

Reply: Birnbaum’s (2012) statistical tests of independence have unknown Type-I error rates and do not replicate within participant

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Abstract

Birnbaum (2011, 2012) questioned the iid (independent and identically distributed) sampling assumptions used by state-of-the-art statistical tests in Regenwetter, Dana and Davis-Stober’s (2010, 2011) analysis of the “linear order model”. Birnbaum (2012) cited, but did not use, a test of iid by Smith and Batchelder (2008) with analytically known properties. Instead, he created two new test statistics with unknown sampling distributions.

Our rebuttal has five components: 1) We demonstrate that the Regenwetter et al. data pass Smith and Batchelder’s test of iid with flying colors. 2) We provide evidence from Monte Carlo simulations that Birnbaum’s (2012) proposed tests have unknown Type-I error rates, which depend on the actual choice probabilities and on how data are coded as well as on the null hypothesis of iid sampling. 3) Birnbaum analyzed only a third of Regenwetter et al.’s data. We show that his two new tests fail to replicate on the other two-thirds of the data, within participants. 4) Birnbaum selectively picked data of one respondent to suggest that choice probabilities may have changed partway into the experiment. Such nonstationarity could potentially cause a seemingly good fit to be a Type-II error. We show that the linear order model fits equally well if we allow for warm-up effects. 5) Using hypothetical data, Birnbaum (2012) claimed to show that “true-and-error” models for binary pattern probabilities overcome the alleged short-comings of Regenwetter et al.’s approach. We disprove this claim on the same data.

Keywords: binary choice models, true-and-error models, iid sampling, statistical testing.

1 Introduction.

Imagine that you are offered the choice between two wheels of chance, as displayed in Figure 1. The chosen wheel of chance, in such a gamble pair, if played for real

money, will be spun. If the black part of the wheel is oriented towards the Dollar amount when it stops (which is the case in both wheels as displayed in Figure 1) then you win the indicated amount, otherwise nothing. In the left gamble of Figure 1 you can win \$25.2 (with 37.5% chance), whereas in the right gamble you can win \$22.4 (with 48.8% chance). As the screenshot shows, the numerical probabilities of winning are not provided. The decision maker depends on the relative size of the black shaded area to evaluate the chance of winning. When offered such stimuli repeatedly, decision makers tend to fluctuate in the choices they make. For over 50 years, it has been a point of debate how one can model choice variability formally. A natural approach is to model choice behavior probabilistically.

The authorship of this paper is alphabetical. Analyzing the same data several times can be problematic because it may over-utilize the degrees of freedom available and it may lead to selection biases in publication. To avoid publication bias, the authors and the Regenwetter laboratory did not carry out any tests of iid sampling assumptions on the Regenwetter et al. (2011) data other than those reported in this paper. The authors also did not inspect any data of Regenwetter et al. (2011) other than Birnbaum’s (2012) Table 2, to inform their hypotheses or analyses. This means that the correct number of degrees of freedom for Participant 2, Cash I, is unknown for the reduced data sets where 4 trials are dropped. No other analyses are affected by the data inspection. We thank Jason Dana, Clintin Davis-Stober, Marc Jekel, A. A. J. Marley, and Anna Popova, for critical comments on earlier versions, as well as Greg Francis and Uri Simonsohn for useful references. This work was supported by National Science Foundation grants SES-DRMS # 08-20009 (PI: M. Regenwetter) and SES-DRMS # 1062045 (PI: M. Regenwetter), and by an Arnold O. Beckman Research Award (PI: M. Regenwetter), awarded by the University Research Board of the University of Illinois at Urbana-Champaign. Any opinions, findings, and conclusions or recommendations expressed in this publication are those of the authors and do not necessarily reflect the views of colleagues, the National Science Foundation, the Korea Institute of Public Finance, or the University of Illinois. Address: Michel Regenwetter, Department of Psychology, 603 E. Daniel St., Champaign, IL 61820. Email: regenwet@illinois.edu

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Regenwetter, Dana and Davis-Stober (2010, 2011) [henceforth RDDS] investigated a mathematical model of binary choice probabilities with a distinguished history in economics, operations research, and psychology, whose mathematical structure has been studied intensely over several decades (see, e.g., Becker, DeGroot, & Marschak; 1963, Block & Marschak, 1960, Bolotashvili, Kovalev, & Girlich, 1999; Cohen & Falmagne, 1978, 1990; Fiorini, 2001; Fishburn, 1992; Fishburn & Falmagne, 1989; Gilboa, 1990; Grötschel, Jünger & Reinelt, 1985; Heyer & Niederée, 1992; Koppen, 1991, 1995; Marschak, 1960), but for which there did not previ-

Figure 1: Screen shot of a Cash I paired-comparison stimulus (see also RDDS, Figure 2)



ously exist an appropriate statistical test. This model has been studied under several labels, including “binary choice model”, “linear ordering polytope”, “random preference model”, “random utility model” and “rationalizable model of stochastic choice”, and it has been stated in several different mathematical forms that make the same empirical predictions (see, e.g., Fishburn, 2001; Regenwetter & Marley, 2001). We will refer to it as the *linear order model*. According to this model, preferences form a probability distribution over linear orders, i.e., over rankings without ties. The probability that a person chooses one gamble over another is the probability that s/he ranks the chosen gamble higher than the non-chosen gamble. Denote the probability that a person chooses x (say, the left gamble in Figure 1) over y (say, the right gamble in Figure 1) as P_{xy} . The linear order model makes restrictive predictions: It requires that the *triangle inequalities* hold, according to which, for all distinct choice options a, b, c ,

$$P_{ab} + P_{bc} - P_{ac} \leq 1. \quad (1)$$

This model has a particular mathematical form that long eluded statistical testing: For inequality constraints like these, standard likelihood ratio tests are not applicable, goodness-of-fit statistics need not satisfy the familiar asymptotic χ^2 (Chi-squared) distributions, and it is not even meaningful to count parameters (e.g., binary choice probabilities) to obtain degrees of freedom of a test. Formally adequate statistical tests for such models have been discovered only recently (Davis-Stober, 2009). Regenwetter, Dana and Davis-Stober (2010, 2011) were the first to carry out such a state-of-the-art “order-constrained” test of the linear order model. Even breakthrough results come at a price: To our knowledge, there does currently not exist a statistical test for the triangle inequalities that does not assume iid (*independent and identically distributed*) sampling of empirical observations.

Notice that the triangle inequalities (1) make no mention of time, of the individual making these choices, or of repeated observations. They do not require binary

choice probabilities to remain constant over time or be the same for different decision makers (i.e., they do not require an identical distribution), nor do they require binary choices to be made stochastically independently of each other (see Regenwetter, submitted, for a thorough discussion). Birnbaum (2011, 2012) [henceforth MB] has questioned the iid sampling assumption used by RDDS’s statistical test and recommended his own models, the so called “true-and-error” models, as an alternative. Regenwetter (submitted) shows that MB is mistaken to attribute the iid assumption to the linear order model itself, i.e., to the triangle inequalities (1). Among the leading models of probabilistic choice, the linear order model stands out in being invariant under non-stationary choice probabilities (i.e., invariant under certain violations of the “identically distributed” part of “iid”). Regenwetter (submitted) also shows that, in contrast, a number of published papers on “true-and-error” models do, in fact, require binary choice to be iid in both the model formulation and in the statistical test.

The main concern in this paper, however, is with Birnbaum’s (2012) claim that the iid assumption, used by the state-of-the-art test in RDDS, is violated in the RDDS data. We will first provide a brief introduction to the RDDS experiment, then show that the RDDS data pass a well-known test of iid sampling without a hitch. We then document that Birnbaum’s (2012) proposed tests of iid sampling have unknown Type-I error rates that even appear to change with the way in which binary choices are coded, and do not actually appear to test iid sampling per se. We add to this conclusion the finding that Birnbaum’s (2012) tests fail to replicate within participant. We provide evidence against MB’s suggestion that the excellent model performance in RDDS might be a Type-II error in which warm-up effects could have made binary choice probabilities shift early in the experiment and then choice probabilities violating the model could have accidentally averaged to satisfy the triangle inequalities. Last but not least, we use Birnbaum’s own (2012) hypothetical data to disprove MB’s claim that “true-and-error” models overcome alleged limitations of the linear order model.

2 The experiment of RDDS.

In a seminal paper, Tversky (1969) used a collection of 5 distinct wheels of chance and formed all 10 possible pairs of these gambles. Faced with variability in behavior, he presented each decision maker 20 times with each gamble pair in an effort to assess their preferences among the five gambles from the observed choice proportions. Tversky augmented the set by 10 “irrelevant” distractor pairs, also presented 20 times. In other words, he presented his participants with a sequence of 400 binary choices like the

Table 1: First 36 out of 800 pairwise choices of Participant # 100 in RDDS.

