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

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Abstract

When people are 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 challenged recently by the *tainting hypothesis*, according to which the inferior option contaminates the attribute space in which it is located, 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 preregistered 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 how presentation format influences behavior in preferential and perceptual decision-making tasks.

Keywords

repulsion effect, attraction effect, context effects, decision making, open data, open materials, preregistered

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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 in which one chooses between buying an apple and buying 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 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 nonhuman 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 in which individuals were asked to choose the largest of three rectangles (see Choplin & Hummel, 2005, for another perceptual task showing the attraction effect).

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

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to which similar yet clearly inferior choice alternatives "taint" the attribute space in which they are located (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 preregistered experiments 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 we will report below, we generally observed large repulsion effects, with an attraction effect being observed only 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 nonpreferential 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 no incentives 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), comprising an attraction effect in the context of positive incentives (monetary gains and no tainting of attribute space) and a repulsion effect in the context of negative incentives (monetary losses and tainting of attribute space). This control allowed us to test the tainting hypothesis and also to explore the moderating role of the attribute distance between the options.

Method

Our main hypotheses, experimental methods, and analysis procedures were preregistered on the Open Science Framework.[1] Ethical approval was obtained through the institutional review boards of the University of Basel Faculty of Psychology (Experiments 1 and 4b) and the College of Arts and Sciences at Syracuse University (Experiments 2, 3, and 4a). The data were partly anonymized prior to the analysis. All details on the preregistration, data collection, and anonymization, as well as full trial lists, experimental code, raw individual data, and R data-analysis scripts can be found on the OSF (https://osf.io/48kyp/).[2]

Participants and procedure. A total of 62 participants (44 women, 18 men; age: range = 19–55 years, 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 on monitors with screen resolutions of 1,920 × 1,080 pixels. After giving informed consent and answering the demographic questions, participants completed a calibration task that familiarized them with the response buttons (for details, see the Supplemental Material available online). 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 asked 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 to 60 min to complete, including four breaks. Afterward, participants received the reward accumulated in the main experimental task (Swiss francs, or CHF = 7.10–8.80; M = 8.03, SD = 0.36) in addition to the course-credit equivalent of 1 hr.

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.

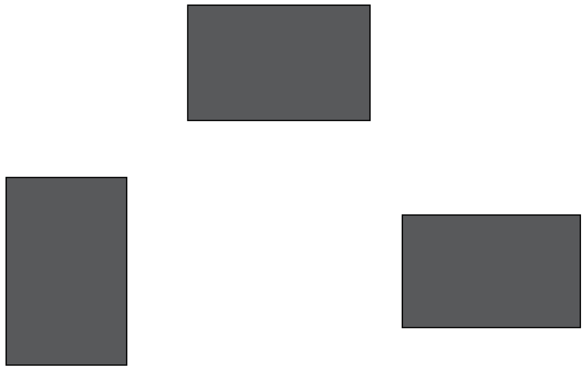

Fig. 1. Example of an experimental trial. Participants had to indicate the rectangle with the largest area (in this example, the rectangle on the left, which is the narrow-but-high rectangle). The rectangle in the middle is the wide-but-low rectangle, and the rectangle on the right is a decoy for the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

Our experimental design consisted of one between-subjects manipulation (gain/loss framing) and five within-subjects factors. The gain/loss framing concerned whether the task was framed within a context of gains (gain condition; 31 participants: 19 women, 12 men; age: range = 19–48 years, M = 25.77, SD = 7.74) or within a context of losses (loss condition; 31 participants: 25 women, 6 men; age: range = 19–55 years, 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). The loss condition mirrored the gain condition. Participants were initially endowed with CHF 10 at the beginning of the experiment and lost money for incorrect responses: approximately CHF 0.005 for each intermediate response and CHF 0.01 for each incorrect response. 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 (gain condition: M = CHF 7.97; loss condition: M = CHF 8.09), $t(60) = 1.33$, $p = .187$, $d = 0.34$, 95% confidence interval (CI) = [−0.17, 0.86].

