ε)* = *E(ε′)* = 0 and variances σ and σ′. Note that *EU(A) – EU(B) > EU(C) – EU(D)* for any utility fuction, *u*. Depending on the error variances, the probability to make an erroneous response in the choice between *C* and *D* could be greater than that in the choice between *A* and *B* (for example, if σ = σ′), in which case we would observe more frequent erroneous *BC* than *AD* preference patterns. Thus, the standard test of inequality of different types of violations is not a diagnostic test of EU plus random error.

Since this EU-based error theory, however, assumes EU, it does not allow us to test EU; it merely provides an excuse for the violations. Therefore, in order to test EU empirically (rather than by assumption) we need more general error models that allow us to empirically estimate error rates and to distinguish true preferences from response errors. Such models have been developed (Birnbaum, 2013; Birnbaum & Bahra, 2012).

Suppose that each person has a true preference pattern, which may be one of the four possible response combinations. Let p_{AC}, p_{AD}, p_{BC}, and p_{BD} denote the probabilities of the four true preference patterns, which represent the relative frequencies of subjects with these patterns. Let *e* represent the probability of an error in reporting one's true preference for the choice between *A* and *B*. Similarly, *e′* is the probability of an error for the choice between *C* and *D*. Is it possible that, given the data in Table 1, all subjects conform to EU? In other words, are the data in Table 1 compatible with $p_{BC} = p_{AD} = 0$? The answer is "yes," despite the statistically significant inequality in the two types of violations.

In the true and error (TE) model, the probability to observe the preference pattern *BC* is given as follows:

(1) $P(BC) = p_{AC}(e)(1 - e') + p_{AD}(e)(e') + p_{BC}(1 - e)(1 - e') + p_{BD}(1 - e)(e')$

In this expression, *P(BC)* is the probability of observing this preference pattern, *e* and *e′* are the error rates in the choice problems *A* versus *B* and *C* versus *D*, respectively. The error probabilities are assumed to be less than ½ and to be mutually independent. *P(BC)* is the sum of four terms, each representing the probability of having one of the true patterns (p_{AC}, p_{AD}, p_{BC} and p_{BD}), and having the appropriate pattern of errors and correct responses to produce the

BC data pattern. For example, a person who truly has the *AC* pattern could produce the *BC* pattern by making an error on the first choice and correctly reporting her preference on the second choice. There are three other equations like (1), each showing the probability of an observed data pattern given the model.

Given only the data of Table 1, this model is under-determined. There are four response frequencies to fit, which have three degrees of freedom because they sum to the number of participants. The four true probabilities must sum to 1, and there are two error probabilities. Thus, we have three degrees of freedom in the data and five parameters to estimate, so many solutions are possible. Two solutions that reproduce the data in Table 1 perfectly are shown in Table 2:

Parameter	Model 1: EU holds	Model 2: EU does not hold
p_{AC}	0.10	0.05
p_{AD}	0.00	0.00
p_{BC}	0.00	0.34
p_{BD}	0.90	0.61
e	0.10	0.14
e'	0.40	0.14

Table 2: Two models of Table 1 that fit equally well.

This table shows that we might try to "save" EU in this case by assuming that people have unequal error rates in the two choice problems. But if the error rates are actually equal, we should reject EU. Thus, given only the data in Table 1 it is not possible to rationally decide one way or the other. As noted in Wilcox (2008), none of the early error models he reviewed allowed any independent way to answer this question without making an assumption that affects the conclusion. We need a way to evaluate EU without assuming that error rates are necessarily equal or that EU is correct. Put another way, we need to enrich the structure of the data so that we can determine error rates empirically. This enrichment is possible via replications of each choice problem (Birnbaum, 2013).

Consider the case of one choice problem presented twice, for example, Choice *AB* above. With two replicates, there are four response patterns possible, *AA*, *AB*, *BA*, and *BB*. The probability that a person will show the *AB* preference reversal is given as follows:

(2) $P(AB) = p(1 - e)(e) + (1 - p)e(1 - e) = e(1 - e).$

where p is the true probability of preferring A and e is the error rate on the choice between A and B. The probability of the opposite reversal, BA, is also $e(1 - e)$. These expressions show that if we present each choice problem at least twice to the same people, we can estimate the error rates for each choice problem without having to assume that error rates are equal or that EU is true. An important property of the true and error models is that with the proper experimental designs, they not only allow estimation of parameters to address empirical questions, but they are empirically testable.

With two replications of two choice problems (e.g., in a test of EU), there are $2 \times 2 \times 2 \times 2 = 16$ possible response patterns, which have 15 degrees of freedom. But there are only 5 parameters to estimate from the data: two error terms and three probabilities of true response patterns (Because the four probabilities sum to 1, the fourth probability is determined. The general model (which allows all four true preference patterns to have nonzero probability) is now over-determined, leaving $15 - 5 = 10$ degrees of freedom to test the general error model. EU theory is a special case of this general model in which two of the true probabilities are fixed to zero (in Table 1, EU assumes $p_{BC} = p_{AD} = 0$).

Parameters can be estimated to minimize the deviations of fit of the predicted to observed frequencies of response patterns, which can be measured by the standard Chi-Square formula:

(3) $\chi^2 = \Sigma(f - q_i)^2/q_i,$

where f_i is the observed frequency and q_i is the predicted frequency of a particular response pattern. Two statistical tests are of primary importance: first, one can test the TE model as a fit to the 16 response frequencies. Second, one can test EU as a special case of that model. The difference in χ^2 between a fit of the general model (that allows all four response patterns to have non-zero probabilities) and the χ^2 for the special case (EU, in which $p_{BC} = p_{SR} = 0$) is theoretically chi-square distributed with 2 degrees of freedom. This test allows us to determine whether observed violations of EU are real, or whether they might be attributed to response errors.

In Experiment 1, we used four replications of each choice problem. With two choice problems and four replications, there are 256 possible response patterns ($4^4 = 256$). When cell frequencies are small, we use the G-statistic, which is a likelihood ratio test that takes on

similar values to the standard index and is theoretically distributed as Chi-Square: $G = 2\sum f_i ln(f_i/q_i)$. The G index is regarded as more accurate when cell frequencies are small; cells with zero frequency (i.e., $f_i = 0$) have no effect on the statistic, whereas the standard Chi-Square is known to be inflated when expected frequencies are low (see Özdemir and Eyduran, 2005; McDonald, 2009). Additional details about true and error models (including more elaborate models and analyses) are presented in Appendices B and C.

3 Experimental Method and Design of Experiment 1

The experiment was conducted at the University of Kiel with 54 participants, mostly economics and business administration students (all undergraduates). Altogether there were six sessions each consisting of nine subjects and lasting about 90 minutes. Subjects received a 5 Euro show-up fee and had to respond to 176 pairwise choice questions which were arranged in four booklets of 44 choices each. After a subject finished all four booklets one of her choices was randomly chosen and played out for real. The average payment was 19.14 Euro for 90 minutes, i.e. 12.76 Euro per hour, which clearly exceeds the usual wage of students (about 8 Euro per hour). As noted in Cox, Sadiraj, and Schmidt, (2015), this system is incentive compatible with EU theory; that is, a person who perfectly satisfies EU should report her true preferences on all choice problems.

Choices were presented as in Figure 2 and subjects had to circle their preferred alternative. Prizes were always ordered from lowest to highest. Explanation and playing out of lotteries involved a container containing numbered tickets from one to 100. Suppose a subject could for instance play out lottery A in Figure 2. Then she would win 20 Euro if the ticket drawn numbered from 1 to 50, 30 Euro for tickets numbered from 51 to 80, and 40 Euro for a ticket between 81 and 100. All this was explained in printed instructions that were given to the participants and read aloud. Following instructions, subjects had to answer four transparent dominance questions as a test of understanding, which were checked by the experimenter before the participant was allowed to proceed.

A:	50% to win 20 Euro	B:	33% to win 10 Euro
	30% to win 30 Euro		34% to win 15 Euro
	20% to win 40 Euro		33% to win 60 Euro

Figure 2: Format for presentation of a choice between lotteries

Choice problems in the booklets were presented in pseudo-random order. The ordering was different in each booklet with the restriction that successive choice problems not test the same property. Only after finishing each booklet did a subject receive the next one. Moreover, for half of the subjects each booklet contained only coalesced or only split choice problems whereas for the other half split and coalesced choice problems were intermixed in each booklet. Our design included 11 tests of independence conditions, nine of which were investigated in both coalesced and canonical split forms. All 20 tests were replicated four times with counterbalanced left-right positioning. Additionally, in order to check the attentiveness of subjects, each booklet included two transparent stochastic dominance problems, one based on outcome monotonicity and one on event monotonicity.

The lottery pairs for each test are presented in Table 3. Each lottery pair consists of a safe lottery S (in which you can win prize s_i with probability p_i) and a risky lottery R for which possible prizes and probabilities are denoted by r_i and q_i respectively. The lotteries were adapted from previous studies that reported high violation rates but we adjusted outcomes in order to get an average expected value of about 12 Euro. Table 3 shows only the coalesced forms of the lottery pairs. Some of these choice problems were also presented using the canonical split form of those pairs. The canonical split forms of these choices can be found in Appendix A. For example, Choice 5 in Table 3 is the choice between $S = (\$19, .2; \$0, .8)$ and $R = (\$44, .1; \$0, .9)$. The canonical split form of this choice is the form in which probabilities on corresponding ranked branches are equal and the number of branches is minimal. Choice 7 in Appendix A shows Choice 5 in canonical split form: $S^* = (\$19, .1; \$19, .1; \$0, .8)$ versus $R^* = (\$44, .1; \$0, .1; \$0, .8)$. Objectively, S is the same as S^* and R is the same as R^*.

The first six tests in Table 3 include four problems that had previously shown common consequence effects (CCE1-4) and two problems with common ratio effects (CRE1 and 2). The paradoxes of Allais are special variants of such CCE and CRE. This type of CCE can be formally described by $S = (z, p_1; s_2, p_2; s_3, p_3)$, $R = (z, q_1; r_2, q_2; r_3, q_3)$, $S' = (z, p_1 - \alpha; z', \alpha; s_2, p_2; s_3, p_3)$, and $R' = (z, q_1 - \alpha; z', \alpha; r_2, q_2; r_3, q_3)$ where all lotteries are presented in coalesced form. S' and R' are constructed from S and R by shifting probability mass (α) from the common consequence z to a different common consequence z', and converting to coalesced form. An EU maximizer will prefer S over R if and only if she prefers S' over R'. In principle, there is no

restriction on the ordering of the consequences in this notation (for example, z and z' might be the lowest and highest consequences or vice versa). In practice, in all of our tests, $z = \$0$ is the lowest consequence and z' is either the middle or highest consequence of the three. In all of the cases of this design, there are no more than three distinct consequences in each test and lotteries are presented in coalesced form.

