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Assessing the sensitivity of information distortion to four potential influences in studies of risky choice

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Abstract

The emergence of a leading alternative during the course of a decision is known to bias the evaluation of new information in a manner that favors that alternative. We report 3 studies that address the sensitivity of predecisional information distortion and its effects in hypothetical risky decisions with regard to 4 potential influences: choice domain, repeated choice, memory requirements, and intermediate progress questions. In Experiment 1 (N = 515), the magnitude of information distortion was similar in 5 choice domains (varied between participants) involving monetary gambles, song downloads, frequent-flyer miles, political decisions, or medical decisions. Information distortion mediated the relationship between our manipulation of initial preferences and participants' final choices, with the magnitude of the indirect effect being roughly similar across domains. These results replicate and extend previous findings. Additionally, distortion decreased significantly over 4 similar decision problems (within participants), but remained significant in the fourth problem. In Experiment 2 (N = 214), information distortion increased significantly when previously viewed information remained available, apparently because reiterating that information strengthened emerging preferences. In Experiment 3 (N = 223), the removal of intermediate progress questions that measure information distortion and emerging preferences did not significantly affect final choices, again replicating previous results. We conclude that predecisional information distortion is a relatively stable and robust phenomenon that deserves a prominent role in descriptive theories of choice.

Keywords: information distortion, mediation, memory, preference formation, risky choice.

1 Introduction

Decision makers frequently distort new information to favor an initial or emerging preference among choice options (Carlson, Meloy, & Russo, 2006; DeKay, Patiño-Echeverri, & Fischbeck, 2009b; Holyoak & Simon, 1999; Russo, Medvec, & Meloy, 1996; Russo, Meloy, & Medvec, 1998; Russo, Meloy, & Wilks, 2000; Simon, Krawczyk, & Holyoak, 2004; Simon, Pham, Le, & Holyoak, 2001; Simon, Snow, & Read, 2004; see Brownstein, 2003, for an early review). This tendency of incipient preferences to bias the interpretation of additional decision inputs, such as product information, event probabilities, and legal arguments, is known as *predecisional information distortion*. Evidence indicates that it is a coherence-driven phenomenon (Holyoak & Simon, 1999; Russo, Carlson, Meloy, & Yong, 2008; Simon et al., 2001; Simon, Krawczyk, et al., 2004), with the distortion of new information mediating the influence of initial preferences on final choices (DeKay, Stone, & Miller, 2011; DeKay, Stone, & Sorenson, 2012; also see Russo & Chaxel, 2010, for evidence of an indirect effect on choice). Information distortion may simplify decisions by reducing or eliminating difficult tradeoffs, leading to potential benefits in some situations. However, the biasing effect of early preferences on the evaluation of new information, coupled with the (more appropriate) effect of those evaluations on subsequent preferences and choices, can also contribute to poor decision outcomes (Levy & Hershey, 2006; Russo, Carlson, & Meloy, 2006; Simon, 2004).

The risk of poor outcomes notwithstanding, predecisional information distortion has been observed in a wide variety of domains, including consumer decisions (Bond, Carlson, Meloy, Russo, & Tanner, 2007; Carlson et al., 2006; Russo et al., 1996, 1998, 2006, 2008), professional decisions (Russo et al., 2000), employment decisions (Simon, Krawczyk, et al., 2004), scholarship decisions (Bond et al., 2007), legal decisions (Carlson & Russo, 2001; Holyoak & Simon, 1999; Hope, Memon, & McGeorge, 2004; Simon et al., 2001; Simon, Snow, et al., 2004), medical decisions (Kostopoulou, Russo, Keenan, Delaney, & Douiri, 2012; Levy & Hershey, 2006; Wallsten, 1981), personal and policy decisions involving risk (DeKay et al., 2009a, 2009b; Russo & Yong, 2011), and choices between risky monetary gambles (DeKay

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et al., 2011, 2012; Glöckner & Herbold, 2011). It has been observed among students (in numerous studies), members of the general public (DeKay et al., 2009b; Levy & Hershey, 2006; Simon, Snow, et al., 2004), professional salespeople and auditors (Russo et al., 2000), prospective jurors (Carlson & Russo, 2001), and physicians (Kostopoulou et al., 2012; Wallsten, 1981). Information distortion occurs when initial preferences are installed via experimental manipulation (e.g., Carlson et al., 2006; DeKay et al., 2011, 2012; Russo et al., 1996, 1998; Simon, Snow, et al., 2004) and when they develop naturally as part of the decision-making process (e.g., Carlson & Russo, 2001; Russo et al., 1996, 1998, 2008; Simon et al., 2001).

Although this wealth of studies clearly establishes the ubiquity of information distortion, the mix of choice domains, participant populations, and experimental procedures makes it difficult to assess the sources of variability in the magnitude of distortion. For instance, Carlson and Pearo (2004) found that graduate students and university staff members distorted information in favor of a leading alternative by an average of 0.43 points (on a 9-point scale) when choosing between two real wines, whereas Russo et al. (2000) found that professional salespeople distorted information by an average of 1.37 points (on the same 9-point scale) when choosing between two hypothetical restaurants. Using a similar design, Meloy and Russo (2004, Study 2) found significantly greater information distortion in decisions about college courses than in decisions about employees, though they did not report the size of the difference. More broadly, Russo and colleagues (e.g., Russo et al., 1996, 1998, 2000, 2006, 2008) have examined information distortion by asking participants to evaluate new information items during the course of the decision, as the items are presented, whereas Simon and colleagues (e.g., Holyoak & Simon, 1999; Simon et al., 2001; Simon, Krawczyk, et al., 2004; Simon, Snow, et al., 2004) have typically utilized pre-choice versus post-choice comparisons to examine shifts in the ratings of information items (though they have sometimes collected interim ratings as well). These two methodologies yield different metrics and statistical tests, further complicating cross-study comparisons.

In this article, we present three studies designed to assess the sensitivity of information distortion (or conversely, its stability) with regard to four potential influences. The first potential influence is choice domain. In contrast to the above literature, in which information distortion in different domains has been studied using different participant populations and experimental procedures, we vary domain while holding other factors as constant as possible. In Experiment 1, we examine information distortion in five domains of risky choice, using a common set of numerical probabilities for positive, negative, and null outcomes and a common set of ratios for the numerical magnitudes of those outcomes. One advantage of using choices between risky prospects is that it allows the quantitative features of the alternatives to be matched across domains to a degree that would be difficult to achieve by other means (e.g., by attempting to match the diagnosticity or usefulness of qualitative attributes across domains).

The second potential influence is repetition. Do people learn to avoid information distortion over a series of similar decision problems? For example, might physicians distort information less as they gain experience with common diagnostic and treatment decisions? Although several studies have presented participants with two to six decision problems in a fixed, counterbalanced, or random order, most authors have not addressed the potential effects of serial position. In two studies with a fixed problem order (Carlson & Russo, 2001, Study 1) or a partially fixed problem order (Russo et al., 2000, Study 2), position effects appear to be small or absent, and any differences could reflect the content of the problems (e.g., civil and criminal cases in Carlson & Russo) rather than their position. In a rare but welcome statistical test, Carlson et al. (2006, Study 1) found no effect of problem order for counterbalanced choices between backpacks and restaurants, though it is unclear whether they tested for serial position specifically. Participants' lack of awareness of information distortion (DeKay et al., 2011, Russo et al., 2000, 2006; Russo & Chaxel, 2010; Russo & Yong, 2011) also suggests that the bias might be maintained across multiple decisions. The question remains, however, whether it persists in a longer series of similar decision problems (e.g., four problems from the same domain), where the opportunity for insight and learning may be greater. In the current research, we address the potential effects of repetition on information distortion and final choices in Experiment 1 and on final choices in Experiment 3.

