Modulators of Preference Reversals in Aesthetic Choices

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Modulators of Preference Reversals in Aesthetic Choices

by

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Abstract

Understanding the factors that produce choice reversals is of interest to researchers and marketers alike. I established a reversal paradigm for studying reversals in subjective choices between pairs of paintings. Participants chose between a pair of average-beauty paintings both before and after a contrast manipulation. In a reversal block, some contrast manipulation trials aimed to weaken preference for the chosen painting by pairing it with more-beautiful paintings, and others aimed to strengthen preference for the nonchoice painting by pairing it with less-beautiful paintings. Across a set of experiments more reversals occurred in reversal (vs. control) blocks, as anticipated, though this reversal effect was not significant in some experiments. I also identified several important moderators of reversals. Participants who reversed tended to have lower initial relative preference ratings and initial choice identification accuracy than consistent participants, and they also selected more higher-beauty choices during the contrast trials. Overall, the present study successfully induced reversals in subjective choices and identified several factors that create reversals. The stage is now set for future studies to refine the reversal paradigm, to use the paradigm to establish clearer links between choice reversals and their causes, and to attempt to generalize the paradigm to more applied contexts.
Acknowledgements

I would like to thank my Master’s supervisor, Dr. Glen Bodner, as well as my supervisory committee, Drs. Christopher Sears and John Ellard, and my internal-external examiner, Dr. Justin Weinhardt. I would also like to express my gratitude to my friends and family for their support.
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CHAPTER 1: INTRODUCTION

Making choices is a common and influential part of daily life. Evaluations and decisions are often made quickly and impulsively (Kahneman, 2011) and can have enduring effects on our preferences. Because of these quick yet enduring effects, it is important to understand how preferences develop and are modified by experience. Preference can be described as liking a particular stimulus over another (e.g., Zellner, Mattingly, & Parker, 2009), or the degree of positivity felt towards an individual stimulus. Although preferences have been studied in terms of their development through affective and cognitive processes (e.g., Zajonc, 1980; Zajonc & Markus, 1982), my thesis focused on the behavioral aspect of preferences, measured by reversals in choices between the same pairs of stimuli at two points in time.

The study of aesthetics and its relation to preferences has a long history (e.g., Fechner, 1876), and past research has used a variety of classes of stimuli. For example, people show a striking agreement in their judgments of facial attractiveness (for a review, see Rhodes, 2006). Preferences for music genres have also been studied, and have been related to personality characteristics (e.g., Rentfrow & Gosling, 2003), and even to political views (e.g., Fox & Williams, 1974). The structure of songs has also been studied to identify characteristics that listeners prefer (e.g., Boyle, Hosterman, & Ramsey, 1981).

My thesis investigated people's choices between abstract paintings. Although art appreciation is generally believed to be highly subjective, as captured by the adage “beauty is in the eye of the beholder”, research shows that people tend to agree on what makes paintings beautiful (e.g., greater complexity, semantic meaning; see Lindell & Mueller, 2011), and tend to be fairly consistent in their preferences (e.g., Vessel & Rubin, 2010). However, people may be more likely to reverse their choices when preferences have not been established and/or when the
two choices are roughly equivalent. For example, people may be more fluid in their preferences for which of two current pop songs, fast food combos, or pairs of shoes they prefer. Knowledge of the factors that modulate such reversals would not only further our understanding of how subjective choices are made, but it would also have clear relevance to marketers, advertisers, and economists. Before describing my efforts to create a paradigm designed to induce subjective choice reversals, I first outline common methods used in previous research to successfully influence and/or reverse peoples’ preferences in terms of their evaluations or choices.

**Inducing Preference Reversals**

Researchers have identified three common means of inducing preference reversals. *Task-induced preference reversals* are achieved by changing the nature of the task. This class of preference reversals is often studied in gambling tasks, where participants are asked to choose between options at one time, and bid on each option at another time (e.g., Lichtenstein & Slovic, 1971; Bostic, Herrnstein, & Luce, 1990). For example, Lichtenstein and Slovic (1971) found that most often, participants who chose the $P$-bet, an option with a high probability of winning a small amount and a low probability of losing a smaller amount, later reported a higher bid for the $S$-bet, an option with a low probability of winning a large amount and a large probability of losing a small amount. The authors suggested that participants focus more on the concrete payoffs when the task involves bidding than when choosing between options, leading to reversals of participants’ initially preferred option. In a non-gambling study, Oliver (2006) asked participants to either choose a single option or to rank-score a set of options selecting or ranking in which country they would most like to live based on life expectancy. A chosen option was labeled a preference reversal if it did not match the expected option, determined based on standard economic theory. More preference reversals occurred when participants chose a single
option than when they rank-scored all of the options in different questions, with the highest ranked option as their final choice. Task modulated people’s preferences, such that more reversals occurred with a forced choice task than with bidding or ranking tasks.

Frame-induced preference reversals occur when two equivalent options are framed in opposing ways, such as presenting disease treatment-program choices worded either in terms of “lives lost” or “lives saved”, as pioneered by Tversky and Kahneman (1986). In their study, one group chose between Program A (200 of 600 people will be saved) and Program B (1/3 probability that 600 will be saved, and 2/3 probability that nobody will be saved). Another group chose between Program A (400 of 600 will die) and Program B (1/3 probability that nobody will die, and 2/3 probability that 600 will die). Of the first group, 72% chose Program A (framed in terms of lives saved), whereas of the second group, only 22% chose Program A (framed in terms of lives lost)—even though each pairs of programs was identical in probabilistic outcomes. Thus, the framing of the options led to a between-group preference reversal.

Context-induced preference reversals occur when preferences depend on the availability of other options and their relative inferiority/superiority (for a review, see Engelmann & Hein, 2013). This method works by introducing a third option that alters the evaluation of another two options, usually termed a target and a competitor (e.g., Busemeyer, Barkan, Mehta, & Chaturvedi, 2007; Simonson, 1989; Tsetos, Usher, & Chater, 2010; Tversky & Simonson, 1993; Wedell, 1991). Based on the nature of the third option, three prominent effects appear. Huber, Payne, and Puto (1982) asked groups of participants to choose between consumer options (e.g., for either cars, restaurants, beers, films, or television sets), presented either with a target and a competitor, or with a third option added that was similar but inferior to the target. More participants chose the target when all three options were given, resulting in an attraction effect. Simonson (1989)
successfully induced *compromise effects* for consumer products: Adding a dominant third option led participants to provide higher share values for the target, which now had moderately good attributes compared to the competitor and third option. Finally, *similarity effects* occur when the third option is dominant and similar to the competitor, resulting in the dissimilar target being chosen more often than the competitor. These effects have been also found with choices between gambles or college applicants (Tversky, 1972). They highlight the importance of the options in creating and shifting the context and influencing decisions, independent of task and setting.

Preference reversals have also been induced in riskless, perceptual and inferential judgments (e.g., Trueblood, 2012; Trueblood, Brown, Heathcote, & Busemeyer, 2013; Trueblood, 2015). However, whether reversals can also be induced for inherently subjective choices has yet to be explored. A goal of my thesis was to explore whether such reversals can also be engineered for subjective choices, without any explicit attributes attached to the options. Because the aesthetics literature has focused on how context can be found to influence aesthetic judgments, I will next summarize some of these findings, which led to the development of my choice reversal paradigm.

**Context Effects on Aesthetic Judgments**

Context effects on aesthetic judgments are commonly induced through contrast and assimilation manipulations (e.g., Hovland, Harvey, & Sherif, 1957). When considering subjective preferences, for example, a contrast effect occurs when a target stimulus (e.g., a painting) is deemed less pleasant when presented in the context of a more-pleasant stimulus (e.g., a more-beautiful painting), and is deemed more pleasant when presented in the context of a less-pleasant stimulus (e.g., a less-beautiful painting). An assimilation effect is the converse: A target painting is deemed more pleasant when presented with a more-beautiful painting, and is deemed
less pleasant when presented with a less-beautiful painting.

Research in this area was sparked by perceptual contrast effects such as the Ebbinghaus illusion: Circles of the same size are perceived as smaller if they are surrounded by big circles, or bigger if they are surrounded by smaller circles (e.g., Girgus & Coren, 1982). In another study (Herr, Sherman, & Fazio, 1983), participants were asked to judge an ambiguous animal’s “ferocity”. Those primed with extremely ferocious animals tended to contrast and report the ambiguous animal as less ferocious, whereas those primed with moderately ferocious animals tended to assimilate and report the ambiguous animal as similarly ferocious. Thus, how participants perceived the target depended on the priming context.

In the realm of aesthetics, Parker, Bascom, Rabinovitz, and Zellner (2008) found that a given musical melody was liked less when presented after a more-pleasant melody than when it was presented first (negative contrast). When the more-pleasant musical melody was presented second, it was liked more than if it had been presented first (positive contrast). A different pattern has been reported for evaluations of facial beauty. Geiselman, Haight, and Kimata (1984) found assimilation effects in ratings of facial beauty, where a target face was rated more attractive when presented with two relatively more-attractive faces, and was rated as less attractive when presented with two relatively less-attractive faces.

Research on the effects of context on judgments of artistic stimuli has predominantly yielded contrast rather than assimilation effects. For example, Dolese, Zellner, Vasserman, and Parker (2005) found that participants who first viewed paintings from Goya’s “dark period” rated paintings from his “tapestry period” as more beautiful than participants who only viewed his tapestry paintings—a contrast effect. Mallon, Redies, and Hayn-Leichsenring (2014) found that participants rated abstract paintings as more beautiful after viewing less-beautiful abstract
paintings, and as less beautiful after viewing more-beautiful abstract paintings (see also Arielli, 2012). And, of particular relevance to my thesis, Tousignant and Bodner (2014) found contrast effects on beauty ratings for photographs of buildings that persisted even when attempts to induce an assimilation pattern were made. Given my goal of inducing reversals for subjective choices, I therefore used a contrast manipulation, as outlined in the next section.

