Journal of Experimental Social Psychology 112 (2024) 104575

Contents lists available at ScienceDirect

Journal of Experimental Social Psychology

journal homepage: www.elsevier.com/locate/jesp

# Choosing not to get anchored: A choice mindset reduces the anchoring bias☆

Krishna Savani [a,1,*], Monica Wadhwa [b,1,*]

[a] The Hong Kong Polytechnic University, Li Ka Shing Tower M857, Hung Hom, Kowloon, Hong Kong
[b] Department of Marketing, Fox School of Business, Temple University, Alter Hall, 1801 Liacouras Walk, Philadelphia, PA 19122, United States of America

ARTICLE INFO

*Keywords:*
Anchoring
Bargaining
Negotiations
Choice mindset
Nudge

ABSTRACT

In negotiations, first offers serve as potent anchors. After receiving a first offer, although people clearly have a choice about what amount to counteroffer, they often fail to adjust away from the first offer. We identify a simple nudge, a reminder that people have a choice, that can reduce the anchoring bias. We argue that a choice nudge leads people to think of more potential counteroffers that they can make, which reduces the extent to which they are anchored to the first offer. Seven studies conducted with US residents recruited from online research platforms tested this hypothesis. We found that merely reminding buyers that they have a choice led them to anchor away from sellers' first offers in a painting buying task (Studies 1 and 2) and a used car negotiation (Study 3). A choice reminder nudged people to consider more counteroffers (Study 4a) and asking people to consider more counteroffers reduced the anchoring bias (Study 4b). Consistent with the idea that thinking of counteroffers requires cognitive resources, we found that the effect of a choice nudge is attenuated under high cognitive load (Study 5). Study 6 ruled out an alternative motivational account for the choice nudge effect. This research contributes to the choice mindset literature by showing that highlighting the semantic concept of choice can help correct a pervasive decision-making bias, and to the anchoring literature by showing that thinking of more counteroffers can reduce the anchoring bias, at least in contexts in which the direction of adjustment from the anchor is known.

The concept of choice is pervasive in our everyday lives (Schwartz, 2000, 2004). Extensive research in psychology, behavioral economics, and marketing has examined how the freedom to choose influences people's motivation and overall well-being (e.g., Patall, Cooper, & Robinson, 2008; Schwartz & Cheek, 2017). Researchers studying choice have largely assumed that choice is an objective state of the world—people either have multiple options to choose from, or they lack both options and freedom to choose. However, recent research suggests that choice could also be a subjective perception—sometimes, even when people don't have many options to choose from, they believe that they have a choice, whereas on other occasions, even when people have multiple options available, they don't realize that they have a choice (Savani, Markus, Naidu, Kumar, & Berlia, 2010). Accordingly, recent research has proposed that choice can also be a mindset, "a state of mind in which people perceive their own and others' actions through a lens of choice" (Ma, Yang, & Savani, 2019, p. 2). We argue that if choice is indeed a subjective state, could merely reminding people that they have a choice influence their subsequent decisions? In the current research, we examine this question in the context of anchoring. Specifically, we ask whether mere reminders of choice can reduce the anchoring bias.

## 1. The anchoring bias

The anchoring bias refers to people's tendency to base their judgments on numbers that happen to be salient at the time of the judgment. In the original demonstration of this bias, participants first spun a wheel of fortune, which landed on either 10 or 65, and then estimated the percentage of African countries in the United Nations (Tversky & Kahneman, 1974). The researchers found that participants' responses were biased toward the numbers randomly generated on the wheel of fortune; the median estimate for participants who received an anchor of 10 was 25%, whereas the median estimate for those who received an anchor of

☆ This paper has been recommended for acceptance by Ed Hirt.

* Corresponding authors.
*E-mail addresses:* krishna.savani@polyu.edu.hk (K. Savani), monica.wadhwa@temple.edu (M. Wadhwa).

[1] Both authors contributed equally and are listed in alphabetical order of their last names.

https://doi.org/10.1016/j.jesp.2023.104575
Received 11 July 2022; Received in revised form 23 October 2023; Accepted 27 November 2023
Available online 12 January 2024
0022-1031/

65 was 45%. Since this compelling demonstration, the anchoring bias has been documented in numerous real-world contexts, including consumer spending (Critcher & Gilovich, 2007), real estate prices (Bucchianeri & Minson, 2013), and credit card payments (McHugh & Ranyard, 2016).

Multiple explanations have been proposed to explain the process underlying anchoring effects. One explanation is that people first make a judgment about whether the value to be estimated is above or below the anchor, but then fail to sufficiently adjust their estimate from the anchor (Tversky & Kahneman, 1974). This explanation has been particularly useful to explain anchoring effects in paradigms in which people are aware of the direction in which they need to adjust their response from the anchor. To illustrate, if people are asked to guess the year George Washington was elected president, a logical anchor would be 1776, the year the US gained independence. Participants would likely assume that the president was elected a few years later, so they would come up with a response by adjusting a few years after 1776. Recent research has documented that people fail to sufficiently adjust their estimate from the anchor because they do not put in enough cognitive effort in the adjustment process—when people were incentivized to think through and revise their estimate, the anchoring bias was reduced (Simmons, LeBoeuf, & Nelson, 2010).

Other research has also provided evidence for the insufficient adjustment argument. Specifically, when people know the direction in which their response should be adjusted from the anchor, factors that could influence their motivation or ability to thoroughly engage in the adjustment process influence their extent of anchoring. For example, making people angry motivates them to engage in the adjustment process with enhanced vigor, which reduces anchoring (Inbar & Gilovich, 2011). Moreover, reducing the availability of cognitive resources, which interferes with people's ability to come up with different estimates, increases the anchoring bias (Epley & Gilovich, 2006; Inbar & Gilovich, 2011; Kruglanski & Freund, 1983). In a similar vein, people are more likely to anchor to salient numbers when solving math problems if they are under time pressure (Smith & Windschitl, 2011).

Another explanation of the anchoring bias is the selective accessibility of information related to the anchor. This explanation is particularly relevant to the standard paradigm in which anchors are either randomly selected or provided by the experimenter and people are not aware of the direction in which they need to adjust their response from the anchor. In such paradigms, people are first asked to judge whether the true value is above or below the anchor, and then asked to make an estimate. Continuing with the African Nations example, before providing their final estimate, some participants were first asked if the percentage of African nations in the UN was below or above 10, whereas others were asked if it was above or below 65. This comparative assessment increases the accessibility of information activated in memory by the anchor (Mussweiler & Strack, 1999). For example, people exposed to the anchor of 10 might be more likely to recall information consistent with the idea that there are only a few countries in Africa (e.g., "Africa has a few big countries, such as Nigeria, Ethiopia, Egypt, Kenya, Sudan and South Africa"). In contrast, those exposed to the anchor of 65 might be more likely to recall information consistent with the idea that Africa has a lot of countries (e.g., "the map of western Africa appears very fragmented"). As accessibility effects are automatic, this research predicts that factors that can impact people's motivation or ability to adjust, such as time pressure and cognitive load, do not influence the extent of anchoring in situations in which they are not aware of the direction in which they need to adjust from the anchor (Mussweiler & Strack, 1999, 2000).