In Table 3, the first row of a choice problem always characterizes the lotteries S and R and the second one the lotteries S' and R'. For example, in Choice 5 of CCE1 we have $z = s_1 = 0$, $p_1 = 0.8$, $p_2 = 0.2$, $s_2 = 19$, $p_3 = 0$ for S; and we have $q_1 = 0.90$, $q_2 = 0.10$, $r_2 = 44$, $q_3 = 0$ for R; in Choice 13, S' and R' are constructed by setting $\alpha = 0.4$ and $z' = r_2 = 44$. In this case, S' has added a new branch leading to $\$44$, but the branches leading to 44 are coalesced in R'. The four CCE problems of Table 3 are adapted from Starmer (1992). In all cases, S' has either added a branch leading to the highest consequence or lost the branch leading to the lowest consequence. The typical pattern of violations in CCE1-4 is that people prefer R over S but S' over R'.

A test of CRE can be formally described by $S = (z, p_1; s_2, p_2)$, and $R = (z, q_1; r_2, q_2)$, $S' = (z, 1 - \beta(1 - p_1); s_2, \beta p_2)$, $R' = (z, 1 - \beta(1 - q_1); r_2, \beta q_2)$, i.e. S' and R' are constructed from S and R by multiplying all probabilities by β and assigning the remaining probability $1 - \beta$ to the common consequence z. EU implies again that people choose either the risky or the safe lottery in both choice problems. In CRE1 (from Birnbaum, 2001) and CRE2 (from Starmer and Sugden, 1989), substantial violations of EU have been observed with many people choosing R and S'.

Property	No.	Safe Gamble			Risky Gamble		
		p_1	p_2	p_3	q_1	q_2	q_3
		s_1	s_2	s_3	r_1	r_2	r_3
CCE1 *SR*	5	0.80	0.20		0.90	0.10	
		0	19		0	44	
S′R′	13	0.40	0.20	0.40	0.50	0.50	
		0	19	44	0	44	
CCE2 *SR*	1	0.89	0.11		0.90	0.10	
		0	16		0	32	
S′R′	2	1.00			0.01	0.89	0.10
		16			0	16	32
CCE3 *SR*	5	0.80	0.20		0.90	0.10	
		0	19		0	44	
S′R′	6	1.00			0.10	0.80	0.10
		19			0	19	44

CCE4 *SR*	9	0.70 0	0.30 21		0.80 0	0.10 21	0.10 42
S′R′	10	0.70 0	0.20 21	0.10 42	0.80 0	0.20 42	
CRE1 *SR*	15	0.98 0	0.02 23		0.99 0	0.01 46	
S′R′	16	1.00 23			0.50 0	0.50 46	
CRE2 *SR*	20	0.80 0	0.20 28		0.86 0	0.14 44	
S′R′	19	0.40 0	0.60 28		0.58 0	0.42 44	
UTI *SR*	29	0.73 0	0.02 15	0.25 60	0.74 0	0.01 33	0.25 60
S′R′	30	0.73 0	0.02 15	0.25 33	0.74 0	0.26 33	
LTI *SR*	33	0.75 1	0.23 34	0.02 36	0.75 1	0.24 33	0.01 60
S′R′	34	0.75 33	0.23 34	0.02 36	0.99 33	0.01 60	
UCI *SR*	37	0.20 9	0.20 10	0.60 24	0.20 3	0.20 21	0.60 24
S′R′	38	0.40 9	0.60 21		0.20 3	0.80 21	
LDI *SR*	23	0.60 1	0.20 18	0.20 19	0.60 1	0.20 2	0.20 32
S′R′	24	0.10 1	0.45 18	0.45 19	0.10 1	0.45 2	0.45 32
UDI *SR*	25	0.20 6	0.20 7	0.60 20	0.20 1	0.20 19	0.60 20
S′R′	26	0.45 6	0.45 7	0.10 20	0.45 1	0.45 19	0.10 20

Table note: The first lottery pair of a choice problem always characterizes the lotteries *S* and *R* and the second one the lotteries *S′* and *R′*.

Table 3: The lottery pairs

The remaining five independence properties in Table 3 are weaker than the independence axiom of EU. Some of these are assumed or implied by RDU (Quiggin, 1981, 1982; Luce, 1991, 2000; Luce and Fishburn, 1991; Luce and Marley, 2005), CPT (Starmer and Sugden, 1989; Tversky and Kahneman, 1992; Wakker and Tversky, 1993). These properties test between the class of RDU models (including CPT) and earlier configural weight models that violate those properties (Birnbaum & Stegner, 1979; Birnbaum, 2008). TI (Wu, 1994) is a special case of ordinal independence (Green and Jullien, 1988). If two lotteries share a common tail (i.e. identical probabilities of receiving any outcome better than x_{i+1}), then the preference between these lotteries must not change if this tail is replaced by a different

common tail. Upper Tail Independence (UTI) requires that $S = (s_1, p_1; s_2, p_2; \alpha, p_3) \prec R = (r_1, p_1; \gamma, p_2; \alpha, p_3)$ iff $S' = (s_1, p_1; s_2, p_2; \gamma, p_3) \prec R' = (r_1, p_1; \gamma, p_2 + p_3)$, where $r_1 < s_1 < s_2 < \gamma < \alpha$. TI, however, also demands that preferences must not change if lower common tails are exchanged which will be called lower tail independence (LTI). TI is implied by many models including all variants of RDU as well as CPT. Therefore, rejecting TI would provide serious evidence against all these models. Wu (1994) and Birnbaum (2001, 2008) reported large violations of TI, contradicting RDU, CPT, and EU. Our tests of UTI were adapted from Wu (1994). LTI has, as far as we know, not been tested before. Our construction of lotteries in the test of LTI is similar to that used in the test of UTI.

Birnbaum and Navarrete (1998) deduced the properties of lower and upper cumulative independence (LCI and UCI) as theorems that must be satisfied by CPT and RSDU models but which will be violated according to configural weight models. Formally, UCI demands that If $S = (s_1, p_1; s_2, p_2; \alpha, p_3) \prec R = (r_1, p_1; \gamma, p_2; \alpha, p_3)$ then $S' = (s_1, p_1 + p_2; \gamma, p_3) \prec R' = (r_1, p_1; \gamma, p_2 + p_3)$, where $r_1 < s_1 < s_2 < \gamma < \alpha$. Substantial violations of UCI were reported by Birnbaum and Navarrete (1998) and in subsequent research summarized in Birnbaum (2008). Our lottery pairs are taken from Birnbaum, Patton, & Lott (1999), who observed a typical violating pattern of RS'.

The final property is distribution independence (DI), proposed by Birnbaum and Chavez (1997) as a test of the weighting function in CPT. Whereas certain configural weight models imply that DI holds, DI should be violated according to RDU and CPT, if the weighting function is not linear (Birnbaum, 2005b), as commonly inferred from empirical research (Camerer and Ho, 1994; Wu and Gonzalez, 1996; Tversky and Fox, 1995; Gonzalez and Wu, 1999; Abdellaoui, 2000; Bleichrodt and Pinto, 2000; Kilka and Weber, 2001; Abdellaoui, Vossmann, and Weber, 2005). For three-outcome lotteries, DI demands that $S = (s_1, \beta; s_2, \beta; \alpha, 1 - 2\beta) \prec R = (r_1, \beta; r_2, \beta; \alpha, 1 - 2\beta)$ if and only if $S' = (s_1, \delta; s_2, \delta; \alpha, 1 - 2\delta) \prec R' = (r_1, \delta; r_2, \delta; \alpha, 1 - 2\delta)$, where $r_1 < s_1 < s_2 < r_2$. When α is the highest outcome, the condition is called upper distribution independence (UDI); when α is the lowest consequence, it is called lower distribution independence (LDI). The lotteries used in our tests of UDI and LDI are taken from Birnbaum (2005b). That paper and Birnbaum and Chavez (1997) reported either no violations or violations contrary to CPT with inverse-S weighting function.

Consistency check: We examined the eight tests of transparent dominance per person. Out of *54* subjects, 49 (91%) had no violations in eight tests; five had one and one person had two violations; a total of 7 violations out of 8 x 54 = 432 tests is 1.6%). We conclude from these checks that our subjects were indeed motivated and attentive.

4 Results of Experiment 1

Table 4 summarizes tests of independence properties, using the true and error model. For each property tested (first column), Table 4 shows the estimated probabilities of the four possible response patterns in columns two through five. Columns six and seven show the estimated error rates for the choice problems between S and R *(e)* as well as between S' and R' *(e')*. The final column presents chi-square statistics between the fit of a general model (a model that allows all four response patterns to have non-zero probabilities) and the fit of a model that satisfies the respective independence condition (i.e., $p_{RS'} = p_{SR'} = 0$). One asterisk (two asterisks) in this column indicate that we can reject the null of $p_{RS'} = p_{SR'} = 0$ in favor of the general model at the 5% (1%) significance level. A subscript, S in the first column indicates that in that test, the choice problems were presented in canonical split form.

According to the independence axiom of EU, $p_{RS'} = p_{SR'} = 0$. Our results show that even allowing for a different error rate in each choice problem, EU is systematically violated in the manner reported in previous research. In tests CCE1-4 and CRE1-2, independence can be rejected. Estimated probabilities of violations range from *10%* to *64%* (mean *28%*); apart from CCE3, the vast majority of violations are of one type. In CCE3, there are substantial violations of both pattern SR' and of RS'. In summary, we find that typical violations of the independence properties of EU reported for raw responses are also observed when errors are taken into account; in other words, we conclude that the violations observed in previous research are replicated in our experiment and are not attributable to random error.

