

## Not all contrast effects are created equal: extent of processing affects contrast strength

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### Abstract

Prevailing theories of judgmental contrasts propose mechanisms ranging from relatively low versus high degrees of thought. The present research tests the hypothesis that the degree of thought involved in producing judgmental contrast has important implications. In three experiments, participants' ability or motivation to engage in effortful thinking was manipulated. In Experiment 1, varying personal relevance produced equivalent contrast effects, but these judgments differed in certainty. In two additional studies, despite equivalent amounts of contrast, a manipulation of the order of the standards and target of comparison led to differences in certainty (Experiment 2) and attitude-behavioral intention correspondence (Experiment 3). This is the first research to show that amount of thinking has implications for the strength and consequences of the judgment.

### Introduction

Judgments are often comparative in nature. One of the earliest judgmental phenomena uncovered was that of contrast—movement of a target judgment away from an anchor serving as a comparison standard (e.g., Helson & Rohles, 1959; Hovland, Harvey, & Sherif, 1957; Sherif & Hovland, 1961). To illustrate, consider early work that investigated people's attitudinal responses to persuasive communications (Hovland et al., 1957). Researchers found that one's own attitude was used as a standard of comparison, and perceptions of communications that differed greatly from participants' own positions were contrasted. Thus, someone with a very favorable attitude toward prohibition would perceive a communication opposed to prohibition as more against prohibition than someone with an anti-prohibition attitude. Numerous studies have established that contrast is especially prone to occur when there is a large discrepancy between the comparison standards and the target such as when the comparison standards are extreme (e.g., Dijksterhuis et al., 1998; Herr, Sherman, & Fazio, 1983; Mussweiler & Strack, 2000).

Contrast has been observed in various domains, including attitudes (e.g., Hovland et al., 1957; Sherif & Hovland, 1961), behavior (e.g., Crusius & Mussweiler, 2012; Dijksterhuis et al., 1998; Schubert & Hafner, 2003), nonevaluative judgments (e.g., Helson, 1947; Herr et al., 1983), and person

perception (e.g., Herr, 1986; Mussweiler, 2003). Despite substantial interest in judgmental contrast, there is surprisingly little work on moderators that determine when a judgment resulting from contrast is consequential. For example, what factors affect the likelihood of "contrasted" judgments being likely to guide behavior? This is an important applied question as being able to predict and influence behavior is a fundamental goal and contribution of social psychology (e.g., Fishbein & Ajzen, 2010). As a starting point for examining the consequentiality of contrast effects, we pose the question of whether contrast effects that are equivalent in magnitude, but arise through different degrees of thought, can be more or less impactful. This research is, to our knowledge, the first attempt to examine how contrast effects of similar magnitude can be significantly different in their consequences.

### Contrast can be based on low or high thought

Over the last 50 years, some theories have portrayed contrast as resulting from relatively automatic or low thought processes, whereas other theories suggest that contrast can result from relatively high amounts of conscious thought (for a similar argument, see Schwarz & Bless, 2007). For instance, adaptation-level theory (Helson, 1947, 1964) and the inclusion/exclusion model (Schwarz & Bless, 1992, 2007),

propose relatively low elaboration mechanisms for contrast whereby individuals naturally contrast their judgment with a standard relatively automatically and effortlessly. Indeed, some research has shown that contrast can occur when there is little opportunity for thought (e.g., Dijksterhuis et al., 1998; Helson, 1947; Scherer & Lambert, 2009; Schubert & Hafner, 2003).

On the other hand, information search models of contrast such as the selective accessibility model (Mussweiler, 2003, 2007; Mussweiler & Strack, 1999) and the extended search model (Ford & Thompson, 2000) propose more elaborative processes for contrast. According to these models, contrast effects are most likely to occur when features of the target or standard lead perceivers to judge the target and standard to be dissimilar from one another. Then, a selective search supporting the initial dissimilarity judgment occurs, which results in knowledge consistent with the dissimilarity hypothesis becoming more accessible, which in turn leads to contrast. Rather than being the result of a relatively effortless comparison process, contrast is proposed to require a number of steps that indicate a relatively high level of thinking.

### Differential consequences of effortless versus effortful contrast

The objective of the present paper is not to support or refute any existing specific theory or model of contrast, test among them, or determine the level of effort associated with a given theory. Rather, the objective is to understand when contrast effects are likely to be more versus less impactful. Practitioners who aim to use contrast effects in various domains of application (e.g., advertising; see Chien, Wegener, Hsiao, & Petty, 2010; Meyers-Levy & Sternthal, 1993), need to know what might make a contrast effect influential versus unimportant. In the current research, we test whether the amount of thought underlying a contrast effect determines how consequential that contrast effect is even when the same extent of contrast is observed. If our perspective is correct, for example, it would mean that if a person came to view a job applicant as relatively unqualified after exposure to a highly qualified applicant, it would be important to know whether this contrast effect occurred with relatively high or low thought. The more thought that went into the contrast effect, the more consequential (e.g., predictive of hiring) the judgment would be.<sup>1</sup>

Previous work on how consequential attitudes are (i.e., referred to as their *strength*) has demonstrated that higher amounts of thinking tend to produce attitudes that are more persistent over time, resistant to change, and impactful on other judgments and behavior (Krosnick & Petty, 1995). Even

when high versus low thought processes result in apparently similar attitudes (i.e., equivalent in extent of change or extremity), attitudes resulting from high thought are stronger than attitudes resulting from less thought (for a review, see Petty, Haugtvedt, & Smith, 1995). Therefore, two attitudes can appear similar in valence and extremity but these attitudes could be very different in their durability and impact depending on the extent of elaboration involved in their formation.

