
It's Different When I Do It: Feature Matching in Self-Other Comparisons

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Two studies explored the application of feature-matching and cancellation models to self-other comparisons. College participants completed a questionnaire about their religious behaviors and saw another questionnaire supposedly completed by another student. Participants in Study 1 (N = 114) who were explicitly provided direction of comparison instructions showed a direction of comparison effect, rating the person whose questionnaire they saw last as more religious. Participants in Study 2 (N = 103), who were not given explicit direction of comparison instructions, did not. Most important, in both studies, the extent to which self and other overlapped on shared features affected self- and other judgments asymmetrically. Participants appeared to cancel out behaviors shared by the self and other when rating the other person (i.e., they gave lower ratings when there was more overlap) but not when rating themselves.

Imagine a faculty search in your department. Candidates on the search committee's short list have all authored seminal papers in the field, have grant funding, and have won teaching awards. Although you should be delighted with the caliber of the pool, your decision about who the most qualified candidate is will likely not focus on these shared, positive features because they do not distinguish between the candidates. Past research has demonstrated that you will "cancel out" these shared features and your evaluations of the candidates may suffer as a result.

Now, imagine making a slightly different comparison: Your professional organization will be giving an annual award and you are wondering who will receive it. Now, you are part of the pool of eligible candidates. Like your colleagues, you have seminal articles, a history of grant support, and recognition for great teaching. This article investigates whether you will cancel out these features in

evaluating yourself the same way that you did when evaluating others.

Feature-Based Models of Comparison

For many dimensions on which people judge themselves, there are no clear absolute standards, and relative comparisons to other people provide a basis for knowing where we stand (Festinger, 1954). When we compare ourselves to other people, self-judgments become contextually based, with other people providing the context. Although a reliance on contextual cues can produce poor judgments under some circumstances (e.g., Tversky & Kahneman, 1974), our general tendency to use contextual information may be a wise strategy overall given the cognitive space and retrieval difficulties posed by storing large numbers of absolute standards.

Tversky (1977) generated a model of relative comparison that yielded a number of interesting predictions and findings outside the realm of self-other comparisons.¹ When comparing two things, Tversky argued that people would map the features of one thing, the target of comparison, onto the features of the thing they were comparing it to (the referent), noting which features

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were unique to one option or the other and which were shared. Subsequent work drawing on Tversky's original model has largely focused on two phenomena that emerge when feature matching is used in comparisons. Direction of comparison effects demonstrate that evaluations differ depending on which object is the target of comparison and which is the referent. Cancellation effects demonstrate that evaluations downplay the role of shared features.

Comparison: Not a Two-Way Street?

Logically, it seems that comparative judgments that locate two options along some continuum should not be affected by the ordering of the options. In differentiating the target and the referent of comparison, Tversky's model captured the fact that—logical or not—the sequence in comparison does matter: Comparisons are asymmetrical and sometimes intransitive. Tversky and Gati (1978) demonstrated the intransitivity of similarity judgments in a between-subjects design. When participants were asked to compare North Korea to China, they judged the two countries to be more similar than when they compared China to North Korea. Participants' familiarity with China was generally greater, and so when they focused on China as the target of comparison, they generated more unique features about it than they were able to generate about North Korea when it was the target of comparison.

Direction of comparison effects have been found to influence not only similarity judgments but also preference judgments, particularly when the unmatched features are either predominantly positive or predominantly negative. If the two things being compared have unique positive features, people prefer the target of comparison, presumably because they focus on these unique positive features after matching up and canceling shared features. However, if the two things being compared have unique negative features, people prefer the referent because the target suffers from a focus on its unique negatives (Houston, Sherman, & Baker, 1989). One of the most robust ways of manipulating direction of comparison is to present the options in sequence. The first option becomes the referent and the second option becomes the target, given that no comparison can be made until both options have been presented. Thus, the relative advantage of being presented first (as the referent) or second (as the target) depends on whether the option's unique features are positive or negative.

Cancellation of Shared Features

In addition to direction of comparison effects, feature-based comparative models predict how shared features will be used in comparative judgments. Tversky (1977) hypothesized that people initially base their simi-

ilarity judgment of two items on the number of shared features and then adjust downward according to the number of leftover unique features. However, when people are deciding which of two items they prefer, they cancel out shared features because they provide no clue as to which option is better (Hodges, 1997; Houston & Sherman, 1995). When comparing two options, a large number of shared features may be set aside and a small number of unique features can produce a lopsided preference. In a soccer match, for example, the first scoreless 85 minutes of the game may be all but forgotten when one team rallies and scores twice in the last 5 minutes.