The within-subjects factors were set type, difficulty, target option, decoy type, and attribute distance, resulting in a 2 × 3 × 2 × 3 × 3 within-subjects design. Set type coded which of the core rectangles was larger, WL or NH. Difficulty coded the difference in areas between the two core rectangles. They differed by 3%, 7%, or 30% (catch trials) relative to the larger one (e.g., if the WL rectangle was larger than the NH rectangle, then the area size of the NH rectangle was 97%, 93%, and 70%, respectively, of the area of the WL rectangle). Target coded which of the two core rectangles was the target (i.e., which orientation the decoy had). Decoy type coded whether the decoy was smaller on the target's weaker attribute (range decoy), on the target's stronger attribute (frequency decoy), or on both attributes (range-frequency decoy). This terminology was introduced in the original article on the attraction effect (Huber et al., 1982). Attribute distance coded the difference in area between the target and the decoy, which was 2%, 5%, or 9%. In the catch trials, these differences were 20%, 50%, and 90%, respectively. In total, there were 108 different factor combinations. For each level of set type and difficulty (six levels), we created nine unique, symmetrical WL-NH rectangle pairs (230–270 pixels width, 150–190 pixels height) and applied the 18 decoy-related manipulations to each of them, resulting in a total of 972 trials (for the full trial list, see the preregistration). The main factors of interest were decoy type and attribute distance, whereas the other factors served 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-subjects factors.

Our design differed from the original design of Trueblood et al. (2013) in the following aspects: (a) offering incentives and manipulation of gain/loss framing between subjects (vs. no incentives), (b) always showing core rectangles of unequal sizes (vs. always the same size, except for catch trials), (c) manipulating attribute distance (vs. always the same attribute distance), and (d) randomly varying the absolute size of rectangles (vs. core rectangles that were 80 × 50 pixels large with some jitter applied).

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):

$$\text{RST} = \frac{\Pr(\text{T})}{\Pr(\text{T}) + \Pr(\text{C})},$$

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 indicate the presence of an attraction and a repulsion effect, respectively. By using the RST as a dependent measure, we automatically controlled for individual prior preferences for horizontally or vertically aligned