The idea that attitudes of the same valence and extremity can vary in their strength is one of the key notions promulgated by dual process theories of persuasion such as the elaboration likelihood model (Petty & Cacioppo, 1986) and the heuristic-systematic model (Chaiken, Liberman, & Eagly, 1989) and is also endorsed by the one-process unimodel (Kruglanski & Thompson, 1999; see Chaiken & Trope, 1999, for a review). For example, under low-thinking conditions, attitudes might change because of a highly credible source or being in a good mood because these variables serve as a simple peripheral cue (e.g., if an expert said it, it must be true; e.g., Chaiken, 1980; Petty, Cacioppo, & Goldman, 1981). However, under high-thinking conditions, the same variable can produce the same evaluative judgment by a different process such as by biasing the thoughts generated (e.g., positive emotions or a highly credible source lead to more positive thoughts than negative emotions or a low credible source; see Chaiken & Maheswaran, 1994; Petty, Schumann, Richman, & Strathman, 1993). Notably, even though the extent of attitude change that results from low- and high-thinking processes can be the same, the attitudes formed as a result of high-thinking processes tend to be stronger than those formed under low-thinking processes (e.g., Barden & Petty, 2008; Wegener, Clark, & Petty, 2006).

### Current research: elaboration and strength of contrast judgments

The primary goal of the current research is to provide the first test of the idea that one of the most pervasive judgmental phenomena in social psychology—contrast—is also susceptible to an attitude strength analysis. That is, we propose that contrast effects of similar extremity can differ significantly in their consequences as a function of the extent of thought that goes into the contrast effect. To assess how consequential a contrast judgment is, we start by focusing on one of the most widely accepted and studied indicators of strength—judgmental certainty (see Gross, Holtz, & Miller, 1995; Tormala & Rucker, 2007 for reviews). Certainty refers to a sense that one's judgment is correct or valid. In the domain of attitudes, evaluations of the same valence and extremity that are held with relatively high certainty have been shown to be more persistent (e.g., Bassili, 1996; Bizer, Tormala, Rucker, & Petty, 2006), resistant (e.g., Swann, Pelham, & Chidester,

<sup>1</sup>A similar analysis can be made for the other prominent judgmental bias—assimilation. We return to this issue in our general discussion.

1988; Tormala & Petty, 2002), and predictive of behavior (e.g., Fazio & Zanna, 1978; Rucker & Petty, 2004), than attitudes held with relatively low certainty. After showing consequences for certainty in two experiments, our final experiment demonstrates contrast judgments formed with greater thought are more predictive of behavioral intentions than the same contrast judgments formed with lesser thought.

We set up each experiment to meet two conditions. First, the experiments were designed to produce differences in the amount of thinking. Second, the experiments were designed to result in similar degrees of contrast, despite differences in amount of thinking. Specifically, in all studies, we used strong standards of comparisons that would be salient even when individuals' motivation to think was low, which we expected would result in equivalent degrees of contrast.

## Experiment 1

Experiment 1 manipulated the thoughtfulness of the contrast by manipulating the personal relevance of the stimuli, as considerable prior research has used this induction to influence the extent of thinking (see Petty & Cacioppo, 1990; Thomsen, Borgida, & Lavine, 1995). A manipulation of personal relevance produces more thoughtful processing when personal relevance is high rather than low. Because we used strong and clear comparison standards that would be salient under both high and low thought conditions, we anticipated similar degrees of contrast under both high and low thought (e.g., Dijksterhuis et al., 1998; Herr, 1986; Herr et al., 1983; Mussweiler & Strack, 2000). However, we expected to find greater certainty associated with the judgment when personal relevance was high as opposed to low.

## Method

### *Participants and design*

One hundred ten undergraduates participated in partial fulfillment of a course requirement. Participants were randomly assigned to the cells of a 2 (standards of comparison: desirable, undesirable)  $\times$  2 (personal relevance: high, low) between-subjects experimental design.

### *Procedure*

Participants were informed that the purpose of the study was to provide their opinions of consumer products. They read information designed to make the products (digital media players) low or high in personal relevance. Subsequently, participants received product descriptions about two digital media players that served as the standards of comparison. Each product description had a sentence about the player and

six bulleted characteristics. The standards of comparison (described below) were designed to be either very desirable or undesirable and thus to elicit mostly positive or negative attitudes, respectively.

Next, participants were exposed to information about a moderately desirable digital media player that served as the target of comparison. The target player was moderately positive—neither as good as the desirable players, nor as bad as the undesirable players. The target player was introduced as a great digital media player that had 30 GB of storage, a 10 hour battery life, good durability, and satisfactory sound quality among its features. The standards were presented before the target to allow for a high degree of elaboration of the target when participants were motivated to think. After reading all three descriptions, participants gave ratings of their attitudes toward the target player and their certainty in those attitudes. Finally, participants were thoroughly debriefed and dismissed.

## Independent variables

### *Personal relevance manipulation*

The relevance manipulation was adapted from previous research (e.g., Chaiken & Maheswaran, 1994; Petty, Cacioppo, & Schumann, 1983). First, participants in the high-relevance condition were told that the items they were to view would be test-marketed in the participants' local city, whereas those in the low-relevance condition were told that the products were being test-marketed in a distant city. Second, participants in the high-relevance condition were also told that the products might be available as a free gift, whereas participants in the low-relevance condition were told that their gift would not be any of the products in the experiment. Participants should think more about products that they could purchase and that they might receive as a gift compared with ones they would not have an opportunity to purchase and would definitely not receive.