Researchers have also provided an attitudinal explanation of the anchoring effect in standard paradigms in which the anchors are randomly generated, and thus, it is not clear whether the true value lies above or below the anchor (Blankenship, Wegener, Petty, Detweiler-Bedell, & Macy, 2008; see also Wegener, Petty, Blankenship, & Detweiler-Bedell, 2010). Blankenship, Wegener, Petty, Detweiler-Bedell and Macy (2008) exposed all participants to varying background information before the anchoring task, and then assigned them to either a high or a low cognitive load condition. They found that under low cognitive load, participants' estimates were not only influenced by randomly generated numerical anchors but also by background information that was made accessible before the anchoring task. However, in the high cognitive load condition, the anchoring effect was primarily influenced by the salient anchors, not by the background information. Importantly, the anchoring effect persisted longer in low cognitive load condition than in the high load condition. Thus, consistent with the selective accessibility account, this attitudinal account of anchoring suggests that people use relevant information that is accessible at the time of judgment. However, drawing on attitude theories, this account suggests that people can more effectively access relevant information when they have the mental capacity to do so.

In the current research, we focus on an important real-life context—the amount people are willing to pay in scenarios where there is room to bargain. Unlike in research studying anchoring effects using the standard paradigm, in such bargaining scenarios, the anchors are not randomly generated and the direction in which people need to adjust from the anchor is obvious (e.g., buyers know that they need to adjust the price downwards). Thus, in the current research, drawing upon the insufficient adjustment explanation, we expect that the extent to which people are anchored to the asking price should be reduced when people are exposed to a nudge that can encourage them to more thoroughly think through the adjustment process.

## 2. Choice mindset and anchoring bias

A choice involves selecting one or more options from a larger set of available alternatives. For some choices, one option is clearly superior to the other options; thus, people can relatively quickly make such choices without much deliberation. However, in most choice contexts, decision-makers are likely to face alternatives that involve tradeoffs (e.g., the better-quality product is also more expensive, whereas the cheaper product is of lower quality; Dhar & Gorlin, 2013). When making such choices, people first carefully examine the options, focus their attention on options within the final consideration set, and then choose the option they find the most attractive (Bettman, Johnson, & Payne, 1990; McNeill & Wyeth, 2011). Consistent with this idea, an eye-tracking study found that when participants were making choices, they spent most of their time processing selected options more thoroughly before making their final selection (Russo & Leclerc, 1994).

Recent research has found that in addition to being a behavioral act, choice is also a psychological state of mind. Even when people's objective choices are held constant, there is variation in how many choices people perceive as having and making (Savani et al., 2010). Even in the absence of making an actual choice, when the semantic concept of choice is activated, people view their own and others' behaviors through the lens of choice (Savani & Rattan, 2012; Savani, Stephens, & Markus, 2011). When a choice mindset is activated, people believe that they themselves and others have multiple options available (Ma et al., 2019; Nanakdewa, Madan, Savani, & Markus, 2021; Wang & Savani, 2022). Moreover, a choice mindset enhances analytic thinking (Savani, Stephens, & Markus, 2017), which in turn nudges people to think through their decisions more thoroughly; in particular, analytic thinking has been found to weaken framing effects (Kokis, Macpherson, Toplak, West, & Stanovich, 2002; McElroy & Seta, 2003). Drawing upon a synthesis of this research, we argue that in a bargaining context, a choice mindset should lead people to consider multiple options about how they can respond to a first offer. In other words, a choice mindset should make people consider more potential counteroffers that they can make, which should help them adjust their counteroffer further away from the first offer (i.e., the anchor).

Thinking of more potential estimates or counteroffers requires cognitive resources as people need to mentally scan the feasible option

space (Hammond, Keeney, & Raiffa, 2015). Therefore, we further propose that as people need cognitive resources to thoroughly think about the potential options that they have, the effect of a choice mindset on anchoring would be attenuated when people are under cognitive load. In contrast, if the effect of a choice mindset on anchoring occurs due to non-cognitive factors, such as increased self-relevance of the decision, then this effect should not be affected by cognitive load. Thus, manipulating cognitive load helps us test whether a choice nudge influences anchoring through a cognitive process.

By documenting these effects, the current research seeks to make three important contributions. First, anchoring has been shown to be pervasive even in high-stakes decisions. If a simple choice nudge can reduce the anchoring bias, it could significantly improve the quality of people's decisions. Second, although extensive research has investigated how giving people multiple options to choose from influences their motivation and well-being (Patall et al., 2008), the current research would be among the first to examine how mere reminders of choice influence people's judgments and decision-making. Third, the current research is the first to document that a choice nudge can make people think of more estimates, and that thinking of more estimates reduces the anchoring bias. These findings contribute to both the choice mindset and anchoring literatures.

## 3. Overview of studies

We tested our hypothesis across seven studies. Study 1 used a between-participants design to assess whether a choice nudge reduces anchoring to first offers in a paintings purchase task. Study 2 sought to provide a pre-registered replication using a within-participant design. Study 3 was designed to provide yet another pre-registered replication using a used car bargaining scenario. Using an experimental causal chain design (Spencer, Zanna, & Fong, 2005), Studies 4a and 4b tested whether a choice nudge leads people to consider more counteroffers when thinking of what offer to make in a paintings purchase task, and whether thinking of more counteroffers reduces the extent of anchoring to first offers. Study 5 examines whether the effect of a choice nudge would be attenuated when participants' cognitive resources were constrained. Finally, Study 6 examined an alternative motivational account, which suggests that a choice nudge could increase the importance of the task or increase people's motivation to engage with the task.

In all experiments, we report all experimental conditions, measures, and participants. Sample size was determined before any data analysis. We report all numbers with two significant digits after the decimal point. The questionnaires, data, and analysis code are available on https://osf.io/rmkp6/. The hypotheses, sample size, methods, and analysis plan of Studies 2, 3, 4a, 4b, 5 and 6 were pre-registered. As the data quality from Amazon Mechanical Turk (MTurk) participants often decreases in quality if we seek a large number of participants at once, we sought Mturk participants in batches of 200; however, data were analyzed only after all batches were run. We restricted our sample to participants residing in the US with an approval rating of 97% and with at least 100 complete HITs. Across all studies, we only retailed responses with a value of "1" under the "Finished" column in the raw data file downloaded from Qualtrics™. We deleted responses that reflected the research team's attempts to test the survey before analyzing the data.

## 4. Study 1

This study tested whether a choice nudge can encourage buyers to make offers that are further away from the seller's offer.

### 4.1. Method

**Participants.** Based on recent research, we expected an effect size of Cohen's $d = 0.30$ (the average effect size in Studies 1, 2, 3, and 5 in Ma et al., 2019). We sought 400 US residents on Mturk, which would give us approximately 85% power to detect an effect of Cohen's $d = 0.30$ with $\alpha = 0.05$ (two-tailed). In response, we obtained 385 complete responses. Eleven responses that bypassed a duplicate IP address filter built into the survey program were discarded because they could represent multiple responses from the same individuals. The final sample consisted of 374 participants (202 women, 156 men, 2 others, 14 missing responses; mean age 37.60 years, $SD = 11.36$). Participants were randomly assigned to either the choice condition or the control condition. The final sample size had 80% power to detect an effect size of $d = 0.29$ with $\alpha = 0.05$ (two-tailed).

**Procedure.** Participants were asked to imagine that they wanted to buy some paintings and had come to know of a street fair where painters sell their paintings. They were further informed: "You know that there is room for negotiation at the street fair." Thereafter, participants were shown 12 paintings, each with a quoted price that ranged from $100 to $200 in $10 increments. The order of the 12 paintings was fixed. In the control condition, participants were told, "The quoted price for this painting is $_. If you wanted to buy this painting, what amount would you offer (in dollars) for this painting?" In the choice condition, we added the following line immediately after the quoted price: "You can choose to offer any amount that you want for this painting. It's your choice!" Participants across conditions were only allowed to respond with a counteroffer between $0 and the quoted price.

