When the Good Looks Bad: An Experimental Exploration of the Repulsion Effect

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Word count: 952 (Introductions), 1025 (Discussion), 16 (Acknowledgement)

Total words: 1993/2000

References: 35/40

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This research was supported by the Center for Economic Psychology and the Center for Cognitive and Decision Sciences, University of Basel, Switzerland, and the Swiss National Science Foundation (Grant 100014_165591 to David Kellen).

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Abstract

When choosing among different options, context seems to play a vital role. For instance, adding a third option can increase the probability of choosing a similar dominating option. This *attraction effect* is one of the most widely studied phenomena in decision-making research. Its prevalence, however, has been recently challenged by the *tainting hypothesis*, according to which the inferior option contaminates the attribute space it is located in, leading to a *repulsion effect*. In an attempt to test the tainting hypothesis and explore the conditions under which dominated options make dominating options look bad, we conducted four (pre-registered) perceptual decision-making studies with a total of 301 participants. We identified two factors influencing individuals' behavior: *stimulus display* and *stimulus design*. Our results contribute to a growing body of literature showing crucial differences between preferential and perceptual decision-making tasks.

Keywords: repulsion effect, attraction effect, context effects, decision making

When humans make decisions, context matters. Several studies have shown that the introduction of a *decoy option* that is similar to but objectively worse than one of the already-available options increases the probability that the similar-but-better option is chosen–an *attraction effect* (e.g., Berkowitsch, Scheibehenne, & Rieskamp, 2014; Gluth, Hotaling, & Rieskamp, 2017; Heath & Chatterjee, 1995; Huber, Payne, & Puto, 1982). For example, in a scenario where one chooses between buying an apple and a banana, the introduction of an equally expensive but less attractive banana will increase the probability that its more attractive counterpart is chosen. In recent years, the attraction effect has played an important role in the comparison of different models of decision making (Bhatia, 2013; Roe, Busemeyer, & Townsend, 2001; Trueblood, Brown, & Heathcote, 2014; Tsetsos, Usher, & Chater, 2010; Usher & McClelland, 2004). This importance derives from the notion that such kind of context effects represent general properties of decision-making behavior. This notion is supported by studies demonstrating contextual effects in perceptual and inferential judgments made by humans and non-human primates (e.g., Parrish, Evans, & Beran, 2015; Trueblood, 2012; Trueblood, Brown, Heathcote, & Busemeyer, 2013). A prominent example is Trueblood et al.'s (2013) demonstration of different context effects–such as the attraction effect–in a perceptual task where individuals were asked to choose the largest of three rectangles (see Choplin & Hummel, 2005, for another perceptual task showing the attraction effects).

Despite the wealth of evidence for the attraction effect, its robustness has recently been challenged in a large-scale replication attempt in which a *repulsion effect* was found to occur just as often (Frederick, Lee, & Baskin, 2014). Repulsion effects are expected under the *tainting hypothesis* (Simonson, 2014, p. 518), according to which similar, yet clearly inferior choice alternatives "taint" the attribute space they are located in (see also Kreps, 1990, p. 28 for a thought experiment). The repulsion effect has not yet been explored systematically, and the few attempts to observe it have failed to find robust effects (Simonson, 2014, p. 518). Finally, it is currently unclear how the attraction effect is affected by the distance (in the attribute space) between the dominating and dominated options (Soltani, De Martino, & Camerer, 2012; Wedell, 1991).

The present set of four pre-registered studies is an attempt to close these gaps by testing the tainting hypothesis and exploring the conditions under which attraction/repulsion effects are observed. In line with Trueblood et al. (2013), we used a perceptual decision-making task, which provided us with fine-grained control over the features of the stimuli. As reported below, we generally observed large repulsion effects, with an attraction effect only being observed under a very specific set of circumstances. Our investigations showed that two features of our experimental designs, *stimulus design* and *stimulus display,* played a critical role in which context effect we observed. The influence of such features raises concerns regarding the generalizability of context effects to non-preferential choice tasks.

Experiment 1

Previous research has demonstrated that attraction effects disappear when individuals are provided with an unattractive set of options (Huber, Payne, & Puto, 2014, p. 523; Malkoc, Hedgcock, & Hoeffler, 2013). This absence of attraction effects could be due to attribute-space tainting. This possibility highlights the fact that most studies have been conducted along with either positive incentives for the participants (e.g., Herne, 1999) or none whatsoever (e.g., Trueblood et al., 2013). If the occurrence of attraction effects is indeed modulated by the overall attractiveness of the choice context, then one could in principle manipulate it by introducing negative incentives (i.e., losses), which are well-known to have a disproportionate weight in people's choices (Kahneman & Tversky, 1979).

In Experiment 1 we tested this possible explanation by manipulating monetary incentives. We expected to observe a *gain/loss framing effect* (along the lines of Tversky and Kahneman's, 1981, famous Asian disease problem), comprised of an attraction effect in the context of positive incentives (monetary gains; no tainting of attribute space), and a repulsion effect in the context of negative incentives (monetary losses; tainting of attribute space). This control allowed us to test the tainting hypothesis, but also to explore the moderating role of the attribute distance between the options.

Method

Our main hypotheses, experimental methods, and analysis procedures were pre-registered on the Open Science Framework. Ethical approval was obtained through the institutional review boards of the Faculty of Psychology at the University of Basel (Experiments 1 and 4b) and the College of Arts and Sciences at Syracuse University (Experiments 2, 3, and 4a). The data were partly blinded prior to the analysis. All details on the pre-registration and blinding, raw individual data, and R data-analysis scripts can be found at https://osf.io/4hw6m/.