### *Standards of comparison*

Participants received descriptions of either extremely desirable or undesirable products relative to the target. The standards were modeled after the most and least desirable players available at the time of the study in order to ensure that they would result in contrast. In the desirable standards condition, both of the desirable players had 80 GB of storage, 20 hour battery life, excellent sound, and extremely good durability among the features. In the undesirable standards condition, both of the standards had less than 3 GB of storage, 5 hours of battery life, average sound, and poor durability among their attributes.

## Dependent measures

### Attitudes

Participants provided their attitudes toward the target product on four 7-point semantic differential scales (*bad-good*, *dislike-like*, *negative-positive*, *unfavorable-favorable*). These items were highly correlated and averaged to form an overall index of the attitude toward the target product (Cronbach's  $\alpha = .98$ ).

### Certainty

To assess certainty, participants were asked three items adapted from prior research (e.g., Rucker & Petty, 2004). We asked participants how certain they were of their attitudes, how convinced they were that their attitudes were correct, and how confident they were in their attitudes. Responses were given on 7-point scales (*not at all certain/convinced/confident* to *extremely certain/convinced/confident*). These items were highly correlated and averaged to form an index of overall certainty (Cronbach's  $\alpha = .95$ ).

## Results

### Attitudes

As expected, there was a contrast effect on attitudes. Those in the undesirable standards condition rated the target media player more positively ( $M = 6.18$ ,  $SD = .99$ ) than those in the desirable standards condition ( $M = 3.89$ ,  $SD = 1.31$ ),  $F(1, 106) = 106.75$ ,  $p < .001$ ;  $\eta_p^2 = .50$ . There were no other significant effects ( $F_s < 1$ ). Means are displayed in Table 1.

### Attitude certainty

Consistent with our key hypothesis, there was a significant main effect of the personal relevance manipulation such that participants in the high-relevance condition reported greater certainty ( $M = 5.72$ ,  $SD = 1.29$ ) in their judgment of the target product than those in the low-relevance condition ( $M = 5.23$ ,  $SD = 1.29$ ),  $F(1, 106) = 5.62$ ,  $p = .02$ ;  $\eta_p^2 = .05$  (see Table 1). In addition, participants were more certain of their attitudes in the undesirable standards condition ( $M = 5.84$ ,  $SD = .96$ ) than in the desirable standards condition ( $M = 5.01$ ,  $SD = 1.54$ ),  $F(1, 106) = 12.97$ ,  $p < .001$ ;  $\eta_p^2 = .11$ . The interaction of relevance and standards was not significant,  $F(1, 106) = 2.38$ ,  $p = .13$ ;  $\eta_p^2 = .02$ .

We also conducted an analysis on the effects of relevance on certainty, but with the extremity of the attitude entered as a covariate to rule out a potential alternative explanation of the certainty effects. Given extremity is an indicator of stronger attitudes (Abelson, 1995), it is conceptually possible that differences in extremity might have affected certainty. Although we did not observe a difference in attitudes as a function of personal relevance, it is possible that differences in absolute magnitude, as opposed to favorability, explained the differences in certainty. If this alternative interpretation were true, including extremity as a covariate should eliminate the effect of personal relevance on attitude certainty. To test this possibility, extremity was calculated by taking the absolute value of participants' attitude scores minus the theoretical midpoint of the scale (Barden & Petty, 2008). Critically, the effect of the relevance condition remained significant,  $F(1, 105) = 5.80$ ,  $p = .02$ ;  $\eta_p^2 = .05$ .

**Table 1** Impact of Desirability of Standard and Amount of Thought on Extent of Contrast and Strength of Contrast From Experiments 1–3

| Experiment 1     |                       |                    |                       |                    |
|------------------|-----------------------|--------------------|-----------------------|--------------------|
| Condition        | Attitude ratings      |                    | Attitude certainty    |                    |
|                  | Undesirable standard  | Desirable standard | Undesirable standard  | Desirable standard |
| Low relevance    | 6.10 (1.04)           | 3.95 (1.34)        | 5.74 (1.03)           | 4.54 (1.31)        |
| High relevance   | 6.25 (.94)            | 3.84 (1.32)        | 5.94 (.89)            | 5.45 (1.63)        |
| Experiment 2     |                       |                    |                       |                    |
| Condition        | Luxuriousness ratings |                    | Attitude certainty    |                    |
|                  | Nonluxurious standard | Luxurious standard | Nonluxurious standard | Luxurious standard |
| Standards after  | 5.25 (.95)            | 4.77 (.84)         | 5.52 (1.16)           | 5.42 (1.12)        |
| Standards before | 5.23 (.89)            | 4.67 (1.01)        | 5.73 (.98)            | 5.88 (.95)         |
| Experiment 3     |                       |                    |                       |                    |
| Condition        | Attitude ratings      |                    | Behavioral intentions |                    |
|                  | Undesirable standard  | Desirable standard | Undesirable standard  | Desirable standard |
| Standards after  | 4.58 (1.37)           | 3.98 (1.10)        | 4.47 (1.37)           | 4.04 (.92)         |
| Standards before | 5.06 (1.12)           | 3.82 (1.06)        | 5.08 (.95)            | 3.95 (1.01)        |

## Discussion

The results of Experiment 1 provide initial support for our hypothesis that contrast effects comparable in extremity can occur under both high- and low-thinking conditions, but high-thought contrast judgments are stronger. The personal relevance manipulation did not affect attitudes; rather attitudes were only affected by the desirability of the standards. Most importantly, the personal relevance manipulation impacted attitude strength, assessed via judgmental certainty. Participants who showed contrast judgments in the high-relevance condition were more certain of their judgments than participants who showed equivalent contrast effects in the low-relevance condition, providing the first evidence that contrast effects that appear similar can differ in their strength.

