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The Attraction Effect in Perceptual Decision-Making: A Case of Dominance Asymmetry

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ABSTRACT

The attraction effect (AE), or asymmetric dominance effect, occurs when the presence of a clearly inferior decoy increases the choice of a target over a competitor. While robust in value-based domains, findings with perceptual stimuli have been inconsistent, with some studies even reporting reversals in triangular arrangements of stimuli. Across four experiments and a reanalysis of prior data, we demonstrate that these inconsistencies are attributable to the presence or absence of genuine item-wise dominance asymmetry.

Experiment 1 established that novel star-shaped stimuli reliably produced strong target–decoy (TD) dominance over competitor–decoy (CD) dominance, whereas traditional rectangles showed weaker but still positive asymmetry. Experiment 2 provided the first robust demonstration of a positive AE with perceptual stimuli in a triangular layout using stars, while Experiment 3 showed that rectangle stimuli, when presented in a triangular layout, produced an aggregate null rather than a negative effect. Similarly, the reanalysis of data from a previous triplet experiment involving bars stimuli pointed towards a null effect. Experiment 4 again linked inconsistent findings from linear versus triangular alignment of triplet rectangles to the presence of asymmetric dominance, while also demonstrating an interaction between the differential ease of comparison in pairs and presentation format.

Together, these results demonstrate that AE is a robust phenomenon that emerges whenever decoys create strong item-level dominance asymmetry. Apparent inconsistencies with perceptual stimuli reflect stimulus-specific dominance structures. This work clarifies the boundary conditions of the AE, reinforces its domain generality, and provides methodological guidance for future research on context effects in perceptual decision-making.

Keywords: preference reversals, attraction effect, asymmetric dominance, perceptual, decoy

1 GENERAL INTRODUCTION

The principle of *Independence of Irrelevant Alternatives* (IIA) is foundational in rational choice theory, stating that the relative odds of choosing between two alternatives should remain unchanged by the introduction or removal of additional alternatives (Luce, 1957):

$$\frac{P(i|C)}{P(j|C)} = \frac{P(i|C')}{P(j|C')}$$

for options i, j in sets $C \subset C'$.

The *regularity* axiom is a related, but weaker, requirement that adding an alternative cannot increase the probability of choosing any of the original options (Luce, 1977):

$$P(i|C') \leq P(i|C)$$

for any i in the original set.

Tversky's work in the 1970s empirically demonstrated violations of IIA—the "similarity effect" showed that new, similar alternatives could alter relative choice odds between original items, challenging the basic premises of deterministic choice theory (Tversky, 1972). In response to these findings, Luce observed that regularity was the only rational choice axiom yet unviolated by data (Luce, 1977). This status changed following the landmark study by Huber, Payne, and Puto (Huber et al., 1982), who showed that the *attraction effect* violates regularity: the addition of a decoy can *increase* the choice probability of an existing option. It is now recognized that regularity is a necessary but not sufficient condition for IIA: any violation of regularity implies a violation of IIA, but IIA can be violated without a corresponding violation of regularity.

The attraction effect (AE) or asymmetric dominance effect is a widely studied choice bias in decision-making where the presence of a third option (the "decoy": D) influences decision-makers to prefer one of the original options (the "target": T) over the other (the "competitor": C). The phenomenon is both practically and theoretically important. Practically, it serves as a behavioral nudge to influence consumers' choices (Devine et al., 2025). Theoretically, as we discussed, it demonstrates a violation of the assumption in rational choice theory: regularity, and hence IIA.

Over the past forty years, researchers have documented the attraction effect across various decision-making contexts. For instance, choices under risk exhibit systematic shifts when a dominated alternative is introduced (Farmer et al., 2017), and category-based inference similarly shows sensitivity to contextual distractors (Trueblood, 2012; Choplin and Hummel, 2005). Preference for a target option also changes in typical consumer goods settings (Huber et al., 1982; Noguchi and Stewart, 2014), as well as during sequential motor planning tasks (Farmer et al., 2017). Beyond studies with adult humans, developmental and comparative studies reveal that this context-dependent shift occurs in young children (Zhen and Yu, 2016), domestic cats (Scarpi, 2011), nonhuman primates (Parrish et al., 2015), various avian species including hummingbirds (Bateson et al., 2003), amphibians such as frogs (Lea and Ryan, 2015), social insects like honeybees (Shafir et al., 2002), and even in slime molds (Latty and Beekman, 2011).

Studies by Choplin and Hummel (2005) and Trueblood et al. (2013), which demonstrated attraction effects even in the perceptual domain, became essential milestones in establishing the ubiquity of the effect and challenging the possibility of cardinal representations of value in the brain (Vlaev et al., 2011). Cardinal representations refer to models in which values are encoded on an absolute, context-independent scale, allowing comparisons to be made based on fixed neural metrics. The presence of attraction effects in both value-based and perceptual tasks indicates that the brain's encoding of magnitude is relative and context-dependent, rather than strictly cardinal. Demonstrating these effects in perceptual judgments—where attributes have clear physical units—provides strong evidence that context-dependent comparison processes are fundamental and not restricted to economic or subjective value domains.

Despite its wide documentation across species and domains, recent years have seen a growing number of studies reporting inconsistent, muted, or even reversed attraction effects (repulsion effect) (Frederick et al., 2014; Yang and Lynn, 2014). However, Huber et al. (2014) identified several boundary conditions that can constrain the effect: strong prior preferences between core options, difficulty detecting dominance, individual variation in attribute weighting, and pronounced aversion to or preference for the decoy. They argued that studies failing to meet one or more of these criteria unsurprisingly reported inconsistent effects. Subsequent work has further highlighted such constraints. In the perceptual domain, Spektor et al. (2018, 2022) found reversed effects when stimuli were arranged in a triangular configuration, leading some to question the domain generality of the attraction effect. Most notably, Trendl et al. (2021) rigorously tested the attraction effect with naturalistic stimuli (movies), while satisfying all boundary conditions identified by Huber et al. (2014). However, they observed a precisely zero effect, leading them to conclude that the attraction effect may be limited to stylized experimental stimuli.

We suggest that these recent failures may reflect the violation of an additional, previously overlooked boundary condition: the requirement that the decoy be truly asymmetrically dominated. We developed a

novel class of perceptual stimuli that affords clear and unambiguous asymmetric dominance (explained below) and adopted a pairwise comparison framework commonly used in the choice literature (explained next) to test whether restoring this relational structure reinstates the standard attraction effect.

1.1 Asymmetric Dominance of Decoy

The attraction effect, also known as the asymmetric dominance effect, occurs when the presence of a clearly inferior decoy option increases the likelihood of choosing the target over the competitor (Huber et al., 1982). Central to this phenomenon is the notion of *asymmetric dominance*—the structural relationship between options in the choice set that differentiates targets, competitors, and decoys.

Historically, asymmetric dominance has been defined primarily in **attribute-based terms**, where a decoy is considered asymmetrically dominated if it is inferior to the target on *every* attribute but is partially dominated by (or equal to) the competitor on at least one attribute (Kaptein et al., 2016; Bhatia, 2013; Žofák, 2016; Helgadóttir, 2015). For example, a decoy may be dominated by the target in both price and quality but be better than or equal to the competitor in at least one attribute. This attribute-wise definition of asymmetric dominance has guided much of the experimental literature in designing decoys.

However, the original conceptualization by Huber et al. (1982) adopts an **item-based** definition: an alternative is asymmetrically dominated if it is dominated by one option in the choice set but not by another. This perspective focuses on the overall dominance relationships between *whole options* rather than isolating attribute-level comparisons. Importantly, this item-based asymmetry is broader and encapsulates attribute-based definitions as a special case.

Formally, these definitions can be summarized as follows, where D_x and D_y denote decoy attribute values, T_x and T_y target attribute values, and C_x and C_y competitor attribute values:

$$\text{Attribute-Based Asymmetric Dominance:} \quad D_x \leq T_x, \quad D_y \leq T_y, \quad D_y \leq C_y, \quad D_x > C_x \tag{1}$$

$$\text{Item-Based Asymmetric Dominance:} \quad P(T \mid TD) \gg 0.5, \quad P(C \mid CD) \approx 0.5 \tag{2}$$

where $P(X \mid XY)$ is the probability of choosing option X from binary pair XY, computable as choice frequencies.

It should be noted, however, that the above formalizations concern only structural definitions of asymmetry reflected in behavior, without any commitment to the underlying cognitive process. The distinction is important because it separates structural boundary conditions about dominance asymmetry from assumptions about the cognitive comparison process. Our approach adopts the item-based definition of asymmetry to remain as general as possible and focuses on measurable choice behavior without committing to specific cognitive mechanisms (Banerjee et al., 2024b).

Figure 1 illustrates an example set of items located in a two-dimensional attribute space (x and y), where the criterion value is given by the product of the attributes (i.e., $x \times y$). Here, item C represents a rectangle with width = 5 units and height = 10 units, whereas item T represents a rectangle with width = 10 units and height = 5 units. Assuming that the two rectangles are perceived as equivalent in area (each having 50 square units), a decoy is an item that is inferior to the target in one or both attributes and thereby favors the target. If the decoy is inferior to the target in the target's strongest attribute, it is called a frequency decoy (D_F); if it is inferior to the target in the target's weakest attribute, it is called a range decoy (D_R). If the decoy is inferior to the target in both attributes, it is called a range-frequency decoy (D_{RF}) (Huber et al., 1982). For simplicity, we consider here D_R, which corresponds to a rectangle with an area of 40 square units.

Imagine we ask subjects to rank-order the items along one attribute dimension at a time. Irrespective of whether they do an ordinal comparison of two or three items at a time, the decoy in our example will be asymmetrically dominated according to the attribute-based definition, since it does not surpass the target on either attribute but is at least as good as, or better than, the competitor on one attribute dimension. In contrast, if we present pairs of items at a time and ask subjects to make choices based on the overal criterion values, and they integrate the two attributes into a common currency (here, area), the same decoy

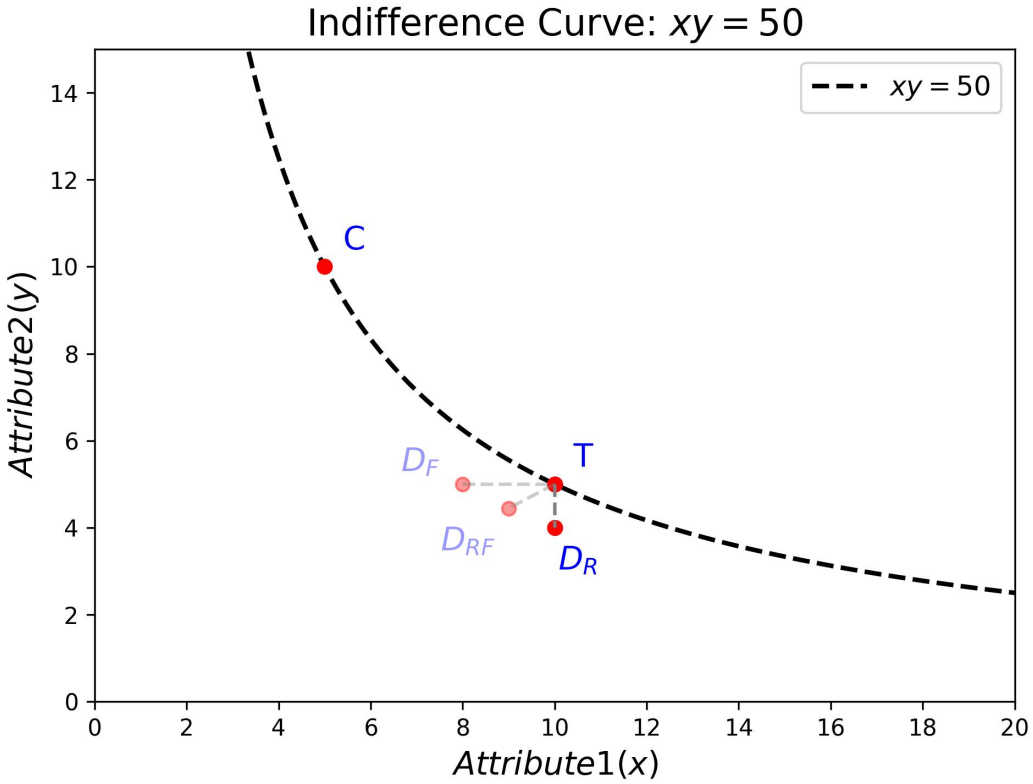

Figure 1. Example of asymmetric dominance in a two-attribute space. The dashed curve shows the indifference curve ($xy = 50$). Items C and T lie on the curve (area = 50). Three possible decoys are shown: D_F (frequency decoy), D_R (range decoy), and D_{RF} (range-frequency decoy).

will no longer be asymmetrically dominated (the item-based definition). Instead, it will be symmetrically dominated by both the target and the competitor (each with an area of 50 compared to the decoy's 40).

1.2 Pairwise Comparisons

Sequential sampling models have provided a rich framework for understanding multi-alternative, multi-attribute choice by viewing decision making as a process of repeated comparisons among options and attributes, with attention shifting to compare and evaluate subsets of options throughout the deliberation process (Roe et al., 2001; Usher and McClelland, 2004). Prominent models of choice (Russo and Dosher, 1983; Wollschläger and Diederich, 2012; Kornienko, 2013; Trueblood et al., 2014; Ronayne and Brown, 2017; Noguchi and Stewart, 2018; Evans et al., 2021) assume that this subset is a pair—pairwise comparisons are at the heart of these models. Eye-tracking studies (Noguchi and Stewart, 2014) also suggest that alternatives are compared in pairs, often along a single attribute dimension.

We primarily draw inspiration for our asymmetric dominance thesis from the stochastic ordinal comparison model introduced by Srivastava and Schrater (2015). Their framework formalizes decision-making as a Bayesian inference problem, where the relative desirability of an option is computed from past choice histories across varying contexts. In this setting, the model infers probabilistic desirabilities by considering the likelihood that an item would be chosen in each observed subset of options, with attentional weights operationalized as the probability of encountering each context. Rather than assigning exogenous utility values, the model learns from observed pairwise choice outcomes and histories to update its beliefs about the desirability of each item, yielding measurable choice probabilities that reflect context-sensitive and history-dependent preference structures.

For a simpler illustration, we present a non-stochastic version of the ordinal pairwise comparison model, formally defined as follows. Consider a set of alternatives $\{A_1, A_2, \ldots, A_m\}$. Each alternative is evaluated

by comparing it pairwise with every other alternative. For any comparison between A_i and A_j ($i \neq j$), the model assigns to A_i a score $V_i(A_i, A_j)$, where $V_i(A_i, A_j) = 1$ if A_i wins, $V_i(A_i, A_j) = 0$ if A_j wins or there is a tie. The total evidence for A_i is then

$$V_i = \sum_{j \neq i} V_i(A_i, A_j),$$

and the selected alternative is

$$A^* = \arg\max_i V_i \quad \text{(the alternative with highest } V_i\text{)}.$$

To accommodate cases in which some pairs are compared more frequently or are given greater attentional weight, we introduce non-negative weights n_{ij} for each ordered pair (A_i, A_j), yielding the generalized evidence function

$$V_i = \sum_{j \neq i} n_{ij}\, V_i(A_i, A_j).$$

Table 1 presents the simplest case with $n_{ij} = 1$ for all $i \neq j$, in which each pair is compared once. This highlights how cumulative pairwise victories can capture asymmetric dominance and thereby influence the final choice. This formulation is descriptive: it specifies how aggregate outcomes arise through the sequential, weighted aggregation of pairwise wins, without committing to any particular psychological or computational process that generates the individual scores $V_i(A_i, A_j)$. Each computation may involve comparisons along single attribute dimensions or holistic evaluations, depending on the structure and commensurability of the attributes. By leaving the basis of comparison unspecified, the model remains general and applicable across contexts. Moreover, the framework allows direct experimental manipulation of the attentional weights n_{ij}, while keeping the aggregation rule itself agnostic to the underlying comparison process.

Table 1. Pairwise "votes" for ordinal comparison scenarios: (A) AE occurs; (B) No AE.

	Pairwise Contest	Winner	Points to T	Points to C
(A) AE	T vs. D	T	+1	0
	T vs. C	Tie	0	0
	C vs. D	D or Tie	0	0
	Total		**1**	**0**
(B) No AE	T vs. D	T	+1	0
	T vs. C	Tie	0	0
	C vs. D	C	0	+1
	Total		**1**	**1**

Other influential models in the literature provide complementary perspectives on pairwise comparison and attention. Noguchi and Stewart (2018) propose the Multialternative Decision by Sampling (MDbS) model, where decision makers randomly sample attribute-option pairs and make ordinal judgments. Process-tracing data indicate that gaze shifts between pairs drive context effects through differences in winning frequencies. MDbS emphasizes attribute-wise ordinal sampling but does not explicitly model asymmetric dominance at the item level.

The Attribute Commensurability and Context Effects (ACE) model, proposed by Hayes et al. (2024) highlights how the comparability of attribute scales influences attentional allocation and, consequently, context effects. Incommensurable attributes produce salient, discriminable comparisons, resulting in robust context effects, whereas commensurable scales lead to holistic processing and attenuated effects. This work formalizes an important link between attribute presentation, attention, and context effects, though asymmetric dominance is operationalized indirectly via attention modulation.

Cataldo and Cohen (2019) offer a flexible comparison-process framework that explains attraction, compromise, and similarity effects by how attentional focus shifts between dimension-level and alternative-level comparisons based on presentation format. Their account emphasizes how stimulus format affects cognitive comparison strategy, but does not treat asymmetric dominance as a mandatory structural condition.

Finally, Trueblood et al. (2022) introduce an attentional dynamics model that captures both attraction and repulsion effects through flexible allocation of attention in multi-attribute choice. Their model suggests that repulsion effects emerge when attention disproportionately focuses on dissimilar alternatives, reversing expected dominance patterns. This highlights the dynamic nature of attention but does not explicitly embed asymmetric dominance as a fixed principle.

Together, these models underscore the importance of pairwise comparisons and attentional weighting in explaining context effects. The Srivastava and Schrater (2015) model uniquely integrates asymmetric dominance as a core component while allowing attention to operate independently, providing the theoretical foundation for our asymmetric dominance thesis. Other models may be extended to include explicit dominance asymmetry, which remains an avenue for future work.

1.3 Overview of Experiments

The current investigation systematically explores the role of item-level asymmetric dominance and attentional modulation in driving the attraction effect across four experiments.

Experiment 1 serves as a diagnostic test to establish dominance asymmetry in binary (pairwise) comparisons. We introduce a novel class of star-shaped stimuli designed to reliably produce robust dominance asymmetry, comparing their performance against traditional rectangular stimuli known to produce inconsistent or reversed attraction effects (Spektor et al., 2018).