Participants and procedure. A total of 62 participants (44 female, age 19-55, $M = 25.39$, $SD = 8.37$), mostly students of psychology at the University of Basel, with normal or corrected-to-normal vision participated in Experiment 1. The experiment was conducted in the laboratory with screen resolutions of 1920x1080 pixels. After giving informed consent and filling out the demographic questions, participants completed a calibration task that familiarized them with the response buttons (see Supplemental Materials for details). After completing the calibration task, participants received instructions for the main task and were given three practice trials that were not part of the main task. On each trial, participants were shown three rectangles of different sizes and had to choose the one with the largest area (see Trueblood et al., 2013). The rectangles were presented in a triangle around the center of the screen, with the vertical positions being jittered across trials. Figure 1 illustrates an example trial. The main task took approximately 45-60 min to complete, including four breaks. Afterwards, participants received the reward accumulated in the main experimental task (CHF 7.10 – 8.80, *M* = 8.03, *SD* = 0.36) in addition to the course-credit equivalent of 1 hour.

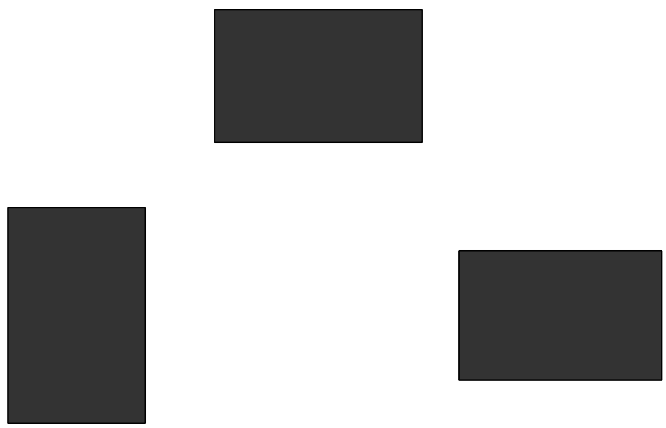

Figure 1. Example of an experimental trial. Participants had to indicate the rectangle with the largest area (in this example the rectangle on the left, narrow/high rectangle). The rectangle in the middle is the wide/low rectangle (WL), and the rectangle on the right is a decoy for WL.

Materials and design. In the main task, participants always saw three rectangles with different area sizes. The two core rectangles differed in orientation, with one being narrow but high (NH; i.e., vertical orientation) and the other wide but low (WL; i.e., horizontal orientation). The third option, the decoy, had the same orientation as one of the core rectangles, but had a smaller area. The option with the unique orientation in each of the trials was the competitor. Note that the repulsion effect is supposed to increase the choice share of the option with the different orientation of the decoy. For notational brevity, we call the option with the decoy's orientation the target, and the other rectangle the competitor, independently of the underlying hypotheses.

Our experimental design consisted of one between-subject manipulation (gain/loss framing) and five within-subject factors. The gain/loss framing concerned whether the task was framed within a context of gains (gain condition: odd participant numbers; 31 participants, 19 female, age 19-48, $M = 25.77$, $SD = 7.74$) or within a context of losses (loss condition: even participant numbers; 31 participants, 25 female, age 19-55, $M = 25.00$, $SD = 9.06$). In the gain condition, participants started with an initial endowment of CHF 0, receiving approximately CHF 0.01 for each correct response (i.e., choosing the rectangle with the largest area), CHF 0.005 for each intermediate response (i.e., choosing the rectangle with the second-largest area), and nothing for each incorrect response (i.e., choosing the rectangle with the smallest area). In the loss condition, participants were endowed with CHF 10 at the beginning and lost nothing, CHF 0.005, and CHF 0.01 for correct, intermediate, and incorrect responses, respectively. At the end of the experiment, participants could receive up to CHF 10 if all responses were correct, and CHF 0 if all responses were incorrect. As such, the incentive structures of both conditions were identical ($M_{\text{gain condition}}$ = CHF 7.97; $M_{\text{loss condition}}$ = CHF 8.09), $t(60) = 1.33$, $p = .187$, $d = 0.34$.

The within-subject factors were set type, difficulty, target option, decoy type, and attribute distance, resulting in a 2x3x2x3x3 within-subject design. Factor "set type" codes which of the core rectangles was larger, WL or NH. Factor "difficulty" codes the difference in areas between the two core rectangles. They differed either by 3%, 7%, or 30% (catch trials) relative to the larger one (i.e., if WL was larger than NH, then the area size of NH was 97%, 93%, and 70% of WL, respectively). Factor "target" codes which of the two core rectangles is the target (i.e., which orientation the decoy has). Factor "decoy type" codes whether the decoy is smaller on the target's weaker attribute (*range decoy*), the target's stronger attribute (*frequency decoy*), or on both attributes (*range-frequency decoy*). This terminology was introduced in the original paper on the attraction effect (Huber et al., 1982). Factor "attribute distance" codes the difference in area between the target and the decoy, which was either 2%, 5%, or 9%. In the catch trials, these differences were 20%, 50%, and 90%, respectively. In total, there were 108 different factor combinations. We created nine unique, symmetrical WL-NH rectangle pairs (230-270 pixels width, 150-190 pixels height) and applied the 108 manipulations to each of them, resulting in a total of 972 trials (for the full trial list, see pre-registration). The main factors of interest are decoy type and attribute distance, while the other factors serve as controls to nullify certain decision strategies (e.g., "pick the unique rectangle" or "take the larger of the two similar ones") and ultimately balance the experimental design. See Figure 2 for an illustration of the within-subject factors.

We used two different instances of dependent variables. When evaluating participants' overall performance in the main task, we considered the proportions of correct, intermediate, and incorrect choices. When testing for the context effects, we relied on the *relative choice share of the target* (RST; Berkowitsch et al., 2014): $\mathrm{RST} = \frac{\mathrm{Pr(T)}}{\mathrm{Pr(T)+Pr(C)}}$, where Pr(T) is the proportion of target choices and Pr(C) is the proportion of competitor choices. RST values range from 0 (competitor is always chosen) to 1 (target is always chosen), where RST = .50 indicates an absence of context effects, and RST > .50 and RST < .50 indicates the presence of an attraction and repulsion effect, respectively. By using the RST as a dependent measure, we automatically control for individual prior preferences for horizontally or vertically aligned rectangles. Directional pre-registered hypotheses have been tested with one-tailed tests, where applicable. All other analyses were analyzed using two-tailed tests.