**Developing a Reversal Paradigm**

The paradigm I developed used a contrast manipulation to try to reverse people’s choices for pairs of average-beauty abstract paintings (see Figure 1). Participants were first asked to choose which of two normatively average abstract paintings they felt was more beautiful (choice 1). In a critical reversal block, contrast manipulations were then used to attempt to reverse their choice. To this end, on two trials, participants were asked to choose between the chosen painting and a normatively more-beautiful painting, to make the chosen painting seem less beautiful by contrast. On two other trials, participants were asked to choose between the nonchosen painting and a normatively less-beautiful painting, to make the nonchosen painting seem more beautiful by contrast. The pair of chosen and nonchosen paintings was then presented again (choice 2), and I measured whether participants reversed their choice. The reversal rate in the reversal block was then compared to a baseline reversal rate in a control block in which the contrast manipulation was performed on a different pair of average-beauty paintings.

Experiment 1 tested and established the viability of the reversal paradigm using a within-subject design. Experiments 2-5 then aimed to replicate and amplify the reversal effect in a between-group design, while also measuring several additional factors that might predict reversals. Experiment 2 provided an initial test of a between-group reversal effect. Experiment 3 examined whether adding a block of delay trials before or after the contrast manipulations could
amplify the reversal effect. Experiment 4 reports a control condition that aimed to establish whether the contrast manipulation was responsible for the reversal effect. Finally, Experiment 5 attempted to amplify the reversal effect by presenting the reversal/control blocks three times.

**Exploring Other Modulators of Reversals**

A second goal of my thesis was to explore other potential factors that might influence choice reversals. The factors I explored, which are described in more detail after Experiment 5, were: (1) relative preference ratings for chosen paintings, (2) how often participants chose the higher-beauty painting in the blocks, (3) boredom ratings, (4) decision fatigue ratings, and (5) whether participants accurately identified their initial choices after the main experiment.

Analyses of participants’ relative preference ratings are reported in each experiment, and are also reported pooled across several experiments after Experiment 5. Analyses of the other factors are reported only in the pooled analysis section, to maximize power.
Figure 1. Trial configurations used in Experiments 1-5, with A = average-beauty painting, L = low-beauty painting, and H = high-beauty painting (see text for details). Sections in dotted lines occurred only in the indicated experiment. Each block was bookended by the choice 1 average-beauty pair presented in the opposite left/right order. The asterisk refers to the hypothetical choice 1 selection, which dictated the structure of the reversal (and nonreversal) block. If A2 was chosen, then the assignment of A1 and A2 was reversed in the reversal block.
CHAPTER 2: EXPERIMENT 1

Initial Demonstrations of a Choice Reversal Effect

Experiment 1 provided two initial tests of whether contrast manipulations can increase reversals in subjective choices between pairs of abstract average-beauty paintings. To measure reversals, the same choice was presented before (choice 1) and after (choice 2) a reversal block, and another pair of paintings was presented before and after a control block (i.e., a within-subject design). Block order was counterbalanced across participants. In the reversal block, one pair of contrast trials was designed to increase preference for the nonchosen painting (nonchoice+), and another pair was designed to decrease preference for the chosen painting (choice-). Within the reversal blocks each average painting (nonchoice and choice) was presented twice to control for the potential influence of mere exposure (i.e., that liking increases with additional stimulus exposures; e.g., Zajonc, 1968). Had only the nonchoice+ trials been presented, then contrast and more exposures to the nonchoice painting would both work to produce a reversal effect. And had only the choice- trials been presented, then contrast would work to produce a reversal effect but mere exposure would work against the reversal effect (because the chosen painting would be presented more times than the nonchoice painting). Thus, the potential confound of mere exposure was eliminated by presenting both nonchoice+ and choice- trials such that the number of exposures to each painting was controlled. The trials in the control block were identical to those in the reversal block, except the average paintings from choice 1 and choice 2 were replaced with another pair of average-beauty paintings not presented elsewhere in the experiment. Thus, the control block trials were not expected to influence choice 2.

In Experiment 1B, an additional preference measure was added after each pair trial: Participants indicated how much they preferred the painting they had chosen over the painting
they had not chosen. This *relative preference rating* measure was adopted from previous studies examining how contrast can condense or expand the degree of relative preference (e.g., Zellner, Hoer, & Feldman, 2014). These ratings allowed me to answer three questions. First, would relative preference ratings for choice 1 be lower when a reversal occurred? Second, would relative preference ratings for choice 2 differ as a function of block type (reversal vs. control), and whether or not a reversal occurred? Third, did relative preference ratings for consistent choices in reversal block nonetheless decrease across choice 1 and 2, thus showing an influence of the reversal block on relative preference when a reversal was not induced?

**Method**

**Participants**

University of Calgary undergraduates participated online for psychology course credit. There were 96 participants in each of Experiment 1A (62 female; mean age 21) and 1B (70 female; mean age 21), and half of each set of participants was assigned to each block order in a counterbalanced fashion. Participants who took longer than 30 minutes to complete any of the reported experiments were replaced (most experiments took about 10 min). This criterion resulted in 0 replacements in Experiment 1A and 1 replacement in Experiment 1B. Expertise can affect aesthetic experience and judgment (e.g., Rota & Zellner, 2007; Pihko et al., 2011). However, only 4 participants self-identified as art experts on a post-experiment questionnaire (0.3%). Therefore art expertise was not considered further.

**Stimuli**

The stimuli were abstract paintings selected through three initial norming studies using additional participants from the same pool. At the outset, 240 images spanning as wide a range of beauty as possible were selected from image databases (e.g., Artstor, Oxford Art Online) and
Google searches. It proved necessary to include a few paintings by somewhat well-known artists (e.g., O'Keefe) to obtain enough high-beauty paintings. The paintings were selected so as not to obviously depict semantic or representational content. Each image was resized to 500 pixels on the longest dimension. To select low-, average-, and high-beauty paintings, 21 participants rated the beauty of all 240 paintings on a 9-point scale (1 = ugly, 5 = neither ugly nor beautiful, 9 = beautiful). Based on their ratings, 36 paintings were selected that were rated as close to 1 (low), 5 (average), and 9 (high) as possible, with standard errors below .5. An additional 23 participants rated the beauty of these 36 paintings twice. The final set of 24 paintings (Figure 2) was then selected using the same criteria, based on the mean ratings (i.e., averaged across the two ratings per painting). An additional 22 participants rated the final set twice, and the mean ratings reported in Figure 2 reflect these final ratings averaged across the two ratings per painting. The correlation between participants’ rating 1 and 2 (averaged across individual paintings) between time points was .85, and the correlation between painting rating 1 and 2 (averaged across individual participants) was .80. Another 80 participants were asked to choose the more-beautiful painting between 3 or 4 average/average pairs, 8 average/low pairs, and 8 average/high pairs, each pair presented twice in a random order (10 participants for each of 8 counterbalances). Based on the mean proportion of participants who chose each painting across choices, 4 average/average pairs were selected to elicit as close to a 50/50 split in choices as possible ($M = .48$), 32 average/low pairs were selected to maximize the proportion of average-beauty choices ($M = .89$), and 32 average/high pairs were selected to maximize the proportion of high-beauty choices ($M = .91$).
<table>
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<td>6.78 (.20)</td>
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</tr>
<tr>
<td>3.73 (.53)</td>
<td>5.32 (.42)</td>
<td>7.09 (.37)</td>
</tr>
</tbody>
</table>

**Figure 2.** Final set of paintings and their mean (SE) beauty ratings out of 9.

In Experiment 1, each participant received one reversal block and one control block, the order of which was counterbalanced across participants. The same average/average pair was presented before and after each block to measure reversals (see Figure 1). The left/right order of the average-beauty paintings always changed across presentations. A filler block, consisting of two high/low pairs of paintings not used elsewhere (each used once) was presented before choice 1 in each block, to provide task practice and to help mask the structure of the main trials. These filler trials were not analyzed.
The reversal and control blocks each consisted of six randomly mixed trials, bookended by the average-beauty pair (Figure 1). The participant’s choice on choice 1 dictated the structure of the reversal block. The reversal block included two nonchoice+ trials in which the nonchosen average painting was paired with a low-beauty painting. The reversal block also included two choice- trials in which the chosen average painting was paired with a high-beauty painting. There were also two high/low-beauty pair trials; these trials reinforced choosing the more-beautiful painting in each pair within the contrast trials, while controlling for the number of exposures to each low, average, and high-beauty painting (i.e., 2 exposures per painting). Each painting appeared once on the left and once on the right.

The control block was identical to the reversal block, except a different pair of average-beauty paintings, that was not presented elsewhere in the experiment, was used (Figure 1). Participants viewed the same pair types in the control block, but the contrast manipulations were performed on this new pair of average-beauty paintings. Thus, the control block was not expected to impact choice 2. Although reversals were also expected in the control block, I was primarily interested in whether the reversal rate would be higher in reversal than control blocks.

Assignment of the average/average pairs to block type (reversal vs. control) and role (reversal pair vs. control pair) was counterbalanced across participants. Each participant received the same sets of 2 low-beauty and 2 high-beauty paintings, but assignment of average paintings and block type were fully counterbalanced. In total, each of 8 counterbalances was assigned to 12 participants.