### 4.2. Results

As the range of participants' counteroffers varied across trials based on the quoted price of the painting, for each trial, we converted participants' dollar values into ranks. This procedure puts the dependent variable on the same scale across different trials. Higher counteroffers received bigger ranks. Note that lower ranks indicate that participants' counteroffers were further away from the quoted price. See Table S1 in the Supplementary Materials for descriptive statistics for each trial. We averaged the rank of participants' counteroffers across all 12 trials, and found that participants in the choice condition made lower counteroffers, $M = 172.56$, 95% CI [158.91, 186.21], $SD = 92.31$, than those in the control condition, $M = 201.07$, 95% CI [189.91, 212.22], $SD = 79.20$, $t(372) = 3.21$, $p = .001$, Cohen's $d = 0.33$.

Thus, Study 1 found that in a buying task, asking participants to think about their choices when making counteroffers helped them move further away from the first offers in a negotiation task.

## 5. Study 2

The goal of this study was to provide a conceptual replication of Study 1. To increase our statistical power, we used a mixed within-between design in this and all subsequent experiments (Gelman, 2018).

### 5.1. Method

The methods and analyses for this study were pre-registered (https://osf.io/wavs8).

**Power analysis.** This and all subsequent studies used a within-participant design, and the data were analyzed using a hierarchical linear model (HLM; Raudenbush & Bryk, 2002). We conducted power analysis for a within-participant $t$-test assuming $d = 0.33$ (from Study 1), alpha = 0.05 (one-tailed), and power = 80%, which indicated that we need to recruit 59 participants. However, as the effect size obtained in a within-participant design might be lower than in Study 1's between-participant design, we decided to recruit 400 participants.

**Participants.** We pre-registered a sample size of 400. A survey seeking 400 US residents on Mturk elicited 451 complete responses. Six responses that bypassed a duplicate IP address filter built into the survey program were discarded because they could represent multiple responses from the same individuals. The final sample consisted of 445 participants (220 women, 180 men, 45 missing responses; mean age

38.17 years, $SD = 11.89$). The final sample size had 80% power to detect an effect size of $d_z = 0.13$ with $\alpha = 0.05$ (two-tailed).

**Procedure.** Participants were randomly assigned to one of two conditions. In one condition, the first six trials were in the control condition and the last six in the choice condition, and vice-versa in the other condition. Thus, the choice nudge was manipulated within-participants across trials and between-participants for a given trial. We used the same paintings negotiation task as in Study 1. The order of the 12 paintings was fixed for all participants.

### 5.2. Results

We pre-registered the data analysis procedure. For each trial, we first converted participants' dollar values into ranks. Higher counteroffers received bigger ranks, and lower ranks indicate that participants' counteroffers were further away from the quoted price. As per the pre-registered analysis plan, we analyzed the data using a hierarchical linear model (HLM; Raudenbush & Bryk, 2002), treating the 12 trials as nested within participants. The rank of participants' counteroffer was the dependent variable, and experimental condition (control = 0, choice = 1) and the quoted price were the predictors. The slope of the experimental condition was allowed to vary across participants randomly, and the between-participants covariance between the intercept and the slope was estimated (i.e., we ran a random slopes model). See Table S2 in the Supplementary Materials for descriptive statistics for each trial, and Table S3 in the Supplementary Materials for the variance parameters from the HLM.

The effect of the quoted price was non-significant, $b = 0.0023$, 95% *CI* [−0.073, 0.068], $SE = 0.036$, $z = 0.06$, $p = .949$. Importantly, there was a significant effect of the experimental condition, $b = -5.80$, 95% *CI* [−10.78, −0.83], $SE = 2.54$, $z = 2.30$, $p = .011$ (one-tailed, given the pre-registered directional hypothesis; $p = .022$, two-tailed). The negative sign of the coefficient indicates participants counteroffered with smaller amounts in the choice condition than in the control condition, and thus, the size of the anchoring effect was significantly smaller in the choice condition than in the control condition.

In additional analyses, we tested whether the effect of the experimental manipulation varied across the two between-participant conditions (some saw the choice condition first and others saw the choice condition second); however, there was no interaction between the within-participant variable (choice vs. control) and the between-participant variable (order of the two conditions), $p = .97$. Additionally, there was no interaction between the within-participant variable (choice vs. control) and participants' gender, $p = .85$.

Thus, Study 2 conceptually replicated the findings of Study 1 using a within-participants design. Once again, nudging people to think about their choices helped them make counteroffers that were further away from the first offers in a negotiation task.

## 6. Study 3

A goal of Study 3 was to conceptually replicate the finding of Study 2 using a different buying scenario. Specifically, we simulated a used car buying decision. In the US, buyers typically make their own counteroffers when purchasing both new cars and used cars, thus making this an important context to examine. Further, in Studies 1 and 2, we used only round numbers as first offers (i.e., the last digit of the first offer was always zero. Research suggests that precise offers lead people to anchor more to first offers than round offers (Loschelder, Stuppi, & Trötschel, 2014; Mason, Lee, Wiley, & Ames, 2013). Therefore, in this study, we tested whether our finding replicates when we use precise numbers as first offers.

### 6.1. Method

The methods and analyses for this study were pre-registered (https://osf.io/k2c3t).

**Participants.** We pre-registered a larger sample size of 600 for this study. A survey seeking 600 US residents on Mturk elicited 857 complete responses. We seem to have obtained 257 additional responses because we were piloting an unrelated survey after the current study, and a large number of participants quit the survey after completing the current study but before completing the pilot survey. Twelve responses that bypassed a duplicate IP address filter built into the survey program were discarded because they could represent multiple responses from the same individuals. The final sample consisted of 845 participants (309 women, 281 men, 1 others, 254 missing responses; mean age 36.91 years, $SD = 11.11$). The final sample size had 80% power to detect an effect size of $d_z = 0.10$ with $\alpha = 0.05$ (two-tailed). As in Study 2, we employed a mixed within-between design in this study.

**Procedure.** Participants were asked to imagine they were on the market to buy a used car and subsequently viewed various used cars on offer by a dealer. They were further informed: "Note that there is often quite some room to negotiate the price of used cars with dealers." Thereafter, participants were presented with 6 cars, one on each screen. We showed a photograph of each car, information about key attributes, and a quoted price, ranging from $15,599 to $19,781. The order of the six cars was fixed. For about half the participants, the first three cars were in the control condition and the last three in the choice condition, and vice-versa for the remaining participants.

In the control condition, participants were told, "The dealer has quoted you a price of $__. If you wanted to buy this car, how much would you offer (in dollars) for this car?" In the choice condition, we added this line immediately after the quoted price: "You can choose to offer any amount that you want for this car. It's your choice!" Participants were only allowed to respond with a counteroffer between $0 and the quoted price.

### 6.2. Results

Following pre-registered exclusion criteria, we excluded the counteroffers that were below $10,000 (5% of total counteroffers), as these likely represented non-serious responses. We also excluded 17 offers above the quoted price, bypassing the restriction built into the survey. For each trial, we converted participants' dollar values into ranks. Higher counteroffers received bigger ranks, and lower ranks indicate that participants' counteroffers were further away from the quoted price. As per the pre-registered analysis plan, we analyzed the data using a hierarchical linear model treating the 6 trials as nested within participants. The rank of participants' counteroffer was the dependent variable, and experimental condition (control = 0, choice = 1) and the quoted price were the predictors. The slope of the experimental condition was allowed to vary across participants randomly, and the between-participants covariance between the intercept and the slope was estimated (i.e., we ran a random slopes model). See Table S4 in the Supplementary Materials for descriptive statistics for each trial, and Table S5 in the Supplementary Materials for the variance parameters from the HLM.