It is important that the differences in attitude certainty held controlling for the absolute extremity of the attitude. Because certainty and extremity are sometimes correlated, increases in extremity can lead to increases in certainty and vice versa (Gross et al., 1995). However, because differences of personal relevance on attitude certainty remained significant after controlling for absolute extremity, we have evidence that the amount of processing, not extremity, was responsible for the obtained certainty differences.

There was an unexpected main effect of standards desirability on certainty, such that people who compared the target player to undesirable media players were more certain than people who compared the target to desirable players. The informational differences between the undesirable players and the target player were more pronounced than the differences between the desirable players and the target player, so, despite a similar degree of elaboration, participants might have found the difference between the undesirable players and the target player more apparent, and this might have influenced certainty (see Haddock, Rothman, Reber, & Schwarz, 1999). Whatever the cause, this effect is not problematic for our main objective, which was to test the hypothesis that contrast effects of similar magnitude formed through high or low thought processes can appear similar in terms of extremity, but nonetheless differ in strength (i.e., certainty).

Although the results of Experiment 1 were consistent with our core hypothesis and past research suggests that the manipulation of relevance would affect the amount of comparison-relevant thought (see Petty et al., 1995), we did not measure perceived thought. Consequently, in Experiment 2, we measured perceived amount of thought as a potential mediator. We also used a new manipulation of elaboration. When different manipulations of the same construct produce the same effects, we can converge on the hypothesized underlying variable—extent of processing—as being responsible (Shadish, Cook, & Campbell, 2002).

## Experiment 2

Although Experiment 1 supported our hypothesis, the particular manipulation used to influence extent of thinking is one that may be somewhat difficult to implement in some applied contexts. For example, different products or advertising campaigns might naturally differ in their personal relevance and it might sometimes be difficult to make low-relevance products higher in relevance in order to increase elaboration. Thus, it would be desirable to examine whether the extent of thinking can be varied by presenting the same information to all participants at a given level of relevance.

In order to manipulate extent of thinking while holding the information constant, in Experiment 2, we varied the placement of the standards relative to the target. We selected this manipulation because it does not affect personal relevance, and past research has shown that that varying the order of target and standards does not result in differing amounts of contrast (Stapel, Koomen, & van der Pligt, 1997, Study 3).<sup>2</sup> However, we hypothesized that even though order of target and standards would have no effect on the amount of contrast, order would have an effect on the amount of thought engaged in and thus the strength of the contrast produced. When standards precede the target, people have a greater opportunity to elaborate on the comparison of the standards and target (or engage in dissimilarity testing) than when the target precedes the standards. Because people have the standards at the outset, as each new piece of information about the target is encountered they can reflect on the information in comparison to the standards. Thus, the judgment of the target is made online and the contrast may occur online and with relatively more thought. In contrast, when the standards follow the target, the subsequent comparison (or dissimilarity testing) requires that people compare a summary evaluation of the target to a summary evaluation of the standards, which could be less elaborative (for discussion, see Hastie & Park, 1986).

Indirect support for the possibility that timing would affect amount of thought comes from prior work that has found that judgments formed online involve more elaboration than memory-based judgments (e.g., Bizer et al., 2006; Mackie & Asuncion, 1990). As a result, online judgments can be stronger (e.g., held with greater certainty) than memory-based judgments (Bizer et al., 2006). In our paradigm, when the standards follow the target, the judgment of the target in comparison to the standards cannot be made online because the standards are not encountered until after the target. However, when the standards come first, the contrastive judgment of the target compared with the standards can be made online, even though the standards must be remembered. If it

<sup>2</sup>The Drenth committee reviewed this paper as part of the task force to examine the validity of Stapel's work and found no evidence of fraud (Drenth, Zwemmer, de Klerk, & Klaasen, 2012).

is the case that having the standards first facilitates the formation of online contrast judgments of the target, then this should yield more elaborative processing and thus greater attitude certainty than when the standards come second. Therefore, we predicted that the manipulation of order would result in contrastive judgments that do not differ in extremity (Stapel et al., 1997) but do differ in elaboration and thus certainty.

In this experiment, we also measured perceived elaboration and used it as a mediator of the hypothesized order effect on certainty. Prior research has shown that perceived elaboration correlates with actual elaboration and that perceived elaboration mediates the effect of actual elaboration on certainty (Barden & Petty, 2008). Finding mediation of the order effect on certainty by perceived thinking would enhance support for our proposed mechanism.

Finally, we made several additional changes to increase the generalizability of our findings. Although in Experiment 1 we used novel stimuli, in our second experiment, we switched to stimuli that were already familiar to participants. In addition, instead of having people report their overall evaluations of the target, they were asked to make a few more specific judgments (e.g., perceived expensiveness) to show that our effects can occur with specific judgments as well as global evaluations.

## Method

### Participants and design

One hundred seventy-five undergraduates participated in partial fulfillment of a course requirement. Participants were randomly assigned to the cells of a 2 (standards of comparison: nonluxurious, luxurious)  $\times$  2 (placement of standards: before target, after target) between-subjects factorial design.

