Consumers Undervalue Multi-Option Alternatives

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Initial choices often lead to downstream choices. Initial choices among restaurants lead to downstream choices among entrées, and initial choices among resorts lead to downstream choices among activities. How do the options available for downstream choices (e.g., entrées, activities) affect initial choices among multi-option alternatives (e.g., restaurants, resorts)? Forward-looking consumers should be able to identify their most-preferred downstream choice options (e.g., favorite entrée, favorite activity) and select an initial alternative accordingly. Less-preferred downstream choice options may therefore be disregarded. Yet 15 studies across four domains (three consumer goods and risky gambles) indicate this is not how consumers choose. Instead, adding a less-preferred option to an otherwise-attractive set of downstream choice options decreases the choice share of the corresponding multi-option alternative. The effect size increases as the difference in value between the more-preferred and less-preferred options increases. Mouse-tracking reveals that participants who attend more to a less-preferred downstream choice option (e.g., entrée, activity) are less likely to choose the corresponding multi-option alternative (e.g., restaurant, resort). A manipulation of attention to the downstream choice options provides causal evidence for this mechanism. This work enhances our understanding of multi-option decision-making and how decision-makers assess the overall value of choice sets.

Keywords: judgment and decision making, choice biases, mouse-tracking

Only rarely does a person's full decision process conclude at the moment of choice. Instead, each node in a decision tree typically leads to more decisions. Sometimes these are implicit: choosing a home implies choices among where to eat, where to shop, and who to visit. Other times these are explicit: choosing a restaurant implies choosing items from a menu and choosing to watch TV implies choosing a show.

Even this narrower case of explicit multi-option alternatives is ubiquitous. Multi-retailer gift cards constitute multi-option alternatives, since any part of the balance that is spent at a single retailer cannot be spent at another. Airline choices also constitute multi-option alternatives, as a given airline might have multiple routes available from origin A to destination B. Food and drink tickets, common at festivals and conferences, are typically multi-option alternatives, as are game tokens at arcades which can be used for a variety of games. A restaurant that offers more than one type of

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cuisine (e.g., a Japanese restaurant with sushi and ramen) is a multi-option alternative. Ultimately, many multi-attribute choices can be—and are—characterized as multi-option alternatives. For instance, purchasing a car includes multi-option alternative(s): a consumer might decide on a Ford Mustang, only to be faced with the choice of blue versus yellow. Each of the examples above is an example of a multi-option alternative, because even though a branch of the decision tree has been selected, there is still another choice to be made. That is, there are multiple sub-options within the alternative. Buying a multi-retailer gift card leaves open the opportunity to spend it at different retailers. Choosing an airline still requires choice of a flight, and buying tokens at an arcade still requires a choice of which game to play. Deciding to buy a Ford Mustang still requires a selection of color.

The examples above help to give the intuition for the structure of choices that we are interested in. Of course, with each of these examples, there might be many other considerations at play: whether other people are involved in the decision process, familiarity with the menu and options, decision history, complementarity versus substitutability of the sub-options, etc. In this article, we remove these extra considerations to better understand how people integrate across options in a choice set when deciding among multi-option alternatives.

In our studies, we examine how people make choices in situations with multi-option alternatives; specifically, we compare their behavior to a counterfactual situation in which the multi-option alternative only contains their most-preferred option. We find they regularly sacrifice a chance at maximizing utility by integrating the value of less-attractive options in a multi-option alternative. We demonstrate the robustness of this result across several consumer domains, with between- and within-subject designs. Moreover, we provide insight into the process by measuring and manipulating attention to the options. In doing so, we demonstrate that increased attention to an inferior sub-option decreases the likelihood of choosing a multi-option alternative.

Value-Maximizing Decision Rules in Multi-Option Choices

To a value-maximizing consumer, a multi-option alternative is worth at least as much as its most-valuable option, because they can ignore the less-valuable option, knowing that they will choose the more-valuable option instead. Formally, we consider the case in which an individual decides between a single option S and a multi-option alternative M_{HL}, where M_{HL} is the choice set $\{M_H, M_L\}$ (i.e., the choice between M_H and M_L in which M_H, "multi-high," is preferred to M_L, "multi-low"). If there is no uncertainty regarding the values of M_H or M_L, then the value of M_{HL} is $\max\{M_H, M_L\}$. Since M_H is preferred to M_L, $\max\{M_H, M_L\} = M_H$. Thus, the choice between S and M_{HL} simplifies to the choice between S and M_H. S should be no more likely to be chosen when pitted against M_{HL} than when pitted against M_H. If there are some states of the world in which M_L is preferred to M_H, S may be less likely to be chosen when pitted against M_{HL} than when pitted against M_H. (For example, a consumer may generally prefer hot coffee (M_H) to iced coffee (M_L) but make an exception when the temperature exceeds 90° F.[1]) In other words, adding M_L as an option to M_H should not decrease the probability of choosing M_{HL}.

Though the analysis above suggests that decision makers should value a multi-option alternative equivalently to the expected maximum of its constituent pieces, some findings from the behavioral literature suggest that people might value a multi-option alternative more than the maximum value of its component options because the multi-option alternative enables choice (Bown, Read, and Summers 2003; Brehm 1966; Mochon 2013; Shin and Ariely 2004). Because people often value the ability to choose, the added value from the mere presence of choice itself could lead to an overvaluation of multi-option alternatives.

Valuation of Sets

Alternatively, people might undervalue a multi-option alternative. If valuation processes for multi-option alternatives are similar to those for bundles of goods, then people would undervalue multi-option alternatives because bundles of goods are valued according to a weighted average of their components' values (Brough and Chernev 2012; Chernev and Gal 2010; Gaeth *et al.* 1991; Shenhav and Karmarkar 2019; Yadav 1994). There is related evidence that consumers estimate the value of a product to be a weighted average of its features (Troutman and Shanteau 1976; Weaver, Garcia, and Schwarz 2012), and adding a less-attractive bonus to a product decreases its value (Simonson, Carmon, and O'Curry 1994). These "weighted averages" are not mathematical averages of numerical properties, but rather central tendencies of subjective values. People can rapidly extract average economic value from a set of products (Yamanashi Leib *et al.* 2020) and sometimes use these assessments inappropriately (Kahneman and Frederick 2002; Kahneman 2003; Pink, Mullainathan, and Milkman 2026). These findings suggest that adding a less-desirable option to a multi-option alternative (in which one ultimately chooses a single option) decreases its value in the same way that adding a lower-valued

1 Throughout, we consider cases in which the state of the world is exogenously determined: neither the choice set nor the choice affects the preference ordering among S, M_H, and M_L.

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component to a bundle decreases the bundle's value (in which one ultimately receives the entire bundle): the worse the option, the greater the decrease.

A decrease in the likelihood of choosing the multi-option alternative would also be consistent with the choice implications of a repulsion effect, which has been most-commonly observed in perceptual (vs. preferential) tasks (Evans *et al.* 2021; Frederick, Lee, and Baskin 2014; Liao *et al.* 2021; Spektor, Kellen, and Hotaling 2018; Spektor, Kellen, and Klauer 2022). The repulsion effect represents a reversal of the attraction effect. In the repulsion effect, adding a dominated option can decrease the desirability of the dominating option. An established mechanism for the repulsion effect is "tainting," which would suggest that adding a lower-valued sub-option would decrease the perceived value of the higher-valued sub-option. We test for the presence of this mechanism in study 2 (and study 2b), and ultimately find it is insufficient to account for our findings.

Process Tracing

A possible mechanism contributing to shifts in valuation (and choices) is attention. In binary choices, people tend to choose the alternative they have looked at longer (Armel, Beaumel, and Rangel 2008; Atalay, Bodur, and Rasolofoarison 2012; Chandon et al. 2009; Fiedler and Glöckner 2012; Fisher 2021; Krajbich, Armel, and Rangel 2010; Lim, O'Doherty, and Rangel 2011; Mormann *et al.* 2012; Newell and Le Pelley 2018; Pachur *et al.* 2018; Pärnamets *et al.* 2015; Pieters and Warlop 1999; Vaidya and Fellows 2015). Moreover, people tend to update their values and choose in line with the choice attribute that they focus on (Bhui and Jiao, 2023; Busemeyer and Diederich 2002; Busemeyer and Townsend 1993; Cohen, Kang, and Leise 2017; Dai and Busemeyer 2014; Fisher 2017; Kim, Seligman, and Kable 2012; Roe, Busemeyer, and Townsend 2001; Smith and Krajbich 2018), including in situations with more than two alternatives (Gluth *et al.* 2020; Krajbich and Rangel 2011; Thomas, Molter, and Krajbich 2021). Use of mouse-tracking and information-search paradigms has successfully connected information acquisition and attentional patterns to choice (Diehl 2005; Johnson *et al.* 1989; Konovalov and Krajbich 2020; Payne 1976; Payne, Bettman, and Johnson 1988; Reeck, Wall, and Johnson 2017; Stillman, Krajbich, and Ferguson 2020; Stillman, Shen, and Ferguson 2018).

Based on this literature, we expect that the implied valuation of multi-option alternatives and resulting choices will be related to both alternative-level attention (i.e., how much attention each alternative receives) and sub-option-level attention (i.e., how much attention each sub-option receives). In particular, we predict that attention to an inferior sub-option will causally decrease choice likelihood of a multi-option alternative. This mechanism is consistent with an attention-driven weighted averaging process.

Extending Prior Research on Multi-Option Alternatives

There are thus two possibilities regarding how people might misvalue multi-option alternatives relative to the value-maximizing benchmark: they may overvalue them (because multi-option alternatives enable choice) or they may undervalue them (because multi-option alternatives may be evaluated as a whole entity via a weighted average). Prior research on choice sets and assortments indicates that such sets may be evaluated directly as whole entities rather than as choices among independent options (Sood, Rottenstreich, and Brenner 2004), indicating a key role for the weighted average findings regarding set valuation described above.

Such research on assortment choice has revealed numerous factors influencing assortment attractiveness, including similarity (Gourville and Soman 2005; Friedman, Savary, and Dhar 2018; Karmarkar 2017), number of unacceptable options (Kahn and Lehmann 1991), and the presence of dominated options (Bogard *et al.* 2024). This literature primarily focuses on how assortments compare to one another and how assortment framing (e.g., as sets vs. alternatives in the case of Sood *et al.* 2004) affects valuation. They do not focus on the relative valuation of a multi-option alternative in comparison to its highest-valued sub-option. A separate line of research on agenda effects indicates people use different strategies in early- versus late-stage choice, indicating that the decision agenda can affect final outcomes (Hauser 1986; Plott and Levine 1978; Tversky and Sattath 1979) and that consumers may neglect factors used earlier to prescreen alternatives (Chakravarti, Janiszewski, and Ülkümen 2006). Together, these findings might therefore suggest that the valuation of a later-stage choice set may differ from that of an early-stage choice set. Yet again, these literatures do not address the relative valuations of a multi-option alternative and its highest valued sub-option. That is the focus of our investigation.