Building on these pairwise dominance findings, Experiment 2 tests whether the star stimuli elicit a classic attraction effect in a triplet choice context, assuming the same underlying process in both pair and triplet choice context.

Experiments 3 and 4 address a long-standing puzzle in the perceptual literature: why traditional rectangular stimuli produce positive attraction effects when arranged linearly but reversed effects when arranged in triangular configurations. Experiment 3 investigates this effect through a controlled replication, while Experiment 4 systematically manipulates spatial layout to test attentional mechanisms implicated in modulating evidence accumulation.

Collectively, these studies provide a comprehensive empirical test of the boundary condition that asymmetric dominance of the decoy, coupled with attentional sampling processes, governs the presence and magnitude of attraction effects in perceptual choice.

2 EXPERIMENT 1

2.1 Introduction

In the pre-registered within-subjects experiment 1, we used two independent variables—stimulus type (rectangle vs. star) and comparison pair (CD vs. TD). We predicted a significant interaction effect on the dependent variables of accuracy and reaction times. In addition, we collected perceived difficulty ratings of choice (exploratory) when alternatives were compared in pairs.

2.2 Method

2.2.1 Participants

Sixty-seven (15 female) university students with normal or corrected-to-normal vision (M: 21.57 years, SD: 2.88, range: 17-31), participated in the study.

2.2.2 Apparatus and Stimuli

The experiment was designed using JavaScript and conducted on laboratory computers with screen resolutions of $1920\,\text{px} \times 1080\,\text{px}$ and a screen size of 24 inches (16:9). Stimuli were presented in pairs

(target-decoy pairs and competitor-decoy pairs), and consisted of two types of shapes: rectangles and star-like shapes.

In each trial, the stimuli consisted of two horizontally aligned black-colored shapes on a white background. The two stimuli were positioned horizontally around the screen center at 30% and 70% of the total screen width ($x = 576$ px and $x = 1344$ px, respectively), separated by 768 px ($\approx 21.3\,\mathrm{cm}$; $\approx 20.1^\circ$ of visual angle at a $60\,\mathrm{cm}$ viewing distance). The vertical positions of the stimuli were jittered across trials. The stimulus pair for each trial was derived from the respective set of triplets, which in turn were created in steps from the core stimuli. The term core stimuli refers to the two base stimuli forming the primary comparison set in each trial—the Target (T) and Competitor (C). These rectangular shapes were pre-generated and matched on the relevant decision criterion (equal area) to ensure baseline equivalence before constructing decoy variations. Decoy stimuli (D) were subsequently derived from these core shapes by systematically adjusting one or both dimensions. In each trial, either a target-decoy pair or a competitor-decoy pair was displayed.

2.2.2.1 Rectangle Stimulus

To generate the rectangle-based stimuli, we first created a set of 6 random rectangles (the "W" set, wide) by sampling from a bivariate normal distribution (mean height: 170 px, mean width: 250 px; standard deviation: 25 px for each attribute, no correlation), following Spektor et al. (2018). To create matched but vertically oriented rectangles (the "N" set, narrow), the width and height of the W set were swapped for each pair, preserving area but altering orientation. This resulted in 6 core pairs, each consisting of a wide (W) and a narrow (N) rectangle.

For each of the 6 core pairs, we randomly assigned one of three trial types (range, frequency, range-frequency) (Huber et al., 1982), with each trial type appearing twice across the 6 pairs. Following the triplet-triplet design by Wedell (1991), for every pair, two sets of triplets were generated: (1) with W as the target (T) and N as competitor (C), and (2) with N as the target and W as competitor. For each triplet, a decoy rectangle (D) was constructed based on trial type manipulation rules (by reducing width and/or height by 10 to 25 px relative to the target rectangle, according to the assigned trial type).

This procedure yielded 12 triplets (6 W-target, 6 N-target), each consisting of a T, C, and D rectangle. For each triplet, two experimental trials were constructed by presenting either the Target-Decoy (TD) pair or the Competitor-Decoy (CD) pair. Thus, the rectangle stimulus set resulted in a total of 24 experimental trials per participant.

Quantitatively, the resultant target-decoy distance—measured as the relative difference in *criterion values* considering both attributes—had a mean of 11.36% (SD = 2.17%) and ranged from 8.43%–14.71%. The distribution was approximately symmetrical without strong skewness.

2.2.2.2 Star Stimulus

A parallel procedure was followed for star-shaped stimuli. Each star-like shape was derived from a base rectangle, with four distinct sections removed. These sections consisted of two pairs of inward-facing isosceles triangles, where the bases of the triangles were equal to and touched the four sides of the rectangle, resulting in a star-like shape. Figure 3, panels A and B, display two sample trials.

The shape characteristics were determined by two key parameters: the base rectangle's width and the height of the removed triangles. Specifically, for the first out of the two sets of core stimuli, the mean rectangle width (μ_{w_1}) was set to 180 px, while the mean triangle height (μ_{d_1}) was set to 40 px. The respective variances were 30 px ($\sigma^2_{w_1}$) and 40 px ($\sigma^2_{d_1}$), with no correlation between the two.

Additionally, the height H_1 for the shape was determined by adding a random adjustment to the width. This adjustment was drawn from a normal distribution with a mean of 20 px and a standard deviation of 5 px. The value of H_2, representing the second height, was set equal to H_1.

The participants were instructed that the shapes represented objects drawn with sand, and their task was to identify which of the given shapes would require the least amount of extra colored sand to be extended into a perfect square. Of the two core shapes, one had a wider base rectangle and a larger removed triangle height, while the other had a narrower base rectangle and a proportionally smaller removed triangle height. For simplicity, we refer to the wider shape as "W" (wide) and the narrower shape as "N" (narrow).

To generate the stimuli, the W shapes were created first using the width and height distributions mentioned above. Then, ensuring that both W and N shapes required the same amount of extra area to form a perfect square, the N shapes were derived. As with rectangles, 6 core pairs of W/N stars formed the basis of the stimulus set. Each core pair was randomly assigned one of three trial types (range, frequency, range-frequency), such that all types appeared twice. For each pair, both possible target-competitor arrangements (W as target, N as target) were used. Corresponding decoy stars were created by manipulating the base rectangle's width (reduced by 8–15 px) and the height of the removed triangles (increased by 4–7 px) relative to the target, according to the assigned trial type. Thus, there were 12 triplets (6 W-target, 6 N-target), each producing two experimental trials (TD and CD). This resulted in 24 experimental star trials per participant.

The target-decoy distance—measured as the relative difference in *criterion values* considering both attributes—had a mean of 13.97% (SD = 2.14%) and ranged from 10.53% to 16.91%. The distribution was approximately symmetrical without strong skewness.

Additionally, 12 catch trials were included throughout the experiment. In these trials, participants always received the instruction: "Choose the shape that requires the least amount of extra coloured sand to make it a perfect square."—the same instruction as for star trials. Catch trials presented vertically oriented rectangles pairs designed so that one option clearly required less extra sand to complete into a square than the other, varying in only one attribute (width for rectangles).

To summarize, each participant completed 24 rectangle-based trials, 24 star-based trials, and 12 catch trials, for a total of 60 trials. For rectangle stimuli, the criterion value was defined as the area (width × height). For star stimuli, the criterion value was the area of additional 'sand' required to complete the shape into a perfect square, calculated using the geometric parameters of the base rectangle and the removed triangles. The OSF repository contains the exact stimulus set. In the *Supplementary Material section 1*, we present further details on the star stimuli creation with the full formula for criterion value matching.

Although the rectangle and star stimuli involved different surface attributes (rectangles: width and height; stars: base width and height of the removed triangular section) and corresponded to different judgment tasks (estimating overall area versus estimating sand required to complete a square), in both cases, stimulus variation was systematically governed by two orthogonal attributes. Importantly, decoy manipulations were constructed within the same underlying attribute framework, ensuring consistency in the experimental implementation of the attraction effect across the two stimulus types.

2.2.3 Design

The experimental conditions were defined by two independent variables: (1) Stimulus Type (rectangle vs. star), and (2) Comparison Pair (Target-Decoy [TD] vs. Competitor-Decoy [CD]). In a within-subjects design, each participant experienced all four combinations of these variables: (1) Rectangle, CD, (2) Rectangle, TD, (3) Star, CD, and (4) Star, TD.

The trials were presented using block randomization to ensure balanced exposure to all conditions. The experiment consisted of 12 blocks, each containing one trial per condition. Each condition was presented 12 times, resulting in 48 experimental trials. The Fisher-Yates algorithm was applied to randomize the order of conditions within each block, ensuring that the sequence of trials was unpredictable while maintaining balance. Additionally, 12 catch trials were included as exclusion criteria and were randomly interspersed throughout the experiment. The catch trials were distributed across the trial sequence using the same randomization method, ensuring a unique and unbiased presentation for all participants.

Each trial comprised a choice response phase followed by a rating task, with no time constraints imposed. The number of trials per participant was selected to balance robust data collection with participant comfort. Our design maximized data quality and engagement while minimizing fatigue, in light of the absence of imposed time constraints and the inclusion of additional rating tasks in each trial. Moreover, recent simulation-based evidence (Miller, 2024) demonstrated that increasing participant sample size typically contributes more to statistical power than a dramatic increase in trial numbers, especially when longer experimental durations risk inducing fatigue or noncompliance. Reduced trial load thereby supports better data reliability and sustained task engagement in within-subject perceptual and decision-making paradigms. Additionally, stimulus randomization and block balancing procedures ensured full counterbalancing of all relevant conditions, supporting robust statistical inference despite a modest number of trials per participant.

2.2.4 Procedure

At the beginning of each trial, a fixation cross was presented for 500 ms. This was immediately followed by the stimulus display, which remained on the screen until the participant responded via key press—there was no time limit imposed for the choice response. Upon making a selection, a rating task appeared, which similarly remained visible until a key press response was recorded. There was no time restriction for ratings. After each trial, the next fixation cross was presented for 500 ms before the onset of the following trial.

For rectangle trials, participants were instructed to select the rectangle with the largest area. For trials with star-like shapes, they were instructed to choose the shape requiring the least amount of additional colored sand to extend it into a perfect square. Participants selected the alternative in each trial using the left or right arrow keys. Following their decision, using number keys 1-7, they rated the difficulty of their choice on a 7-point Likert scale, where one represented "extremely easy" and seven represented "extremely difficult."

Before the main experiment, each participant completed a feedback-based practice session which involved only star stimuli. These practice trials ensured comprehension of the more complex stimulus evaluation instructions. No separate practice was required for the rectangular stimuli, as participants readily understood and performed this task during pilot testing. In the practice session, the participants were presented with 10 pairs of star shapes in random order. In five trials, the W stimulus was the expected answer, and in five trials, the N stimulus was the expected answer. Participants could click on each black shape to transform it into a perfect square, with the extra-filled portion highlighted in red. When a shape was clicked, a numerical value representing the extra sand required (e.g., 21,458 units) appeared below the shape in an arbitrary unit of measurement. The practice session was to ensure that participants understood the task instructions, especially for the star task.

2.3 Results

Out of a total of 67 participants, we excluded data from 6 participants because their performance was lower than 0.8 in the catch trials, where in each trial, there was clearly one best option out of the two. Additionally, we excluded a total of 119 individual trials (4.06%) that were either too fast ($<$100 ms) or too slow ($>$20,000 ms).

We performed repeated measures ANOVAs on accuracy, reaction time, and perceived difficulty ratings. The interaction effects of *Stimulus Type* (Star vs. Rectangle) $\times$ *Comparison Pair* (CD vs. TD) on all three were significant. ANOVA results are in Table 2. Interaction plots are shown in Figure 2. The main effects of the pair were significant for difficulty rating, accuracy, and RT. On average, the CD pair was rated as more difficult than the TD pair, and performance (both accuracy and RT) was better for the TD pair than the CD pair. Similarly, the main effects of stimulus type were also significant for accuracy and RT. Performance for rectangular stimuli (both RT and accuracy) was better compared to star stimuli. However, considering both the stimulus type and the comparison pair interaction, the pattern of results diverged between the stated difficulty and the revealed difficulty (as measured by accuracy), as well as RT.

In the post hoc analysis, the accuracy difference between the two pairs was significant for the star stimuli ($t(60) = 7.079$, $p < 0.001$, Cohen's $d = 1.016$, mean difference = 0.180, SD = 0.199). In contrast, the TD CD accuracy difference for rectangle stimuli, while still significant, was notably smaller ($t(60) = 5.092$, $p < 0.001$, Cohen's $d = 0.576$, mean difference = 0.085, SD = 0.130). Moreover, a pairwise t-test between the delta accuracies (TD–CD) for the two stimulus types (star vs. rectangle) revealed a significant difference, $t(60) = 3.132$, $p = 0.003$, Cohen's $d = 0.567$.

To ensure that the observed differences between stimulus types were not driven by variability in the distribution of %Target–Decoy distances (TD_dist%), we conducted a supplementary sensitivity analysis that included only trials where the TD_dist% ranges overlapped for both rectangular and star stimuli. The results of this analysis closely mirrored those derived from the full dataset, confirming that the reported effects reflect genuine stimulus-type differences rather than artifacts of distributional variation (see *Supplementary Material, Section 2*, for details). Additionally, robustness checks that varied participant exclusion thresholds and trial-level RT cutoffs (Supplementary Table S5) revealed that the observed pattern of results for accuracy remained stable across all tested criteria.

Table 2. Repeated measures ANOVA results for accuracy, reaction time, and difficulty rating. Each row reports the F value, p value, and partial eta squared (η_p^2) for the corresponding main effect or interaction.

Factor	$F(1, 60)$	p	η_p^2
Accuracy			
Stimulus type	11.06	.002	.16
Pair	75.82	$< .001$	.56
Stimulus type $\times$ Pair	9.81	.003	.14
Reaction Time			
Stimulus type	27.08	$< .001$	.31
Pair	9.62	.003	.14
Stimulus type $\times$ Pair	4.47	.039	.07
Difficulty Rating			
Stimulus type	2.50	.119	.04
Pair	22.82	$< .001$	.28
Stimulus type $\times$ Pair	8.77	.004	.13

2.4 Discussion

Using accuracy as a proxy for decoy's dominance, the results support our hypothesis regarding the asymmetry of dominance, at least in the pairwise comparisons. The strong main effect of pair in the accuracy results reveals a differential accessibility of dominance relations between TD and CD pairs. This likely arises because the Target and Decoy (T and D) are close in attribute space, affording easier ordinal comparisons—often directly along a single attribute dimension. In contrast, the Competitor and Decoy (C and D) are placed farther apart in the attribute space, such that each alternative tends to be superior on different attributes: one is stronger on one attribute, while the other excels on another. This configuration reduces the ease of establishing clear ordinal dominance in CD pairs, requiring attention to trade-offs or joint evaluation across attributes.

The interaction effect in the accuracy results suggests that the decoy's dominance asymmetry was strong, though not ideal, for star stimuli but less pronounced for rectangle stimuli. To rule out the possibility that these results were influenced by a baseline preference bias for either wide (W) or narrow (N) core options, we fit a linear mixed-effects model including target type and its interaction with pair type. The interaction was not significant (p = .54), confirming that the dominance asymmetry was robust across both W and N targets (see the Supplementary Material section 3 for full details).

Surprisingly, for self-reported difficulty and RT, there was a difference between CD and TD only with rectangular stimuli. It is not clear why the less accurate star stimuli do not show a significant difference in RT and perceived difficulty between the CD and TD pairs. One possible explanation is that the star stimuli, by virtue of their more complex and novel geometry, were generally perceived to be at a higher level of task difficulty that reduced participants' sensitivity to subtle pairwise differences. This increased overall challenge may have led to greater variance both within and across participants, thereby masking any RT or perceived difficulty effects that would otherwise mirror the strong asymmetry observed in accuracy. Alternatively, the star task may have encouraged a more deliberative or strategy-driven mode of processing, such that participants did not rely on "fast" ease-of-comparison cues to the same extent as with familiar rectangles. This difference in cognitive approach could dampen RT and subjective difficulty differences, even as underlying choice asymmetries remain robust. Future work could probe these possibilities by using eye-tracking or confidence ratings to directly compare processing dynamics across stimulus types.

Note that while the star stimuli produced robust asymmetric dominance, with participants reliably preferring the target over the decoy in TD pairs but not showing similar choice in CD pairs, rectangles also showed significant, though weaker, asymmetry. Thus, although stars provide a stronger diagnostic of dominance asymmetry, even rectangles cannot be considered entirely free of item-level asymmetry.