The third and fourth potential influences have been brought to our attention by colleagues, audience members at conference presentations, and reviewers of previous articles. The third issue we examine is whether the magnitude of information distortion is sensitive to the memory requirements of the task. In the dominant paradigm, developed by Russo and colleagues, participants view information about both alternatives one attribute at a time and evaluate that information before proceeding to the next attribute. In the course of considering several attributes, participants may forget or misremember the values of the two alternatives on the early attributes. In Experiment 2, we address the potential effect of forgetting by including a condition in which all previously viewed information remains readily available to participants as they evaluate new information items.

The anticipated effect of this change is unclear. In similar "on-line" judgment tasks in which impressions or preferences are updated as new information is encountered, there is often little or no relationship between memory for the original information and the final judgment (Hastie & Park, 1986). Applied to judgments of the information itself, this view suggests that whether early information is remembered well or poorly should have little effect on the distortion of later information. Simon et al.'s (2001, Study 2) finding that participants who expected to be tested on their memory (rather than make a deicsion) still distorted legal arguments to form a coherent position on the case is consistent with this account. On the other hand, it is possible that reminding participants of the information on which their emerging preference is based might reinforce or strengthen that leaning. If so, stronger preferences for the emerging favorite would be expected to increase the distortion of later information (Carlson & Russo, 2001; Meloy & Russo, 2004; Russo et al., 1998, 2000). A final possibility is that presenting the accumulated information all together might encourage alternative-based processing in addition to attribute-based processing. Carlson et al. (2006, Study 3) found that an alternative-based processing condition (presenting all information for one alternative together, with the two alternatives on separate pages) eliminated the effect of an initial leader on final choice, though their design precluded the measurement of information distortion in that condition. Our design allows for both attribute- and alternative-based processing and for the assessment of effects on both information distortion and choice.

The fourth issue we investigate is whether the effect of information distortion on choice depends on the presence or absence of intermediate progress questions that assess participants' evaluations of individual information items and their emerging preferences for one alternative or the other. It is possible that such questions alter the decision process in some important way, perhaps by leading participants to choose a preferred option sooner than they normally would. However, in two studies involving either winter coats (Carlson et al., 2006, Study 2) or beach resorts (Russo & Chaxel, 2010), the inclusion or omission of progress questions did not have a significant effect on final choice proportions. Additionally, Simon and colleagues (e.g., Holyoak & Simon, 1999; Simon, Krawczyk, et al., 2004; Simon, Snow, et al., 2004) have documented information distortion using pre-post designs that do not require the overt selection of a tentative leader. Although the issue may appear settled, we believe that the importance and persistence of the question warrants an independent conceptual replication. In Experiment 3, we assess the role of progress questions in choices between risky prospects.

2 Experiment 1: Effects of choice domain and repetition

The first study addresses the potential moderating effects of domain and repetition on information distortion and on the relationship between information distortion and final choice.

2.1 Method

2.1.1 Participants

Five hundred seventeen students at Ohio State University received course credit for participating in this study. Two participants were dropped because they provided incomplete data. The remaining 515 participants were 18 to 41 years old (M = 19.1); 47% were female, 82% were white, 6% were Asian American, 5% were African American, and 5% were Hispanic.

2.1.2 Materials and procedures

Participants in this computer-based study were randomly assigned to one of five choice domains involving monetary gambles (n = 86), song downloads (n = 94), frequent-flyer miles (n = 83), political decisions (n = 83), or medical decisions (n = 85), or to a no-choice control condition (n = 84) designed to provide baseline ratings of relevant information.[1] In each choice domain, participants read a short vignette (see Appendix) and considered pairs of hypothetical risky options in which they could gain or lose something of value. For example, participants assigned to the monetary domain imagined choosing between two urns of colored marbles in a gambling game at the state fair, those in the political domain imagined being a mayoral candidate choosing between two light-rail plans in an attempt to win votes, and those in the medical domain imagined being a physician choosing between two risky treatments for a patient with a serious blood condition.

For each domain, we created four decision problems involving option pairs AB, CD, EF, and GH, which participants considered in random order. We refer to the two options in each pair (e.g., A and B) as Options 1 and 2, respectively. Options in each pair differed on five *information items*: the amounts to be won or lost and the probabilities of winning, losing, or neither (see Table 1 for the values used in monetary gambles). These information items were presented sequentially. The following example shows one of four orders for the information items in the choice between Gambles A and B.

[1] Additional participants (n = 88, not included in the total) completed a choice-only control condition in which all information for each choice was presented simultaneously. This condition was unrelated to the assessment of information distortion and is not discussed further.

Table 1: Characteristics of gambles in the monetary domain of Experiment 1.

Gamble	Amount to win	Probability of winning	Amount to lose	Probability of losing	Probability of no change	Expected value
A	**$32**	.26	$6	**.23**	.51	$6.96
B	**$20**	.29	$8	**.12**	.59	$4.84
C	**$16**	**.49**	$4	.23	.28	$6.92
D	**$28**	**.30**	$3	.25	.45	$7.64
E	$28	**.30**	**$4**	.36	.34	$6.96
F	$24	**.44**	**$8**	.33	.23	$7.92
G	$16	.46	**$4**	**.26**	.28	$6.32
H	$20	.44	**$10**	**.14**	.42	$7.40

Note: Bold values indicate strongly diagnostic items appearing as Items 1 and 4.

Item 1 was designed to influence participants' initial preferences and strongly favored one option over the other:

> If you draw a winning marble from Urn A, you will win $32.
>
> If you draw a winning marble from Urn B, you will win $20.

Items 2 and 3 weakly favored one option over the other:

> With Urn A, there is a 26% chance of winning money.
>
> With Urn B, there is a 29% chance of winning money.
>
> If you draw a losing marble from Urn A, you will lose $6.
>
> If you draw a losing marble from Urn B, you will lose $8.

Item 4 strongly favored the option that was not favored by Item 1:

> With Urn A, there is a 23% chance of losing money.
>
> With Urn B, there is a 12% chance of losing money.

Item 5 always described the chance of neither winning nor losing:

> With Urn A, there is a 51% chance that you will not win or lose any money.
>
> With Urn B, there is a 59% chance that you will not win or lose any money.

Items 1 and 4 were strongly diagnostic, with amount and probability ratios of 1.47–2.50 (e.g., the ratio for Item 1 above is $32/$20 = 1.60), and with one item favoring each option. Items 2 and 3 were weakly diagnostic, with ratios of 1.05–1.33 (e.g., the ratio for Item 2 above is 29%/26% = 1.12), and with one item again favoring each option. Item 5 was intended to be nondiagnostic.

For each decision problem (option pair), all participants considered the same information, but in different orders. As in previous research (Carlson et al., 2006; DeKay et al., 2011, 2012), we manipulated initial preferences by randomly switching the contents of Items 1 and 4, so that Item 1 favored Option 1 for some participants (as in the above example) and favored Option 2 for other participants. Independently, we also randomly switched the contents of Items 2 and 3, to create four possible information orders. We randomized information order in this manner separately for each decision problem for each participant.[2]

After each information item, participants answered three progress questions. For monetary gambles, the first question asked, "Considering only the new information presented on this page, to what extent do you think this information favors Urn A or Urn B?" (for example), with the ends of the nine-point response scale labeled *Strongly favors Urn A* and *Strongly favors Urn B*. The second question asked, "In thinking about your eventual choice between Urns A and B, which urn do you think is leading at the moment? Keep in mind that there is more information to come." The third question asked, "How confident are you that you will eventually choose the urn that is currently leading?," with scale endpoints labeled *50% (Toss-up)* and *100% (Certain)* and with intermediate response options in 5% increments. After Item 5, the second and third questions asked about the final choice rather than the eventual choice.