To our knowledge, only two prior papers have directly examined the contrast between a multi-option alternative and its highest-valued option (Le Lec and Tarroux 2020; Spiller and Ariely 2020). However, these papers leave many questions unanswered.

Le Lec and Tarroux (2020) tested the contrast between a multi-option alternative and its highest-valued option in a single study in a domain in which the relative values of products are subjective. In this study, participants reported their willingness-to-pay (WTP) for multi-option alternatives regarding what websites to spend time on at

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the end of the experiment. The authors found evidence for undervaluation of multi-option alternatives and proposed two explanations: anticipation of future error and holistic evaluation. Spiller and Ariely (2020) focused entirely on a subset of multi-option alternatives: media of exchange (e.g., gift cards and promotional credit). Much like Le Lec and Tarroux (2020), this article used a subjective domain and focused primarily on WTP judgments (though some supplemental studies examined choice). In both cases, the authors found the extent of undervaluation increases with the difference in value between options, consistent with a weighted averaging process.

How are these results reconciled with the findings regarding the inherent value of choice? An important feature of the options-increase-value literature cited above is that a consumer's focus is primarily on the presence or absence of options rather than on the choice set as a singular entity. Thus, based on the most similar prior literature (Le Lec and Tarroux 2020; Spiller and Ariely 2020) and the arguments above, we propose and test three hypotheses:

> **H1:** Consumers are less likely to choose a multi-option alternative than they are to choose its higher-valued option over another fixed alternative.

> **H2:** This reduction in choice share increases with the difference in value between the two component options in the multi-option alternative.

> **H3:** This reduction in choice share is attributable to a reduction in the holistic value of the multi-option alternative (vs. the value of the more-attractive option).

Although some related behavioral results have been previously documented, the process by which undervaluation occurs has not been established. In addition to hypothesis 3 (which provides insight into process), we also rely on both a process-tracing method (mouse-tracking) and an attentional manipulation to inform the mechanism underlying the undervaluation of multi-option alternatives.

The attention literature reviewed above suggests that attention may reflect and/or direct the undervaluation of multi-option alternatives. Therefore, we make two literature-driven assumptions, and then test a resulting hypothesis:

> **Assumption 1:** Increased attention to an alternative is associated with an increased likelihood of choosing that alternative.

> **Assumption 2:** Increased attention to a sub-option is associated with an increase in the weight on that sub-option in choice.

> **H4:** Consumers are less likely to choose a multi-option alternative when:
>
> **H4a:** relative attention to S (vs. M_{HL}) is higher;
>
> **H4b:** relative attention to M_L (vs. M_H) is higher.

This article contributes to the literature in several ways, but the primary contributions are (1) to provide greater process evidence and (2) to generalize an effect that was previously quite narrowly examined. More specifically, we document process in two ways. First, we provide evidence of mechanism: adding an inferior sub-option decreases the value of the holistic multi-option alternative, rather than just the superior sub-option. Second, we use process tracing to better understand the relationship between information processing (i.e., what is attended to) and undervaluation. We measure and manipulate attention, and demonstrate a causal role of attention in choice. In terms of robustness, we demonstrate undervaluation in four different domains (including one domain with objectively dominating and dominated component options, expanding the findings beyond preferential choices in which uncertainty over future preference states may play a larger role). We also use a combination of within-subjects and between-subjects studies, many-choice and single-shot studies, and document the effect across different numbers of sub-options.

In addition, and in contrast to Le Lec and Tarroux (2020) and the primary results of Spiller and Ariely (2020), we use choices to document the effect.[2] Such choices are arguably more common in consumers' daily life than WTP ratings and sometimes exhibit qualitatively different effects (Amir and Ariely 2007; Grether and Plott 1979; Lichtenstein and Slovic 1971; O'Donnell and Evers 2019), perhaps because WTP ratings reflect market prices rather than personal preferences (Evangelidis, Jung, and Moon 2022). In the current case, this is important to examine as choices may encourage consumers to peer down the decision tree to possible outcomes whereas WTP judgments may encourage them to value a choice set holistically. We also rule out several possible explanations with additional studies and features (e.g., trinary choices). Finally, we use data on heterogeneous preferences to consider the marketing consequences of undervaluation with different levels of targeting precision.

Measuring Undervaluation

Though the concept of undervaluation is straightforward, finding an efficient, effective way to measure it within-subjects is decidedly less so. Eliciting WTPs of

2 Spiller and Ariely (2020) included some evidence from choice studies regarding media of exchange in their supplementary materials.

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multi-option alternatives would be simpler, but ratings are less-than-ideal for the reasons described in the prior paragraph. Therefore, we measure undervaluation primarily within-subject in a series of carefully constructed choices.

In each of our studies, we examine undervaluation by comparing choices in two types of decisions: test choices and control choices. In test choices, participants choose between a single-option alternative (S) and a multi-option alternative (M_{HL}). Within the multi-option alternative (M_{HL}), there are two component options: M_H and M_L, between which the participant could make a future choice. In all cases, value(M_H) > value(M_L). In the control choices, participants choose between the same single-option alternative (S) and the a priori determined higher-valued component option (M_H). Undervaluation occurs when the proportion of S choices is greater in test choices than in control choices. In other words, when participants choose M_{HL} less often than they choose M_H, this indicates that participants undervalue M_{HL} relative to its best component option (M_H).

Across six studies in the main text, we document the undervaluation phenomenon and investigate the underlying mechanism. In study 1, we find evidence of undervaluation (hypothesis 1) in a consumer domain with subjective values, and find stronger undervaluation as the difference between M_H and M_L increases (hypothesis 2). In study 2, we provide evidence for the mechanism underlying our effect (hypothesis 3). In study 3, we examine whether the effect holds for more sub-options within an alternative. In study 4, we demonstrate the effect in a second consumer domain, using a between-subjects, one-shot design. In study 5, we document undervaluation in a third consumer domain, and provide correlational evidence for our hypothesized attention mechanism (hypothesis 4). In study 6, we move into a more controlled domain (incentivized gambles) with objective dominance of M_H over M_L, and provide causal evidence for hypothesis 4.

In the web appendix, we describe nine additional studies in which we (1) replicate the main behavioral and mechanistic results, (2) rule out possible alternative explanations (e.g., inattention or confusion, inferences about multi-option alternative quality), and (3) examine the relationships between undervaluation and several individual difference measures (e.g., risk aversion). For a comparison of the methods across all studies, see Web Appendix O. All of our data, preregistrations, code, instructions, comprehension questions, and stimuli are available on ResearchBox: https://researchbox.org/124. All of the studies in this article were collected using Qualtrics. For all studies, we have reported all measures, conditions, and data exclusions. All studies (and their respective sample sizes) were preregistered; some sample sizes were determined via formal power analyses, while others were based on prior studies.

STUDY 1

In study 1, we tested for undervaluation of multi-option alternatives in a relevant consumer domain. We also investigated a potential moderator of the effect. Specifically, we investigated whether the subjective value difference between the best and worst component options is related to undervaluation, consistent with an averaging process.

Method

Participants. For this preregistered study, we collected responses from 305 Amazon Mechanical Turk (MTurk) workers. They earned $1.25 for their participation.

Materials and Procedure. First, participants rated 50 well-known films on a scale of 0–10 (measured in increments of 0.1 via a slider) regarding how much they wanted to watch each of them. For each film, participants had the option to click a box labeled "I've never heard of this movie".

Next, participants were asked to imagine that they were planning to go see a movie and were asked (on each of 30 trials) to choose which of two hypothetical theaters they would go to (figure 1). Each of the theaters was described as having either one or two movies playing. Participants were told that if they chose a theater with two movies, they would get to choose one of the two movies to see. Specifically, participants were instructed to make their choices as follows: "In each decision, you should choose whichever theater has the movie you want to see more right now." Participants completed comprehension questions before the choices to ensure that they understood that (1) the choices were hypothetical, (2) that they would get to choose one (and only one) movie to watch, even if they chose a theater with two movies, and (3) that their choices would not influence the number of choices that they would have to make.

The 30 choices were randomly generated for each participant. They fell into 3 categories (with 10 choices in each category) but were presented in randomized order. The first category of choices were *test* choices. In each of these trials, participants chose between a theater with one available movie (i.e., movie S) and a theater with two available movies (i.e., movies M_H and M_L, where the participant's prior rating of M_H was greater than the rating of M_L). Each test choice trial comprised three unique movies, which did not overlap between trials. Thus, the 10 test choices consisted of 30 different movies. These movies were drawn randomly from the movies that the participant rated. If a participant rated fewer than 30 films, then we generated as many trials as possible from their rated films before drawing from the

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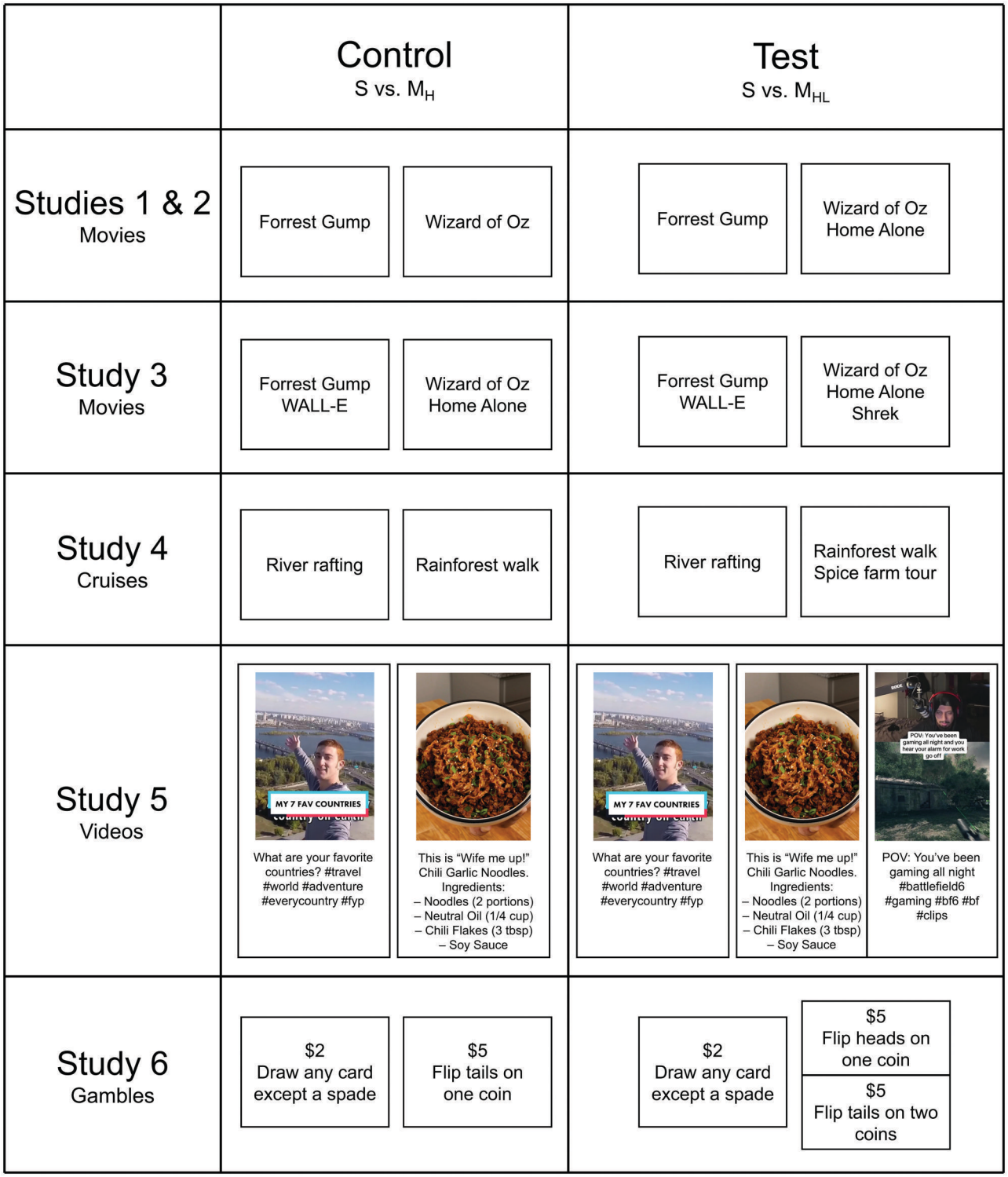
FIGURE 1

STUDY DESIGN.