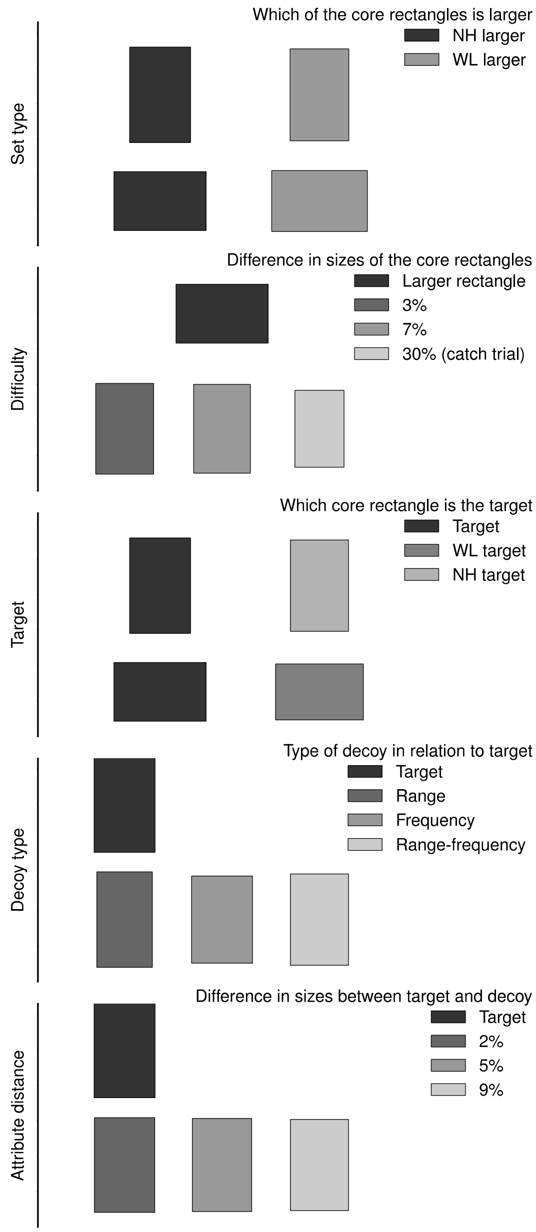

Figure 2. Illustration of the stimuli used across the five within-subject factors in Experiment 1 and 2. WL = wide/low rectangle (i.e., horizontally aligned); NH = narrow/high rectangle (i.e., vertically aligned). See Method/Materials and Design for all explanations of the manipulations.

Results

Data pre-treatment and accuracy checks. Following Trueblood et al. (2013), all of our studies excluded participants according to their overall accuracy in catch trials (in our case, less than 2/3 correct instead of a relative exclusion rule) and trials according to their speed (responses faster than 100ms and longer than 8s). In a second step, we checked whether the order of choice proportions matched the areas of the rectangles. In other words, the largest rectangle should be chosen more often than the second-largest one, which in turn should be chosen more often than the smallest one. Finally, we tested whether trial difficulty influenced choice accuracy such that more difficult trials led to a lower accuracy compared to less difficult trials and catch trials. Overall, we only excluded a small portion of the participants. The remaining participants' responses were generally accurate and tracked the areas of the stimuli. The test results for all four studies are reported in detail in the Supplemental Material.

Confirmatory hypothesis testing. We excluded the catch trials from all hypothesis tests, leaving us with 648 trials per participant. To test for the gain-loss framing effect, we compared the RSTs in the between-subject conditions with a one-tailed t-test for independent samples. **T**he mean RSTs in the gain condition did not differ from their loss-condition counterparts ($M = .43$, $SD = .07$, vs. $M = .43$, $SD = .05$), $t(60) = -0.14$, $p > .250$, $d = -0.04$. In the loss condition, a large repulsion effect was present as confirmed by a one-tailed, one-sample t-test on RSTs; $t(30) = 7.18$, $p < .001$, $d = 1.29$. In the gain condition, there was no attraction effect, $t(30) = -5.57$, $p > .250$, $d = -1.00$, but another clear repulsion effect (as reflected in the sign of the t value and effect size).

To test for the superiority of the range decoy relative to the other decoy types in the gain condition (replication of Trueblood et al., 2013), we computed a one-tailed repeated-measures t-test on mean RSTs. The range decoy did not lead to a stronger attraction effect than the other decoy types (M = .43, SD = .07 vs. M = .43, SD = .07), t < 1, d = 0.18. Figure 3 (hatched bars) reports the choice proportions for each of the rectangles.

