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REASONS FOR THE REFERENT: REDUCING DIRECTION OF COMPARISON EFFECTS

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Two studies examined the effects of asking college-aged participants to analyze their feelings regarding two options presented (student government candidates and college courses) that either shared positive features but had unique negative features, or shared negative features but had unique positive features. Past feature-matching studies have revealed a direction-of-comparison effect such that the valence of the unique (unmatched) features of the second option disproportionately influenced people's preferences. Asking participants in the present studies to list reasons about options before rating them reduced direction-of-comparison effects. Content analysis of the reasons indicated that analyzing reasons may have caused participants to use more of the features in their evaluations of the options and to consider unique features of both options, rather than disproportionately focusing on unique features of just the second option. The studies suggest a context in which analyzing reasons may improve judgments.

A waiter comes to your table, announces there are two specials for the evening, and asks which you'd like to hear about first. "Doesn't make a difference," your dining companion replies, forgetting momentarily that you are a psychologist and that of course it makes a difference. You quickly point out numerous examples of order effects on judgment, from presentation order in competing persuasive appeals (Miller & Campbell, 1959), to the importance of first impressions (Jones, Rock, Shaver, Goethals, & Ward, 1968). Most relevant to your dinner selection, you would be sure to describe the order effects that have been found when people are asked to state their preference among two options with some shared, or overlapping, features—and some features unique to one or

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the other option. Studies examining feature matching as a judgment strategy (Tversky, 1977) have shown that features unique to one option (such as the fact that the seafood enchiladas are made with real crab and come with homemade salsa, or that the barbeque turkey is low-fat and a bestseller) are treated differently than shared features that do not distinguish between the two options (e.g., both entrées will be served by a surly waiter and contain excessive amounts of sodium). In terms of order effects, these researchers have shown that, all other things being equal, the unique features of the second option get the greatest weight in our preference judgments. Thus, if the waiter first tells you about the seafood, and then the turkey, the low-fat and best-selling qualities of the turkey will have a disproportionate influence on your decision, whereas if you first hear about the turkey and then the seafood enchiladas, the real crab and homemade salsa will predominate. The features shared by the two options (the surly waiter and high sodium) are given relatively little attention in preference judgments. If the restaurant is good, it will not make that big a difference which entrée you order, but it is easy to see how feature matching may be used in more consequential choices, such as decisions involving large sums of money and long-term consequences (e.g., purchasing a car or choosing a retirement plan) or political decisions (e.g., Houston, Doan, & Roskos-Ewoldson, in press)—choices that most people would probably prefer not be affected by such arbitrary variables as the order in which the options were presented.

Varying the valence of the unique features can result in some interesting preference reversals. In the restaurant example above, assuming that homemade salsa and low-fat are positive features, feature-matching models predict that the second option (or the subject of comparison, because it is compared to the first option, also known as the referent) would be chosen, because people are focusing on the positive unique features that they will get by choosing this option. However, there are other situations in which a choice must be made between two options with unique negative features. For example, imagine making the slightly more serious decision between two medical procedures, both done by skilled physicians in an excellent hospital: The first causes excruciating pain, followed by occasional numbness, and the second leaves a permanent unsightly scar and an odd tingling sensation. In cases such as this, where the two options have unique negative features, feature-matching models predict a preference for the first option—the referent (the option to which the subject of comparison is compared). Once again, people are focusing on the unique features of the second option, but in this case, because these unique features are negative, the subject of comparison seems quite bad, so people avoid it by picking the first option. Previous studies have shown this "direction-of-comparison" effect, even when

the two options were rated equally if seen in isolation (Houston & Sherman, 1995; Houston, Sherman, & Baker, 1991). Thus, when feature matching is used as a decision strategy, simply computing a linear combination of all the features that make up an option will not necessarily do a good job predicting people's judgments (e.g., Hodges, 1997; Houston, Sherman, & Baker, 1989).

What might be the advantage of only focusing on a subset of features (the unique features of the subject of comparison), rather than using all the information provided about the two options? It could simplify the comparison process. As the waiter in the earlier example drones on with ever more features of the daily specials, we may find it difficult to keep track of them all. When the waiter starts to enumerate the features of the second special, we can note which features were shared by the first option and ignore them, because they (the shared features) are not going to help us choose between the two options. What we are left with are the unique features of the second option. Thus, it is not just seeing an option second that makes it the subject of comparison; it is seeing it in the context of being compared to something else (Agostinelli, Sherman, Fazio, & Hearst, 1986; Houston et al., 1989). It is more difficult at this point to go back and think about the unique features of the first option because there is no way of flagging them as "unique" until a description of the second option is presented (Agostinelli et al., 1986; Houston et al., 1989; Sanbonmatsu, Kardes, & Gibson, 1991).

The direction-of-comparison effect is quite robust and has been replicated in several studies (Agostinelli et al., 1986; Houston & Sherman, 1995; Houston, Sherman, & Baker, 1991; Sanbonmatsu et al., 1991). However, if the effect is due to a greater focus on the unique features of the second option at the time of comparison, then reducing this asymmetrical focus should reduce the effect. Houston and Sherman (1995) have demonstrated one method of doing this: By presenting the descriptions of both options simultaneously rather than sequentially, direction-of-comparison effects were eliminated. When the options were presented sequentially, the unique features of the subject of comparison had a recency advantage. In addition, by seeing the options simultaneously, features of both the first and second option could be easily flagged as unique, rather than just those of the second option.