### Procedure

Participants were told that they would be evaluating an advertisement for the Ramada Hotel chain, and each participant received a 160-word print advertisement. Half of the participants were asked, prior to receiving the advertisement, to compare the Ramada with other popular hotels. The remaining participants were also told to compare the Ramada with other popular hotels, but this instruction came after they had already read the advertisement for the Ramada. The comparison instruction listed hotels that were either more or less luxurious and expensive than the Ramada. Finally, participants judged the quality of the Ramada hotel, rated the certainty in their judgment, and provided an indication of their perceived effort in reaching their judgment.

## Independent variables

### Standards of comparison

Participants in the luxurious standards condition were told to compare the Ramada to other hotels such as the Ritz-Carlton, Hilton, and Embassy Suites, which were chosen on the basis of their price per night (relatively high) as well as their status in our student participant population as luxury hotels as assessed through a pretest. In contrast, participants who received the nonluxurious standards were told to compare the Ramada to other hotels such as Motel 6, Super 8 Motel, and Travelodge, which were chosen on the basis of their price per night (low) and their brand perception of being relatively mundane in a pretest. The luxurious and nonluxurious hotels are extreme on the dimension of luxury, therefore, we expected the Ramada to seem more luxurious when compared with the nonluxurious hotels than to the luxurious hotels (a contrast effect).

### Placement of standards

All participants always read about the Ramada and were told to compare it with the standards (i.e., comparison hotels). However, in the *standards before* condition, participants received the comparison instruction and the names of the comparison hotels prior to reading about the target. In the *standards after* condition, participants received the comparison instruction and the names of the comparison hotels after initially reading about the target. If the participants know of the standards before processing the target, these standards could color assessment of the target's expensiveness in a thoughtful way. If the participants do not know of the standards until after processing the target, however, it is more likely that their overall impression of the target will be directly contrasted from the impressions of the standards, which should require less thought.

## Dependent measures

### Perceived quality

Participants were asked to assess, using 8-point scales, the perceived expensiveness (*very inexpensive* to *very expensive*); perceived quality (*poor quality* to *high quality*); and perceived luxuriousness (*not very luxurious* to *extremely luxurious*) of the Ramada. These items were highly correlated and were averaged to form an overall assessment of the perceived quality of the target hotel (Cronbach's  $\alpha = .75$ ).

### Certainty

Certainty was assessed by asking participants how certain they were in each of their three judgments of the Ramada Inn

on a 7-point scale (*not at all certain* to *extremely certain*). These three items were aggregated to form an overall measure of certainty (Cronbach's  $\alpha = .69$ ). The certainty item was taken from prior research (e.g., Fazio & Zanna, 1978; Rucker & Petty, 2004).

### Perceived elaboration

Perceived elaboration was assessed by asking participants, "How much effort did you put into the task," on an 8-point scale from *very little effort* to *very much effort*.

## Results

### Perceived quality

As anticipated, there was a significant contrast effect such that the target, Ramada, was rated as being higher in quality when the comparison standards were nonluxurious ( $M = 5.24$ ,  $SD = .92$ ) compared with luxurious ( $M = 4.73$ ,  $SD = .92$ ),  $F(1, 171) = 13.44$ ,  $p < .001$ ;  $\eta_p^2 = .07$  (see Table 1). No other effects were significant ( $F_s < 1$ ). Thus, replicating prior research (Stapel et al., 1997), the contrast effect was equivalent regardless of the order in which standards and target were presented.

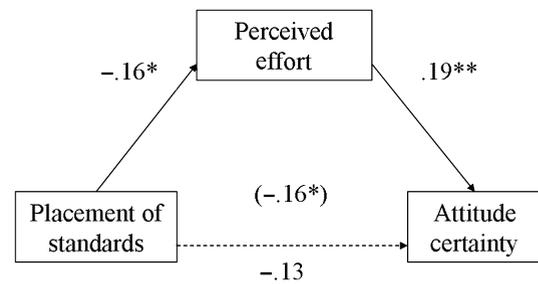
### Certainty

Participants reported greater certainty in their evaluation of the Ramada when the standards came before the target ( $M = 5.80$ ,  $SD = .96$ ) than after the target ( $M = 5.46$ ,  $SD = 1.14$ ),  $F(1, 171) = 4.43$ ,  $p = .04$ ;  $\eta_p^2 = .03$  (see Table 1). No other effects were significant ( $F_s < 1$ ).

As in Experiment 1, we also tested whether the effects on certainty held controlling for judgment extremity. Critically, the main effect of standards placement on certainty remained significant after controlling for extremity of judgments,  $F(1, 170) = 4.66$ ,  $p = .03$ ;  $\eta_p^2 = .03$ .

### Perceived elaboration

On the measure of perceived elaboration, participants reported exerting greater effort in the judgment task when the standards came prior to the target ( $M = 6.60$ ,  $SD = 1.08$ ) as opposed to after the target ( $M = 6.27$ ,  $SD = 1.04$ ),  $F(1, 171) = 4.10$ ,  $p = .05$ ;  $\eta_p^2 = .02$ . There was also a marginal main effect of luxuriousness of standard, such that participants reported more effort when the standard was nonluxurious ( $M = 6.59$ ,  $SD = 1.03$ ) compared with luxurious ( $M = 6.28$ ,  $SD = 1.09$ ),  $F(1, 171) = 3.52$ ,  $p = .06$ ;  $\eta_p^2 = .02$ . No other effects were significant ( $F_s < 1$ ).