[2]Due to a programming error, Item 1 for the AB option pair in the song downloads domain was weakly diagnostic rather than strongly diagnostic in two of the four possible orders. We excluded all data for this option pair/domain combination, with no effect on our substantive conclusions.

For options in the other four choice domains, the probabilities were identical to those for monetary gambles (see Table 1). The amounts that could be won or lost were scaled to create reasonable values by multiplying the Table 1 amounts by 1 for song downloads, 250 for frequent-flyer miles, 500 for votes, and 0.25 for years of life. For example, the above values of $32 and $20 in Item 1 were scaled to 16,000 and 10,000 votes in the political domain.

Participants in the no-choice control condition considered all 20 information items in each choice domain (5 items $\times$ 4 option pairs) in random order, separately for each of the five domains (100 items total). After each item, they rated the extent to which it favored one option or the other, again using a nine-point scale. Each item used different labels for the options (e.g., an item referring to Urns K1 and K2 might be followed by one referring to Urns S1 and S2), thereby preventing the emergence of preferences that could affect subsequent evaluations (Russo et al., 1998). These ratings served as a baseline for assessing information distortion in the choice domains.

2.2 Results and discussion

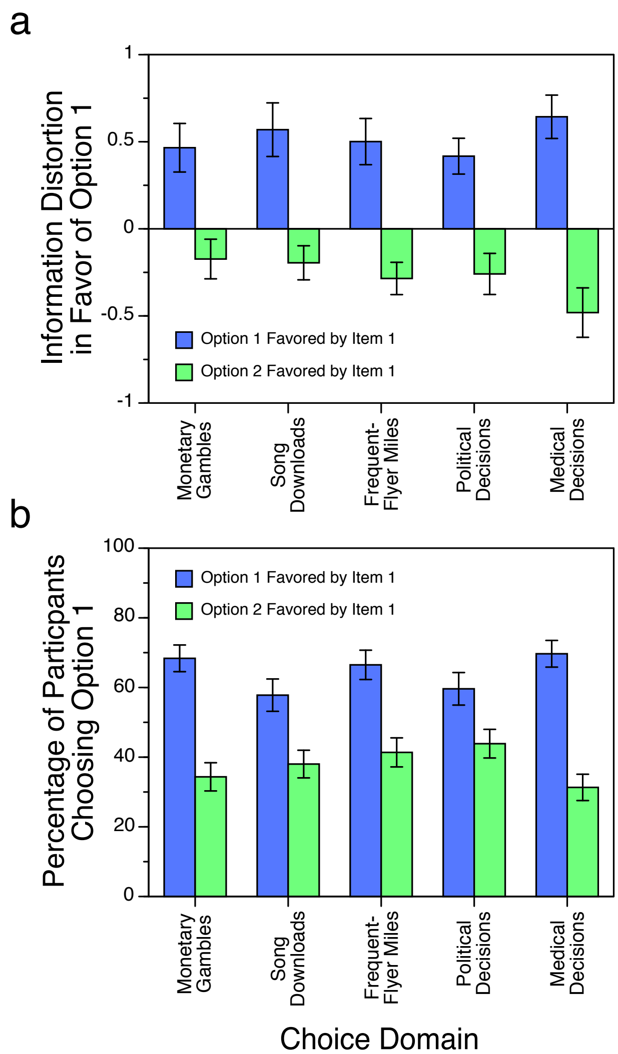
2.2.1 Manipulation check

Participants correctly identified which option was favored by Item 1 (e.g., Gamble A in the above example) 94% of the time. We excluded the other 6% of cases from subsequent analyses. For retained cases, participants selected the option favored by Item 1 as the initial leader 98% of the time, indicating that our manipulation of participants' initial preferences was highly successful.[3]

2.2.2 Information distortion

For Items 2–5 of each option pair, we calculated the difference between each item evaluation from the five choice domains and the corresponding item mean from the control condition. We coded these differences so that positive values indicated information distortion in favor of Option 1. For example, if a participant provided a rating of 4 for Item 3 and the control-condition mean for the corresponding item was 5.7, that participant's distortion score for that item would be +1.7 (since lower ratings on the response scale favored Option 1). If the participant instead gave a rating of 6 to that item, the corresponding distortion score would be -0.3. We then averaged these

Figure 1: In Experiment 1, the effects of our manipulation of participants' initial preferences on information distortion (a) and on final choices (b) were roughly similar across the five choice domains. Error bars indicate standard errors.



scores over Items 2–5 to calculate a participant's mean distortion score for each decision problem.[4]

We used repeated-measures regression to predict mean distortion in the direction of Option 1 on the basis of our manipulation of Item 1 (coded +0.5 if it favored Option 1 and -0.5 if it favored Option 2), domain (coded using orthogonal contrasts), and the interaction of Item 1 and domain. Results indicated that Items 2–5 were distorted in the direction of the option favored by Item 1, $b = 0.82$, $\chi^2(1) = 68.75$, $p < .0001$ (see Figure 1a). The effect of do-

[3] We did not exclude the 2% of cases in which participants initially favored the other option because, unlike the initial question regarding the valence of Item 1, this preference question did not have an objectively correct answer (especially as participants were reminded to anticipate additional information).

[4] We use decision problem rather than information item as the unit of analysis to avoid unnecessary complexity and because we are also interested in the effect of information distortion on choice. For item-based analyses, see DeKay et al. (2009b, 2011).

main was not significant, $\chi^2(4) = 1.65$, $p = .80$, nor was the interaction, $\chi^2(4) = 2.56$, $p = .63$, suggesting approximately equal levels of distortion across choice domains.

Overall, mean distortion in the direction of the option favored by Item 1 was equal to $b/2$, or 0.41.[5] For comparison to other research, we also computed *leader-signed distortion* for Items 2–5 using methods outlined by Russo et al. (1998, 2000, 2008). Evaluation differences, as computed above, were signed as positive if they favored the option that was leading after the previous information item and signed as negative if they favored the trailing option. For example, if a participant indicated that Option 1 was leading after Item 2 and then provided a rating of 4 for Item 3 when the control-condition mean for the corresponding item was 5.7, that participant's leader-signed distortion score would be +1.7 (since lower ratings on the response scale favored Option 1). However, if the participant had indicated that Option 2 was leading after Item 2, a rating of 4 for Item 3 would yield a leader-signed distortion score of −1.7, indicating that the distortion favored the trailing option. Results indicated that mean leader-signed distortion was positive and significant, $M = 0.46$, $\chi^2(1) = 96.24$, $p < .0001$, and that it did not vary significantly across domains, $\chi^2(4) = 1.53$, $p = .82$.

2.2.3 Final choices

Using repeated-measures logistic regression, we predicted participants' final choices on the basis of Item 1 (our manipulation of initial preference), domain, and their two-way interaction. Participants chose Option 1 more often when Item 1 favored that option than when it favored Option 2 (65% vs. 38%, respectively), $b = 1.09$, OR = 2.98, $\chi^2(1) = 73.73$, $p < .0001$ (see Figure 1b). The effect of domain was not significant, $\chi^2(4) = 2.38$, $p = .67$. The interaction between Item 1 and domain was nearly significant, $\chi^2(4) = 9.15$, $p = .057$, but the effect of Item 1 was positive and significant for each of the five domains, all ps $\leq .023$.