Having established that star stimuli yield a particularly clear dominance structure, we next asked whether this advantage would carry over to triplet choice sets. Experiment 2 therefore tested whether the newly

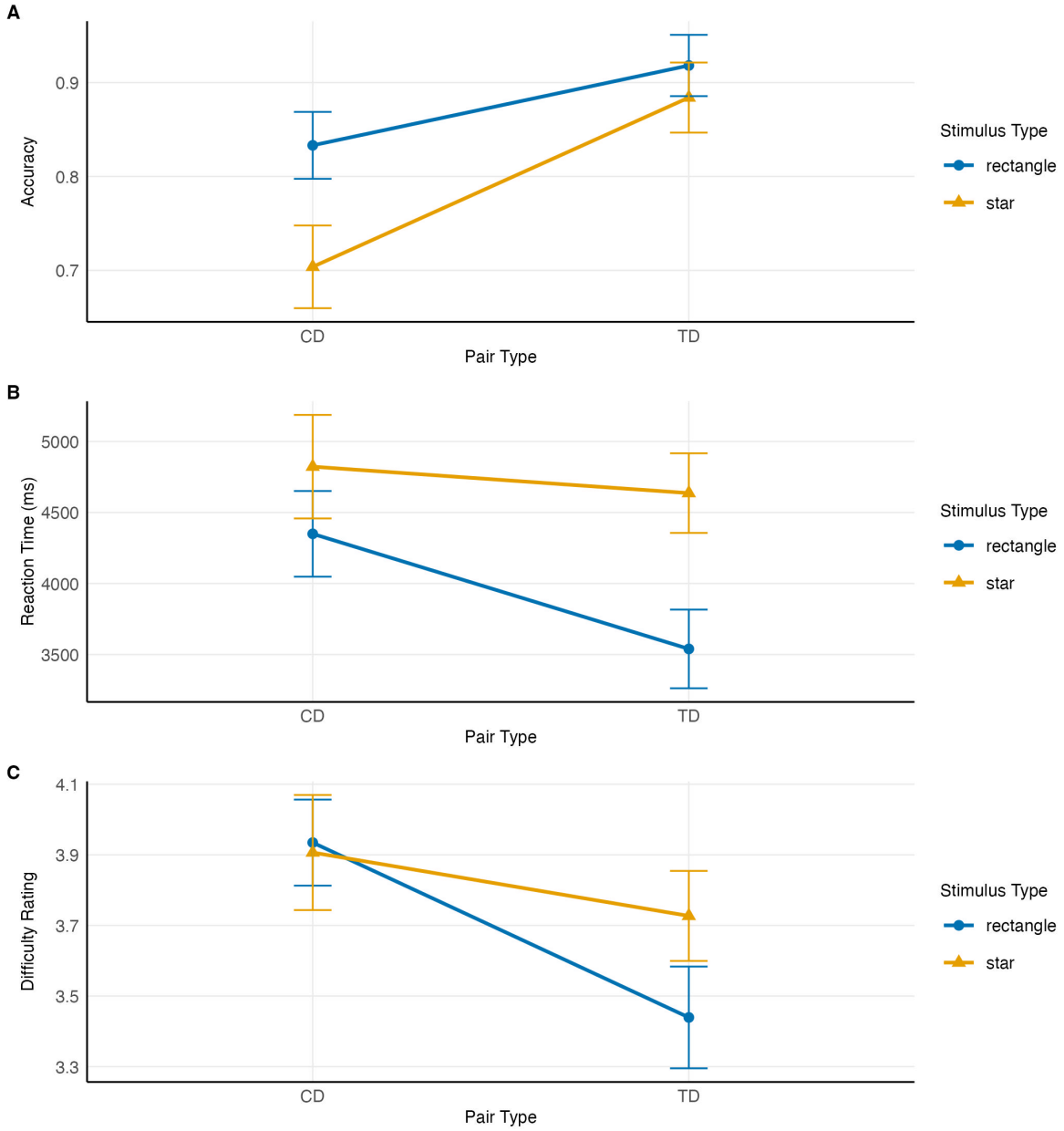

Figure 2. Interaction effects of stimulus type and pair on accuracy, reaction time, and difficulty. Error bars represent 95% within-subject confidence intervals.

developed star stimuli elicit a robust attraction effect in a triangular triplet arrangement, addressing prior reports of null or even reversed effects with perceptual stimuli.

3 EXPERIMENT 2

3.1 Introduction

Building on the results of Experiment 1, which showed that star stimuli produce strong asymmetric dominance in pairwise comparisons, we next asked whether this property would carry over to triplet choice sets. The assumption was that the underlying cognitive process producing asymmetric dominance in pairwise comparisons would also operate in the case of triplets and would lead to a positive attraction

effect. Experiment 2, therefore, tested whether the novel star-shaped stimuli generate such an effect in the standard triangular configuration used in decoy paradigms.

3.2 Method

3.2.1 Participants

Fifty-four students (12 female) with normal or corrected-to-normal vision (M: 20.17 years, SD: 3.08, range: 17-32), participated in the study.

3.2.2 Apparatus and Stimuli

The experiment was designed using JavaScript and conducted on similar laboratory computers. In each trial, the stimuli consisted of three different black-colored star shapes on a white background. The three stimuli (target, competitor, and decoy) were simultaneously displayed at the vertices of a virtual, upright equilateral triangle centered on the display, with their vertical positions jittered across trials. The base of the triangle was defined horizontally by points located at 30% and 70% of the total screen width, corresponding to pixel coordinates $x = 576$ and $x = 1344$ on a 1920-pixel-wide monitor—yielding a base length of 768 pixels. The height of the triangle was calculated as $h = (\sqrt{3}/2) * s$, where s is the base length, resulting in a height of approximately 665 pixels. The top vertex was located at the horizontal midpoint of the display ($x = 960$), vertically positioned 665 pixels above the center of the base line, i.e., at $y = -125$ relative to screen center ($y = 540$). The screen coordinates are defined relative to the screen center (0, 0), with positive y values extending downward.

At a fixed viewing distance of 60 cm on a 24-inch monitor (16:9), the triangle base corresponded to 212.5 mm (20.1° visual angle), and its height to 184.1 mm (17.4° visual angle). Thus, the left vertex was at $(576, , 540)$, the right at $(1344, , 540)$, and the top vertex at $(960, , -125)$, ensuring consistent and well-controlled spatial separation for all stimulus triplets across trials.

The star-like shapes were constructed following the method used in Experiment 1. Each star-like shape had the width of the base rectangle and the height of the removed triangles as its two attributes.

3.2.3 Design

Experiment 2 employed a forced-choice design in which participants made a selection among three simultaneously presented star-like shapes, constructed to differ systematically along two attribute dimensions: the width of the base rectangle and the height of the removed triangles.

In addition to the transition from pair to triplet comparisons, Experiment 2 incorporated a bias correction that distinguished it from Experiment 1. A pilot study revealed a systematic preference among participants for the wider (W) shape. To counteract this bias in the main experiment, the computed width of the narrower (N) shape—denoted as w_2—was increased by 10 pixels (See Supplementary Material Section 4). This adjustment aimed to equalize the perceptual appeal of the stimuli and ensure a more balanced choice distribution.

A total of 180 trials were administered to each participant, which took approximately 40 minutes to complete. Of these, 144 were main trials, equally divided between 72 trials with the wider star shape (W: wide) as the target and 72 trials with the narrower star shape (N: narrow) as the target. Additionally, 36 catch trials were included to assess participant-level exclusion criteria. Similar to Experiment 1, each participant completed a feedback-based practice session with 10 trials before the main experiment.

This experimental design was chosen to maximize control over stimulus presentation, enable robust counterbalancing of all conditions, and facilitate reliable statistical inference, while also accounting for participant fatigue and engagement.

3.2.4 Procedure

At the start of each trial, a fixation cross appeared at the center of the screen for 500 ms. Immediately thereafter, the three star-like stimuli were displayed in a triangular arrangement on a white background. The stimulus display remained visible until participants made a choice response using the arrow keys; there was no imposed time limit for this response. Upon selection, participants proceeded to a rating task, which

also remained visible until a response was made via key press, with no time restriction. After each trial, a new fixation cross appeared for 500 ms before the onset of the next trial.

During the practice session, participants were provided feedback following each selection to help familiarize them with the task. The main experiment did not include feedback. This study was not preregistered.

3.3 Results

Data were collected from 54 participants. First, 11 participants were excluded due to a technical error resulting in missing response time (RT) data for the majority of their trials; for these participants, only the first 10 trials contained RT data, while the remaining 170 trials were missing. This technical exclusion ensured data quality and left 43 participants for further analysis. Subsequently, one participant who failed to meet the predefined catch trial accuracy threshold of 0.8 was excluded, resulting in a final sample of 42 participants.

Within the retained sample, all catch trials were removed from further analysis. Individual trials were further excluded if their RT was less than 100 ms or exceeded 20000 ms, excluding 4.07% of main trials and yielding 7,252 valid trials for subsequent analysis.

We quantified context effects using the equal-weights version of the Relative Choice Share of the Target (RST), as defined by Katsimpokis et al. (2022), for a triplet-triplet design (Wedell, 1991). This measure captures how often the target is chosen over the competitor, where an RST of 0.5 denotes no context effect. Formally,

$$RST_{\text{EW}} = \frac{1}{2}\left(\frac{T_X}{T_X + C_X} + \frac{T_Y}{T_Y + C_Y}\right)$$

where T_X and C_X represent the target and competitor selections when the decoy favors option X, and T_Y and C_Y represent these counts when the decoy favors option Y. In this context, X and Y indicate the two core stimuli. Values above 0.5 indicate a bias toward the target. It should be noted that this metric that is widey used for the attraction effect—change in choice probabilities with decoy introduction—is itself an empirical measure of IIA violation, generally called the context effect.

A two-tailed one-sample t-test against 0.5 revealed that the mean RST was significantly above chance ($M = 0.537$, $SD = 0.050$); $t(41) = 4.815$, $p < 0.001$, Cohen's $d = 0.723$. Complementary Bayesian t-tests using the Jeffreys–Zellner–Siow prior (scale $r = 0.743$) provided strong evidence supporting the alternative hypothesis ($BF_{10} = 1035.9$), indicating that the observed effect is approximately 1036 times more likely under the presence of a positive context effect than under the null hypothesis.

To verify the robustness of these findings, we conducted extensive sensitivity analyses varying participant catch trial accuracy thresholds and trial-level RT exclusion criteria. The results of these analyses are reported in Supplementary Table S6, which shows that the positive context effect remains statistically significant and stable across all tested exclusion permutations. Figure 3 shows two example trials and the overall distribution of the choice share in the two contexts. Figure 4 depicts a corresponding violin plot for the overall RST values. Further descriptive statistics on choice frequencies and response time are reported in the Supplementary Material Section 7.

3.4 Discussion

To our knowledge, this study is the first to demonstrate the positive attraction effect for perceptual stimuli arranged in a triangle. Experiment 2 demonstrated that the newly introduced star stimuli elicit a reliable attraction effect in triplet choice sets. This finding provides direct support for the hypothesis that asymmetric dominance in pairwise comparisons is a necessary precondition for the AE: the same stimuli that produced strong asymmetry in dyadic judgments (Experiment 1) also yielded robust attraction in the triplet format.

Together, Experiments 1 and 2 strengthen the case that perceptual attraction effects depend critically on the presence of clear item-level dominance relations. Having established this principle with novel star stimuli, we next turned to a long-standing puzzle in the literature: why rectangle stimuli, which show reliable

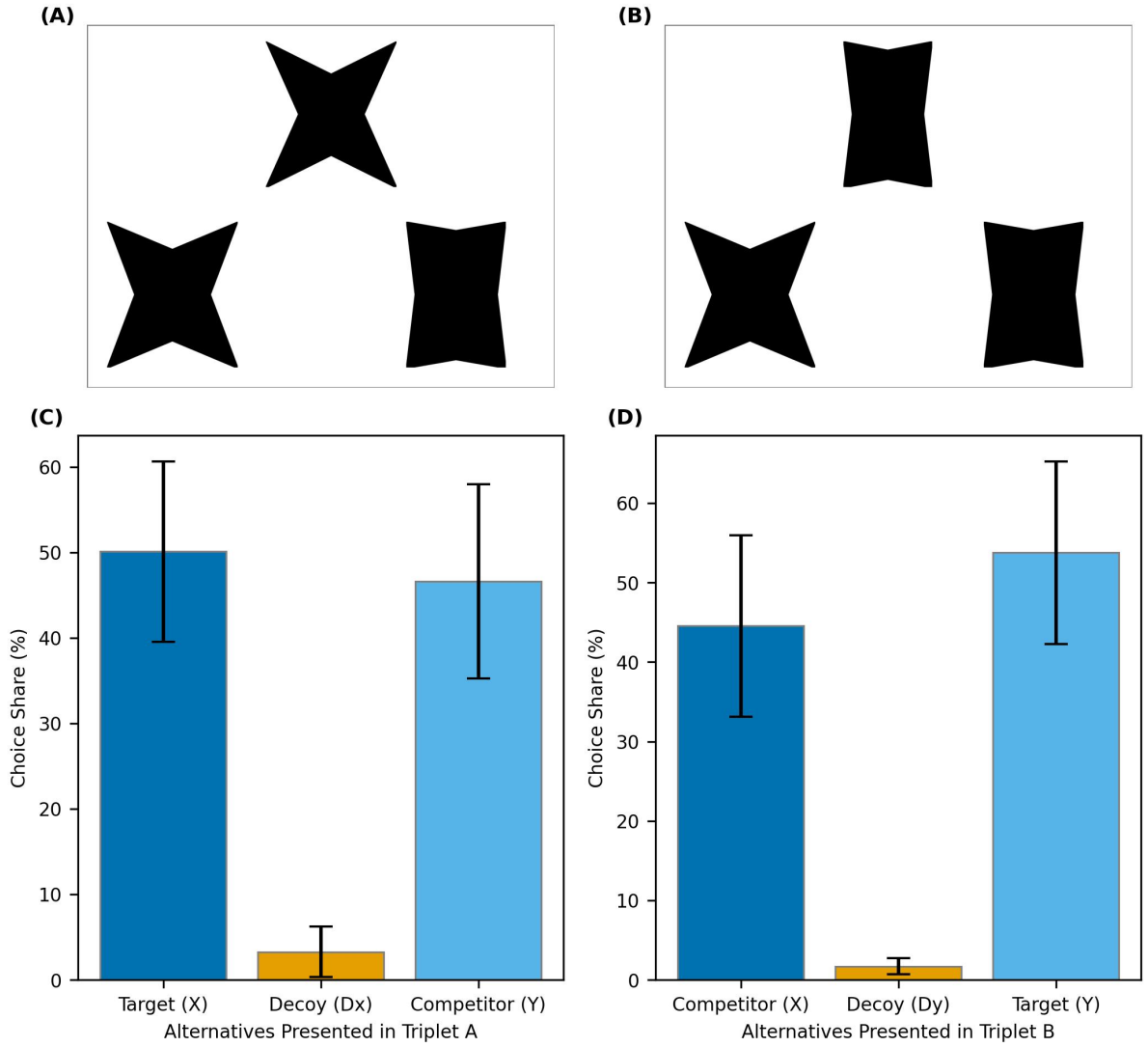

Figure 3. Example trials and choice shares in *Experiment 2*. (A) A trial where the wider stimulus serves as the target. (B) A trial where the narrower stimulus is the target. (C) Choice shares when X is the target and Dx (a decoy inferior to X) is presented. (D) Choice shares when Y is the target and Dy (a decoy inferior to Y) is presented. Error bars represent 95% within-subject confidence intervals.

AEs in linear arrangements, have sometimes produced reversed effects in triangular configurations. One possibility is that this puzzling outcome reflects the weaker asymmetry in decoy's dominance we observed for rectangles in pairwise tasks. Experiment 3, therefore, tested whether the apparent reversal would persist under replication, or whether it would instead resolve into a reduced—but still positive—attraction effect.

4 EXPERIMENT 3

4.1 Introduction

Experiment 3 addressed the puzzling finding that rectangle stimuli sometimes yield a reversed (negative) attraction effect in triplet choice sets (Spektor et al., 2018). Given that Experiment 1 revealed only weak asymmetric dominance for rectangles in pairwise comparisons, we predicted that in triplet set, the same stimuli should not generate a true repulsion effect but rather a reduced or null attraction effect. To test this, we ran a conceptual replication of the triangular triplet design of Spektor et al. (2018).

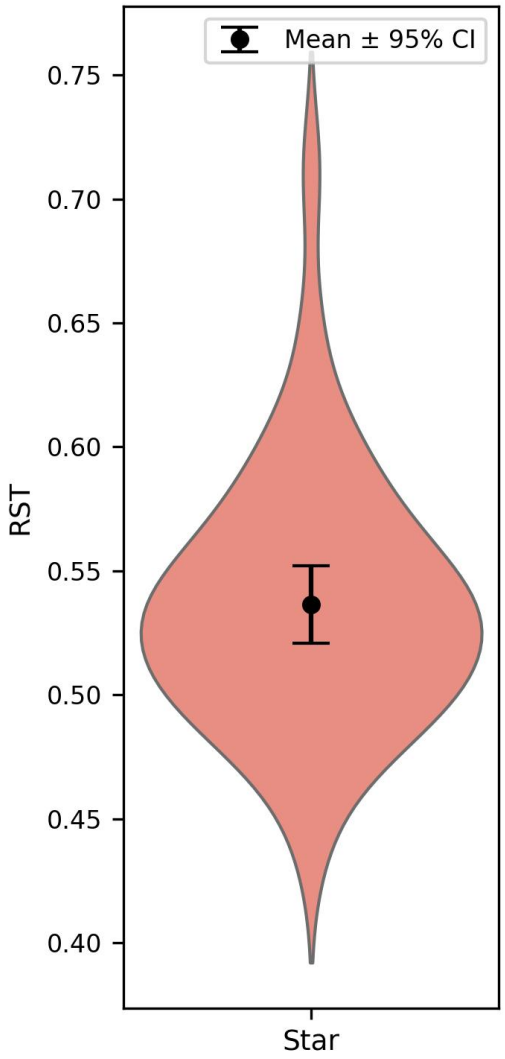

Figure 4. Relative share of the target in Experiment 3. The dot represents the mean, and the error bars indicate 95% confidence intervals based on between-subject variability.

In parallel, we revisited the study by Spektor et al. (2022), which had reported a repulsion effect with bar stimuli. Their original analysis relied on RST scores collapsed across multiple within-subject manipulations, raising the possibility that the observed effect reflected the interaction of these factors rather than the decoy's role itself. To address this, we reanalyzed their data at the trial level, isolating the decoy manipulation.

The motivation for including both the direct replication and the reanalysis was two-fold. First, both Spektor et al. (2018) and Spektor et al. (2022) reported negative attraction effects using perceptual stimuli. We aimed to examine whether multi-attribute perceptual stimuli would produce results consistent with our theoretical framework—that is, whether weak or absent asymmetric dominance would lead to null rather than negative attraction effects. Second, the dataset from Spektor et al. (2022) is both publicly available and atypical in its multi-factorial design, allowing us to test—within a model comparison framework—whether the negative effect reported originally persists when all design factors are explicitly accounted for.

Our interpretation regarding the absence of strong asymmetric dominance of the decoys in Spektor et al. (2022) is theoretically motivated, based on their described stimulus construction, but was not directly tested within their dataset. Based on this reasoning, we anticipated that both the replication (Experiment 3) and the subsequent reanalysis would yield null rather than negative effects. Under our framework, when

dominance asymmetry is not robustly established, we expected an absence of a context effect (i.e., a true null) rather than a reversal.

4.2 Method

4.2.1 Participants

Seventy-six volunteers (19 female) with normal or corrected-to-normal vision (M: 20.93 years, SD: 2.56, range: 17-28), participated in the experiment.

4.2.2 Apparatus and Stimuli

This experiment was designed using JavaScript and conducted on the same laboratory computers as in previous experiments. Rectangle stimuli were created following the procedures described in Experiment 1, and the decoy construction method (including range, frequency, and range-frequency decoys) was consistent with Experiment 1. The spatial arrangement and display calibration for stimulus presentation were identical to Experiment 2; that is, on each trial, three stimuli were simultaneously displayed at the vertices of an upright, virtual equilateral triangle centered on the screen (see Section 3.2.2, Apparatus and Stimuli, Experiment 2, for full geometric and display details).

4.2.3 Design

Experiment 3 employed a forced-choice design in which participants made a selection among three simultaneously presented rectangles, each differing in area. The arrangement of rectangles and decoy generation methods (range, frequency, and range-frequency decoys) were matched to those used in Experiment 1 to enable direct comparison across experiments.