The effect of the quoted price was non-significant, $b = -0.0018$, 95% *CI* [−0.005, 0.001], $SE = 0.001$, $z = -1.24$, $p = .215$. Importantly, there was a significant effect of the experimental condition, $b = -12.38$, 95% *CI* [−21.31, −3.45], $SE = 4.56$, $z = -2.72$, $p = .003$ (one-tailed, given the pre-registered directional hypothesis; $p = .007$, two-tailed). The negative sign of the coefficient indicates participants counteroffered with smaller amounts in the choice condition than in the control condition, and thus, the size of the anchoring effect was significantly smaller in the choice condition than in the control condition. In non-preregistered analyses, we tested whether this effect held while including all counteroffers below the quoted price without the $10,000 exclusion, and found that it did: $b = -11.56$, 95% *CI* [−20.64, −2.48], $SE = 4.63$, $z = 2.49$, $p = .013$. The average effect size across all trials was Cohen's $d = 0.27$ (see Table S1 in Supplementary Materials).

Thus, Study 3 conceptually replicated the finding of Study 2 using a different negotiation task in which we used precise offers as anchors. Once again, merely asking people to think about their choices led to counteroffers that were further away from the seller's first offer.

## 7. Study 4a

Our conceptualization suggests that a choice nudge can make people think of more counteroffers, which, in turn reduces anchoring. This is because a key psychological mechanism underlying the anchoring effect is that the anchor dominates people's thoughts (Tversky & Kahneman, 1974). The more options participants think of, chances are that they will think of a wider range of options, thereby weakening the psychological predominance of the anchor. We test the first part of our causal chain in this study—impact of choice nudge on the number of different offers people consider in a bargaining context.

### 7.1. Method

The methods and analyses for this study were pre-registered (https://osf.io/4r9yu).

**Participants.** We pre-registered a sample size of 216 for this study. A survey seeking 216 US residents on Prolific Oxford elicited 218 complete responses. Eight responses that bypassed a duplicate IP address filter built into the survey program were discarded because they could represent multiple responses from the same individuals. The final sample consisted of 210 participants (107 women, 101 men, 2 missing responses, mean age 42.99 years). The final sample size had 80% power to detect an effect size of $d_z = 0.17$ with $\alpha = 0.05$ (one-tailed). This study employed a between-subjects design with choice reminder as the manipulated factor.

**Procedure.** As in Study 3, participants were asked to imagine that they wanted to buy some paintings and had come to know of a street fair where painters sell their paintings. Thereafter, participants were shown 12 paintings, each with a quoted price that ranged from $100 to $200 in $10 increments. The order of the 12 paintings was fixed. In the control condition, participants were told, "The quoted price for this painting is $_. Imagine you are interested to buy this painting and are thinking of what offer to make. What are the different offers you think you can make for this painting." Participants were then asked to write down all the potential offers that came to their mind, one in each line; there were ten separate lines. In the choice condition, we added the following line immediately after the quoted price: "You can choose to offer any amount that you want for this painting. It's your choice!"

### 7.2. Results

We computed the number of numeric offers that participants generated in each trial (while ignoring any text entries). We analyzed the data in a long format with trials nested within participants. We ran a similar HLM as in Studies 2 and 3. The number of counteroffer options generated was the dependent variable, and experimental condition (control = 0, choice = 1) was the predictor. See Table S6 in the Supplementary Materials for descriptive statistics for each trial, and Table S7 in the Supplementary Materials for the variance parameters from the HLM. There was a significant effect of the experimental condition, $b = 0.17$, 95% *CI* [0.06, 0.27], $SE = 0.53$, $z = 3.17$, $p = .001$ (one-tailed, given the pre-registered directional hypothesis; $p = .002$, two-tailed). The positive sign of the coefficient indicates that participants generated more counteroffer options in the choice condition than in the control condition.

In additional non-preregistered analyses, for each painting, we computed the range of the counteroffers that participants generated (while only including valid counteroffers that were equal to or below the quoted price). A parallel HLM to the one reported above found that the counteroffer range was non-significantly wider in the choice condition than in the control condition, $b = 1.24$, 95% *CI* [−0.31, 2.79], $SE = 0.79$, $z = 1.56$, $p = .059$ (one-tailed; $p = .118$, two-tailed).

Study 4a provides support for our conceptualization that a choice nudge makes people think of more counteroffer options upon being faced with a first offer. When a choice reminder was made salient, participants thought of more offers that they could make, compared with when the choice reminder was missing.

## 8. Study 4b

Findings from Study 4a support our hypothesis that a choice nudge can make people think of more offers. In this study, we examine whether thinking of a higher number of offers can reduce anchoring. We further test whether participants in the more counteroffers condition generated a wider range of options than those in the fewer counteroffers condition, and whether this difference explains lower anchoring in the more counteroffers condition.

### 8.1. Method

The methods and analyses for this study were pre-registered (https://osf.io/8vkbf).

**Participants.** We pre-registered a sample size of 200 for this study. A survey seeking 200 US residents on Prolific Oxford elicited 201 complete responses (101 women, 96 men, 3 missing responses, mean age 42.72). The final sample size had 80% power to detect an effect size of $d_z = 0.20$ with $\alpha = 0.05$ (two-tailed). This study employed a between-subjects design.

**Procedure.** As in Study 4a, participants were asked to imagine that they wanted to buy some paintings and had come to know of a street fair where painters sell their paintings. Thereafter, participants were shown 12 paintings, each with a quoted price that ranged from $100 to $200 in $10 increments. The order of the 12 paintings was fixed. In the control condition, participants were told, "The quoted price for this painting is $_. Imagine you are interested to buy this painting and are thinking of what offer to make. What are the different offers you think you can make for this painting." Participants were then asked to write down either two potential offers or eight potential offers that came to their mind. Subsequently, participants were asked "The quoted price for this painting is $_. If you wanted to buy this painting, what amount would you offer (in dollars) for this painting?"

### 8.2. Results

As pre-registered, we excluded seven participants who did not generate the number of options that they were asked to generate in the instructions in two or more trials. The analysis procedure was virtually identical to that used in Study 2. The rank of the counteroffer was the dependent variable, experimental condition (two counteroffers = 0, eight counteroffers = 1) was the predictor, and the quoted price was a control variable. See Table S8 in the Supplementary Materials for descriptive statistics for each trial, and Table S9 in the Supplementary Materials for the variance parameters from the HLM.

The effect of the quoted price was non-significant, $b = 0.0053$, 95% *CI* [−0.041, 0.051], $SE = 0.024$, $z = 0.23$, $p = .82$. Importantly, there was a significant effect of the experimental condition, $b = -6.44$, 95% *CI* [−10.51, −2.37], $SE = 2.08$, $z = 3.10$, $p = .001$ (one-tailed, given the pre-registered directional hypothesis; $p = .002$, two-tailed). The negative sign of the coefficient indicates participants counteroffered with smaller amounts in the "eight counteroffers" condition than in the "two counteroffers" condition, and thus, the size of the anchoring effect was significantly smaller in the more counteroffers condition.

In additional non-preregistered analyses, for each painting, we computed the range of the options that participants generated (while only including valid options that were equal to or below the quoted price). A parallel HLM to the one reported above found that as expected,

the options range was wider when the quoted price was higher, $b = 0.15$, 95% *CI* [0.13, 0.17], $SE = 0.0082$, $z = 18.32$, $p < .001$. Importantly, there was a significant effect of the experimental condition, $b = 29.57$, 95% *CI* [27.44, 31.69], $SE = 1.08$, $z = 27.31$, $p < .001$. The positive sign of the coefficient indicates participants generated a wider range of options in the "eight counteroffers" condition than in the "two counteroffers" condition.