2.2.4 Mediation

Next, we assessed whether the effect of our Item 1 manipulation on participants' final choices was mediated by the distortion of Items 2–5.[6] We predicted participants' choices using Item 1, mean distortion in the direction of Option 1, domain, the interaction between Item 1 and domain, and the interaction between mean distortion and domain. The direct effect of Item 1 on participants' choices remained significant, $c' = 0.69$, OR = 2.00, $\chi^2(1) = 24.84$, $p < .0001$, but was smaller than when information distortion was not in the model (1.09 in the preceding section). Mean distortion in the direction of Option 1 also predicted participants' choices of Option 1, $b = 1.03$, OR = 2.80, $\chi^2(1) = 132.62$, $p < .0001$. Neither of these paths was significantly moderated by domain, $\chi^2(4) = 8.37$, $p = .079$, and $\chi^2(4) = 3.80$, $p = .43$, for the two interactions, respectively.

The magnitude of the indirect effect of Item 1 on participants' choices is equal to the effect of Item 1 on distortion ($a = 0.82$) times the effect of distortion on choices ($b = 1.03$), or $ab = 0.84$, with a bootstrapped 95% CI = 0.65–1.10, indicating significant mediation.[7] Exponentiating the indirect effect yields an odds ratio of 2.33, 95% CI = 1.92–3.92. As shown in Table 2, estimates of the indirect effects in the five domains were similar, with 95% CIs showing substantial overlap. These results are consistent with the nonsignificant moderating effects of domain on the two components of the indirect effect.

Table 2 also includes total effects, which incorporate both the direct and indirect effects of our Item 1 manipulation on final choices. Although we have no definitive explanation for the significant direct effect, c', the effect is neither surprising nor troubling. For example, it could result from primacy-related differences in attention, memory, attribute weighting, or other processes, either separately or in combination.

A potential concern about these mediation analyses is that the mediator (information distortion) is based on the difference between a participant's rating of an item in one of the choice conditions and the corresponding item mean from the control condition. As noted by DeKay et al. (2011), this difference may include not only information distortion, but also the participant's "true" undistorted evaluation of the information (to the extent that differs from the mean). For example, a participant with a particularly steep utility function for losses might evaluate the difference between losing \$6 in Gamble A and \$8 in Gamble B of our original example as being particularly favorable to Gamble A, relative to other participants. This possibility makes it difficult to interpret the relationship between participants' scores on the mediator and participants' final choices (coefficient b in Table 2) as an ef-

[5] b is the difference between distortion in the direction of Option 1 when that option was favored by Item 1 and when Option 2 was favored by Item 1 (i.e., the difference between the blue and green bars in Figure 1a, averaged over domains). For distortion in the direction of the option favored by Item 1, the signs of the green bars for Option 2 must be reversed, making them positive. The result, averaged over all bars, is $b/2$. Mean distortion in each of the five domains may be computed as $a/2$ using the values in Table 2 (b has a different meaning in that table).

[6] In this section, in Table 2, and in the mediation section for Experiment 2, we use a, b, and c' in the usual manner for unstandardized regression coefficients in mediation analyses. See Table 2 for details. Elsewhere in this article, we use b for all unstandardized regression coefficients, regardless of the predictor or the model.

[7] We computed CIs for indirect effects by drawing 50,000 bootstrapped samples (10,000 from each domain), estimating the coefficients a and b for each sample, multiplying them together, and observing the percentiles of the distribution of ab. We computed CIs for total effects from the same samples.

Table 2: Mediation results for each choice domain in Experiment 1.

Domain	a	b	c'	Indirect effect = ab (95% CI)	Total effect = $ab + c'$ (95% CI)
Monetary gambles	0.64	1.27	1.15	0.81 (0.33–1.45)	1.97 (1.22–2.91)
Song downloads	0.76	0.91	0.39	0.70 (0.31–1.25)	1.08 (0.43–1.85)
Frequent-flyer miles	0.79	0.92	0.69	0.72 (0.35–1.21)	1.41 (0.78–2.18)
Political decisions	0.68	1.19	0.16	0.80 (0.34–1.37)	0.96 (0.24–1.70)
Medical decisions	1.12	0.90	1.18	1.01 (0.57–1.68)	2.20 (1.52–3.07)
All domains	0.82	1.03	0.69	0.84 (0.65–1.10)	1.53 (1.24–1.92)

Note: In this table and in the mediation sections of the text, a is the coefficient of Item 1 (the manipulation of initial preference) in a regression for predicting the distortion of Items 2–5, b is the coefficient of information distortion in a logistic regression for predicting final choice when Item 1 is also a predictor, and c' is the coefficient of Item 1 in the same logistic regression (i.e., it is the direct effect of Item 1 on final choice). Results in the bottom row are from models that include domain and its relevant interactions. All values in the right four columns are from logistic regressions and may be exponentiated to yield odds ratios.

fect of information distortion rather than an effect of true preferences or an effect of the combination. In short, the significance of b shows that "distorted preferences" affect choice, not that the distortion component of distorted preferences affects choice. However, mediation depends not only on coefficient b but also on coefficient a, which reflects the relationship between our manipulation of Item 1 and our distortion metric. Barring a consistent failure of random assignment (across four decisions in each of five domains), individual differences in true preferences should be uncorrelated with our manipulation and hence cannot mediate the relationship between that manipulation and final choice. In other words, even if our distortion metric includes individual differences in true preferences, such preferences cannot explain the large differences in choice percentages in Figure 1b. Information distortion, on the other hand, remains a plausible mediator of those differences.

2.2.5 Effects of repeated choice

Finally, we examined whether repeated exposure to similar decision problems diminished the magnitude of information distortion and whether this effect varied by domain. Because the order of decision problems was randomly determined for each participant, a particular decision problem (e.g., the choice between Options A and B) could appear in any position in the sequence of four choices. We predicted mean distortion in the direction of Option 1 on the basis of Item 1, problem position (coded 1–4), domain, and all two- and three-way interactions. There are two results of interest. First, the two-way interaction between Item 1 and problem position was negative and significant, $b = -0.13$, $\chi^2(1) = 6.00$, $p = .014$, indicating that the effect of Item 1 (information distortion) decreased over the course of the study. Second, the three-way interaction was not significant, $\chi^2(4) = 3.45$, $p = .49$, suggesting that this decrease was roughly similar across domains. Subsequent models revealed that, while the effect of Item 1 decreased monotonically from $b = 1.01$ for the first problem to $b = 0.61$ for the fourth problem, it remained significant at each of the four problem positions, all ps < .0001.

We conducted a parallel analysis of participants' final choices using logistic regression. Neither the two-way interaction between Item 1 and problem position nor the three-way interaction involving domain was significant, $\chi^2(1) = 0.04$, $p = .84$ and $\chi^2(4) = 3.52$, $p = .47$, respectively. Thus, there is a disconnect between the effect of repetition on information distortion (a significant decease) and on final choices (no significant change). This result is puzzling, but as noted in the previous section, information distortion is not the only process that can affect participants' final choices.

We can think of two possible explanations for the decrease in distortion over repeated decisions. The first is that some participants became aware of their biased evaluation of information over the course of the experiment and took steps to decrease that bias. As in previous research (DeKay et al., 2011, Russo et al., 2000, 2006; Russo & Chaxel, 2010; Russo & Yong, 2011), we asked participants at the end of the study (a) whether they noticed themselves interpreting new information as being more in favor of the leading alternative and (b) how likely it was that they did so even if they did not notice. Because responses to these questions were highly correlated, $r(429) = 0.64$, $p < .0001$, we averaged them into a single awareness metric, which we then centered and added to the above regression for predicting information distortion. We included interactions involving awareness, but omitted domain and its interactions. There are two relevant results. First, the three-way interaction between Item 1, problem position, and awareness was not significant, $\chi^2(1) = 1.50$, $p = .22$, indicating that the decrease in distortion was not significantly greater for participants with higher self-reported awareness. Second, the original two-way interaction between Item 1 and problem position remained negative and significant, $b = -0.12$, $\chi^2(1) = 5.75$, $p = .016$, despite the addition of awareness and its interactions to the model. These results suggest that the decrease in distortion had little if anything to do with participants' recognition of the bias.