Note.—In studies 1–3, participants first rated their desire to watch each of 50 films. Participants then made 30 hypothetical choices between two theaters, each of which was showing one or two films. Participants were told to imagine that if they chose a two-film theater, they would get to choose one of the two films to watch. In study 4, participants made one hypothetical choice between two cruises, each of which had one or two shore excursions available. In study 5, participants made 21 incentive-compatible decisions between two alternatives, each of which contained one or two short-form videos. Finally, in study 6, participants made 34 incentive-compatible decisions between two alternatives, each of which contained one or two gambles. Study 5 measured attention to the options, and study 6 manipulated attention to the options.

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unrated films (i.e., the films for which they selected "I've never heard of this movie"). In this study, only one participant rated fewer than 30 films.

As an example, consider a participant who rated 42 of 50 films, and clicked "I've never heard of this movie" for the remaining 8 films. To generate the test trials for this participant, we drew (without replacement) 30 films from the 42 that were rated, and randomly sorted them into 10 groups comprising 3 films each. One of these groups may be the triad {Wizard of Oz, Home Alone, Forrest Gump}, which our example participant rated {6.5, 4.3, 8.2}, respectively. Within each triad, we randomly assigned one of the films to be S. In this case, Forrest Gump is randomly assigned to be S. The remaining films {Wizard of Oz, Home Alone} were designated M_H and M_L. Since the example participant rated Wizard of Oz higher than Home Alone, Wizard of Oz is M_H and Home Alone is M_L.

The second category of choices were *control* choices. These trials consisted of a choice between two theaters, each with one available movie (e.g., Forrest Gump vs. Wizard of Oz). Importantly, each of the 10 control choices was matched to one of the test choices. Specifically, each control choice was a choice between the single-movie theater and the a priori higher-rated film from the two-movie theater from the matched test choice. Using the test choice example above (movie S vs. movies M_H or M_L; e.g., Forrest Gump vs. Wizard of Oz or Home Alone), the control choice would be movie S versus movie M_H (where movie M_H's rating was higher than movie M_L's rating prior to any choices; e.g., Forrest Gump vs. Wizard of Oz).

Finally, the last category of 10 trials were *filler* choices, each of which comprised two single-movie theaters (e.g., Black Panther vs. Saving Private Ryan). The filler trial films were randomly selected from the rated films, and independently from the selection process for the test trials. These trials were used for our preregistered exclusion criteria and to add some variability to the choices being made by participants.

After rating the movies and before making any choices, participants demonstrated their understanding of the task by completing several comprehension check questions, including a question about the meaning of the test choices, i.e., that choosing M_{HL} implied that they would get to choose which movie from the set $\{M_H, M_L\}$ they wanted to see. They were required to get all questions correct before moving on to the choices. In the web appendix, we describe a replication of study 1 and 1b (namely, study 1c) that uses an open-ended comprehension check question to firmly rule out "misunderstanding the task" as the driving force behind the results that we describe below.

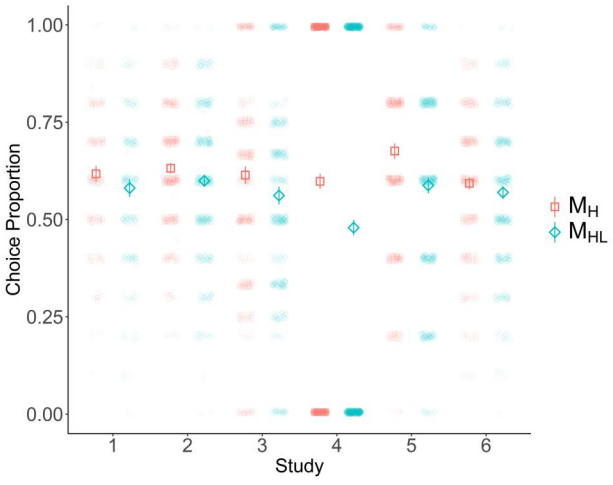
Exclusions. All of the following choices were determined in our preregistration. We excluded anyone who failed to rate at least 20 films, as they would have fewer usable test and control choices and thus noisier estimates. We also excluded anyone whose filler choices were not directionally predicted by their ratings in a logistic regression of *ChooseLeft* on (*RatingLeft* – *RatingRight*) as that indicates lack of minimal attention during ratings, choice, or both. These criteria resulted in the exclusion of 37 participants, leaving us with a final sample size of 268. We excluded any test-control choice pairs that were generated from unrated films. For instance, if a participant only rated 25 films, then we were only able to generate eight valid test-control choice pairs. The other two test-control choice pairs were generated from unrated films, and thus excluded from analysis.

Results

To test for undervaluation, for every participant, we calculated $S > M_{HL}$ as (Chose S in test choices/number of valid test choices). We also calculated $S > M_H$ as (Chose S in control choices/number of valid control choices). We tested $(S > M_{HL}) - (S > M_H)$ using a one-sample *t*-test and found evidence for undervaluation, $M = 0.04$, 95% CI [.02, .06], $t(267) = 3.73$, $p < .001$. In other words, choice of M_{HL} was 4 percentage points lower than choice of M_H (see figure 2 for choice proportions of M_{HL} and M_H and table 1 for means and effect sizes across all studies). Among the participants, 40% (106/268) exhibited undervaluation, 34% (91/268) exhibited no difference, and 26% (71/268) exhibited an effect in the opposite direction. The 34% exhibiting no

FIGURE 2

MAIN EFFECT ACROSS STUDIES.



Note.—Main effect of undervaluation across studies. Participants are less likely to choose M_{HL} than they are to choose M_H when compared to the same single-option alternative S. Bars indicate s.e.m. across participants.

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TABLE 1

MAIN EFFECT OF UNDERVALUATION ACROSS ALL STUDIES.

Study	M_H	M_{HL}	Main effect	95% CI	*t* statistic[b]	*p* value
1	0.618	0.580	0.04	[.02, .06]	3.73	<.001
1b	0.632	0.590	0.04	[.03, .05]	6.75	<.001
1c	0.667	0.645	0.02	[.01, .04]	3.18	.002
1d	0.636	0.613	0.02	[.01, .04]	2.84	.004
2	0.632	0.600	0.03	[.02, .04]	5.86	<.001
2b	0.630	0.605	0.03	[.01, .04]	2.94	.003
3[a]	0.613	0.565	0.05	[.03, .07]	4.95	<.001
3b[a]	0.614	0.534	0.08	[.04, .12]	3.83	<.001
4	0.615	0.471	0.14	[.11, .18]	—[c]	<.001
5	0.676	0.588	0.09	[.07, .10]	11.44	<.001
6	0.593	0.570	0.02	[.01, .04]	3.12	.001
6b	0.634	0.569	0.06	[.04, .08]	5.64	<.001
6c	0.635	0.580	0.05	[.03, .08]	5.04	<.001
6d	0.572	0.508	0.06	[.03, .09]	4.28	<.001
6e	0.576	0.510	0.06	[.02, .09]	3.37	<.001

Note.—Bold-numbered studies are in the main text; italic-numbered studies are in the Web Appendix.

[a]Based on preregistered analysis of 1v1 and 1v2 choices where $Rating_{M_H} - Rating_{M_L} \geq 2$.

[b]Each of these tests are consistent with the results of a corresponding Wilcoxon signed-rank test.

[c]This test was a two-sample test of proportions, so a *t* test was not appropriate. The χ^2 statistic was 82.21.

difference were not perfectly consistent, as in many cases it reflected multiple offsetting inconsistencies. This indicates that if there were more choices, the proportion of people exhibiting undervaluation would likely be greater. Moreover, even if there were no heterogeneity across participants, we would expect some participants to show an effect of "overvaluation" simply due to measurement error.

Because M_H and M_L were randomly selected (and thus, the difference between ratings varied across choices and participants), we can examine the role of preference strength for M_H over M_L in undervaluation. We used matched choice sets as the unit of observation with the variable Choice Difference as our outcome measure, defined as (Chose S in test − Chose S in matched control), thereby taking a value of +1, 0, or −1. We regressed Choice Difference on the rating of S, the summed rating of M_H and M_L, and the difference in rating between M_H and M_L, clustering the standard errors at the subject level (equation 1). Returning to our example participant from the methods above, the M_H-M_L preference strength for the example test choice (Forrest Gump vs. Wizard of Oz or Home Alone) is equal to 6.5 − 4.3 = 2.2. We computed the preference strength in this way for all triads of films.

$$ChoiceDifference_{it} = \beta_0 + \beta_1 S_{it} + \beta_2 (M_H + M_L)_{it} + \beta_3 (M_H - M_L)_{it} + \varepsilon_{it} \quad \textbf{(1)}$$

We find significant evidence for an effect of M_H-M_L preference strength on undervaluation, $b_3 = 0.019$, SE = 0.003, $p < .001$ (figure 3), such that participants exhibited more undervaluation for multi-option alternatives with bigger rating differences. We replicated each of these results in a direct replication, study 1b, detailed in Web Appendix B. We interpret these results as indicating the effect grows with preference strength, although measurement error could plausibly contribute (such that smaller rating differences are more likely to be associated with mismeasured preferences). We further examine this possibility in study 6c.

Moreover, in Web Appendix A, we demonstrate that the undervaluation effect persists regardless of whether the rating of S is higher than M_H, lower than M_L, or falls between the two. When S is rated higher than M_H, the choice share of M_H is below 0.5 (and the choice share of M_{HL} is even lower). This indicates that adding a less-preferred option (M_L) to M_H decreases choice share, rather than merely adding noise and pushing the choice share toward 50%.