In a follow-up HLM model, we used participants final counteroffer rank as the outcome variable, and the quoted price, the experimental condition, and the options range as predictors. The effect of the quoted price was significant, $b = 0.064$, 95% *CI* [0.015, 0.113], $SE = 0.025$, $z = 2.57$, $p = .010$. The effect of the options range was also significant, $b = -0.39$, 95% *CI* [−0.50, −0.28], $SE = 0.57$, $z = 6.86$, $p < .001$. The effect of experimental condition was now non-significant, $b = 5.16$, 95% *CI* [−0.27, 10.35], $SE = 2.65$, $z = 1.95$, $p = .051$, and even flipped in direction. Thus, conceptually, the effect of the number of counteroffers condition was mediated by the wider range of counteroffers generated in the "eight counteroffers" condition than in the "two counteroffers" condition (Baron & Kenny, 1986).

### 8.3. Discussion

Together findings of Studies 4a and 4b show that a choice nudge can make people think of more options, as measured by the number of offers participants thought of (Experiment 4a) and thinking of more offers can reduce anchoring (Study 4b), thereby establishing an experimental causal chain. Additionally, there is some evidence that generating more counteroffers reduces anchoring because participants generate a wider range of offers. In Study 4b, the effect of the number of counteroffers manipulation was fully explained by the range of the counteroffers generated.

## 9. Study 5

One goal of Study 5 was to provide stronger support for the underlying mechanism. Our conceptualization suggests that the choice nudge reduces anchoring through a cognitive mechanism—it makes people consider more counteroffers (Stephens & Levine, 2011). If this is true, then restricting people's cognitive resources, which would be needed to think about more counteroffers (Shiv & Fedorikhin, 1999, 2002; Wadhwa & Zhang, 2015), should eliminate the effect of a choice nudge. A second goal of this study was to examine an alternative possibility which suggests that the choice nudge reduces anchoring through a self-serving motivational mechanism; that is, it makes people focus on their self-interest, which leads them to make more aggressive counteroffers. If this alternative account is valid, then limiting people's processing resources should not affect anchoring, given that a focus on "self-interest is automatic, viscerally compelling, and often unconscious" (Moore & Loewenstein, 2004, p. 189). To rule in a cognitive mechanism, we used a divided attention paradigm to tax people's processing resources in this study (Brandstätter, Lengfelder, & Gollwitzer, 2001; Brünken, Steinbacher, Plass, & Leutner, 2002).

### 9.1. Method

The methods and analyses for this study were pre-registered (https://osf.io/eh5kg).

**Participants.** We pre-registered a sample size of 400, which was determined by the consistent replicability of our choice nudge paradigm in prior studies. A survey seeking 400 participants was posted on the student subject pool at a university on the US East Coast. In response, 359 participants completed the study (125 women, 234 men; mean age 20.20 years, $SD = 1.58$). The study design was a 2 (choice nudge: absent vs. present, within-participants) X 2 (processing load: absent vs. present, between-participants). The final sample size had 80% power to detect an effect size of $d_z = 0.21$ of the choice nudge with $\alpha = 0.05$ (two-tailed) within each of the two between-participant conditions, and 80% power to detect an effect size of $d = 0.30$ with $\alpha = 0.05$ (two-tailed) across the two between-participant conditions.

**Procedure.** The choice manipulation and the negotiation tasks used in this study were identical to those used in Study 2. In the *processing resources-constrained* condition, logos of different brands, including social media brands, were flashed on the screen while participants were engaged in the negotiation task. As companies frequently place advertisements on the right side of the screen (Chae & Hoegg, 2013), we flashed logos on the right side of the screen. Logos of 29 brands, including 15 social media brands (e.g., Facebook, Twitter) and 14 other retail brands (e.g., Target, Macy's), were flashed in random order for two seconds each. Participants were asked to count the number of times logos of social media brands were flashed. In the instructions preceding this task, we provided a list of the 15 social media brands. After logos of all 29 brands were displayed once, they kept repeating in random order. To mirror real-world experience, the horizontal and vertical locations of the logos on the right third of the screen were also randomized. At the end of the negotiation task, participants were asked to indicate the number of times logos of social media brands were flashed, and to ignore the logos from then on. In the *processing resources-unconstrained* condition, the right side of the screen was blank.

Thereafter, given past experience with this subject pool, we administered an attention check: participants were presented with four multiple-choice questions in which they had to identify one of four words that was closest in meaning to a target word (adapted from Chandler, Rosenzweig, Moss, Robinson, & Litman, 2019). As per pre-registered criteria, we excluded 107 participants who failed this attention check. See Supplementary Materials for additional exploratory measures that were included but not pre-registered.

### 9.2. Results

In the pre-registration, we proposed to first examine the 95% CI of the residual trial-level variance in each condition and then combine the two conditions into an overall model only if the homogeneity of variance assumption was valid. However, the 95% confidence interval in each condition did not contain the estimate of the other condition (see Tables S10a and S10b in the Supplementary Materials), indicating that the two conditions have significantly different error variances. Thus, we only report results from the HLM models within each of the two processing conditions.

In the *processing resources-unconstrained* condition, we found a nonsignificant effect of the quoted price, $b = -0.034$, 95% *CI* [−0.13, 0.067], $SE = 0.051$, $z = 0.66$, $p = .511$. As predicted, the effect of the choice condition was significant, $b = -7.96$, 95% *CI* [−15.05, −0.87], $SE = 3.62$, $z = 2.20$, $p = .014$ (one-tailed, as we pre-registered a directional hypothesis; $p = .028$, two-tailed), indicating that participants showed lower anchoring in the trials in the choice condition than in the trials in the control condition. In the *processing resources-constrained* condition, we found a nonsignificant effect of the quoted price, $b = 0.071$, 95% *CI* [−0.034, 0.18], $SE = 0.054$, $z = 1.32$, $p = .186$. The effect of the choice condition was nonsignificant, $b = 1.41$, 95% *CI* [−7.60, 10.42], $SE = 4.60$, $z = 0.31$, $p = .759$, indicating that participants showed similar degrees of anchoring in the trials in both the choice condition and the control condition. In particular, the effect of the choice nudge disappeared once participants' processing resources were constrained. Further, the 95% confidence interval of the *beta* coefficient of the choice nudge in each condition did not contain the *beta* coefficient of the other condition.

In additional analyses reported in the Supplementary Materials, we found that participants' negative mood was similar across the two processing resources conditions; however, those in the *resources-unconstrained* condition had a higher positive mood. Including positive mood as a covariate in the analysis did not influence the results, and participants' positive mood was unrelated to their counteroffers in the

negotiation task.

### 9.3. Discussion

This experiment provides further evidence for our proposed cognitive mechanism, suggesting that a choice reminder nudges people to think more thoroughly about the offers that they can make. Consistent with the findings of the previous studies, in the control condition, the choice nudge helped people make counteroffers that were further away from the first offer. However, when participants were required to count logos displayed on the screen while working on the negotiation task, the choice nudge had no effect. The findings are consistent with the idea that the effect of the choice nudge in the negotiation task runs through a cognitive mechanism. An alternative motivational account would suggest that a choice nudge can make people focus on their self-interest, which leads people to make lower counteroffers, However, focus on self-interest does not require cognitive resources as it is automatic (Moore & Loewenstein, 2004, p. 189). Thus, if self-serving motivation was driving the effect of a choice nudge on anchoring, then restraining cognitive resources should not have reduced anchoring.