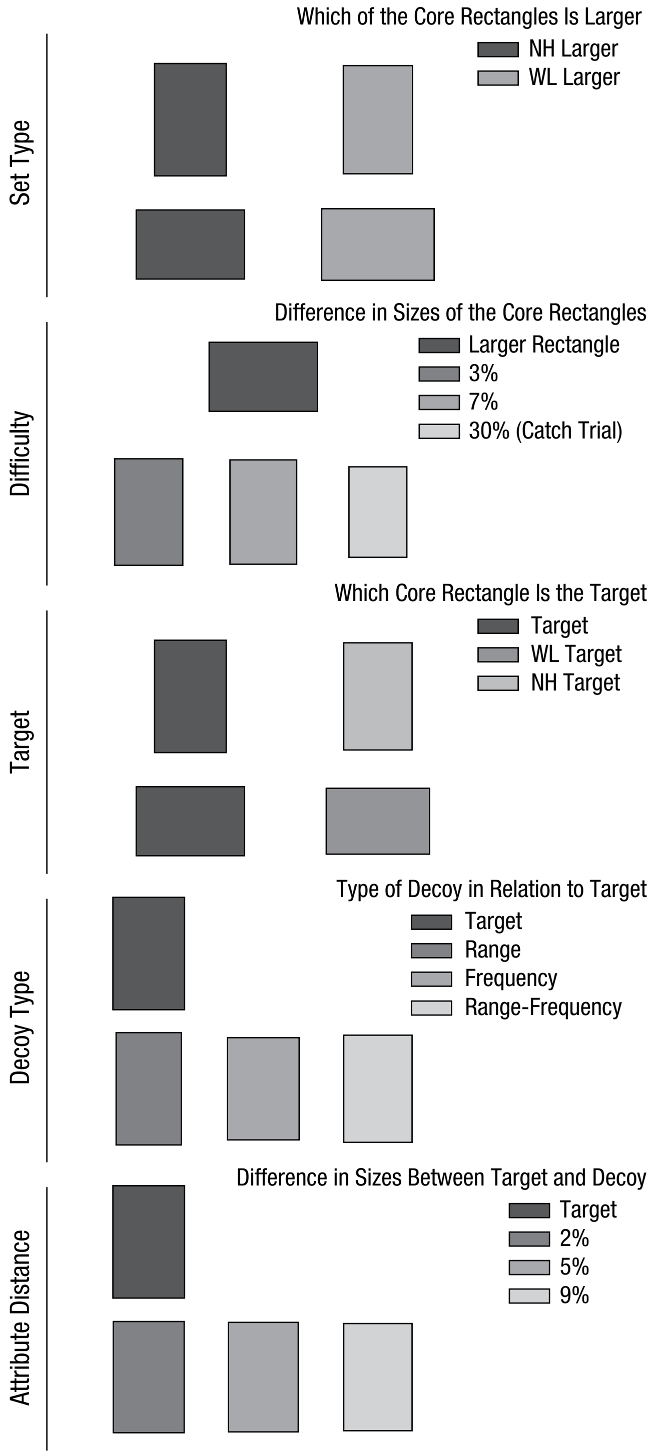

Fig. 2. Illustration of the stimuli used for each of the five within-subjects factors in Experiments 1 and 2. WL = wide-but-low rectangle (i.e., horizontally aligned); NH = narrow-but-high rectangle (i.e., vertically aligned). See the Method section for an explanation of the manipulations.

rectangles. Directional preregistered hypotheses were tested with one-tailed tests, where applicable. All other analyses were conducted using two-tailed tests. CIs for *d*s were obtained using noncentral *t* distributions. In the case of η_p^2s, we obtained the CIs via nonparametric bootstrapping (using 10,000 samples per interval).

Results

Data pretreatment and accuracy checks. Following the study by Trueblood et al. (2013), in all of our experiments, we excluded participants according to their overall accuracy in catch trials (in our case, less than two-thirds correct instead of a relative exclusion rule) and trials according to their speed (speed criterion: responses faster than 100 ms and longer than 8 s). In Experiment 1, no participants were excluded on the basis of their accuracy on catch trials, and 1.29% of the trials were excluded on the basis of the speed criterion. 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 with less difficult trials and catch trials. Overall, we excluded only 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 experiments 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-subjects conditions with a one-tailed *t* test for independent samples. The mean RSTs in the gain condition did not differ from their loss-condition counterparts (M = .43, SD = .07 vs. M = .43, SD = .05), $t(60) = -0.14$, $p > .250$, $d = -0.04$, 95% CI = [−0.54, 0.47]. In the loss condition, a large repulsion effect was present, as confirmed by a one-tailed, one-sample *t* test on RSTs, $t(30) = 7.18$, $p < .001$, $d = 1.29$, 95% CI = [0.81, 1.76]. In the gain condition, there was no attraction effect, $t(30) = -5.57$, $p > .250$, $d = -1.00$, 95% CI = [−1.43, −0.56], 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 (as observed by 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), $t(30) = 0.99$, $d = 0.18$, 95% CI = [−0.18, 0.53]. Figure 3 (hatched bars) reports the choice proportions for each of the rectangles.

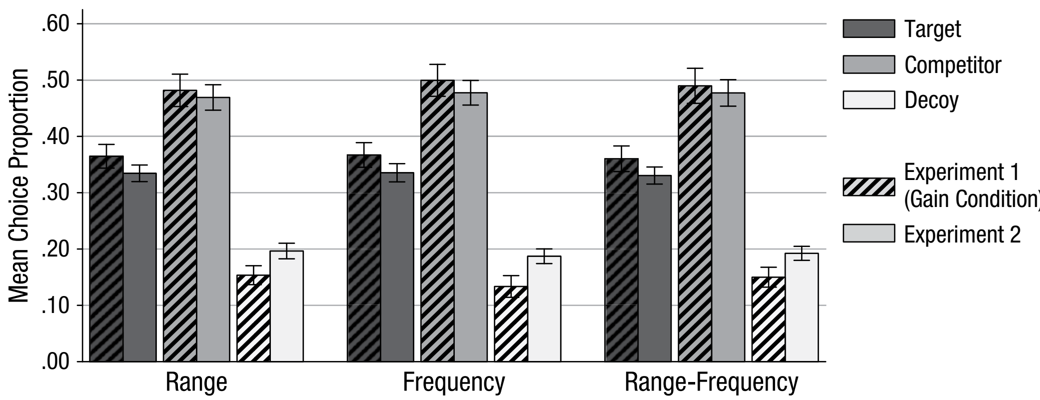

Fig. 3. Choice proportions for different decoy types in the gain condition of Experiment 1 and in Experiment 2. "Target" refers to the core rectangle that is similar to the decoy, independently of the underlying hypothesis. Range decoys were weaker on the target's weaker attribute (i.e., were narrower than the target if the target was oriented vertically), frequency decoys were weaker on the target's stronger attribute (i.e., were shorter than the target if the target was oriented vertically), and range-frequency decoys were weaker on both attributes (i.e., were narrower and shorter). Error bars indicate 95% confidence intervals.