Similarly, Sanbonmatsu et al. (1991) demonstrated that the more people rely on memory-based judgments, the more likely they are to show direction-of-comparison effects. Participants in their study first saw two options, with either evaluative-set instructions (designed to make them form evaluative judgments of the options before they began comparing them) or memory-set instructions (designed to make them try to remember the features of the options before they began comparing them). The

former resulted in reduced direction-of-comparison effects. The context of the evaluative-set condition may have made participants view the unique features of the referent as more relevant than they would be in the comparative context usually found in feature-matching studies: Global evaluations are more akin to absolute judgments than relative judgments.

Sanbonmatsu et al.'s work shows that direction-of-comparison effects are more likely to occur when subjects focus on memorizing individual features of options instead of making global judgments of the options. But in general, memory-based judgments are rarer than on-line judgments, especially for evaluative judgments (Hastie & Park, 1986); and direction-of-comparison effects have been obtained in several studies in which subjects were not given memory-set instructions (e.g., Houston et al., 1991; Houston & Sherman, 1995). Furthermore, people are not always receptive to cues that a feature-matching strategy be unwise in a particular context, which suggests people may find themselves using this strategy without consciously engaging it (Hodges, 1997). Without simultaneous presentation of the options, is there another way of preventing the direction-of-comparison effect without explicit instructions to make global evaluations prior to seeing the options?

The judgment context investigated in the present studies, in which people were asked to list reasons why they felt the way they did about the options after seeing them, was expected to give people another chance to make evaluations that incorporate all of the options' features. Furthermore, asking about reasons provides an additional opportunity for people to think about features of the two options simultaneously. Asking people to list reasons for their opinions should prompt a thorough search for all information known about the options, not a selective pruning that leaves only the unique features of the most recently encountered option.

Previous research by Wilson and his colleagues (e.g., Wilson, Hodges, & LaFleur, 1995) has demonstrated that, when people are asked to list the reasons why they feel the way they do about an attitude object, they readily come up with such a list. The reasons listed are plausible and easy to verbalize, but not always the actual reasons behind their attitudes. Reasons that are harder to express, or that people are not conscious of (e.g., people aren't always aware that they like familiar things better, Zajonc, 1980) may not make it onto the list. However, in a manner akin to a self-perception effect (Bem, 1972), once people have their list of reasons before them, they perceive it as diagnostic of how they feel and adjust their reported attitudes to correspond with reasons given (Wilson & Hodges, 1992; Wilson, LaFleur, & Lindsey, in press).

In a feature-matching context, asking people to list their reasons after seeing two options—but before making a preference judgment—may cause them to consider the features of the two options simultaneously, in a manner that resembles Houston and Sherman's (1995) simultaneous presentation manipulation. Analyzing reasons may help people focus on features of both options, rather than concentrating only on unique features of the subject of comparison. Furthermore, Wilson and his colleagues' findings show that the content of people's reasons is likely to influence their subsequent judgments. If all features are considered, including the unique features of the referent, then direction-of-comparison effects should be attenuated. Thus, asking people to analyze reasons may interrupt the judgment context that normally produces feature-matching effects (Hodges, 1997).

A necessary link in this proposed chain of events is that unique features of the referent be considered viable reasons. Previous reasons analysis research has suggested that reasons which are plausible, easily verbalizable, and accessible are likely to turn up in people's lists of reasons (Wilson et al., in press). Certainly, unique features of the referent in past feature-matching studies satisfy the first two of these categories: The features are plausible in that they are the only information participants in these studies have to base their judgments on, and they are imminently verbalizable in that participants have just read them. The question of accessibility is more difficult to answer. Although unique features of the subject of comparison appear to be more accessible than unique features of the referent, no evidence exists that unique features of the referent are *in*accessible. Furthermore, past feature matching research has demonstrated that the elevated accessibility of unique features of the subject of comparison does not fully explain their sway in people's judgments (Houston et al., 1989). Most important, differential accessibility would be expected only to the extent that feature-matching is used: If analyzing reasons changes the judgment context such that feature matching does not occur, the asymmetrical focus on the unique features of the subject of comparison should also not occur.

In addition, it is notable that Wilson et al. (1995) found simply asking people to list everything they could recall about a target person did not function in the same manner as asking them to list reasons why they felt the way they did. Furthermore, results demonstrated that the effect of analyzing reasons was independent of the extent to which judgments were memory-based. It appears that the simple accessibility of information in memory is not the same as being a "reason;" the information must also be seen as applicable to the judgment at hand (e.g., Higgins, 1996). If unique features of the referent are at least somewhat accessible, and furthermore deemed applicable, it seems quite likely that people will see them as viable reasons.

Thus, listing reasons could potentially render judgments in these contexts more normative. There is a certain irony in this, given that a number of reasons analysis studies have demonstrated some undesirable side-effects of asking people to list their reasons: Reduced attitude-behavior consistency (Wilson, Bybee, Dunn, Hyman, Rotondo, 1984), reduced satisfaction with the decision over time (Wilson et al., 1993), and greater deviation from experts' judgments (Wilson & Schooler, 1991). However, Wilson and his colleagues have been careful to note that thinking about reasons for one's attitudes may not always have negative consequences, and that under some circumstances, listing reasons may boost decision quality by producing more thorough judgments. Listing reasons is likely to be problematic when important reasons are omitted because they are hard to verbalize or people do not have access to them (e.g., Wilson et al., 1993), or when accessibility of one kind of information has been systematically biased (e.g., the Wilson et al. 1995 study reexposed people to a subset of the stimulus information and inserted a delay between presentation of information and reasons analysis).

As long as both the unique features of the referent and subject are available in memory and seem applicable to the question "Why do you feel the way you do about these options?," then asking people to analyze reasons before making preference judgments should result in equal attention being paid to the unique features of both the subject of comparison and the referent, as opposed to a greater focus being given to the unique features of the subject of comparison. Specifically, unique positive features of the subject of comparison will no longer disproportionately boost its ratings, whereas unique negative features of the subject of comparison will no longer disproportionately hurt its ratings. If analyzing reasons successfully focuses people on what would have been the under-weighted unique features of the referent, the direction-of-comparison effect should disappear, and the result should be no clear preference between two options that are rated equally in isolation.