Trial	Stimulus Set	Left gamble L	Right gamble R	Observed Choice
1	Cash I	41.7% chance of \$23.8	33.3% chance of \$26.6	R
2	Distractor	18% chance of \$27.5	12% chance of \$31.43	L
3	Noncash	24% chance of ~ 4 music CDs	20% chance of ~ 7 paperbacks	R
4	Cash II	36% chance of \$24.44	28% chance of \$31.43	<u>R</u>
5	Cash I	37.5% chance of \$25.2	45.8% chance of \$22.4	R
6	Distractor	24% chance of \$22	16% chance of \$22	L
7	Noncash	22% chance of ~ 40 movie rentals	26% chance of ~ 40 coffees	L
8	Cash II	40% chance of \$22	32% chance of \$27.5	<u>R</u>
9	Cash I	41.7% chance of \$23.8	29.2% chance of \$28	L
10	Distractor	4% chance of ~ 40 coffees	20% chance of ~ 4 music CDs	R
11	Noncash	18% chance of ~ 15 sandwiches	24% chance of ~ 4 music CDs	L
12	Cash II	36% chance of \$24.44	44% chance of \$20	<u>R</u>
13	Cash I	37.5% chance of \$25.2	33.3% chance of \$26.6	L
14	Distractor	6% chance of ~ 40 coffees	16% chance of ~ 7 paperbacks	R
15	Noncash	20% chance of ~ 7 paperbacks	22% chance of ~ 40 movie rentals	L
16	Cash II	28% chance of \$31.43	40% chance of \$22	<u>L</u>
17	Cash I	45.8% chance of \$22.4	29.2% chance of \$28	L
18	Distractor	8% chance of ~ 7 paperbacks	16% chance of ~ 40 coffees	L
19	Noncash	26% chance of ~ 40 coffees	18% chance of ~ 15 sandwiches	R
20	Cash II	36% chance of \$24.44	32% chance of \$27.5	<u>R</u>
21	Cash I	37.5% chance of \$25.2	41.7% chance of \$23.8	R
22	Distractor	14% chance of \$22	26% chance of \$22	R
23	Noncash	22% chance of ~ 40 movie rentals	24% chance of ~ 4 music CDs	L
24	Cash II	44% chance of \$20	28% chance of \$31.43	<u>L</u>
25	Cash I	45.8% chance of \$22.4	33.3% chance of \$26.6	L
26	Distractor	8.3% chance of \$28	25% chance of \$25.2	R
27	Noncash	20% chance of ~ 7 paperbacks	26% chance of ~ 40 coffees	L
28	Cash II	40% chance of \$22	36% chance of \$24.44	<u>R</u>
29	Cash I	37.5% chance of \$25.2	29.2% chance of \$28	R
30	Distractor	28% chance of \$20	12% chance of \$27.5	L
31	Noncash	22% chance of ~ 40 movie rentals	18% chance of ~ 15 sandwiches	R
32	Cash II	32% chance of \$27.5	44% chance of \$20	<u>R</u>
33	Cash I	45.8% chance of \$22.4	41.7% chance of \$23.8	L
34	Distractor	10% chance of ~ 40 coffees	12% chance of ~ 15 sandwiches	R
35	Noncash	26% chance of ~ 40 coffees	24% chance of ~ 4 music CDs	R
36	Cash II	36% chance of \$24.44	28% chance of \$31.43	R

Note: The symbol ~ stands for “approximately”.

one in Figure 1. The 400 pairwise choices were spread over five weekly test sessions of 80 choices each. RDDS

built on this design by asking altogether twice as many pairwise choices, but in a single session and using a com-

puter. They considered three distinct sets of 5 lotteries, as well as some Distractor items. Their Cash I set was the contemporary dollar equivalent of Tversky's (1969) stimuli, whereas their Cash II and Noncash stimulus sets were new. Like Tversky (1969) they presented each lottery pair 20 times (except for the Distractors, which varied).

Table 1 gives an example summary of the first 36 trials of the RDDS experiment¹ for Participant #100. On Trial 1, the decision maker faced the choice between a "41.7% chance of winning \$23.8" (presented as a wheel of chance on the left side of the screen) and a "33.3% chance of winning \$26.6" (presented on the right side). This was a stimulus from the Cash I set. The decision maker chose the gamble presented on the right. On Trial 2, the decision maker was presented with the first of 200 Distractor items, which were intended to interfere with the memory of earlier choices, thus making it difficult to recognize repeated items. Trial 3 involved gambles for non-cash prizes, namely a "24% chance of winning a gift certificate worth approximately 4 music CDs" or a "20% chance of winning a gift certificate for approximately 7 paperback books".

In Table 1, Trials 4, 12, 20, 28, and 36 are set apart by horizontal lines. The lottery with a "36% chance of winning \$24.44" was presented in each of these five trials. The side of the screen on which each lottery appeared was randomized. The lottery presentation, unbeknownst to the participant, cycled through the four stimulus sets in the order Cash I, Distractor, Noncash, Cash II. The pair of lotteries presented in a given trial was picked randomly from its stimulus set with the constraint that it had not appeared in the previous five trials from that set; and each individual lottery was chosen with the constraint that it had not appeared in the previous trial from that set. This is why the trials involving a "36% chance of winning \$24.44" are separated by at least eight pairwise choices, and why the repetition of the lottery pair in Trial 4 did not occur until at least 24 trials later; in this case, it was in Trial 36.

Many prominent probabilistic models of choice in the behavioral and economic sciences, including the models that Tversky (1969) considered,² assume that the decision maker has a single fixed deterministic preference state throughout the experiment and that variability in observed choices is due to noise or error in one form

¹These followed an initial set of 18 trials, not shown in the table, designed to familiarize the participant with the task.

²Tversky attempted to reject weak stochastic transitivity in favor of a modal choice model of a lexicographic semiorder. See Regenwetter (submitted) for an explanation and mathematical proofs about what is allowed to vary and what is required to be fixed, in such models, as well as the role of independence assumptions in these models and in statistical tests of these models. See Iverson and Falmagne (1985) and RDDS for an explanation why Tversky's (1969) attempt did not succeed, despite hundreds of citations of Tversky's paper reporting that it succeeded.

or another (Becker et al., 1963; Birnbaum, 2004; Block & Marschak, 1960; Carbone & Hey, 2000; Harless & Camerer, 1994, 1995; Hey, 1995, 2005; Hey & Carbone, 1995; Loomes, 2005; Loomes, Moffatt & Sugden, 2002; Loomes, Starmer & Sugden, 1991; Loomes & Sugden, 1995, 1998; Luce, 1959; Luce & Suppes, 1965; Marschak, 1960; Tversky, 1969). The linear order model tested in RDDS models preferences themselves as probabilistic. For example, a decision maker could be uncertain about what he or she prefers on a given trial.

The statistical test currently available for such models requires that one can combine multiple observations together to estimate choice probabilities from choice proportions:

1. Writing x for the lottery with a "36% chance of winning \$24.44" and y for the lottery with a "28% chance of \$31.43," the statistical test in RDDS treated Trials 4 and 36 as two independent draws from a single underlying Bernoulli process with probability P_{xy} of choosing x over y .
2. Similarly, for two lottery pairs, say, x versus y and a versus b , from the same stimulus set (thus separated by at least 4 trials), the statistical test assumed that those two binary choices were independent draws from two Bernoulli processes with probabilities P_{xy} and P_{ab} .

Because each stimulus set was analyzed separately, there was no assumption about the relationship between choices from different stimulus sets, say, the choices made on Trials 1 and 2, for instance. The iid assumptions applied only to choices within stimulus set. These two assumptions allow a researcher to use choice proportions as estimators of choice probabilities and this is precisely how they are routinely used in quantitative analyses of probabilistic choice models in psychology, econometrics, and related disciplines. It is the first iid sampling assumption above that MB has questioned. Birnbaum (2012) claims that this iid assumption is violated in the RDDS data. We will now consider the legitimacy of that inference.

3 A test of iid sampling suggested in Smith and Batchelder (2008).

Smith and Batchelder (2008, p. 727) provided a statistical test of iid sampling in binary choice data. Birnbaum (2012) cited Smith and Batchelder but left out any application of their test. We fill this gap by implementing Smith and Batchelder's (2008) test on the RDDS data. This test uses the analytically derived expected value and standard error of a particular test statistic.

Let (i, i') denote some gamble pair. Throughout this section, we will enumerate only the 20 trials that the gamble pair (i, i') was presented in RDDS (not the 800 trials of their experiment). So, Trials 4 and 36 in Table 1 become $t = 1$ and $t = 2$ for the gamble pair (i, i') where i : “36% chance of \$24.44” and i' : “28% chance of \$31.43”. For $t \in \{1, 2, \dots, 20\}$, let

$$X_{it} = \begin{cases} 1 & \text{if the decision maker chooses} \\ & \text{alternative } i \text{ on trial } t, \\ 0 & \text{otherwise.} \end{cases}$$

We wish to test, for each gamble pair (i, i') , whether the X_{it} result from 20 independent and identically distributed Bernoulli trials, for $t = 1, 2, \dots, 20$. By Smith and Batchelder (2008, p. 727), we can consider the following quantity defined in their Eq. 21, which checks for a 1-step choice reversal from trial t to trial $t + 1$:

$$A_{it} = \begin{cases} 1, & \text{if } X_{i,t} \neq X_{i,t+1}, \\ 0, & \text{otherwise.} \end{cases}$$

Based on this quantity, we consider the number of 1-step choice reversals given by

$$A_i = \sum_{t=1}^{19} A_{it}. \quad (2)$$

By Smith and Batchelder (2008), if the 20 Bernoulli trials are independent and identically distributed, i.e., have fixed probability θ_i of success, we must have

$$E(A_i) = 38 \theta_i (1 - \theta_i). \quad (3)$$

We give a proof in the Appendix. Smith and Batchelder (2008) did not provide a standard error for A_i . We show in the Appendix that the standard error equals

$$SE(A_i) = \sqrt{38 \theta_i (1 - \theta_i) (1 - 2\theta_i (1 - \theta_i))}. \quad (4)$$

Table 2 shows the results of this test when applied to the RDDS data. For each of the 18 participants in Regenwetter et al. (2011), we carried out 30 tests (for the 30 binomials that RDDS use for each person, 10 for each of Cash I, Cash II, and Noncash) to see whether we may assume, for each of the 30 gamble pairs, iid draws from a fixed Bernoulli process to obtain 30 distinct binomials per respondent. This analysis of iid sampling involved a total of $18 \times 30 = 540$ hypothesis tests.³ We determined confidence intervals using the point estimate given in our Eq. 3 and the standard error, $SE(A_i)$ given in our Eq. 4. We report the number of significant violations (marked in **bold**)

³Besides a full analysis of all data sets, the table also provides an analysis for the reduced data sets where we dropped the first four trials for each participant and for each gamble pair. We provide the rationale for this analysis later. Table 2 shows no major changes when we drop the first four trials.

using a margin of error of $2 SE(A_i)$, or $1.96 SE(A_i)$ (reported in parentheses, when different).