In 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) × 3 (decoy distance) mixed analysis of variance (ANOVA) on RSTs. As expected on the basis of 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, $F(2, 120) = 25.67$, $p < .001$, $\eta_p^2 = .30$, 95% CI = [.17, .46]. 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 the solid lines in Fig. 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 nonsignificant interaction term ($F < 1$).

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 was supported because our participants took longer to respond when they chose the target option (M = 1,815 ms, Mdn = 1,446, SD = 1,241) than when they chose the decoy (M = 1,755 ms, Mdn = 1,364, SD = 1,262) or the competitor (M = 1,675 ms, Mdn = 1,330, SD = 1,157). Another potential covariate is the presence of strong prior trade-offs, meaning that individuals have a strong preference for one of the core options before the introduction of the decoy (Huber et al., 2014, p. 522). In our paradigm, we had two different levels of prior trade-offs coded into the difficulty. In the easier trials, participants had a stronger prior preference for the correct alternative because it was easier to perceive which of the core rectangles was larger. We checked for 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 persisted across all factor levels). Because 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

A total of 61 undergraduate students at Syracuse University with normal or corrected-to-normal vision participated in Experiment 2 (age: range = 18–33 years, M = 18.98, SD = 2.07). Besides the gain/loss framing and monetary incentives, the experimental task, procedure, and design were identical to those in Experiment 1. Participants received only a performance-independent course-credit equivalent of 1 hr. Three participants were excluded on the basis of their accuracy on catch trials, leading to a final sample size of 58. We excluded 0.4% too-fast or too-slow trials. Qualitatively, results did not change when all data were included in the analyses.