The second possible explanation concerns the specific features of our task. Recall that the diagnostic information appearing in Item 1 was always countered by diagnostic information favoring the other option in Item 4. If, over four decision problems, participants learned to anticipate the appearance of countervailing information, they may have tempered their confidence in their initial preference in later problems. On the basis of previous research (Carlson & Russo, 2001; Meloy & Russo, 2004; Russo et al., 1998, 2000), we would expect this lowered confidence to reduce the magnitude of information distortion. To assess this possibility, we combined each participant's response to the leading-option question with his or her response to the confidence question to yield a strength-of-preference variable that ranged from -50 (certain to choose Option 2) to 50 (certain to choose Option 1). We then used participants' initial strength of preference after Item 1 as the dependent variable in a regression with Item 1 (our manipulation), problem position, and their interaction as predictors. The interaction between Item 1 and problem position was not significant, $\chi^2(1) = 1.33$, $p = .25$, indicating that participants' enthusiasm for their initial preference did not decrease significantly over the course of the study. We conclude that participants' insight into the structure of our task is unlikely to have been responsible for the observed decrease in distortion.

Given that (a) information distortion remained significant in the fourth and final decision problem, (b) the decrease in distortion did not translate to a corresponding decrease in the percentage of participants choosing the option targeted by Item 1, and (c) the decrease in distortion was apparently unrelated to participants' awareness of the bias or to their insight regarding a key feature of our task, we are somewhat underwhelmed by the decrease in distortion over repeated decisions, despite the statistical significance of the trend.

2.2.6 Summary

In Experiment 1, the magnitude of information distortion was approximately equal in the five choice domains examined. Furthermore, information distortion mediated the effects of initial preferences on final choices in a similar manner across domains. Although information distortion decreased over the course of four similar decisions, it remained significant for each of those decisions. Taken together, these results indicate that information distortion is a robust phenomenon that can persist across repeated decisions.

3 Experiment 2: Sensistivity to memory requirements

The next two experiments address additional design characteristics that have the potential to influence the presence and magnitude of information distortion. Because domain appears not to have a substantial effect, both experiments are restricted to choices between risky monetary gambles. In Experiment 2, we examine whether information distortion is sensitive to the memory requirements of the preference-formation task by including a condition in which information items, once seen, remain available to participants as they view and evaluate subsequent items.

3.1 Method

3.1.1 Participants

Two hundred sixteen students at Wake Forest University received course credit for their participation. Two participants were dropped for providing incomplete data. The remaining 214 participants were 18 to 23 years old (M = 18.9); 49% were female, 82% were white, 5% were Asian American, 4% were African American, and 3% were Hispanic.

3.1.2 Procedures

Participants in this paper-and-pencil study were randomly assigned (with unequal probabilities) to a standard experimental condition (n = 86), a no-memory-required condition (n = 85), or a no-choice control condition (n = 43). All conditions involved monetary gambles similar to those in Experiment 1, but with colored tickets rather than marbles, and with larger and more varied expected values (see Table 3).

The standard condition was similar to that in Experiment 1, but information order was varied across four versions of the questionnaire rather than being randomized for each participant. Versions differed in which of the two strongly diagnostic items (1 or 4) favored Gamble 1

Table 3: Characteristics of monetary gambles in Experiments 2 and 3.

Gamble	Amount to win	Probability of winning	Amount to lose	Probability of losing	Probability of no change	Expected value
Experiment 2						
A	**$51**	.26	$24	**.23**	.51	$7.74
B	**$35**	.29	$26	**.12**	.59	$7.03
C	**$52**	**.49**	$33	.23	.28	$17.89
D	**$73**	**.30**	$31	.25	.45	$14.15
E	$63	**.28**	**$21**	.36	.36	$10.08
F	$61	**.48**	**$32**	.33	.19	$18.72
G	$46	.46	**$29**	**.26**	.28	$13.62
H	$49	.44	**$42**	**.14**	.42	$15.68
Experiment 3						
E	$63	**.30**	**$19**	.36	.34	$12.06
F	$61	**.44**	**$34**	.33	.23	$15.62

Note: Only Gambles E and F changed from Experiment 2 to Experiment 3. Bold values indicate strongly diagnostic items appearing as Items 1 and 4.

and in which of the two weakly diagnostic items (2 or 3) favored Gamble 1. Gamble pairs appeared in the same random order (AB, EF, CD, GH) in all four versions (for this reason, we do not consider the effect of repeated decisions in this experiment).

The no-memory-required condition was similar, except that all previously viewed information about the gamble pair was visible at the top of each page, just before the new information item was presented. The older information was grouped by gamble rather than by information item, but the information for each gamble was presented in the same (manipulated) order in which it had been seen. For example, a participant considering Item 4 for Gamble Pair AB might have seen the following:

> *Previous Information about Urns A and B*
>
> Urn A
>
> You will win $51 if a green ticket is drawn.
>
> There are 26 green tickets, so the chance of winning is 26%.
>
> You will lose $24 if a red ticket is drawn.
>
> Urn B
>
> You will win $35 if a green ticket is drawn.
>
> There are 29 green tickets, so the chance of winning is 29%.
>
> You will lose $26 if a red ticket is drawn.
>
> *New Information about Urns A and B*
>
> In Urn A, there are 23 red tickets, so the chance of losing is 23%.
>
> In Urn B, there are 12 red tickets, so the chance of losing is 12%.

In the first question following such items, participants evaluated “only the new information presented above” using the same response scale as in Experiment 1.

In the no-choice control condition, the 20 information items (5 items × 4 gamble pairs) appeared in either a random order or the reverse order. As in the control condition of Experiment 1, each information item referred to a unique pair of gambles.

3.2 Results and discussion

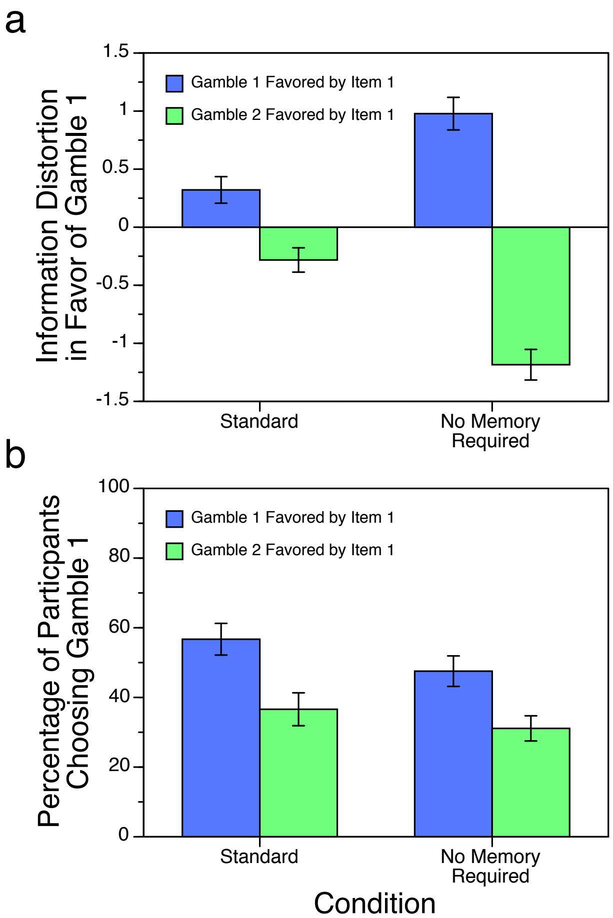
3.2.1 Manipulation check

We excluded the 3% of cases in which participants incorrectly identified the gamble favored by Item 1. For retained cases, participants selected the favored gamble as the initial leader 97% of the time, indicating that our manipulation of initial preferences was again successful.