STUDY 1

OVERVIEW

Participants were first given two descriptions of student government candidates. For half of the participants, these descriptions consisted of unique negative features and shared positive features. The other half of the participants read about student government candidates who had unique positive features and shared negative features. Crossing this manipulation, half of the participants were asked to analyze reasons

(introspect about why they felt the way they did) about the candidates, while the other half in the control condition completed a filler task.

It was expected that participants in the control condition would show the usual direction-of-comparison effects, preferring the subject of comparison (the second option) when the options had unique positive features and preferring the referent when the options had unique negative features. However, these direction-of-comparison effects (and the accompanying lopsided focus on unique features of the subject of comparison) would be reduced for participants who analyzed reasons, because listing reasons would both encourage participants to think about all the features of the options (not just those focused upon in comparative judgments) and allow participants to reconsider features of both options simultaneously.

METHOD

Participants. One hundred seventy-seven introductory psychology students (103 females, 73 males, and 1 person who did not specify a sex) participated in exchange for course credit.

Student Government Candidate Descriptions. Four descriptions of hypothetical candidates for student government representatives were developed. Each description contained three positive features—such as the fact that the candidate was a compelling speaker or a team builder (mean evaluation of positive features = 5.84 on a 7-point scale, 1 = very negative, 7 = very positive), three negative features—such as the fact that the candidate got bogged down in details or was power hungry (mean evaluation of negative features = 2.24), and one neutral feature—such as being a political science major (mean evaluation of neutral features = 4.48). Two of the four candidates shared three positive features and one neutral feature, but had unique negative features. The other two candidates shared three negative features and one neutral feature, but had unique positive features. In addition to pretesting the individual features, the candidate descriptions were also rated in pretesting, and earned approximately equal ratings (range from 3.40 to 3.44 on a 7-point scale, higher numbers were more positive, with standard deviations ranging from .85 to 1.40).

Procedure. Upon entering the lab, participants were informed that the study dealt with decision making. The experimenter told them they would read about candidates running for a position in student government. Participants then received a packet and were told to read the through it one page at a time, without turning back to previous pages. The packet contained two candidate descriptions, on separate pages, that had either unique negative and shared positive features, or unique positive and shared negative features. The order within pairs was coun-

terbalanced across subjects. Thus, roughly half the participants who read about the pair of candidates with unique positive and shared negative features saw one of these candidates first and the other half saw the other half of this pair first, and the same procedure was carried out for candidates with unique negative and shared positive features.

Following the descriptions, participants in the reasons condition received written instructions to think about why they liked or disliked the two candidates they had read about, and were asked to list their reasons on the page below the instructions in order to organize their thoughts. Control participants were instructed to write about why they had picked their major or why they were considering a particular major.

The last page of the packet asked participants to evaluate each of the candidates they had read about on three 7-point scales: How qualified the candidate was, how good a job the candidate would do, and how much the candidate deserved their vote. Higher numbers indicated more positive ratings. Finally, participants were asked which candidates they would vote for if given a choice.

RESULTS

Examining the voting question first, a 2 (feature valence condition) × 2 (reasons condition) ANOVA was run on the categorical votes, converting votes for the first option (the referent) to 1s and votes for the second option (the subject) to 0s.[1] One participant did not answer the voting question. There was a significant effect for feature valence condition, $F(1, 172) = 20.05$, $p < .001$, indicating the standard direction-of-comparison effect (participants were more likely to vote for the first candidate when two candidates had unique negative and shared positive features, and more likely to vote for the second candidate when the candidates had unique positive and shared negative), but there was also a significant interaction of feature valence condition and reasons condition, $F(1, 172) = 4.46$, $p = .036$ (see Figure 1). A contrast directly testing the hypothesis that the subject referent effect was present only in the control condition was also significant, $F(1, 172) = 21.84$, $p < .001$.[2] There was no significant main effect for the reasons condition.

1. Loglinear analyses were also performed (and were performed in Study 2) with similar results. For a justification of using ANOVA with categorical data, see Cochran (1950), Rosenthal and Rosnow (1984), and Winer (1971).

2. Contrast weights in the control condition were 1 for the unique positive features condition and -1 for unique negative features condition. Both feature valence conditions were weighted 0 for subjects who analyzed reasons.