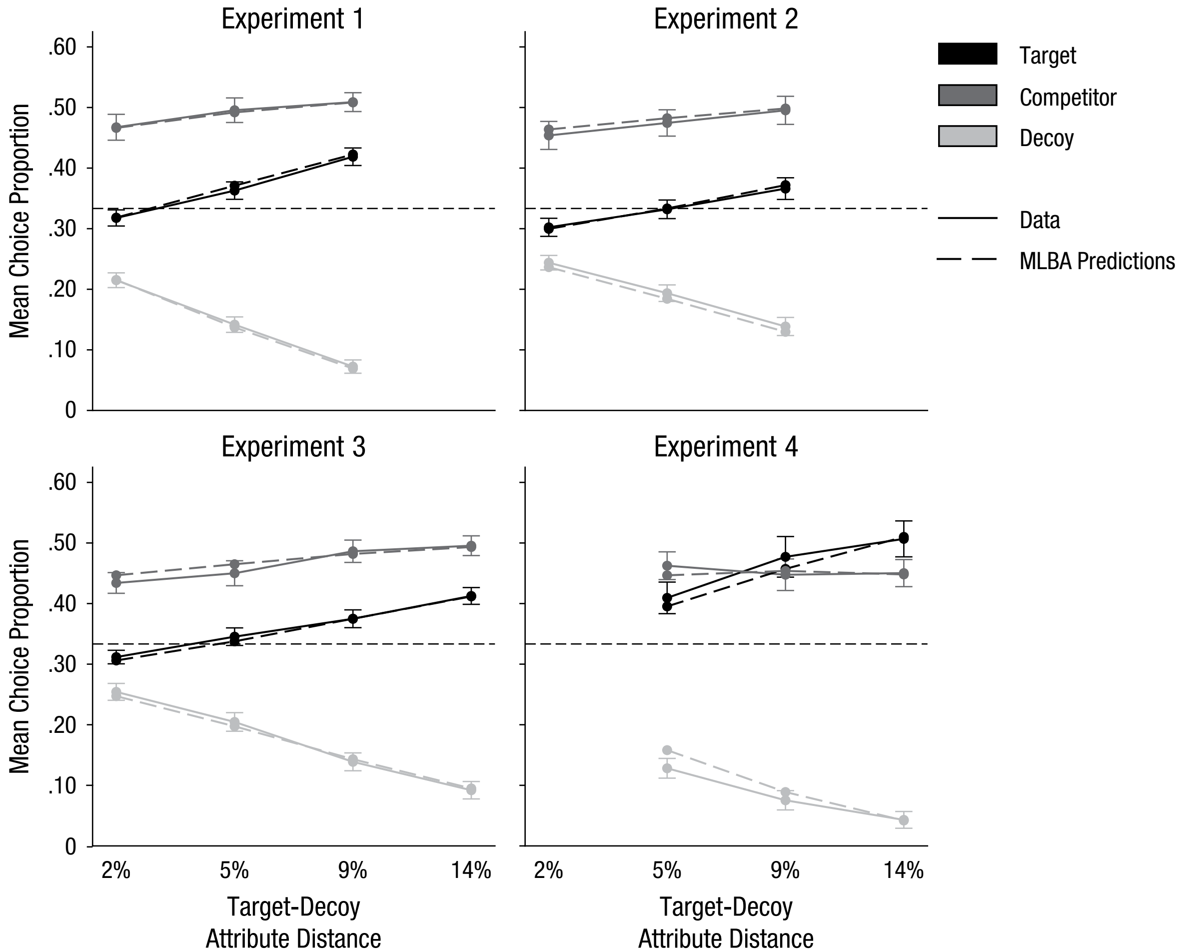

Fig. 4. Choice proportions for different target-decoy distances and predictions of the multiattribute-linear-ballistic-accumulator (MLBA) model in all experiments. Distances are always in relative area of the target (i.e., 2% indicates that the area of the decoy is 98% of the target's area). The graph for Experiment 1 includes both framing conditions. The graph for Experiment 4 includes only the new-trials condition of Experiment 4a, because the direct-replication condition did not manipulate target-decoy attribute distance. Error bars indicate 95% confidence intervals, and the dashed horizontal lines at .33 on the *y*-axes represent chance.

Results

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, $t(57) = 8.78$, $p < .001$, $d = 1.15$, 95% CI = [0.82, 1.48]. 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, the NH rectangle was

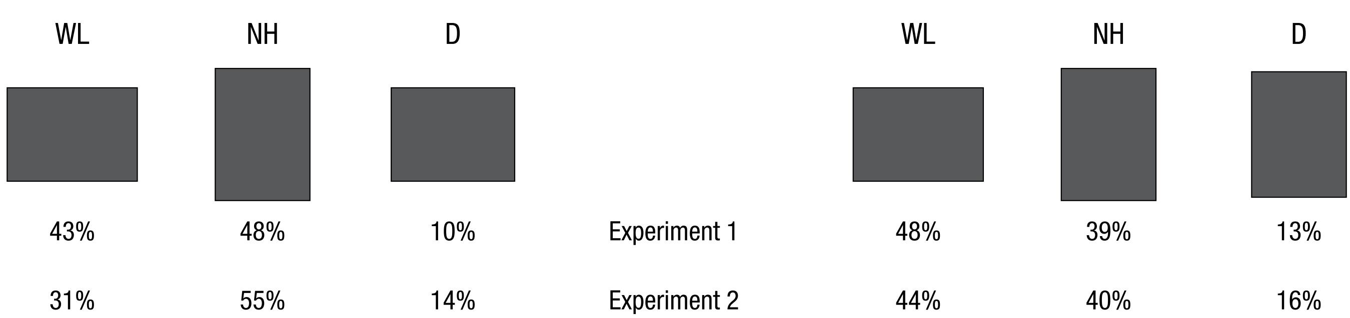

Fig. 5. Illustration of two trials sharing the same core rectangles (trial IDs 439 and 443, respectively). Plotted below each rectangle are their corresponding choice proportions for Experiments 1 and 2, respectively. In this case, the decoy (D) rectangle is a range decoy, the wide-but-low (WL) rectangle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

preferred over the WL rectangle, when the decoy was similar to the WL rectangle. On the other hand, if a decoy similar to the NH rectangle was present, then individuals preferred the WL rectangle over the NH rectangle. In the second and last preregistered 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-subjects factor. This analysis confirmed the predicted main effect of distance, $F(2, 114) = 5.25$, $p < .01$, $\eta_p^2 = .08$, 95% CI = [.02, .22]. 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 the solid lines in Fig. 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 with the 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 with Trueblood et al.'s frequency-decoy attribute distance.