Procedure

Participants signed up for the study online through the Department’s research participation site. From there, they linked to the online study. The instructions told participants to
allot 30 minutes to complete the experiment in one sitting. They were asked to maximize their browser window to enable them to view the full paintings without scrolling, and they were asked to exit other applications including email and Facebook and to put away their phones, to limit distractions. They were then given the following instructions:

On each trial in this study you will be shown two paintings side by side. Your task is to choose which painting you think is more beautiful. Make your choice based on your automatic and spontaneous feelings for the two paintings at that moment. Recommended ways to choose between the two paintings include imagining which painting you would like to see again, or which painting you would most prefer to hang on your wall. You will be shown some pairs of paintings twice because we wish to determine whether what people deem beautiful in art is stable or variable over time. You should not feel any pressure to choose the same painting each time, nor should you feel any pressure to choose a different painting each time. Instead, each time you see a pair of paintings just choose based on your automatic and spontaneous feelings for each painting at that moment.

These instructions were intended to avoid biasing participants either toward or away from reversing their choices, though I acknowledge that mentioning both potential “pressures” could have worked either to increase or decrease the reversal rate.

In Experiment 1B, participants’ relative preference ratings were also measured. After each choice, participants indicated on a 9-point scale the degree of preference for the chosen painting over the nonchosen painting (1 = slightly more, 5 = somewhat more, 9 = very much more).
Results and Discussion

A p-value of .05 was used to assess significance for all analyses. Effect size was measured using phi (Φ) for chi-square tests, and eta-squared ($\eta^2$) for analyses of variance (ANOVA).

Experiment 1A

The reversal rate (Figure 3A) was first compared as a function of block type (reversal vs. control; a within-subject factor) using McNemar’s tests. Averaged across block order, reversals were more frequent in reversal than control blocks (.22 vs. .10), $\chi^2(1, N = 96) = 4.65, p = .03, \Phi = .22$, thus establishing a reversal effect (Figure 3A). Chi-square tests revealed that this reversal effect was significant in Block 2 (.31 vs. .13), $\chi^2(1, N = 96) = 4.94, p = .03, \Phi = .23$, but not in Block 1 (.13 vs. .06), $\chi^2(1, N = 96) = 1.10, p = .29$. Averaged across block type more reversals occurred in Block 2 than in Block 1 (.22 vs. .09), $\chi^2(1, N = 96) = 5.50, p = .02, \Phi = .24$.

Experiment 1B

Choice reversals. Experiment 1B replicated the reversal effect: Reversals were more frequent in reversal than control blocks overall (.22 vs. .08), $\chi^2(1, N = 96) = 5.76, p = .02, \Phi = .25$ (Figure 3B). The reversal effect was again significant in Block 2 (.25 vs. .08), $\chi^2(1, N = 96) = 4.80, p = .03, \Phi = .22$, but not in Block 1 (19 vs. .08), $\chi^2(1, N = 96) = 2.22, p = .14$. Unlike in Experiment 1A there was no effect of block order: averaged across block type the rate of reversals was similar in Blocks 1 and 2 (.17 vs. .14), $\chi^2(1, N = 96) = 0.15, p = .70$.

Relative preference ratings. Next, I examined participants’ relative preference ratings for the average paintings they chose. I first examined whether these ratings changed across choice 1 and 2 (see Figure 3C and 3D for Blocks 1 and 2, respectively). Relative preference ratings were analyzed separately for each block using a mixed-factor ANOVA with block type
Figure 3. Experiment 1: Choice reversal rate by block (1, 2) and block type (reversal, control), and across blocks, in Experiment 1A (A) and 1B (B); relative preference rating difference from the midpoint of 5 in Experiment 1C (95% CI) by block type and by whether participants’ reversed in Block 1 (C) and Block 2 (D).

(reversal vs. control; between-subjects), the participant’s consistency in that block (reversed vs. consistent; between-subjects), and choice (choice 1 vs. 2; within-subjects) as the factors.

Although the mean ratings were analyzed, Figure 3 plots the difference in ratings from the scale midpoint (5 = “somewhat more”), thus ranging from +4 to -4, to help make the pattern of effects easier to apprehend.

The ANOVA for block 1 did not produce any significant effects. Relative preference ratings were similar for reversal and control blocks (5.24 vs. 5.44), $F(1, 92) = 0.17, MSE = 1.29,
were marginally lower when a reversed (vs. consistent choice) occurred (4.85 vs. 5.49), $F(1, 92) = 3.21, MSE = 23.75, p = .08$, and were similar for choice 1 and 2 (5.27 vs. 5.41), $F(1, 92) = 1.71, MSE = 3.26, p = .19$. The interactions were not significant: block type and consistency, $F(1, 92) = 0.57, MSE = 4.24, p = .45$, block type and choice, $F(1, 92) = 0.34, MSE = 0.65, p = .56$, choice and consistency, $F(1, 92) = 1.40, MSE = 2.66, p = .24$, and the three-way interaction, $F(1, 92) = 0.02, MSE = 0.03, p = .90$.

The ANOVA for block 2 yielded a significant main effect of consistency in line with the Block 1 pattern: Participants who reversed had lower relative preference ratings, across block type and choice, than those who did not reverse (3.88 vs. 5.34), $F(1, 92) = 6.32, MSE = 51.67, p = .01, \eta^2 = .07$. Ratings were similar in reversal and control blocks (4.73 vs. 5.29), $F(1, 92) = 0.01, MSE = 0.05, p = .94$, and for choice 1 and choice 2 (4.93 vs. 5.09), $F(1, 92) = 0.00, MSE = 0.00, p = .99$. The interactions were nonsignificant: block type and consistency, $F(1, 92) = 0.37, MSE = 3.01, p = .55$, block type and choice, $F(1, 92) = 2.46, MSE = 4.21, p = .12$, choice and consistency, $F(1, 92) = 0.32, MSE = 0.54, p = .57$, and three-way interaction, $F(1, 92) = 1.80, MSE = 3.07, p = .18$.

I also examined whether the reversal block influenced relative preference ratings for consistent participants. For consistent choices, did the reversal block nonetheless decrease participants’ relative preference for their chosen paintings? To find out, relative preference ratings for consistent choices were analyzed, separately for each block, using a mixed-factor ANOVA with block type (reversal vs. control; between-subjects) and choice (1 vs. 2; within-subjects) as the factors. The critical test was the interaction. If the contrast manipulation had an influence on consistent-choice trials, then relative preference ratings should be lower for choice 2 than for choice 1 in reversal blocks relative to control blocks. Contrary to this possibility, neither
ANOVA produced any significant effects: in the block 1 ANOVA, block type, $F(1, 81) = 0.25$, $MSE = 1.81$, $p = .62$, choice, $F(1, 81) = 0.04$, $MSE = 0.07$, $p = .85$, interaction, $F(1, 81) = 0.49$, $MSE = 0.84$, $p = .48$; in the block 2 ANOVA, block type, $F(1, 81) = 0.86$, $MSE = 7.21$, $p = .36$, choice, $F(1, 81) = 0.54$, $MSE = 0.97$, $p = .46$, interaction, $F(1, 81) = 0.10$, $MSE = 0.17$, $p = .76$. Thus, participants who did not reverse also gave consistent relative preference ratings for the average paintings they chose.

**Summary**

In summary, the reversal rate in both Experiment 1A and 1B was greater in reversal blocks than control blocks, providing initial support for the viability of the reversal paradigm. Participants who reversed generally gave lower relative preference ratings than those who remained consistent. However, consistent participants did not give lower relative preference ratings across critical trials. Thus, most participants were immune to the attempt to influence their choices, as well as their relative preference for their choices.
CHAPTER 3: EXPERIMENT 2

The Between-Group Paradigm

Beginning with Experiment 2, a fully between-subject design was adopted in which each participant received either 2 reversal or 2 control blocks. This design eliminated the potential for interactions between block type and block order, which occurred in Experiment 1A but not in Experiment 1B. The between-subject design also allowed me to analyze a new measure of choice reversals, namely the proportion of each group who showed a reversal (i.e., either 1 or 2 reversals). This new measure obviated the need to consider block order, which might be expected to yield variable patterns given the low rates of reversals. The trials of Experiments 2A and 2B were otherwise analogous to Experiment 1A and 1B, respectively.

Starting with Experiment 2, I also measured three new potential modulators of reversals: boredom, decision fatigue, and initial choice identification accuracy. These new measures are reported and discussed after Experiment 5, where they are considered in conjunction with pooled analyses across experiments.

Method

Participants

Additional participants were selected from the same pool. There were 96 participants in each of Experiments 2A (78 female; mean age 20; 4 participants were replaced for exceeding 30 min), and 2B (57 female; mean age 22; 1 participant was replaced for exceeding 30 min), and half of each set of participants were assigned to the reversal group and half to the control group.

Procedure

Experiment 2A was identical to Experiment 1A except participants either received 2 reversal blocks or 2 control blocks; this factor was counterbalanced across sets of 48 participants.
Experiment 2B was identical to Experiment 2A except participants also made relative preference ratings after each choice, as in Experiment 1B.

**Results and Discussion**

**Experiment 2A**

A chi-square test showed that the choice reversal rate (i.e., the proportion of participants who showed at least 1 reversal) did not differ across the reversal and control groups (.35 vs. .31), $\chi^2(1, N = 96) = 0.19, p = .67$ (Figure 4A). Thus, the reversal effect found using a within-subject design in Experiment 1 did not replicate when a between-group design was used.