Property	Choices	$p_{SS'}$	$p_{SR'}$	$p_{RS'}$	$p_{RR'}$	e	e'	Test
CCE1	5, 13	0.44	0.02	**0.30**	0.24	0.15	0.11	**20.36****
CCE1$_S$	7, 14	0.52	**0.20**	0.00	0.28	0.13	0.16	**12.77****
CCE2	1, 2	0.02	0.00	0.10	0.88	0.02	0.08	**15.33****
CCE2$_S$	3, 4	0.09	0.03	0.05	0.84	0.07	0.07	**7.61***
CCE3	5, 6	0.25	**0.21**	0.16	0.39	0.16	0.12	**18.69****
CCE3$_S$	7, 8	0.52	**0.24**	0.00	0.25	0.13	0.16	**12.63****
CCE4	9, 10	0.67	0.01	**0.29**	0.02	0.14	0.09	**21.96****
CCE4$_S$	11, 12	0.80	0.01	0.02	0.17	0.14	0.12	0.82
CRE1	15, 16	0.25	0.00	**0.64**	0.11	0.11	0.07	**44.64****
CRE1$_S$	17, 18	0.44	0.00	**0.46**	0.10	0.15	0.05	**27.21****
CRE2	20, 19	0.57	0.00	**0.20**	0.23	0.14	0.11	**18.00****
CRE2$_S$	21, 22	0.84	0.02	0.01	0.12	0.17	0.12	0.45
UTI	29, 30	0.06	0.01	**0.52**	0.40	0.13	0.18	**18.76****
UTI$_S$	31, 32	0.17	0.01	0.00	0.82	0.14	0.18	0.05
LTI	33, 34	0.04	0.00	0.14	0.82	0.05	0.15	3.96
LTI$_S$	35, 36	0.04	0.00	0.01	0.95	0.06	0.08	0.24
UCI	37, 38	0.14	0.08	0.13	0.66	0.13	0.22	3.88
UCI$_S$	37, 39	0.13	0.09	0.02	0.76	0.13	0.09	5.07
LDI	23, 24	0.94	0.00	0.00	0.06	0.02	0.05	0.00
UDI	25, 26	0.16	0.02	0.03	0.79	0.09	0.10	1.80

Table note: * denotes a significance level of 5%, ** a significance level of 1%.

Table 4: Tests of independence conditions

A quite different picture arises when the same choice problems are presented in canonical split form. In CCE4$_S$ and CRE2$_S$ violations are reduced and not significant (i.e., EU cannot be rejected), and in CCE1$_S$ the violations of EU are in the opposite direction from that observed in coalesced form (i.e., the *SR′* pattern is more frequent than the *RS′* pattern), and these reversed violations are statistically significant. One case (CRE1) shows the same pattern in both split and coalesced forms as in previous research. We can, therefore, conclude that splitting effects have a substantial influence on tests of the independence axiom of EU. These results are consistent with Birnbaum's (2004a; 2008) hypothesis that CCE are largely due to violations of coalescing.

Next, consider the tests of the weaker independence conditions implied or assumed by RDU, CPT and EU. For UTI we observe a substantial and systematic violation: the estimated probability of the violating pattern *RS′* amounts to *52%*. These results agree with the high violation rates observed by Wu (1994) and by Birnbaum (2001, 2008). We conclude that violations of UTI are not caused by errors but reflect true preferences. This is a serious challenge for CPT and the whole class of rank-dependent models (including EU) which all

imply that UTI holds. It is, however, noteworthy that violations of UTI virtually disappear when we present choices in canonical split form. The estimated frequency of the *RS′* pattern decreases from *52%* in the coalesced test to *0%* in the split test while the frequency of the opposite violation *SR′* amounts to only *1%* in both cases. Therefore, violations of UTI seem to be likely caused by violations of coalescing.

Our new test of LTI did not generate significant violations, nor did the test of UCI. Comparing our results for UCI to previous ones (Birnbaum and Navarrete, 1998; Birnbaum, Patton, & Lott, 1999), the failure to observe significant violations may be due to large error rates in our tests. In fact, the estimated error rates in our coalesced test of UCI are the highest of all choice problems. The high error rates may explain why we estimated only relatively low true violation rates for UCI whereas our observed violation rates (on average 37% for the coalesced presentation and 24% for the split one) fall in line with previous results.

Our tests of LDI and UDI agree with the conclusions of Birnbaum and Chavez (1997) and Birnbaum (2005b). Systematic violations of DI are predicted by CPT but are not observed in our tests, replicating previous failures to confirm predictions of the inverse-S weighting function commonly proposed for rank-dependent models.

Problems	$p_{SS'}$	$p_{SR'}$	$p_{RS'}$	$p_{RR'}$	E	e'	Test
1-3	0.02	0.00	0.07	0.90	0.02	0.07	*7.89**
5-7	0.47	0.00	**0.28**	0.26	0.15	0.13	*17.42***
9-11	0.70	0.00	0.06	0.24	0.14	0.14	*1.78*
10-12	0.82	0.14	0.00	0.04	0.09	0.12	*7.83**
13-14	0.52	**0.22**	0.00	0.26	0.11	0.16	*18.52***
15-17	0.29	0.00	0.13	0.57	0.11	0.15	*8.04**
19-21	0.76	0.03	0.07	0.14	0.11	0.12	*4.56*
20-22	0.56	0.00	**0.23**	0.20	0.14	0.16	*17.31***
29-31	0.06	0.00	0.13	0.80	0.13	0.13	*10.66***
30-32	0.18	**0.40**	0.00	0.42	0.18	0.18	*24.72***
33-35	0.02	0.02	0.01	0.95	0.05	0.07	*3.51*
34-36	0.05	0.13	0.01	0.81	0.15	0.08	*3.76*
38-39	0.13	0.15	0.01	0.71	0.22	0.08	*3.85*
41-42	0.35	0.04	**0.35**	0.27	0.20	0.14	*15.19***

Table 5: Tests of Splitting effects

Because violations of coalescing rule out RDU and CPT models (as well as EU) and because the tests of independence depend on the form of presentation (split or coalesced), we also present direct tests of these splitting effects in Table 5. Table 5 compares choices in a given

lottery pair in coalesced form with the same choice in canonical split form. If no splitting effects occur, each subject chooses the risky lottery in both problems or the safe lottery in both problems, aside from error. In contrast, Table 5 shows that many people make different choice responses, even when corrected for errors, where $e\ (e')$ is the estimated error rate of the choice problem stated first (second) in the first column of the table. The last column shows chi-square tests comparing the fit of a special case model that satisfies coalescing ($p_{RS'} = p_{SR'} = 0$), and the fit of a general model that allows for splitting effects (allowing non-zero probabilities of all four possible response patterns). It turns out that in nine out of 14 tests the null hypothesis of coalescing, $p_{RS'} = p_{SR'} = 0$, can be rejected in favor of the general model allowing for splitting effects. Violations of coalescing contradict CPT, RDU, and EU models, among others.

5 Experiment 2

Experiment 1 concluded that violations of common ratio and common consequence independence cannot be attributed to error and are strongly affected by the form in which choice problems are presented. When branches are split in canonical split form, violations of independence are either reduced markedly or in some cases reversed. Such results are largely consistent with configural weight models (Birnbaum, 2008), which can violate coalescing. In Experiment 2, we replicate and extend these findings for Allais paradoxes in a new experiment with American participants with different lotteries, and we test a new manipulation, called *double splitting* in which both the upper and lower branches are split. This manipulation allows us to test among variations of configural weighting models.

Birnbaum and Stegner (1979, Equation 10) proposed a "revised" configural weight model in which configural weights are transferred from branch to branch in proportion to the absolute weight of the branch that loses weight. This model is now known as the transfer of attention exchange (TAX) model (e.g., Birnbaum & Navarrete, 1998). Consider gambles defined as $G = (x_1, p_1; x_2, p_2; \ldots x_j, p_j; \ldots x_i, p_i; \ldots; x_n, p_n)$, where the outcomes are ordered such that $x_1 \geq x_2 \geq \ldots \geq x_j \geq \ldots \geq x_i \geq \ldots \geq x_n \geq 0$ and $\Sigma\ p_j = 1$. Note that $x_j \geq x_i$. The TAX model can be written as follows:

(4)
$$U(G) = \frac{\sum_{i=1}^{n} t(p_i)u(x_i) + \sum_{i=2}^{n}\sum_{j=1}^{i-1}[u(x_i) - u(x_j)]\omega(p_i, p_j, n)}{\sum_{i=1}^{n} t(p_i)}$$

where $U(G)$ is the utility of the gamble, $t(p)$ is a weighting function of probability, $u(x)$ is the utility function of money, and $\omega(p_i, p_j, n)$ is the configural transfer of weight between outcomes x_i and x_j in a gamble with n branches. Note that if the configural transfer is positive, then the branch with a higher valued consequence transfers weight to branches with lower valued consequences; when it is negative, then the higher-valued branch gains weight and the lower valued branches give up this same weight. The weight transfers are assumed as follows:

$$(5) \qquad \omega(p_i, p_j, n) = \begin{vmatrix} \delta t(p_j)/(n+k), & \delta \geq 0 \\ \delta t(p_i)/(n+k), & \delta < 0 \end{vmatrix}$$

where n is the number of distinct branches, and k was fixed to 1. If $\delta = 1$ and $k = 1$ (Birnbaum & Chavez, 1997), then the weights of two, three, and four equally likely outcomes will be (2/3, 1/3), (3/6, 2/6, 1/6), and (.4, .3, .2, .1), for lowest to highest valued branches, respectively. For small consequences, $0 < x < \$150$, it has been found that $u(x) = x$ provides a reasonable approximation to choice data from undergraduates. In addition the weighting function of probability has been approximated as $t(p) = p^{0.7}$. This model is known as the "prior" TAX model because these parameters from 1997 were used (with considerable success) to predict results of new experiments, such as those of Birnbaum (2004a, 2005a, 2007, 2008).

Viscusi's (1989) prospective reference theory is a special case of TAX in which $\delta = 0$ (Birnbaum, 2005a); when $t(p) = p$ and $\delta = 0$, the TAX model reduces to EU.

This model is the same as in Birnbaum and Navarrete (1998), except that the consequences are ranked here in decreasing value (e.g., $x_1 \geq x_2$, to match the notational convention used in CPT), so $\delta < 0$ in Birnbaum and Navarrete (1998) corresponds to $\delta > 0$ in this model.

According to the TAX model, splitting the branch of a gamble leading to the highest consequence in the gamble should make the gamble better and splitting the lowest branch of a gamble should make a gamble worse (Birnbaum, 2008).

Except for two cases, the results of Experiment 1 (Table 5) are consistent with the predictions of the prior TAX model; that is, splitting the branch with the best consequence of a gamble tends to make it more attractive and splitting branch with the worst consequence of a gamble makes it worse. In the two cases not predicted from previous parameters (Choice Problems #13-14 and #30-32) it was found that splitting both the upper and lower branches of

the risky gamble increased the frequency with which the split form was chosen relative to splitting the middle branch of the safe gamble.