## 10. Study 6

Study 6 sought to achieve two goals. First, we tested the motivational account more directly by measuring participants' motivation to get a low price in the bargaining task. Additionally, we also measured how important it was for participants to get a low price on the bargaining task. If a choice nudge motivates people to focus on their self-interest, participants in the choice condition should report higher motivation to get a low price than those in the control condition and should perceive the task as more important to them. Second, Study 6 provided a more stringent test for our hypothesis. In the previous studies, participants in both the control and choice conditions were informed that there is often quite some room to negotiate; however, in the choice, we had specifically stated, "You can choose to offer any amount that you want" but there were no parallel instructions in the control condition. To address this issue, in the current study, we reminded participants in both conditions that they could offer any amount they wanted.

### 10.1. Method

The methods and analyses for this study were pre-registered (https://osf.io/8b37z).

**Participants.** We pre-registered a sample size of 200. A survey seeking 200 US residents on Prolific elicited 200 complete responses, all from unique IP addresses (96 women, 100 men, 4 of other gender; mean age 44.31 years, $SD = 14.62$). The final sample size had 80% power to detect an effect size of $d_z = 0.18$ with $\alpha = 0.05$ (one-tailed).

**Procedure.** The procedure, using a mixed within-between design, was very similar to that of Study 2—participants viewed the same 12 paintings in a fixed order. There were two major changes. First, to make the conditions more parallel, we instructed participants in the control condition: "You can offer any amount." In the choice condition, we instructed: "You can choose to offer any amount that you want for this painting. It's your choice!" Second, we informed participants that they would be going to two different street fairs to look at paintings. For half the participants, the first fair was assigned to the control conditions and the second fair to the choice condition, and vice-versa for the other half. For each fair, after they provided counteroffers to the six paintings, participants were asked two questions to measure their motivation to get a low price ("How motivated were you to get the lowest price for the paintings?" response scale: *0 = not at all motivated* to *8 = very motivated*, and "How much effort did you put in to come up with your counteroffer for the paintings?" response scale: *0 = no effort at all* to *8 = a lot of effort*; adapted from Burgmer & Englich, 2013) and two questions to measure the importance of the task ("How important was it for you to complete the paintings task effectively?" and "To what extent was getting the best price for the paintings important for you?" response scale: *0 = not at all important* to *8 = extremely important*; adapted from Fisher, Minbashian, Beckmann, & Wood, 2013).

### 10.2. Results

The two motivation items were intercorrelated ($\alpha_{control} = 0.70$, $\alpha_{choice} = 0.69$), as were the two importance items ($\alpha_{control} = 0.69$, $\alpha_{choice} = 0.72$). The four items combined were also intercorrelated ($\alpha_{control} = 0.81$, $\alpha_{choice} = 0.83$). Mean motivation was similar across the control condition, $M = 7.33$, 95% CI [6.98, 7.68], $SD = 2.52$, and the choice condition, $M = 7.45$, 95% CI [7.10, 7.79], $SD = 2.50$, $t(199) = 1.11$, $p = .27$. Mean importance was also similar across the control condition, $M = 7.34$, 95% CI [7.11, 7.56], $SD = 1.60$, and the choice condition, $M = 7.34$, 95% CI [7.12, 7.57], $SD = 1.63$, $t(199) = 0.22$, $p = .82$. The average of the motivation and importance items was also similar across the control condition, $M = 7.33$, 95% CI [7.06, 7.60], $SD = 1.93$, and the choice condition, $M = 7.40$, 95% CI [7.12, 7.67], $SD = 1.95$, $t(199) = 0.99$, $p = .32$. These non-significant differences indicate that participants' self-reported motivation to obtain a low price and importance of the task cannot account for any difference between the choice and control conditions in the anchoring effect.

For the main analysis, we pre-registered an identical data analysis procedure as in Study 2. Given the prior experiments and given our pre-registered directional hypothesis, we report one-tailed *t*-tests and 90% CI for the hypothesized effect. The effect of the quoted price was non-significant, $b = 0.00$, 95% CI [−0.044, 0.044], $SE = 0.022$, $z = 0.00$, $p = 1.00$. Importantly, there was a significant effect of the experimental condition, $b = -2.83$, 90% *CI* [−5.86, 0.21], $SE = 1.55$, $z = 1.83$, $p = .034$ (one-tailed, given the pre-registered directional hypothesis; $p = .068$, two-tailed). The negative sign of the coefficient indicates participants counteroffered with smaller amounts in the choice condition than in the control condition. See Table S11 in the Supplementary Materials for descriptive statistics for each trial, and Table S12 in the Supplementary Materials for the variance parameters from the HLM.

In three additional models, we added participants' motivation to obtain a low price, importance of the task, and the combined motivation-importance score as block-level predictors, one at a time. In all three models, the effect of the choice nudge was statistically significant ($p = .041$, 0.035, and 0.040, respectively, one-tailed), whereas the effect of the newly added variable was not ($p = .176$, 0.125, and 0.111, respectively, two-tailed).

### 10.3. Discussion

Study 6 improved upon the previous experiments in two respects. We used a tighter comparison between the choice and control conditions by informing participants in both conditions that they can offer any amount they want. Additionally, we found that participants' motivation to obtain a low price and the importance of the task did not vary by condition; nevertheless, participants exhibited lower anchoring in the choice condition, as documented in the previous studies. Thus, the current study failed to find support for a motivational account.

## 11. General discussion

Seven experiments documented that a simple choice nudge can reduce anchoring bias. Study 1 found that when asked to think about their choices in a paintings task, participants made offers that were further away from the first offer. Study 2 conceptually replicated this finding using a within-participant design. Study 3 provided another conceptual replication in a different domain—a used car purchase task. Studies 4a and 4b provided evidence for the idea that a choice nudge makes people consider more counteroffers (Study 4a), and thinking of more counteroffers reduces the anchoring bias (Study 4b). Providing

further support for the underlying mechanism, Study 5 shows that when people's processing resources were constrained using a dual-task paradigm, the choice nudge no longer helped people anchor away from the first offer. Finally, Study 6 ruled out motivation-related accounts—it is not the case that participants in the choice condition show less anchoring because they are more motivated to obtain a low price or because they perceive the task as more important.

### 11.1. Theoretical implications

The current research makes multiple theoretical contributions. First, the current research contributes to the literature on the choice mindset. Past research has largely used indirect manipulations of the choice mindset, such as asking people to recall their past choices, which lack ecological validity. In the current research, we developed a direct manipulation of the choice mindset in which people were instructed to think about their choices when making judgments and decisions. These findings indicate that a choice nudge can directly influence people's judgments. Additionally, this research is the first to show that a choice nudge can influence decision-making outcomes. Although past research showed that the choice mindset shapes people's attitudes and social judgments (Savani et al., 2011; Savani & Rattan, 2012), the current research shows that a choice nudge can also reduce a pervasive decision-making bias. These findings have wide-ranging implications for both social psychology and judgment and decision making, which have focused on documenting and correcting for numerous decision-making biases.

Second, much of past research in judgment and decision-making has examined factors that influence people's choices, but not how reminding people of their choices can impact their subsequent decision-making. The present research highlights that the concept of choice can influence people's judgments and decisions in unrelated tasks. Some research in social psychology has documented that making an atypically large number of choices (e.g., making 292 choices one after another) can lead to ego depletion (Vohs et al., 2008), which would predict that making choices should increase cognitive biases. In contrast, we find that merely thinking about choices reduces a decision-making bias. Thus, the current research suggests that making the concept of choice salient can improve people's decision making. However, it is possible that making many decisions in which people have to expend cognitive effort might deplete people's self-control resources and impair their decision making.

Our research also has important practical implications. The anchoring bias is one of the most prevalent decision-making biases, which can often negatively impact real-world decision-making. For example, anchoring bias could lead to sub-optimal outcomes in important negotiations (e.g., Galinsky & Mussweiler, 2001), including the most important negotiation in people's lives—buying real estate (e.g., Northcraft & Neale, 1987). Our findings suggest that people would benefit from a choice mindset when making important decisions.