**Experiment 2B**

**Choice reversals.** Counter to Experiment 2A, here the reversal rate in the reversal group was more than double the rate in the control group (.29 vs. .13), $\chi^2(1, N = 96) = 4.04, p = .04, \Phi = .205$ (Figure 4B). The absence of a reversal effect in Experiment 2A appears to have been associated with a high rate of reversals in the control group. Indeed, a chi-square test showed that the reversal rate in the control group was higher in Experiment 2A than in 2B (.31 vs. .13), $\chi^2(1, N = 96) = 4.94, p = .03, \Phi = .23$.

**Relative preference ratings.** I next examined, via ANOVAs, whether relative preference ratings for each choice differed as a function of group (reversal vs. control), and participant consistency (reversed [reversed on 1 or 2 blocks] vs. consistent [reversed in neither block]). Relative preference ratings (Figure 4C) were analyzed for choice 1 and choice 2 separately, each averaged across block, using between-subject ANOVAs. In Experiments 2-5, consistency was considered a participant-level attribute. That is, participants who reversed on one block were considered “reversed” participants when averaging across blocks. Figure 4C presents relative preference rating means as differences from the midpoint of 5.
Figure 4. Experiment 2: Choice reversal rate (proportion of participants who made 1 or 2 reversals) by group in Experiment 2A (A) and 2B (B); relative preference rating difference from the midpoint of 5 in Experiment 2B (C; 95% CI) by group and by whether participants reversed (1 or 2 reversals) or were consistent for both blocks.

The ANOVA for choice 1 tested whether initial relative preference ratings for average-beauty pairs were predictive of whether a participant would go on to show a reversal. The main effect of group did not reach significance; ratings were similar for the reversal and control groups (5.13 vs. 5.00), $F(1, 92) = 1.58, MSE = 4.96, p = .21$. However, there was a main effect of consistency, $F(1, 92) = 5.52, MSE = 17.37, p = .02, \eta^2 = .06$, reflecting lower initial relative
preference ratings for participants who reversed than consistent participants (4.35 vs. 5.25). The interaction was not significant, $F(1, 92) = 1.07, MSE = 3.36, p = .30$.

The ANOVA for choice 2 tested whether participants’ final relative preference ratings for average-beauty pairs differed as a function of group (reversal vs. control) and participant consistency (reversed vs. consistent). Choice 2 ratings did not differ between reversal and control groups (4.81 vs. 5.26), $F(1, 92) = 0.02, MSE = 0.06, p = .89$, or between participants who reversed or were consistent (4.53 vs. 5.17), $F(1, 92) = 2.36, MSE = 6.67, p = .13$, nor was there an interaction $F(1, 92) = 1.02, MSE = 2.88, p = .32$. The manipulation did not lower participants’ relative preference ratings regardless of whether they showed a reversal effect.

To assess whether the reversal block trials reduced relative preference ratings among consistent participants, I conducted an ANOVA comparing their relative preference ratings, with group (reversal vs. control) as a between-subject factor, and choice (1 vs. 2) as a within-subject factor. The nonsignificant main effects of group and choice, $F(1, 92) = 0.30, MSE = 1.63, p = .59$, and $F(1, 92) = 0.52, MSE = 0.47, p = .47$, were qualified by a significant interaction, $F(1, 74) = 4.09, MSE = 3.72, p = .047, \eta^2 = .05$. Relative preference ratings were higher for choice 1 than choice 2 for the reversal group (5.31 vs. 4.88), $F(1, 33) = 4.97, MSE = 3.09, p = .03, \eta^2 = .13$, but not for the control group (5.20 vs. 5.40), $F(1, 41) = 0.75, MSE = 0.86, p = .39$. Thus, here when the reversal block failed to induce a reversal, it succeeded at decreasing relative preference ratings (cf. Experiment 1B).

**Summary**

Experiment 2 adopted a between-group design that yielded a reversal effect in Experiment 2B but not in Experiment 2A. The sole methodological difference across Experiment 2A and 2B was the addition of relative preference ratings. However, a reversal effect occurred in
Experiment 1 whether these ratings were made (Experiment 1B) or not (Experiment 1A), making it less likely that this difference is critical. Further testing is needed to reconcile the discrepancy in results across Experiments 2A and 2B. I suggest that a “fluke” higher rate of reversals in the control group in Experiment 2A than 2B may have been the culprit. Furthermore, the power to detect a reversal effect in Experiment 2A of the small-to-medium size found in Experiment 2B was only .52, thus preventing strong conclusions from being drawn about Experiment 2A.

Analyses of relative preference ratings yielded two important effects in Experiment 2B. First, participants who reversed generally provided lower relative preferences ratings for their choice 1 paintings than consistent participants, suggesting that weaker initial preferences might increase later reversals. Second, among consistent participants, relative preferences for average-beauty pairs across choice 1 and 2 decreased for the reversal group, whereas they increased (though not significantly) for the control group. This pattern was not obtained in Experiment 1B, but consistency was measured somewhat differently there (i.e., at the block level). Speculatively, the participant-level measure afforded by the between-group design in Experiment 2 may have provided a more reliable measure of consistency, thus enabling an effect to be detected.
CHAPTER 4: EXPERIMENT 3

Adding a Delay Block

The goal of Experiment 3 was to try to amplify the reversal effect in the between-group design, given the mixed evidence for the effect in Experiment 2. To this end, I increased the delay between choice 1 and 2 by adding a 12-trial delay block either immediately after the choice 1 (Experiment 3A) or immediately before choice 2 (Experiment 3B), as shown in Figure 1. Experiment 3 was otherwise identical to Experiment 2B.

The influence of delay (and/or of interference from additional trials; Experiment 3 confounded the two) on reversals has not been systematically investigated. However, delay has been shown to modulate other effects, including the influence of misinformation on memory. Loftus, Miller, and Burns (1978) found that participants’ memory for critical details of an initial event (i.e., a stop sign) was impaired by misleading post-event information (i.e., a question that implied it was a yield sign), relative to neutral post-event information (i.e., a question that did not mention a sign type). This misinformation effect was greater when a delay occurred between the initial event and the misinformation, relative to when it occurred between the misinformation and the final test. In other words, misinformation had more impact when memory for the initial event was weaker and when memory for the misinformation was fresher.

If delay has an analogous influence on choice reversals, then the reversal effect might be more robust when the delay block occurs just after choice 1 and prior to the reversal/control block (i.e., in Experiment 3A). Alternatively, putting the delay block right after choice 1 might allow time for participants’ initial choice to consolidate in memory (e.g., Muller & Pilzecker, 1900). If so, then putting the delay block after the reversal/control block and just prior to choice
2 might yield a more robust reversal effect (i.e., in Experiment 3B). This placement would prevent consolidation of choice 1 while also allowing the reversal block influence to consolidate.

**Method**

**Participants**

Additional participants from the same pool completed Experiment 3A (N = 96, 73 female; mean age 21) or Experiment 3B (N = 96, 77 female; mean age 20), and assignment of the reversal versus control group was counterbalanced (48 per group, per experiment). No participants were replaced for exceeding the 30 min criterion.

**Procedure**

Experiment 3 was identical to Experiment 2B, except for the addition of a delay block (see Figure 1). A delay block was inserted either before (Experiment 3A) or after (Experiment 3B) each reversal/control block. The 12 trials in each delay block used paintings not presented elsewhere, and the block matched the reversal/control blocks in structure (2 average/low pairs, 2 average/high pairs, 2 high/low pairs), except each set of 6 trials was presented twice in a different random order each time.

**Results and Discussion**

**Experiment 3A**

**Choice reversals.** When the delay block occurred just after choice 1, the rate of choice reversals was identical in the reversal and control groups (.29 vs. .29), \( \chi^2(1, N = 96) = 0.00, p = 1.00 \) (Figure 5A). Adding a delay after choice 1 may have allowed participants’ initial choice to become consolidated, resulting in a less effective reversal manipulation. If so, then a reversal effect should occur in Experiment 3B, where the delay occurred after each reversal/control block.

**Relative preference ratings.** Relative preference ratings for choice 1 were analyzed as in
Figure 5. Experiments 3 and 4: Choice reversal rate (proportion of participants who made 1 or 2 reversals) by group in Experiment 3A (A) and 3B and 4 (B); relative preference rating difference from the midpoint of 5 in Experiment 3A (C; 95% CI) and Experiments 3B and 4 (D; 95% CI) by group and by whether participants reversed (1 or 2 reversals) or were consistent in both blocks.

Experiment 2, with group (reversal vs. control) and consistency (reversed vs. consistent) as between-group factors (Figure 5C). The main effect of group was not significant, $F(1, 92) = 0.17$, $MSE = 0.44$, $p = .68$, and ratings were similar in the reversal and control groups (5.05 vs. 4.91). However, there was a main effect of consistency: Participants who reversed again had lower choice 1 ratings than consistent participants (4.43 vs. 5.21), $F(1, 92) = 4.58$, $MSE = 11.98$, $p = .04$, $\eta^2 = .05$. The interaction was marginally significant, $F(1, 92) = 3.80$, $MSE = 9.94$, $p = .054$, $\eta^2 = .04$: Initial relative preference ratings were lower for reversed than consistent participants in
the reversal group (4.00 vs. 5.49), \( F(1, 48) = 9.09, MSE = 21.88, p < .01, \eta^2 = .16 \), but not in the control group (4.86 vs. 4.93), \( F(1, 48) = 0.02, MSE = 0.05, p = .90 \). This result provides some indication that lower initial relative preference ratings are predictive of choice reversals.