A total of 162 trials were presented to each participant. These trials included 144 main trials and 18 catch trials, ensuring all conditions were fully counterbalanced.

The trial count was determined to balance robust data quality with participant comfort and engagement. Randomization of trial presentation ensured that each participant experienced every condition, supporting reliable statistical inference.

4.2.4 Procedure

At the start of each trial, a fixation cross appeared at the center of the screen for 500 ms. Immediately thereafter, three rectangle stimuli were displayed in a triangular formation on a white background. Participants were instructed to select the rectangle with the largest area. The stimulus display remained on the screen until the participant made a choice response by pressing the corresponding key; there was no imposed time limit for this response. After selection, the next trial began following a 500 ms fixation interval.

Practice trials were presented at the beginning of the session, during which participants received feedback after each response to familiarize them with the task requirements. No feedback was provided during the main experiment. This study was not preregistered.

4.3 Results

Of the 76 participants tested, two were excluded due to a technical error that prevented complete reaction time (RT) logging. An additional six participants were excluded for performing below the predetermined accuracy threshold (i.e., less than 80%) on catch trials.

At the trial level, responses with implausible RTs were excluded. Specifically, trials with RTs below 100 ms or exceeding 20000 ms were removed. This procedure led to the exclusion of 126 trials, representing approximately 1.14% of the total data, and resulted in 10892 valid trials available for analysis.

We performed a two-tailed one-sample t-test on the overall RST. We were unable to replicate the negative AE reported in previous studies; rather, we found that overall RST ($M = 0.500$, $SD = 0.053$) was not significantly different from the null (0.5); $t(67) = 0.029$, $p = 0.98$, Cohen's $d = 0.004$. To further quantify the evidence, we conducted a Bayesian one-sample t-test using the default Jeffreys–Zellner–Siow (JZS) prior with a scale parameter $r = 0.707$. The Bayes factor (BF_{10} = 0.13) indicated that the data were about seven times more consistent with the null hypothesis than with the alternative. Figure 5 shows the overall

distribution of the choice share in the two contexts, and Figure 6 depicts a corresponding violin plot for the overall RST values. Robustness checks paralleling those conducted in Experiment 2—varying both participant-level and trial-level exclusion thresholds—consistently supported the null hypothesis across all tested parameters (Supplementary Table S7), confirming that the lack of a context effect in Experiment 3 reflects a genuine absence rather than an artifact of data filtering. Further descriptive statistics on choice frequencies and response times are also reported in the Supplementary Material Section 7.

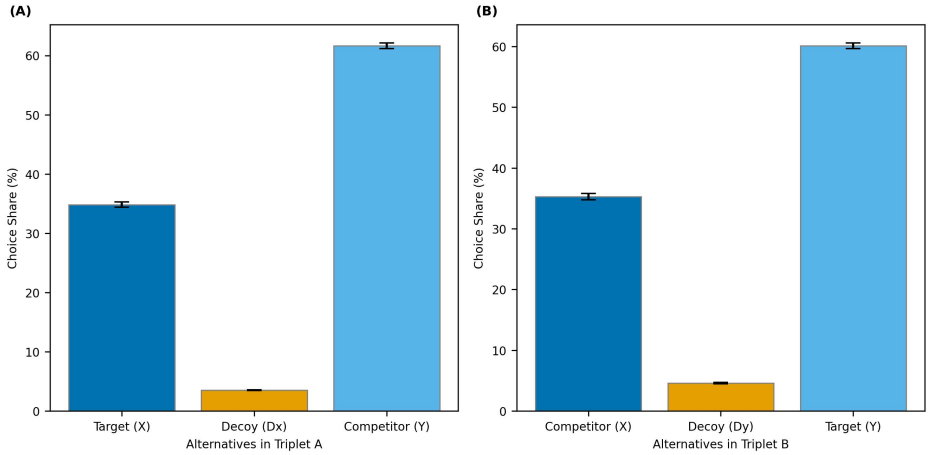

Figure 5. Choice shares in *Experiment 3* for the two triplet contexts. (A) Choice shares when X is the target and Dx (a decoy inferior to X) is presented in Triplet A. (B) Choice shares when Y is the target and Dy (a decoy inferior to Y) is presented in Triplet B. Error bars represent 95% within-subject confidence intervals calculated using the Cousineau-Morey method.

4.3.1 Re-analysis of Spektor et al. (2022)

Because bar stimuli have previously yielded reversed attraction effects, we conducted a re-analysis of the original data from Spektor et al. (2022). That study manipulated several design factors simultaneously, making it difficult to isolate the role of any single manipulation. We therefore compared a set of competing models, including a null model, a baseline additive model, and multiple interaction models. The best-fitting Generalized Linear Mixed-Effects Model (GLMM) yielded a non-significant estimate for the target variable (see Supplementary Material Section 4 for details). This pattern is most consistent with a null attraction effect.

Experiment 3 tested whether rectangles, which had previously been reported to elicit a negative attraction effect in triangular arrangements (Spektor et al., 2018), would reproduce this pattern. Instead, we observed a null effect: neither a reliable positive nor a negative attraction effect emerged. This finding suggests that the relationship between stimulus geometry and the attraction effect is not straightforward. Rectangles produced weak but significant dominance asymmetries in pairwise comparisons (Experiment 1) and have reliably supported the attraction effect in linear arrangements in prior work (Trueblood et al., 2013), yet in triangular displays they failed to produce the effect.

The re-analysis of Spektor et al. (2022) provided converging evidence. Although their original report suggested a repulsion effect, this was based on analyses collapsing across multiple within-subject manipulations. By isolating the decoy's contribution with a trial-level GLMM approach, we found no reliable attraction or repulsion effect. While we did not directly test whether the bar stimuli used in their study achieved item-wise dominance asymmetry, our interpretation of these results is theoretically grounded in the assumption—based on their stimulus construction—that such dominance asymmetry was likely weak or absent. Together, the replication and the re-analysis support the conclusion that when dominance asymmetry is not robustly established, the expected outcome is a true null effect, rather than a reversal of the attraction effect.

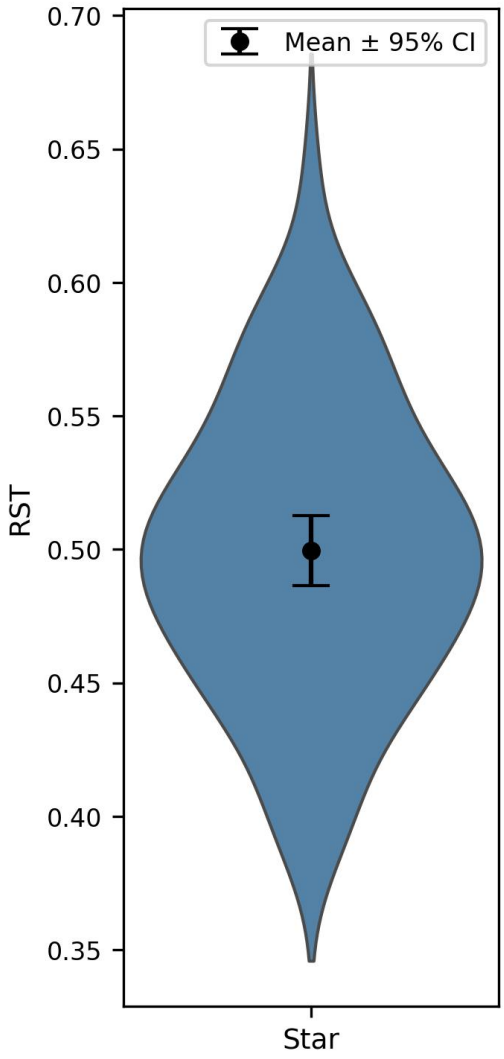

Figure 6. Relative share of the target in *Experiment 3*. The dot represents the mean, and the error bars indicate 95% confidence intervals based on between-subject variability.

4.4 Discussion

Taken together, the replication and reanalysis reinforce the conclusion that AE depends not only on the presence of asymmetric dominance but also on how spatial arrangements channel attention to target–decoy versus competitor–decoy relations. A linear layout may enhance the accessibility of these comparisons, whereas triangular arrangements may dilute or redistribute attentional salience across pairs.

Overall, Experiments 1–3 suggest that AE is not guaranteed by perceptual decoys alone: it requires the structural condition of item-based dominance asymmetry. This raises a critical next question: does spatial alignment of options interact with the underlying cognitive processes to produce dominance asymmetry? Experiment 4 was designed to test this possibility.

5 EXPERIMENT 4

5.1 Introduction

Across Experiments 1–2, our findings converged on the idea that the attraction effect (AE) critically depends on the decoy's dominance asymmetry. When decoys created strong asymmetry in pairwise comparisons, as with our novel star stimuli, we observed both clear pairwise asymmetries (Exp. 1) and

robust AE in triplet choices (Exp.2). Moreover, in Experiment 3, the null attraction effects—both in our reanalysis of bar stimuli and in the failed replication of rectangles in triangular displays (Spektor et al., 2018, 2022)—shift the interpretation away from accounts based on a repulsion effect and instead toward a boundary condition defined by the presence or absence of asymmetric dominance. Rectangles presented a more intriguing case: they produced significant asymmetry in pairwise comparisons (Exp. 1) and reliably elicited AE in linear triplet arrangements in prior work, yet failed to do so in triangular arrangements (Exp. 3). This discrepancy raises a key question: why should display layout matter at all?

It is important to note that Experiment 1 involved rectangle pairs presented in linear horizontal alignment. It is also noteworthy that in a triangular arrangement with equal representation of all six possible (T, C, D) configurations, each participant encounters one-third of trials where both TD and CD pairs are oblique, one-third where the TD pair is horizontal and the CD pair is oblique, and one-third where the CD pair is horizontal and the TD pair is oblique. This ensures that in triangular display, the attentional advantages are distributed across conditions, potentially diluting the overall dominance asymmetry. By contrast, linear triplets always contain horizontally aligned pairs, potentially amplifying dominance asymmetry in favor of the target.

One possibility, then, is that linear arrangements introduce systematic attentional biases. For instance, the central item always appears at fixation, and transitions between horizontally adjacent items may be easier than between outer items (Spektor et al., 2022). Moreover, matched orientations in a linear alignment may make target–decoy (TD) pairs consistently salient, regardless of their specific positions. This assumption is reasonable. Indeed, the Multi-attribute Linear Ballistic Accumulator (MLBA) model (Trueblood et al., 2014; Turner et al., 2018; Evans et al., 2019) makes a similar assumption to explain the same positive effect in the linear alignment of rectangle stimuli. According to the model, the relative salience of pairwise comparisons drives the AE: TD comparisons are weighted more strongly than competitor–decoy (CD) comparisons because of attribute similarity, which favors the target. More general frameworks (Trueblood et al., 2022) extend this idea by allowing attentional weights to be modulated by factors such as attribute bias or presentation format. Although these models can retrospectively attribute differences in AE to spatial modulation of attentional weights, existing explanations remain inferential, hinging on untested assumptions about attention allocation.

Psychophysical research strengthens this prediction. Across diverse paradigms, horizontally aligned stimuli enjoy processing advantages—faster reaction times, lower perceived difficulty, and stronger attentional capture—relative to oblique or vertical alignments (Chen et al., 2019; Tsal, 1989). Yet, choice-based studies of context effects have not directly tied these perceptual benefits to the underlying attentional weights that determine preference construction. Thus, a direct behavioral test of whether spatial alignment affects the ease of TD versus CD comparisons remains missing.

Experiment 4 addresses this gap by directly measuring the perceived difficulty of TD and CD comparisons under two alignment conditions: horizontal (linear) and oblique. By asking participants to rate difficulty in addition to making choices, we isolate attentional salience mechanisms and empirically test the key assumption that spatial alignment modulates attentional weights.

Unlike Experiment 1, where the main hypotheses concerned choice behavior—since dominance asymmetry was naturally defined in terms of accuracy—the present experiment focused on subjective difficulty ratings. In Experiment 4, we operationalized the attentional salience of each stimulus pair through the perceived difficulty of pairwise comparisons. This approach enabled us to quantify salience differences between Target-Decoy (TD) and Competitor-Decoy (CD) pairs in different alignments.

Hypotheses:

1. **Main effect of Pair Type:** TD comparisons will be rated as easier than CD comparisons.
2. **Main effect of Alignment:** Horizontal comparisons will be rated as easier than oblique comparisons, reflecting the horizontal attention advantage.
3. **Interaction:** The TD–CD ease advantage will be greatest when comparisons are horizontally aligned. This advantage should be attenuated when comparisons are presented obliquely; if spatial alignment systematically favors CD over TD, the TD–CD difference may be further reduced.

5.2 Method

5.2.1 Participants

Forty-two university students (9 female) with normal or corrected-to-normal vision (M: 21.45 years, SD: 2.86, range: 17-27), participated in the experiment.

5.2.2 Apparatus and Stimuli

Experiment 4 was conducted on a 24-inch, 1920 × 1080-pixel monitor (16:9) viewed at approximately 60 cm, with all display calibration, rectangle-based stimulus generation, and attribute matching procedures identical to those described in Experiment 1. Stimuli were presented as black rectangles on a white background.

The spatial configuration of each stimulus pair followed the virtual equilateral triangle arrangement detailed for Experiments 2 and 3 (see Apparatus and Stimuli, Experiments 2–3). On each trial, two rectangles were simultaneously displayed at two of the three vertices of the triangle (left-top, right-top, or left-right combinations). The vertex coordinates, triangle base and height, and corresponding visual angles were as previously specified: base at $x = 576$ and $x = 1344$ (768 pixels/212.5 mm/$20.1°$), top at ($x = 960$, $y = -125$), triangle height 665 pixels (184.1 mm/$17.4°$). This ensured that spatial separation, size, and visual angle were strictly comparable to those in earlier experiments.

5.2.3 Design

Experiment 4 employed a within-subjects forced-choice design in which participants selected the rectangle with the larger area from two simultaneously presented stimuli. Each trial consisted of either a Target–Decoy (TD) or a Competitor–Decoy (CD) pair, constructed from the corresponding triplets used in previous experiments.

For every combination of *Pair Type* (TD or CD) and *trial type* (range, frequency, range–frequency), four spatial presentation variants were used: *aligned0*, *aligned1*, *oblique0*, and *oblique1*. On each trial, only two of the three triangle vertices were occupied by rectangles. For CD pairs, aligned variants corresponded to the Competitor (C) and Decoy (D) occupying the left and right base vertices (aligned0: C left, D right; aligned1: D left, C right). Oblique variants corresponded to one item at a base vertex and the other at the top vertex (oblique0: C left, D top; oblique1: C right, D top). The same assignment logic was employed for TD pairs. For the final analysis, both aligned variants were combined into a single category, “aligned,” and both oblique variants were combined into “oblique.”

This design yielded 24 total experimental trials per participant, with presentation order fully randomized. Each trial also included a difficulty rating task following the choice response. The number of trials was chosen to balance comprehensive coverage of all spatial arrangements with participant comfort, given the inclusion of rating tasks and the precision required for spatial positioning judgments.

5.2.4 Procedure

At the start of each trial, a fixation cross appeared at the center of the screen for 500 ms. Immediately thereafter, two rectangle stimuli were displayed at the designated triangle vertices on a white background. The stimulus display remained visible until the participant made a choice response via key press; there was no imposed time limit for this response. Upon making their selection, participants proceeded to a rating task in which they rated the difficulty of their decision on a 7-point Likert scale, where one represented ”extremely easy” and seven represented ”extremely difficult.” The rating display also remained visible until a response was recorded via key press, with no time restriction. After each trial, a new fixation cross appeared for 500 ms before the onset of the next trial.

5.3 Results

Data from all forty-two participants were analyzed. We first applied the same duration exclusion criteria as preregistered in Experiment 1 (removing trials shorter than 100 ms or longer than 20 s), which resulted in the exclusion of 2.37% of trials.

A two-way repeated-measures ANOVA with *Pair Type* (TD vs. CD) and *Presentation Alignment* (aligned vs. oblique) as within-subject factors was conducted on participants’ difficulty ratings. The analysis revealed

Figure 7. Example Trials in Experiment 4. Stimuli pair are in oblique and horizontally aligned positions from left to right

a main effect of Pair Type, with TD comparisons rated as easier than CD comparisons, $F(1, 41) = 27.22$, $p < .001$, $\eta_p^2 = .40$. There was also a main effect of Presentation Alignment, with horizontal (aligned) comparisons rated as easier than oblique comparisons, $F(1, 41) = 29.19$, $p < .001$, $\eta_p^2 = .42$. Importantly, these effects were qualified by a significant interaction between Pair Type and Presentation Alignment, $F(1, 41) = 5.21$, $p = .028$, $\eta_p^2 = .11$ (see Figure 8). To verify that the observed interaction was not driven by specific exclusion criteria, we conducted a sensitivity analysis similar to those employed in previous experiments, varying participant accuracy thresholds and trial-level RT cutoffs. As summarized in Supplementary Table S8, all main and interaction effects for our primary variable, difficulty rating, remained significant and stable across all tested parameter combinations.

Descriptive statistics are summarized in Table 3. CD comparisons were rated as more difficult (aligned: $M = 4.09$, $SD = 1.05$; oblique: $M = 4.35$, $SD = 0.91$) than TD comparisons (aligned: $M = 3.38$, $SD = 0.96$; oblique: $M = 3.94$, $SD = 0.97$).

To probe the interaction, we conducted four planned pairwise comparisons with Holm–Bonferroni correction (Table 4). TD–CD differences were significant in both the aligned ($t(41) = -4.99$, $p_{\text{holm}} < .001$, $d_z = -0.77$) and oblique conditions ($t(41) = -3.87$, $p_{\text{holm}} < .001$, $d_z = -0.60$). The strongest difference was between TD-aligned and CD-oblique ($t(41) = -6.95$, $p_{\text{holm}} < .001$, $d_z = -1.07$). By contrast, CD-aligned and TD-oblique did not differ reliably ($t(41) = 1.24$, $p_{\text{holm}} = .222$, $d_z = 0.19$). This pattern is consistent with our hypotheses: the TD–CD asymmetry advantage was maximal under horizontal alignment and attenuated under oblique alignment.

Additionally, when accuracies were collapsed across presentation formats, no meaningful difference was observed between TD (M = 0.870, SD = 0.167) and CD (M = 0.850, SD = 0.145) conditions. A Wilcoxon signed-rank test indicated no significant difference in accuracy between TD and CD trials ($W = 286$, $p = .277$). A paired-samples t-test showed a consistent pattern, $t(41) = 1.16$, $p = .254$, Cohen's $d = 0.18$. Further descriptive and inferential statistics for accuracy and reaction times are reported in the Supplementary Material (Section S7, Table S12-S14) for completeness.