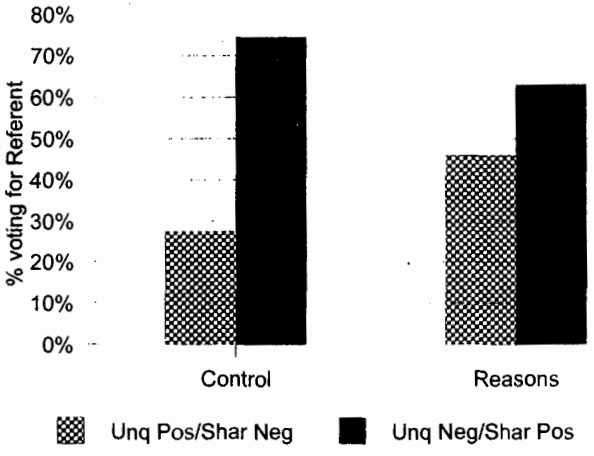

FIGURE 1. Voting in Study 1

The three evaluative measures (how qualified the candidates were, how good a job they would do, and how much they deserved the participants' votes) were highly intercorrelated (Cronbach's alpha = .86). These three measures were thus averaged together to create one composite evaluative measure. A 2 (candidate position) × 2 (feature valence condition) × 2 (reasons condition) within-between ANOVA on this averaged candidate rating revealed no main effects for feature valence or reasons condition, and no interaction effect of feature valence and reasons condition (See means in Table 1). There was a within-subjects position effect revealing a preference for the first candidate participants read about, $F(1, 173) = 6.23$, $p = .014$. Also, consistent with previous studies, there was a direction-of-comparison effect, demonstrated by the significant interaction of candidate position and feature valence condition, $F(1, 173) = 23.06$, $p < .001$, indicating that participants showed a relative preference for the first candidate (the referent) when the two candidates had unique negative and shared positive features and a relative preference for the second candidate (the subject of comparison) when the two candidates had unique positive and shared negative features. There was no two-way interaction of reasons condition and candidate position, nor was there a three-way interaction of feature valence condition, reasons condition, and candidate position. However, a focused contrast directly addressing the predicted hypothesis that direction-of-comparison effect was larger in the control condition than the reasons condition was significant, $F(1, 173) = 13.50$, $p < .001$. (See

Note: Due to a typesetting error, the tables in this paper were mislabelled, but have been corrected in this copy. The journal has been notified of the error.

TABLE 1. Mean Candidate Evaluations in Reasons Study, Listed by Condition

	Referent	Subject	Difference
Unique Positive/Shared Negative			
Control	3.72 (-3)	4.04 (3)	-.32
SD	1.02	.923	
Reasons	3.63 (-1)	3.78 (1)	-.15
SD	1.13	.938	
Unique Negative/Shared Positive			
Control	4.24 (3)	3.46 (-3)	.78
SD	1.24	1.08	
Reasons	4.26 (1)	3.54 (-1)	.72
SD	1.13	1.08	

Note. Contrast weights in parentheses. $N = 177$.

contrast weights in Table 1.) Another way to describe this analysis is that the differences in the mean rating of the referent and the mean rating of the subject (found in the far right column of Table 1) are larger in the control condition than in the reasons condition.

Reasons Content Coding. The reasons listed by participants in the reasons condition were coded for content. Two research assistants coded a subset of the reasons and agreed about the content of the reasons 95% of the time (i.e., they agreed on what the reason was about). They also coded the valence of the reasons and agreed upon whether the reasons were positive, negative, or neutral 83% of the time. One of these research assistants coded the remainder of the reasons. Feature-matching models postulate that the unique features of the subject of comparison will be weighted more heavily in preference judgments, and the present study was designed to test if analyzing reasons could prevent feature-matching effects by preventing a lopsided focus on the unique features of the subject of comparison (the second option seen). Unfortunately, it is impossible to compare the content of reasons across participants in the control and reasons analysis conditions, because control participants never listed reasons (asking half of the people to analyze reasons was the between-subjects variable that created these two groups). However, it is still interesting to note the following about the reasons listed by the participants in the reasons analysis condition: First, a higher percentage of participants' reasons specifically mentioned unique features of the subject of comparison (22%) than they did unique features of the referent (18%), $F(1, 83) = 4.40$, $p = .039$ (e.g., features possessed only by the subject of comparison were more likely to show up as reasons for liking or not liking the candidates than features possessed only by the referent), which suggests that, even in the reasons condition, there was slightly more

attention paid to unique features of the subject of comparison. However, there was no significant difference in the percentage of reasons that more generally pertained to the subject of comparison (65%) and the percentage of reasons that more generally pertained to the referent (68%), $F(1, 83) = .93$, $p = .338$. Thus, although the unique features of the subject of comparison still received extra attention in the reasons condition, overall, participants roughly divided their reasons equally between the subject of comparison and the referent. (These percentages add up to a percentage greater than 100% because some reasons addressed both candidates.)

Stepwise regression analyses[3] were run to see if the number of positive and negative reasons about the referent and subject could be used to predict which candidate participants would vote for. Votes for the first option (the referent) were converted to 1s and votes for the second option (the subject) were converted to 0s.(Thus, higher numbers indicated preference for the referent.) For participants in the unique positive-shared negative features condition, both the number of positive reasons listed about the referent ($b = .199$, $t(36) = 3.32$, $p = .002$) and the number of positive reasons listed about the subject ($b = -.138$, $t(36) = -2.29$, $p = .03$) significantly improved the predictive ability of the regression equation, together predicting 25% of the variability in people's votes. In other words, for candidates that possessed unique positive features, the more positive reasons about the referent a participant listed about the referent, the more likely she or he was to vote for the referent, and the more positive reasons about the subject that a participant listed, the less likely she or he was to vote for the referent. Neither the number of negative reasons about the subject nor the referent significantly improved the regression equation in the unique positive-shared negative condition.

For participants in the unique negative-shared positive condition, both the number of negative reasons listed about the referent ($b = -.241$, $t(43) = -3.59$, $p < .001$) and the number of negative reasons listed about the subject ($b = .164$, $t(43) = 2.73$, $p = .009$) significantly improved the predictive ability of the equation, together predicting 25% of the variability in people's votes. Adding the number of positive reasons about the subject and the referent did not improve the equation. Thus, in the unique negative-shared positive condition, the fewer negative reasons that people listed about the referent and the more negative reasons that they listed about the subject, the more likely they were to vote for the referent.

3. The split in the dichotomous dependent variable was moderate enough to justify use of regular regression (see Tabachnick & Fidell, 1989). However, logistic regression analyses were also run, with similar results.

These results are consistent with the idea that analyzing reasons reduces the direction-of-comparison effect because it engages a judgment process in which unique features of the referent are at least as important as unique features of the subject of comparison: The more reasons listed by a participant that were valenced toward unique features of the referent, the more likely the participant was to vote counter to what would be predicted by the usual direction-of-comparison effect. These results also replicate previous reason-analysis studies that have found a tendency for people's post-reasons ratings and decisions to be consistent with the content of their reasons (Wilson et al., 1995; Wilson & Schooler, 1991).