Double splitting: Consider, for example, $A = (\$98, 0.5; \$2, 0.5)$ and $B = (\$98, 0.25; \$98, 0.25; \$2, 0.25; \$2, 0.25)$. B is the same as A except for coalescing, and the manipulation from A to B is called "double splitting" because both upper and lower branches are split in the same way. To test the effect of this manipulation, each of these gambles can be compared with a third gamble, C. Will A be chosen over C more often than B over C, when corrected for error? According to the prior TAX model, the overall utility of a gamble tends to decrease as the number of branches increases, consistent with the idea that increasing complexity of an alternative lowers its evaluation (Sonsino, Ben-Zion, & Mador, 2002). This manipulation allows us to compare three variants of the TAX model in which the term $(n + 1)$ in Equation (5) is replaced by $(n + k)$, where $k > 0, k < 0$ (where $n + k > 0$), or $k = 0$; these cases imply that double splitting will tend to decrease, increase, or have no effect on the utility of a gamble, respectively.

6. Method of Experiment 2

Participants made choices between gambles, viewed via computers in a lab. Each choice appeared as in Figure 3:

```
7. Which do you choose?
          M: 10 blue marbles to win $40
             10 green marbles to win $40
             80 black marbles to win $2
      OR
          N: 10 red marbles to win $98
             10 white marbles to win $2
             80 black marbles to win $2
```

Figure 3: Format for the display of a choice problem in Experiment 2.

Instructions read (in part) as follows:

"Your prize will be determined by the color of marble that is drawn randomly from an urn. The urns always have exactly 100 marbles, so the number of any given color tells you the percentage that wins a given prize." Participants clicked a button beside the gamble they would rather play in each trial. They were informed that 3 lucky participants would be selected at random to play one of their chosen gambles for real money, with prizes as high as $100, so they should choose carefully. Prizes were awarded as promised.

The main design tested the extent to which the Allais paradoxes are produced by violations of restricted branch independence, by violations of coalescing, or by random error. Figure 4 shows the choices used in the main design of Experiment 2. The triangle in Figure 4 represents a probability simplex depicting lotteries of the form, ($98, p; $40, 1 – p – q; $2, q). The abscissa shows q, the probability to win $2 and the ordinate shows p, the probability to win $98. A "sure thing" to win $40, $98, or $2 corresponds to the lower left corner, upper left corner, and lower right corner of this figure, respectively.

Each line segment in Figure 4 represents a choice between a "safe" gamble and a "risky" gamble. Each of these six choice problems was presented in both coalesced and in canonical split form, making 12 choice problems in the main design. In Sample 2, the same design was used but consequences $98, $40, and $2 were changed to $96, $48, and $4, respectively.

Design of Experiment 2

Figure 4: Main design of Experiment 2 (See also Table 6).

Table 6 lists the choice problems of Figure 4. Each choice problem in Table 6 is created from the problem in the row above by either coalescing/splitting or by applying restricted branch independence. A person should make the same choice response in every choice problem of Table 6, according to EU, aside from random error. That is, a person should either choose the risky gamble in all 13 choice problems or choose the safe gamble in all 13 cases. According to CPT, people should make the same choice responses in problems that differ in coalescing but they can violate restricted branch independence.

Problem No.	Choice Problem		% R Sample 1	% R Sample 2	True & Error Model— Sample 1		True & Error Model Sample 2	
	S	R	N=104	N=107	p	e	p	e
1	($40, .2; $2, .8)	($98, .1; $2, .9)	0.59	0.66	0.64	0.17	0.72	0.13
1a	($40, .1; $40, .1; $2, .8)	($98, .1; $2, .1; $2, .8)	0.42	0.40	0.40	0.11	0.37	0.11
2	$40 for sure	($98, .1; $40, .8; $2. .1)	0.57	0.63	0.60	0.13	0.65	0.08
2a	($40, .1; $40, .8; $40, .1)	($98, .1; $40, .8; $2. .1)	0.59	0.56	0.61	0.09	0.57	0.11
3	($98, .8; $40, .2)	($98, .9; $2, .1)	0.25	0.17	0.16	0.15	0.09	0.10
3a	($98, .8; $40, .1; $40. .1)	($98, .8; $98, .1; $2. .1)	0.60	0.47	0.69	0.23	0.46	0.14
4	($40, .6; $2, .4)	($98, .1; $40, .4; $2. .5)	0.73	0.74	0.80	0.13	0.88	0.19
4a	($40, .1; $40, .4; $40. .1; $2, .4)	($98, .1; $40, .4; $2. .1; $2, .4)	0.42	0.46	0.38	0.19	0.44	0.13
5	($98, .4; $40, .6)	($98, .5; $40, .4; $2. .1)	0.46	0.41	0.43	0.17	0.35	0.19
5a	($98, .4; $40, .1; $40. .4; $40, .1)	($98, .4; $98, .1; $40. .4; $2, .1)	0.46	0.40	0.42	0.21	0.38	0.09
6	($98, .4; $40, .2; $2. .4)	($98, .5; $2, .5)	0.21	0.24	0.09	0.15	0.13	0.15
6a	($98, .4; $40, .1; $40. .1; $2, .4)	($98, .4; $98, .1; $2. .1; $2, .4)	0.40	0.34	0.34	0.19	0.30	0.10
6c	($98, .1; $98, .3; $40. .2; $2, .4)	($98, .5; $2, .3; $2. .1; $2, .1)	0.23	0.20	0.18	0.08	0.09	0.13

Table 6: Choice problems used in main design of Experiment 2 (Sample 1); Problem Number refers to Figure 4.

In Choice problem 1 of Table 6 (first row), the common branch is 80 marbles to win $2 (Problem 1), 80 to win $40 (Problem 2), or 80 to win $98 (Problem 3). Problem 4 is generated from Problem 1 by changing 40 marbles to win $2 to 40 to win $40; Problem 5 is generated from Problem 4 by changing the common branch of 40 marbles to win $2 to 40 marbles to win $98. Problem 6 is generated from Problem 1 by changing a common branch of 40 marbles to win $2 to 40 marbles to win $98.

Choice problems marked 1a, 2a, 3a, etc. are the same as 1, 2, 3, etc, respectively, except these are presented in canonical split form. For example, Problem 1a is the same as Problem 1, except that the branch of 20 marbles to win $40 in the safe gamble has been split to two

branches of 10 marbles each to win $40, and the branch of 90 marbles to win $2 in the risky gamble has been split into a branch with 80 marbles to win $2 and 10 to win $2. Problem 6c is another split form of Problem 6, designed to improve the "safe" gamble relative to the "risky" one. Problems 1, 1a, 2, 3, and 3a (Sample 1) match choice problems used by Birnbaum (2004a, Series A).

There were 13 additional choice problems, shown in Table 7, which were presented on alternating trials. These additional trials tested double splitting of two branch gambles into four and eight branch gambles as well as other tests of coalescing and idempotence. The same design was used in Sample 2, except $96, $44, and $4 were substituted for $98, $40, and $2. The order of choice problems was randomized with the restriction that successive choice problems not come from the same design. Complete materials can be viewed at URLs:

http://psych.fullerton.edu/mbirnbaum/decisions/allais5_.htm

http://psych.fullerton.edu/mbirnbaum/decisions/allais6_.htm

Participants were 211 undergraduates enrolled in lower division psychology courses at California State University, Fullerton (USA). There were 104 and 107 who served in Samples 1 and 2, respectively. There were 57% and 62% females, and 87% and 86% were 20 years of age or younger in the two groups, respectively. Each person completed the entire study twice, separated by other tasks that required about 20 minutes intervening time.

Table 7 on next page.

Problem			% R Sample 1	% R Sample 2	True & Error Sample 1		True & Error Sample 2	
No.	S	R	N=104	N=107	p	e	p	e
7	$40 for sure	($98, .5; $2, .5)	0.36	0.37	0.34	0.06	0.35	0.05
7a	$40 for sure	($98, .25; $98, .25; $2, .25; $2, .25)	0.40	0.38	0.40	0.02	0.37	0.02
7b	$40 for sure	($98, .13; $98, .13; $98, .12; $98, .12; $2, .12; $2, .12; $2, .13; $2, .13)	0.40	0.38	0.39	0.05	0.38	0.02
7c	($40, .25; $40, .25; $40, .25; $40, .25)	($98, .25; $98, .25; $2, .25; $2, .25)	0.35	0.34	0.34	0.04	0.33	0.05
8	$48 for sure	($98, .5; $2, .5)	0.32	0.36	0.30	0.06	0.35	0.02
8a	$48 for sure	($98, .25; $98, .25; $2, .25; $2, .25)	0.40	0.37	0.40	0.03	0.37	0.02
8b	$48 for sure	($98, .13; $98, .13; $98, .12; $98, .12; $2, .12; $2, .12; $2, .13; $2, .13)	0.39	0.38	0.39	0.02	0.38	0.02
8c	($48, .25; $48, .25; $48, .25; $48, .25)	($98, .25; $98, .25; $2, .25; $2, .25)	0.35	0.33	0.34	0.06	0.31	0.04
9	$50 for sure	($98, .7; $2, .3)	0.59	0.57	0.59	0.02	0.58	0.02
9a	$50 for sure	($98, .7; $2, .1; $2, .1; $2, .1)	0.54	0.57	0.55	0.07	0.58	0.05
9b	$50 for sure	($98, .5; $98, .1; $98, .1; $2, .3)	0.67	0.64	0.69	0.05	0.65	0.08
10a	($98, .85; $98, .05; $2, .1)	($98, .9; $2, .05; $2, .05)	0.44	0.42	0.41	0.14	0.40	0.10
10d	($98, .85; $96, .05; $2, .1)	($98, .9; $4, .05; $2, .05)	0.40	0.39	0.34	0.18	0.34	0.15

Table 7: Tests of double splitting (#7, 7a, 7b; 8, 8a, 8b), idempotence (7a, 7c; 8a, 8c), and stochastic dominance (10d).

7. Results of Experiment 2

The proportions representing preference for the "risky" gamble in the main design are shown under "%R" in Table 6. According to EU, people should make the same responses in all rows of Table 6, apart from error. Instead, the observed choice percentages vary from a low of 17% (Choice Problem 3 in Sample 2) to a high of 74% (Problem 4 in Sample 2).   These are extremely large violations of EU in raw choice proportions.