While the anchoring bias can significantly impact decision-making, there are other significant biases, such as the default bias (Johnson & Goldstein, 2003), the status quo bias (Samuelson & Zeckhauser, 1988), and the availability bias (Tversky & Kahneman, 1974), all of which result in part from a failure to thoroughly consider all of one's options. To the extent that a choice mindset can lead people to thoroughly think through the available options, the current research suggests that many of these common decision-making biases can be reduced through a choice nudge.

Finally, our last experiment suggests that multitasking can have unintended consequences on the effectiveness of nudges. When participants were engaged in multitasking, the choice nudge no longer influenced the extent to which they moved away from the first offer. Given the frequency with which people multitask in their everyday lives (Kraushaar & Novak, 2010), this finding suggests that the effect of well-intentioned nudges introduced by policymakers can be disrupted. Future research can examine the impact of multitasking on the effectiveness of nudges in different contexts.

### 11.2. Limitations and future directions

Our research has multiple limitations. One limitation is that all studies used a bargaining scenario in which the anchors were externally provided and participants knew the direction in which they needed to adjust their offer from the anchor. In contrast, in many situations, externally provided anchors are randomly generated and the direction in which the response needs to be adjusted is not immediately clear. In such situations, when the direction is not known, it is likely that the choice nudge will lead people to consider alternatives both below and above the anchor. Thus, people are likely to consider a wider range of values. To the extent that choice nudge makes people think of more offers in both the directions, it might lead to a null effect overall.

Another limitation of this research is that the current studies focused on a cognitive mechanism to explain why the choice nudge reduces anchoring. The anchoring bias occurs due to two parallel processes—an automatic process (selective accessibility of the anchor) and a cognitive process (gradual adjustment of the estimate away from the anchor). Both processes are often simultaneously at play. As we hypothesized that a choice nudge reduces the anchoring bias by making people consider more offers that they can counter with, in the current research, we focused on elucidating the latter cognitive process. However, it is possible that in addition to encouraging a more thorough processing of the options available, the choice nudge also reduces anchoring because it dampens the effects of selective accessibility. Future research can study the impact of the choice nudge on automatic processes in anchoring.

Third, in this research, we primarily recruited participants located in the US from online research platforms, such as Amazon Mechanical Turk and Prolific Academic. Although online research platform participants are demographically similar to community participants (Goodman, Cryder, & Cheema, 2013) and are more ethnically diverse than undergraduate student populations (Behrend, Sharek, Meade, & Wiebe, 2011), online participants could differ from student and community participants on a number of psychological traits and tendencies (Goodman et al., 2013). Despite the differences, research shows that decision making studies using online platform participants produce reliable results that are consistent with those documented in past research conducted with community and student populations. However, given that all findings were from online platform participants located in the US, the cultural generalizability of our findings is unclear. Future research could examine the effect of choice nudge on anchoring effects with other populations, including those from non-Western countries.

Choice is an integral part of people's everyday lives. Many people are probably thinking about choices much of the time. Whereas past work identified some potentially negative consequences of the salience of choice (Savani et al., 2011; Savani & Rattan, 2012), the current research suggests that thinking about choices can help people make better decisions.

## Open practices

The pre- questionnaires, data, and analysis code for all experiments are available on https://osf.io/rmkp6/. The pre-registrations for Studies 2, 3, 4a, 4b, 5, and 6 are available at https://osf.io/wavs8, https://osf.io/k2c3t, https://osf.io/4r9yu, https://osf.io/8vkbf, https://osf.io/eh5kg, and https://osf.io/8b37z.

## Author note

Both authors contributed equally and are joint first authors. The work described in this paper was partially supported by a Humanities and Social Sciences Prestigious Fellowing Scheme award from the Research Grants Council of the Hong Kong Special Administrative

Region, China (Project No. PolyU 35001423). Parts of this research was conducted while Krishna Savani was at Nanyang Technological University. We thank Dayana Bulchand, Sylvia Chin, Ee Hwee Lau, Andrea Low, and Samantha Seah for invaluable research assistance.

Information redacted from main text to preserve masked review: This study was approved by the Hong Kong Polytechnic University's IRB protocol HSEARS20220506001–01 titled "Attitudes, decision-making, and performance" and Nanyang Technological University's IRB protocol IRB-2015-07-018 titled "Role of implicit processes in cultural learning."

## Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

## Data availability

I have shared a link with the data and code in the manuscript.

## Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jesp.2023.104575.

## References


Baron, R. M., & Kenny, D. A. (1986). The moderator–mediator variable distinction in social psychological research: Conceptual, strategic, and statistical considerations. *Journal of Personality and Social Psychology, 51*(6), 1173.

Behrend, T. S., Sharek, D. J., Meade, A. W., & Wiebe, E. N. (2011). The viability of crowdsourcing for survey research. *Behavior Research Methods, 43*(3), 800–813.

Bettman, J. R., Johnson, E. J., & Payne, J. W. (1990). A componential analysis of cognitive effort in choice. *Organizational Behavior and Human Decision Processes, 45*, 111–139.

Blankenship, K. L., Wegener, D. T., Petty, R. E., Detweiler-Bedell, B., & Macy, C. L. (2008). Elaboration and consequences of anchored estimates: An attitudinal perspective on numerical anchoring. *Journal of Experimental Social Psychology, 44*(6), 1465–1476.

Brandstätter, V., Lengfelder, A., & Gollwitzer, P. M. (2001). Implementation intentions and efficient action initiation. *Journal of Personality and Social Psychology, 81*, 946–960.

Brünken, R., Steinbacher, S., Plass, J. L., & Leutner, D. (2002). Assessment of cognitive load in multimedia learning using dual-task methodology. *Experimental Psychology, 49*, 109–119.

Bucchianeri, G. W., & Minson, J. A. (2013). A homeowner's dilemma: Anchoring in residential real estate transactions. *Journal of Economic Behavior & Organization, 89*, 76–92.

Burgmer, P., & Englich, B. (2013). Bullseye! How power improves motor performance. *Social Psychological and Personality Science, 4*(2), 224–232.

Chae, B., & Hoegg, J. (2013). The future looks "right": Effects of the horizontal location of advertising images on product attitude. *Journal of Consumer Research, 40*(2), 223–238.

Chandler, J., Rosenzweig, C., Moss, A. J., Robinson, J., & Litman, L. (2019). Online panels in social science research: Expanding sampling methods beyond mechanical Turk. *Behavior Research Methods, 51*(5), 2022–2038.

Critcher, C. R., & Gilovich, T. (2007). Incidental environmental anchors. *Journal of Behavioral Decision Making, 21*(3), 241–251.

Dhar, R., & Gorlin, M. (2013). A dual-system framework to understand preference construction processes in choice. *Journal of Consumer Psychology, 23*, 528–542.

Epley, N., & Gilovich, T. (2006). The anchoring-and-adjustment heuristic: Why the adjustments are insufficient. *Psychological Science, 17*, 311–318.

Fisher, C. D., Minbashian, A., Beckmann, N., & Wood, R. E. (2013). Task appraisals, emotions, and performance goal orientation. *Journal of Applied Psychology, 98*(2), 364.

Galinsky, A. D., & Mussweiler, T. (2001). First offers as anchors: The role of perspective-taking and negotiator focus. *Journal of Personality and Social Psychology, 81*, 657–669.

Gelman, A. (2018). The failure of null hypothesis significance testing when studying incremental changes, and what to do about it. *Personality and Social Psychology Bulletin, 44*(1), 16–23.