DISCUSSION

Although an overall direction-of-comparison effect was found, the results of both the categorical voting question and the continuous ratings suggest that asking people to analyze why they feel the way they do about two options reduced this direction-of-comparison effect. Whereas there is normally a preference for the referent when options have unique negative and shared positive features, and a preference for the subject of comparison when options have unique positive and shared negative features, this pattern is dampened by asking people to list reasons why they feel the way they do about the options before they rated them or chose between them.

Notably, the predicted pattern of results emerged much more clearly in the categorical "voting" measure than it did for the continuous ratings. It is important to remember that the descriptions of the two candidates were constructed to be equal, both in overall evaluation and in terms of ratings of the features, so the deck is stacked against finding differences in continuous evaluations. Of the 177 participants in Study 1, 19 rated both candidates identically on all three scales and 113 gave averaged ratings that were within one scale point of each other, indicating that the two candidates were seen as quite close in evaluative terms. Although the continuous ratings have a sensitivity advantage in terms of potentially being able to pick up more subtle differences in people's ratings of the two options, they do not force participants to explicitly express a preference, as does the categorical voting measure. The categorical rating measure also has a certain ecological advantage, at least in Study 1s election context: In elections, results ultimately boil down to votes, not ratings.

It is interesting to note that the effect of analyzing reasons appears to be much more powerful in the unique positive-shared negative condition than in the unique negative-shared positive condition. Given past

work demonstrating that negative information is more potent than positive information (e.g., Pratto & John, 1991; Skowronski & Carlston, 1989), this is not surprising. The unique negative features of the subject of comparison are especially potent because they are both unique and negative. Analyzing reasons has less of an effect against this double whammy than it does in the unique positive-shared negative condition.

STUDY 2

OVERVIEW

In order to extend the results of the first study, a second study was conducted using new stimuli. Study 2 was conceptual replication of Study 1, using college courses as options instead of student government candidates. In addition, one course description appeared as a stimulus in every condition in Study 2, sharing positive features with the other option in the shared positive condition and sharing negative features with another option in the shared negative condition. By holding the features of one option constant across all conditions, evaluative changes in this option could be solely attributed to manipulations of the valence of shared features and reasons analysis. Thus, not only did Study 2 test whether the results of Study 1 generalized to a new set of stimuli in a different realm (courses instead of candidates), it also sought to demonstrate that the exact same option would evaluated differently due to direction-of-comparison effects, and whether these differences could be erased by analyzing reasons.

METHOD

Participants. Undergraduate psychology students participated in exchange for course credit. After discarding data for a participant who had only been speaking English for 1 year and for another participant who failed to follow instructions, the sample contained 115 participants (61 females and 54 males).

Course Descriptions. Three descriptions of hypothetical college courses were developed. Each description contained three positive features—such as the fact that the instructor was friendly, or the information covered was useful later in life (mean evaluation of positive features in pretesting = 6.06 on a 7-point scale, 1 = very negative, 7 = very positive) and three negative features—such as the fact that the instructor was inexperienced or the course met at night (mean evaluation of negative features in pretesting = 2.65). One course description (hereafter referred to as the "constant course") shared positive features with the second

course description and negative features with the third course description. Thus, one course was seen by all the participants, paired half the time with another course that had unique negative and shared positive features, and paired the other half of the time with another description that had unique positive and shared negative features. The order in which the courses were presented was counterbalanced. In addition to pretesting individual features, the course descriptions were also pretested in isolation and earned approximately equal ratings (range from 3.68 to 4.05 on a 7-point scale, higher numbers were more positive; standard deviations ranged from 1.42 to 1.46).

Procedure. Upon entering the lab, participants were informed that the study dealt with decision making. They received a packet that asked them to imagine picking classes for the upcoming term and trying to decide between two courses. The instructions told them to read through the packet one page at a time, without turning back to previous pages. The packet contained two course descriptions on separate pages with either unique negative and shared positive features, or unique positive and shared negative features. Following the descriptions, participants were either asked their reasons for feeling the way they did about the courses (reasons condition), or to discuss why they had picked their major or why they were considering a particular major (control condition).

The last page of the packet asked participants to evaluate each of the courses they had read about on three 7-point scales: How much they liked the sound of the course, how likely it was they would choose to take the course, and how much they thought they would enjoy the course. Higher numbers indicated more positive ratings. Participants were then asked which of the courses they would pick if given a choice.

RESULTS

Once again, the categorical choices were converted to 1s if the participants picked the first option (the referent) and 0s if the participants picked the second option (the subject of comparison). There was a significant effect for feature-valence condition, $F(1, 111) = 11.52$, $p = .001$, replicating the direction-of-comparison effect found in Study 1 and other studies: Participants were more likely to choose the first course when the two courses had shared positive and unique negative features, and more likely to choose the second course when the two courses had shared negative and unique positive features (see Figure 2). There was no main effect of reasons condition. The interaction of feature-valence condition and reasons condition was only marginally significant, $F(1, 111) = 3.17$, $p = .078$, but as in Study 1, a contrast directly testing the hypothesis that

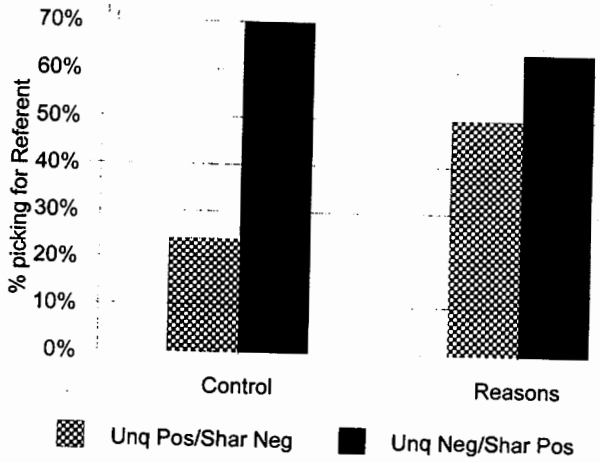

FIGURE 2. Picks in Study 2

the subject referent effect was only present in the control condition was significant, $F(1, 111) = 13.16$, $p < .001$.