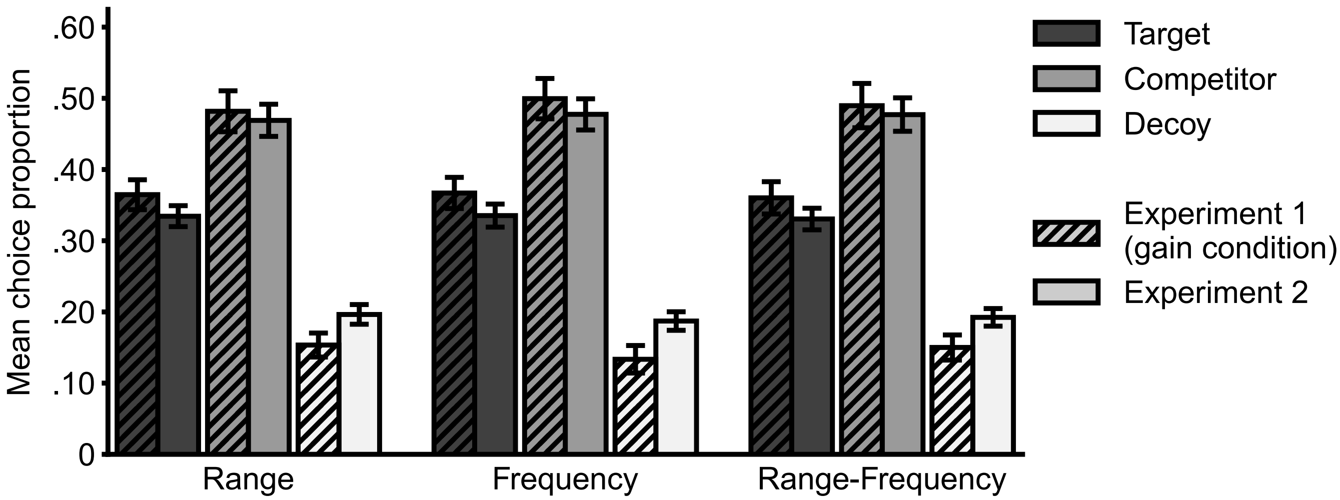

Figure 3. Choice proportions for different decoy types in the gain condition of Experiment 1 (hatched bars) and Experiment 2. For reasons of notational brevity, target always refers to the core rectangle that is similar to the decoy, independently of the underlying hypothesis. Range decoys are weaker on the target's weaker attribute (i.e., are narrower than the target if the target is oriented vertically), frequency decoys are weaker on the target's stronger attribute (i.e., are shorter than the target if the target is oriented vertically), and range-frequency are weaker on both attributes (i.e., are narrower and shorter). Error bars indicate 95% CI.

As our last hypothesis test, we checked for the influence of distance in the attribute space between the target and decoy. We anticipated the possibility of different effects for different conditions, so we performed a 2 (gain/loss framing) x 3 (decoy distance) mixed ANOVA on RSTs. As expected based on the results of the first hypothesis, there was no main effect of gain/loss framing (F < 1). There was, however, the predicted main effect of distance, $F(2, 120)$ = 25.67, p < .001, $\eta_p^2 = .30$. It was characterized by an increase of RSTs (i.e., weakening repulsion effects), with *M* = .41 (*SD* = .08), *M* = .42 (*SD* = .07), and *M* = .45 (*SD* = .06), for the 2%, 5%, and 9% distances, respectively (see Figure 4, top left panel, for the choice proportions of the individual rectangles). This main effect was independent of gain/loss framing, as corroborated by a non-significant interaction term ($F < 1$).

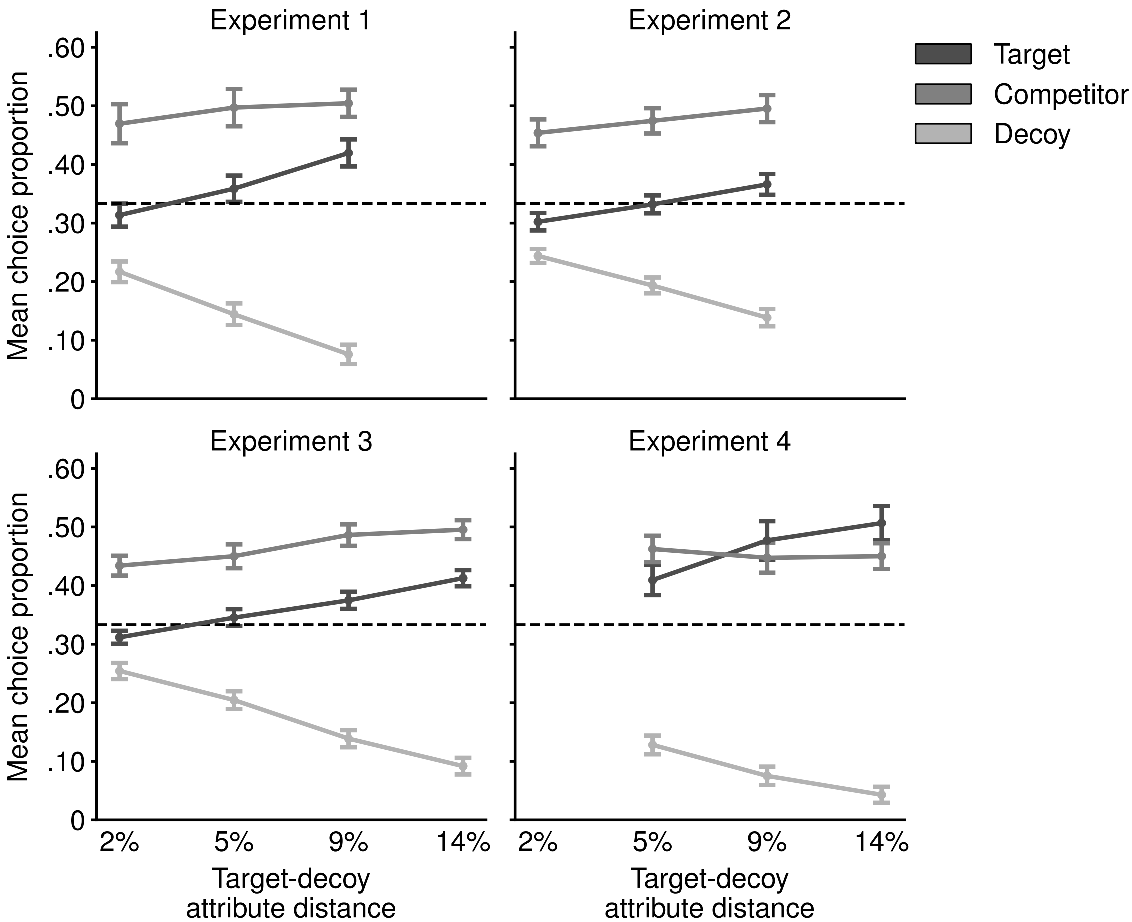

Figure 4. Choice proportions for different target-decoy distances in all experiments. Distances are always in relative area of the target (i.e., 2% indicates that the area of the decoy is 98% of the target's). Experiment 1 includes only the gain-framing condition (as per pre-registration). Experiment 4 includes only the new-trials condition of Experiment 4a, as the direct-replication condition did not manipulate target-decoy distance. Error bars indicate 95% CI.