Goodman, J. K., Cryder, C. E., & Cheema, A. (2013). Data collection in a flat world: The strengths and weaknesses of mechanical turk samples. *Journal of Behavioral Decision Making, 26*(3), 213–224.

Hammond, J. S., Keeney, R. L., & Raiffa, H. (2015). *Smart choices: A practical guide to making better decisions*. Harvard Business Review Press.

Inbar, Y., & Gilovich, T. (2011). Angry (or disgusted), but adjusting? The effect of specific emotions on adjustment from self-generated anchors. *Social Psychological and Personality Science, 2*(6), 563–569.

Johnson, E. J., & Goldstein, D. G. (2003). Do defaults save lives? *Science, 302*, 1338–1339.

Kokis, J. V., Macpherson, R., Toplak, M. E., West, R. F., & Stanovich, K. E. (2002). Heuristic and analytic processing: Age trends and associations with cognitive ability and cognitive styles. *Journal of Experimental Child Psychology, 83*(1), 26–52.

Kraushaar, J. M., & Novak, D. C. (2010). Examining the affects of student multitasking with laptops during the lecture. *Journal of Information Systems Education, 21*(2), 241–252.

Kruglanski, A. W., & Freund, T. (1983). The freezing and unfreezing of lay-inferences: Effects on impressional primacy, ethnic stereotyping, and numerical anchoring. *Journal of Experimental Social Psychology, 19*(5), 448–468.

Loschelder, D. D., Stuppi, J., & Trötschel, R. (2014). "€ 14,875?!": Precision boosts the anchoring potency of first offers. *Social Psychological and Personality Science, 5*, 491–499.

Ma, A., Yang, Y., & Savani, K. (2019). "Take it or leave it!" A choice mindset leads to greater persistence and better outcomes in negotiations. *Organizational Behavior and Human Decision Processes, 153*, 1–12.

Mason, M. F., Lee, A. J., Wiley, E. A., & Ames, D. R. (2013). Precise offers are potent anchors: Conciliatory counteroffers and attributions of knowledge in negotiations. *Journal of Experimental Social Psychology, 49*, 759–763.

McElroy, T., & Seta, J. J. (2003). Framing effects: An analytic–holistic perspective. *Journal of Experimental Social Psychology, 39*(6), 610–617.

McHugh, S., & Ranyard, R. (2016). Consumers' credit card repayment decisions: The role of higher anchors and future repayment concern. *Journal of Economic Psychology, 52*, 102–114.

McNeill, L., & Wyeth, E. (2011). The private label grocery choice: Consumer drivers to purchase. *The International Review of Retail, Distribution and Consumer Research, 21*, 95–109.

Moore, D. A., & Loewenstein, G. (2004). Self-interest, automaticity, and the psychology of conflict of interest. *Social Justice Research, 17*(2), 189–202.

Mussweiler, T., & Strack, F. (1999). Hypothesis-consistent testing and semantic priming in the anchoring paradigm: A selective accessibility model. *Journal of Experimental Social Psychology, 35*, 136–164.

Mussweiler, T., & Strack, F. (2000). Numeric judgments under uncertainty: The role of knowledge in anchoring. *Journal of Experimental Social Psychology, 36*, 495–518.

Nanakdewa, K., Madan, S., Savani, K., & Markus, H. R. (2021). The salience of choice fuels independence: Implications for self-perception, cognition, and behavior. *Proceedings of the National Academy of Sciences - PNAS, 118*(30), 1.

Northcraft, G. B., & Neale, M. A. (1987). Experts, amateurs, and real estate: An anchoring-and-adjustment perspective on property pricing decisions. *Organizational Behavior and Human Decision Processes, 39*, 84–97.

Patall, E. A., Cooper, H., & Robinson, J. C. (2008). The effects of choice on intrinsic motivation and related outcomes: A meta-analysis of research findings. *Psychological Bulletin, 134*, 270–300.

Raudenbush, S. W., & Bryk, A. S. (2002). *Hierarchical linear models: Applications and data analysis methods* (2nd ed.). Sage.

Russo, J. E., & Leclerc, F. (1994). An eye-fixation analysis of choice processes for consumer nondurables. *Journal of Consumer Research, 21*, 274–290.

Samuelson, W., & Zeckhauser, R. (1988). Status quo bias in decision making. *Journal of Risk and Uncertainty, 1*(1), 7–59.

Savani, K., Markus, H. R., Naidu, N. V. R., Kumar, S., & Berlia, N. (2010). What counts as a choice? US Americans are more likely than Indians to construe actions as choices. *Psychological Science, 21*, 391–398.

Savani, K., & Rattan, A. (2012). A choice mindset increases the acceptance and maintenance of wealth inequality. *Psychological Science, 23*, 796–804.

Savani, K., Stephens, N. M., & Markus, H. R. (2011). The unanticipated interpersonal and societal consequences of choice victim blaming and reduced support for the public good. *Psychological Science, 22*, 795–802.

Savani, K., Stephens, N. M., & Markus, H. R. (2017). Choice as an engine of analytic thought. *Journal of Experimental Psychology. General, 146*(9), 1234–1246.

Schwartz, B. (2000). Self-determination: The tyranny of freedom. *American Psychologist, 55*, 79–88.

Schwartz, B. (2004). *The paradox of choice*. Harper Collins.

Schwartz, B., & Cheek, N. N. (2017). Choice, freedom, and well-being: Considerations for public policy. *Behavioural Public Policy, 1*(1), 106–121.

Shiv, B., & Fedorikhin, A. (1999). Heart and mind in conflict: The interplay of affect and cognition in consumer decision making. *Journal of Consumer Research, 26*, 278–292.

Shiv, B., & Fedorikhin, A. (2002). Spontaneous versus controlled influences of stimulus-based affect on choice behavior. *Organizational Behavior and Human Decision Processes, 87*, 342–370.

Simmons, J. P., LeBoeuf, R. A., & Nelson, L. D. (2010). The effect of accuracy motivation on anchoring and adjustment: Do people adjust from provided anchors? *Journal of Personality and Social Psychology, 99*, 917–932.

Smith, A. R., & Windschitl, P. D. (2011). Biased calculations: Numeric anchors influence answers to math equations. *Judgment and Decision making, 6*(2), 139–146.

Spencer, S. J., Zanna, M. P., & Fong, G. T. (2005). Establishing a causal chain: Why experiments are often more effective than mediational analyses in examining psychological processes. *Journal of Personality and Social Psychology, 89*(6), 845–851.

Stephens, N. M., & Levine, C. S. (2011). Opting out or denying discrimination? How the framework of free choice in American society influences perceptions of gender inequality. *Psychological Science, 22*(10), 1231–1236.

Tversky, A., & Kahneman, D. (1974). Judgment under uncertainty: Heuristics and biases. *Science, 185*, 1124–1131.

Vohs, K. D., Baumeister, R. F., Schmeichel, B. J., Twenge, J. M., Nelson, N. M., & Tice, D. M. (2008). Making choices impairs subsequent self-control: A limited-resource account of decision making, self-regulation, and active initiative. *Journal of Personality and Social Psychology, 94*, 883–898.

Wadhwa, M., & Zhang, K. (2015). This number just feels right. *Journal of Consumer Research, 41*(5), 1172–1185.

Wang, Y., & Savani, K. (2022). The salience of choice reduces social responsibility: Evidence from lab experiments and compliance with covid-19 stay-at-home orders. *PNAS Nexus, 1*(4).

Wegener, D. T., Petty, R. E., Blankenship, K. L., & Detweiler-Bedell, B. (2010). Elaboration and numerical anchoring: Implications of attitude theories for consumer judgment and decision making. *Journal of Consumer Psychology, 20*(1), 5–16.