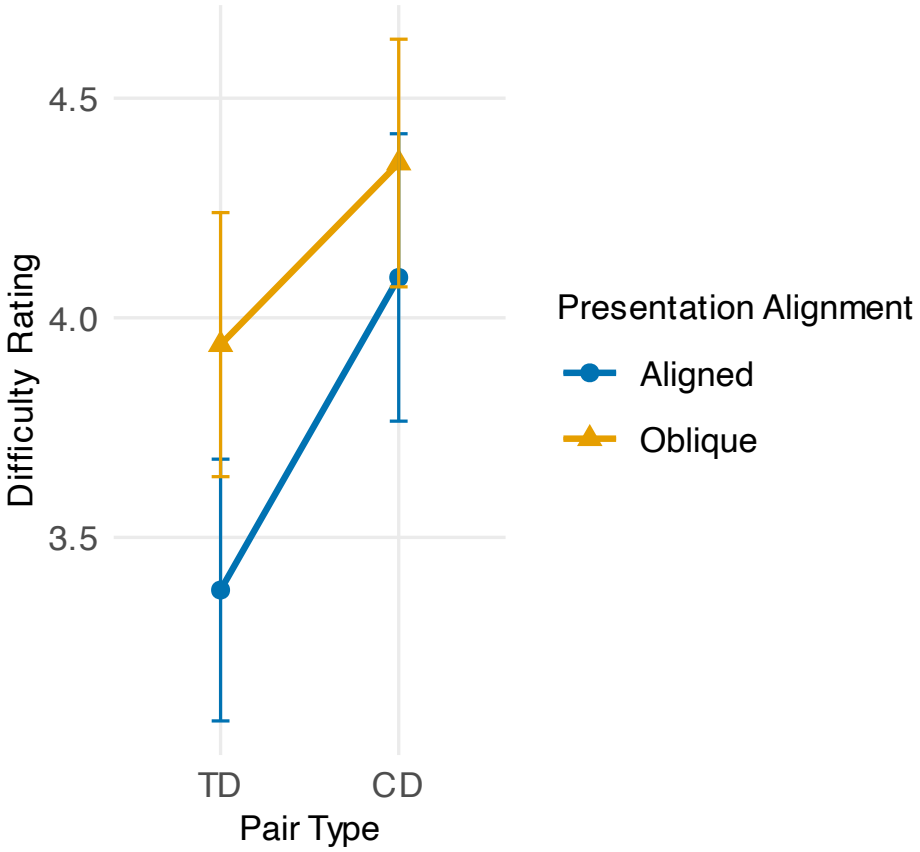

Figure 8. Mean difficulty ratings in Experiment 4 as a function of Pair Type (TD vs. CD) and Presentation Alignment (horizontal vs. oblique). Error bars indicate 95% confidence intervals of the mean (within-subject). The interaction reflects that the TD–CD ease advantage was more pronounced in the horizontal condition and attenuated in the oblique condition.

Table 3. Mean difficulty ratings (M) and standard deviations (SD) by condition.

Condition	M	SD
CD aligned	4.09	1.05
CD oblique	4.35	0.91
TD aligned	3.38	0.96
TD oblique	3.94	0.97

Table 4. Planned pairwise comparisons of difficulty ratings in Experiment 4. p-values are Holm–Bonferroni corrected.

Contrast	$t(41)$	p_{holm}	d_z	95% CI
TD aligned vs. CD aligned	-4.99	$< .001$	-0.77	[-1.11, -0.42]
TD oblique vs. CD oblique	-3.87	$< .001$	-0.60	[-0.92, -0.26]
TD aligned vs. CD oblique	-6.95	$< .001$	-1.07	[-1.45, -0.69]
CD aligned vs. TD oblique	1.24	.222	0.19	[-0.11, 0.50]

Discussion

Unlike Experiment 1, in Experiment 4 the rectangles clearly lacked an item-wise asymmetric dominance of decoys in terms of choice accuracies. Beyond accuracy, Experiment 4 evaluated attentional biases as indexed by subjective difficulty ratings, which capture participants' relative ease of processing pairs under different spatial arrangements. It provided direct evidence that the accessibility of asymmetric dominance

depends on spatial alignment. As predicted, TD comparisons were rated as easier than CD comparisons. We also observed a main effect of alignment, with horizontally aligned comparisons rated as easier than oblique ones, consistent with prior psychophysical work on horizontal processing advantages. Crucially, these two effects interacted: the TD–CD ease difference was strongest for horizontal pairs and attenuated for oblique pairs.

These findings help reconcile the puzzling discrepancy observed with rectangles. Rectangles, in Experiment 1, produced detectable pairwise asymmetry and produced robust AE in linear arrangements in prior studies, but they failed to yield AE in triangular displays (Experiment 3). Experiment 4 suggests a principled explanation: the weaker dominance asymmetry inherent to rectangles becomes accessible and behaviorally consequential only when supported by horizontal alignment. In triangular layouts, where TD and CD pairs are frequently oblique, this asymmetry is perceptually harder to access, undermining the conditions for AE to emerge. More broadly, our findings indicate that asymmetric dominance, as a condition, is also dependent on attentional weighing of pairwise comparisons.

6 GENERAL DISCUSSION

The present research set out to clarify the conditions under which the attraction effect (AE) emerges and to identify the mechanisms that drive it. By combining four experiments with reanalyses of prior work, we examined how item-based dominance asymmetries and presentation factors shape both pairwise judgments and multi-option choices. Our findings highlight the central role of asymmetric dominance as a boundary condition in producing AE, while also pointing to possible cognitive processes that manifest this boundary condition. In what follows, we summarize the main results, outline their theoretical implications, note methodological contributions, and discuss limitations and future directions.

6.1 Summary of Findings

Across four experiments, we systematically investigated when and why the attraction effect (AE) emerges. A consistent pattern was observed: AE is closely tied to asymmetric dominance in pairwise comparisons. When stimuli such as stars produced strong dominance asymmetry of decoys, we observed both reliable asymmetries in pairwise judgments (Experiment 1) and robust AE in triplet choices (Experiment 2). By contrast, a reanalysis of prior work with bar stimuli (Spektor et al., 2022) showed no AE in triplets. Rectangles presented a more nuanced case: they produced clear asymmetry in pairwise comparisons (Experiment 1) and elicited AE in linear triplets in past studies, but failed to show AE in triangular arrangements (Experiment 3). Experiment 4 further clarified this discrepancy. Item-wise asymmetric dominance of decoys disappeared when rectangle pairs were presented in both horizontally aligned and oblique arrangements. At the same time, Experiment 4 showed that participants perceived target–decoy (TD) comparisons as easier than competitor–decoy (CD) comparisons overall, and that horizontal arrangements selectively enhance this perceived ease advantage. Together, these findings offered a coherent explanation for the mixed results involving rectangles.

6.2 Theoretical Implications

Our findings have several important theoretical implications for understanding the attraction effect and decision-making processes more broadly. First, they provide strong support for the item-based definition of asymmetric dominance originally proposed by Huber et al. (1982) as a prerequisite for the attraction effect. While much of the subsequent literature has adopted an attribute-based definition focused on specific attribute comparisons, our results suggest that the original, more general item-based conceptualization better captures the conditions necessary for producing the attraction effect. This account explains why stars consistently yield robust AE, while other stimuli, when they lack systematic dominance asymmetry, fail to produce it. In this sense, AE is best understood as a consequence of structural asymmetries, not as a universal artifact of decoy presence.

Second, our results help resolve apparent contradictions in the literature regarding the domain generality of the attraction effect. The failure to observe positive attraction effects in perceptual tasks with triangular arrangements (Spektor et al., 2018, 2021, 2022) had called into question whether the effect generalizes beyond higher-level decision domains. By demonstrating robust AE with perceptually complex stimuli

(e.g., stars), our findings suggest that the attraction effect is indeed domain-general, but critically dependent on the presence of item-level dominance asymmetries.

Third, our item-based asymmetric dominance framework offers new insight into other established boundary conditions for the attraction effect. Previous work has demonstrated that attraction effects are attenuated or even abolished when attributes are commensurable—that is, when they can be directly compared on a common scale (Hayes et al., 2024; Banerjee et al., 2024b). Within our framework, commensurability likely facilitates holistic or integrated evaluation, wherein attributes are combined into a common currency or composite value before comparison. This process reduces the effective item-level dominance asymmetry of the decoy, as the decoy's attribute-wise advantage over the competitor may be offset by other attributes during integration. As a result, the key dominance-asymmetry boundary condition for robust attraction is weakened. Thus, our account suggests that the effects of commensurability arise primarily by suppressing dominance asymmetry, rather than via a separate cognitive process, unifying these phenomena within a single explanatory framework.

Furthermore, our dominance asymmetry framework offers a compelling explanation for the zero attraction effect observed by Trendl et al. (2021) using naturalistic movie stimuli. Their study represents a particularly instructive case because it rigorously satisfied all boundary conditions identified by Huber et al. (2014), yet still failed to demonstrate any attraction effect. Trendl et al. (2021) concluded that "the attraction effect is limited to choice tasks where options are represented in a stylized format with objectively defined attributes," but our findings suggest an alternative explanation grounded in the absence of genuine dominance asymmetry.

Crucially, while Trendl et al. (2021) carefully constructed their stimuli to ensure that decoys were clearly dominated—with decoys rated at least three points lower than both targets and competitors on seven-point scales—they failed to establish the asymmetric dominance relationships that our work identifies as essential. Their experimental design explicitly created target–decoy pairs with "many shared genres that are likely to be perceived as similar" while ensuring "target–competitor pairs with no genre overlap that are likely to be perceived as different." This design choice, while methodologically sound for establishing dominance, inadvertently created conditions for symmetric rather than asymmetric dominance.

Under our framework, the zero attraction effect in Trendl et al. (2021)'s study reflects not the inherent limitations of naturalistic stimuli, but rather the absence of the critical boundary condition we identify: genuine item-level dominance asymmetry. Their decoys were symmetrically dominated by both targets and competitors, violating the fundamental requirement that decoys must create stronger dominance relationships with targets than with competitors. The fact that participants correctly identified and avoided decoys (choosing them in only 4.3% of trials) confirms that dominance was perceived; however, the symmetric nature of this dominance eliminated the differential comparison advantage that drives attraction effects.

This interpretation suggests that naturalistic stimuli can theoretically support attraction effects when dominance asymmetry conditions are met. The challenge lies in constructing naturalistic choice sets where decoys are more clearly dominated by targets than by competitors—a task that may require careful attention to attribute structure and similarity relationships that go beyond traditional experimental design considerations. Trendl et al. (2021)'s findings thus provide strong convergent evidence for our theoretical framework: attraction effects emerge when decoys create asymmetric rather than merely symmetric dominance relationships, regardless of stimulus type or domain.

The case of rectangles further refined our theoretical understanding. While the presence or absence of dominance asymmetry in pairwise comparisons could explain the presence or absence of AE in rectangle triplets, at the process level, we found an interaction between the differential ease of comparison in specific item pairs (TD vs CD) and the presentation format. A speculative explanation is axis-congruence: because rectangles have width and height as defining attributes and are naturally encoded along orthogonal axes (in our case, horizontal and vertical axes), their evaluation may depend on whether these axes align with the spatial arrangement of options. When alignment is congruent (e.g., linear layouts), TD comparisons are facilitated, whereas incongruence (e.g., oblique triangular layouts) may reduce accessibility. This explanation is speculative and likely specific to rectangles or even bars, due to their attribute structures. Importantly, it remains an open question whether other visual perceptual stimuli would show similar susceptibility.

While we did not observe aggregate-level repulsion (reversed attraction) effects in any of our experiments, the existence of repulsion effects is robustly documented in the literature, either in the traditional regularity violation terms (Spektor et al., 2018, 2022; Banerjee et al., 2024a) or in broader operationalization—where instead of 3 alternative forced choice, a rank order of preference is recorded with decoys that are objectively superior and frequently chosen (Dumbalska et al., 2020). By carefully designed attribute structure and stimulus design, it may be possible to induce the negative attraction effect (still violating the regularity principle). Importantly, our theoretical framework extends to these phenomena: by incorporating attentional weights into the ordinal pairwise comparison model, the framework can account for how presentation format and context-dependent attention can shift the balance of subjective comparisons, thus explaining both standard attraction and its reversal—the repulsion effect—in certain choice contexts.

6.3 Methodological Contributions

This study makes several methodological contributions to the field of decision-making research. We introduced a novel perceptual stimulus—the star-shaped figure—that effectively creates conditions for asymmetric dominance. This stimulus design offers researchers a new tool for investigating the attraction effect in perceptual domains while maintaining the critical feature of dominance asymmetry. The effectiveness of this stimulus derives from its complex perceptual properties that require integrating multiple visual features, thereby creating conditions where dominance relationships between alternatives become more pronounced.

Our methodological approach also demonstrates the importance of verifying dominance asymmetry at the pairwise level before implementing decoy paradigms. The underlying assumption that the process responsible for dominance asymmetry structure in pairwise comparisons would also operate in the triplet set to favor the target was supported by our findings. The design in Experiment 1, directly comparing accuracy in TD versus CD pairs, provided a template for researchers to validate stimulus sets prior to testing AE in full triplet contexts. This validation step may be particularly valuable when working with perceptual stimuli where dominance relationships may be less intuitive than in value-based decision-making.

Additionally, by directly comparing different stimulus types (stars, bars, rectangles) within the same experimental paradigm, our experiments provide a controlled demonstration of how stimulus properties may influence dominance asymmetry and, in turn, the emergence of AE. This comparative approach could be extended to other types of stimuli to further map the boundary conditions of the effect.

Methodologically, our work also advances the literature by combining multiple behavioral measures: pairwise comparisons, triplet choices, and subjective difficulty ratings (Experiment 4). This triangulation allowed us to connect perceptual asymmetries observed in simple comparisons to the aggregate choice patterns observed in more complex sets, thereby providing converging evidence that AE is grounded in the psychological experience of comparative ease rather than being a statistical artifact of choice proportions.

Finally, we integrated new experimental evidence with reanalyses of existing datasets (e.g., Spektor et al. (2018)). This combination of replication, extension, and synthesis increases confidence in the robustness of our conclusions while also clarifying why prior findings sometimes appeared inconsistent. Specifically, our results show that apparent contradictions are better understood as reflecting stimulus- and arrangement-specific boundary conditions, rather than a fundamental instability of AE itself.

6.4 Limitations and Future Directions

Despite its contributions, this work has several limitations that suggest important avenues for future research. First, while our novel star stimuli successfully produced dominance asymmetry and a reliable attraction effect, the specific properties that make these stimuli effective remain underspecified. A stronger dominance asymmetry could itself be a result of attribute incommensurability (Walasek and Brown, 2023; Hayes et al., 2024). Future research could systematically vary stimulus properties to identify which features are critical for generating effective asymmetric dominance in perceptual tasks.

Second, our findings with rectangles illustrate that attraction effects are not uniformly robust across stimulus types or presentation formats. The weaker but significant asymmetry of dominated decoys in horizontally presented pairwise comparisons vanished when pairs were distributed in horizontally aligned and oblique arrangements—a result that directly highlights the moderating role of spatial configuration in producing dominance asymmetry. This susceptibility of dominance asymmetry to stimulus presentation

is consistent with evidence from prior work on the effect of presentation formats on AE, as reviewed by Spektor et al. (2021), tested by Cataldo and Cohen (2018, 2019); Evans et al. (2019), and more recently by Hasan et al. (2025). Whether this sensitivity of asymmetric dominance, and hence the attraction effect, to the presentation format generalizes to other perceptual stimuli beyond rectangles remains a fruitful direction for future research.

Third, although we inferred attentional weighting from choice and subjective difficulty ratings, we did not directly measure attentional allocation. Process-tracing approaches such as eye-tracking or mouse-tracking would provide more direct evidence about how participants allocate attention and engage in the comparison processes that drive the attraction effect. Previous research demonstrates that eye-tracking can illuminate the mechanisms underlying context effects in multiattribute choice, especially by revealing how attention is distributed between choice alternatives (Noguchi and Stewart, 2014; Marini et al., 2020; Molter et al., 2022; Cataldo and Cohen, 2019). For instance, Noguchi and Stewart (2014) and Marini et al. (2020) have shown that shifts in gaze patterns reflect comparative processes pivotal to the emergence (or suppression) of decoy effects, while Molter et al. (2022) demonstrate that gaze-dependent evidence accumulation predicts behavioral responses in multi-alternative choice. Incorporating such measures in future work would not only clarify the attentional dynamics involved in perceptual attraction effects but would also help to distinguish between competing process models.

Fourth, the present work focused on pairwise asymmetric-dominance boundary conditions and presentation layout manipulations, but other situational constraints may also play a role. Factors such as decision time pressure, cognitive load could interact with the process responsible for dominance asymmetry, shaping the emergence and strength of AE. Prior research has demonstrated that time constraints may reduce the incidence or magnitude of context effects, including the attraction effect, by limiting the opportunity for deliberation and comparative processing (Spektor et al., 2021; Pettibone, 2012). Similarly, increased cognitive load might disrupt the necessary information integration, potentially attenuating or altering context effects (see, e.g., Spektor et al. (2021)). Examining the roles of these factors and their interactions would further refine the boundary conditions identified in this study.

Fifth, we did not include dedicated pairwise Target-Competitor (T-C) trials in Experiment 1 to empirically verify the assumed equivalence between core alternatives. Although T and C were designed to be equal on respective criterion values, this equivalence was achieved by design rather than confirmed behaviorally. Prior attraction effect research typically establishes such equivalence by design to ensure that any shift in relative choice share arises from the inclusion of the decoy rather than pre-existing bias between the core options. Future studies should explicitly incorporate T-C pairwise comparisons to validate this assumption and strengthen interpretations regarding asymmetric dominance and the robustness of the attraction effect under different stimulus types.

Yet, another limitation of the present work is that our two stimulus sets—rectangles and star-shaped figures—differ not only in their perceptual appearance but also in the specific judgment task associated with each. For rectangles, participants identified the item with the greatest area, whereas for stars, they judged the item requiring the least sand to complete a square. These differences raise the possibility that task-related demands or stimulus complexity may modulate the observed attraction effect. However, both stimulus sets were carefully designed such that variation was restricted to two orthogonal attributes, and the decoy manipulations were defined within the same attribute-based framework. We therefore interpret the present findings as evidence that the principle demonstrated here is not exclusive to one stimulus type or task, while recognizing that a broader range of stimuli and tasks will need to be examined in future work to establish the generality of the claims.