3.2.2 Information distortion

We predicted mean information distortion in the direction of Gamble 1 on the basis of Item 1 (coded +0.5 if it favored Gamble 1 and −0.5 if it favored Gamble 2), condition (coded +0.5 for no memory required and −0.5 for standard), and the interaction between these variables. As in Experiment 1, Items 2–5 were distorted in the direction of the gamble favored by Item 1, $b = 1.38$, $\chi^2(1) = 55.54$, $p < .0001$. However, this effect was moderated by condition, $\chi^2(1) = 22.48$, $p < .0001$, with much greater

Figure 2: In Experiment 2, information distortion was significantly greater when participants were reminded of previously viewed information in the no-memory-required condition (a), but this difference between conditions did not carry over to final choices (b). Error bars indicate standard errors.



distortion in the no-memory-required condition, $b = 2.16$, $\chi^2(1) = 41.40$, $p < .0001$, than in the standard condition, $b = 0.60$, $\chi^2(1) = 9.07$, $p = .0026$ (see Figure 2a). As before, mean distortion in the direction of the gamble favored by Item 1 was equal to $b/2$, which in this study was 0.69 overall, 1.08 in the no-memory-required condition, and 0.30 in the standard condition. For comparison, the mean values for leader-signed distortion were 0.78 overall, 1.16 in the no-memory-required condition, and 0.41 in the standard condition.

One possible explanation for the large difference between conditions is that, despite our instructions to evaluate only the new information, participants in the no-memory-required condition evaluated all of the information acquired to that point. However, this explanation would predict a similar difference between conditions for Item 4 (the last diagnostic item) and for Item 5 (which added only redundant, nondiagnostic information). In fact, the moderation of distortion by condition (the interaction between our Item 1 manipulation and condition) was large and significant for Item 4, $b = 2.57$ vs. 0.62 in the two conditions, $\chi^2(1) = 15.89$, $p < .0001$, but not significant for Item 5, $b = 0.12$ vs. –0.16, $\chi^2(1) = 0.42$, $p = .52$, suggesting that participants were evaluating the new information items as instructed.

A more likely explanation for the greater distortion in the no-memory-required condition is that the repeated information, which always included the highly diagnostic Item 1, strengthened participants' preferences for the leading option, with stronger preferences leading to greater distortion of subsequent information. If this explanation is correct, the effect of Item 1 (our manipulation) on participants' strength of preference for Gamble 1 (on the -50 to 50 scale described earlier) prior to the evaluation of later items should be larger in the no-memory-required condition than in the standard condition. This was indeed the case, as evidenced by a large and significant interaction between Item 1 and condition in predicting strength of preference prior to the evaluation of Items 3–5, $b = 9.64$, $\chi^2(1) = 6.99$, $p = .0082$.[8]

3.2.3 Final choices

Final choices were also affected by the direction of Item 1, $b = 0.76$, OR = 2.13, $\chi^2(1) = 13.13$, $p = .0003$, but this effect was not significantly moderated by condition, $\chi^2(1) = 0.09$, $p = .76$ (see Figure 2b). As was the case in our analysis of repetition in Experiment 1, there is a discrepancy between the results for information distortion (significant moderation by condition) and for final choices (no significant moderation). We offer a speculative explanation in the following section.

3.2.4 Mediation

As in Experiment 1, the effect of Item 1 on final choices was mediated by the distortion of later information, with an indirect effect $ab = 1.16$, bootstrapped 95% CI = 0.82–1.62 (see Table 2 for mediation notation). Although the indirect effect was substantially larger in the no-memory-required condition, $ab = 1.42$, 95% CI = 0.90–2.15, than in the standard condition, $ab = 0.60$, 95% CI = 0.23–1.11, this difference did not result in a corresponding difference in final choice percentages (see Figure 2b). Possibly, participants in the no-memory-required condition took a fresh look at the two gambles in each pair after all

[8] The difference in strength of preference between conditions prior to the evaluation of Item 2 was smaller and not significant, $b = 4.29$, $\chi^2(1) = 1.18$, $p = .28$. This is as it should be, because the assessment of strengh of preference (confidence) after Item 1 occurred before any information was repeated in the no-memory-required condition.

information was visible, negating the effect of increased information distortion.

There is mixed support for this fresh-look explanation. The effect of Item 1 on strength of preference was significantly larger (or nearly so) in the no-memory-required condition than in the standard condition after Items 2, 3, and 4 (differences of 8.7, 11.7, and 8.5, respectively, on the −50 to 50 scale), but was smaller and not significant after the last information item (difference = 1.6), suggesting re-evaluation at the end of the decision process. The pattern for participants' choice of the leading option after each item was more gradual, however, as judged by the logistic regression coefficient for the interaction between our Item 1 manipulation and condition (2.19, 1.20, 0.53, and −0.12 after Items 2–5, respectively). This smooth decrease in the difference between conditions on this binary measure is less supportive of the fresh-look explanation than is the abrupt drop in the continuous strength-of-preference measure.

Mediation results in the two conditions also suggest a fresh look by participants in the no-memory-required condition. For the standard condition, the components of the indirect (distortion-mediated) effect of Item 1 on final choice were a = 0.60 and b = 0.99, both ps $\leq$ .0026, and the direct effect was positive but not quite significant, c' = 0.51, p = .11. For the no-memory-required condition, a = 2.16 and b = 0.66, both ps < .0001, and the direct effect was negative, though not quite significant, c' = –0.53, p = .13. The difference between conditions was significant for a (as already noted; see Figure 2a), nearly significant for b, $\chi^2(1)$ = 3.20, p = .074, and significant for c', $\chi^2(1)$ = 5.07, p = .024. In other words, the no-memory-required condition increased the magnitude of information distortion (the a path), but decreased both the influence of that distortion on choice (the b path) and the direct effect of our Item 1 manipulation on choice (the c' path). The negative sign for c' in the no-memory-required condition is of particular interest. DeKay et al. (2012) reported a negative (but not significant) direct effect of the Item 1 manipulation on the difference between participants' certainty equivalents for two gambles in a similar study of information distortion. Importantly, all of the information describing the two gambles was visible to participants when they provided their certainty equivalents, just as it was visible to participants at the end of the no-memory-required condition in this study. DeKay et al. (2012) noted that some participants may have reconsidered their preferences when they viewed complete information in the certainty-equivalent task. Participants in the no-memory-required condition of this study may have done so as well, "correcting" somewhat for the effect of their high level of information distortion when making their final choices. This correction need not imply awareness of distortion itself.

3.2.5 Summary

When we eliminated the memory requirements of the task by making previously viewed information available to participants, the distortion of new information increased substantially. Apparently, reminding participants of the basis for their current leaning reinforced or strengthened that leaning, which in turn increased the distortion of later information. Somewhat surprisingly, however, greater distortion in the no-memory-required condition did not translate to a stronger effect of our Item 1 manipulation on final choices. Participants in that condition may have re-evaluated their preferences once all of the information was available, partially negating the usual effect of information distortion on choices.