Canceling shared features may help illuminate preferences by adjusting the scale on which differences are measured (Hodges, 1997). Interesting side effects occur, however, when the shared features are all or predominantly of one valence. If two options share negative features, then when these features are canceled out, the quality of the pair of options appears to go up. Two options that look fairly dismal in isolation can start to look okay when compared to each other, as long as they share the same negative features. If, on the other hand, two options share positive features, and these features are canceled out in making a preference judgment, then both options suffer. Thus, two options that look good in isolation start to look mediocre. Furthermore, cancellation can persist beyond the initial comparison—shared features appear to remain canceled out even when new options are added to the pool of choices (Hodges, 1997).

Comparing the Self and Others

In making comparisons involving the self, however, people may not use the same strategies they use for comparing countries and job candidates. Self-information should be readily available, given that people are the resident "experts" on themselves. Because people should know where they stand on various dimensions without having to rely on contextual information to make their judgments, self-other comparisons might fall into a special class. A variety of mechanisms that uniquely affect judgments involving the self support this idea. First, there is a notable tendency to make judgments that protect or promote the self (e.g., Alicke, LoSchiavo, Zerbst, & Zhang, 1997; Ross & Sicoly, 1979; but see also Kruger, 1999). Furthermore, the self is a highly accessible reference point (Catrambone, Beike, & Niedenthal, 1996; Dunning & Hayes, 1996; Rogers, Kuiper, & Kirker, 1977) that affects the judgments of others (Biernat, Manis, & Kobrynowicz, 1997; Catrambone & Markus, 1987).

However, despite these distinctions, other studies suggest that self-other comparisons are similar in many ways to other kinds of comparisons (Holyoak & Gordon, 1983). In particular, self-evaluations are far from context independent. In a classic study, Morse and Gergen

(1970) demonstrated that subjects' self-esteem was enhanced by comparison to a socially undesirable person and reduced by comparison to a socially desirable (near saintly) person.

Taking either extreme position—that the self is not subject at all to comparison effects or that comparisons involving the self are no different from any other comparisons—is unlikely to yield fruitful results. Self-evaluations are most likely a combination of special self-knowledge and context effects (e.g., W. M. Klein & Kunda, 1993), with a number of factors affecting the balance between the two. For example, "isolated self-concepts" (e.g., one's eye color) may not require a social context, whereas "interrelated self-concepts" (e.g., "younger than my brother") involve references to other people (Niedenthal & Beike, 1997).

Furthermore, certain individual differences seem to moderate the degree to which people's judgments of themselves are influenced by context. Campbell (1990) and Baumgardner (1990) have demonstrated a positive correlation between a clear, certain self-concept and self-esteem. Thus, self-judgments by people with higher self-esteem may be less susceptible to contextual influences such as feature matching. Lability of self-judgments may also vary according to which dimension of the self is being assessed. A dimension about which a person habitually thinks and that she considers important—in other words, a dimension for which a person is schematic (Markus, Smith, & Moreland, 1985)—may be less susceptible to context effects.

Feature-Matching Models and Self-Other Judgments

As is the case for comparisons not involving the self, explicitly manipulating the direction of comparison in similarity judgments of the self and other can produce intransitive results. People tend to give higher ratings of similarity when asked how similar other people are to them than when asked how similar they are to other people (Catrambone et al., 1996; Holyoak & Gordon, 1983; Srull & Gaelick, 1983). However, although linguistic direction of comparison instructions ("How similar is X to Y" or "How similar is Y to X") can produce these effects, when an explicit direction of comparison is not given (e.g. "How similar are these two people?"), the default direction of comparison appears to be to use the more familiar self as a referent (Catrambone et al., 1996; Karylowski, 1990). In other words, the self functions as a "habitual referent" (Catrambone et al., 1996; Dunning & Hayes, 1996). It appears that the self serves this function not because a different mechanism is used for judgments involving the self but because the self is the more complex, more salient, and more densely described item in the comparison (Bowdle & Medin, in press).