Method

A total of 72 participants, mostly psychology students at Syracuse University, with normal or corrected-to-normal vision participated in Experiment 3 (age: range = 18–23 years, M = 18.98, SD = 1.17). Apart from the following changes, the experimental task, procedure, and design were identical to those in Experiment 2. The main difference was the addition of a new attribute distance factor level. We added the 14% target-decoy attribute-distance level as a logical progression of our previous factor levels. Consequently, to 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%. The target-decoy distance in the catch trials was fixed to 20% and did not scale as in our previous experiments. 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) × 4 (difficulty) × 2 (target) × 4 (distance) within-subjects design. In total, there were 64 different factor combinations within each participant. For each level of set type and difficulty (eight levels), we had 9 unique trials, resulting in a total of 576 trials (for the full trial list, see preregistration). Consequently, the experiment took about 40 min to complete, and participants received the course-credit equivalent of 1 hr. In Experiment 3, 9 participants were excluded on the basis of their accuracy on catch trials, leading to a final sample size of 63. We excluded 0.73% too-fast or too-slow trials. Qualitatively, results did not change when all data were included in the analyses.

Results

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, $t(62) = 9.27$, $p < .001$, $d = 1.17$, 95% CI = [0.84, 1.47]. In the second and last preregistered 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-subjects factor confirmed the predicted main effect of distance, $F(3, 186) = 5.76$, $p < .001$, $\eta_p^2 = .08$, 95% CI = [.03, .19]. 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 the solid lines in Fig. 4, bottom left panel, for the choice proportions of the individual rectangles).

Exploratory analyses. A unique feature of Experiment 3 was that in catch trials, the decoy's area size always differed by 20% from the target's area size. As a robustness check, we performed an RST analysis using the catch trials (after excluding participants who were too inaccurate and responses that were too fast or slow). This analysis was particularly conservative given that decoys were farther away from the target in the attribute space (compared with that in 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, $t(62) = 2.60$, $p = .01$, $d = 0.33$, 95% CI = [0.07, 0.58].

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: (a) stimulus design, (b) stimulus display (i.e., arrangement of the rectangles on screen), and (c) absolute size of the rectangles. We believed that the latter's influence was only marginal, resulting in two critical factor combinations that we explored in Experiment 4: (a) a stimulus design similar to that in 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

A total of 83 participants, mostly undergraduate students at Syracuse University, with normal or corrected-to-normal vision participated in Experiment 4a (age: range = 18–55 years, *M* = 19.02, *SD* = 4.14). Twenty-three psychology students at the University of Basel participated in Experiment 4b (age: range = 18–30 years, *M* = 22.11, *SD* = 3.38; demographic data of 5 participants were lost). Apart from the stimulus design and stimulus display, both subexperiments 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) × 3 (decoy type) within-subjects design in the direct-replication condition (for details, see Trueblood et al., 2013, pp. 903–904) and from a 2 (difficulty: no area difference between the core rectangles, as in Trueblood et al., 2013, vs. 7% area difference) × 3 (distance between decoy and target: 5%, 9%, and 14%) × 3 (decoy type) within-subjects 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 was identical to the design of the direct-replication condition (i.e., the core rectangles were mostly around 80 × 50 pixels large). With 36 factor combinations, the design of the new-trials condition was significantly simpler than the one used in Experiments 1 and 2 (108 factor combinations) and Experiment 3 (64 factor combinations) and only slightly more complex than the design of the direct-replication condition (6 factor combinations).

Experiment 4b was 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 to 3 (see Fig. 1). Both experiments took about 40 min to complete, and participants received the course-credit equivalent of 1 hr.

In Experiment 4a, 20 participants were excluded on the basis of their accuracy on catch trials, leading to a final sample size of 63: 30 in the new-trials condition and 33 in the direct-replication condition. In Experiment 4b, 3 participants were excluded because of a computer crash or on the basis of their accuracy on catch trials, leading to a final sample size of 20. Across the two subexperiments, we excluded 1.21% too-fast or too-slow trials. Qualitatively, results did not change when all data were included in the analyses.