Seventh, the generalizability of attraction effects to naturalistic decision contexts remains an important open question (Frederick et al., 2014; Huber et al., 2014; Trendl et al., 2021). Our framework suggests that the absence of asymmetric dominance—rather than stimulus type itself—may explain null effects in naturalistic contexts. Future research should systematically construct naturalistic choice sets in which genuine item-level dominance asymmetry is clearly established, to determine whether attraction effects can emerge in ecologically valid decision environments and to further refine boundary conditions across diverse stimulus domains.

Eighth, our present findings are restricted to visual perceptual choice with controlled dominance asymmetry, leaving open the question of whether the attraction effect generalizes to other sensory modalities such as auditory or tactile domains. To guide this endeavor, the computational and neurocognitive framework

articulated by Busemeyer et al. (2019) is especially relevant: their sequential sampling models propose that context effects such as the attraction effect arise from dynamic processes involving shifting attentional focus, lateral inhibition among competing options, and real-time integration of attribute comparisons—core computations implemented through a distributed network of prefrontal and parietal regions. This framework predicts not only where in the brain such effects may emerge, but how their magnitude and neural signatures will depend on stimulus structure and deliberation time, offering specific, testable hypotheses for new domains. Although this account suggests the possibility of domain-general mechanisms for context effects (Pisauro et al., 2017; Levy and Glimcher, 2012; Philiastides et al., 2011), recent findings demonstrate modality-specific effects in the timing and locus of irrelevant information processing (Li et al., 2024). Thus, future studies must carefully design behavioral and neuroimaging paradigms tailored to each sensory modality, to empirically test whether dominance asymmetry produces attraction effects and whether their neural basis aligns with Busemeyer et al.'s process-level predictions across different domains.

A related and broader theoretical issue concerns the interpretation of attraction effects within the neuroscience of decision-making. Historically, the attraction effect has been viewed as a violation of rational value computation by economic models of choice. However, recent critiques (Hayden and Niv, 2021) contend that regions such as orbitofrontal cortex encode structured comparative signals rather than single value representations, suggesting that so-called "irrational" context effects may instead be lawful outcomes of the way brains process multi-attribute information. Our dominance asymmetry framework provides a bridge between these perspectives, proposing that attraction effects and preference reversals reflect structural features of comparative processing rather than failures of value encoding per se. Neuroimaging studies that rigorously manipulate dominance asymmetry, coupled with process-tracing of comparative attention and attribute integration, will be necessary to establish whether attraction effects are best explained by competitive structural mechanisms or truly signal limits to rational economic computation.

Together, these limitations and future directions highlight how specifying effective stimulus properties, testing the generalizability of dominance asymmetry beyond traditional visual domains, incorporating richer process-tracing and neuroimaging measures, and broadening the scope across sensory modalities, tasks, and theoretical frameworks can advance our understanding of when and why the attraction effect emerges. By integrating behavioral, computational, and neural approaches, future research will be positioned to clarify whether the attraction effect reflects deep structural mechanisms of comparative decision-making or signals fundamental constraints on value-based rationality.

6.5 Conclusion

Across four experiments, we resolved apparent contradictions in the literature by demonstrating that the attraction effect reliably emerges in perceptual decision-making when proper conditions of item-wise asymmetric dominance are met. These findings refine theoretical accounts of AE by highlighting the central role of pairwise comparison processes, while also providing methodological guidance for how to design and validate effective stimuli. More broadly, they support a unified framework in which the attraction effect reflects fundamental properties of human information processing that generalize from consumer choices to perceptual judgments. By establishing clear boundary conditions, our work clarifies when AE should and should not be expected, offering a stronger foundation for future investigations of this robust but sometimes elusive decision bias.

CONFLICT OF INTEREST STATEMENT

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

ETHICS STATEMENT

All experiments were approved by the Institute Ethics Committee at Indian Institute of Technology Kanpur, India (IITK/IEC/2023-24/II/36). Participants provided informed consent before participation and received compensation for their time.

AUTHOR CONTRIBUTIONS

TR: Conceptualization, Data curation, Formal analysis, Methodology, Software, Validation, Visualization, Writing – original draft, Writing – review & editing.

NaS: Methodology, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing.

NiS: Data curation, Investigation, Methodology, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing.

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DATA AVAILABILITY STATEMENT

Stimulus files, raw data, and analysis code for Experiment 1, Experiment 2, Experiment 3, and Experiment 4 are made available online at OSF. Experiment 1 was preregistered, and the preregistration is available at OSF.

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Supplementary Material

1 CRITERION VALUE COMPUTATION FOR STIMULI

Rectangle Stimuli

For rectangle stimuli, the criterion value was defined as the area:

$$\textit{Criterion value}_{\text{rectangle}} = w \times H$$

where w denotes the width and H the height of the rectangle. Both attributes were systematically varied to create the required ranges of target–decoy distances.

Quantitatively, the target–decoy distance (TD_dist%) for rectangles had a mean of 11.12% (SD = 2.13%), ranging from 7.23% to 14.54%.

Star Stimuli

1.0.0.1 Construction and Geometry

Star-shaped stimuli were derived from a base rectangle of width w and height H, with four inward-facing isosceles triangles (each of height d) removed—two from the vertical sides (base H, height d), two from the horizontal sides (base w, height d). This produces a star-like silhouette (see Supplementary Figure S1).

1.0.0.2 Criterion Value Formula (Stepwise Derivation)

The criterion value for the star equals the extra area needed to fill the star shape into a perfect square (of side H):

1. **Area of the square:**
$$A_{\text{square}} = H^2$$
2. **Area of the base rectangle:**
$$A_{\text{rect}} = w \times H$$
3. **Total area removed by triangles:**
$$A_{\text{removed}} = 2\left(\frac{1}{2}Hd\right) + 2\left(\frac{1}{2}wd\right) = Hd + wd$$
4. **Area of the star:**
$$A_{\text{star}} = wH - (Hd + wd) = wH - Hd - wd$$
5. **Criterion value:**
$$\text{CV}_{\text{star}} = H^2 - (wH - Hd - wd) = H^2 - wH + Hd + wd$$

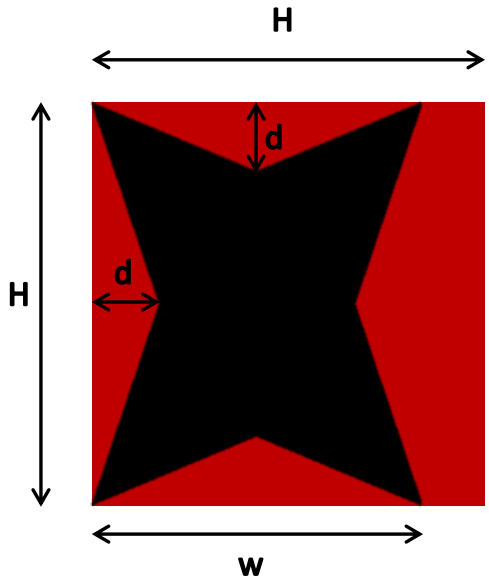

Figure S1. Example of a star-shaped stimulus derived from a rectangle (width w, height H) with four triangles (height d) removed. Figure is annotated to show w, H, d, and the extra area (criterion value) required to complete to a square. The filled-in square and removed triangles are depicted, with all parameters clearly labeled.

$$\boxed{\text{Criterion value}_{\text{star}} = H^2 - wH + Hd + wd}$$

Quantitatively, the target–decoy distance for stars (TD_dist%) had a mean of 13.97% (SD = 2.10%), ranging from 10.53% to 16.91%.

1.0.0.3 Practice Session: Comprehension of Criterion Value

A feedback-based practice session ensured participants understood the computation: participants could click star shapes to see them filled to a perfect square, with extra area highlighted and a numerical value displayed underneath. This provided intuitive feedback on the "extra sand" concept (criterion value). No special practice was needed for rectangles.

1.0.0.4 Comparability and Range

Both stimuli types are governed by two orthogonal, independently varied attributes. The stimulus file with all parameter values is available in the OSF repository.

2 SENSITIVITY ANALYSIS FOR EXPERIMENT 1

In attraction-effect paradigms, the *Target–Decoy distance percentage (TD_dist%)*—that is, the relative difference between the target and decoy on the relevant criterion dimension—should ideally have similar distributions across stimulus types to allow for a fair comparison of context effects. Matching TD_dist% ensures that any observed behavioral differences are not driven by differences in relative discriminability but instead reflect genuine psychological or perceptual effects.

In practice, however, perfect alignment of TD_dist% distributions is often difficult to achieve when stimuli differ geometrically or when parametric constraints interact nonlinearly, as in the case of the star stimuli. The nonlinear dependencies introduced by the star's criterion value make it infeasible to ensure an exact per-trial match with the rectangular stimuli without generating visually implausible or perceptually imbalanced shapes. Nevertheless, our goal was to create broadly comparable ranges across both stimulus types.

Upon closer inspection, we observed a modest but statistically significant difference in the TD_dist% distributions between rectangles ($M \approx 11.5\%$) and stars ($M \approx 14.0\%$) in our stimulus set (see Fig S2. This discrepancy arose from practical constraints in stimulus generation and was not intentional. Importantly, the chosen TD_dist% ranges are consistent with values commonly used in previous attraction-effect studies to manipulate discriminability (e.g., (Wedell, 1991; Farmer et al., 2017; Spektor et al., 2018))

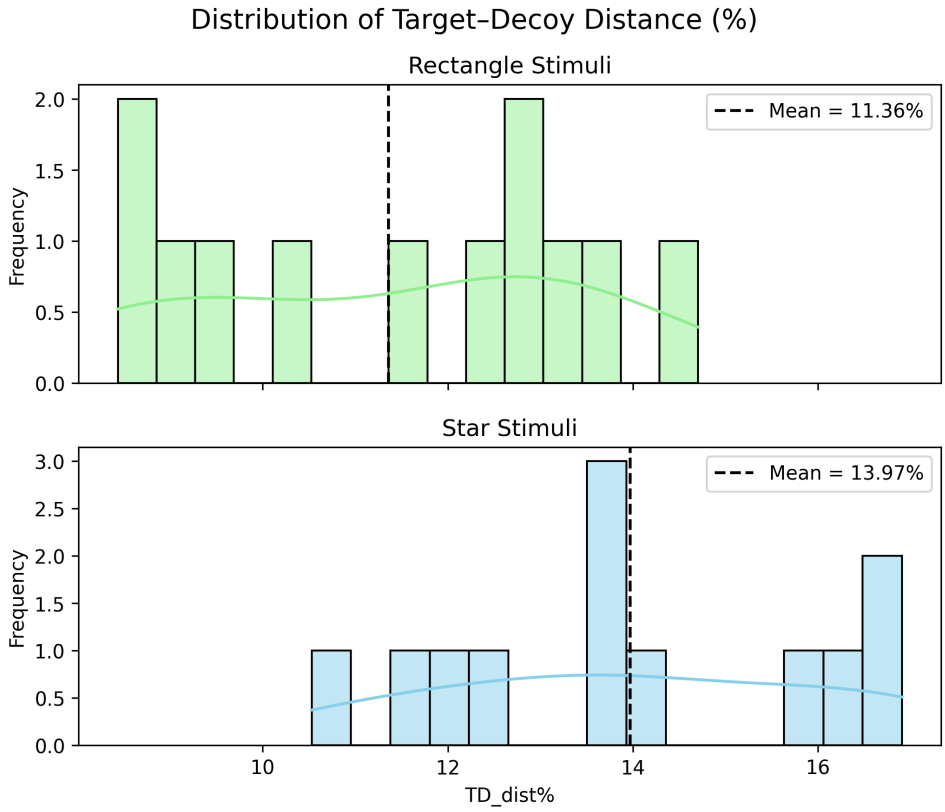

Figure S2. Distribution of target–decoy distance percentages ($\mathrm{TD}_{\mathrm{dist}}\%$) for all rectangle and star stimuli included in the experiment. The distributions reveal a modest but statistically significant difference in the average TD_dist% between rectangles ($M \approx 11.5\%$) and stars ($M \approx 14.0\%$).

To directly address potential concerns that this difference might have influenced the main results, we conducted a *sensitivity analysis* restricted to the subset of trials for which the TD_dist% ranges overlapped for both rectangles and stars, calculated separately for each participant. While the original dataset comprised 2,817 trials, the filtered subset included 2,451 trials (approximately 87% of the total).

A Wilcoxon signed-rank test comparing the TD_dist% distributions across stimulus types revealed no significant difference after filtering (W = 790.00, p = 0.26). Crucially, the main and interaction effects for accuracy, reaction time, and difficulty ratings all remained robust and statistically significant, with effect sizes and significance levels nearly identical to those obtained from the full dataset (see Supplementary Table S1 and Figure S3).

Table S1. Repeated measures ANOVA results for accuracy, reaction time, and difficulty rating using only trials in the overlapping $\mathrm{TD}_{\mathrm{dist}}\%$ range for both rectangles and stars (sensitivity analysis). Each row reports the F value, p value, and partial eta squared (η_p^2) for the corresponding main effect or interaction.

Factor	$F(1, 60)$	p	η_p^2
Accuracy			
Stimulus type	24.48	$< .001$	.29
Pair	79.39	$< .001$	.57
Stimulus type $\times$ Pair	7.73	.007	.11
Reaction Time			
Stimulus type	24.22	$< .001$	.29
Pair	8.07	.006	.12
Stimulus type $\times$ Pair	4.03	.049	.06
Difficulty Rating			
Stimulus type	3.70	.059	.06
Pair	19.27	$< .001$	.24
Stimulus type $\times$ Pair	6.96	.011	.10

Pairwise post hoc t-tests (sensitivity analysis): Using only trials in the overlapping $\mathrm{TD}_{\mathrm{dist}}\%$ range, the accuracy difference between pairs remained highly significant for the star stimuli ($t(60) = 7.13$, $p < .001$, Cohen's $d = 1.031$, mean difference = 0.181, SD = 0.199), mirroring the unfiltered analysis. For rectangle stimuli, the pairwise difference also remained robust and significant ($t(60) = 4.86$, $p < .001$, Cohen's $d = 0.659$, mean difference = 0.091, SD = 0.146).

These findings confirm that the reported effects are not artifacts of differences in TD_dist% distributions but instead reflect genuine underlying variations in attentional and perceptual processing between the two stimulus types.

3 BASELINE PREFERENCE CHECK IN EXPERIMENT 1

To ensure that the observed dominance asymmetry in Experiment 1 was not confounded by a baseline preference for either the wide (W) or narrow (N) core option, we conducted a generalized linear mixed-effects model (GLMM) analysis. The model predicted trial-level accuracy (correct/incorrect) with fixed effects of pair type (target-decoy [TD] vs. competitor-decoy [CD]), target type (W vs. N), their interaction, and stimulus type (rectangle vs. star). A random intercept for participant was included to account for repeated measures.

3.1 Model specification

The GLMM was fitted using the binomial family with a logit link. The formula in `R` notation was:

```
correct ~ pair * target_type + stimulus_type + (1 | userId)
```

where `correct` is a binary variable (1 = correct, 0 = incorrect), `pair` is TD or CD, `target_type` is W or N, `stimulus_type` is rectangle or star, and `userId` is the participant identifier.

The analysis included 2,809 trials from 61 participants. The random effect variance for participant intercepts was 0.45 (SD = 0.67). Model fit indices were AIC = 2,333.8 and BIC = 2,369.4.

3.2 Results

Table S2. Fixed effects estimates from the GLMM predicting accuracy in Experiment 1. Odds ratios (OR) are derived from the logistic model coefficients.

Predictor	Estimate	*SE*	*z*	*p*	OR
(Intercept)	1.67	0.14	11.56	$< .001$	5.31
Pair TD	1.15	0.16	7.10	$< .001$	3.15
Target type W	-0.01	0.13	-0.08	.94	0.99
Stimulus type star	-0.66	0.11	-6.06	$< .001$	0.52
Pair TD $\times$ Target type W	-0.14	0.22	-0.63	.53	0.87

Note. SE = standard error; OR = odds ratio.

As shown in Table S2, the main effect of pair type was significant, with higher accuracy for TD pairs than for CD pairs (odds ratio = 3.15). There was no significant main effect of target type (W vs. N; $p = .94$), indicating that participants did not exhibit an inherent bias toward either core option. The interaction between pair type and target type was also nonsignificant ($p = .53$), suggesting that the accuracy advantage for TD pairs was consistent regardless of target type.

These results confirm that the dominance asymmetry observed in Experiment 1 cannot be attributed to a baseline preference for either the wide or narrow core option. The effect of pair type on accuracy was robust and stable across target conditions, reinforcing the validity of the main experimental findings.

4 PILOT STUDY AND STIMULUS ADJUSTMENT PROCEDURE IN EXPERIMENT 2

We conducted a pilot study prior to Experiment 2, employing triplet sets typical of attraction effect designs (Target, Competitor, Decoy) with attributes systematically varied. The pilot revealed a moderate bias in favor of one core option (typically the wider star) in the final triplet choice.

In Experiment 1, our design focused exclusively on pairwise Target–Decoy (TD) and Competitor–Decoy (CD) comparisons, omitting dedicated Target–Competitor (T–C) trials. While core options were carefully designed to be equivalent on the relevant criterion dimension (see Section 6.4, Limitations and Future Directions, main text), formal behavioral verification of T–C equivalence was not performed. Although our GLMM analyses found no significant target type effects under pairwise conditions in Experiment 1, the pilot study suggested that triplet presentations might amplify even subtle preference tendencies, necessitating empirical calibration in Experiment 2.

To ensure that such a bias did not confound the measurement of the attraction effect or reduce the effect sizes, we implemented a targeted stimulus adjustment. Specifically, the width of the narrower star shape (w_2) was increased by 10 pixels for Experiment 2. This correction was calibrated to restore choice parity between core shapes within triplet sets, supporting the integrity of the subsequent analyses.

A graphical depiction of these adjustments is presented in Supplementary Figure S4. It illustrates the core star stimuli in 2D attribute space, visually depicting the 10-pixel increase in w_2 applied for Experiment 2. The figure provides a clear visual comparison of the core item configurations across experimental designs.

In summary, our pilot-based adjustments and rigorous baseline checks across designs strengthen the validity and interpretability of context-dependent choice effects, while also highlighting the importance of direct core alternative comparisons in future studies.

5 REPLICATION OF SPEKTOR ET AL. (2018) AND REANALYSIS OF SPEKTOR ET AL. (2022)

Rationale for Replication

In our study, the central thesis is that asymmetric dominance of the decoy is a necessary precondition for the emergence of the attraction effect in triplet choice sets. Given that our Experiment 1 revealed only weak asymmetric dominance of decoys for rectangles in pairwise comparisons, we predicted that such stimuli should not generate a robust repulsion effect, but rather a reduced or null attraction effect.