**Figure 1** Mediation of perceived effort on relationship between placement of standards and target and certainty in judgments in Experiment 2. Value in parentheses indicates the direct effect of placement of standards on certainty before the mediator was included in the model. \* $p < .05$ . \*\* $p < .01$ .

### Mediation analysis

To examine whether the observed differences of standards placement on certainty stemmed from a difference in experienced amount of effort in making the judgment, we conducted a simultaneous regression predicting certainty using both order and perceived elaboration (Preacher, Rucker, & Hayes, 2007; Shrout & Bolger, 2002). The results of this simultaneous regression revealed a significant relationship between perceived elaboration and certainty,  $t(172) = 2.55$ ,  $\beta = .19$ ,  $p = .01$ . Furthermore, the effect of the placement manipulation on certainty was reduced and no longer statistically significant,  $t(172) = -1.72$ ,  $\beta = -.13$ ,  $p = .09$ , when perceived elaboration was controlled (see Figure 1). Finally, a 95% confidence interval of the indirect effect using bootstrapping with 1,000 resamples revealed that zero fell outside of this interval (.01 to .17), further indicating successful mediation.<sup>3</sup>

## Discussion

As in the first experiment, participants showed evidence of contrast from extreme standards by rating the Ramada as higher in quality when compared with the nonluxurious hotels than when compared with the luxurious ones. Furthermore, the extent of this contrast was not impacted by the manipulation of placement of standards, replicating Stapel et al. (1997). Our unique argument in this study, as in Experiment 1, was that although the contrast effects were the same in magnitude, they would differ in certainty and that this strength effect would be due to differences in the amount of thought or elaboration.

To provide an additional statistical test of our hypotheses across studies, we combined Experiments 1 and 2 and exam-

<sup>3</sup>The test of mediation was conducted using bootstrapping as this procedure has been widely argued to be a more appropriate test of mediation than the traditional Sobel test (Preacher et al., 2007; Shrout & Bolger, 2002; Zhao, Lynch, & Chen, 2010).

ined the Elaboration (low vs. high)  $\times$  Measure (attitudes vs. certainty)  $\times$  Experiment (1 vs. 2)  $\times$  Standards (low vs. high) interactions in the combined data set. In Experiment 1, the elaboration manipulation was personal relevance, and in Experiment 2, it was order. All factors were between subjects except the measure, which was treated as a within-subjects variable. The attitude and certainty measures were each standardized prior to analysis. These results produced a Standards  $\times$  Measure interaction,  $F(1,276) = 20.48$ ,  $p < .001$ ;  $\eta_p^2 = .07$ , and an Elaboration  $\times$  Measure interaction,  $F(1,276) = 7.16$ ,  $p = .01$ ;  $\eta_p^2 = .03$ , neither were moderated by Experiment. The first interaction showed that the contrast effect caused by the different standards was evident on the attitudes but not the certainty measure, whereas the second interaction showed that the elaboration manipulation affected certainty but not attitudes.

### Experiment 3

Experiments 1 and 2 showed that contrast effects that are comparable in magnitude can differ in certainty. One reason researchers are interested in certainty as an indicator of strength is because enhanced certainty makes people more likely to act in a manner consistent with their judgments (see Petty, Briñol, Tormala, & Wegener, 2007, for a review). In our final study, we examined whether contrast effects that were the same in magnitude would be differentially predictive of behavioral intentions if they stemmed from different levels of thought. To examine this, as in Experiment 2, we manipulated placement of the standards, but measured behavioral intentions as a dependent variable in order to assess attitude–intention correspondence.

Importantly, when examining correspondence between attitudes and behavioral intentions, what is critical is the relationship between them as opposed to the mean levels of attitudes or behavioral intentions. The reason for this can be understood by example. For instance, if two people have equally positive attitudes toward a product, but one individual's attitude is held with greater certainty, the individual with the more certain attitude should be more inclined to act on this attitude (i.e., make a purchase of the product). This would lead to both larger attitude–behavior intention correlations and greater purchase behavior by the individual with a favorable attitude held with more certainty. However, if the product was on sale, then even individuals with relatively uncertain attitudes might purchase the product as the sale price is sufficient to motivate the purchase. In this situation, behavior (i.e., purchasing) might be equivalent regardless of attitude strength, but for different reasons (i.e., reliance on one's attitude vs. the sale). In short, the more diagnostic measure of stronger attitudes is the correlation between attitudes and behavior rather than the pattern of behavior itself (see Rucker, Petty, & Briñol, 2008).

In sum, despite our expectation that we would observe similar degrees of contrast, regardless of order, we expected that order would have a significant effect on attitude–intention correspondence. To the extent that we find differences in attitude–behavioral intention correspondence for contrast judgments formed under high versus low thinking, it would indicate for the first time that some contrast judgments are potentially more consequential for predicting behavior than others, even though they are the same in magnitude. In addition, to increase generalizability, whereas the previous experiments used consumer products, in our final experiment, we used descriptions of people.

## Method

### Participants and design

One hundred nineteen undergraduates participated in exchange for partial fulfillment of a course requirement. Participants were randomly assigned to the cells of a 2 (standards of comparison: desirable, undesirable)  $\times$  2 (placement of standards: before target, after target) between-subjects factorial design.