As in Study 1, the three evaluative measures of the courses were highly intercorrelated (Cronbach's alpha = .88). These three measures were thus averaged together to create one composite evaluative measure. A 2 (course position) × 2 (feature valence condition) × 2 (reasons condition) within-between ANOVA on the candidate ratings revealed a marginal main effect for feature-valence condition, $F(1, 111) = 3.40$, $p = .068$ (see means in Table 2). Consistent with past feature-matching studies, participants tended to like the courses better when they had shared negative and unique positive features than when they had unique negative and shared positive features. There was no main effect for reasons condition, and no interaction between feature valence condition and reasons condition. Unlike Study 1, there was no within-subjects position effect, but once again there was a direction-of-comparison effect in the form of two-way interaction between feature valence condition and course position, $F(1, 111) = 8.76$, $p = .004$. As in the first study, participants showed a relative preference for the first option (the referent) when the two options had shared positive features and unique negative features and a relative preference for the second option (the subject of comparison) when the two options had shared negative features and unique positive features. There was no two-way interaction of reasons condition and position—nor a three-way interaction of feature valence condition, reasons condition, and position—but as in Study 1, a contrast directly testing the hypothesis that participants showed a greater direction-of-

TABLE 2. Mean Course Evaluations by Condition

	Referent	Subject	Difference
Unique Positive/Shared Negative			
Control	4.07 (-3)	4.58 (3)	-.51
SD	1.27	1.38	
Reasons	4.64 (-1)	4.64 (1)	0.0
SD	1.45	1.38	
Unique Negative/Shared Positive			
Control	4.64 (3)	3.88 (-3)	.76
SD	1.32	1.31	
Reasons	4.29 (1)	3.71 (-1)	.58
SD	1.31	1.12	

Note. Contrast weights in parentheses. $N = 115$.

comparison effect in the control condition was significant, $F(1, 111) = 6.62, p < .02$.

Because one of the courses (the constant course) was the same for all participants in Study 2, it is possible to see how the various factors: (a) feature valence condition (e.g., whether the constant course shared positive features with the other course and each had unique negative features, or whether it shared negative features with the other course and each had unique positive features), (b) order (whether the constant course came first or second), and (c) reasons condition, affected the ratings of just this option. (In other words, this analysis eliminates the noise resulting from using two similarly but not identically rated options.) As can be seen in Table 3, the means for the constant course in the various cells provide a clear illustration of the role of context, and are almost entirely consistent with the hypothesized pattern of results. There was a two-way interaction of valence and whether the constant course was seen first (as the referent) or second (as the subject of comparison), $F(1, 107) = 6.80, p = .01$. Consistent with a direction-of-comparison effect, when the constant course had unique negative features, participants rated it higher when it was seen first than when it was seen second, but when the constant course had unique positive features, it was rated lower when it was seen first than when it was seen second. However, there was also a three-way interaction of valence, whether the option was seen first or second, and reasons condition, $F(1, 107) = 7.02, p = .009$, and the contrast testing the specific hypothesis that feature valence and order made a greater difference in the control condition (see weights in Table 3) was significant, $F(1, 107) = 12.37, p < .001$. Consistent with the idea that analyzing reasons caused participants to consider unique features of the referent as well as those of the subject comparison—as

TABLE 3. Mean Course Evaluations of the "Constant Course" by Condition

	Referent	Subject	Difference
Unique Positive/Shared Negative			
Control	3.56 (-3)	4.86 (3)	-1.3
SD	1.19	1.00	
Reasons	4.51 (-1)	4.24 (1)	.27
SD	1.46	1.39	
Unique Negative/Shared Positive			
Control	4.80 (3)	3.60 (-3)	1.2
SD	1.42	1.23	
Reasons	3.89 (1)	3.64 (-1)	.25
SD	1.47	.84	

Note. Contrast weights in parentheses. $N = 115$.

compared to the control condition where people concentrated only on the unique features of the subject of comparison—the biggest differences in the control and reasons condition can be observed when the constant course was the referent.

Reasons Content Coding. Participants' reasons were coded in the same way as they were coded in Study 1. The author and an undergraduate research assistant coded a subset of the reasons and agreed on content and valence of the reasons 100% of the time. The undergraduate research assistant coded the remainder of the reasons.

Paralleling Study 1, there was no significant difference in the percentage of reasons that generally pertained to the subject of comparison (54%) and the percentage of reasons that generally pertained to the referent (58%), $F(1, 54) = 1.12, p = .30$. (These percentages once again add up to more than 100% because some reasons addressed both courses.) Unlike Study 1, the percentage of participants' reasons that specifically mentioned unique features of the subject of comparison (28%) was not significantly greater than the percentage which mentioned unique features of the referent (26%), $F(1, 54) = .40$, n.s, but the overall patterns of results for the reasons looked quite similar in the two studies.

Stepwise regression using the number of negative and positive reasons about the subject and referent as predictors of participants' choice of course were performed, and as in Study 1—in the unique negative-shared positive condition—both the number of negative reasons listed about the referent ($b = -.20, t(25) = -2.58, p = .02$) and the number of negative reasons listed about the subject ($b = .23, t(25) = 2.82 p = .009$) significantly improved the predictive ability of the equation (together, they explained 38% of the variability in people's choices), whereas neither the number of positive reasons about the subject nor the referent added any predictive power.