4 Experiment 3: Sensitivity to progress questions

In Experiment 3, we assess the role of intermediate progress questions by including a condition that omits those questions. Our focus is on choice rather than information distortion, but based on the mediation analyses in Experiments 1 and 2 (and in DeKay et al., 2011, 2012), we assume that differences in choice result largely (though not entirely) from the indirect effect of information distortion.

4.1 Method

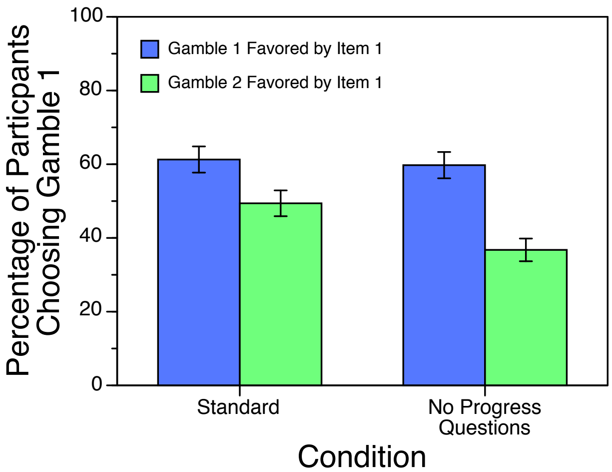
4.1.1 Participants

Two hundred twenty-three students at Ohio State University received course credit for their participation. They were 18 to 53 years old (M = 19.1); 67% were female, 84% were white, 7% were Asian American, 5% were African American, and 1% were Hispanic.

4.1.2 Procedures

Participants in this computer-based study were randomly assigned to a standard experimental condition (n = 116) or a no-progress-questions condition (n = 107). Both conditions involved monetary gambles identical to those in Experiment 2, except for slight changes to Gambles E and F intended to increase the attractiveness of Gamble E (see Table 3). In other respects, the standard condition was the same as the monetary domain of Experiment 1, with participants evaluating each information item and indicating which gamble was leading after each item. In the no-progress-questions condition, participants viewed each information item for at least five seconds before being allowed to continue to the next information item. They chose their preferred gamble after viewing all five

Figure 3: In Experiment 3, the effect of our manipulation of participants' initial preferences on final choices was not significantly affected by the removal of intermediate progress questions. Error bars indicate standard errors.



information items. In both conditions, information order was manipulated as in Experiment 1. Because we would not be able to assess information distortion in the no-progress-questions condition, we did not include a no-choice control condition in this experiment.

4.2 Results and discussion

In order to treat the two conditions similarly, we did not exclude any cases from the standard condition in which participants incorrectly identified the gamble favored by Item 1.

We predicted participants' final choices on the basis of Item 1 (coded as before), condition (coded +0.5 for no progress questions and −0.5 for standard), and the interaction between these variables. As before, final choices were affected by the direction of Item 1, $b = 0.71$, OR = 2.04, $\chi^2(1) = 20.85$, $p < .0001$ (see Figure 3). There was also an unexpected effect of condition, $b = –0.29$, OR = 0.75, $\chi^2(1) = 4.61$, $p = .032$, such that participants were slightly less likely to choose Gamble 1 in the no-progress-questions condition than in the standard condition. More important, however, condition did not significantly moderate the effect of Item 1, $\chi^2(1) = 2.23$, $p = .14$. The effect of Item 1 was significant in both conditions and, if anything, was greater in the no-progress-questions condition, $b = 0.97$, OR = 2.64, $\chi^2(1) = 17.14$, $p < .0001$, than in the standard condition, $b = 0.49$, OR = 1.63, $\chi^2(1) = 5.38$, $p = .020$. The lack of significant moderation by progress questions replicates the results of Carlson et al. (2006) and Russo and Chaxel (2010).

As in Experiment 1, we assessed whether the effect of Item 1 on participants' final choices changed from the first to the fourth decision problem considered, and whether such a change differed by condition. To that end, we added problem position (1–4) and its interactions to the above model. The two-way interaction between Item 1 and problem position was not significant, $\chi^2(1) = 0.14$, $p = .71$, nor was the three-way interaction between Item 1, problem position, and condition, $\chi^2(1) = 0.27$, $p = .60$. These results replicate the nonsignificant effects of problem position on final choices in Experiment 1.

5 General discussion

The experiments presented above address the sensitivity of information distortion with regard to four potential influences. In Experiment 1, we assessed information distortion and its effect on choice in five decision domains. Although distortion has been previously observed in a variety of domains, differences between participant populations, distortion measures, manipulations of initial preferences, and the specific attributes of the choice alternatives make comparisons across studies and conditions difficult. We eliminated these issues by conducting a large (N = 515) study in which the procedures were identical across domains and in which the attributes of the choice alternatives (probabilities and outcomes of risky prospects) were matched as closely as possible. Under these circumstances, the magnitude of information distortion did not vary significantly by domain. This result replicates and refines the well-known finding that information distortion occurs in many settings. In addition, Experiment 1 replicates previous findings by DeKay et al. (2011, 2012; also see Russo & Chaxel, 2010) that information distortion mediates the influence of early preferences on final choices and extends that work by demonstrating that the strength of the indirect effect is relatively stable across choice domains.

Experiment 1 also addressed the potential influence of repetition by assessing information distortion over four similar decision problems within each domain. Although distortion decreased monotonically with repetition, it remained significant in each of the four decisions. Additionally, the decrease in distortion did not carry over to final choices: The effect of Item 1 on final choices was not significantly moderated by repetition in Experiments 1 and 3. Finally, the decrease in distortion in Experiment 1 appeared to be unrelated to participants' self-reported awareness of the bias or to their possibly learning to anticipate later countervailing information items (which might have lowered their initial confidence and hence distortion). For these reasons, we are impressed with the persistence of information distortion over the four decisions, despite its significant but modest decline. This persistence is particularly notable because our task would seem to favor insight into the process. Although it is possible that information distortion could be extinguished in a longer series of similar decision problems, distortion is often observed in situations with which people have substantial experience (e.g., choosing between restaurants), suggesting that it is also persistent in real-world settings.

In Experiments 2 and 3, we examined whether information distortion is sensitive to two experimental design characteristics that have previously raised concerns. In Experiment 2, we found that information distortion increased if earlier information remained visible as new information was presented. Evidence suggests that the reiteration of previously viewed information strengthened participants' prior leaning, which in turn fostered greater distortion of new information. However, this increase in distortion did not translate to a corresponding increase in participants' choice of the initially preferred alternative. Some evidence suggests that participants took a fresh look at the two alternatives once complete information was available, negating the effect of increased information distortion.

That distortion is observed when information remains available throughout the task is reminiscent of the results of Brownstein, Read, and Simon's (2004) horserace studies. In those studies, participants viewed four tables of detailed information about each of four horses (a great deal of information), all on a single screen. At several points during the task, participants provided overall assessments of the four horses' chances of winning a race. The estimated chance that the (ultimately) chosen horse would win increased steadily over the course of the task, even though the same information remained visible throughout. These results provide evidence for the predecision reevaluation of alternatives, but not necessarily for the distortion of specific information items (which Brownstein et al. did not assess). For example, their procedure may have allowed a greater role for selective information search (e.g., focusing on the tentative favorite while neglecting some information about the other alternatives), whereas our serial-presentation procedure required that all information be viewed and evaluated. Thus, our findings complement rather than replicate those of Brownstein et al.