Another potential concern with this study is that participants made inferences about the theater quality based on which movie(s) were being shown, and made their choices accordingly. We imagine that participants may be making some inferences, but to rule out this possibility as the main driver of our results, we ran study 1d. In study 1d, participants were told that there were multiple theaters nearby, but that they were on opposite sides of town. Participants had to choose between two trains on each trial, each of which went to a different part of town. Each part of town either had one or two movie theaters, each of which was playing one movie. They were instructed to select the train that would take them to the movie they wanted to see more. As in our previous data, we find evidence of undervaluation ($M = 0.02$, 95% CI [.01, .04], $t(349) = 2.84$, $p = .004$) and moderation by M_H-M_L preference strength ($b_3 = 0.012$, SE = 0.003, $p < .001$). For more details, see Web Appendix D.

Do people really make choices in this order (i.e., first theater, and then movie)? In these studies (and the ones that follow), we have tried to avoid imposing too much structure on how participants should view the decision. In studies 1–1d (as well as studies 2 and 3), we instructed participants to choose whichever theater (or train) had the movie they wanted to see more. If anything, this wording should increase focus on the movie and work against undervaluation.

STUDY 2

A key question is the exact method by which adding M_L to M_H to form the multi-option alternative M_{HL} decreases choice probability. One mechanism would be adding M_L reduces the perceived value of M_H (as in the tainting mechanism in the repulsion effect), thereby

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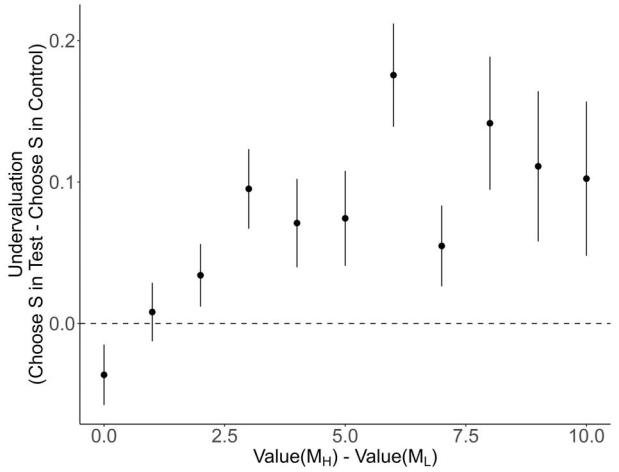
FIGURE 3

PREFERENCE STRENGTH AND UNDERVALUATION.



Note.—Relationship between (M_H-M_L) preference strength and degree of undervaluation in study 1. Undervaluation increases as M_H gets progressively better than M_L. Analysis reported in text controls for rating of S and sum of ratings of M_H and M_L. Bars represent s.e.m. across participants.

reducing the derived value of M_{HL}. A second distinct mechanism would be adding M_L reduces the perceived holistic value of M_{HL}. We designed study 2 to disentangle these mechanisms.

Method

Participants. For this preregistered study, we collected responses from 601 MTurk workers. They earned $1.50 for their participation.

Materials and Procedure. The procedure for this study was nearly identical to study 1. The only difference is that in addition to choices, participants were asked to rate 8 options or alternatives during the choice portion of the study. Following the respective choice, participants reported their rating (on the same 0–10 scale they had used for the ratings at the beginning of the study) of (1) S in a test choice, (2) M_H in a test choice, (3) M_L in a test choice, (4) M_{HL} in a test choice, (5) S in a control choice, (6) M_H in a control choice, (7) the left option in a filler choice, and (8) the right option in a filler choice. The first 6 ratings are of primary interest and came from 6 different sets of matched trials (figure 4).

Exclusions. As specified in our preregistration, we excluded anyone who failed to rate at least 20 films. We also excluded anyone whose filler choices were not directionally predicted by their ratings in a logistic regression of *ChooseLeft* on (*RatingLeft* – *RatingRight*). These criteria resulted in the exclusion of 38 participants, leaving us with a final sample size of 563. As specified in our preregistration, we excluded any test-control choice pairs that were generated from unrated films.

Results

As in previous studies, we find evidence of undervaluation, $M = 0.03$, 95% CI [.02, .04], $t(562) = 5.86$, $p < .001$. Next, we make several comparisons between ratings. As specified in our preregistration, to reduce sampling variability in the selected movies and thereby increase power, we account for the initial ratings of the options in our analyses by subtracting the initial ratings.[3] First, we look at whether the rating of S changes, depending on whether it is part of a test choice or a control choice, and find a marginally significant decrease in the rating of S following test (vs. S following control) ($M = -0.20$, 95% CI [−.41, .01], $t(562) = -1.90$, $p = .06$). Next, we make within-subject comparisons among three values: (1) the rating of M_H following control choices, (2) the rating of M_H following test choices, and (3) the rating of M_{HL} following test choices, using one-sample *t*-tests (table 2).

We find that M_H following control is rated significantly higher than M_{HL} following test ($M = 0.23$, 95% CI [.02, .44], $t(562) = 2.14$, $p = .03$), but we do not find a significant difference between M_H following control and M_H following test ($M = -0.07$, 95% CI [−.27, .12], $t(562) = -0.72$, $p = .47$). The difference between M_H following control and M_{HL} following test is significantly larger than the small difference between M_H following control and M_H following test ($M = 0.30$, 95% CI [.09, .51], $t(562) = 2.83$, $p = .005$).

This test provides evidence that adding a lower-value option decreases the value of the set, rather than decreasing the value of the higher-value option. Moreover, this effect is greater than any effect on the value of M_H alone.[4]

STUDY 3

In the next study, we extended our results to a broader array of choice set sizes. Rather than just examining choices between two one-movie theaters (1v1) and choices between one one-movie theater and one two-movie theater (1v2), we also examined choices between one one-movie theater and one three-movie theater (1v3), two two-movie theaters (2v2), and one two-movie

3 For M_{HL}, we subtracted the initial rating of M_H.

4 A smaller but otherwise identical study, study 2b, is presented in Web Appendix E. We interpret the results to be consistent.

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TABLE 2

MEAN RATINGS OF M_H AND M_{HL}.

Comparison	M_H (control)	M_H (test)	M_{HL} (test)
Mean rating (SE)	6.13 (0.11)	6.37 (0.11)	5.86 (0.11)
Mean (rating – initial rating) (SE)	−0.13 (0.08)	−0.06 (0.08)	−0.36 (0.09)

Note.—For the bottom row, we first subtract (from each post-choice rating) the rating of movie M_H provided by the participant during the first task in the study.

theater and one three-movie theater (2v3). This enabled us to consider the effect of adding a marginal option beyond the 1v1 set. In particular, the final comparison (a two-movie theater vs. a three-movie theater) enabled us to test a possible explanation for our results. Specifically, it is possible that participants chose M_{HL} less often than M_H because of an aversion to the second-stage choice. (Participants were informed that they would make the same number of choices no matter what they chose in any given trial, but it is conceivable they may have preferred to have psychological closure.) If this aversion were driving our results, then we would not observe undervaluation in the 2v3 case, because both options require a second-stage choice.

Method

Participants. For this preregistered study, we collected responses from 813 MTurk workers. They earned $1.50 for their participation.

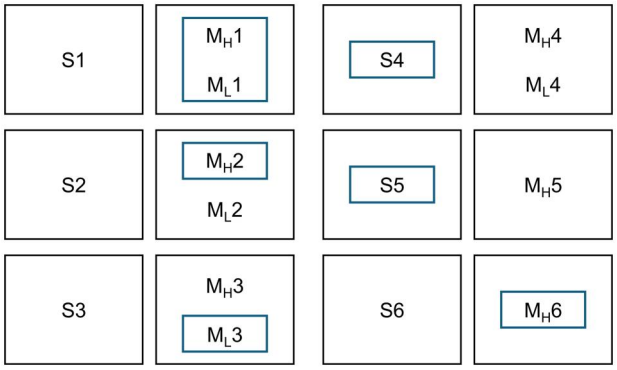
Materials and Procedure. First, as in studies 1 and 2, participants rated 50 well-known films on a scale of 0–10 (measured in increments of 0.1 via a slider) regarding how much they wanted to watch each. For each film, participants could click a box labeled "I've never heard of this movie."

Next, participants were asked to imagine that they were planning to go see a movie and were asked (on each of 42 trials) to choose which of two hypothetical theaters they would go to (figure 1). Each of the theaters was described as having either one, two, or three movies playing. Participants were told that if they chose a theater with more than one movie, they would get to choose one of the movies to see. Participants completed comprehension questions before the choices to ensure that they understood that (1) the choices were hypothetical, (2) that they would get to choose one (and only one) movie to watch, even if they chose a theater with more than one movie, and (3) that their choices would not influence the number of choices that they would have to make.

The 42 choices presented to participants were randomly generated for each participant. Of these 42 choices, 17 were filler choices, while the remaining 25

FIGURE 4

PRIMARY RATINGS IN STUDY 2.



Note.—These six ratings followed choices from six different matched sets. When asked about an individual option (i.e., S, M_H, or M_L), participants were asked "What is your rating of this option?" When asked about the multi-option alternative (i.e., M_{HL}), participants were asked "What is your rating of watching a movie of your choice from this option?"

were used for our primary analyses. For more information on the filler choices, see Web Appendix F. These 25 primary trials fell into five sets of trials, each of which included five matched trials. Each set of five matched trials included one trial of each of five types. The first type of trials was 1v1 choices. In each of these trials, participants chose between two theaters, each with one available movie (i.e., movie S_A and movie M_A); these correspond to the control choices from study 1. The second type of trials were 1v2 choices, where participants chose between a single-option theater (showing S_A) and a theater with two available movies (i.e., movies M_A and M_B); these correspond to test choices from study 1. The third type of trials were 1v3 choices, where participants chose between a single-option theater (showing S_A) and a theater with three available movies (i.e., movies M_A, M_B, and M_C). The fourth type of trials were 2v2 choices, where participants chose between two two-option theaters (i.e., one theater showed movies S_A and S_B, and the other theater showed movies M_A and M_B). The fifth type of trials were 2v3 choices, where participants chose between a theater showing two movies (S_A and S_B) and a theater showing three movies (M_A, M_B, and M_C). In all cases, Rating(S_A) $\geq$ Rating(S_B), Rating(M_A) $\geq$ Rating(M_B), and Rating(M_A) $\geq$ Rating(M_C).

Each matched set of five trials thus included five different movies assigned to the roles of S_A, S_B, M_A, M_B, and M_C, and there were five such matched sets. Across all matched sets of trials, S_A, S_B, M_A, M_B, and M_C were unique and non-overlapping. Thus, the 25 primary

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choices consisted of 25 different movies divided into 5 sets of 5 movies each. These movies were drawn randomly from the rated movies. If a participant rated fewer than 25 films, then we generated as many trials as possible from their rated films before drawing from the unrated films.

After rating the movies but before making any choices, participants demonstrated their understanding of the task by completing several comprehension questions, including a question about the meaning of the test choices (i.e., that choosing a theater with multiple options implied that they would get to choose which movie from the set they wanted to see). They were required to get all questions correct before moving on to the choices.