Relative preference ratings for choice 2 were also analyzed as a function of group and consistency. The similarity in choice 2 ratings for reversal and control groups (5.38 vs. 5.33), \( F(1, 92) = 1.20, MSE = 3.72, p = .28 \), and for reversal and consistent participants (5.21 vs. 5.41), \( F(1, 92) = 0.25, MSE = 0.77, p = .62 \), were qualified by a significant interaction, \( F(1, 92) = 8.30, MSE = 25.72, p = .01, \eta^2 = .08 \). As for choice 1, relative preference ratings for choice 2 were lower for reversal than consistent participants in the reversal group (4.43 vs. 5.76), \( F(1, 46) = 354.91, MSE = 1030.37, p = .02, \eta^2 = .12 \), but not in the control group (6.00 vs. 5.05), \( F(1, 46) = 2.67, MSE = 8.78, p = .11 \). Therefore, in the reversal group, those who reversed had a lower relative preference for their new choice than those who did not switch showed for their consistent choice.

Relative preference ratings for consistent participants were again examined as a function of group (reversal vs. control) and choice (1 vs. 2), to determine if the reversal manipulation weakened consistent participants’ relative preference ratings. The analysis revealed no significant effects. Ratings were similar for the reversal and control groups (5.63 vs. 4.99), \( F(1, 66) = 2.70, MSE = 13.60, p = .11 \), and for choice 1 and 2 (5.21 vs. 5.41), \( F(1, 66) = 1.34, MSE = 1.44, p = .25 \), and the interaction was not significant, \( F(1, 66) = 0.17, MSE = 0.18, p = .68 \). The reversal paradigm did not reliably influence consistent participants in Experiment 3A, either by increasing reversals or decreasing relative preference ratings for consistent participants.

**Experiment 3B**

**Choice reversals.** In contrast to Experiment 3A, inserting the delay block after the contrast manipulation yielded a larger reversal effect in the reversal group than in the control
group (.42 vs. .23), \( \chi^2(1, N = 96) = 3.85, p = .049, \Phi = .20 \) (Figure 5B). The reversal effect was not much larger here than in Experiment 2B, suggesting that delay did not amplify the effect. However, the effect was detectable at the same 18-trial lag between critical trials used in Experiment 3A where the effect was completely absent.

**Relative preference ratings.** Relative preference ratings (Figure 5D) for choice 1 were again examined as a function of group and consistency. Despite the significant reversal effect, ratings did not differ for reversal and control groups (5.68 vs. 5.06), \( F(1, 92) = 1.59, MSE = 6.04, p = .21 \), or for reversed and consistent participants (5.24 vs. 5.43), \( F(1, 92) = 0.42, MSE = 1.58, p = .52 \), and the interaction was also not significant, \( F(1, 92) = 0.53, MSE = 2.01, p = .45 \). Thus, initial relative preference ratings were not predictive of reversals.

The choice 2 analysis also did not reveal significant effects. Ratings were similar in the reversal and control groups (4.81 vs. 4.55), \( F(1, 92) = 0.14, MSE = 0.72, p = .71 \), and for reversed and consistent participants (4.66 vs. 4.69), \( F(1, 92) = 0.01, MSE = 0.06, p = .91 \), and there was no interaction, \( F(1, 92) = 0.20, MSE = 0.98, p = .66 \). Relative preference ratings were not predictive of reversals in Experiment 3B, unlike in Experiments 2B and 3A.

Next, I focused on consistent participants’ relative preference ratings as a function of their group (reversal vs. control) and choice (1 vs. 2). Ratings were similar for consistent participants in the reversal and control groups (5.43 vs. 4.78), \( F(1, 63) = 2.50, MSE = 13.25, p = .12 \). Relative preference ratings were lower for choice 2 than for choice 1 (4.69 vs. 5.43), \( F(1, 63) = 4.76, MSE = 18.91, p = .03, \eta^2 = .07 \), but the interaction with group was not significant, \( F(1, 63) = 0.42, MSE = 1.68, p = .52 \). Thus, consistent participants showed similarly reduced relative preference ratings for their choice 2 painting. Speculatively, the delay, consisting of 12
trials with a “fresh” set of paintings, may have made participants tire of the “stale” average - beauty painting pairs and thus led them to give lower relative preference ratings.

Summary

Experiment 3 again provided mixed evidence for a reversal effect, here when the delay between choice 1 and 2 was increased. The effect was completely absent when a delay block was placed before each reversal/control block (Experiment 3A), whereas a significant effect occurred when a delay block was placed after each reversal/control block (Experiment 3B). However, the effect in Experiment 3B was not much larger than in the otherwise identical condition in Experiment 2B without a delay. The absence of the reversal effect in Experiment 3A suggests that delay has a different influence in the reversal paradigm than in the misinformation effect paradigm (Loftus et al., 1978). Instead, I speculate that a delay after choice 1 may have allowed those choices time to consolidate. Placing the delay after the reversal blocks in Experiment 3B allowed as much time for consolidation of choice 1 as occurred in Experiments 1 and 2, while providing more time to consolidate the choices made within the reversal block. I elaborate further on the potential role of consolidation in producing reversals in the General Discussion.

As in Experiment 2, the power to detect the small-to-medium effect reported in Experiment 3B within Experiment 3A was only .50, therefore it is also possible that Experiment 3A was simply a false negative result. A replication of Experiment 3A with greater power will be needed.

The effects on relative preference ratings were inconsistent across Experiments 2B, 3A and 3B. For example, reversed participants had lower choice 1 ratings than consistent participants in Experiment 2B and 3A, but not in Experiment 3B. The reversal group also gave lower ratings for choice 2 in Experiment 3A, but not in Experiments 2B or 3B. And ratings did not differ between choice 1 and 2 for consistent participants in the reversal group in either
Experiment 3A or 3B, but did so in Experiment 2B. Therefore, further consideration of these ratings is reserved until a pooled analysis across experiments is reported after Experiment 5.
Testing a Nonreversal Control Group

In Experiments 1-3, the reversal and control block trials were identical except for whether the average-beauty paintings within those trials were the choice 1 and 2 pair or another, novel pair. However, this one difference could generate a reversal effect through two potential means: (1) a genuine effect of the contrast manipulation on the average-beauty paintings in the reversal blocks (as has been assumed thus far), and/or (2) an effect of presenting each average-beauty painting more often in reversal blocks than control blocks. Each member of a critical pair of paintings was presented twice in reversal blocks but never in control blocks, thus the reversal effect could somehow be an indirect effect of more exposures to the average-beauty paintings rather than to the influence of the choice- and nonchoice+ manipulations.

To examine this possibility, I tested a nonreversal control group in Experiment 4. This group was identical to the reversal group in Experiment 3B (where a reversal effect was found) except the contrast trials were now set up to reinforce participants’ initial choices, rather than to reverse them. To do so, in each block participants were given 2 nonchoice- trials to weaken the nonchoice painting, and 2 choice+ trials to strengthen the choice painting (see Figure 1). If the reversal effect in Experiment 3B was due to contrast, the nonreversal group’s reversal rate should match the control group in Experiment 3B. If the reversal effect in Experiment 3B was due to more exposures to the average-beauty paintings, the nonreversal group’s reversal rate should match the reversal group in Experiment 3B. If both contrast and exposures promote reversals, then an intermediate effect should be obtained.
Method

Participants

An additional 48 participants from the same pool were tested (27 female; mean age 21; 1 participant was replaced for exceeding 30 min) following data collection in Experiment 3B.

Procedure

The nonreversal group in Experiment 4 was identical to the reversal group in Experiment 3B, except for the structure of the 4 contrast trials (see Figure 1). Participants received 2 nonchoice- trials where the nonchoice painting was paired with high-beauty paintings, to reinforce the participant’s initial choice, and 2 choice+ trials where the choice painting was paired with low-beauty paintings, again to reinforce the participant’s initial choice.

Results and Discussion

Choice reversals

The nonreversal group in Experiment 4 was compared to the reversal and control groups from Experiment 3B. Although the reversal rate was lower for the nonreversal group than the reversal group (.29 vs. .42), this difference did not reach significance, $\chi^2(1, N = 96) = 1.64, p = .20$ (Figure 5B). Similarly, although the reversal rate was higher for the nonreversal group than the control group, this difference also did not reach significance (.29 vs. .23), $\chi^2(1, N = 96) = 0.49, p = .49$. In sum, the nonreversal group rate was neither significantly lower than the reversal group rate, nor significantly higher than the control group rate.

Relative preference ratings

Relative preference ratings were similar for nonreversal group members who reversed vs. were consistent, both for choice 1 (4.32 vs. 5.10), $F(1, 46) = 2.15, MSE = 6.05, p = .15$, and choice 2 (4.46 vs. 5.32), $F(1, 46) = 2.14, MSE = 7.32, p = .15$ (Figure 5D). Although the choice
reinforcement intended by the nonreversal block might have been expected to strengthen consistent participants’ relative preference ratings, their choice 1 and 2 ratings were similar (5.10 vs. 5.32), $F(1, 33) = 0.81, MSE = 0.83, p = .38$, suggesting that the intended reinforcement may not have occurred.