However, unlike Study 1, none of the predictors were significant in the unique positive-shared negative condition.

GENERAL DISCUSSION

The basic pattern of results in Study 2 resembles Study 1. The categorical ratings look very similar. If the categorical results from the two studies are combined in a meta-analysis, the interaction of reasons condition and valence is highly significant, $z = 2.71$, $p = .006$[4] The continuous ratings, especially in Study 2, are consistent with this pattern too: Without reasons, there is a preference for the subject of comparison when two options have unique positive features and a preference for the referent when two options have unique negative features, but these preferences are reduced when people analyze reasons. The continuous ratings of the "constant course" in Study 2 powerfully illustrate the effects of judgment context. The exact same course description varied by more than a full scale point on a 7-point scale, depending on the valence of features shared with the other option, whether it was seen first or second, and whether participants were asked to analyze reasons.

Examining reasons participants in the reasons analysis condition listed provides some insight as to how the reasons manipulation works. Because the reasons manipulation specifically asked participants to consider why they felt the way they did about options they had seen, rather than why they preferred one over the other, features of both options may have been seen as relevant reasons. Taken with Sanbonmatsu et al.'s (1991) findings that direction-of-comparison effects are heightened when judgments are memory-based, current results suggest that people's default strategy—when they go back to make comparative judgments—is to use the streamlined, matched, and canceled information (i.e., information already processed in such a way as to facilitate comparisons). However, when asked to do something else—specifically to say why an option is liked or disliked—all features of an option once again become relevant, not just those that are alignable in making comparisons.

Consistent with this interpretation, only a small number of reasons that participants listed dealt with comparative judgments: Only about 10% of reasons listed in both studies expressed a preference for one option over the other. Furthermore, the reasons content results are consistent with the idea that the asymmetrical focus on unique features of the subject of comparison (a hallmark of feature matching) is attenuated when people are asked to analyze reasons. In both studies, roughly equal numbers of reasons addressed the subject of comparison and the referent, and in neither study did there appear to be a disproportionate focus given to the unique features of the subject of comparison.

4. The F statistics from each study were converted to z scores, and an average of the z scores (weighted by degrees of freedom) was computed.

Houston and his colleagues (Houston et al., 1991; Houston, Doan, & Roskos-Ewoldson, in press; Houston & Sherman, 1995) have postulated two components to feature matching in preference judgments: The direction-of-comparison effect and decreased attention to the shared features. Only one of these appears to have been affected by the reasons manipulation, which is consistent with Houston and Sherman's (1995) findings that presenting the options simultaneously (as opposed to sequentially) had no effect on the cancellation of shared features, but did reduce the direction-of-comparison effect. In the present study, the effects of canceling shared features would be visible only in the continuous ratings because changing the weight of the shared features would affect both options equally, and thus not the relative preferences that determine choice. If analyzing reasons also caused additional attention to be given to the shared features (as well as unique features of the referent), then participants' ratings should have gone down in the unique positive-shared negative condition, and up in the unique negative-shared positive condition. The means in Study 1 were in this direction, but the interaction was far from significant. The means in Study 2 were actually in the opposite direction, although not significantly, and it must also be noted that this opposite pattern is visible mainly in the ratings of the referent, where it is identical to the pattern that would be hypothesized if reasons had merely caused people to more heavily weight the unique features of the referent (e.g., raising ratings of the referent when it had shared negative and unique positive features and lowering ratings of the referent when it had shared positive and unique negative features). Furthermore, the shared valence reasons that people listed about the subject and referent were not as useful in predicting their choices as were the unique valence reasons.

On the basis on these results, it appears that asking people to analyze reasons does not increase the accessibility or perceived applicability of the shared features: It only makes the unique features of the referent seem more relevant. Further research may uncover a manipulation that highlights the shared features in the decision context. One possibility is that if there were a delay between seeing the information and analyzing reasons, the shared features might be more likely to turn up in the reasons because they are seen twice and thus might be better remembered (along the lines of Wilson et al.'s 1995 results).

Results of the current study are important because they demonstrate a limit on direction-of-comparison effects. A common criticism leveled

at those who study judgment biases and heuristics is well summed up by one person's reaction to the field: "If we're so dumb, how come we made it to the moon?" (Nisbett & Ross, 1980, p. 249). The present studies show that we are not slaves to our decision strategies, and that a relatively simple manipulation (and one not unlike naturalistic admonitions to "think carefully" or "go back and consider the points of each option") can reduce the direction-of-comparison effect. The usual direction-of-comparison effects found in feature-matching studies can be considered non-normative, in the sense that they result in preferences that deviate from the objective ratings of the options, causing people to show a systematic preference between two options that are equally rated in isolation (Houston et al., 1989). However, if people are motivated to spontaneously engage in something akin to reasons analysis, for example, because they are making a decision in a context in which accountability is important (e.g., Tetlock & Boettger, 1994), the deviation may be corrected and judgments improved. Using a between-subjects design for the reasons manipulation with a forced choice, as this study did, improvement takes the form of making participants in the reasons condition look more indifferent when choosing between two equally rated options. In contrast, participants who did not analyze reasons showed marked preferences consistent with past feature-matching studies.