Exploratory analyses. To assess the robustness of the repulsion effect, we tested the impact of potential covariates. It has been argued that the magnitude of attraction effects increases with deliberation time (Pettibone, 2012). Descriptively, this notion is supported as our participants took longer to respond when they chose the target option (*M* = 1815ms, *Md* = 1446ms, *SD* = 1241ms) than when they chose the decoy (*M* = 1755ms, *Md* = 1364ms, *SD* = 1262ms) or the competitor (*M* = 1675ms, *Md* = 1330ms, *SD* = 1157ms). Another potential covariate is the presence of *strong prior trade-offs*, meaning that individuals have strong preference for one of the core options before the introduction of the decoy (Huber et al., 2014, p. 522). In our paradigm, we have two different levels of prior trade-offs coded into the difficulty: In the easier trials, participants have a stronger "prior preference" for the correct alternative, as it is easier to perceive which of the core rectangles is larger. We checked for both the influence of response time and difficulty and found that both factors influenced the absolute size of the repulsion effect but without a sign flip (i.e., the repulsion effect persists across all factor levels). Since the effects were rather fragile across experiments, we report them together with further analyses in detail in the Supplemental Material.

Experiment 2

The goal of Experiment 2 was to replicate the repulsion effect using a more streamlined design that drops the gain-loss framing manipulation as well as the monetary incentives. The main empirical findings that we aimed to establish were the consistent repulsion effect and its dependency on the attribute distance between the target and decoy.

Method and Results

A total of 61 undergraduate students at Syracuse University with normal or corrected-to-normal vision participated in Experiment 2 (age 18-33, M = 18.98, SD = 2.07). Besides the gain-loss framing and monetary incentives, the experimental task, procedure, and design was identical to Experiment 1. Participants only received a performance-independent course-credit equivalent of 1 hour.

Figure 5. Illustration of two trials sharing the same core rectangles (trial IDs 439 and 443, respectively). Plotted below each rectangle are their corresponding choice proportions for Experiment 1 and 2, respectively. In this case, the decoy rectangle (D) is a range decoy, the wide/low rectangle (WL) is 3% smaller than the narrow/high rectangle (NH), and the distance between the target and D is 5%.

Confirmatory hypothesis testing. We excluded the catch trials from all hypothesis tests, leaving us with 648 trials per participant. A one-tailed, one-sample t-test on RSTs confirmed the same repulsion effect observed in Experiment 1, $t(57) = 8.78$, $p < .001$, $d = 1.15$. The repulsion effect is manifested by means of a relative preference for the competitor over the target. See Figure 5 for an example of such a set. In that example, NH is preferred over WL, when the decoy is similar to WL. On the other hand, if a decoy similar to NH is present, then individuals preferred WL over NH. As the second and last pre-registered hypothesis test, we checked for the influence of distance in the attribute space between the target and decoy. We performed a repeated-measures ANOVA on RSTs with distance as the within-subject factor. This analysis confirmed the predicted main effect of distance, $F(2, 114) = 5.25$, $p < .01$, $\eta_p^2 = .08$. It was characterized by an increase of RSTs (i.e., weakening repulsion effects), with mean values of .40 (*SD* = .08), .41 (*SD* = .08), and .43 (*SD* = .08), for the 2%, 5%, and 9% distances, respectively (see Figure 4, top right panel, for the choice proportions of the individual rectangles).

Experiment 3

Experiment 3 aimed at making the target-decoy distance more comparable to one used by Trueblood et al. (2013). One notable difference between their design and ours is the distance between targets and decoys in the attribute space. Specifically, they used relative size differences of (on average) 16% and 10% for the range/range-frequency and frequency decoys, respectively. They found that range and range-frequency decoys led to attraction effects, whereas frequency decoys did not produce significant effects (although the observed pattern was in the direction of the attraction effect). In our previous experiments, the largest target-decoy distance was 9%, comparable to Trueblood et al.'s. frequency-decoy attribute distance.

Method and Results

A total of 72 participants, mostly psychology students at Syracuse University, with normal or corrected-to-normal vision participated in Experiment 3 (ages 18-23, *M* = 18.98, *SD* = 1.17). Apart from the following changes, the experimental task, procedure, and design was identical to Experiment 2. The main difference is the addition of a new attribute distance factor level. As a logical progression of our previous factor levels, we added the 14% target-decoy attribute distance level. Consequently, to still maintain the balancing that controls for different decision strategies, we added a new difficulty factor level for which the core rectangles differed in area sizes by 11%. Instead of the scaled target-decoy distance in the catch trials used in our previous experiments, we fixed it to 20% in Experiment 3. Having seen no differences in target types, we removed this factor and used only range-frequency decoys. The changes resulted in a 2 (set type) x 4 (difficulty) x 2 (target) x 4 (distance) within-subject design. In total, there were 64 different factor combinations within each participant. For each of these combinations we had 9 unique trials, resulting in a total of 576 trials (for the full trial list, see pre-registration). Consequently, the experiment took about 40 minutes to complete and participants received the course-credit equivalent of an hour.