Choice Problems 1 and 3 also produce large violations of EU (from 59% and 66% preferring the risky gamble in Problem 1 to only 25% and 17% in Problem 3). This Allais Paradox is reversed in Problems 1a and 3a, in split form. This reversal of the direction of the Allais paradox under splitting contradicts RDU, CPT and EU models, and it replicates previous results with the same choice problems (Birnbaum, 2004a). It also confirms results from Experiment 1 (CCE1 and CCE1s). The next section analyzes the question of whether errors can account for these violations.

7.1 *True and Error Analysis*

The true and error model was fit to the replicated presentations of each choice problem in each sample of Experiment 2, and was fit to the data combined across samples as well. Details are presented in Appendix B, which shows that the true and error model gave a good fit to the data for replications and that the rival model of response independence (implied by random preference models) could be rejected in every statistical test.

Because the true and error model gives a good representation of the data, we can use its parameter estimates of the true choice probabilities to address the main issue of this article: Can we attribute violations of EU to random error?

The estimated error rates and true choice probabilities for the true and error model are presented in Tables 6 and 7 for the main design and double-splitting designs, respectively. According to EU, a person should choose either the "risky" gamble or the "safe" gamble in every choice of Table 6, but should not switch, except by random error. The choice proportions, corrected for error should therefore be constant in all rows of Table 6. Instead, the estimated true probabilities choosing the risky gamble vary from as low as 0.09 and 0.13 for Problem #6 in Samples 1 and 2, to as high as 0.80 and 0.88 for Problem #4, respectively. Similarly, they vary from 0.15 and 0.10 in Problem #3 to 0.64 and 0.72 in Problem #1. These represent huge violations of EU, corrected for error.

According to RDU, CPT, and EU, choice responses should not differ if choice problems are presented in coalesced or split form. Instead, there are large effects of splitting in Tables 6 and 7. Choice Problems 1 versus 1a, 3 versus 3a, and 4 versus 4a produced splitting effects large enough to reverse modal choices. In all three of these cases, the branch leading to the highest consequence of one gamble is split and the lowest branch of the other gamble was split. Most people who switched preferences did so as predicted by the TAX model; that is, choice proportions increased for gambles whose higher branch was split relative to gambles

whose lower branch was split.

When gambles are presented in canonical split form, it should be easy to see and cancel identical branches, if a person wanted to follow this editing principle of original prospect theory. If a person used cancellation in Experiment 2, her data would satisfy restricted branch independence. Instead, there are significant violations of restricted branch independence; note that Problems 1a, 4a, 5a, and 6a all yield estimated probabilities less than 0.5, but Problems 2a and 3a yielded estimated probabilities that exceed 0.5 in Sample 1. As shown in Table 6, violations of restricted branch independence are not diminished by the correction for error.

The observed violations of restricted branch independence are opposite from Allais paradoxes in coalesced form, and they are opposite predictions of CPT based on the inverse-S probability weighting function. These findings corroborate previous results (Birnbaum, 2004a) and extend them by showing that these failures of CPT for the Allais paradoxes cannot be attributed to random error.

The prior TAX model correctly predicted the modal choices in nine cases of Table 6 but failed to predict the modal response in Choice Problems # 2, 2a, 6, and 6a. In all four of these cases, people tend to prefer the gamble that gives three positive outcomes over a gamble giving only one or two possible consequences. CPT with prior parameters of Tversky and Kahneman (1992) failed to predict these same four choices (2, 2a, 6, and 6a), but it also failed to predict four other cases that were correctly predicted by prior TAX: #1a, 3a, 4a, and 6c, all involving choice problems in split form.

Appendix B analyzes a true and error model in which different people are allowed to have different amounts of noise in their data. The analysis shows that even allowing individual differences in error rates, the same conclusions are reached regarding EU and CPT.

Appendix C considers a more complex true and error model in which the probability to make an error depends not only on the choice problem but also depends on a person's true preference state. Can we save EU by doubling the number of error rate parameters to be estimated from the data? For example, perhaps those who truly prefer the risky gamble in Choice Problem 1 are more likely to make an error in Problem 3 than those who truly prefer the safe gamble. This error model would imply a shift of choices from risky to safe in Problems 1 and 3; but can it save the EU model? As shown in Appendix C, even this more complex error model requires us to reject the EU model.

7.2 Double Splitting and Stochastic Dominance

Table 7 shows that double splitting (splitting both the upper and lower branches of a gamble) did not affect overall choice proportions by very much. The largest effect occurred in Choice Problems 7 and 7a; in all other tests, data are consistent with the hypothesis that fewer than 5% of participants were affected by this manipulation. The prior TAX model with $k = 1$ in Equation (4) implies that double splitting should have reduced the utility of the lotteries. Instead, it appears that TAX would be improved with $k = 0$, instead of $k = 1$. A caveat is that with so many choice problems using splitting manipulations in this study, some participants may have learned to recognize the equivalence of the splitting manipulation; therefore, a larger effect might be observed in situations where fewer choices of this type would be presented. However, if these results generalize, it suggests that the proportion of weight transferred in the TAX model might better be represented as δ/n rather than $\delta/(n + 1)$ in Equation (5).

Choice Problem 10d (Table 7) is a test of first order stochastic dominance of a type that has produced high rates of observed violations in previous studies (e.g., Birnbaum, 2005a; Birnbaum & Navarrete, 1998). In this new study, the observed percentages of violations are 60 and 61% in Samples 1 and 2, respectively. Problem 10a is a test of coalescing/splitting related to Problem 10d, which shows similar rates of violation. The prior TAX model correctly predicts these two modal choices, which violate both EU and CPT. Corrected for error, estimated true rates of violation are higher for both problems.

8 Conclusions

Three major conclusions can be drawn from our study: First, we reject the hypothesis that violations of independence properties tested in the coalesced form can be explained as a mere consequence of choice errors. Even when we control for errors, EU can be significantly rejected in all six tests of common consequence and common ratio effects in Experiment 1. In addition, we found significant violations of upper tail independence controlling for errors. Second, violations of coalescing are systematic. In nine of fourteen tests coalescing could be rejected, which refutes EU, RDU, and CPT. Third, when both lotteries in each choice are presented in canonical split form, only one test (a common ratio effect) remained significant in the same direction, all other tests were either insignificant, unsystematic, or significant in the opposite direction. Experiment 2, we replicated the main findings of Experiment 1 and extended them, showing that the Allais paradoxes, the effect of splitting, and the violations of restricted branch independence cannot be attributed to error.

These results agree with Birnbaum's (2004a) conclusion that the Allais paradox is largely due to violations of coalescing. Schmidt and Seidl (2014) reached similar conclusions regarding the common ratio effect. But neither of those studies controlled for errors, so the present findings, corrected for error, strengthen the case for these earlier conclusions.

Given our evidence, the question arises whether one form of presenting lotteries in a choice (coalesced or canonical split form) may be regarded as "better" in some sense than the other. Indeed, when Savage (1954) found that he violated his sure-thing principle in the common consequence effect, he reformed the Allais choices in (what we now call) canonical split form in order to convince himself to satisfy his own axiom. Violations of first-order stochastic dominance are nearly eliminated when choice problems are presented in canonical split form (see review in Birnbaum, 2008). From these considerations, one might be tempted to conclude that the canonical split form of a choice is the "right" way to present a choice.

Presenting choices in canonical split form cannot save EU, however, as shown by the significant violations even when choices are presented in canonical split form (Tables 4 and 6). Birnbaum and Bahra (2012) found that when sufficient data are collected from each person to fit an individual true and error model to each person, violations of restricted branch independence remain significant. But would restricting the theoretical domain to choices in canonical split form save RDU and CPT models? These models can violate restricted branch independence. Experiment 1 shows significant violations of UTI in the coalesced form but not in the split form. Furthermore, the weaker independence conditions implied by RDU and CPT are not significantly violated in our data in canonical split form (Table 4). The splitting results of Table 5 that violate RDU and CPT as do violations of stochastic dominance in coalesced form in Table 7 would be considered outside the new, limited domain of these models, which would be considered applicable only to choices in canonical split form. In this approach, restricting the domain to canonical split form, CPT and RDU models could no longer be considered as theories of the Allais paradoxes and other results in the literature that have used coalesced lotteries. Further, to account for violations of restricted branch independence in canonical split form (e.g., Birnbaum, 2008), these models would need to give up the inverse-S weighting function, since the violations are opposite in direction from those predicted by that weighting function.

Another potential problem with this approach arises, however, from the fact that how a lottery is split in canonical form of a choice problem depends on the other lottery with which it

is compared. That means that lottery A appears in one form when paired with B and in another form when paired with C. If the utility of a lottery depends on how it is split, and if how it is split depends on the lottery with which it is compared (as is the case in canonical split form), it means that utility of one gamble varies depending on the other lottery with which it is compared. But the RDU and CPT models assume that the utility of a lottery is independent of how it is split and independent of the other lottery of a choice. Violations of these types of independence could result in apparent violations of transitivity (Birnbaum and Schmidt, 2008), which would then rule out these models even in the restricted domain of canonical split forms.

Because splitting effects rule out a number of popular models including CPT, and because the operational definition of branch splitting depends on the format for presentation of lotteries (i.e., the mode of representing and displaying probabilities and choices), the question naturally arises, is there a procedure or format in which violations of coalescing and other violations of CPT are minimized? So far, 15 different formats have been tested by Birnbaum (2004b, 2006), Birnbaum, Johnson, and Longbottom (2008), all showing strong evidence against CPT.

Violations have been observed when lotteries are represented via pie charts, histograms, lists of equally likely consequences, aligned and unaligned matrices showing the connections between tickets and prizes, with lotteries presented side-by-side or one above the other, with and without event framing (using same or different colored marbles for common probability-consequence branches), with probabilities of consequences and with decumulative probabilities (probabilities to win prize x or more), with positive, negative, and mixed gambles, and with other variations of format and method. Although there are small effects of format, no format has yet been found in which violations of coalescing or stochastic dominance are reduced to levels that might allow retention of RDU or CPT.

In experiments with many trials, rates of violation of EU tend to decrease with practice. Birnbaum and Schmidt (in press) analyzed the data of Experiment 1 and showed that violations of coalescing diminished significantly with practice but persisted throughout the experiment. Whether or not people can learn to satisfy EU in a long enough experiment, perhaps with education and training remains an open question.