Since we are looking for evidence of mistaken acceptance of the linear order model in RDDS, we also provide an analysis where we leave out the two data sets (Cash I & II, Participant 16) where Regenwetter et al. (2011) already rejected the linear order model (underlined). Rejections by Smith and Batchelder’s (2008, Eq. 21) test occurred at a rate of $\sim 3\%$, well within standard Type I error range. There is no reason to conclude, based on this test, that the binary choices of each individual in Regenwetter et al.’s (2011) data were anything but independent and identically distributed Bernoulli trials, hence that the choice frequencies originated from anything but Binomials. The null hypothesis of iid sampling in RDDS is retained in this hypothesis test.

4 Type-I error rates of Birnbaum’s (2012) tests.

In contrast to Smith and Batchelder’s test statistic, whose expected value and standard error we reviewed above, Birnbaum (2012) created two new test statistics with unknown sampling distributions. Using these new statistics, Birnbaum (2012) estimated two quantities p_ν and p_r from the data and, without formal proof, interpreted these quantities p_ν and p_r as p-values of tests of iid sampling for a given participant in RDDS. Birnbaum concluded that a data set in RDDS violates the iid assumption “significantly” (at an α level of 0.05) when $p_\nu < 0.05$, respectively, when $p_r < 0.05$.

To better understand Birnbaum’s test statistics, we can borrow tools from an ongoing debate in the behavioral, statistical, and medical sciences. That debate is primarily concerned about “publication bias”, “p-hacking”, “data peeking”, the “file drawer problem”, etc. (Francis, 2012a, 2012b; Ioannidis & Trikalinos, 2007; Macaskill, Walter & Irwig, 2001; Simmons, Nelson & Simonsohn, 2011). We tap into some of the tools with which this literature investigates whether reported p-values match what is expected for a given set of hypotheses and a given effect size. Specifically, we build on the fact that p-values are, themselves, random variables (Murdoch, Tsai & Adcock, 2008). The p-values of a continuous statistic must satisfy a uniform distribution under the null hypothesis, whereas the p-values of finite statistics can display more complicated behavior (Gibbons & Pratt, 1975; Hung, O’Neill, Bauer & Kohne, 1997; Murdoch et al., 2008). We will consider the distribution of Birnbaum’s (2012) p_ν - and p_r -values. In particular, we use Monte Carlo simulation to check how closely the actual Type-I error rates match the stated nominal α -level of each test (Little, 1989).

Table 2: Test of iid binary choice following Eq. 21 and text in Smith and Batchelder (2008, p. 727).

Particip.	Full Data Sets				First 4 Trials removed			
	Total 30 tests	Cash I 10 tests	Cash II 10 tests	Noncash 10 tests	Total 30 tests	Cash I 10 tests	Cash II 10 tests	Noncash 10 tests
1	1	0	0	1	1	0	1	0
2	0	0	0	0	0	0	0	0
3	0	0	0	0	0	0	0	0
4	1 (2)	1	0 (1)	0	1	1	0	0
5	0	0	0	0	0	0	0	0
6	3	1	2	0	3	1	2	0
7	0	0	0	0	0	0	0	0
8	0	0	0	0	0	0	0	0
9	1	1	0	0	1	1	0	0
10	0	0	0	0	0	0	0	0
11	0	0	0	0	0	0	0	0
12	1	0	0	1	0	0	0	0
13	2	1	1	0	0	0	0	0
14	0	0	0	0	0	0	0	0
15	4	2	2	0	3	1	2	0
16	4	<u>2</u>	<u>2</u>	0	3	<u>2</u>	<u>1</u>	0
17	0	0	0	0	0	0	0	0
18	0	0	0	0	0	0	0	0
# signif.	17 (18)	8	7 (8)	2	12	6	6	0
# tests	540	180	180	180	540	180	180	180
After dropping Cash I and Cash II for Participant 16:								
# signif.	13 (14)	6	5 (6)	2	9	4	5	0
# tests	520	170	170	180	520	170	170	180

Figures 2 and 3 show five histograms for the distribution of p_ν -values and p_r -values (computed separately for p_ν and p_r) for five different sets of binary choice probabilities. Each histogram tallies the distribution of p -values for 3,000 simulated iid samples.⁴ In each case, we expect 100 p -values per bin under the null hypothesis, as indicated by the horizontal line. Even for 3,000 simulated iid samples, the actual observed numbers of p -values in each bin varies substantially around that expected number. For p_ν in Figure 3, a Kolmogorov-Smirnoff test comes out significant in each histogram, suggesting that the p_ν -values are not uniformly distributed as they should be if we treat the underlying statistic as a continuous random variable. Furthermore, it appears that the distribu-

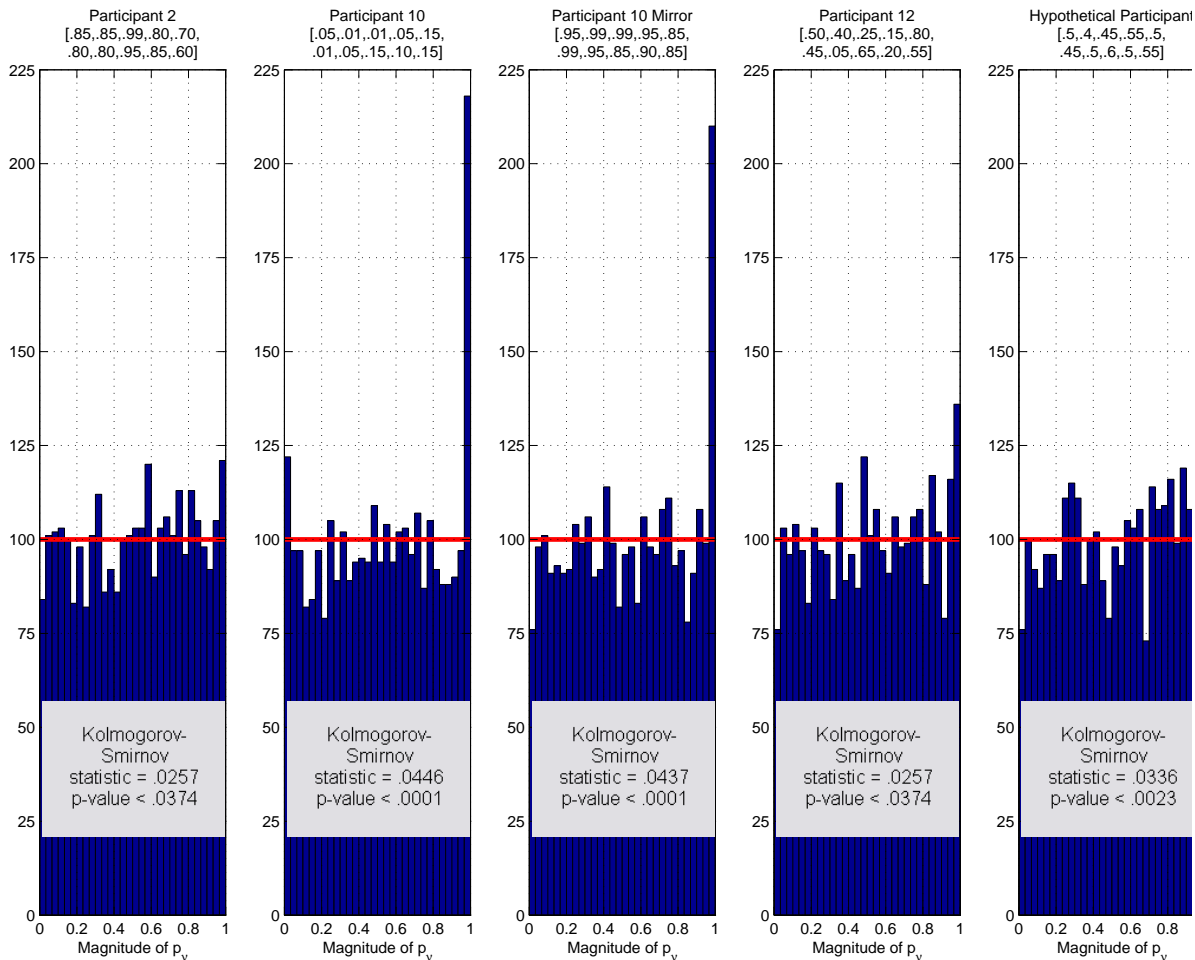
tion of p_ν -values is different for different choice probabilities, even though in each case, the data were simulated via the null hypothesis of iid sampling. The distribution of p_ν -values appears to reflect other properties of the data, not just whether or not iid holds.⁵ In Figure 3, a Kolmogorov-Smirnoff test comes out significant in one of the five cases, suggesting that the p_r -values in question are not uniformly distributed as they should be under the null hypothesis.