Interestingly, the same rectangular stimuli have been shown to produce both positive attraction effects—when arranged in a linear display layout (as in Trueblood et al. (2013) and Experiment 4a in Spektor et al. (2018))—and negative effects, as reported in Experiments 1, 2, 3, and 4b of Spektor et al. (2018) when presented in a triangular display layout. Moreover, other perceptual stimuli, such as bars, have also produced negative attraction (repulsion) effects in triangular layouts (Spektor et al., 2022).

Our analysis of both Spektor et al. (2018) and Spektor et al. (2022) revealed a methodological feature uncommon among attraction effect studies. Typically, such studies equate the two core alternatives on the decision-relevant dimension or criterion value (e.g., equal area, equal value-for-money), ensuring that any bias in choice behavior can be attributed solely to the presence of a decoy. In contrast, these two studies implemented a richer within-subject design with multiple, simultaneously manipulated factors—including correct option/larger, set type, difficulty, target, decoy type, and attribute distance. The stimuli consisted of either rectangle or bar pairs (wide-but-low and narrow-but-high), with participants asked to select the option with the highest total criterion (area or total filled length).

Crucially, the experimental designs in both studies manipulated the discriminability between core options (e.g., 3% versus 7% difference), a feature rarely included but theoretically important for understanding the strength of pre-decoy preferences.

Because the data from both studies are publicly available, it is feasible to reanalyse them to assess whether the originally reported negative attraction effects persist when all manipulations are explicitly modelled. However, given the likelihood that these manipulations interact with the underlying requirement of asymmetric dominance for the decoy, we chose instead to conduct a conceptual replication of Spektor et al. (2018)'s triangular triplet design using stimuli of comparable size.

Nonetheless, we did reanalyze the data from Spektor et al. (2022) and found that the previously reported reversed attraction effect is not robust, but rather better explained as null findings when multiple design factors are jointly modelled.

Model Specification for Reanalysis

We fitted a series of generalized linear mixed-effects models (GLMMs) using the lme4 package (Bates et al., 2015), based on data from 54,916 trials across 55 participants. The models predicted binary responses (NH = 1, WL = 0) and included random intercepts for participants. The primary predictor of interest was the *target* variable, which indicates which of the two core options (NH or WL) was designated as the target on a given trial. This variable captures the hypothesized influence of the decoy.

Candidate models included progressively more complex specifications, beginning with a null model (random intercepts only), an additive model including *larger*, *target*, *difficulty*, and *condition*, and extending to interaction models incorporating two-way and three-way interactions among these predictors. In total, fifteen models were estimated, labeled A through D3 in Table S3.

Model	**Formula**
A0	$\text{True\_response\_bin} \sim 1 + (1\|\text{subject\_id})$
A	$\text{True\_response\_bin} \sim \text{larger} + \text{target} + \text{difficulty} + \text{condition} + (1\|\text{subject\_id})$
B1	$\text{True\_response\_bin} \sim \text{larger} * \text{target} + \text{difficulty} + \text{condition} + (1\|\text{subject\_id})$
B2	$\text{True\_response\_bin} \sim \text{larger} * \text{difficulty} + \text{target} + \text{condition} + (1\|\text{subject\_id})$
B2_reduced	$\text{True\_response\_bin} \sim \text{larger} * \text{difficulty} + \text{target} + (1\|\text{subject\_id})$
B3	$\text{True\_response\_bin} \sim \text{larger} * \text{condition} + \text{target} + \text{difficulty} + (1\|\text{subject\_id})$
B4	$\text{True\_response\_bin} \sim \text{target} * \text{difficulty} + \text{larger} + \text{condition} + (1\|\text{subject\_id})$
B5	$\text{True\_response\_bin} \sim \text{target} * \text{condition} + \text{larger} + \text{difficulty} + (1\|\text{subject\_id})$
B6	$\text{True\_response\_bin} \sim \text{difficulty} * \text{condition} + \text{larger} + \text{target} + (1\|\text{subject\_id})$
C1	$\text{True\_response\_bin} \sim \text{larger} * \text{target} + \text{larger} * \text{difficulty} + \text{condition} + (1\|\text{subject\_id})$
C2	$\text{True\_response\_bin} \sim \text{larger} * \text{difficulty} + \text{condition} * \text{target} + (1\|\text{subject\_id})$
C3	$\text{True\_response\_bin} \sim \text{larger} * \text{condition} + \text{difficulty} * \text{target} + (1\|\text{subject\_id})$
D1	$\text{True\_response\_bin} \sim \text{larger} * \text{difficulty} * \text{condition} + \text{target} + (1\|\text{subject\_id})$
D2	$\text{True\_response\_bin} \sim \text{larger} * \text{target} * \text{condition} + \text{difficulty} + (1\|\text{subject\_id})$
D3	$\text{True\_response\_bin} \sim \text{larger} * \text{target} * \text{difficulty} + \text{condition} + (1\|\text{subject\_id})$

Table S3. Candidate GLMER models for binary response (`True_response_bin`), from null (A0) to additive (A) to interactions (B1–B6, C1–C3, D1–D3).

The best-fitting model (Model D1) can be expressed as:

$$\text{logit}\big(P(\text{NH response})\big) = \beta_0 + \beta_1\,\text{larger} + \beta_2\,\text{difficulty} + \beta_3\,\text{condition} + \beta_4\,\text{target} + \beta_{5-8}\,\text{interactions} + u_{\text{subject}}. \tag{S1}$$

Expanded form (on the logit scale):

$$\begin{aligned}\text{logit}\big(P(Y_{ij}=1)\big) &= \beta_0 + \beta_1\,\text{larger}_{ij} + \beta_2\,\text{difficulty}_{ij} + \beta_3\,\text{condition}_{ij} + \beta_4\,\text{target}_{ij} \\ &\quad + \beta_5\,(\text{larger} \times \text{difficulty})_{ij} + \beta_6\,(\text{larger} \times \text{condition})_{ij} + \beta_7\,(\text{difficulty} \times \text{condition})_{ij} \\ &\quad + \beta_8\,(\text{larger} \times \text{difficulty} \times \text{condition})_{ij} + u_{0j}.\end{aligned} \tag{S2}$$

Where:

- Y_{ij}: binary response for trial i from participant j, coded as 1 = NH chosen, 0 = WL chosen.

- $\text{logit}(p) = \ln\left(\frac{p}{1-p}\right)$: log-odds link function for binomial models.
- β_0: fixed intercept (baseline log-odds when predictors are at their reference levels).
- $\beta_1, \ldots, \beta_4$: main effects of *larger* (which option had greater fill), *difficulty* (3% vs. 7%), *condition* (baseline vs. preferential), and *target*.
- $\beta_5, \beta_6, \beta_7$: coefficients for two-way interactions.
- β_8: coefficient for the three-way interaction.
- $u_{0j} \sim \mathcal{N}(0, \sigma^2)$: subject-specific random intercept, capturing individual differences in baseline choice tendencies.

In `R` notation (using `lme4`; Bates et al. (2015)), the model was specified as:

```
True_response_bin ~ larger * difficulty * condition
                      + target + (1 | subject_id)
```

Here, `larger * difficulty * condition` expands into all main effects and their interactions up to the three-way term, `+ target` adds the fixed effect of the decoy-targeted option, and `(1 | subject_id)` specifies random intercepts per participant.

Model Comparison

Table S4 reports the AIC and BIC values for all fifteen models. Both information criteria converged in identifying Model D1 (highlighted in the table) as the preferred specification.

Table S4. Model comparison based on AIC and BIC values. The best-fitting model (D1) is highlighted.

Model	AIC	BIC
Null	74771.88	74789.70
A	61746.59	61800.07
B1	61747.69	61810.08
B2	61261.99	61324.38
B2_reduced	61278.17	61331.65
B3	61683.93	61746.32
B4	61746.17	61808.56
B5	61743.18	61805.57
B6	61748.57	61810.97
C1	61262.81	61334.12
C2	61258.78	61330.09
C3	61683.57	61754.88
D1	61187.11	61276.24
D2	61684.65	61773.78
D3	61266.01	61355.15

Results

In the best-fitting model, the fixed-effect estimate for *target* was small and not statistically significant ($\beta = 0.0335$, $p = 0.09$). By contrast, *larger*, *difficulty*, and their higher-order interactions produced reliable effects. These findings indicate that the *target* variable—central to the definition of the attraction effect—did not substantially influence choice once other design manipulations were accounted for.

This conclusion is illustrated in Figure S5, which displays the fixed-effect estimates from the best-fitting model (D1) with 95% confidence intervals. The plot shows reliable effects of *larger*, *difficulty*, and their

interactions, whereas the coefficient for *target* is small and its confidence interval overlaps zero, indicating no substantial influence of the decoy-designated option.

5.1 Conclusion

The re-analysis indicates that the reversed attraction effect reported by Spektor et al. (2022) is not robust once multiple design factors are simultaneously modeled. The effect of the *target* variable is best characterized as null, consistent with the absence of a reliable attraction—or repulsion—effect under bar stimuli.

6 ROBUSTNESS OF RESULTS ACROSS EXPERIMENTS

To assess the robustness of our findings, we systematically examined the effects of commonly used participant- and trial-level exclusion criteria on the primary measure of each experiment. Experiment 1 was pre-registered, providing a reference for exclusion thresholds, whereas other experiments were not. Thresholds were therefore selected based on Experiment 1 and previous literature, including the one replicated in Experiment 3 (Spektor et al., 2018).

Exclusion Criteria

Participant-level exclusion was based on catch trial accuracy. Two thresholds were considered: 0.8 (reflecting the pre-registered criterion in Experiment 1) and a more lenient 0.677, informed by prior literature (Spektor et al., 2018). Participants performing below the threshold were excluded.

Trial-level exclusion removed implausibly fast or slow responses. Trials with reaction times below 100 ms were excluded. Upper bounds included three alternatives: a fixed 20,000 ms threshold (pre-registered in Experiment 1), a fixed 8,000 ms threshold (used in prior literature), and a participant-specific threshold defined as the 75th percentile plus 1.75 times the interquartile range of each participant's RT distribution.

Robustness Analysis Approach

For each experiment and each combination of participant- and trial-level exclusion criteria, we recomputed the primary measure (relative share of target, RST, or other experiment-specific dependent variables). Frequentist analyses used one-sample t-tests against null values (0.5 for RST), with effect sizes calculated as Cohen's d. Bayesian analyses were conducted in R using the `BayesFactor` package to compute Bayes factors. Experiment 4 did not include catch trials.

Results

Across all four experiments, the direction and statistical significance of the primary effects were preserved under all exclusion criteria.

Tables S5–S8 provide detailed results for each experiment, including sample sizes, trials retained, mean and SD of primary measures, t-values, p-values, effect sizes, and Bayes factors. Each table follows a similar layout.

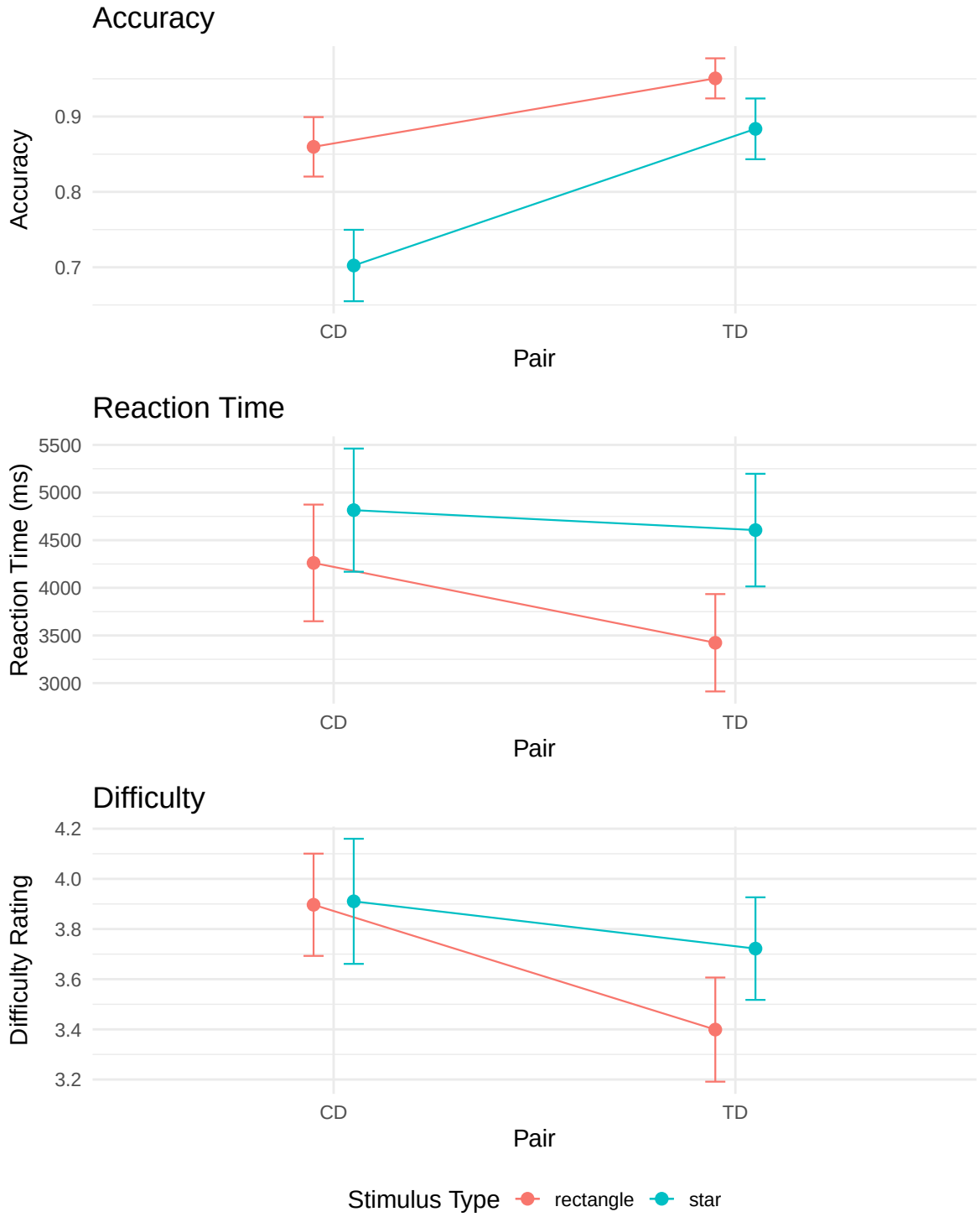

Figure S3. Interaction effects of stimulus type and pair on accuracy, reaction time, and difficulty rating, using only trials with matched overlapping $\mathrm{TD}_{\mathrm{dist}}\%$ ranges between rectangles and stars (sensitivity analysis). Error bars represent 95% within-subject confidence intervals.

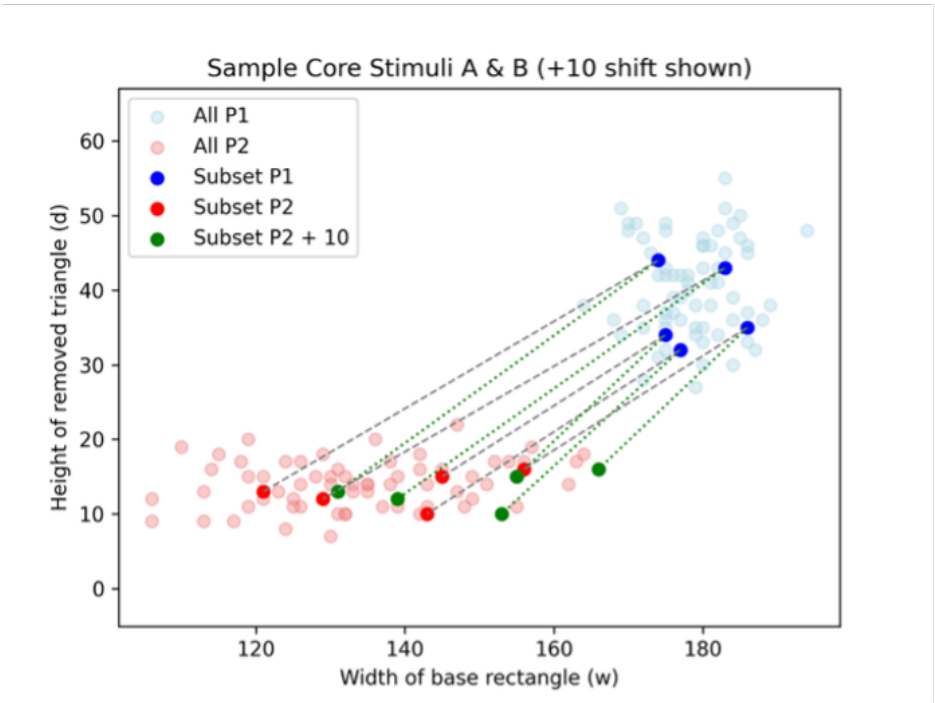

Figure S4. Core star items plotted in 2D attribute space. The adjustment of the narrower (w_2) item by 10 pixels for Experiment 2 is indicated, highlighting the bias correction relative to Experiment 1.

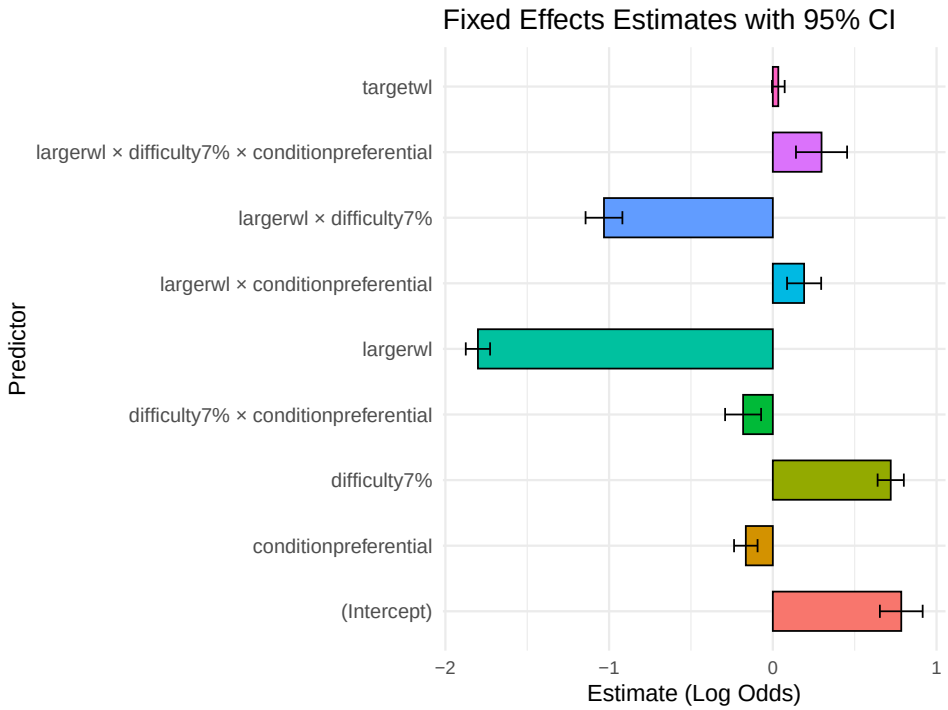

Figure S5. Estimated effects of each predictor on the probability of choosing the narrow–high (NH) option. Predictors are listed vertically; the horizontal axis shows the size and direction of each effect on the log-odds scale. Error bars are 95% confidence intervals. Effects to the right (positive) increase the chance of choosing NH; effects to the left (negative) decrease it.