Given the present results showing that splitting effects cannot be attributed to error, it seems time to set aside those models that cannot account for these phenomena and concentrate our theoretical and experimental efforts on differentiating models that can account for these

phenomena. Models which imply splitting effects include (stripped) original prospect theory (apart from its editing rule of combination), subjectively weighted utility (Edwards, 1954; Karmarkar, 1979), prospective reference theory (Viscusi, 1989), gains-decomposition utility (Luce, 2000; Marley and Luce, 2001), entropy modified linear weighted utility (Luce, Ng, Marley, and Aczel, 2008a, 2008b), and configural weight models (Birnbaum, 2005a, 2007, 2008).

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Appendix A

Problem	No.	Safe				Risky			
		p_1 / s_1	p_2 / s_2	p_3 / s_3	p_4 / s_4	q_1 / r_1	q_2 / r_2	q_3 / r_3	q_4 / r_4
CCE1$_S$	7	0.80 / 0	0.10 / 19	0.10 / 19		0.80 / 0	0.10 / 0	0.10 / 44	
	14	0.40 / 0	0.10 / 19	0.10 / 19	0.40 / 44	0.40 / 0	0.10 / 0	0.10 / 44	0.40 / 44
CCE2$_S$	3	0.89 / 0	0.01 / 16	0.10 / 16		0.89 / 0	0.01 / 0	0.10 / 32	
	4	0.01 / 16	0.89 / 16	0.10 / 16		0.01 / 0	0.89 / 16	0.10 / 32	
CCE3$_S$	7	0.80 / 0	0.10 / 19	0.10 / 19		0.80 / 0	0.10 / 0	0.10 / 44	
	8	0.10 / 19	0.80 / 19	0.10 / 19		0.10 / 0	0.80 / 19	0.10 / 44	
CCE4$_S$	11	0.70 / 0	0.10 / 21	0.10 / 21	0.10 / 21	0.70 / 0	0.10 / 0	0.10 / 21	0.10 / 42
	12	0.70 / 0	0.10 / 21	0.10 / 21	0.10 / 42	0.70 / 0	0.10 / 0	0.10 / 42	0.10 / 42
CRE1$_S$	17	0.98 / 0	0.01 / 23	0.01 / 23		0.98 / 0	0.01 / 0	0.01 / 46	
	18	0.50 / 23	0.50 / 23			0.50 / 0	0.50 / 46		
CRE2$_S$	21	0.80 / 0	0.06 / 28	0.14 / 28		0.80 / 0	0.06 / 0	0.14 / 45	
	22	0.40 / 0	0.18 / 28	0.42 / 28		0.40 / 0	0.18 / 0	0.42 / 45	
UTI$_S$	31	0.73 / 0	0.01 / 15	0.01 / 15	0.25 / 60	0.73 / 0	0.01 / 0	0.01 / 33	0.25 / 60
	32	0.73 / 0	0.01 / 15	0.01 / 15	0.25 / 33	0.73 / 0	0.01 / 0	0.01 / 33	0.25 / 33
LTI$_S$	35	0.75 / 1	0.23 / 34	0.01 / 36	0.01 / 36	0.75 / 1	0.23 / 33	0.01 / 33	0.01 / 60
	36	0.75 / 33	0.23 / 34	0.01 / 36	0.01 / 36	0.75 / 33	0.23 / 33	0.01 / 33	0.01 / 60
UCI$_S$	37	0.20 / 9	0.20 / 10	0.60 / 24		0.20 / 3	0.20 / 21	0.60 / 24	
	39	0.20 / 9	0.20 / 9	0.60 / 21		0.20 / 3	0.20 / 21	0.60 / 21	

Table note: The first lottery pair of a choice problem always characterizes the lotteries S and R and the second one the lotteries S' and R'.

Table A1: The lottery pairs in canonical split form (Experiment 1)

Appendix B: Supplemental Analyses of True and Error Models

Let p represent the proportion of people who truly prefer R in the choice problem comparing S and R, and let e represent the error rate for this problem. The probability of choosing gamble R on both presentations (i.e., both repetitions of the same choice problem) is represented as follows:

$$P(RR) = p(1-e)^2 + (1-p)e^2$$

In other words, those people who truly prefer R over S have correctly detected and reported their preference twice and those who truly prefer S have made two errors. The probability of switching from R to S is given as follows:

$$P(RS) = p(1-e)e + (1-p)e(1-e) = e(1-e)$$

This is the same as $P(SR)$, so the probability of reversals is $2e(1-e)$. Finally, the probability of choosing S twice is $P(SS) = pe^2 + (1-p)(1-e)^2$. In theory, this model should reproduce the four observed frequencies corresponding to $P(RR)$, $P(RS)$, $P(SR)$, and $P(SS)$, which have 3 degrees of freedom (since the four sum to the number of participants). The model uses two parameters (p and e), leaving 1 df to test the model. The parameters are estimated to minimize the $\chi^2(1)$ between observed and predicted frequencies.

Table B.1 presents the $\chi^2(1)$ tests of fit corresponding to the estimated values of p and e in Experiment 2 that are given in Tables 6 and 7. There are two $\chi^2(1)$ tests of fit for each set of data: one is a test of the true and error model and the other is a test of response independence [i.e., $P(RR^*) = p(R)p(R^*)$, $P(RS^*) = p(R)p(S^*)$, etc.], based on the same exact data entries, with the same number of parameters and using the same number of degrees of freedom (Birnbaum, 2013).

All 52 tests of response independence in Table B.1 are significant (median Chi-Squares in Samples 1 and 2 were 37.43 and 45.25, respectively), but only 4 of the 52 Chi-Squares for the true and error model were significant ($p < .05$) with medians of 0.74 and 0.33, respectively. At the .05 level, we expect 2.6 tests to be significant by chance; the binomial probability to observe 4 or more "significant" (out of 52 with $p = 0.05$) is 0.26, so we can retain the hypothesis that the true and error model is compatible with these data. Birnbaum (2013) reviewed similar results in other studies. Table B.2 shows the response frequencies for each choice problem of Experiment 2, aggregated over both samples. Aggregated data lead to the same conclusions as data for the two samples fit separately.

Choice Problem	Sample 1 (n = 104)				Sample 2 (n = 107)			
	TE parameters		CHISQ		TE parameters		CHISQ	
	p	e	Indep	TE	p	e	Indep	TE
1	0.64	0.17	18.72	0.31	0.72	0.13	26.50	0.00
1a	0.40	0.11	38.91	1.77	0.37	0.11	37.28	0.05
2	0.60	0.13	31.69	1.08	0.65	0.08	49.56	0.00
2a	0.61	0.09	48.93	3.81	0.57	0.11	39.30	1.18
3	0.16	0.15	15.92	4.68	0.09	0.10	16.00	1.30
3a	0.69	0.23	7.10	0.67	0.46	0.14	30.58	0.99
4	0.80	0.13	20.97	1.08	0.88	0.19	7.30	3.82
4a	0.38	0.19	19.13	4.67	0.44	0.13	32.22	0.17
5	0.43	0.17	20.27	0.86	0.35	0.19	14.02	0.27
5a	0.42	0.21	10.78	0.03	0.38	0.09	47.98	0.53
6	0.09	0.15	7.26	2.42	0.13	0.15	9.96	0.04
6a	0.34	0.19	13.71	0.50	0.30	0.10	42.40	1.96
6c	0.18	0.08	35.95	0.07	0.09	0.13	8.94	0.00
7	0.34	0.06	62.53	2.18	0.35	0.05	67.48	0.40
7a	0.40	0.02	91.90	0.32	0.37	0.02	86.84	0.20
7b	0.39	0.05	69.89	0.11	0.38	0.02	90.75	0.00
7c	0.34	0.04	75.58	0.14	0.33	0.05	71.01	0.98
8	0.30	0.06	60.02	0.81	0.35	0.02	90.25	0.00
8a	0.40	0.03	80.57	0.00	0.37	0.02	90.59	0.00
8b	0.39	0.02	88.07	0.95	0.38	0.02	90.89	0.95
8c	0.34	0.06	71.02	7.59	0.31	0.04	73.89	0.49
9	0.59	0.02	91.99	0.32	0.58	0.02	87.55	0.20
9a	0.55	0.07	58.23	0.08	0.58	0.05	70.20	0.40
9b	0.69	0.05	67.68	0.98	0.65	0.08	64.40	7.36
10a	0.41	0.14	27.24	0.04	0.40	0.10	42.53	0.47
10d	0.34	0.18	14.91	0.03	0.34	0.15	21.79	0.00

Table B.1: Fit of the True and Error Model to Replication Data in Samples 1 and 2.

Choice	Response Pattern				Indep	Est. parameters		TE model
No.	AA*	AB*	BA*	BB*	CHISQ_I	p	e	CHISQ_TE
1	52	28	25	106	45.2	0.68	0.15	0.17
1a	104	23	18	66	75.8	0.38	0.11	0.61
2	65	22	17	107	80.1	0.63	0.10	0.64
2a	71	12	25	103	88.5	0.59	0.10	4.46
3	144	25	19	23	30.6	0.12	0.12	0.82
3a	67	36	26	82	35.9	0.56	0.18	1.61
4	29	19	35	128	26.6	0.84	0.16	4.67
4a	92	34	19	66	52.3	0.41	0.15	4.18
5	89	30	32	60	34.0	0.39	0.18	0.06
5a	95	27	25	64	52.0	0.40	0.14	0.08
6	137	30	23	21	16.8	0.11	0.15	0.92
6a	108	24	26	53	51.0	0.32	0.14	0.08
6c	147	20	19	25	41.7	0.14	0.10	0.03
7	66	7	14	124	130.7	0.65	0.06	2.28
7a	78	4	4	125	178.7	0.62	0.02	0.00
7b	76	7	6	122	160.0	0.62	0.03	0.08
7c	65	10	6	130	146.5	0.67	0.04	0.99
8	64	6	9	132	149.5	0.67	0.04	0.59
8a	77	5	5	124	171.0	0.62	0.02	0.00
8b	78	4	4	125	178.7	0.62	0.02	0.00
8c	63	5	12	131	142.8	0.68	0.05	2.78
9	119	3	5	84	179.4	0.41	0.02	0.49
9a	106	12	11	82	128.1	0.44	0.06	0.04
9b	127	17	4	63	131.3	0.33	0.06	7.46
10a	68	20	24	98	68.9	0.59	0.12	0.36
10d	54	29	30	98	36.4	0.66	0.17	0.02

Table B.2: Frequencies of response patterns in two replications, aggregated over Samples 1 and 2 of Experiment 2 ($N = 211$).