### Procedure

Participants read descriptions of three different people (i.e., one target and two comparison others). Participants were instructed to rate each person on several measures, but participants were not explicitly instructed to compare the people. Each description featured three behaviors performed by the person. The ambiguous target individual pulled into a parking space he saw another person waiting for (negative); bought a new CD (neutral); and bought three raffle tickets from the Girl Scouts who knocked on his door (positive). The two individuals serving as comparison standards (described under independent variables) performed either very positive or negative behaviors and were presented either before or after the target information. After reading about all three people, participants completed dependent measures about the target, which included their attitudes and their behavioral intentions toward him. Finally, participants were debriefed, thanked, and dismissed.

## Independent variables

### Standards of comparison

Participants were given information about two individuals who, based on their behaviors, were designed, and pretested, to be highly positive or negative compared with the target. Specifically, in the desirable standards condition, participants read about two people who had performed very positive behaviors. For example, one person led a campaign to keep

students safe from nighttime robberies and the other purchased extra groceries for the elderly tenant who lived below his apartment. In the undesirable standards condition, participants read about two people who performed very negative behaviors. For example, one person stood up a date to go to a sporting event with friends, and the other scribbled an obscenity on the walls of a stall at the movie theater.

### Placement of standards

As in Experiment 2, the behaviors of the comparison standards came either before or after participants read about the behaviors of the target individual.

### Dependent measures

#### Attitudes

Attitudes were assessed in the same manner as Experiment 1. The responses were highly correlated and combined to form an overall evaluation of the target person (Cronbach's  $\alpha = .95$ ).

#### Behavioral intentions

Participants were asked to what extent they would want to have the target as a friend (*not at all to a lot*) and whether they would be willing to hire the target for a job (*not at all willing to extremely willing*). These items were positively correlated and were averaged to form an aggregate measure of behavioral intentions ( $r = .73$ ).

### Results

#### Attitudes

As anticipated, there was a significant main effect of the standards on participants' attitudes toward the target,  $F(1, 115) = 18.41, p < .001; \eta_p^2 = .14$  (see Table 1). The target was rated more positively when the standards were undesirable ( $M = 4.83, SD = 1.26$ ) compared with desirable ( $M = 3.90, SD = 1.07$ ), a contrast effect. No other effects were significant ( $F_s < 2.5, p_s > .14$ ). Thus, as in Experiment 2, the contrast effect was comparable regardless of placement of the standards.

#### Behavioral intentions

There was a main effect of standards on participants' behavioral intentions such that participants had more positive behavioral intentions when the standards were undesirable ( $M = 4.78, SD = 1.21$ ) compared with desirable ( $M = 3.99, SD = .96$ ),  $F(1, 115) = 15.44, p < .001; \eta_p^2 = .12$  (see Table 1 for all means). In addition to these effects, there was a marginal interaction of placement and standards,

$F(1, 115) = 3.11, p = .08; \eta_p^2 = .03$ , indicating that standards tended to have a greater impact on intentions when the standards came first than second. This result is consistent with the idea that people are more willing to act on their attitudes if the standards came first.<sup>4</sup>

Of crucial interest, as discussed earlier, is not the pattern of behavioral intentions, but the correlation between attitudes and behaviors, which indicates how much the attitudes influence behavior. If the order manipulation indeed impacted the strength of the attitudes, as in Experiment 2, attitude-behavior correspondence should be greater when standards came first as opposed to second. Consistent with our hypothesis, attitudes were more predictive of intentions when the standards preceded the target  $r(62) = .77$  compared with when they followed the target  $r(57) = .57$ , Fisher's  $r$  to  $Z = 1.98, p < .05$ .<sup>5</sup>

### Discussion

Experiment 3 produced a contrast effect regardless of the order of presentation of target and standards, replicating Experiment 2. That is, people had more positive attitudes toward the target when the standards were very undesirable than when they were very desirable. More uniquely, attitudes were more predictive of intentions when the standards came first than when the target was first. That is, people were more willing to rely on their contrasted attitudes in the former than the latter condition. This finding is important for two reasons. First, by using another means to assess attitude strength, this result provides converging support for our perspective. Second, because behavioral intentions are often viewed as one of the best indicators of actual behavior (Fishbein & Ajzen, 1975, 2010), this experiment suggests this difference in strength can be consequential.

<sup>4</sup>In addition, we conducted a repeated measures analysis with measure (attitudes or intentions) as the within-subjects variable and standard (desirable or undesirable) and placement of standard (before target or after target) as between-subjects variables. This analysis showed that the contrast effect on attitudes and behavior was equivalent, as evidenced by a main effect of standard,  $F(1, 115) = 22.75, p < .001; \eta_p^2 = .17$ , which was not moderated by measure,  $F(1, 115) = 1.12, ns$ . No other main effects or interactions were significant ( $F_s < 2.5, p_s > .12$ ).

<sup>5</sup>We were also asked to perform a regression analysis specifically looking for an interaction of standards, placement, and attitudes on behavioral intentions. We performed this analysis and did not find a Standards  $\times$  Placement  $\times$  Attitudes interaction ( $B = .18, SE = .14$ ),  $t(111) = 1.26, p = .21$  or a Placement  $\times$  Attitudes interaction ( $B = .11, SE = .14$ ),  $t(112) = .82, p = .41$ . However, we do not view this as a concern because some have written that these are slightly different tests and that correlation may be better in testing differences in degree, whereas regression may be more sensitive to differences in form (e.g., Arnold, 1982). Indeed, in our data, both conditions show a positive relationship between attitudes and behavioral intentions (i.e., similar form), but they differ in the degree of positivity.