Exclusions. As specified in our preregistration, we excluded anyone who failed to rate at least 18 films, as they would have fewer usable primary choices and thus noisier estimates. We also excluded anyone whose filler binary choices were not directionally predicted by their ratings in a logistic regression of *ChooseLeft* on (*RatingLeft – RatingRight*) as that indicates lack of minimal attention during ratings, choice, or both. These criteria resulted in the exclusion of 38 participants, leaving us with a final sample size of 775.

Also in line with our preregistration, we excluded any choice sets that were generated from unrated films. For instance, if a participant only rated 21 films, then we were only able to generate 4 valid 5-film sets. The remaining 5-film set would be generated from unrated films and thus excluded from analysis. Finally, for statistical power and as preregistered, our analyses only included sets in which the added sub-option had a rating at least 2 points lower than the existing sub-option (s).[5] We preregistered this criterion because (1) when you have more options, it is more likely that the direction of preference could be miscoded due to measurement error and (2) with a small enough difference, option value might outweigh the effects of undervaluation (see the implied x-intercept in figure 3).

Results

In this study, we can test for undervaluation of several theaters. First, analogous to analyses in studies 1 and 2, we can compare choices in 1v1 and 1v2 trials to test for undervaluation of the 2-movie theater in 1v2 choices. To test for undervaluation in this case, for every participant, we calculated $S_A > M_{AB}$ as (Chose S in 1v2 choices/number of valid 1v2 choices). We also calculated $S_A > M_A$ as (Chose S in 1v1 choices/number of valid 1v1 choices). We tested $(S_A > M_{AB}) - (S_A > M_A)$ using a one-sample *t*-test and found evidence for undervaluation, $M = 0.05$, 95% CI [.03, .07], $t(655) = 4.95$, $p < .001$ (figure 5). In other words, choice of M_A was 5 percentage points higher than choice of M_{AB}.

We used the analogous approach to compare 1v1 against 1v3, 1v2 against 2v2, and 2v2 against 2v3. In each case, we find undervaluation (1v1 vs. 1v3: $M = 0.05$, 95% CI [.03, .08], $t(602) = 4.09$, $p < .001$; 1v2 vs. 2v2: $M = 0.05$, 95% CI [.03, .07], $t(656) = 4.72$, $p < .001$; 2v2 vs. 2v3: $M = 0.03$, 95% CI [.002, .06], $t(463) = 2.15$, $p = .03$[6]).

STUDY 4

In study 4, we tested for undervaluation of multi-option alternatives in a new consumer domain. In addition to demonstrating the effect in another domain, this study expands on the previous studies in three ways: (1) it is fully between-subjects, (2) we use a single-shot decision as our dependent variable, and (3) we do not require participants to rate individual options before making choices.

Method

Participants. For this preregistered study, we collected responses from 5000 MTurk workers. They earned $0.50 for their participation.

Materials and Procedure. First, participants were asked to imagine that they were going on a Caribbean cruise, during which there might be a variety of different experiences and activities that they could do. They rank-ordered six categories of experiences from most to least interesting: Nature, Animals, Adventure, Relaxation, History, Food/Drink.

Next, participants were told that they were going to choose between two cruises. They were told that the cruises are very similar (same price, same type of ship, same itinerary, same cruise company, same departure/return city), except for one difference: they offer different excursion options during the day in port. They were told that if a cruise offers more than one option, then they would get to pick which excursion they wanted to do. Participants were also told that they would make the same number of decisions in the study, no matter what they chose in any specific decision.

Participants were assigned to one of two between-subjects conditions: test and control. In the test condition, participants chose between a single-option cruise (S) and a

5 For the most basic comparison, between 1v1 and 1v2 trials, we required $\text{Rating}(M_A) \geq \text{Rating}(M_B) + 2$. For the comparison between 1v1 and 1v3 trials, we required $\text{Rating}(M_A) \geq \text{Rating}(M_B) + 2$ and $\text{Rating}(M_A) \geq \text{Rating}(M_C) + 2$. For the comparison between 1v2 and 2v2 trials, we required $\text{Rating}(S_A) \geq \text{Rating}(S_B) + 2$. Finally, for the comparison between 2v2 and 2v3 trials, we required $\text{Rating}(M_B) \geq \text{Rating}(M_C) + 2$.

6 All of these results are consistent with those of another identical study, study 3b. See Web Appendix G for more details.

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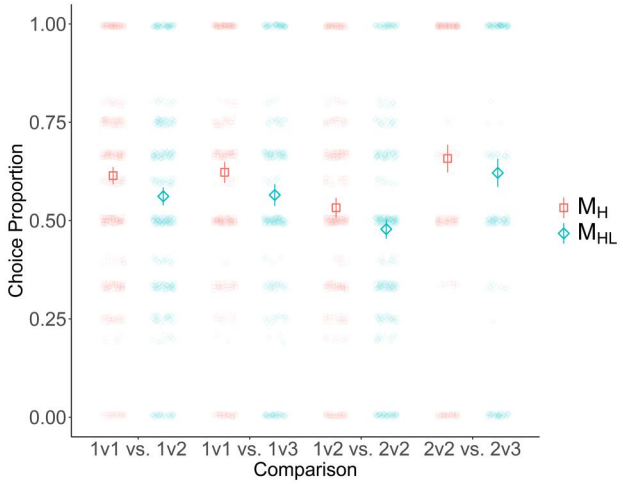
FIGURE 5

UNDERVALUATION IN STUDY 3.



Note.—Main effect of undervaluation in study 3, using multi-option alternatives with different numbers of options. Participants are less likely to choose the alternative with an additional (inferior) sub-option. Bars indicate s.e.m. across participants.

multi-option cruise with two options (M_{HL}). In the control condition, participants chose between two single-option cruises (S and M_H). Each of S, M_H, and M_L was an activity in one of the six categories from the ranking task. The identities of S, M_H, and M_L were assigned to each participant based on their rank-ordering of the activity categories and previously-collected pilot data. In these pilot data, we asked participants to rank-order the categories and also rate each of the individual options. Based on these pilot data, we imputed preference orderings for study 4 participants. Specifically, S was the top-rated activity (i.e., the activity with the highest average rating from the pilot data) in the participant's second-ranked category. M_H was the top-rated activity (i.e., the activity with the highest average rating from the pilot data) in the participant's first-ranked category, and M_L was the worst-rated activity (i.e., the activity with the lowest average rating from the pilot data) in the participant's fifth-ranked category. For example, for a participant who rated Adventure > Animals > Food/Drink > History > Nature > Relaxation, S would be a wildlife sanctuary visit, M_H would be zip-lining through the rainforest, and M_L would be mangrove exploration. Our main comparison is the proportion of participants who chose S in each condition. Data from our pilot study imply that M_H is preferred on average to S, which is preferred on average to M_L, but we do not have subject-specific ratings or rank-orderings of these three options.

We also included two attention/robustness check questions. After making their first choice, all participants chose between two single-option cruises (M_H and M_L). Finally, participants were asked to report which of two categories of activities (their third- and sixth-ranked categories) they were more interested in.

Exclusions. As specified in our preregistration, we excluded participants who chose M_L over M_H in the second choice ($N = 734$) and/or reported being more interested in their sixth-ranked category than their third-ranked category ($N = 349$). Some participants ($N = 83$) met both criteria, so we excluded a total of 1,000 participants.

Results

To test for undervaluation, we compared the proportion of participants who chose M_H to the proportion who chose M_{HL}. In the control condition, 61.5% of participants chose M_H, while in the test condition, 47.1% of participants chose M_{HL} (test of proportions, $\chi^2(1) = 82.21$, $p < .001$, 95% CI = [11.3%, 17.5%]). As preregistered, we tested whether these results were robust to including the excluded participants. The results hold in all versions of these robustness checks (include M_L over M_H: $\chi^2(1) = 86.74$, $p < .001$, 95% CI = [11.3%, 17.3%]; include sixth over third: $\chi^2(1) = 68.56$, $p < .001$, 95% CI [9.3%, 15.1%]; include all: $\chi^2(1) = 70.22$, $p < .001$, 95% CI [9.1%, 14.7%]).

STUDY 5

In studies 5 and 6, we tested for our attention mechanism using correlational (study 5) and causal (study 6) designs. First, in study 5, we tested for undervaluation of multi-option alternatives in another consumer domain. In addition to demonstrating the effect in a third consumer domain, this study expands on the previous consumer good studies in three ways: (1) it is incentive-compatible, (2) it does not require participants to rate individual options before making choices, and (3) it examines the relationship between attention (measured with mouse-tracking) and undervaluation.

Method

Participants. For this preregistered study, we collected responses from 810 participants on CloudResearch Connect. They earned $2.00 for their participation.

Materials and Procedure. First, participants were asked to rank-order six categories of videos regarding how interested they would be in watching them: Animals, Travel, Gaming, Beauty/Fashion, Cooking, and Music/Performance.

Next, participants were told that they would make multiple choices between two alternatives, each of which would contain either one or two TikTok videos. The videos were represented by a screenshot and the caption to the video. They were told that if they chose an alternative that

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contained more than one option, then they would get to pick which video they would watch. Participants were informed that one trial would be randomly selected and they would watch the video they selected in that trial. Participants were also told that they would make the same number of decisions in the study, no matter what they chose in any specific decision.

Participants made 21 decisions in total: Five of these were test choices (S vs. M_{HL}), five were (matched) control choices (S vs. M_H), one was a choice between M_H and M_L, and 10 were filler choices. Each of S, M_H, and M_L was a TikTok video from one of the six categories from the ranking task, represented by a screenshot and caption. The identities of S, M_H, and M_L for each of the five matched trial pairs were assigned to each participant based on their rank-ordering of the activity categories, as well as previously collected pilot data. In each matched pair, we assigned S, M_H, and M_L in a similar method to study 4. Specifically, data from our pilot study imply that in each trial pair, M_H is preferred on average to S, which is preferred on average to M_L, but we do not have subject-specific ratings or rank-orderings of these three options.

For more details on trial creation, see Web Appendix H.

To access the information about each option, participants had to move their cursor over the option (figure 6). While their cursor was hovered over an option, the information was visible. When they moved their cursor out of the box, the information disappeared. Participants were allowed to hover over each option for as long as they wanted to, as many times as they wanted to, and in whatever order they wanted to.

We included one attention check question. Participants were asked to report which of two categories of activities (their third- and sixth-ranked categories) they were more interested in.

Next, participants completed two short-form decision-making scales: the six-item version of the maximizing scale (Nenkov *et al.* 2008; Schwartz *et al.* 2002) and the 15-item version of the need for cognitive closure scale (Kruglanski *et al.* 1993; Roets and Van Hiel 2011; Webster and Kruglanski 1994). Finally, we selected one random trial from the main task, and participants watched the video that they chose on that trial.