Table 3 provides comparisons of the simulated sampling distributions for p_ν and p_r among pairs of binomial collections that generated the iid data. For the comparison of the simulated sampling distributions, the Kolmogorov-Smirnov test finds the three pairs differ significantly from each other for p_ν . The corresponding test for p_r did not yield any significant disagreements among

⁴Our Table 5 shows that we replicated the value of p_ν (using only 10,000 pseudo-random permutations to save computation time) that Birnbaum (2012) provided for Cash I (using 100,000 permutations). Even running the Monte Carlo simulation with 10,000 pseudo-random permutations per run used up months of computer time.

⁵The Smith and Batchelder test explicitly accounts for binary choice probabilities by incorporating them into Eqs. 3 and 4.

Figure 2: Illustrative analysis of the sampling distribution of p_v approximated through 3,000 simulated iid data sets using the maximum likelihood binomial parameters of three participants from Regenwetter et al. (2011) Cash I, and a hypothetical participant. The underlying binomial probabilities are given above the histograms. The expected frequency in each bin under the uniform null is given by the horizontal line. The Kolmogorov-Smirnov statistic is significant in each case, i.e., each distribution differs significantly from a uniform on $[0, 1]$.



simulated sampling distributions, even though Figure 3 suggests a deviation from uniformity for the sampling distribution for p_r on data simulated from the collection of binomials that best fits Participant 10 in Cash I of RDDS. This makes the analysis for p_r somewhat more ambiguous.

In each histogram of Figures 2 and 3, the left tail of the distribution is of utmost importance, because it shows how often one will observe small p-values when the null hypothesis holds. This means that the left tail of the histogram gives an idea of Type-I error rates: A spike in the left tail suggests that the Type-I error rate is higher than it should be, because there are too many small p-values. A trough in the left tail suggests a conservative test because there are not enough small p-values to reject the null at a rate of α when we use a significance level of α .

The requirement that a p-value be uniformly distributed under the null hypothesis applies only for continuous statistics. However, the novel statistics underlying p_v and p_r can, in fact, only take finitely many different values in data like those in RDDS. Therefore, it may be more informative to compare nominal Type-I error rates (α) with the actual rates of false rejection, for various nominal Type-I error rates when analyzing data sets that we know to be iid. We report this in Table 4. There appears to be little rhyme or reason to the actual Type-I error rates. For p_v , the test appears to be conservative, except for data simulated according to Participant 10's best fitting binomials. Strangely, though, as we move from Participant 10 to its "mirror," where we replaced the binomial probabilities of "success" by probabilities of

Table 3: Comparison of simulated sampling distributions for p_ν and p_r for different collections of binomials.

Paired comparison among Binomial collections	Kolmogorov-Smirnov test for p_ν		Kolmogorov-Smirnov test for p_r	
	Statistic	p-value	Statistic	p-value
Participants 2 vs. 10	.0437	.01	0.0317	.10
Participants 2 vs. 12	.0113	.99	0.0257	.27
Participants 2 vs. hypothetical	.0207	.54	0.0227	.42
Participants 10 vs. 12	.0430	.01	0.0143	.92
Participants 10 vs. hypothetical	.0433	.01	0.0287	.17
Participants 12 vs. hypothetical	.0163	.82	0.0210	.52
Participants 10 vs. 10 Mirror	0.019	.65	0.0297	.14

“failure” the test⁶ is no longer conservative, even though these binary choice probabilities are the same and differ only by how pairwise choices are labeled. A test of iid should not depend on whether “a pairwise choice of x over y ” is always coded as “a success (for x)” or always coded as “a failure (for y)” in the Bernoulli process and the corresponding Binomials.

For the test based on p_r , even though Figure 3 suggested that four of the five distributions of p-values, in their entirety, do not differ significantly from a uniform distribution, it is rather salient that the Type-I error rates are nonetheless inflated for two of the three cases. Again, the actual Type-I error rate appears to vary quite substantially, depending on the underlying binomial probabilities. This strongly suggests that the results of Birnbaum’s tests do not depend just on whether data are iid or not, they depend on the choice probabilities themselves. They also depend on the way that binary choices are coded. This does not strike us as a desirable property of a meaningful test for iid sampling.

The analyses in this section were based on simulating iid data from given collections of binary choice probabilities. For real data, where we do not know the underlying binary choice probabilities that hold under the null hypothesis, we cannot know the Type-I error rates of Birnbaum’s tests. All in all, in contrast to the Smith and Batchelder (2008) test, which rests on analytically derived expected values and standard errors, and which the RDDS data pass with flying colors, Birnbaum’s (2012) two tests of iid sampling currently lack a solid and coherent mathematical foundation.

⁶For example, we replace $P_{AB} = 0.05$ by $P_{AB} = 1 - 0.05 = .95$, $P_{AC} = .01$ by $P_{AC} = 1 - 0.01 = .99$, etc. This “mirror” amounts to a relabeling of pairwise choices. In Table 7, the analogue is to switch 1’s and 0’s in the table. This choice of coding is arbitrary and should not influence the behavior of any meaningful statistical test.

5 Do the findings of Birnbaum (2012) replicate within participant?

We now consider whether small values of p_ν and/or p_r , if they were to serve as a proxy for iid violations, at least have a coherent substantive interpretation. Birnbaum (2012) analyzed only a fraction of RDDS’ data. As we explained in the introduction and illustrated in Table 1, the experiment of Regenwetter et al. (2011) contained three different stimulus sets, labeled Cash I, Cash II, and Noncash, as well as various Distractor items many of which resembled either the Cash or the Noncash items. All stimuli and distractors were mixed with each other within the same experiment (see Table 1). When thinking about iid sampling, we may be concerned about memory effects: The decision maker might recognize previously seen stimuli, recall the choices previously made, and attempt to either be consistent or seek variety. Hence, choices might be interdependent and/or choice probabilities might drift over time because memory of earlier choices might interfere with new choices.

While all Cash I and Cash II stimuli were two-outcome gambles for very similar cash amounts of money, the Noncash gambles involved prizes such as free movie rentals, free coffee, free books, etc. The purpose of the Distractor items and of the intermixing of different stimulus sets was to reduce or eliminate the role of memory in repeated choices from the same stimulus set. Yet, if nonetheless memory affected the choice probabilities or created dependencies, this effect should be most pronounced in the Noncash condition because these stimuli were arguably much more recognizable. Second, the Cash I and Cash II stimuli looked so similar to each other that only a person with knowledge of the experimental design can tell which stimuli belong to which

Figure 3: Illustrative analysis of the sampling distribution of p_r approximated through 3,000 simulated iid data sets using the maximum likelihood binomial parameters of three participants from Regenwetter et al. (2011) Cash I, and a hypothetical participant. The underlying binomial probabilities are given above the histograms. The expected frequency in each bin under the uniform null is given by the horizontal line. The Kolmogorov-Smirnov statistic is significant in one case, i.e., the distribution differs significantly from a uniform on $[0, 1]$ for the iid samples from the best fitting collection of binomials of Participant 10.