Confirmatory hypothesis testing. We excluded the catch trials from all hypothesis tests, leaving us with 432 trials per participant. A one-tailed, one-sample t-test on RSTs confirmed the repulsion effect in this experiment as well, $t(62) = 9.27$, $p < .001$, $d = 1.17$. As the second and last pre-registered hypothesis test, we checked for the influence of attribute-space distance between the target and the decoy. A repeated-measures ANOVA on RSTs with distance as the within-subject factor confirmed the predicted main effect of distance, $F(3, 186) = 5.76$, $p < .001$, $\eta_p^2 = .08$. It was characterized by an increase of RSTs (i.e., weakening repulsion effects), with mean values of .42 (*SD* = .06), .44 (*SD* = .08), .44 (*SD* = .07), and .45 (*SD* = .06) for the 2%, 5%, 9%, and 14% distances, respectively (see Figure 4, bottom left panel, for the choice proportions of the individual rectangles).

Exploratory analyses. A unique feature of Experiment 3 is that the decoy's area size always differed by 20% from the target's. As a robustness check, we performed an RST analysis using the catch trials (after excluding too inaccurate participants and too fast/slow responses). This analysis is particularly conservative given that decoys were further away from the target in the attribute space (compared to Trueblood et al., 2013) and that the largest rectangle was more clearly perceivable. Nevertheless, a two-tailed, one-sample t-test on RSTs (M = .49, SD = .03) confirmed the robustness of the repulsion effect; $t(71) = 2.29$, $p = .02$, $d = 0.27$.

Experiment 4

Experiment 4 aimed at identifying the moderators that promote the occurrence of attraction/repulsion effects. We identified three factors that might influence whether attraction effects or repulsion effects occur: (i) stimulus design, (ii) stimulus display (i.e., arrangement of the rectangles on-screen), and (iii) absolute size of the rectangles. We believe that the latter's influence is only marginal, resulting in two critical factor combinations that we explored in Experiment 4: a) A stimulus design similar to our previous experiments arranged as in Trueblood et al. (2013), and b) Trueblood et al.'s stimulus design arranged as in our previous experiments.

Method and Results

A total of 83 participants, mostly undergraduate students at Syracuse University, with normal or corrected-to-normal vision participated in Experiment 4a (ages 18-55, M = 19.02, SD = 4.14. Twenty-three psychology students at the University of Basel participated in Experiment 4b (ages 18-30, M = 22.11, SD = 3.38; demographic data of five participants were lost). Apart from the stimulus design, both sub-experiments were identical to Experiments 2 and 3.

In Experiment 4a, we contrasted a *direct-replication condition* (N = 40) with a *new-trials condition* (N = 43) in which we made minimal changes to the way the stimuli were generated. In both conditions, the stimuli were closely arranged along a horizontal line (with some vertical jitter), as done in Trueblood et al. (2013). The new-trials condition contained the very same 180 filler trials as in the direct-replication condition. The remaining 540 experimental trials stemmed from a 2 (target) x 3 (decoy type) within-subject design in the direct-replication condition (see

Trueblood et al., 2013, pp. 903–904 for details) and from a 2 (difficulty: no area difference between the core rectangles as in Trueblood et al., 2013, vs. 7% area difference) x 3 (distance between decoy and target: 5%, 9%, and 14%) x 3 (decoy type) within-subject design in the new-trials condition. Apart from difficulty (one additional level) and decoy distance (two additional levels), the stimulus design of the new-trials condition is identical to the design of the direct-replication condition (i.e., the core rectangles were mostly around 80 pixels x 50 pixels large). With 36 factor combinations, the new-trials condition's design is significantly simpler than the one used in Experiments 1 and 2 (108 factor combinations) and 3 (64 factor combinations), and only slightly more complex than the direct-replication condition (6 factor combinations).

Experiment 4b is a replication of Trueblood et al.'s (2013) attraction-effect experiment, with the sole exception that the options were presented in a triangular arrangement as used in our Experiments 1-3 (see Figure 1). Both experiments took about 40 minutes to complete and participants received the course-credit equivalent of an hour.

Confirmatory hypothesis testing. We excluded the filler trials from all hypothesis tests, leaving us with 540 trials per participant. As expected, we successfully replicated the attraction effect in the direct-replication condition of Experiment 4a ($M_{RST} = .55$, $SD_{RST} = .09$), $t(32) = 2.92$, $p < .01$, $d = 0.51$. Contrary to our expectations, we did not observe a repulsion effect in the new-trials condition ($M_{RST} = .50$, $SD_{RST} = .07$). If anything, participants' behavior tended to go in the direction of the attraction effect, $t(29) = 0.33$, $p = .63$, $d = 0.06$. The difference in mean RSTs between the two conditions was significant; $t(61) = 1.98$, $p = .03$, $d = 0.50$. In contrast to Experiment 4a, we observed a strong repulsion effect in Experiment 4b ($M_{RST} = .47$, $SD_{RST} = .04$), as confirmed by a two-tailed one-sample t-test, $t(19) = 3.65$, $p < .01$, $d = 0.82$.

Exploratory analyses. We began by checking the influence of distance in the attribute space between the target and decoy in the new-trials condition of Experiment 4a. Specifically, we performed a repeated-measures ANOVA on RSTs with distance as the within-subject factor. As in the previous experiments, this analysis confirmed a main effect of target-decoy attribute distance, $F(2, 58) = 24.71$, $p < .001$, $\eta_p^2 = .46$. This effect was characterized by an increase of RST, with mean values of .47 (*SD* = .07), .51 (*SD* = .09), and .53 (*SD* = .07) for the 5%, 9%, and 14% distances, respectively (see Figure 4, bottom right panel, for the choice proportions of the individual rectangles). In contrast to the previous experiments, this main effect seems to suppress the global RST analysis: Individuals seem to show a repulsion effect for the shortest target-decoy attribute distance, an attraction effect for the largest target-decoy attribute distance, and a null effect for the in-between attribute distance. To confirm this intuition, we ran three two-tailed one-sample t-tests on the RSTs within each distance level separately. Indeed, we observed a small-to-moderate repulsion effect for the 5% distance, $t(29) = 2.31$, $p = .03$, $d = 0.42$, no context effect for the 9% distance, $t < 1$, $d = 0.16$, and a small-to-moderate attraction effect for the 14% distance, $t(29) = 2.10$, $p = .04$, $d = 0.38$. In a next step, we checked for the influence of decoy type separately for each condition of Experiment 4a and Experiment 4b. In none of these cases did decoy type influence the strength of the attraction effect or repulsion effect, all *p*s ≥ .20.