Exclusions. As specified in our preregistration, we excluded participants who reported being more interested in their sixth-ranked category than their third-ranked category. We also excluded participants who did not mouse over all of the boxes during the instruction phase as they are instructed to do.[7] These exclusions resulted in the exclusion of 122 participants, leaving a final sample size of $N = 688$.

Results

Choice. To test for undervaluation, we compared choices in test choices versus control choices and found significant evidence of undervaluation ($M = 0.09$, 95% CI [.07, .10], $t(687) = 11.44$, $p < .001$). Overall, 39% (268/688) of participants exhibited undervaluation, 51% (348/688) exhibited no effect, and 10% (72/688) exhibited an effect in the opposite direction.

We did not find a meaningful relationship between undervaluation and either of the individual difference measures (maximizing: $r = 0.01$, $t(686) = 0.28$, 95% CI [−.06, .09], $p = .78$; need for cognitive closure: $r = -0.04$, $t(686) = -1.16$, 95% CI [−.12, .03], $p = .25$).

Next, we turn to the attention results. As specified in hypothesis 4, we expect that choice of a multi-option alternative—and ultimately, undervaluation—to be associated with both alternative-level and sub-option-level attention.

Attention to S. We first test for a relationship between alternative-level attention and choice (hypothesis 4a). First, in a trial-level analysis of the test trials using logistic regression, we regress choice of M_{HL} on the proportion of time spent on S (relative to M_{HL}), and we find a significant relationship between attention to an alternative and choice, $b = -5.58$, SE = 0.37, $p < .001$. Increased attention to S is associated with a lower probability of choosing M_{HL}.

In another test of alternative-level attention, we examine whether alternative-level attention is related to choice at the aggregate level. For each subject, we computed (1) the proportion of choices for M_{HL} and (2) the average proportion of attention on S (vs. M_{HL}). Attention is predictive of choice at the aggregate level, as well, $b = -1.23$, SE = 0.11, $p < .001$. These results regarding choice of M_{HL} are reflected in undervaluation as well.[8]

Attention to M_L. Next, we look to sub-option-level attention. Hypothesis 4b states that increased attention to the inferior sub-option will be associated with a lower likelihood of choosing the multi-option alternative. In a trial-level analysis, we regressed choice

7 If a participant did not mouse over either option during one (or more) of their choices, we would have excluded them, in line with our preregistration. However, no participants met this exclusion criterion.

8 Specifically, we also computed the trial-level effect of attention on undervaluation, by regressing choice difference (choice of S in test – choice of S in matched control) on the attention difference (proportion of attention to S in test – proportion of attention to S in matched control), and find a significant relationship, $b = 0.43$, SE = 0.03, $p < .001$. We also computed the average difference in option-level attention between test and control trials (i.e., average proportion spent on S in S vs. M_{HL} choices – average proportion spent on S in S vs. M_H choices). We find a positive relationship between this average proportion mouse difference and degree of undervaluation, $b = 0.63$, SE = 0.07, $p < .001$. Participants who spent relatively more time inspecting S when paired with M_{HL} than when paired with M_H undervalued M_{HL} more.

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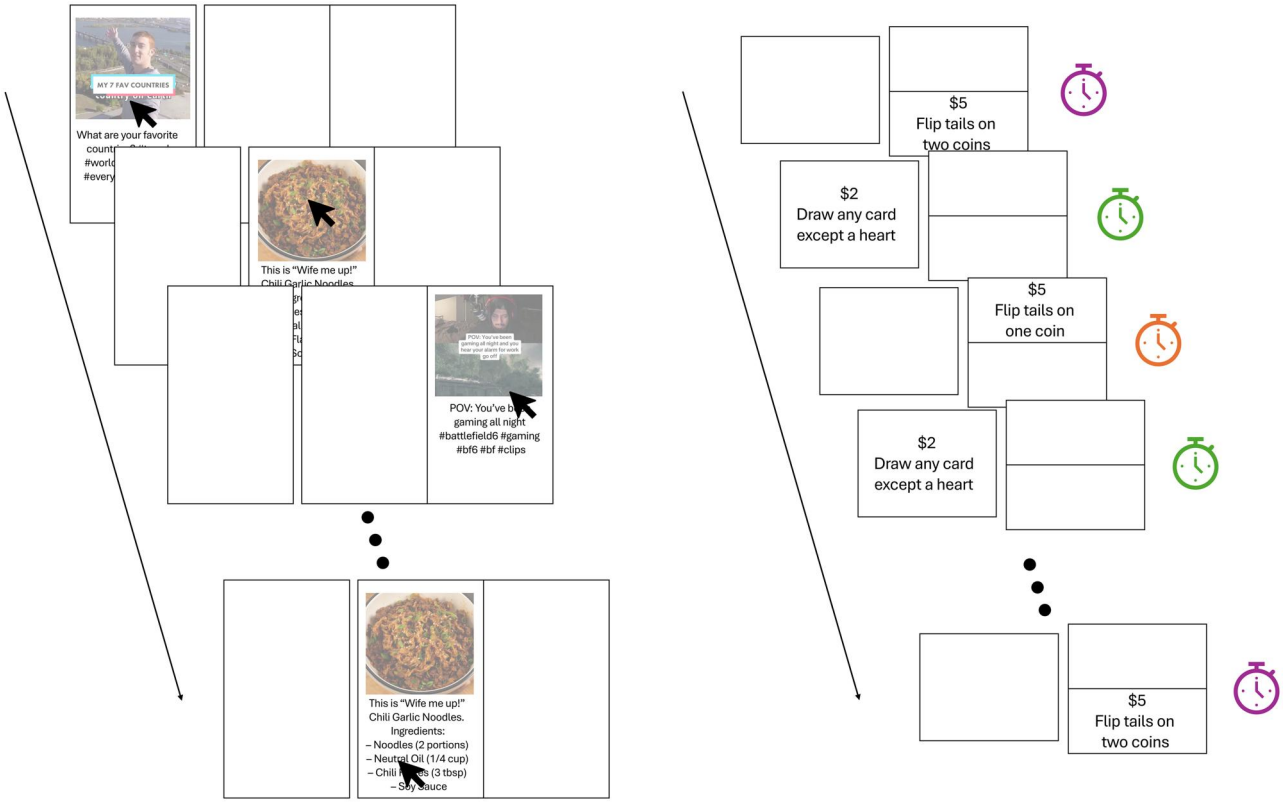
FIGURE 6

STUDY 5 AND STUDY 6 METHODS.



Note.—The left panel (study 5) demonstrates how participants used their cursor to reveal information about each option. Participants could mouse over each option for as long as they wanted to, as many times as they wanted to, and in whatever order they wanted to. The right panel (study 6) shows how participants were exposed to the information about each option. The amount of time that each option was visible, and the order in which the information appeared were prespecified.

for M_{HL} on the proportion of time spent on M_L relative to M_H (i.e., time spent on M_L/total time spent on M_{HL}). We find a significant negative relationship ($b = -2.06$, SE $= 0.16$, $p < .001$), which implies that an increase in the time spent looking at M_L (i.e., the inferior option in M_{HL}) is associated with a lower likelihood of choosing M_{HL} (i.e., a higher likelihood of choosing S) (figure 7). Moreover, at the aggregate level, we see that the average proportion of time spent on M_L (vs. M_H) is associated with a lower proportion of choices for M_{HL}, $b = -0.32$, SE $= 0.11$, $p = .004$. As with the alternative-level attention results, these results regarding choice of M_{HL} are reflected in undervaluation as well.[9]

A primary limitation of study 5 is that our process evidence is correlational. Although past work (Fisher 2021) has demonstrated a causal role of attention in choice, the present studies have not directly tested this. Therefore, in study 6, we directly manipulate attention to M_H and M_L and examine the effect on undervaluation.

STUDY 6

In the prior studies, we established the presence of undervaluation in three distinct consumer domains, including one with incentive-compatible choices. In study 5, we tested for a relationship between attention and undervaluation and found significant correlational evidence. In study 6, we provide causal evidence of the attention mechanism, by manipulating the amount of

9 Attention to M_L should predict choice of M_{HL} (in test choices), but not choice of M_H (in control choices). In both trial- and aggregate-level analyses of undervaluation, we see a significant relationship between attention to M_L and undervaluation ($b = 0.27$, SE $= 0.03$, $p < .001$; $b = 0.18$, SE $= 0.08$, $p = .03$). However, because it is impossible to measure attention to M_L in control choices, these metrics (especially at the aggregate level) are necessarily particularly noisy.

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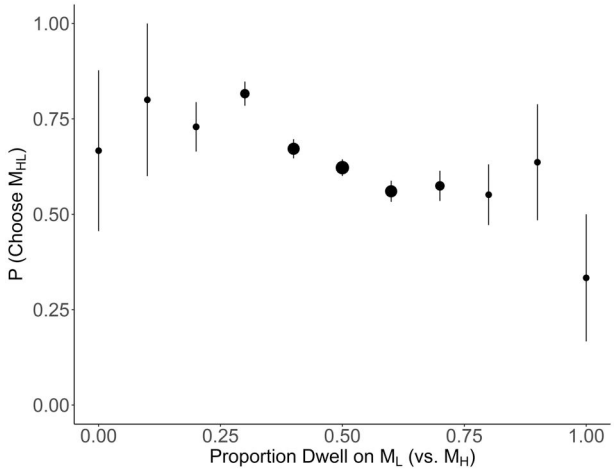
FIGURE 7

MOUSE-TRACKING AND CHOICES.



Note.—Relationship between mouse-tracking and choices in test choices. As participants spent more time on M_L (relative to M_H), they were less likely to choose the multi-option alternative (M_{HL}), $b = -2.06$, SE = 0.16, $p < .001$. Sixty-five percent of all proportions of time spent on M_L lie in the interval [0.3, 0.7]. Within that interval, the coefficient is still significant and negative ($b = -1.51$, SE = 0.45, $p < .001$). Bars represent s.e.m. across participants.

attention to the two sub-options in the multi-option alternative (i.e., M_H and M_L). We also switch to the domain of risky choice, using card draws, die rolls, and coin flips. This new domain enables greater control and allows for objective dominance relationships within the multi-option alternative, while maintaining a similar experimental design. Unlike studies 1–3, this permits us to establish a rank ordering without initial ratings. Unlike studies 4 and 5, this permits us to identify rank orderings at the individual level with no (or at least much-reduced) preference heterogeneity. The objective dominance in this domain further reduces the likelihood that participants undervalue a multi-option alternative due to uncertainty about their own future preferences.

Method

Participants. For this preregistered study, we collected complete responses from 639 participants on CloudResearch Connect.[10] They earned $3.00 for their participation. Five randomly selected participants also received the outcome of one of their decisions, as detailed below.

10 We preregistered that we would collect data from 600 participants (and that is all we recruited on CloudResearch), but CloudResearch provided more participants than we asked for.