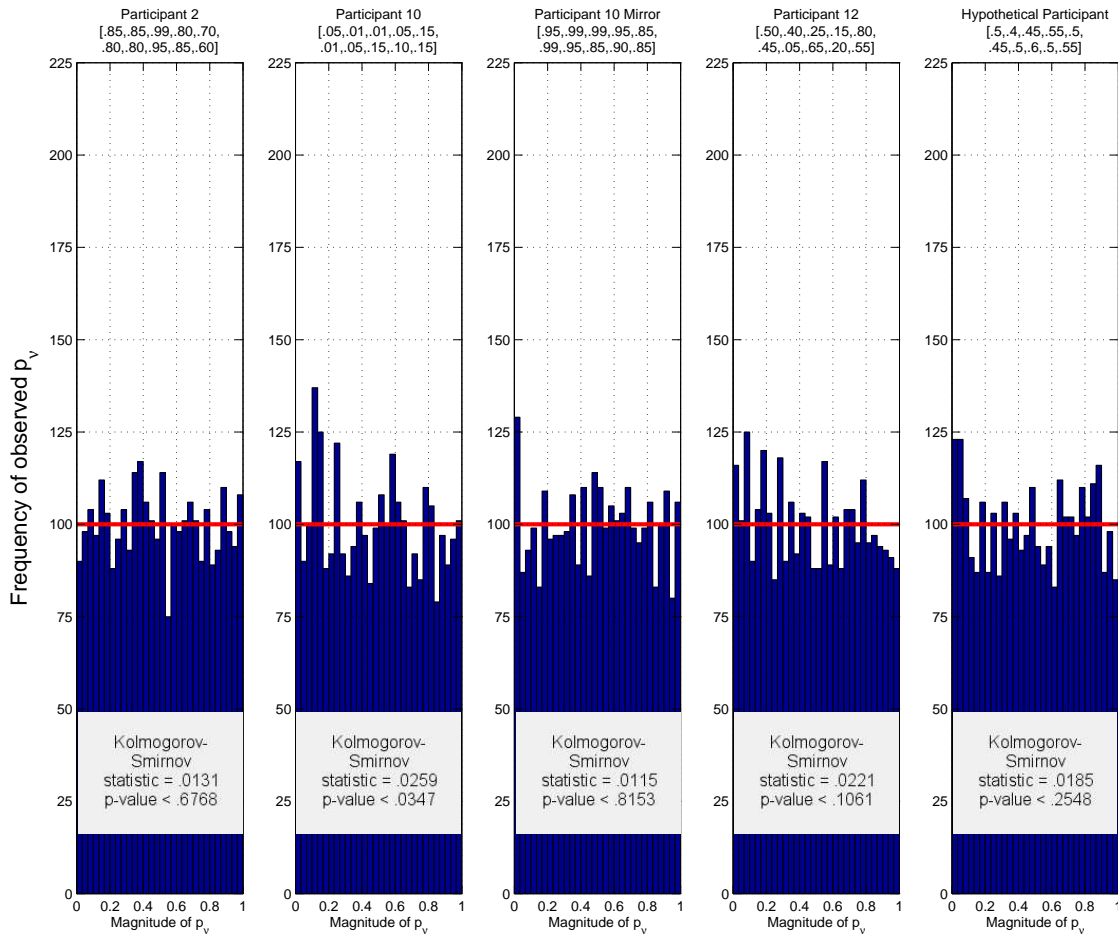


Table 4: Nominal versus actual Type-I error rates for Birnbaum’s (2012) tests of iid.

Participant #	Nominal α -level for p_v					Nominal α -level for p_r				
	1%	4%	5%	6%	10%	1%	4%	5%	6%	10%
Participant 2	0.9%	3.4%	4.8%	5.6%	9.5%	1.1%	3.7%	4.3%	5.6%	9.7%
Participant 12	0.7%	3.1%	4.3%	5.1%	8.9%	1.1%	4.4%	5.4%	6.5%	11.4%
Hypothetical Participant	0.5%	3.3%	4.4%	5.4%	8.9%	1.4%	5.2%	6.4%	7.4%	11.7%
Participant 10	1.4%	4.6%	5.6%	6.7%	10.5%	1.1%	4.3%	5.0%	6.3%	10.2%
Participant 10 (mirror)	0.7%	3.1%	4.2%	5.2%	9.2%	1.4%	4.7%	5.7%	6.7%	10.2%

stimulus set.⁷ Since the data collections for Cash I, Cash II, and Noncash gambles were fully interwoven with each other, any substantive conclusions about non-iid responses, if valid in one stimulus set, should replicate in another. We do not know how to make conceptual sense of concluding, say, that a person's choices on Trials 1, 5, 9, 13, . . . , 797 were iid, while choices on Trials 4, 8, 12, 16, . . . , 800 were not iid.

Because of these considerations, we checked whether Birnbaum's (2012) conclusions about non-iid sampling are consistent across stimulus sets, and whether the alleged violations are indeed more pronounced in the Non-cash condition. Hence, we applied Birnbaum's (2012) R code not only on the Cash I gambles, as was done in Birnbaum (2012), but also on the Cash II and Noncash gambles. The results of our analysis of these three sets are given in Table 5 under the heading⁸ "Full Data Sets". The R code computes simulated random permutations of the data: We used 10,000 such pseudo-random permutation iterations per analysis. Values of p_ν and p_r smaller than 0.05 are marked in **bold**. Values that would round to 0.05 are given to three significant digits. Cases where the linear order model is rejected are in parentheses. Cases that were undefined due to division by zero are marked with a —. We confirm Birnbaum's finding that, in Cash I, four values of p_ν are smaller than 0.05.⁹ In addition, there are six such values in Cash II and four in Noncash. For p_r , Birnbaum (2012) reports six values smaller than 0.05 in Cash I, and we find five in each stimulus set. However, it is important to note that not a single individual generated small values of p_ν or p_r for all three sets.

Following the train of thought in Birnbaum (2012), each of these "significant" values might suggest, by itself, that the participant might violate iid sampling. However, the Noncash case has relatively few "violations", even though this should be the prime source of potential memory effects that could cause interdependencies and/or make the probabilities change in some systematic way. This lack of replicability is consistent with our concern in the previous section, namely that small values of p_ν and/or p_r may be difficult to interpret. On the other hand, if we give MB the benefit of the doubt and we presume that the tests really do detect violations of iid, then the lack of replications could alternatively be interpreted as indicating very small effect sizes. In that case, the iid

assumption might be violated, but only so slightly that it does not turn up significant very often. In that case, the question would arise how the analysis of RDDS would really be affected by an iid assumption that is only an approximation, but a close approximation, of the data.

We have shown that the RDDS data pass Smith and Batchelder's (2008) test of iid sampling with top marks. We have shown that Birnbaum's (2012) statistics p_ν and p_r may not be p-values, that their Type-I error rates are unknown, and that these statistics appear to depend on more than just iid sampling alone, they even appear to depend on how data are coded. We have now established that no single participant, out of 18 participants, has consistently small values of p_ν and/or p_r across all three stimulus sets, either. Combining these observations, we see no merit in interpreting values of $p_\nu < 0.05$ and/or $p_r < 0.05$ as pin-pointing individual participants who violate iid sampling. Likewise, we see no justification for the much broader blanket statement that "the data of Regenwetter, et al. (2011) do not satisfy the iid assumptions required by their method of analysis" (Birnbaum, 2012, p. 99).

6 Could RDDS's findings be an artifact of warm-up effects?

The discussion around Birnbaum's (2012) Table 2 suggests that decision makers might change their choice probability after the first few trials. We consider whether the great model fit in RDDS could be an accidental artifact of drifting choice probabilities in the first few trials due to some sort of warm-up period during which the decision makers familiarized themselves with the experiment.

Since Birnbaum (2012) stressed that violations of iid sampling may have led to false acceptance of the linear order model in Regenwetter et al. (2011), we consider whether, by dropping the first four of twenty trials for all gamble pairs, we are able to reject the linear order model on more participants. Starting from Birnbaum's (2012) Table 2, we dropped the first four trials for each gamble pair, every stimulus set, and every participant. Note that, to decide how many trials to drop, we inspected the data of only the one participant and one stimulus set discussed in Birnbaum's (2012) Table 2.¹⁰

Table 6 shows the results of two analyses of the data in Regenwetter et al. (2011) using a newer software for

⁷See the gamble pairs on Trials 1, 5, 9, etc. versus the gamble pairs on Trials 4, 8, 12, etc. in Table 1, keeping in mind that numerical probabilities were not provided.

⁸Table 5 shows no major changes with the first 4 trials dropped. We provide the rationale for this analysis later.

⁹Birnbaum initially reported a larger number of violations. After the Regenwetter lab had difficulties replicating his results, he corrected his data extraction program, and reported (Birnbaum, 2012) values of p_ν and p_r for Cash I that members of the Regenwetter lab (Y. Cha and M. Choi) were able to confirm independently.

¹⁰This is important because looking at data to generate a hypothesis before testing that hypothesis is problematic in that the data inspection uses up some unknown number of degrees of freedom in the data and could bias the outcome of a hypothesis test. Since we did not extract any information from any other data sets, we also did not lose degrees of freedom in any other data sets, nor did we bias our alternative hypothesis.

Table 5: Summary of p_ν and p_r values, rounded to two significant digits, according to the method of Birnbaum (2012) for Cash I, Cash II, and Noncash of Regenwetter et al. (2011), for both the full data sets, as well as the reduced data sets where the first four trials for each gamble pair were dropped.