**Summary**

The nonreversal group in Experiment 4 showed a reversal effect that was not different from either the reversal or control groups in Experiment 3B. Although this outcome is ambiguous, and requires further replications, it suggests that the reversal effect may be partly due to the contrast manipulation, and partly due to extra exposures to the average-beauty paintings. It was surprising that the nonreversal group, whose choices were reinforced during the nonreversal block trials, did not show a lower reversal rate than the control group. Future studies should include a nonreversal condition, with an eye on more definitively establishing which aspect(s) of the reversal block generate reversals.
CHAPTER 6: EXPERIMENT 5

Adding More Reversal/Control Blocks

Experiment 5 reports a final attempt to amplify the reversal effect—this time by repeating the contrast manipulation. Here the reversal/control blocks were presented three times between choice 1 and 2, rather than once. This resulted in the same lag between choice 1 and choice 2 as in Experiments 3A and 3B (i.e., 18 trials).

Method

Participants

An additional 96 participants from the same pool were tested (57 female; mean age 21), half in the reversal group and half in the control group. None warranted replacing for exceeding the 30 min criterion.

Procedure

Experiment 5 was identical to Experiment 2B, except the reversal and control blocks were presented three times (in a freshly randomized order each time), rather than once.

Results and Discussion

Choice reversals

Although the reversal effect was in the expected direction, it was not larger in the reversal group than the control group (.38 vs. .25), $\chi^2(1, N = 96) = 1.75, p = .19$ (Figure 6A).

Relative preference ratings

Relative preference ratings for choice 1 were again evaluated as a function of group and consistency (Figure 6B). Ratings were similar for the reversal and control groups (5.31 vs. 5.43), $F(1, 92) = 0.02, MSE = 0.07, p = .88$, for participants who reversed and for those who did not (5.05 vs. 5.52), $F(1, 92) = 1.49, MSE = 4.81, p = .23$, and group and consistency did not interact,
$F(1, 92) = 0.56, MSE = 1.82, p = .46$. Initial relative preference ratings did not predict reversals, like in Experiment 3B and 4, but unlike in Experiment 2B.

![Graph](image)

**Figure 6.** Experiment 5 results: Choice reversal rate (proportion of participants who made 1 or 2 reversals) by group; relative preference rating difference from the midpoint of 5 in Experiment 5 (C; 95% CI) by group and by whether participants reversed (1 or 2 reversals) or were consistent for both blocks.

The choice 2 analysis, again with group and consistency factors, also did not reveal any significant effects. Ratings were similar for reversal and control groups (5.69 vs. 5.79), $F(1, 92) = 0.01, MSE = 0.04, p = .91$, and for reversed and consistent participants (5.52 vs. 5.84), $F(1, 92) = 0.78, MSE = 2.37, p = .38$. The interaction was also not significant, $F(1, 92) = 0.48, MSE = 1.44, p = .49$. Relative preference ratings for choice 2 were not dependent on group, or whether participants showed a reversal, as was true for choice 1, and in line with Experiments 2B and 3B.

I again assessed the relative preference ratings for consistent participants as a function of group and choice. Ratings were similar for consistent participants in the reversal and control groups (5.55 vs. 5.76), $F(1, 63) = 0.28, MSE = 1.48, p = .60$. However, the main effect of choice was marginal, in the direction of an *increase* in relative preference ratings from choice 1 to 2 (5.49 vs. 5.84), $F(1, 63) = 3.34, MSE = 3.85, p = .07, \eta^2 = .05$. The opposite pattern should have
occurred if the contrast manipulations reduced participants’ relative preference. The interaction between group and choice was not significant, $F(1, 92) = 0.004, MSE = 0.005, p = .95$.

**Summary**

Contrary to expectation, repeating the contrast block three times did not increase its potency and amplify the reversal effect—indeed, the effect did not reach significance here. Replicating Experiment 5 with more participants is necessary, however, because the power to detect the small-to-medium effect found in Experiment 2B was only .52.
CHAPTER 7: POOLED ANALYSES OF EXPERIMENTS 2, 3, AND 5

Participants in Experiments 2-5 had only 2 opportunities to reverse their choices, thus even with groups of 48, it is perhaps not surprising that a reversal effect was not consistently obtained. Indeed, the power to detect a reversal effect in the experiments yielding nonsignificant reversals was low. Moreover, the substantial reversal rates in the control groups were unanticipated. This section reports more powerful, pooled analyses of the reversal and relative preference rating effects, as well as of an additional measure of the role of the contrast manipulation in generating a reversal effect.

Choice Reversals

To provide a more powerful test of the reversal effect, I pooled the reversal groups (N = 240) and the control groups (N = 240) from Experiments 2A, 2B, 3A, 3B, and 5—all of which used the between-group design (recall that the reversal effect was also obtained in the within-subject design in both Experiment 1A and 1B). Across the pooled experiments, the reversal rate was 11% greater in the reversal group than in the control group (.35 vs. .24), $\chi^2(1, N = 480) = 6.28, p = .01, \Phi = .26$ (Figure 7A), suggesting that the reversal effect can be obtained with sufficient power. Nonetheless, the General Discussion considers reasons why the reversal effect may have been more robust in some experiments than others.

Relative Preference Ratings

I also pooled the reversal groups (N = 192) and the control groups (N = 192) from Experiments 2B, 3A, 3B, and 5 to determine whether there were any consistent effects on relative preference ratings, given these effects were also variable across experiments. Relative preference ratings (Figure 7B), pooled across experiments, were analyzed separately for each choice as a function of group (reversal vs. control) and participant consistency (reversed vs.
Figure 7. Pooled Experiments 2, 3, and 5 results: Choice reversal rate (proportion of participants who made 1 or 2 reversals) by group (A); relative preference rating difference from the midpoint of 5 (B; 95% CI) by group and by whether participants reversed (1 or 2 reversals) or were consistent for both blocks; higher-beauty choice rate in contrast manipulation trials (C; 95% CI) by group and by whether participants reversed or were consistent for both blocks; boredom and decision fatigue ratings (D; 95% CI) by group and by whether participants reversed or were consistent for both blocks; choice reversal rate by group and initial choice identification accuracy (E; 95% CI).
consistent). Choice 1 ratings were not significantly different between the pooled reversal and control groups (5.29 vs. 5.10), $F(1, 380) = 1.17, MSE = 3.75, p = .28$. However, a main effect of consistency was obtained, reflecting lower relative preference ratings for choice 1 for participants who showed a reversal compared to consistent participants (4.82 vs. 5.35), $F(1, 380) = 7.27, MSE = 23.34, p = .01, \eta^2 = .02$. The interaction was nonsignificant, $F(1, 380) = 0.17, MSE = 0.54, p = .68$.

No significant effects were found in the choice 2 analysis, where relative preference ratings were similar for reversal and control groups (5.17 vs. 5.22), $F(1, 378) = 0.33, MSE = 1.22, p = .56$, and for reversed and consistent participants (5.01 vs. 5.28), $F(1, 378) = 1.09, MSE = 3.97, p = .30$, and the interaction was not significant, $F(1, 378) = 1.06, MSE = 3.85, p = .30$. Choice 2 ratings were not lower for reversed than for consistent participants, which is somewhat surprising given that the former participants chose a different painting. Perhaps participants were biased towards the midpoint of the relative preference rating scale. More intriguingly, perhaps the contrast manipulation leads participants to symmetrically switch from moderate preference from one painting to a moderate preference for the other painting.

Finally, ratings for consistent participants did not differ as a function of group (reversal vs. control; 5.42 vs. 5.21), $F(1, 272) = 1.12, MSE = 5.97, p = .29$, or choice (1 vs. 2; 5.34 vs. 5.28), $F(1, 272) = 0.40, MSE = 0.72, p = .53$, and the two factors did not interact, $F(1, 272) = 0.92, MSE = 1.64, p = .34$. Thus, with the exception of a trend in Experiment 2B, there was no evidence that consistent participants’ ratings were influenced by the reversal block.

**Higher-Beauty Choice Rate**

Although the reversal effect in the pooled analysis was significant, the reversal rate in the nonreversal condition in Experiment 4 was not different than either the reversal or control group.
rates in Experiment 3B. If contrast induces the reversal effect, then the reversal rate should have been lower in the nonreversal condition than in the reversal group. Thus, the reversal effect may have been partly due to the greater number of exposures to the average-beauty paintings in the reversal blocks, rather than to the contrast manipulation per se. For example, perhaps with more exposures participants tire of both the average paintings, biasing them toward reversing.

I analyzed another measure of the effectiveness of the contrast manipulation to try to better establish a functional role for contrast in producing the reversal effect. Specifically, I measured the mean rate of participants’ higher-beauty choices (i.e., average over low, high over average, high over low) within the reversal or control block trials, pooled across Experiments 2A, 2B, 3A, 3B, and 5 (Figure 7C). If the reversal effect is driven by the contrast manipulations then reversals in the reversal group should be associated with more choosing of the higher-beauty option. If the reversal effect in the reversal group is driven by more exposures to the average-beauty paintings, there is no reason to expect reversals to be associated with making more higher-beauty choices.

Higher-beauty choice rates were compared in an ANOVA with group (reversal vs. control) and consistency (reversed vs. consistent) as between-group factors. Neither main effect was significant: reversal and control groups chose higher-beauty paintings at a similar rate (.76 vs. .80), $F(1, 476) = 1.24, MSE = .04, p = .27$, as did reversal and consistent participants (.80 vs. .77), $F(1, 476) = 1.55, MSE = .05, p = .21$. However, critically, the interaction was significant, $F(1, 476) = 4.80, MSE = .16, p = .03, \eta^2 = .02$. In the reversal group, participants who reversed made more higher-beauty choices than those who did not reverse (.80 vs. .74), $F(1, 238) = 6.67, MSE = .22, p = .01, \eta^2 = .03$. In the control group, participants who reversed did not make more higher-beauty choices (.78 vs. .80), $F(1, 238) = .40, MSE = .01, p = .53$. This pattern provides
important new evidence that the reversal effect was due, at least in part, to the contrast manipulation, rather than to more exposures to the average-beauty paintings.
CHAPTER 8: POTENTIAL MODULATORS OF CHOICE REVERSALS

In this section I report analyses of several potential modulators of choice reversals that were measured after the choice trials in Experiments 2A, 2B, 3A, 3B, and 5. For brevity, and to maximize power, these measures were analyzed after pooling this set of experiments.