Materials and Procedure. This study was similar to the earlier studies, with the following changes. First, choice options were gambles, as depicted in figure 1. These gambles were drawn from a large pool of potential gambles from various combinations of drawing cards of different suits, flipping coins or pairs of coins, or rolling numbers on a six-sided die. Ten sets of control choices and test choices were matched on winning dollar amounts and probabilities but could vary (non-systematically) in terms of the specific mechanism, as shown in figure 1. For example, a test choice might be: S = {$2 if you roll a 1 or 2 or 3 or 4 or 5} versus M_{HL} = {$5 if you flip heads on one coin OR $5 if you draw a club}. A matched control choice might be: S = {$2 if you roll a 2 or 3 or 4 or 5 or 6} versus M_H = {$5 if you flip tails on one coin}.

Another difference from prior studies is that participants did not rate any options or categories, as we could model each option's value in terms of its payout and probability. In addition to test choices and control choices, participants also made 10 filler choices between two single-option alternatives (e.g., choosing among gambles A and B), three attention-check questions, and one M_H versus M_L choice from a randomly selected test choice. In total, participants made 34 incentivized choices. Other than the M_H versus M_L choice, which always appeared last, the other trials appeared in a randomized order.

In this study, we manipulated attention by revealing (and hiding) each option for a pre-specified amount of time in a semi-random sequence (figure 6). Specifically, participants completed two within-subjects conditions: pro-undervaluation and anti-undervaluation. In pro-undervaluation, we showed M_L for twice as long as M_H (~6 vs. ~3 seconds). In anti-undervaluation, we showed M_H for twice as long as M_L (~6 vs. ~3 seconds). We required M_L to be seen first and last in pro-undervaluation and M_H to be seen first and last in anti-undervaluation, while the rest of the reveals were shuffled without repetition (i.e., S could not follow S). The S option was shown for an amount of time in between the M_H and M_L options (~4.5 seconds). In all control choices, we showed S and M_H in alternating order for the same amount of time (~4.5 seconds). For more details on the implementation of the attention manipulation, see Web Appendix I.

At the end of data collection, we randomly selected five participants and then randomly selected one of their choices to play out. Participants were aware of this structure. We opted to reward one trial instead of all of the trials to avoid stockpiling/strategy variability across trials (Juechems *et al.* 2017). In this study, three of the five participants won money ($5, $5, and $2). See Web

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Appendix I for additional information on the trial generation process.

As in earlier studies, participants completed several comprehension check questions, including a question about the meaning of the test choices, i.e., that choosing M_{HL} implied that they would get to choose one option from the set $\{M_H, M_L\}$. They were required to get all questions correct before moving on to the choices.

Exclusions. As specified in our preregistration, we excluded anyone who picked an obviously dominated option in any of the three attention-check questions. We also excluded any participant who reported navigating away from the screen, and excluded participants who did not view the information for the proper amount of time on more than one trial (due to navigating away or some other computer issue), which was recorded in our data as observation time(s) on any of the options below 500 ms. These criteria resulted in the exclusion of 127 participants, leaving us with a sample size of 512.

Results

First, we test for undervaluation across all choices. As in previous studies, we find significant evidence of undervaluation, $M = 0.02$, 95% CI [.01, .04], $t(511) = 3.12$, $p = .001$. We find that 42% (213/512) of participants exhibited undervaluation overall, 26% (134/512) exhibited no effect and 32% (165/512) exhibited an effect in the opposite direction.

Next, we turn to our attention manipulation. We expect greater levels of undervaluation in the pro-undervaluation trials (vs. the anti-undervaluation trials). Indeed, this is what we observe. In the pro-undervaluation trials, we observe significant undervaluation, $M = 0.04$, 95% CI [.02, .06], $t(511) = 3.78$, $p < .001$. In the anti-undervaluation trials, we do not observe significant undervaluation, $M = 0.01$, 95% CI [−.01, .03], $t(511) = 0.90$, $p = .37$. The within-subject difference between these two conditions was significant, $M = 0.03$, 95% CI [.001, .06], $t(511) = 2.05$, $p = .04$.

Replications

We also previously ran four studies in the domain of gambles (6b–6e) without attention manipulations. Participants in all studies exhibited undervaluation (6b: $M = 0.06$, 95% CI [.04, .08], $t(208) = 5.64$, $p < .001$; 6c: $M = 0.05$, 95% CI [.03, .08], $t(240) = 5.04$, $p < .001$; 6d: $M = 0.06$, 95% CI [.03, .09], $t(193) = 4.28$, $p < .001$; 6e: $M = 0.06$, 95% CI [.02, .09], $t(175) = 3.37$, $p < .001$). In study 6b, we replicate the attention results of study 5 in the domain of gambles. In the remaining studies, we also examine the relationship between undervaluation and individual difference measures (risk aversion, analytic vs. holistic thinking, and elaboration on potential outcomes); we do not find any meaningful relationships between these individual difference measures and undervaluation. In these studies, we also provide evidence against a few alternative explanations for the main effect of undervaluation. First, we rule out strong avoidance of multi-option alternatives by showing that people are more likely to pick M_{HL} (vs. S) than they are to pick M_L (vs. S). Second, we provide further evidence against the notion that noisy/careless responding and/or misunderstanding could explain our effect; when we limit our analysis to participants who (1) got every comprehension question correct on the first try, (2) never selected a dominated alternative, or (3) the combination of these criteria, our effect does not go away. If anything, the effect size increases. Finally, in study 6e, participants made choices between M_H and M_L. This rules out the possibility that the results are driven by the fact that participants never encounter the second-stage choice. This is in addition to the fact that in every study, participants answered a comprehension check question that they understood the number of choices they faced would not depend on the options they chose. Additional details are available in Web Appendices J–M.

GENERAL DISCUSSION

Across 15 controlled online laboratory studies in four domains, we document a consistent effect: decision-makers undervalue multi-option alternatives. More specifically, they are less likely to choose a multi-option alternative M_{HL} than they are to choose its highest-valued sub-option (M_H). This effect also holds when the multi-option alternative has more than two options. Moreover, we find that the strength of undervaluation is related to the difference in values between M_H and M_L: as the difference in values increases, undervaluation increases as well. We also find process evidence which suggests that undervaluation is driven by a devaluation of the holistic option M_{HL} and not just M_H. We further find a correlational and causal role of attention in the choice of the multi-option alternative. In replication studies (see the Web Appendix), we provide evidence against multiple alternative explanations (noisy responding; strong delayed choice aversion).

Related Work and Alternative Explanations

There are several literatures that address similar phenomena, including assortment choice (Bogard *et al.* 2024; Friedman, Savary, and Dhar 2018; Gourville and Soman 2005, Kahn and Lehmann 1991; Karmarkar 2017; Sood, Rottenstreich, and Brenner 2004) and multi-alternative (i.e., more than 2 alternatives) choice. However, these literatures ultimately do not address the question at hand. Assortment choice research focuses on the evaluation of assortments but does not typically

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enable comparisons to a value-maximizing benchmark, nor does it compare evaluations of assortments to the evaluations of the constituent parts of the assortment in a choice context.

Regarding multi-alternative choice research, our test choices (i.e., choices between a single-option alternative S and a multi-option alternative M_{HL}) are formally equivalent to trinary choices (i.e., choices between S, M_H, and M_L). However, we demonstrate in the Web Appendix that participants are significantly less likely to choose M_{HL} (in a test choice) than they are to choose M_H or M_L (in a trinary choice), indicating these two types of choices are neither psychologically nor practically equivalent.

Within related literature on assortment and multi-option choice, a natural comparison is Sood *et al.*'s (2004) finding that lone options are more likely to be chosen than grouped items when grouped items are initially compared to one another. This is because the intra-group comparisons lead to focusing on downsides of grouped options more than single options. Yet in the current examination in the risky domain, there are no tradeoffs among the grouped options, suggesting such comparative loss aversion does not explain these results. In addition, if comparative loss aversion were the explanation, in study 2 M_H ought to exhibit the same decrease in attractiveness following test choices that M_{HL} does, yet it did not. Another point of comparison is the model proposed by Kahn and Lehmann (1991). This model suggests that the value of an assortment will decrease as the number of unacceptable items in the assortment increases. However, our results do not depend on this characterization. Indeed, M_L is not preferred to M_H, but it is preferable to receiving nothing, so it is not an unacceptable item. Our findings suggest that model may extend beyond unacceptable items and into less-desirable items.

As mentioned in the introduction, agenda effects share certain similarities with this work. However, this literature does not offer explanations for the present results. For example, past work has documented the use of different decision strategies during early versus later stage choices (e.g., non-compensatory versus compensatory strategies for integrating attribute values; Bettman and Park 1980; Billings and Marcus 1983; Johnson and Meyer 1984; Payne 1976). In the present article, we do not focus on which attributes are utilized (and when), but rather on the relative values of the multi-option alternative and its highest-valued sub-option.[11] In essence, the phenomenon presented in the current article seems to be an additional instance in which agenda effects matter. Moreover, in contrast to work on agenda effects, we find that undervaluation persists even in the simplest case: a single option pitted against a two-option alternative.

A third comparison point is Gourville and Soman (2005), who find a decrease in market share when non-alignable products are added to a product line, termed "overchoice." Our results are distinct in two key ways. First, the present article focuses primarily on the preference ordering of the sub-options, rather than the alignability of them. Second, in terms of results, we find an increase in choice share when M_H is added to M_L, even when the two are less alignable (e.g., studies 6b, 6d, and 6e, with low transparency pairings). A fourth comparison is to a working paper by Pink, Mullainathan, and Milkman (2026), who find preference reversals between one-stage and two-stage choices. Although the results are conceptually consistent with ours, the design of the multi-option alternatives differs. Pink *et al.* focus on situations in which people choose between two multi-option alternatives: one containing the highest-valued option, and one with a higher average value. While both papers highlight the use of averaging in the evaluation of a multi-option alternative, the present article demonstrates that the averaging is weighted by attention. Moreover, the present article shows that this process specifically affects the evaluation of the set, rather than the evaluation of the sub-options. A fifth and final comparison point is Bogard *et al.* (2024). Here, brand inferences and distrust of the choice architect are key drivers of negative reactions to choice sets with inferior options. In some of our studies, these mechanisms may be at play, but a key feature of the studies in Bogard *et al.* is that the dominance relationships are explicit, numeric, and objective. Conversely, in all of our consumer goods studies, the dominance relationship among multi-option alternatives is not explicit, numeric, or objective. Inclusion of an idiosyncratically less-preferred option is unlikely to induce the negative inferences regarding the choice architect that Bogard *et al.* find with objectively inferior options.

Our current findings also have certain similarities with the uncertainty effect, indicating that in certain cases, consumers value a prospect as less valuable than its worst outcome (Gneezy, List, and Wu 2006; Mislavsky and Simonsohn 2018; Simonsohn 2009; Yang, Vosgerau, and Loewenstein 2013). However, two key distinctions are that (1) the uncertainty effect addresses a risky outcome, whereas here the decision maker chooses an option from the multi-option alternative and (2) the uncertainty effect is assessed relative to the worse outcome whereas undervaluation is assessed relative to the better outcome.