Table S5. Robustness of Experiment 1 results across different participant- and trial-level exclusion criteria. **part_threshold:** participant-level catch accuracy threshold; **RT_Low / RT_Upper:** lower and upper bounds for reaction times (ms); **n_users / n_trials:** number of participants and trials before exclusion; **Trials removed%:** proportion of trials removed due to RT criteria; **effect / F / num_df / den_df / p_value / pes:** effect tested, F statistic, numerator and denominator degrees of freedom, p-value, and partial eta squared.

part_threshold	RT Low	RT Upper	n_users	n_trials	Trials removed%	effect	F	num_df	den_df	p	pes
0.8	100	20000	61	2809	0.041	stimulus_type	11.06	1	60	0.002	0.156
0.8	100	20000	61	2809	0.041	pair	75.82	1	60	< 0.001	0.558
0.8	100	20000	61	2809	0.041	stimulus_type:pair	9.81	1	60	0.003	0.140
0.8	100	15000	61	2745	0.063	stimulus_type	10.60	1	60	0.002	0.150
0.8	100	15000	61	2745	0.063	pair	64.35	1	60	< 0.001	0.517
0.8	100	15000	61	2745	0.063	stimulus_type:pair	9.15	1	60	0.004	0.132
0.8	100	10000	61	2567	0.123	stimulus_type	10.71	1	60	0.002	0.151
0.8	100	10000	61	2567	0.123	pair	56.12	1	60	< 0.001	0.483
0.8	100	10000	61	2567	0.123	stimulus_type:pair	9.44	1	60	0.003	0.136
0.8	100	8000	61	2421	0.173	stimulus_type	11.78	1	60	0.001	0.164
0.8	100	8000	61	2421	0.173	pair	51.52	1	60	< 0.001	0.462
0.8	100	8000	61	2421	0.173	stimulus_type:pair	11.41	1	60	0.001	0.160
0.677	100	20000	61	2809	0.041	stimulus_type	11.06	1	60	0.002	0.156
0.677	100	20000	61	2809	0.041	pair	75.82	1	60	< 0.001	0.558
0.677	100	20000	61	2809	0.041	stimulus_type:pair	9.81	1	60	0.003	0.140
0.677	100	15000	61	2745	0.063	stimulus_type	10.60	1	60	0.002	0.150
0.677	100	15000	61	2745	0.063	pair	64.35	1	60	< 0.001	0.517
0.677	100	15000	61	2745	0.063	stimulus_type:pair	9.15	1	60	0.004	0.132
0.677	100	10000	61	2567	0.123	stimulus_type	10.71	1	60	0.002	0.151
0.677	100	10000	61	2567	0.123	pair	56.12	1	60	< 0.001	0.483
0.677	100	10000	61	2567	0.123	stimulus_type:pair	9.44	1	60	0.003	0.136
0.677	100	8000	61	2421	0.173	stimulus_type	11.78	1	60	0.001	0.164
0.677	100	8000	61	2421	0.173	pair	51.52	1	60	< 0.001	0.462
0.677	100	8000	61	2421	0.173	stimulus_type:pair	11.41	1	60	0.001	0.160

Table S6. Robustness of Experiment 2 results across different participant- and trial-level exclusion criteria. **CatchAcc_Thres:** catch trial accuracy threshold for participant exclusion; **RT_Lower_ms / RT_Upper_ms:** lower and upper bounds for reaction times; **PSpec:** a participant-specific upper threshold, defined as the 75th percentile plus 1.75 times the interquartile range of that participant's RT distribution. **InitN / InitTrials:** initial participants and trials; **Part_N / Trials_after_Part:** after participant-level exclusion; **Final_N / Final_Trials:** after both exclusions; **Trials_Removed_%:** percentage of trials removed; **Mean_RST / Std_RST:** mean and SD of relative share of target; **t_stat / p_val / Cohen's d / BF10:** t-test statistics, p-values, effect size, and Bayes factor.

Condition	CatchAcc	RT Lower (ms)	RT Upper (ms)	Init N	Init Trials	Part N	Trials after Part	Final N	Final Trials	Trials Removed %	Mean RST	Std RST	t	p	Cohen's d	BF10
No Tech-Error Exclusion	0.80	100	20000	54	9720	52	9360	52	9048	3.33	0.54	0.05	5.73	<0.001	0.80	29268.82
No Tech-Error Exclusion	0.80	100	8000	54	9720	52	9360	52	8044	14.06	0.54	0.05	5.73	<0.001	0.79	28772.98
No Tech-Error Exclusion	0.80	100	PSpec	54	9720	52	9360	52	8832	5.64	0.54	0.05	5.60	<0.001	0.78	18659.41
No Tech-Error Exclusion	0.68	100	20000	54	9720	52	9360	52	9048	3.33	0.54	0.05	5.73	<0.001	0.80	29268.82
No Tech-Error Exclusion	0.68	100	8000	54	9720	52	9360	52	8044	14.06	0.54	0.05	5.73	<0.001	0.79	28772.98
No Tech-Error Exclusion	0.68	100	PSpec	54	9720	52	9360	52	8832	5.64	0.54	0.05	5.60	<0.001	0.78	18659.41
With Tech-Error Exclusion	0.80	100	20000	43	7740	42	7560	42	7252	4.07	0.54	0.05	4.82	<0.001	0.74	1035.96
With Tech-Error Exclusion	0.80	100	8000	43	7740	42	7560	42	6269	17.08	0.54	0.05	4.86	<0.001	0.75	1176.81
With Tech-Error Exclusion	0.80	100	PSpec	43	7740	42	7560	42	7038	6.91	0.54	0.05	4.68	<0.001	0.72	702.82
With Tech-Error Exclusion	0.68	100	20000	43	7740	42	7560	42	7252	4.07	0.54	0.05	4.82	<0.001	0.74	1035.96
With Tech-Error Exclusion	0.68	100	8000	43	7740	42	7560	42	6269	17.08	0.54	0.05	4.86	<0.001	0.75	1176.81
With Tech-Error Exclusion	0.68	100	PSpec	43	7740	42	7560	42	7038	6.91	0.54	0.05	4.68	<0.001	0.72	702.82

Table S7. Robustness of Experiment 3 results across different participant- and trial-level exclusion criteria. **CatchAcc_Thres:** catch trial accuracy threshold for participant exclusion; **RT_Lower_ms / RT_Upper_ms:** lower and upper bounds for reaction times (ms); **InitialN / InitialTrials:** number of participants and trials before exclusion; **Part_N / Trials_after_Part / Final_N / Final_Trials:** number of participants and trials after exclusions; **Trials_Removed_%:** percentage of trials removed; **Mean_RST / Std_RST:** mean and SD of relative share of target; **t_stat / p_val / Cohen's d / BF10:** t-test statistic, p-value, effect size, and Bayes factor.

Condition	CatchAcc	RT Lower (ms)	RT Upper (ms)	Initial N	Initial Trials	Part N	Trials after Part	Final N	Final Trials	Trials Removed %	Mean RST	Std RST	t	p	Cohen's d	BF10
No Tech-Error Exclusion	0.8	100	20000	76	12314	70	11342	70	11216	1.11	0.50	0.05	0.09	0.93	0.01	0.13
No Tech-Error Exclusion	0.8	100	8000	76	12314	70	11342	70	10889	3.99	0.50	0.05	0.01	0.99	0.00	0.13
No Tech-Error Exclusion	0.8	100	PSpec	76	12314	70	11342	70	10694	5.71	0.50	0.05	0.29	0.78	0.03	0.14
No Tech-Error Exclusion	0.677	100	20000	76	12314	72	11666	72	11537	1.11	0.50	0.05	0.41	0.68	0.05	0.14
No Tech-Error Exclusion	0.677	100	8000	76	12314	72	11666	72	11193	4.06	0.50	0.05	0.34	0.73	0.04	0.14
No Tech-Error Exclusion	0.677	100	PSpec	76	12314	72	11666	72	10998	5.73	0.50	0.05	0.58	0.56	0.07	0.15
With Tech-Error Exclusion	0.8	100	20000	74	11990	68	11018	68	10892	1.14	0.50	0.05	0.03	0.98	0.00	0.13
With Tech-Error Exclusion	0.8	100	8000	74	11990	68	11018	68	10566	4.10	0.50	0.05	-0.05	0.96	-0.01	0.13
With Tech-Error Exclusion	0.8	100	PSpec	74	11990	68	11018	68	10371	5.87	0.50	0.05	0.22	0.82	0.03	0.14
With Tech-Error Exclusion	0.677	100	20000	74	11990	70	11342	70	11213	1.14	0.50	0.05	0.36	0.72	0.04	0.14
With Tech-Error Exclusion	0.677	100	8000	74	11990	70	11342	70	10870	4.16	0.50	0.06	0.28	0.78	0.03	0.14
With Tech-Error Exclusion	0.677	100	PSpec	74	11990	70	11342	70	10675	5.88	0.50	0.06	0.52	0.60	0.06	0.15

Table S8. Robustness of Experiment 4 difficulty ratings across different trial-level RT exclusion criteria. **RT_Low / RT_Upper:** lower and upper bounds for reaction times (ms); **n_users_retained / n_users_removed:** participants included / excluded; **n_trials_retained / RemovedTrials%:** trials kept and percentage removed; **effect / F / num_df / den_df / p_value / pes:** effect tested, F statistic, numerator and denominator degrees of freedom, p-value, and partial eta squared.

RT Low	RT Upper	n_users retained	n_users removed	n_trials retained	RemovedTrials%	effect	F	num_df	den_df	p	pes
100	20000	42	0	989	2.37	pair	27.22	1	41	<0.001	0.399
100	20000	42	0	989	2.37	presentation_format	29.19	1	41	<0.001	0.416
100	20000	42	0	989	2.37	pair:presentation_format	5.21	1	41	0.028	0.113
100	15000	42	0	969	4.34	pair	26.44	1	41	<0.001	0.392
100	15000	42	0	969	4.34	presentation_format	28.77	1	41	<0.001	0.412
100	15000	42	0	969	4.34	pair:presentation_format	6.36	1	41	0.016	0.134
100	10000	42	0	929	8.29	pair	23.50	1	41	<0.001	0.364
100	10000	42	0	929	8.29	presentation_format	27.52	1	41	<0.001	0.402
100	10000	42	0	929	8.29	pair:presentation_format	7.39	1	41	0.010	0.153
100	8000	42	0	903	10.86	pair	23.12	1	41	<0.001	0.361
100	8000	42	0	903	10.86	presentation_format	27.75	1	41	<0.001	0.404
100	8000	42	0	903	10.86	pair:presentation_format	6.54	1	41	0.014	0.138

7 SUPPLEMENTARY RESULTS: DESCRIPTIVE AND SECONDARY ANALYSES ACROSS EXPERIMENTS

In this section, we report descriptive statistics and secondary inferential analyses for all experiments. All primary analyses are reported in the main text; here, we provide detailed participant-wise summaries and additional exploratory comparisons to support transparency and interpretability.

For each experiment, descriptive statistics include the mean, standard deviation (SD), median, interquartile range (IQR), and 95% confidence intervals (CI) for each dependent variable (accuracy, reaction time, and difficulty ratings), separately for each combination of experimental conditions (e.g., stimulus type $\times$ pair type). The number of participants (N) retained reflects exclusion of low-performing individuals (catch trial accuracy < 0.8) and trials outside the duration cutoffs ($<$100 ms or $>$20,000 ms). These statistics provide an overview of central tendency, variability, and precision of estimates across conditions.

Where applicable, secondary inferential analyses are also reported. These include repeated-measures ANOVAs and post-hoc paired comparisons, conducted to explore patterns not directly addressed in the primary analyses. Significant effects of experimental manipulations are summarized with standard APA-style statistics (F, df, p, partial η^2 or t, df, p, Cohen's d). These supplementary analyses are intended to provide additional context to the main findings and to allow readers to evaluate the robustness and consistency of the observed effects across experiments.

Tables S9–S12 present the descriptive statistics and, where relevant, secondary inferential results, in a structured format for easy reference.

Table S9. Descriptive statistics for Experiment 1 across stimulus and pair types. **Mean / SD / Median / IQR / 95% CI:** participant-wise mean, standard deviation, median, interquartile range, and 95% confidence interval of the dependent variable; **N:** number of participants included.

Stimulus	Pair	Mean Acc	SD Acc	Median Acc	IQR Acc	95% CI Acc	Mean RT	SD RT	Median RT	IQR RT	95% CI RT	Mean Diff	SD Diff	Median Diff	IQR Diff	95% CI Diff	N
rectangle	CD	0.83	0.16	0.91	0.19	[0.79, 0.87]	4353.23	2324.47	3820.75	2813.28	[3769.90, 4936.56]	3.94	0.80	4.00	1.00	[3.74, 4.14]	61
rectangle	TD	0.92	0.12	1.00	0.08	[0.89, 0.95]	3541.06	1883.37	2963.75	1552.67	[3068.42, 4013.69]	3.44	0.83	3.50	1.10	[3.23, 3.65]	61
star	CD	0.70	0.19	0.67	0.27	[0.66, 0.75]	4828.74	2517.94	4477.50	3710.03	[4196.86, 5460.62]	3.91	0.96	3.83	1.47	[3.67, 4.15]	61
star	TD	0.88	0.16	1.00	0.18	[0.84, 0.92]	4641.23	2374.49	4357.25	2887.25	[4045.35, 5237.12]	3.73	0.80	3.91	1.20	[3.53, 3.93]	61

Table S10. Descriptive statistics for Experiment 2 choice percentages and reaction times. Choice percentages are in %, RT is in milliseconds. **CI95:** 95% confidence interval of the mean.

Alternative	Mean	SD	Median	IQR	CI95
Target	51.78	29.46	53.85	48.61	[51.30, 52.26]
Competitor	45.49	30.32	41.18	51.47	[44.99, 45.98]
Decoy	2.73	6.28	0	2.78	[2.63, 2.84]
RT	3942.83	3836.59	2369	3505.5	[3854.53, 4031.13]

Table S11. Descriptive statistics for Experiment 3. Choice percentages are reported in %, reaction times (**RT**) in milliseconds.**CI95:** 95% confidence interval of the mean.

Alternative	Mean	SD	Median	IQR	CI95
Target (%)	47.62	49.95	0.0	100.0	[46.62, 48.61]
Competitor (%)	48.34	49.98	0.0	100.0	[47.35, 49.34]
Decoy (%)	4.04	19.69	0.0	0.0	[3.65, 4.43]
RT (ms)	2326.76	2273.20	1596.0	1567.0	[2281.46, 2372.05]

Table S12. Experiment 4: Participant-wise descriptive statistics for Accuracy, RT, and Difficulty rating. Accuracy is proportion, RT is in milliseconds, Difficulty rating is on its scale.

Pair	Presentation	Accuracy_Mean	Accuracy_SD	Accuracy_Median	Accuracy_IQR	Accuracy_CI95	RT_Mean	RT_SD	RT_Median	RT_IQR	RT_CI95	Difficulty_Mean	Difficulty_SD	Difficulty_Median	Difficulty_IQR	Difficulty_CI95
CD	aligned	0.85	0.19	0.92	0.19	[0.79, 0.91]	3817.84	2098.70	3290.08	2111.18	[3163.84, 4471.84]	4.09	1.05	4.17	1.17	[3.76, 4.42]
CD	oblique	0.85	0.17	0.83	0.17	[0.80, 0.90]	4240.15	2086.01	3913.83	3105.68	[3590.10, 4890.19]	4.35	0.90	4.42	1.00	[4.07, 4.63]
TD	aligned	0.89	0.21	1.00	0.17	[0.82, 0.95]	2966.74	1527.09	2488.58	1232.88	[2490.86, 3442.61]	3.38	0.96	3.27	1.17	[3.08, 3.68]
TD	oblique	0.85	0.18	0.83	0.17	[0.80, 0.91]	3736.91	1717.62	3627.02	2437.17	[3201.66, 4272.16]	3.94	0.96	3.92	1.29	[3.64, 4.24]

Table S13. Repeated-measures ANOVA results for Experiment 4. Accuracy and RT are reported side by side for each effect.

Effect	Accuracy			RT		
	F(df1, df2)	p	η_p^2	F(df1, df2)	p	η_p^2
pair	1.26(1, 41)	0.269	0.03	9.35(1, 41)	0.004	0.19
presentation	0.8(1, 41)	0.375	0.02	6.69(1, 41)	0.013	0.14
pair:presentation	0.48(1, 41)	0.492	0.01	0.76(1, 41)	0.387	0.02

Table S14. Planned pairwise post-hoc comparisons for Accuracy and RT in Experiment 4. p-values are Holm–Bonferroni corrected. Cohen's d_z and 95% CI are reported.

Contrast	DV	t	df	p_{raw}	p_{holm}	d_z	95% CI
TD aligned vs CD aligned	Accuracy	1.13	41	0.266	0.882	0.17	[-0.13, 0.48]
TD oblique vs CD oblique	Accuracy	0.21	41	0.833	1.000	0.03	[-0.27, 0.34]
TD aligned vs CD oblique	Accuracy	1.24	41	0.220	0.882	0.19	[-0.11, 0.50]
CD aligned vs TD oblique	Accuracy	0.06	41	0.956	1.000	0.01	[-0.29, 0.31]
TD aligned vs CD aligned	RT	-2.88	41	0.006	0.019	-0.44	[-0.76, -0.12]
TD oblique vs CD oblique	RT	-1.68	41	0.101	0.202	-0.26	[-0.56, 0.05]
TD aligned vs CD oblique	RT	-4.04	41	0.00023	0.001	-0.62	[-0.95, -0.29]
CD aligned vs TD oblique	RT	0.25	41	0.804	0.804	0.04	[-0.26, 0.34]

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