Current studies also help resolve another finding in the decision-making literature that is counterintuitive to the layperson. Not only do they demonstrate a technique for overcoming direction-of-comparison effect well within our grasp, they also demonstrate a specific context in which analyzing reasons may improve rather than impair judgments, at least in terms of making the judgments more closely resemble objective standards. Wilson and his colleagues have suggested that such contexts may exist, but had not yet identified one. These current studies, in contrast to previous studies incorporating a reasons analysis manipulation, show an ameliorative effect of analyzing reasons by rendering individuals more indifferent toward equally ranked options with unique features of one valence.[5]

Why does analyzing reasons help in this particular context but not in others? The key may lie in what reasons people focus on. When some reasons are easier to list than others, but the more difficult ones are still important determinants of people's attitudes, then listing reasons can make judgments worse. However, when analyzing reasons causes people to consider available information that would otherwise be under-

5. It is, of course, possible that the asymmetries associated with feature matching judgments have some psychological function that allows people to feel more comfortable with their choices, even if the choices are not actually objectively better (e.g., Hodges, 1997).

utilized due to comparison context effects such as feature matching, then it may improve judgments—at least to the extent that the additional information considered is important to making a normative judgment. If all of the features have already been examined in order to provide reasons for why one feels the way one does, the benefit of only considering the unique features of the subject of comparison is lost. Again, a parallel can be drawn to accountability, which consistently makes people's judgments more integratively complex (Tetlock & Boettger, 1994; Tetlock & Kim, 1987), but does not always improve the quality of their judgments (Tetlock & Boettger, 1994). In the present studies, it appears that analyzing reasons helped quite simply because it created a context in which equal weight was bestowed upon the unique features of both the subject of comparison and the referent, thus eliminating non-normative direction-of-comparison effects.

Previous reasons analysis work has demonstrated that people's post-reasons judgments are related to the content of their reasons. Thus, if those reasons are systematically biased (due to additional rehearsal, or the ease with which the reasons can be stated), making one kind of information predominant, we can expect judgments to be similarly biased. For example, in the case of Wilson et al. (1995), some information about a target person was rehearsed more than other information. Participants were given a 17-minute delay, and then asked to analyze reasons, allowing the more heavily rehearsed information to predominate in the reasons. However, if reasons are not systematically biased, and analyzing them in fact helps to balance the attention given to sources of relevant information, the judgments will be unbiased, as in the case of the present studies.

It is perhaps a little unnatural to think about judgments being "improved" by reasons analysis, when that improvement takes the form of bringing people closer to showing indifference toward the two options, as also was the case in the present studies. In fact, one possible interpretation of the results is that analyzing reasons simply caused participants to make more moderate ratings of both options. Wilson and his colleagues have demonstrated several times that analyzing reasons does not generally cause people's attitudes to moderate (see Wilson, Dunn, Kraft, & Lisle, 1989; Wilson & Schooler, 1991). In the present studies, it was expected that evaluations of the two options would moderate toward each other, but there was no evidence in the data that the ratings of the options generally moderated (that is, clustered more closely around the middle of the scale) after analyzing reasons: The variances of the evaluations of the two options were essentially equal in the reasons and control conditions.

It is also possible to imagine a case in which the effects of analyzing reasons could affect the perceived difference in two options that were

not objectively equal and did indeed differ. For example, imagine that the referent is an option with slightly more negative unique negative features than its alternative (the subject of comparison). Because of the direction-of-comparison effect, people might pick the first option anyway (because its unique negative features, although worse than those of the second option, would receive less attention), which would result in a worse choice. Analyzing reasons could play a truly helpful role in this context, by helping people not overlook the unique negative features of the referent, highlighting the fact that this option, too, had its warts—warts that were, in fact, worse than those of the subject of comparison.

In summary, the results of these two studies suggest that one way to disrupt direction-of-comparison effects normally found in choices between options with shared features of one valence and unique features of another valence is to ask people to analyze their feelings about the two options. Instead of showing a marked preference for the subject of comparison when the two options have unique positive features, and for the referent when the two options have unique negative features, people's post-reasons preferences come closer to looking random—which is what would be normatively expected given that the two options were rated equally in isolation. It appears that listing reasons eliminates the people's lopsided tendency to base their judgments on the unique feature of the subject of comparison. The contents of people's reasons demonstrated equal, if not greater, attention to the unique features of the referent, which play second fiddle to the unique features of the subject when feature matching is employed as a strategy. Returning to our hypothetical restaurant menu example, even psychologists can relax while eating out, and let the waiter recite the two dinner specials in whatever order he or she pleases—as long as they are willing to engage in a little reasons analysis before they make their selection.

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THE TRANSFER OF ACTOR-TRAIT ASSOCIATIONS INFERRED FROM BEHAVIOR

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Two experiments examined the transfer of actor-trait associations inferred from behavior descriptions. In Experiment 1, the savings effect found by Carlston and Skowronski (1994) was replicated. In addition, the actor-trait associations were strong enough to produce negative transfer as well. In Experiment 2, a between-subjects design replicated both the positive and negative transfer effects found in Experiment 1. It was argued that actor-trait associations are formed when the actor is salient and behavior strongly activates a trait.

When we observe an individual engaging in behavior with clear and unambiguous trait implications, what type of inferences, if any, do we make? Recent work by Carlston and Skowronski (1994) suggests that observing such behavior spontaneously triggers a trait inference associated with the actor.

Carlston and Skowronski (1994) used the savings or relearning paradigm to study whether people spontaneously make trait inferences based on behavior descriptions. In these experiments, subjects under impression, memory, or no-instruction conditions studied a set of photographs, each paired with a self-description that strongly implied a particular trait. Later, subjects learned a list of photo-trait pairs. Some pairs consisted of photographs paired with the trait implied by the self description originally accompanying the photograph. Carlston and Skowronski argued that, if memory and no-instruction subjects spontaneously generated trait inferences and associated these inferred traits with the actor, then it should be easier to learn familiar photo-trait pairs than new control pairs. This outcome was obtained in each of the five experiments reported. In addition, the magnitude of this savings effect was similar for each instruction condition. These data provide compel-

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