We have addressed several potential alternative explanations with our data. First, we find evidence in study 2 that the value of M_{HL} in test choices is significantly lower than that of M_H in control (or test) choices indicating adding

11 Moreover, in most studies, we use options that do not have easily-alignable attributes.

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M_L decreases choice share by decreasing the evaluation of M_{HL}, not by decreasing the evaluation of M_H. Thus, although our results share similarities with the repulsion effect, we do not find evidence of the "tainting" hypothesis proposed in earlier work (Frederick *et al.* 2014; Simonson 2014). Rather, we find that adding a lower-valued sub-option decreases the value of the choice set overall, rather than decreasing the value of the higher-valued sub-option.

Second, we rule out the possibility that our results are due to noisy or poor responding by showing that the effect is robust to multiple attention checks and comprehension questions; if anything, the effect is larger among participants who show signs of being more attentive. We rule out delayed-choice aversion in two ways: (1) participants understand that they will make a fixed number of decisions and (2) we find that participants choose M_{HL} more often than they choose M_L when pitted against S, so they do not have an absolute avoidance of delayed choices. In Web Appendix A, using a subset of choices for which M_H is chosen less than 50% of the time, we see that adding a less-attractive option further decreases choice share. This indicates undervaluation is not merely a regression to 50%.

Marketing Implications

The present research has a number of implications for marketing managers. First, it reinforces the importance of understanding the choice environment as construed by consumers. The same (e.g., trinary) choice may be construed as a two-stage decision by some consumers and as a single-stage decision by others. The current findings shed light on how dominated options differentially influence choice share depending on whether they are considered as options within multi-option alternatives or isolated alternatives. Thus, understanding how consumers structure the choice environment will permit more accurate predictions of consumer choice.

In addition to improving choice predictions, the current work also suggests ways to structure the choice environment with formal choice structures or informal choice cues to shape how the choice is framed. If marketers can establish a priori what a consumer would likely prefer within a given choice set, they may strategically structure a set to enhance options by excluding less-attractive options (or reduce choice share by including less-attractive options). Of course, the extent to which such practices are likely to be effective will depend on the precision with which marketers are able to predict consumers' choice shares: better prediction models will enable greater tailoring (see the next section on heterogeneity). This structuring is at odds with what one might predict from research on the attraction effect (Huber, Payne, and Puto 1982). Whether the asymmetrically dominated option appears as a third option or as an additional component option within one alternative may reverse its effect on relative choice shares. In a trinary choice setting, the attraction effect benefits M_H because M_H dominates M_L and so M_H's choice share increases. In contrast, when M_H and M_L are framed as component options of M_{HL} (as in all of our studies), choice share for M_{HL} decreases relative to the control choice between S and M_H. However, since we do not observe the attraction effect in our studies, this proposition is yet to be tested.

Finally, these findings reinforce the potential benefit for decision aids to encourage peering down the branches of the decision tree (Shafir 1994). One approach to improve choice outcomes is to simply exclude options from consideration. Alternatively, decision aids may prompt consumers to consider second-stage choices when choosing a first-stage option (Spiller and Ariely 2020). While our research thus far has focused on consumer decisions, it is plausible that similar effects manifest among managers as well.

One further environment in which we expect our results to apply is in choice-based conjoint studies. Many marketers use a version of choice-based conjoint that first forces a choice between options, and then asks participants to indicate whether they would purchase the option they chose. Our results suggest there may be contexts in which the results from these conjoint studies may not accurately reflect true purchase behavior. For contexts amenable to undervaluation, a choice in such a conjoint study might first ask participants whether they would purchase any of the products, and then force participants to choose from the set. If undervaluation drives down purchase rates in the marketplace, then it may similarly drive down purchase rates in the first stage, an effect worth capturing properly. Marketers using choice-based conjoint may consider reversing the order of the questions in order to predict real-world behavior in the presence of undervaluation.

The presence of the undervaluation effect in three different consumer domains (movies, cruises, and videos) and a tightly controlled setting (gambles) provides evidence to suggest that this is a pervasive phenomenon. To illustrate how these implications might play out for a marketer or choice architect, we provide the results of a simulation exercise in the next section.

Impacts of Heterogeneity and Targeting

A key factor in assessing the presence of undervaluation is the heterogeneity of consumer preferences. Given undervaluation, with perfect information about each individual's preferences, offering only each consumer's most preferred option would lead to the highest choice share, regardless of the underlying heterogeneity in preferences. However, marketers regularly have incomplete

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and/or imperfect information. As an example, in studies 4 and 5, we did not have subject-level ratings for each of the options, so it is impossible to know at the individual level whether the assigned M_H and M_L values had the assumed preference ordering (i.e., $M_H > M_L$). Given heterogeneous preferences, it is likely that some participants preferred M_L to M_H. Recent research (Duncan and Sharif 2026) suggests that a mismatch between benefits sought and benefits offered can indeed have a backfiring effect.

To address these concerns about preference heterogeneity, we used the data from studies 1 and 1b, and simulated several scenarios with varying amounts of heterogeneity and ability to target consumers based on their individual preferences; details are given in Web Appendix N. Consider the subset of film pairs (e.g., {The Shawshank Redemption, Legally Blonde}) where one film is favored by 60% to 70% of consumers (i.e., the right half of figure 8). If a marketer has no information about the preferences of the consumers (i.e., has no ability to target consumers; the solid red line in figure 8), then there would be a benefit to offering both films (vs. only one): they could expect roughly a 3 percentage-point increase in take-up by the consumers if both films are available as options. If a marketer has more information (i.e., can correctly target half of the consumers; the dashed green line in figure 8), then it is better (by roughly 1 percentage point) to provide consumers with only the predicted-to-be-preferred film (vs. both). If a marketer has even more complete information (i.e., can correctly target 75% of the consumers; the blue dash-dot line in figure 8), then it is even better (by roughly 3 percentage points) to provide only the predicted-to-be-preferred film to each consumer.

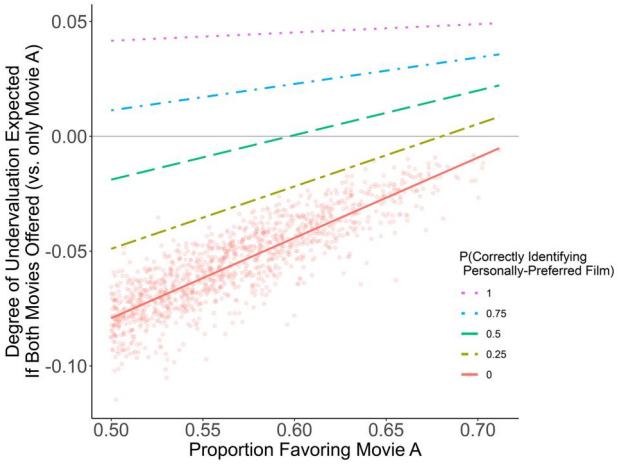
The benefit to providing additional options depends on the extent to which the additional options are preferred by part of the population. But the effects of adding options to an alternative will depend not only on the heterogeneity of the population, but also the targeting capabilities of the marketer. In other words, the degree of benefit depends on preference heterogeneity in the population and the ability to target different segments. Indeed, in this simulation, a 75% majority is equivalent to 75% absolute accuracy in predicting preferences. Heterogeneity of preferences and targeting ability are critical moderators to consider in assessing the extent of undervaluation when perfect personalization is not possible.

Future Directions

Several unanswered questions about this phenomenon remain. First, although the studies were incentive-compatible, the stakes in studies 5 and 6–6e were admittedly modest. Moreover, all of the studies were conducted on online survey platforms (MTurk and CloudResearch Connect). Recent work (Chmielewski and Kucker 2020; Westwood 2025; Zhang *et al.* 2026) has highlighted the decline in data quality (and potential rise in AI agents), particularly on MTurk. Most of the MTurk studies (10 of 13) were collected before 2024, when AI agents were far less of a concern. We also included several exclusion criteria in each study (e.g., comprehension checks and/or response consistency requirements) to maintain a high level of data quality. We are reassured by the fact that the undervaluation effect is robust to various inclusion and exclusion criteria (studies 1c, 4, and 6e).

FIGURE 8

HETEROGENEITY AND UNDERVALUATION.



Note.—Relationship between heterogeneity and expected undervaluation of a multi-option alternative, as a function of knowledge or targeting ability. Expected undervaluation increases with a decrease in heterogeneity (i.e., as preferences become more homogenous) and as knowledge or targeting ability increases. Lines for {0.25, 0.5, 0.75, 1} represent best-fit lines to our simulations. See Web Appendix N for additional details.

One future direction might be an investigation into the construal of multi-option alternatives. In the introduction, we gave the example of a Japanese restaurant with both sushi and ramen. Contained within each of these sub-options may be additional sub-options. The level at which consumers construe these multi-option alternatives (and whether or when they perceive them to be multi-option alternatives or single-option alternatives) may be a fruitful endeavor for future research.

Another promising avenue for future research would include consideration of which consumers are more likely to exhibit stronger effects. We began to explore this in studies 5 and 6c, but our initial findings did not yield diagnostic results. We hypothesized that consumers who were higher on maximizing (Nenkov *et al.* 2008), elaborated more extensively on potential outcomes (Nenkov *et al.* 2009), exhibited stronger needs for

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cognitive closure (Kruglanski *et al.* 1993) or who used more analytic rather than holistic approaches to thinking through decisions (Choi *et al.* 2007) would exhibit less undervaluation. However, none of these hypotheses held. Given the finding of apparent heterogeneity in our studies, identifying the source of such heterogeneity would be informative.

Conclusion

The present research identifies and investigates a curious pattern of choice results: decision makers often choose against their best interests when confronted with a multi-option alternative. This finding has important implications for consumers and practitioners. Offering a multi-option alternative may appear like an attractive approach to increase choice for one's product(s) as it enables appeal across consumers with heterogeneous preferences. But if the options within the alternative are discrepant in value, our results suggest that choice likelihood for that alternative may be lower than it would be otherwise for any given consumer. It is not clear from the present research whether knowledge of this tendency would reduce or reverse the undervaluation effect, but it is important for marketing managers and choice architects to be aware of this way in which consumers' choices defy value-maximizing expectations.

DATA COLLECTION STATEMENT

The first and second authors jointly managed the collection of data for experiment 1 using CloudResearch in October 2020. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 1b using CloudResearch in October 2020. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 1c using CloudResearch in July 2024. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 1d using CloudResearch in December 2025. The data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 2 using CloudResearch in August 2023. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 2b using CloudResearch in March 2023. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 3 using CloudResearch in October 2021. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 3b using CloudResearch in July 2021. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 4 using CloudResearch in July and August 2025. The data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 5 using CloudResearch in March 2026. The data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 6 using CloudResearch in March 2026. The data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 6b using CloudResearch in July 2020. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 6c using CloudResearch in October 2020. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 6d using CloudResearch in May 2020. These data were analyzed by the first author. The first and second authors jointly managed the collection of data for experiment 6e using CloudResearch in July 2020. These data were analyzed by the first author. Data files and study materials are available in a project directory on ResearchBox at https://researchbox.org/124.

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