Boredom Ratings

In each block, participants repeatedly viewed, chose between, and then rated their relative preference for the same small set of paintings. As a result, across trials they may have become bored with the stimuli and/or task, which in turn might have made them more susceptible to reversals. This possibility is consistent with the increase in the reversal effect from block 1 to 2 in Experiment 1. Boredom might even lead participants to develop a novelty preference (cf. the mere exposure effect), thereby leading them to reverse on later trials. The critical average-beauty paintings appeared in reversal blocks but not in control blocks, thus boredom might be greater in the reversal group. Finally, participants who reversed, regardless of group, may have experienced more boredom than consistent participants.

To explore whether boredom influenced reversals, after the main blocks of trials the survey asked participants to “Please rate your level of interest in the choice task at this moment (1 = bored, 5 = neither bored nor interested, 9 = interested)”. These boredom ratings, pooled across Experiments 2, 3, and 5, were analyzed in an ANOVA with group (reversal vs. control) and participant consistency (reversed vs. consistent) as between-group factors (Figure 7D). The main effect of group was significant, but contrary to expectation, it reflected lower boredom ratings in the reversal group than the control group (6.01 vs. 6.25), $F(1, 476) = 4.49, MSE = 19.72, p = .04, \eta^2 = .01$. The reversal groups may thus have been more engaged in the task, and this engagement might have contributed to the increase in reversals. Although the main effect of
consistency was not significant—boredom ratings were similar for reversed and consistent participants (6.16 vs. 6.12), $F(1, 476) = 0.37, MSE = 1.62, p = .54$—the interaction was significant, $F(1, 476) = 4.95, MSE = 21.78, p = .03, \eta^2 = .01$. In the reversal group, boredom ratings were similar for reversed and consistent participants (5.78 vs. 6.13), $F(1, 238) = 1.32, MSE = 6.44, p = .25$. In the control group, however, boredom ratings were higher for reversed than consistent participants (6.71 vs. 6.10), $F(1, 238) = 4.07, MSE = 15.97, p = .045, \eta^2 = .02$, suggesting that reversals in this group might be associated with less engagement in the task. Although boredom may be related to engagement, it does not seem to modulate the reversal effect directly. Further examination of the effects of the reversal paradigm on boredom and engagement level is needed to assess these possibilities.

**Decision Fatigue Ratings**

Across trials, participants might find it increasingly more difficult to choose or rate their relative preference, again due to the small set of stimuli. This difficulty is referred to as *decision fatigue*, and it has been found to lower self-regulation (e.g., Vohs et al., 2008), and to increase impulsivity and susceptibility to persuasive marketing (e.g., Tierney, 2011; Vohs et al., 2005). In line with the expectations for boredom ratings, because the reversal groups made more choices involving the critical average-beauty paintings, their level of decision fatigue might be higher.

To explore whether decision fatigue might influence reversals, after the boredom rating, the software asked participants to “Please rate how difficult it was for you to make your choices by the end of the task (1 = difficult, 5 = neither easy nor difficult, 9 = easy)”. These ratings were analyzed as for boredom ratings (Figure 7D). The pooled reversal group showed slightly lower decision fatigue than the pooled control group (6.70 vs. 6.89), $F(1, 474) = 2.78, MSE = 8.69, p = .096$, following the pattern of boredom ratings (and contrary to expectation). Fatigue ratings
were similar for reversed and consistent participants (6.71 vs. 6.83), $F(1, 474) = 0.11$, $MSE = 0.34$, $p = .741$. The interaction was marginally significant, $F(1, 474) = 2.77$, $MSE = 8.68$, $p = .097$, but decision fatigue was similar for reversed and consistent participants in both the reversal group (6.47 vs. 6.83), $F(1, 237) = 2.34$, $MSE = 7.01$, $p = .13$, and the control group (7.07 vs. 6.83), $F(1, 237) = 0.77$, $MSE = 2.51$, $p = .38$. Interestingly, boredom and decision fatigue ratings were uncorrelated, $r(476) = -0.01$, $p = .76$. In summary, decision fatigue was not a significant modulator of reversals in these experiments.

**Initial Choice Identification Accuracy**

I next examined whether reversals were influenced by whether participants correctly identified their initial choice for each critical average-beauty pair after the main experiment. Participants who remembered their initial choices may have felt more pressure to be consistent with those choices, in line with cognitive dissonance theory (Festinger, 1957). In contrast, participants who could not remember their initial choices may have been more likely to reverse due to a reduction in dissonance and/or due to incorrectly assuming their choices were consistent. In addition, because reversal groups received more exposures to the critical average-beauty paintings than control groups, the former group might show lower initial choice identification accuracy due to greater proactive interference for those paintings.

Beginning with Experiment 2, after making boredom and decision fatigue ratings participants were shown the critical average-beauty pair from each block again, in turn, and were asked to “Please indicate which painting you chose when you were shown this exact pair of paintings in this order earlier in the study.” The paintings appeared in the left/right order presented for choice 1 to help cue participants’ memory.
A logistic regression was conducted on the data pooled across Experiments 2, 3, and 5, with initial choice identification (incorrect [for at least 1 critical pair] vs. correct [on both pairs]) and group (reversal vs. control) as the predictor variables of the reversal rate (Figure 7E). The full model predicted reversals, $\chi^2(3, N = 480) = 177.73, p < .001$. Importantly, a Wald test revealed that initial choice identification was a significant predictor of reversals, $\beta = -3.18$, $\chi^2(1, N = 480) = 11.89, p = .001$, with the odds ratio reflecting the odds of participants reversing when they are correct, compared to when they are incorrect, $\text{Exp}(\beta) = .04$. Surprisingly, group was not a significant predictor once initial choice identification was considered, $\beta = 0.11$, $\chi^2(1, N = 480) = 0.01, p = .92$, $\text{Exp}(\beta) = 1.12$. The interaction was also not a significant predictor, $\beta = -0.13$, $\chi^2(1, N = 480) = .05, p = .79$, $\text{Exp}(\beta) = .88$. Thus, reversals were modulated by initial choice identification accuracy rather than by group.

This analysis raises an important question, namely: Why was a reversal effect found in the pooled analysis, given that group was not a significant predictor of reversals when initial choice identification was included as a factor in the logistic regression? Indeed, the strikingly high reversal rate for incorrect participants was nearly identical for the reversal and control group (.84 vs. .83). An additional chi-square test provided the answer, namely: A greater proportion of participants were incorrect in the reversal group than in the control group (.28 vs. .15), $\chi^2(1, N = 480) = 11.88, p = .001$. Therefore, the reversal effect appears to be linked to lower initial choice identification accuracy. The extra presentations of the critical average-beauty paintings in the reversal blocks may have impaired participants’ ability to recall their initial choice, thus rendering reversal group participants more susceptible to reversals.
CHAPTER 9: GENERAL DISCUSSION

My thesis achieved its two main goals. First, I developed a novel paradigm for rapidly inducing reversals in subjective choices, by using a contrast manipulation previously shown to influence aesthetic judgments (e.g., Mallon et al., 2014; Tousignant & Bodner, 2014). This paradigm complements the set of paradigms that have been found to induce reversals in objective choices (e.g., Oliver, 2006; Trueblood, 2012; Tversky & Kahneman, 1986). Using this paradigm, a choice reversal effect occurred in a within-subject design in which each participant received 1 reversal block and 1 control block (Experiment 1). In addition, a reversal effect also occurred across a pooling of experiments in a between-group design (Experiment 2, 3, 5) in which each participant received either 2 reversal blocks or 2 control blocks. Second, Experiments 2-5 revealed several modulators of choice reversals. For example, participants who gave lower initial relative preference ratings were more likely to reverse. Additionally, participants who incorrectly identified their initial choices were more likely to reverse, and being in the reversal group was associated with a greater likelihood of incorrect identification. Other potential modulators, such as boredom and decision fatigue, were not as clearly predictive of reversals.

Moving forward, it will be important to clarify, through replications and extensions, both the conditions that yield a reversal effect, and those that do not. It will also be important to explore the factors that generally yield choice reversals even in the absence of contrast manipulations. Future work will be informative for research on decision making and aesthetics, as well as for marketers, economists, and laypersons. The remainder of the General Discussion lays out the limitations of the present study and suggests several future research directions.
Establishing a Choice Reversal Paradigm

Establishing a reversal paradigm will require replications of both the significant reversal effects (Experiments 1A/1B, 2B, 3B), as well as the nonsignificant effects (Experiments 2A, 3A, 5), using large enough sample sizes to have sufficient power to detect a small-to-medium effect. In the between-group design, to have a .80 probability of detecting a reversal effect of the size found in Experiment 2B would require at least 150 participants per group. The pooled analysis of the higher-beauty choice rates established a role for the contrast manipulation in producing the reversal effect. However, the ambiguous outcome of the nonreversal condition in Experiment 4 will also requires additional study. In this section I discuss these potential boundary conditions.