Results

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 (RST: *M* = .55, *SD* = .09), $t(32) = 2.92$, $p < .01$, $d = 0.51$, 95% CI = [0.14, 0.87]. We did not observe a repulsion effect in the new-trials condition (RST: *M* = .50, *SD* = .07), contrary to our expectations. If anything, participants' behavior tended to go in the direction of the attraction effect, $t(29) = 0.33$, $p = .63$, $d = 0.06$, 95% CI = [−0.30, 0.42]. The difference in mean RSTs between the two conditions was significant, $t(61) = 1.98$, $p = .03$, $d = 0.50$, 95% CI = [−0.01, 1.01]. We observed a strong repulsion effect in Experiment 4b (RST: *M* = .47, *SD* = .04), in contrast to Experiment 4a, as confirmed by a two-tailed one-sample *t* test, $t(19) = 3.65$, $p < .01$, $d = 0.82$, 95% CI = [0.30, 1.32].

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-subjects factor. As in the previous experiments, this analysis confirmed a main effect of target-decoy attribute distance, $F(2, 58) = 24.71$, $p < .001$, $\eta_p^2 = .46$, 95% CI = [.29, .64]. 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 the solid lines in Fig. 4, bottom right panel, for the choice proportions of the individual rectangles). In contrast to the previous experiments, this main effect seemed to suppress the global RST analysis: Individuals seemed 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, $t(29) = 2.31$, $p = .03$, $d = 0.42$, 95% CI = [0.04, 0.79]; no context effect for the 9% distance, $t(29) = 0.86$, $d = 0.16$, 95%

CI = [–0.20, 0.52]; and a small-to-moderate attraction effect for the 14% distance, $t(29) = 2.10$, $p = .04$, $d = 0.38$, 95% CI = [0.01, 0.75]. In a next step, we checked for the influence of decoy type separately for each condition of Experiments 4a and 4b. In none of these cases did decoy type influence the strength of the attraction effect or repulsion effect (all ps ≥ .20).

A Multiattribute-Linear-Ballistic-Accumulator (MLBA) Account

To gain a more mechanistic understanding of the cognitive process underlying the behavior in our experiments, we fitted the MLBA model (Trueblood et al., 2014) to the data of our experiments. According to the MLBA, the objective attribute values of the options (in the present case, width and height) were converted into subjective representations. These subjective representations were characterized by a parameter m determining whether individuals prefer options that are very diverse or homogeneous with respect to their attributes. Moreover, the subjective importance was attributed to positive and negative attribute comparisons, given by parameters λ_1 and λ_2, respectively. Individuals' preferential attention toward one of the attributes (e.g., paying more attention to widths than heights) was captured by parameter β. Finally, the model postulated a baseline-input parameter I_0 that ensured that a decision was reached eventually.

The MLBA was fitted to the individual choices obtained in our experiments (for details, see the Supplemental Material). As shown in Figure 4 (interrupted lines), the MLBA provided a close qualitative and quantitative account for the patterns observed in all 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 λ_2 and λ_1 parameters. When the model predicted attraction effects, this ratio was significantly lower than when repulsion effects were predicted, Welch's $t(237.56) = 3.86$, $p < .01$, $d = 0.31$, 95% CI = [0.03, 0.59]. This conclusion is in line with another application of the MLBA to the repulsion effect (Trueblood & Dasari, 2017).

General Discussion

The present work attempted to test the hypothesis that the presence of unattractive options taints the attribute space in which they are located, thus making other nearby options less attractive. This repulsion 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 preregistered 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 toward an attraction effect.

The attraction/repulsion continuum

Since the study by Trueblood et al. (2013) was published, the rectangle-size task has been used in six studies involving humans (Farmer, Warren, El-Deredy, & Howes, 2017; Frederick et al., 2014; Trueblood, Brown, & Heathcote, 2015; Trueblood & Dasari, 2017; Turner, Schley, Muller, & Tsetsos, 2018; Zhen & Yu, 2016). Three of these studies found evidence in favor of the attraction effect, two had mixed results, and one showed a tendency in the direction of the repulsion effect. Our study helps to bridge the gap between these results.