A Multiattribute Linear Ballistic Accumulator Account

To gain a more mechanistic understanding of the cognitive process underlying the behavior in our experiments, we fitted the multiattribute linear ballistic accumulator (MLBA) model (Trueblood et al., 2014) to the data of Experiments 3 and 4.

The MLBA describes choice proportions in multiattribute decision tasks by means of five parameters. According to the model, the objective attribute values of the options (in the present case, width and height) are converted into subjective representations. These subjective representations are characterized by a parameter *m* determining whether individuals prefer options that are very diverse or homogenous with respect to their attributes. Moreover, parameters λ_1 and λ_2 capture the subjective importance that is attributed to positive and negative attribute comparisons, respectively. Individuals' preferential attention toward one of the attributes (e.g., paying more attention to widths than heights) is captured by parameter β. Finally, the model postulates a "normalizing" parameter I_0 that ensures that a decision is reached eventually.

The MLBA was fitted to the individual choices obtained in Experiment 3 and 4 (for details, see the Supplemental Material). As shown in Figure 6, the MLBA was able to provide a close qualitative and quantitative account for the patterns observed in both experiments. In terms of parameter values, we found that the main driving force behind whether repulsion effects or attraction effects are predicted was the ratio of the two λ parameters. When the model predicts attraction effects, the ratios differ only slightly, whereas λ_2 is substantially larger than λ_1 when repulsion effects are predicted.

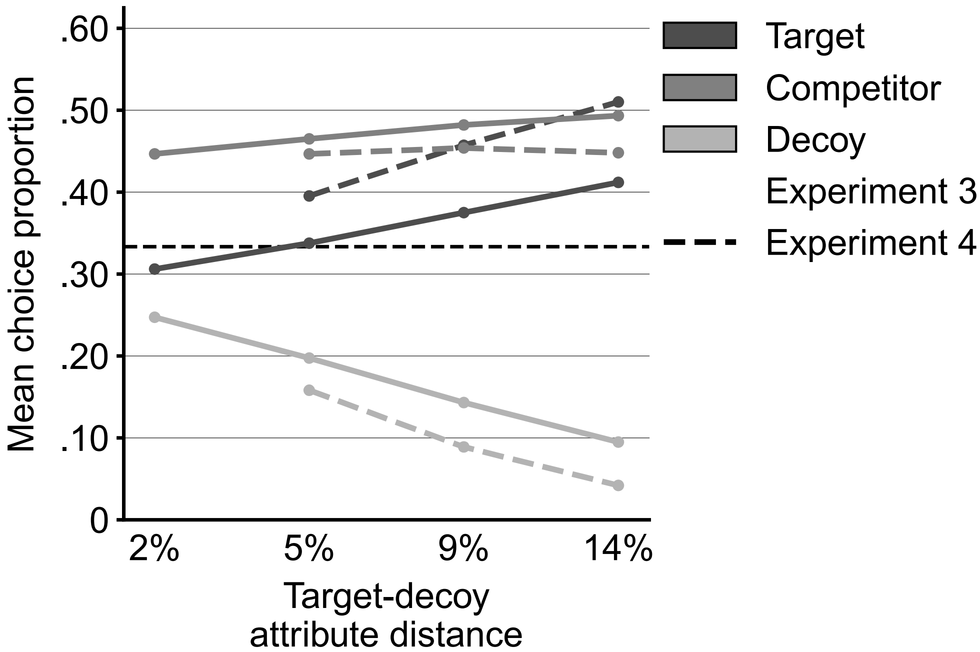

Figure 6. Multiattribute linear ballistic accumulator model predictions for different target-decoy attribute distances in Experiments 3 and 4. Distances are always in relative area of the target (i.e., 2% indicates that the area of the decoy is 98% of the target's). Experiment 4 includes only the new-trials condition of Experiment 4a, as the direct-replication condition did not manipulate target-decoy attribute distance. Lines represent grand-mean predictions.

General Discussion

The present work attempted to test the tainting hypothesis of the repulsion effect according to which unattractive options taint the attribute space they are locating in, thus making other nearby options less attractive. This effect mirrors the attraction effect, in which dominating options appear more attractive. We aimed at determining the conditions under which dominated options yield one of the two effects. Across four pre-registered experiments, we found a large and robust repulsion effect, an effect whose empirical reality had been questioned until recently (e.g., Tsetsos, Chater, & Usher, 2015). Moreover, we also found that both task complexity and arrangement of options on-screen determine whether attraction effects, null effects, or repulsion effects are observed. Finally, by varying the distance in the attribute space between the dominating option and its dominated counterpart, we found that increases in attribute distance shifted choices towards an attraction effect.

The attraction-repulsion continuum

Since Trueblood et al. (2013), the rectangle-size task has been used in four studies involving humans (Farmer, Warren, El-Deredy, & Howes, 2016; Frederick et al., 2014; Trueblood, Brown, & Heathcote, 2015; Zhen & Yu, 2016). Two of these studies found evidence in favor of the attraction effect, one had mixed results, and one showed a tendency in the direction of the repulsion effect. Our study helps bridging the gap between these results.

We observed two main driving factors: 1) arrangement of the rectangles on-screen, and 2) stimulus design. Surprisingly, the influence of the former far surpasses the latter's: As soon as the options are arranged further apart from each other, the attraction effect disappears entirely and even becomes a robust repulsion effect. But the stimulus design also plays a crucial role: As soon as individuals face choice sets of varying difficulty and, more importantly, with varying attribute distances between the target and decoy, the repulsion effect becomes stronger or the attraction effect becomes weaker, depending on the option arrangement.