The designs of Experiments 1 and 2 were parallel except reversal versus control blocks were varied within-subjects in Experiment 1 and between-subjects in Experiment 2. The within-subject design yielded a consistent reversal effect (Experiment 1A/1B), perhaps due to greater sensitivity. However, I shifted to the between-group design in Experiment 2 for three reasons: (1) to avoid block order effects (present in Experiment 1A), (2) because analysis using chi-squares was more straightforward, and (3) I could compute and compare a simple measure of reversals (i.e., the proportion of each group who showed a reversal). Nonetheless, given the inconsistency of the reversal effect in the between-group experiments, the within-subject design might be more reliable and sensitive, and thus preferable.

In Experiment 3, my attempt to amplify the reversal effect by adding a delay block yielded mixed evidence: A reversal effect occurred when the delay was placed just prior to choice 2 (Experiment 3B), but not when it was placed just after choice 1 (Experiment 3A). The earlier delay may have allowed choice 1 to be consolidated, whereas the later delay may have
prevented choice 1 consolidation prior to the contrast trials, while also facilitating the consolidation of the effects of the contrast trials.

Some evidence for a role for consolidation in a choice paradigm comes from studies of a differentiation and consolidation theory of decision making (e.g., Svenson, 1992; Svenson & Benthorn, 1992). Svenson and Benthorn (1992) asked 12-year-old participants to choose between pairs of different items (e.g., stereos), and to rate each item on several attributes (e.g., price, sound quality). When asked to rate the items again after a delay, ratings for the nonchosen item were often lower than their initial ratings, thus validating participants’ initial choice. In my paradigm, the reversal block trials biased participants way from their chosen painting, and toward their nonchosen painting. When faced with choice 2, participants may (intentionally or not) reverse their choice as a means of validating their selections against their chosen painting and/or as a means of validating their selections toward their nonchosen painting during the reversal block trials.

Experiment 4 tested a nonreversal group in which the contrast manipulation trials were set up to maintain or even reinforce the participant’s initial choice. This group should have yielded a lower reversal rate than the reversal group in Experiment 3B—potentially even lower than that experiment’s control group. Instead, the reversal rate fell between the reversal and control groups, and was not significantly different from either. Therefore, the two extra exposures to each of the two average-beauty paintings in the reversal blocks, relative to the control blocks (who received no additional exposures), may be at least partially responsible for the reversal effect. The nonreversal condition warrants further study, including interleaved testing of reversal, nonreversal, and control groups. Another approach would be to replace the nonreversal group with an exposure-only group in which each choice 1 painting is paired with
additional average-beauty paintings rather than with low- or high-beauty paintings. In this way, the reversal blocks would not include a contrast manipulation. If additional exposures drives the reversal effect, then the reversal rate should be similar in the exposure-only and reversal groups. In contrast, if the contrast manipulation drives the reversal effect, then the effect should be similar in the exposure-only and control groups. This possibility also warrants testing.

In Experiment 5, presenting the reversal/control blocks three times did not amplify the reversal effect—in fact, the effect was not significant here. Speculatively, the additional presentations of the same paintings/pairs may have worked against the reversal effect. To evaluate this possibility, Experiment 5 could be replicated using unique high- and low-beauty paintings in each reversal/control block.

Other methods of amplifying the reversal effect should also be explored. For example, research has shown that people are more susceptible to persuasion when fatigued or under cognitive strain (e.g., Burkley, 2008; Kahneman, 2011). Therefore, performing a cognitive load task during the experiment may interfere with participants’ ability to exert cognitive control and/or to experience cognitive dissonance. In turn, this may make participants more susceptible to the reversal manipulation. Selecting participant-specific stimuli might also amplify the reversal effect. Each participant’s ratings could be used to select low-, average-, and high-beauty paintings. This method would ensure that each average-beauty painting was rated “5” by a given participant, and that the contrast manipulations were maximally potent for each participant.

Finally, the rate of reversals in the control blocks was unexpectedly high in my experiments, particularly given the brief intervals between choice 1 and 2. Perhaps some aspect of the task instructions biased participants toward reversals. For example, participants were told that they would see some pairs of paintings twice. They were also advised to “not feel pressure to
choose the same painting each time”, as well as to “not feel any pressure to choose a different painting each time”. These instructions were designed to try to eliminate any pressure either to reverse or not reverse. However, it is entirely possible that these instructions alerted participants to the dependent measure and made them more likely to reverse in both reversal and control blocks. Assuming a reliable reversal paradigm can be established, future studies should examine the influence of instructions and demand characteristics on reversals.

**Discovering Other Modulators of Choice Reversals**

Future work should also build on my initial examination of the factors that modulate spontaneous reversals. For example, I found some evidence that choice reversals were modulated by relative preference ratings and by initial choice identification accuracy. In this section, I consider these findings in turn before suggested some other potential modulators.

The pooled analysis revealed that reversals were associated with lower initial preference ratings for chosen paintings over nonchosen paintings. Yet across participants the mean ratings were around the midpoint of the scale (5 = somewhat more). Therefore, one obvious means of increasing the rate of reversals (whether induced or spontaneous) would be to select average painting pairs that yield lower initial preference ratings. Reversals might not be deemed very surprising, though, if participants essentially showed no preference for their initial choice. However, by maximizing reversals this means would enhance the study of how contrast manipulations yield reversals.

The pooled analysis also revealed that participants who were incorrect in their initial choice identification were very likely to reverse—regardless of whether they were in the reversal or control group. A critical direction for future research will be to unpack the relationship between initial choice identification and the reversal effect. One possibility is that incorrect
participants failed to recall their initial choice, and thus were able to make their second choice without feeling any pressure to be consistent (i.e., no cognitive dissonance). The converse possibility is that participants felt pressure to be consistent, but those who misremembered their initial choice would therefore be biased toward reversing their choice to reduce this pressure. For example, imagine John chose Painting A over Painting B for choice 1. When presented with choice 2, imagine John misremembers having chosen Painting B for choice 1. If John wishes to make a consistent choice 2, he would therefore choose Painting B and thus show a reversal.

To unpack this factor, participants in future studies could be asked whether they had reversed their choices (via a yes/no question)—before they are asked to identify their initial choice. Doing so would help clarify the relationship between initial choice identification and reversals. For example, imagine that Chris showed a reversal, misidentified his initial choice, and answered “no” to the reversal question. In this case, Chris may have been trying to make consistent choices to reduce his cognitive dissonance, biasing him toward a reversal because he forgot his initial choice. Alternatively, imagine that Justin showed a reversal, correctly identified his initial choice, and answered “yes” to the reversal question. In this case, Justin may have knowingly experienced a genuine choice reversal.

The reversal effect seems to depend largely on participants either forgetting or misremembering their choice 1 response. Therefore, eliminating choice 1 altogether might yield a larger influence of the contrast manipulation on the final average/average trial. The reversal contrast trials would attempt to increase, for example, choosing Painting A over Painting B. If the rate of choosing Painting A is greater in contrast blocks than control blocks (which present Paintings C and D rather than Paintings A and B), then contrast would be shown to bias people’s subjective choices (rather than showing a reversal of a given participant’s choice).
This choice-bias paradigm would also have more applied utility. For example, product brands likely focus their advertising campaigns on consumers who have not yet formed a brand preference, and on retaining consumers who already prefer their brand, rather than trying to reverse an already established brand preference. For example, rather than trying to make a Coke drinker choose Pepsi, Pepsi’s advertisers likely focus on targeting youth to get them to choose Pepsi over Coke for their initial cola experience (e.g., by putting a Pepsi machine in their school), and to reinforce Pepsi drinkers for continuing to choose Pepsi over Coke (e.g., by providing coupons for Pepsi in cases of Pepsi).

**Future Directions**

My thesis also opens up several future directions for the study of choice reversals beyond those considered above. First, future studies should seek out other factors that influence reversals, such as the lag between choice 1 and 2. Another potentially informative modulator could be response times for choice decisions. If choice times are longer when reversals occur then this might indicate that reversals are due to use of a controlled process (e.g., System 2 in Kahneman, 2011). In contrast, if choice times are shorter when reversals occur, this could indicate that reversals are reliance on a more automatic process (e.g., System 1). System 1 is more susceptible to persuasion, and participants in the reversal group who use System 1 over System 2 may therefore be more influenced by the contrast manipulations and thus show more reversals.

Second, future studies should explore whether the reversal paradigm influences the affective component of preferences. For example, physiological measures of arousal (e.g., skin conductance, heart-rate variability) could be used to determine whether reversals are associated with an affective shift toward the new choice. Similarly, asking participants about the purpose of
the study to probe for demand characteristics could provide more insight into whether the
reversal paradigm influences the cognitive component of preferences.

Third, whether the choice reversal paradigm generalizes to other stimulus domains is
worthy of consideration. For example, in the aesthetics domain, it is unclear whether a reversal
effect would occur for representational paintings. Such a test would be interesting in light of
claims that abstract and representational artworks are evaluated differently (e.g., Vessel & Rubin,
2010). Photographs, which have also been found to produce contrast effects (e.g., Tousignant &
Bodner, 2014), and applied stimuli such as logos and consumer products, could also be studied.
Indeed, Zellner, Allen, Henley, and Parker (2006) induced a contrast effect on people’s
preferences for juice samples.

Conclusion

My thesis contributes to our understanding of the factors that modulate reversals in
subjective choices. My findings highlight the curious instability of subjective choices. These
preliminary findings lay the groundwork for extending the paradigm to real-world choice
decisions, making this line of research not only valuable to researchers interested in preferences
and aesthetic judgments, but potentially to marketers and economists as well.
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