We observed two main driving factors: (a) arrangement of the rectangles on screen and (b) stimulus design. Surprisingly, the influence of the former far surpassed that of the latter: When the options were arranged farther apart, the attraction effect disappeared entirely and even became a robust repulsion effect. But the stimulus design also played a crucial role: When individuals faced choice sets of varying difficulty and, more important, with varying attribute distances between the target and decoy, the repulsion effect became stronger or the attraction effect became weaker, depending on the option arrangement.

Decoys located farther away from the target make it easier to notice the dominance relationship between them, whereas closer decoys are at risk of being 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. We found no support for this similarity-effect interpretation, because individuals chose the decoy significantly less often than the target, showing an ability to discriminate between the two (see the Supplemental Material).

Interestingly, the MLBA accounted for the present repulsion effects by placing substantially greater weight on negative comparisons relative to positive comparisons (see also Tsetsos et al., 2015). Likewise, it accounted for both the original and reversed similarity effect in another study (Cataldo & Cohen, 2018). However, these successful descriptions 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, in press, for a demonstration of the MLBA's flexibility when fit to only one context effect). A stricter test would therefore require a joint fit of multiple context effects (whose co-occurrence should be reassessed).

The tainting hypothesis

In its present form, the tainting hypothesis simply states that inferior decoys taint the attribute space in which they are located. Previous explanations for this tainting include a similarity mechanism (Frederick et al., 2014, p. 493) and 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 observing a framing-dependent tainting, we found it to be ubiquitous.

Perceptual versus preferential tasks

Recent research has highlighted the importance of comparison processes for context-effect research (Noguchi & Stewart, 2014; Trueblood & Dasari, 2017). Arguably, stimulus display affects the way options are evaluated, with attention potentially mediating the resulting context effect. Relatedly, stimulus display has also been shown to affect context effects in the preferential-choice domain (Cataldo & Cohen, 2018; Chang & Liu, 2008). It remains to be seen whether altering stimulus display in a preferential task can lead to repulsion effects as well. Also of note is the fact that neither the MLBA nor any of the extant models provide any a priori mechanisms for explaining or predicting these behavioral differences.

Other studies have reported perceptual context effects that strongly differ from their consumer-choice counterparts (e.g., Trueblood, 2015; Trueblood & Pettibone, 2017). These differences suggest that, despite some obvious structural similarities, there are fundamental differences between the adopted perceptual and preferential tasks (Dutilh & Rieskamp, 2016; Hotaling, Cohen, Shiffrin, & Busemeyer, 2015). Altogether, it seems unwise to assume by default that choices in a perceptual task are good proxies for preferential decision making or that the underlying processes are comparable. We conclude that in the rectangle-size task, researchers are much more likely to observe a repulsion effect 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 et al., 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 because of the lack of empirical support. The present demonstration that the repulsion effect is a real, robust, and replicable phenomenon changes that.

Action Editor

Timothy J. Pleskac served as action editor for this article.

Author Contributions

All authors developed the study concept and design. D. Kellen and M. S. Spektor coordinated the data collection. M. S. Spektor analyzed and interpreted the data under the supervision of D. Kellen and J. M. Hotaling. J. M. Hotaling performed the computational modeling of the experiments. 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.

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Supplemental Material

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Open Practices

All data and materials have been made publicly available via the Open Science Framework (OSF) and can be accessed at https://osf.io/48kyp/. The experiments were preregistered at the OSF (https://osf.io/48kyp/). The complete Open Practices Disclosure for this article can be found at http://journals.sagepub.com/doi/suppl/10.1177/0956797618779041. This article has received badges for Open Data, Open Materials, and Preregistration. More information about the Open Practices badges can be found at http://www.psychologicalscience.org/publications/badges.

Notes

1. The total number of excluded observations and the reasons for making these exclusions have been reported in the Method section of each experiment. All independent variables and manipulations, whether successful or failed, have been reported in the Method section (all experiments) and were preregistered before data collection began (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 were preregistered before collecting data (Experiments 1–4a).

2. 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-subjects 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 relied on within-subjects effects, for which we had almost 1,000 observations per participant—plenty for within-subjects 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 $d = 0.43$. Because we preregistered our experiments, we did not have any optional stopping rules (except for Experiment 4a, for which we preregistered the optional stopping rule). Participants exceeding the preregistered sample sizes were allowed to participate because they registered for participation before the last participant required to fulfill the preregistration participated (2 in Experiment 1, 1 in Experiment 2, 12 in Experiment 3).

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