Participant #		Full Data Sets			First 4 Trials removed		
		Cash I	Cash II	Noncash	Cash I	Cash II	Noncash
1	p_ν	0.26	0.57	0.20	0.30	0.68	0.32
	p_r	0.09	0.32	0.053	0.21	0.04	0.06
2	p_ν	0	0	0.30	0.47	1	0.38
	p_r	0	0.21	0.30	0.08	0.15	0.02
3	p_ν	1	0.83	0.53	1	0.68	0.67
	p_r	0.51	0.89	0.03	0.12	0.23	0.73
4	p_ν	0.08	0.47	1	(0.58)	0.29	1
	p_r	0.98	0.11	0.80	(0.58)	0.81	0.50
5	p_ν	0.01	0	0.08	0.02	0	0.23
	p_r	0.11	0.85	0.12	0.93	0.77	0.051
6	p_ν	0.11	0	1	0.49	0	1
	p_r	0.050	0	0.12	0.57	0	0.39
7	p_ν	0.23	0	1	0.92	0.58	1
	p_r	0.01	0	0.55	0.35	0.74	0.30
8	p_ν	1	0.18	0	1	0.20	0.57
	p_r	0.90	0.72	0	0.68	0.75	0.74
9	p_ν	0.78	0.27	0.01	0.92	0.26	0.22
	p_r	0.12	0.046	0.01	0.38	0.13	0.65
10	p_ν	0.045	0.15	0.27	0.13	0.26	1
	p_r	0.54	0.68	0.90	0.38	1.00	0.49
11	p_ν	0.47	0.38	1	0.55	0.48	1
	p_r	0.87	0.48	0.46	0.58	0.21	0.20
12	p_ν	0.79	0.06	0.02	0.97	0.07	1
	p_r	0.24	0.88	0	0.22	0.51	0.01
13	p_ν	0.18	0.65	0.48	0.49	0.89	0.70
	p_r	0.01	0.08	0.04	0.16	0.76	0.02
14	p_ν	1	1	1	1	1	1
	p_r	0.39	–	0.60	0.49	–	0.21
15	p_ν	0	0	1	0.01	0.04	1
	p_r	0	0	0.35	0.03	0	0.20
16	p_ν	(1)	(0)	0.23	(1)	(0)	0.06
	p_r	(0.02)	(0.02)	0.36	(0.48)	(0.09)	0.41
17	p_ν	0.75	0.70	0.11	0.55	0.66	0.14
	p_r	0.33	0.34	0.60	0.10	0.88	0.64
18	p_ν	0.95	0.47	0.01	0.98	0.22	1
	p_r	0.31	0.31	0.07	0.24	0.09	0.44

Table 6: Analysis of the linear order model on the full data sets and on reduced data sets where the first four trials for each gamble pair are dropped. A checkmark ✓ indicates perfect fit.

Res.#	Full Data Sets			First 4 Trials removed		
	Cash I $G^2(p)$	Cash II $G^2(p)$	Noncash $G^2(p)$	Cash I $G^2(p)$	Cash II $G^2(p)$	Noncash $G^2(p)$
1	✓	2.01(0.28)	✓	0.11(0.35)	2.82(0.20)	✓
2	✓	✓	2.85(0.33)	[✓]	✓	1.41(0.56)
3	✓	✓	1.41(0.47)	✓	✓	1.41(0.47)
4	3.76(0.14)	0.09(0.76)	✓	8.91(0.01)	0.50(0.69)	✓
5	✓	✓	✓	✓	✓	✓
6	0.35(0.64)	0.08(0.38)	✓	1.02(0.57)	0.45(0.25)	✓
7	✓	✓	3.63(0.18)	✓	✓	3.64(0.18)
8	✓	✓	✓	✓	✓	✓
9	✓	✓	✓	✓	✓	✓
10	✓	0.37(0.27)	✓	✓	0.37(0.27)	✓
11	✓	1.41(0.55)	✓	✓	1.41(0.55)	✓
12	✓	✓	✓	✓	✓	✓
13	✓	✓	✓	0.10(0.39)	✓	✓
14	✓	✓	0.37(0.89)	✓	✓	0.37(0.90)
15	✓	✓	✓	✓	✓	✓
16	16.46(< 0.01)	9.51(0.01)	1.41(0.64)	19.43(< 0.01)	16.68(< 0.01)	✓
17	1.50(0.21)	✓	✓	1.76(0.1)	✓	✓
18	✓	0.33(0.45)	0.37(0.74)	✓	0.39(0.26)	✓

order-constrained inference.¹¹ A checkmark ✓ indicates perfect fit, where the choice proportions fully satisfy the triangle inequalities, hence the model cannot be rejected no matter how small the significance level α . For all cases with choice proportions outside the linear order model, we provide the test statistic G^2 followed by its p -value. G^2 values cannot be compared across cells due to order-constrained inference. Significant violations of the linear order model are marked in **bold**. One analysis [marked in brackets] involved prior inspection of the data. As we drop the first four trials for all stimuli and participants the linear order model fits the data again very well. One person, Participant 16, violates Cash I and Cash II significantly in the full data sets. This person also violates the model in the reduced data. As we move from the full to the reduced data, one nonsignificant violation becomes significant (Participant 4, Cash I), two nonsignificant violations become perfect fits and two perfect fits become

nonsignificant violations, giving a nearly identical overall picture of goodness-of-fit. This pattern of results demonstrates clearly that the excellent fit of the model in RDDS was not an artifact of a potential 4-trial-per-gamble-pair warm-up as Birnbaum’s (2012) discussion of his Figure 2 seems to suggest.

7 What do Birnbaum’s (2012) hypothetical data tell us about “true-and-error” models?

Linear orders are a type of transitive preference.¹² RDDS tested the linear order model as a proxy for testing transitivity of preferences when preferences are allowed to vary between and within persons. Birnbaum (2012) provided three tables of hypothetical data to suggest that one can construct thought experiments in which the approach of RDDS will classify all three data sets as transitive when Birnbaum generated some of the hypothetical

¹¹The new software implemented an improved algorithm for order-constrained inference with higher speed and precision. See <http://labs.psychology.illinois.edu/labs/DecisionMakingLab/qtest/>. As a consequence, the Full Data analysis slightly differs numerically from the results table in Regenwetter et al. (2011).

¹²Transitivity states that if A is preferred to B and B is preferred to C , then A is preferred to C .

data by simulating certain intransitive decision makers. Birnbaum (2012) suggested that “true-and-error” models overcome this challenge. We will now explain briefly how a “true-and-error” model works and then prove that such models do not overcome the stated challenge.

Consider once again Table 1 with the first 36 trials in the RDDS experiment. The basic unit of analysis in RDDS is the binary response on one trial. In contrast, the basic unit of analysis and the basic theoretical primitive in “true-and-error” models is that of a “response pattern”. Consider the Cash II gamble set. Because Cash II involved five distinct lotteries and all possible pairs of these five gambles, there are 10 distinct pairs of gambles in Cash II, each of which was presented 20 times. Each of Trials 4, 8, 12, 16, 20, 24, 28, and 32 is the “first replicate” of a gamble pair in Cash II, whereas Trial 36 is the “second replicate” of the lottery pair used previously in Trial 4. In a “true-and-error” model, the pattern of responses in Trials 4, 8, 12, 16, 20, 24, 28, 32 (and two more later trials), namely RRRLRLRR . . . , form one observation, namely the observed choice pattern for the *first replicate* (see the underlined responses in the last column of Table 1). The second replicate overlaps with the first in time in that Trial 36 is already part of the observed pattern for the second replicate. We call *blocking assumption* the assumption that pairwise choices in Trials 4, 8, 12, 16, 20, 24, 28, 32, and two more later trials can be blocked together to form a single observation RRRLRLRR . . . of one pattern. According to the blocking assumption, the pairwise choice in Trial 36 is not interchangeable with the choice in Trial 4, because Trial 36 is part of the second “replicate”. In Table 1 the respondent happens to have chosen R again as in Trial 4, but if this observed choice were L, the blocking assumption would disallow exchanging the observations in Trials 4 and 36.

In the analysis of RDDS, the 200 trials that make up the data for a given stimulus set (say, Cash II) are treated as 20 observations for each of 10 binomials (this gives the usual 20 observations per binomial that is recommended as a rule of thumb for using asymptotic statistics). In a “true and error model” the same 200 binary choices form 20 observations (20 observed patterns from 20 replicates) of one single multinomial with $2^{10} = 1,024$ cells, i.e., with 1,023 degrees of freedom. This is because there are 1,024 distinct possible patterns of 10 binary choices. For a multinomial with over 1,000 degrees of freedom, 20 observations can be labeled *extremely sparse* data that are nowhere close to warranting the use of asymptotic distributions for test statistics.

We now introduce what we will label the *standard true-and-error* model [henceforth STE] for such a multinomial. The STE model spells out how a binary response pattern, the primitive unit of observation for the model, is

related to individual binary responses on individual trials. In the STE model the decision maker has a single, deterministic, fixed, “true” preference pattern throughout the experiment, and the reason that he or she does not choose consistently with that preference pattern is because she or he makes errors (trembles) with some probability. According to Birnbaum (2004, pp. 59, 61), Birnbaum (2007, p. 163), Birnbaum and Bahra (2007, p. 1024), Birnbaum and Gutierrez (2007, p. 100), Birnbaum (2008a, p. 483), Birnbaum (2008b, p. 315), Birnbaum and Lacroix (2008, p. 125), Birnbaum and Schmidt (2008, p. 82), Birnbaum (2010, p. 369), as well as Birnbaum and Schmidt (2010, p. 604), errors occur independently of each other, with the error probability of each gamble pair being constant over time. Denoting the decision maker’s true preference pattern as B and letting B_s denote the entry in B for gamble pair s , i.e., the person’s true preference for gamble pair s , and denoting by p_s the probability of making an error when responding to gamble pair s , the probability that this decision maker gives response X_s at time t does not depend on t and it equals

$$\begin{cases} (1 - p_s) & \text{if } X_s = B_s & \text{(no error),} \\ p_s & \text{if } X_s \neq B_s & \text{(error).} \end{cases} \quad (5)$$

For example, suppose that there are 10 pairs of gambles. Following the equations in the referenced papers, the probability of a binary pattern in which a given decision maker chooses correctly on Gamble Pairs 6, 7, and 10 and chooses incorrectly on Gamble Pairs 1, 2, 3, 4, 5, 8, 9, according to the STE model, is

$$p_1 p_2 p_3 p_4 p_5 (1 - p_6) (1 - p_7) p_8 p_9 (1 - p_{10}). \quad (6)$$

There are 1,024 such formulae to provide the probabilities of all 1,024 different choice patterns that are possible in the STE model. In Table 1 we used labels L and R to refer to left-hand-side and right-hand-side gambles. Instead, we could also label one gamble as Gamble 0 and the other gamble as Gamble 1 (and in the process drop the distinction of the side on which a given gamble was presented visually), and then record, for each trial a zero or a one to code which gamble was chosen. If we fix the sequence by which we consider the gamble pairs in such a binary coding¹³, we can represent both the “true” preference and each of the observed preference patterns as 10-digit strings of zeros and ones.