Previous research has shown that there are individual differences in the rate of preference reversals in the same choice problems, as if some participants have more "noise" in their data. Would apparent violations of EU be eliminated or diminished if the error model allowed for individual differences in error rates? Put another way: is it possible that different sub-groups of participants who differ in their error rates also differ with respect to their conformance to EU? To investigate this question, participants of Experiment 2 were separated into groups, according to the number of preference reversals between two replicates, and the true and error model was fit in each group separately.

There were 107 "noisy" participants, who had 5 to 7 preference reversals between repetitions (out of 26 choice problems, i.e., 73% to 81% self-agreement), and 104 "low noise" participants who had 4 or fewer reversals (85% self-agreement or better). Analysis of partitioned data is presented in Table B.3. Estimated true choice probabilities are similar between these groups (correlation was 0.90), and the error rates for different choice problems were correlated ($r = 0.82$), but estimated values of e were as expected much higher in the "noisy" than "low noise" group (medians = .15 and .06, respectively). However, large violations of EU (compare Problems 1 and 3 and 4 and 6) appear in both groups, and the violations of RDU and CPT (compare Problems 1 and 1a, and 3 and 3a) also appear in both groups. Therefore, allowing for individual differences in noise levels does not change the conclusions with respect to EU, RDU, and CPT.

| Choice problem No. | Noisy Group (n = 107) | | | | Low Noise Group (n = 104) | | | |
| | TE parameters | | CHISQ | | TE parameters | | CHISQ | |
	p	*e*	Indep	TE	*p*	*e*	Indep	TE
1	0.65	0.20	14.74	2.42	0.71	0.11	34.47	1.77
1a	0.31	0.13	31.32	2.09	0.45	0.10	44.27	0.22
2	0.73	0.16	16.98	0.31	0.54	0.05	67.77	0.40
2a	0.68	0.15	26.28	3.75	0.53	0.06	64.82	0.81
3	0.07	0.16	4.27	0.14	0.16	0.09	32.36	0.99
3a	0.59	0.30	2.63	0.55	0.54	0.09	47.45	1.45
4	0.84	0.23	4.38	1.32	0.85	0.10	30.41	4.52
4a	0.37	0.24	9.53	5.05	0.44	0.08	52.20	0.07
5	0.33	0.23	7.63	0.11	0.43	0.13	29.93	0.66
5a	0.31	0.26	4.66	0.61	0.44	0.06	64.51	0.81
6	0.02	0.19	0.17	0.03	0.18	0.11	25.51	1.77
6a	0.21	0.20	8.31	0.12	0.40	0.08	48.49	0.00
6c	0.10	0.14	8.44	0.15	0.16	0.07	39.88	0.08
7	0.32	0.09	45.95	1.44	0.37	0.02	87.79	0.95
7a	0.42	0.04	76.85	0.00	0.35	0.00	104.00	0.00
7b	0.42	0.05	66.75	0.09	0.35	0.01	95.63	1.66
7c	0.31	0.07	57.13	1.86	0.36	0.02	91.44	0.32
8	0.30	0.07	53.30	1.12	0.35	0.01	99.70	0.83
8a	0.41	0.04	73.19	0.11	0.36	0.01	99.75	0.83
8b	0.42	0.03	80.31	0.14	0.35	0.01	99.70	0.83
8c	0.27	0.09	46.87	3.81	0.37	0.01	99.80	0.83
9	0.69	0.03	77.40	0.14	0.49	0.01	100.07	0.83
9a	0.68	0.09	42.93	0.22	0.46	0.03	84.91	0.20
9b	0.79	0.09	43.25	5.81	0.57	0.03	84.98	1.67
10a	0.45	0.15	26.08	0.33	0.37	0.09	44.09	0.06
10d	0.31	0.27	3.86	0.10	0.35	0.09	44.37	0.06

Table B.3: Fit of the True and Error Model for two groups of participants who differed in number of preference reversals between replicates.

Appendix C: Expanded True and Error Model (TE-4 Model)

Although the model in which each choice problem has a different error rate fits the data well (Appendix B), this appendix considers a more complex model (Birnbaum, 2012) in which the probability of making an error in a given choice problem can also depend on a person's true preference state. This assumption doubles the number of error rate parameters to estimate, but it might provide a means by which the EU model might be saved from data that would otherwise violate it. Thus, one would be adopting a more complex error model in hopes of saving a simpler model of decision-making.

Let e and f represent the probabilities of making an error given a person truly prefers S or R in the SR choice problem, and let e' and f' represent the probabilities of error given a person truly prefers S' or R' in the $S'R'$ problem, respectively. Since there are two choice problems and two error rates for each problem, there are now four error rates in each test of EU. Hence, this model is termed TE-4.

According to TE-4, the probability to show the RS' response pattern on both replications is as follows:

(C.1) $P(RS', RS') = p_{RR}(1-f)^2(f')^2 + p_{RS'}(1-f)^2(1-e')^2 + p_{SR'}(e)^2(f')^2 + p_{SS}(e)^2(1-e')^2$

where $P(RS', RS')$ is the probability to observe RS' response pattern on both replications, and the other terms are as defined in Equation (1). There are 16 such equations for the 16 possible response patterns; the 16 corresponding observed frequencies have 15 degrees of freedom. The model uses four true probabilities of the response patterns (which sum to 1, using 3 df) and 4 error terms, so it uses 7 df, leaving $15 - 7 = 8$ df to test the model.

Because the predicted frequencies of some cells are small in Experiment 2, cells representing the same combinations of response patterns were pooled prior to analysis, leaving 10 cell frequencies to be fit. (For example, frequencies of $SS'SR'$ and $SR'SS'$ were combined). Because these 10 frequencies sum to the number of participants, there are 9 df remaining in the data. There are at most 8 parameters to estimate from the data, which use 7 df (because $p_{SS'}$ + $p_{SR'} + p_{RS'} + p_{RR'} = 1$), leaving at least 2 degrees of freedom in the tests.

The EU-4 model is the special case of TE-4 in which $p_{RS'} = p_{SR'} = 0$. The difference in fit between TE-4 and EU-4 is theoretically Chi-Square distributed with 2 df. The TE-2 model is the special case of TE-4 in which $e = f$ and $e' = f'$; within TE-2, EU-2 is a further special case

in which $p_{RS'} = p_{SR'} = 0$. It is also possible to constrain the error terms to fit the replication data only (as in Table B.1); this constrained case of TE-2 is denoted TE-2c. The advantage of the constrained case is that the errors are estimated in a neutral way (from preference reversals in replications) that is independent of other assumptions, including EU.

This complex, TE-4 model could, in principle, provide an error theory in which errors might be able to save EU from rejection. As it turns out, however, even this model requires rejection of EU for the present data. For example, Tables C.1 and C.2 analyze Problems 1 and 3. Table C.1 presents estimated parameter values for each model and indices of fit. Table C.2 presents the observed frequencies that were fit and the corresponding predictions for four models: TE-2c, EU-2c, TE-4, and EU-4.

Note that the *G* values are acceptable for all three general versions of TE in both studies (Table C.1), so by the usual principle to prefer simpler models to more complex ones, there is no reason to adopt the more complex model. The most parsimonious, acceptable model is TE-2c in both samples. According to this model, 49% and 66% of the subjects violated EU by having *RS'* as their true preference patterns in Samples 1 and 2, respectively.

However, our goal in this case is not to choose the most parsimonious error model, but rather to ask if within this more complex error model, EU-4 might still be saved. The data give a clear answer to that question: "No." The difference in *G* between a TE model and its special case, EU model is theoretically $\chi^2(2)$. All six differences exceed 9.21, which is the critical value of $\chi^2(2)$ with $\alpha = 0.01$, so EU can be rejected in all cases. Table C.2 shows that the predictions of EU-4 fail to fit the observed frequencies well.

Another statistical test helps pinpoint where the data refute EU. We can use a binomial test of the observed frequency of a repeated response pattern violating EU (*SR'SR'* or *RS'RS'*): does it exceed predicted frequency according to EU and this error model? In Samples 1 and 2, the observed frequencies of *RS'RS'* are 26 and 43 out of 104 and 107 participants, respectively. In all six cases (two samples by three models), predictions of the best-fit EU model are much lower than observed. EU-4 in Samples 1 and 2 implies predicted probabilities for the *RS'RS'* pattern of only 0.161 and 0.210. The binomial probabilities to observe 26 or 43 (or more) out of 104 and 107 are 0.013 and .000005, respectively. Thus, even with four error terms that depend on a person's true preference ("safe" or "risky") and the choice problem, EU can be rejected because the *RS'RS'* pattern violating EU is observed too often.

Sample 1

Model	f	f'	e	e'	$p_{SS'}$	$p_{SR'}$	$p_{RS'}$	$p_{RR'}$	G
TE-4	0	0.10	0.34	0.15	0.58	0.04	0.27	0.11	0.50
EU-4	0	0.50	0.43	0.17	0.71	(0)	(0)	0.29	13.91
TE-2	(= e)	(= e')	0.17	0.14	0.35	0.02	0.49	0.14	2.46
EU-2	(= e)	(= e')	0.50	0.14	0.84	(0)	(0)	0.16	31.11
TE-2c	(= e)	(= e')	**0.17**	**0.15**	0.35	0.02	0.49	0.14	2.61
EU-2c	(= e)	(= e')	**0.17**	**0.15**	0.70	(0)	(0)	0.30	112.30

Sample 2

Model	f	f'	e	e'	$p_{SS'}$	$p_{SR'}$	$p_{RS'}$	$p_{RR'}$	G
TE-4	0	0	0.33	0.11	0.46	0.05	0.47	0.03	1.16
EU-4	0.19	0.50	0.50	0.04	0.66	(0)	(0)	0.34	36.52
TE-2	(= e)	(= e')	0.13	0.10	0.25	0.03	0.66	0.06	1.58
EU-2	(= e)	(= e')	0.50	0.10	0.91	(0)	(0)	0.09	51.27
TE-2c	(= e)	(= e')	**0.13**	**0.10**	0.25	0.03	0.66	0.06	1.60
EU-2c	(= e)	(= e')	**0.13**	**0.10**	0.75	(0)	(0)	0.25	278.37

Table C.1: Estimated parameters and index of fit of true and error models to the frequencies of response combinations for Problems 1 and 3. Parameters that have been fixed or constrained are shown in parentheses, respectively.