## General discussion

It has long been known that using extreme comparison standards is a technique that can be used to modify judgments. For example, a person is viewed as more attractive if placed in the context of very unattractive than very attractive people (Kenrick & Gutierrez, 1980). Whereas prior research on contrast effects has focused on how variables affect the magnitude of contrast effects, the current research provides initial evidence for the view that easily modifiable variables, such as the order of presentation of standards and targets can be very important for determining how consequential the contrast effect is. Thus, the current research provides a novel addition to the contrast literature that not all contrast effects that appear similar on the surface are necessarily alike.

Our results may be particularly surprising in light of theories that propose greater elaboration potentially leading to greater extremity of resulting judgments (e.g., Mussweiler, 2003). It is likely that elaboration will affect extremity of contrast in some instances, but we chose situations where this was unlikely to be the case (i.e., the use of strong standards that could lead to contrast by both high and low effort processes). Instead, we found that increased elaboration influenced the confidence in and impact on intentions (i.e., strength) of the contrastive judgment but not the extremity of that judgment. Our findings are consistent with research that shows that different high versus low thought processes can lead to exactly the same judgments on some dimensions across a variety of phenomena, but also can have divergent consequences on other dimensions (e.g., Wegener et al., 2006).

Our research as a whole also suggests an important limitation of current theorizing about the large and substantive literature on contrast effects. Over 50 years of research and numerous books and reviews on contrast (e.g., Biernat, 2005; Biernat & Eidelman, 2007; Stapel & Suls, 2007) have examined the antecedents of contrast, and a large number of moderators and potential mechanisms have been identified as a result of these efforts. However, these theories have neglected the consequences of contrast. Indeed, we could not find one theory of contrast that proposed that differential strength consequences would occur depending on the mechanism of contrast. Theorists instead have appeared to assume that contrast judgments are consequential overall or that mechanism is important only for the magnitude of contrast. Counter to this, we have suggested and provided evidence that different processes can lead to the same extent of contrast in one's judgments, but judgments that nonetheless differ in strength.

To be fair, theories of contrast effects have naturally focused on the initial extent of contrast just as theories of persuasion first focused on the initial extent of attitude change. However, contemporary dual process theories of attitudes have indicated that it is not only important to understand the extent of attitude change, but when that change is consequen-

tial. By incorporating insights from contemporary attitude change theories to the phenomena of judgmental contrast, we were able to show for the first time that contrast effects are also amenable to a "strength" analysis. This brings two literatures that started in a similar place but diverged over the years into isolated areas of inquiry, together once again. This presents both the opportunity and the call for additional work into the study of contrast to better understand when contrast effects guide behavior and when they do not.

To be clear, our research does not pose a problem for existing theories of contrast (which focus on antecedents), but does provide a potential way for various theories of contrast to be integrated. That is, contrast effects that occur through more elaborative processes (e.g., Ford & Thompson, 2000; Mussweiler, 2003, 2007; Mussweiler & Strack, 1999) are more likely to result in judgments that are held with more certainty and predictive of behavioral intentions than are contrast effects that occur through less elaborative processes (e.g., Helson, 1947; Helson & Rohles, 1959; Herr et al., 1983; Schwarz & Bless, 1992, 2007). In addition, theories that allow for more extreme judgments to occur when people process more (e.g., Mussweiler, 2003), could add an additional role of elaboration, that of increased strength.

In addition to this conceptual integration, there are also important practical implications of the present research. Specifically, the limited focus on differential consequences of contrast in prior research is particularly problematic when applying theories of contrast to real-world situations. For example, a known real estate agent tactic is to show their clients a high-priced and undesirable property in addition to showing them the target properties that they actually intend to try to sell to their clients, which makes the target properties seem much more desirable and reasonably priced by comparison (e.g., Cialdini, 1993). Our research (Experiments 2 and 3) suggests that showing the undesirable properties before or after the target properties might not influence how desirable the targets seem in comparison (i.e., the extent of contrast), but could influence how certain the clients are of the desirability of the target properties and how likely they are to act on their impressions. Specifically, being shown the undesirable properties first should be more likely to result in a contrast effect that is held with certainty and more likely to influence intentions to purchase the property. This insight is not discernible from any prior research on contrast effects.

## Implications for assimilation effects

The current research focused on the strength of judgmental contrast. However, our research also has implications for the complimentary bias—assimilation. Assimilation effects occur when judgments move closer to the standards of comparison (Hovland et al., 1957). Just as was the case for contrast, we expect that assimilation can occur under both

high- and low-thinking conditions. For instance, equivalent assimilation effects could occur if we had used less extreme comparison standards and these effects could occur online or be memory-based (perhaps by giving people an impression formation goal or memorization goal, respectively, as was the case in previous research; Bizer et al., 2006; Mackie & Asuncion, 1990). We would expect online assimilation to be more elaborative and to result in stronger judgments (in terms of certainty, attitude–behavior correspondence, etc.) than memory-based assimilation. Future research could therefore apply the current findings and examine their utility for understanding assimilation.

## Conclusion

In three experiments, we showed that not all contrast effects that appear equal are. Contrast effects can occur through high

or low thought, which can produce equivalently extreme judgments, but judgments that nonetheless differ in the certainty with which they are held or in the impact those judgments have on behavioral intentions. Because of this, researchers interested in developing and extending theories of contrast would seem to benefit from considering factors, such as elaboration, that can affect the strength of contrast even when the extremity is the same.

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