Say, if the decision maker’s true preference is binary pattern 0000000000 then, by Formula 6, the observed pattern 1111100110 has probability $p_1 p_2 p_3 p_4 p_5 (1 - p_6) (1 - p_7) p_8 p_9 (1 - p_{10})$. The STE model also spells

¹³This means we disregard the sequence of trial presentations within a replicate. Relabeling the zeros as ones and the ones as zeros yields what we called a “mirror” in Figures 2 and 3, and in Tables 3 and 4.

out what happens if each question (gamble pair) is presented on two replicates. If the decision maker makes 10 choices on 10 distinct gamble pairs in one replicate, and another set of 10 choices on the same 10 gamble pairs in a second replicate, the probability that s/he makes 10 errors on the first replicate and makes no errors on the second replicate, according to STE is,

$$\underbrace{\prod_{j=1}^{10} p_j}_{\text{errors on items 1-10 first replicate}} \times \underbrace{\prod_{i=1}^{10} (1 - p_i)}_{\text{correct choices on items 1-10 second replicate}} \quad (7)$$

We now move to the hypothetical data in Birnbaum (2012). Birnbaum (2012) argued that the linear order model analysis of RDDS may fail to distinguish transitive from intransitive cases when iid is violated. For convenience, we reproduce the hypothetical data in question in Table 7. The columns list hypothetical gamble pairs, the rows list the hypothetical replicates (repetitions). In the interior of the table an entry “1” indicates the choice of the first gamble in the gamble pair, and a “0” indicates a choice of the second gamble in a gamble pair.

Our table also gives the results of the iid test of Smith and Batchelder (2008). For the top data set, there are 10 separate tests, of which one turns out significant. Birnbaum (2012) states that these data were iid generated, hence we have one Type I error by Smith and Batchelder’s (2008) test in ten tests. (Recall that our analysis in Table 2 yielded significant results in 3% of cases in RDDS.) In the data in the center of Table 7, all columns are the same, hence we only need to apply Smith and Batchelder’s test once. Indeed, it is significant, consistent with a violation of iid sampling. In the third data set, which involves only two types of column collections, the corresponding two tests of Smith and Batchelder (2008) turn out significant both times, consistent with a violation of iid sampling. Birnbaum’s (2012) latter two hypothetical data sets are quite different from RDDS’ real data.

Birnbaum (2012) stated that the RDDS analysis, by counting pairwise choice proportions only, treat the three tables the same and classify all three cases as transitive, whereas true-and-error models would distinguish the first, transitive, case from the other two, intransitive, cases. We first show that standard true-and-error models (as used in Birnbaum 2004, pp. 59, 61; Birnbaum, 2007, p. 163; Birnbaum & Bahra, 2007, p. 1024; Birnbaum & Gutierrez, 2007, p. 100; Birnbaum, 2008a, p. 483; Birnbaum, 2008b, p. 315; Birnbaum and Lacroix, 2008, p. 125; Birnbaum & Schmidt, 2008, p. 82; Birnbaum, 2010, p. 369; Birnbaum & Schmidt, 2010, p. 604), will also treat all three tables the same and will likewise classify all three data tables as transitive. For each of Tables A.4-A.6 in Birnbaum (2012), we can expand the formulations of the

STE model in Formulae 5 and 7 to a situation with 10 replicates. The probability of the observations in each table is given by

$$\underbrace{\prod_{i=1}^{10} [B_i(1 - p_i) + (1 - B_i)p_i]^6}_{\text{each column of } M \text{ contains 6 ones}} \times \underbrace{\prod_{j=1}^{10} [B_j p_j + (1 - B_j)(1 - p_j)]^4}_{\text{each column of } M \text{ contains 4 zeros}} \quad (8)$$

For example, if the true preference is $B = 1111111111$, the probability of the data in each table is given by

$$\underbrace{\prod_{i=1}^{10} [(1 - p_i)]^6}_{\text{each column of } M \text{ contains 6 correct choices}} \times \underbrace{\prod_{j=1}^{10} [p_j]^4}_{\text{each column of } M \text{ contains 4 errors}} \quad (9)$$

If, as is usually the case, we restrict the error probabilities to be $p_s < 0.5, \forall s$, then the maximum likelihood estimate will yield the “true” preference pattern 1111111111 and estimated error probabilities of 0.4 for every error term, in every one of the three Tables A.4-A.6 of Birnbaum (2012), as summarized in our Table 7. The STE model analysis cannot distinguish the three hypothetical data tables. Birnbaum (2012) designed these three hypothetical data sets to illustrate alleged weaknesses of the analysis of RDDS and strengths of the “true-and-error” approach. Yet, like the analysis used in RDDS, the STE model analysis cannot differentiate between the data in the three tables either, and it will also classify all three cases as transitive.

In the discussion of Tables A.4-A.6, Birnbaum (2012, p. 106) states that Birnbaum and Bahra (2007) “found that some people had 20 responses out of 20 choice problems exactly the opposite between two blocks of trials. Such extreme cases of perfect reversal mean that iid is not tenable because they are so improbable given the assumption of iid.” If true, then this would mean that Birnbaum and Bahra’s (2007) analysis, which used a STE model with iid errors (Birnbaum and Bahra, 2007, p. 1024), is itself “not tenable” on those data, in Birnbaum’s words.

We have shown that the STE model, the model used in 10 or more published papers, cannot distinguish between the three data tables any better than the analysis in RDDS. We have also shown in Table 7 that Smith and Batchelder’s (2008) test successfully picks up the iid violations that Birnbaum built into two of the tables. Recall that this is the test that the RDDS data passed with flying colors.

Table 7: Hypothetical data in Birnbaum’s (2012) Tables A.4 (top), A.5. (center), and A.6. (bottom). A “1” indicates choice of the first option in pair, a “0” indicates choice of the second option. For each column of data, we also provide the result of a test for iid sampling of Smith and Batchelder (2008, p.727) using confidence intervals of point estimates ± 2 standard errors (or ± 1.96 standard errors. The results of using 1.96 or 2 standard errors matched throughout.).

Repetition	AB	AC	AD	AE	BC	BD	BE	CD	CE	DE
1	1	0	1	1	1	1	0	1	1	1
2	0	0	0	0	1	0	1	0	0	1
3	0	1	1	1	1	1	1	0	1	1
4	1	1	0	0	1	1	1	0	1	0
5	0	0	1	1	0	1	0	1	0	0
6	1	1	1	1	0	1	0	0	1	0
7	1	1	1	0	0	1	1	1	1	1
8	1	1	0	1	1	0	1	1	0	1
9	1	0	0	0	1	0	0	1	1	0
10	0	1	1	1	0	0	1	1	0	1
Reject iid?	no	no	no	yes	no	no	no	no	no	no

Repetition	AB	AC	AD	AE	BC	BD	BE	CD	CE	DE
1	1	1	1	1	1	1	1	1	1	1
2	1	1	1	1	1	1	1	1	1	1
3	1	1	1	1	1	1	1	1	1	1
4	1	1	1	1	1	1	1	1	1	1
5	1	1	1	1	1	1	1	1	1	1
6	1	1	1	1	1	1	1	1	1	1
7	0	0	0	0	0	0	0	0	0	0
8	0	0	0	0	0	0	0	0	0	0
9	0	0	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0	0	0
Reject iid?	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Repetition	AB	AC	AD	AE	BC	BD	BE	CD	CE	DE
1	0	0	1	1	0	0	1	0	0	0
2	0	0	1	1	0	0	1	0	0	0
3	0	0	1	1	0	0	1	0	0	0
4	0	0	1	1	0	0	1	0	0	0
5	1	1	1	1	1	1	1	1	1	1
6	1	1	1	1	1	1	1	1	1	1
7	1	1	0	0	1	1	0	1	1	1
8	1	1	0	0	1	1	0	1	1	1
9	1	1	0	0	1	1	0	1	1	1
10	1	1	0	0	1	1	0	1	1	1
Reject iid?	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Next, consider a modification of the STE model test in which there is still a single true preference, but where the iid assumption for errors is dropped. Then the data in Birnbaum’s (2012) Table A.5 (center of our Table 7)

can originate from a person with transitive true preference pattern 1111111111 who makes no errors for the first 60 binary choice trials (i.e., the first 6 lines in the table) and who makes errors for all remaining trials of the study. However, this person can instead have fixed intransitive true preference pattern 0011001000 and generate the same data because she or he makes no errors on the first 6 trials of stimuli AD, AE, BE but errors in all first 6 trials of stimuli $AB, AC, BC, BD, CD, CE, DE$, then switches to the opposite error behavior for the remaining four replicates. Similar constructions are possible for Birnbaum’s (2012) Tables A.4 and A.6 (top and bottom of our Table 7). If errors are allowed to be interdependent and if error probabilities are allowed to change over the course of the experiment, then “true-and-error” models can generate a perfect fit to *any* data, no matter what fixed “true preference” they use. In other words, “true-and-error” models without iid assumption for errors are neither identifiable nor testable. They are vacuous, even if they permit only one single and fixed “true” preference.