In Experiment 3, we assessed whether the effect of information distortion is sensitive to the presence of intermediate progress questions used to quantify distortion. We found that the effect of our manipulation of initial preferences (and presumably information distortion) on final choices was not significantly different when progress questions were omitted. This result replicates those of Carlson et al. (2006) and Russo and Chaxel (2010) and extends the finding to choices between risky prospects with precise numerical attributes. As noted earlier, Simon and colleagues (e.g., Holyoak & Simon, 1999; Simon, Krawczyk, et al., 2004; Simon, Snow, et al., 2004) have also documented information distortion in a different experimental paradigm that does not rely on progress questions.

5.1 Limitations

We note several limitations of our experiments. First, we examined only risky decisions, in large part because doing so made it easier to equate attributes and attribute levels across domains. We suspect that our findings would hold for riskless choices as well, though equating the diagnosticity and usefulness of information items across job candidates, consumer products, verdict options, and choice alternatives in other domains would necessitate extensive pretesting. That said, it remains possible that our method of standardization stripped away important domain differences that would moderate information distortion in real-world decisions.

Second, our tasks used precise numerical information (probabilities and outcomes), which may limit the generalizability of our results. The magnitude of leader-signed distortion for precise information in the current studies (0.46 in Experiment 1 and 0.41 in the standard condition of Experiment 2) and in others (DeKay et al., 2011, 2012) is near the lower end of the range observed for ambiguous nonnumerical information (see, for example, Russo et al., 1996, 1998, 2008). Whether the effects of potential moderators (e.g., domain, repetition, memory requirements) depend on the precision of the information being distorted is an open question, though the effect of intermediate progress questions appears to be small and nonsignificant regardless of precision.

Third, the current experiments involved only binary choices (as in nearly all previous research on information distortion), raising an additional generalizability concern. To address this issue, Miller and DeKay (2013) recently examined information distortion in choices between either two apartments or four apartments. Results indicated nearly identical levels of distortion, suggesting that previous findings are likely to extend to consumer choices among several alternatives. Miller and DeKay's studies also separated positive distortion of information about the leading alternative from negative distortion of information about the trailing alternative(s), providing evidence for distinct effects that are comingled in the current studies and in other previous research.

Finally, we used only hypothetical scenarios without real consequences. This choice was necessary in Experiment 1, given the broad range of domains we wished to examine (e.g., political and medical decisions), but we could have designed Experiments 2 and 3 differently. However, previous research has documented information distortion in real choices (e.g., Carlson & Pearo,

2004) and there is some evidence that information distortion is exacerbated when monetary incentives are offered (Meloy, Russo, & Miller, 2006).

5.2 Conclusion

The experiments presented above indicate that predecisional information distortion is a relatively stable and robust phenomenon. It is consistently strong in different domains of choice and persists across repeated similar decisions within domains. Moreover, its presence and its effect on choice cannot be ascribed to the memory requirements or measurement procedures of a prevalent experimental design. Although some of these findings replicate previous results or coorborate established beliefs grounded in the larger literature, they also extend those results to new situations involving risk. Other findings, such as those reported for repetition and the availability of previously evaluated information, are more novel. Taken together, these results further establish the ubiquity of predecisional information distortion and suggest that the process deserves a prominent role in descriptive theories of choice.

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Appendix: Vignettes for domains in Experiment 1

Monetary gambles

Choosing between Two Urns in a Gambling Game

Imagine that Ohio has decided to allow a small number of gambling games at the state fair. The state certifies that the games are legitimate and closely monitors their operation. One of the games involves two urns, each of which contains 100 marbles of different colors. To play, you choose one of the two urns and draw one marble from that urn without looking. Depending on the color of the marble that you draw, you may win money, lose money, or neither.

Each urn provides the player with different odds of winning or losing different amounts of money. There is also a chance that you will not win or lose any money. There is no cost to play the game, and money will be exchanged only if you win or lose.

Imagine that you have some extra money with you, and you have decided to play the game once. You need to choose one of the two urns from which to draw a marble.

Song downloads

Choosing between Two Spinners for Song Downloads

Imagine that you buy and download songs from iTunes on a regular basis. As part of a special "loyalty benefits" promotion, Apple has rewarded you with 15 free single-song downloads. You can use these to download songs of your choosing at any time during the next two years.

In addition to these free downloads, the company is also offering you the chance to play a game that involves two computerized spinners displayed on the iTunes webpage. The spaces on the spinners are marked to indicate whether you win more song downloads, lose some of the song downloads that you were just given, or neither. The two spinners offer different chances of winning and losing, as well as different numbers of single-song downloads that can be won or lost.

Because the company knows that their promotion will receive careful scrutiny, they have taken special precautions to ensure that the computerized spinners operate fairly. Imagine that you have already decided to participate in this game and are trying to decide which spinner to choose. You can play only once.

Frequent-flyer miles

Choosing between Two Game Cards for Frequent-Flyer Miles[9]

Imagine that you work in Columbus, and that your job requires you to fly to Las Vegas once per year. Your company uses United Airlines and recently began allowing employees to keep the frequent-flyer miles earned from their flights. Round-trip travel between Columbus and Las Vegas earns 4,000 miles. United Airlines requires 25,000 miles for a free coach-class ticket, and you now have about 8,000 miles in your account.

Because of increased competitiveness on Las Vegas routes, United Airlines is attempting to gain attention by promoting a novel frequent-flyer game. At the beginning

[9]Based on DeKay and Kim (2005).

of your next round trip, you will be offered a choice between two scratch-off game cards. Each card provides you with a chance of winning or losing airline miles, although there is some chance that you will not win or lose any miles. The chances of winning or losing are different for the two cards, as are the numbers of miles that can be won or lost. The miles that you win or lose by playing the game will not affect the miles that you earn by flying to Las Vegas and back.

Several of your colleagues have told you that the game is worth playing, and you have decided to give it a try. Now you need to choose one of the two cards. You can play only once.

Political decisions

Choosing between Two Light Rail Transit Plans

Imagine that you are running for Mayor in a mid-sized American city and that you are currently engaged in a tight race with two other candidates for your party's nomination. The city has been planning to build a light rail transit system for the last couple of years. Recently, the system has become an important political issue. While the vast majority of voters support the construction of a light rail system, there is some disagreement concerning the details. At the moment, there are two dominant plans. These plans differ primarily in terms of route alignment and the destinations served.

Personally, you have no strong preference for one plan or the other, since you believe that the plans would benefit the city about equally. Your political advisors therefore suggest that you endorse whichever plan seems more appealing to the voters (who should have some say, after all). Although it is not yet clear which plan will be more popular, your campaign's analysts have provided you with estimates that they believe accurately reflect your chances of gaining votes, losing votes, or neither, based on the plan you choose to endorse.

Your stance on this issue may or may not be a deciding factor in the campaign, but you know that you must appear decisive. You need to choose one of the two plans and publicly support whichever one you endorse.

Medical decisions

Choosing between Two Medications[10]

Imagine that you are a physician who specializes in blood diseases. One of your patients has a particular incurable blood condition that affects middle-aged women. With the standard treatment, she is expected to live between 5 and 10 years without symptoms, and then die.

There are two medications that may be used to treat the condition. However, because of a negative interaction between the two drugs, only one of them can be taken. Both drugs have been shown to extend the lives of some patients, but they sometimes shorten patients' lives, and sometimes they have no effect at all. The two drugs have different chances of extending or shortening patients' lives and can extend or shorten patients' lives by different amounts. Whether the medications make things better, make things worse, or have no effect is based on unknown biological factors and can't be predicted ahead of time.

Your patient has carefully considered her options, and has decided that taking one of the drugs is worth the risk. However, she finds it very difficult to decide which medication would be best, and looks to you for guidance. You need to choose one of the two drugs to recommend to your patient.

[10] Based on Redelmeier and Tversky (1990).