Response	Study 1					Study 2				
Pattern	Data	TE-2c	EU-2c	TE-4	EU-4	Data	TE-2c	EU-2c	TE-4	EU-4
SS'SS'	18	19.5	36.2	19.2	16.8	18	17.4	49.2	17.3	16.7
SS'SR'	7	3.7	6.6	3.4	3.4	0	2.2	5.6	2.1	0.9
SS'RS'	11	8.9	7.4	9.9	12.5	10	8.8	7.3	8.7	17.8
SS'RR'	2	1.9	1.9	1.8	2.6	1	1.1	1.1	1.0	2.0
SR'SS'	1	3.7	6.6	3.4	3.4	4	2.2	5.6	2.1	0.9
SR'SR'	2	1.7	1.8	2.0	0.7	2	2.2	1.0	2.5	0.3
SR'RS'	1	1.9	1.9	1.8	2.6	0	1.1	1.1	1.0	2.0
SR'RR'	2	1.9	3.4	1.0	0.5	1	1.0	2.5	1.2	1.4
RS'SS'	10	8.9	7.4	9.9	12.5	8	8.8	7.3	8.7	17.8
RS'SR'	2	1.9	1.9	1.8	2.6	2	1.1	1.1	1.0	2.0
RS'RS'	26	26.4	2.0	26.0	16.8	43	43.5	1.3	44.3	22.4
RS'RR'	7	6.0	3.1	5.5	9.4	4	5.4	2.0	5.3	6.6
RR'SS'	1	1.9	1.9	1.8	2.6	0	1.1	1.1	1.0	2.0
RR'SR'	0	1.9	3.4	1.0	0.5	2	1.0	2.5	1.2	1.4
RR'RS'	4	6.0	3.1	5.5	9.4	8	5.4	2.0	5.3	6.6
RR'RR'	10	8.1	15.7	10.0	7.8	4	4.7	16.4	4.0	6.1

Table C.2: Observed frequencies of response patterns (Data) and predicted frequencies for Problems 1 and 3, according to best-fit true and error models.

Tables C.3 and C.4 show the analysis of Problems 1a and 3a, which are the canonical split forms of Problems 1 and 3. According to EU and CPT, how branches are split in a presentation should have no effect on how people choose between gambles. Instead, Problems 1a and 3a give opposite modal choices compared to Problems 1 and 3. Table C.2 shows that for Problems 1 and 3, there were 58 and 73 people who showed *RS'* pattern at least once in Samples 1 and 2 compared with 12 and 9 who showed the *SR'* pattern (not counting the cases who showed both patterns, *SR'RS'*), so the pattern *RS'* was significantly more frequent, $z = 5.50$

and 7.07, respectively. In contrast, Table C.4 shows that pattern *SR'* was more frequent than *RS'* in Problems 1a and 3a of Samples 1 and 2 (49 to 16 and 35 to 22; *z* = -4.09 and -1.72, respectively).

This effect due to splitting is reflected in the best-fit parameters in Tables C.1 and C.3. Whereas Table C.1 shows that the estimated true percentage (TE-2c) of subjects with *RS'* patterns is 49% and 66% in Samples 1 and 2, respectively, corresponding percentages in Table C.3 are only 5% and 10%, respectively. Whereas estimated percentages in Table C.1 of *SR'* are only 2% and 3%, they are 29% and 18% in Table C.3, respectively.

As in Table C.1, EU can be rejected in all six tests of Table C.3; in addition, observed frequencies of repeated patterns violating EU are significantly greater than predicted by EU for one or both repeated violation patterns, via binomial tests. Again, EU can be rejected with any of the error models, and CPT can be rejected because of the effects of splitting.

Tables C.5 and C.6 show response pattern frequencies for other pairwise tests of EU and CPT, which provide additional evidence that neither EU nor CPT can be saved for these data by this more complex error model.

Based on the principle of parsimony, one would prefer TE-2 or TE-2c over TE-4 for our data, since these models use fewer parameters and achieve fairly good fits. However, our purpose in this appendix was not to find the most parsimonious description of data, but to ask if the more complex error models would allow us to retain the EU or CPT models. The results show that even when we allow these additional parameters, neither EU nor CPT can account for the systematic patterns of repeated violations of common consequence independence and coalescing.

Tables C.3, C.4, C.5, and C.6 on next pages.

Sample 1

Model	f	f'	E	e'	$p_{SS'}$	$p_{SR'}$	$p_{RS'}$	$p_{RR'}$	G
TE-4	0.21	0.00	0.02	0.44	0.48	0.00	0.22	0.29	2.82
EU-4	0.17	0.24	0.06	0.45	0.52	(0)	(0)	0.48	14.98
TE-2	$(= e)$	$(= e')$	0.11	0.23	0.30	0.29	0.05	0.36	17.33
EU-2	$(= e)$	$(= e')$	0.11	0.37	0.57	(0)	(0)	0.43	30.14
TE-2c	$(= e)$	$(= e')$	**0.11**	**0.23**	0.30	0.29	0.05	0.36	17.36
EU-2c	$(= e)$	$(= e')$	**0.11**	**0.23**	0.54	(0)	(0)	0.46	43.35

Sample 2

Model	f	f'	e	e'	SS'	SR'	RS'	RR'	G
TE-4	0.21	0.09	0.04	0.17	0.42	0.11	0.17	0.30	1.17
EU-4	0.20	0.31	0.07	0.30	0.55	(0)	(0)	0.45	24.09
TE-2	$(= e)$	$(= e')$	0.11	0.14	0.44	0.18	0.10	0.28	2.95
EU-2	$(= e)$	$(= e')$	0.12	0.31	0.61	(0)	(0)	0.39	26.21
TE-2c	$(= e)$	$(= e')$	**0.11**	**0.14**	0.44	0.18	0.10	0.28	2.96
EU-2c	$(= e)$	$(= e')$	**0.11**	**0.14**	0.60	(0)	(0)	0.40	52.71

Table C.3: Estimated parameters and fit of true and error models to the frequencies of response combinations for Choice Problems 1a and 3a, as in Table C.1.

Response	Sample 1					Sample 2				
Pattern	Data	TE-2c	EU-2c	TE-4	EU-4	Data	TE-2c	EU-2c	TE-4	EU-4
SS'SS'	13	16.0	26.3	15.6	14.6	29	28.2	37.9	29.1	25.6
SS'SR'	16	8.8	8.1	12.0	12.3	8	6.3	6.1	6.9	11.1
SS'RS'	2	2.5	3.6	1.6	1.3	2	4.3	4.8	3.3	2.6
SS'RR'	1	1.9	1.9	1.2	2.0	0	1.2	1.2	1.1	2.4
SR'SS'	11	8.8	8.1	12.0	12.3	6	6.3	6.1	6.9	11.1
SR'SR'	10	15.4	2.8	10.5	10.8	11	12.3	1.3	11.0	5.4
SR'RS'	2	1.9	1.9	1.2	2.0	0	1.2	1.2	1.1	2.4
SR'RR'	8	4.1	3.1	5.9	4.8	8	3.7	3.2	4.9	3.9
RS'SS'	2	2.5	3.6	1.6	1.3	5	4.3	4.8	3.3	2.6
RS'SR'	1	1.9	1.9	1.2	2.0	3	1.2	1.2	1.1	2.4
RS'RS'	6	4.3	2.5	4.7	1.9	8	7.3	1.2	8.2	3.1
RS'RR'	3	6.2	6.8	3.6	6.1	4	3.9	4.1	3.3	6.7
RR'SS'	0	1.9	1.9	1.2	2.0	1	1.2	1.2	1.1	2.4
RR'SR'	4	4.1	3.1	5.9	4.8	2	3.7	3.2	4.9	3.9
RR'RS'	3	6.2	6.8	3.6	6.1	3	3.9	4.1	3.3	6.7
RR'RR'	22	17.8	22.0	22.1	19.7	17	17.7	25.1	17.2	14.7

Table C.4: Observed and Predicted Frequencies of response patterns for Choice Problems 1a and 3a, as in Table C.2.

	Choice Problems			
Pattern	4 X 6	4a X 6a	2 X 5	2a X 5a
SS'SS'	18	75	34	55
SS'SR'	5	6	10	3
SS'RS'	17	12	12	6
SS'RR'	1	9	0	3
SR'SS'	5	8	10	10
SR'SR'	**1**	**2**	**11**	**3**
SR'RS'	1	7	1	1
SR'RR'	0	6	9	2
RS'SS'	29	12	10	15
RS'SR'	5	1	3	3
RS'RS'	**73**	**9**	**33**	**19**
RS'RR'	19	7	17	18
RR'SS'	1	2	3	4
RR'SR'	0	4	1	3
RR'RS'	16	9	18	10
RR'RR'	20	41	39	56
Total	211	210	211	211
G (EU-4)	18.85	1.79	28.06	11.88
SR'	11	26	41	21
RS'	154	49	90	68
z	11.13	2.66	4.28	4.98
Binomial	0.222	0.028	0.108	0.05
	p(x >=73)	p(x >=9)	p(x >= 33)	p(x >=19)
	0.000026	0.138	0.019	0.010

Table C.5: Frequencies of response patterns in other tests of EU from the main design.

Response	Choice Problems					
Pattern	1 X 1a	3 X 3a	4 X 4a	6 X 6a	6a X 6c	5 X 5a
SS'SS'	41	61	24	86	92	61
SS'SR'	4	27	4	18	10	7
SS'RS'	20	4	9	11	15	9
SS'RR'	1	3	5	0	4	6
SR'SS'	4	14	1	17	5	11
SR'SR'	3	**42**	0	**16**	1	**10**
SR'RS'	2	4	3	4	2	6
SR'RR'	5	14	2	15	3	9
RS'SS'	17	2	20	10	22	14
RS'SR'	2	4	4	1	1	5
RS'RS'	**26**	0	**38**	1	**18**	**11**
RS'RR'	16	2	21	5	5	9
RR'SS'	3	6	4	5	2	2
RR'SR'	3	7	7	7	1	11
RR'RS'	9	2	11	0	10	6
RR'RR'	55	19	57	15	20	34
Total	211	211	210	211	211	211
G (EU-4)	12.93	12.79	9.32	1.40	7.40	8.69
SR'	19	104	14	73	20	48
RS'	88	10	99	27	70	49
z	6.67	-8.80	8.00	-4.60	5.27	0.10
Binomial	0.036	0.0036	0.0096	0.278	0.0442	0.0054

Table C.6: Response patterns in tests of coalescing (tests of EU and CPT).