Decoys located further away from the target make it easier to notice the dominance relationship between them, whereas closer decoys are at risk to be confused as equally-sized rectangles. In the latter case, individuals might exhibit the *similarity effect* (Tversky, 1972). At first glance, the repulsion effects we demonstrated might seem like a similarity effect, as both predict an increase of choices for the option dissimilar to the decoy. The crucial difference between the similarity effect and the repulsion effect, however, is that the decoy in the former case is perceived as on par with the target, and as inferior in the latter. In settings like Trueblood et al.'s (2013), where there is no objectively correct choice, it is impossible to tell whether the dominance relationship has been perceived (resulting in a repulsion effect) or whether the target and decoy have been confused with each other (resulting in a similarity effect). However, our experiments controlled for this confound and found no support whatsoever for this "similarity-effect interpretation", given that individuals chose the decoy significantly less often than the target, showing an ability to discriminate between the two (see Supplemental Material).

Interestingly, although the MLBA only predicts repulsion effects under specific circumstances (Tsetsos et al., 2015), it was able to account for the present repulsion effects by placing substantially greater weight on negative comparisons relative to positive comparisons. However, this successful description of the data may stem from the fact that the model was not constrained by having to simultaneously predict other context effects (see Hotaling & Rieskamp, under review, for a demonstration of MLBA's flexibility when fit to only one context effect). A stricter test would require a joint fit of multiple context effects.

The tainting hypothesis

In its present form, the tainting hypothesis simply states that inferior decoys taint the attribute space they are located in. Previous explanations for this tainting included a similarity mechanism (Frederick et al., 2014, p. 493) or the possibility of the target being infected with the decoy's repulsive attributes (Simonson, 2014, p. 518). Both explanations assume higher-level reasoning processes which seem implausible in the rectangle-size task (see Trueblood et al., 2014 for a similar discussion on loss aversion in perceptual tasks). The tainting hypothesis, as we see it, predicts that tainting should be a decreasing function of distance in the attribute space, a result that was supported empirically. But instead of a framing-dependent tainting, we found universal attribute-space tainting.

The observation of a perceptual context effect that is completely opposite to consumer-choice counterpart is not unique to the present study (for a recent example, see Trueblood & Pettibone, 2017). These gross differences suggest that, despite some obvious structural similarities, there are fundamental differences between the perceptual and preferential tasks usually adopted by researchers (Dutilh & Rieskamp, 2016; Hotaling, Cohen, Shiffrin, & Busemeyer, 2015; Wu, Delgado, & Maloney, 2009). To make matters worse, some of the large behavioral differences observed appear to be due to minor changes in one of the features of the experimental design. Indeed, given the purported generality of the attraction effect across judgment domains, none of these minor changes should have been of any major consequence. Also, neither the MLBA nor any of the extant models provides any a priori mechanisms for *explaining* these behavioral differences. Altogether, it seems unwise to assume by default that choices in a perceptual task are proxies for preferential decision making. We conclude that in the rectangle-size task, researchers are much more likely to observe a repulsion than an attraction effect, as the latter requires the joint occurrence of several specific factor combinations, and the former arises in all other cases.

Conclusion

The observation of a robust repulsion effect has implications for current theoretical discussions. The overall performance of different theories has been assessed in terms of their ability to simultaneously account for different context effects observed in the literature, but also in terms of their ability to predict unobserved effects (Tsetsos, Chater, & Usher, 2015; see also Roberts & Pashler, 2000). Until now, the prediction of a repulsion effect has been perceived as an unfavorable feature for a model to have due to the lack of empirical support. The present demonstration that the repulsion effect is a real, robust, and replicable phenomenon changes that.

Research disclosure statements:

The total number of excluded observations and the reasons for making these exclusions have been reported in the Method section. All independent variables and manipulations, whether successful or failed, have been reported in the Method section (all experiments) and pre-registered before collecting data (Experiments 1-4a). All dependent variables or measures that were analyzed for this article's target research question have been reported in the Method section (all experiments) and pre-registered before collecting data (Experiments 1-4a).

Sample size:

In Experiment 1, given that attraction effects are typically very robust and strong, achieving even a weak reversal of the attraction effect in the loss condition would have led to a large effect size in a between-subject comparison. Given our sample size, $1-\beta = .80$, $\alpha = .05$, and a one-tailed test, effect sizes of $d = 0.65$ were detectable. The rest of the analyses rely on within-subject effects, for which we have almost 1,000 observations per participant, plenty for within-subject analyses. The main effect of interest, the repulsion effect, had an effect size of $d > 1.00$. For a power of $1-\beta = .95$, only 13 participants would have been required. With our sample sizes, we were able to detect effects as small as $d = 0.43$.

As we pre-registered our experiments, we did not have any optional stopping rules (except for Experiment 4a, for which we pre-registered the optional stopping rule). Participants exceeding the pre-registered sample sizes were allowed to participate because they registered for participation before the last participant required to fulfill the pre-registration participated (2 in Experiment 1, 1 in Experiment 2, 12 in Experiment 3).

Author contributions:

All authors developed the study concept and design. D. Kellen and M. S. Spektor coordinated the data collection. M. S. Spektor performed the data analysis and interpretation under supervision of D. Kellen and J. M. Hotaling. J. M. Hotaling performed computational modeling on Experiments 3 and 4. M. S. Spektor drafted the manuscript, D. Kellen critically revised it, and J. M. Hotaling gave final critical comments. All authors approved the final version of the manuscript for submission.

Acknowledgements:

We thank Markus Steiner, Tris Buck, Tehilla Mechera-Ostrovsky, Henrik Singmann, and Sebastian Gluth for their help.

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