Finally, we consider what happens in Tables A.4-A.6 (our Table 7) if we consider “true-and-error” models in which the preferences are allowed to vary. For example, a person may have preference pattern $B1 = 1111111111$, say, 60% of the time, and preference pattern $B0 = 0000000000$ on 40% of occasions. We denote this as Hypothesis H. Or the person may have preference state $B2 = 0011001000$, say, 60% of the time, and $B3 = 1100110111$ the other 40% of the time. We denote this as Hypothesis HH.

Write X_s^t for the decision maker’s observed choice for gamble pair s at replicate t , that is, X_s^t is the entry in a given table in column s and row t . Assume for a moment, that there are no errors, i.e., $p_s = 0$, for $s \in \{AB, AC, AD, AE, BC, BD, BE, CD, CE, DE\}$. We obtain a perfect fit for the data in each of Tables A.4–A.6 in Birnbaum (2012) under Hypothesis H by assuming that the decision maker is in state $B1$ whenever he or she gives an answer $X_s^t = 1$ in the Table, and that the decision maker is in state $B0$ whenever he or she gives an answer $X_s^t = 0$ in the Table. Likewise, we obtain a perfect fit of the data in each of Tables A.4-A.6 in Birnbaum (2012) by assuming that the decision maker is in state

$$\left\{ \begin{array}{l} B2 \quad \text{if } X_s^t = 0, \text{ and} \\ \quad \quad s \in \{AB, AC, BC, BD, CD, CE, DE\}, \\ B2 \quad \text{if } X_s^t = 1, \text{ and } s \in \{AD, AE, BE\}, \\ B3 \quad \text{if } X_s^t = 1, \text{ and} \\ \quad \quad s \in \{AB, AC, BC, BD, CD, CE, DE\}, \\ B3 \quad \text{if } X_s^t = 0, \text{ and } s \in \{AD, AE, BE\}. \end{array} \right.$$

In a “true-and-error” model test where preference patterns may vary at any time, both Hypothesis H and Hypothe-

sis HH will fit the data in all three tables perfectly even when setting all error probabilities to zero. The “true-and-error” model with variable preferences is unidentifiable and can generate a perfect fit to *any* data whatsoever, such as those in Tables A.4-A.6 in Birnbaum (2012). Like the previous case, this “true-and-error” model is vacuous.

Combining the last two points, if “true preferences” can vary at any time, if the error probabilities are positive, if these error probabilities are allowed to change at any time, and if errors are allowed to be interdependent, the unidentifiability and nontestability problem is further exacerbated and multiple mutually exclusive “true-and-error” models will vacuously and simultaneously fit *any* data perfectly.

How does MB propose to render tests of “true-and-error” models non-vacuous? First, accommodating non-iid errors seems challenging.¹⁴ Second, MB uses a “blocking” assumption, not needed by RDDS, which regulates, at the researcher’s discretion, when exactly preferences are permitted to change. Under the “blocking” assumption, preferences are fixed during replicates and preferences are permitted to change from one replicate to the next. In other words, the decision maker must keep or may change their preference at arbitrarily determined time points that are selected by the scholar but not communicated to the participant. Considering Table 1, the blocking assumption for a “true-and-error” model with variable preferences from one block to the next assumes that the decision maker stays in the first true preference for the first replicate of those two gamble pairs that were not yet presented in the first 36 trials, whereas the decision maker is allowed to have already moved to a new preference state for the second replicate as of Trial 36 where we observe the second replicate of the lottery pair “36% chance of \$24.44” versus “28% chance of \$31.43”. It is the “blocking” assumption that allows Birnbaum (2012) to gather data into tables like Tables A.4-A.6 where each row is interpreted as one fixed preference state. Our example above has shown that in the absence of the “blocking” assumption both Hypotheses H and HH can simultaneously fit all the data in the three tables perfectly, even though they are mutually incompatible, hence the model becomes vacuous and uninformative. The sequence of trials in RDDS in Table 1, where the second replicate starts on Trial 36 (for some gamble pairs) before the first replicate has even been completed (for some other gamble pairs), shows how implausible it is to assume that a decision maker switches preferences between, but not within, blocks of trials that form a replicate. The decision maker has no way of knowing when she or he may use the first preference state and when she or he may use the second preference state. Similar con-

¹⁴Note that, while the analysis in RDDS only uses iid in its test statistic, the STE model has the iid assumption built into the model itself.

cerns apply also when replicates are fully separated in time and do not overlap.

8 Conclusion.

Every researcher depends on some simplifying assumptions. The state-of-the-art “order-constrained likelihood-ratio” test in RDDS is currently available only under the auxiliary assumption of iid data. Our application of Smith and Batchelder’s (2008) test suggests that RDDS’ data, indeed, satisfy that iid assumption. Birnbaum’s (2012) inference that the RDDS data violate iid rests on questionable mathematical conjectures and leads to incoherent interpretations within participants. In particular, Birnbaum’s proposed test statistics have unknown Type-I error rates that are sometimes larger, sometimes smaller than the nominal α -level, even for the same data, depending on how responses are coded.

Reducing 200 observations for a given stimulus set to a manageable set of statistics that can serve as point estimates of parameters and ultimately help test theories, requires making one assumption or another. We have shown that Birnbaum’s proposed alternative rests on its own, highly restrictive assumptions, some of which, to date, have not been tested. Not only are the errors routinely assumed to be iid, the analysis also fundamentally depends on the “blocking” assumptions according to which pairwise choices at certain time points, such as Trials 4, 8, 12, 16, 20, 24, 28, 32, and two more trials after Trial 36 in RDDS (see Table 1) form one observation, whereas another collection of trials (starting with Trial 36 in Table 1) form another observation. Using Birnbaum’s (2012) hypothetical data, we have illustrated how dropping these assumptions would make the “true-and-error” models vacuous and uninformative. A companion paper has shown that, while many classical probabilistic choice models, such as Luce’s (1959) choice model, the weak utility model (Becker et al., 1963; Block & Marschak, 1960; Luce & Suppes, 1965; Marschak, 1960), and the most heavily used “true-and-error” model in the literature (Birnbaum (2004, 2007, 2008a,b, 2010; Birnbaum & Bahra, 2007; Birnbaum & Gutierrez, 2007; Birnbaum & Lacroix, 2008; Birnbaum & Schmidt, 2008, 2010), require a person to have a single fixed preference throughout an entire experiment, the model in RDDS not only allows preferences to be probabilistic, it even has the somewhat unique property that one can average different probability distributions satisfying the model, and still satisfy the model. Not only does the model in RDDS stand out in its ability to model variability of preferences, it even allows that variability itself to be non-stationary.

We have shown that MB’s inference of iid violations in the RDDS data are premature: The RDDS data do not ap-

pear to violate iid sampling. We have also provided some documentation on Birnbaum’s own (2012) hypothetical data, suggesting the opposite of Birnbaum’s (2012) conclusion: “True-and-error” models hinge far more strongly on their assumptions than does the analysis in RDDS.

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Appendix.

Expected value and standard error of A_i in Eq. 21 of Smith and Batchelder (2008, p. 727).

Writing M for the number of repetitions, let

$$A_i = \sum_{t=1}^{M-1} A_{it}.$$

First, we first show that the expected value of A_i is $E(A_i) = 2(M - 1)\theta_i(1 - \theta_i)$ when iid holds.

$$\begin{aligned} E(A_i) &= E\left(\sum_{t=1}^{M-1} A_{it}\right) \\ &= \sum_{t=1}^{M-1} E(A_{it}) \\ &= \sum_{t=1}^{M-1} P(A_{it} = 1) \\ &= \sum_{t=1}^{M-1} 2\theta_i(1 - \theta_i) \quad \text{by iid} \\ &= 2(M - 1)\theta_i(1 - \theta_i). \end{aligned}$$

Second, we show that, when iid holds, the standard error of A_i equals

$$SE(A_i) = \sqrt{2(M - 1)\theta_i(1 - \theta_i)(1 - 2\theta_i(1 - \theta_i))}.$$

$$\begin{aligned} Var(A_i) &= Var\left(\sum_{t=1}^{M-1} A_{it}\right) \\ &= \sum_{t=1}^{M-1} Var(A_{it}) \quad \text{by independence} \\ &= \sum_{t=1}^{M-1} (E(A_{it}^2) - E^2(A_{it})) \\ &= \sum_{t=1}^{M-1} (P(A_{it}^2 = 1) - E^2(A_{it})) \\ &= \sum_{t=1}^{M-1} (P(A_{it} = 1) - E^2(A_{it})) \\ &= \sum_{t=1}^{M-1} (E(A_{it}) - E^2(A_{it})) \\ &= \sum_{t=1}^{M-1} 2\theta_i(1 - \theta_i)(1 - 2\theta_i(1 - \theta_i)) \quad \text{by iid} \\ &= 2(M - 1)\theta_i(1 - \theta_i)(1 - 2\theta_i(1 - \theta_i)). \end{aligned}$$