Thus, direction of comparison affects self-other judgments of similarity but few studies have addressed whether direction of comparison affects self-other comparisons on other dimensions. This lack of research is notable, given the frequency with which people make comparisons between the self and other people on dimensions other than similarity in everyday life (e.g., "Is he better looking than me?" "Am I more qualified than her for the job?"). One exception is a study by Hoorens (1995), which explored whether direction of comparison instructions affect people's tendency to demonstrate unrealistically optimistic beliefs about the self. Student participants who were instructed to use themselves as the target of comparison believed that they possessed higher levels of positive traits than participants who were instructed to use themselves as the referent.

Hoorens's (1995) study suggests that direction of comparison effects can occur in self-other judgments. On the other hand, direction of comparison instructions appear to have a greater impact in similarity judgments than difference judgments (Bowdle & Medin, in press), and asking people to judge whether the self or other is higher on a particular dimension requires a difference judgment. Thus, direction of comparison effects in self-other judgments that involve difference judgments may be harder to find than those that involve similarity judgments.

Shared and Unique Features in Self-Other Judgments

If direction of comparison effects apply to the comparison of self with others, then these effects should be particularly pronounced when both the self and other have unique features relevant to the dimension under evaluation. If the unique features of the person who is the target are diagnostic of being higher on that dimension, then the target of comparison should be rated higher. Even in the absence of direction of comparison effects, the number of unique and shared features also may affect self-other comparisons. If shared diagnostic features are matched and canceled, then both the self and other may be seen as scoring lower on that dimension. Alternatively, given that cancellation may be used as a way of organizing and simplifying the amount of information that is used to make comparisons (Hodges, 1997), shared features may be cancelled for the other person but not for the self. Because of the privileged status of self-features, we may have no trouble remembering or organizing them. However, to process the features of another person, we may have to rely on matching and canceling. Thus, cancellation may be asymmetric in self-other comparisons.

There is evidence from other research that unique features of both the self and other get special attention

in making social judgments. People are more likely to note where the self stands on a particular dimension if their standing on that dimension is distinctive (McGuire & Padawer-Singer, 1976). In other words, if everyone in my crowd rides a bicycle, then I am less likely to describe myself as a cyclist than if no one else in my crowd rides a bike. In addition, Nelson and Miller (1995) found that people's judgments of other people are more influenced by distinct features than they are by features that are shared with other people. Alicke, Klotz, Breitenbecher, Yurak, and Vrendenburg (1994) found the "better than average effect" (the tendency—and statistical impossibility—of a great majority of people to view themselves as better than others on a variety of evaluative dimensions) is more pronounced when the other person is an average person, as opposed to a well-known person. Interpreted in terms of feature-matching models, people probably generate more unique features for a well-known other person than they do for the average other (by definition, a description of an average person should not contain unique features).

Using an Idiographic Approach

One of the challenges to studying feature-matching models in self-other comparisons is that participants in the research bring half the set of stimuli with them in the form of self-descriptions. Furthermore, given that the outcomes of feature matching rely on the relationship between the two stimuli, the other half of the stimulus set will be evaluated very differently, depending on the extent to which it shares or does not share features with the self. Unlike studies using cars and apartments, a "one size fits all" set of stimuli cannot be used to explore self-other comparisons. To manipulate factors such as the breakdown of shared and unique features, the description of the other person must be constructed idiographically for each research participant. The features of the self and other person in the present studies are behaviors or characteristics that are diagnostic of the dimension on which the comparison is being made. So, in the case of the dimension of religiousness, features are things such as "observed a religious holiday" or "wore a religious symbol."

Overview of the Studies

The present studies tested whether the two hallmarks of feature-matching comparison models—cancellation of shared features and direction of comparison effects—hold for self-other comparisons, using idiographically constructed descriptions of others. Thus, we first ask whether shared features of the self and other cancel out for both. If so, then the more that the self and other overlap on diagnostic features of a dimension, the lower they should be rated on that dimension. Thus, for example,

when the self and other each have five unique features that are equally diagnostic of religiousness, they should both be viewed as more religious than when the self and other share five equally diagnostic features of religiousness.

Alternatively, given the easy accessibility of features of the self, cancellation may not be necessary as an information organization and reduction strategy for the self, whereas it may be used when making evaluations of another person. If feature cancellation is used as an information-processing tool that helps organize incoming information (Hodges, 1997), it may not be necessary to resort to this tool in making judgments about the self, about which much is known. Thus, cancellation may result in lower ratings for the more unfamiliar other person but not for the self.

Our second question addresses whether manipulations of the direction of comparison will affect self-other comparisons on a dimension such as religiousness. If so, we would expect that if both the self and other had unique features (i.e., were described by unique characteristics or behaviors), then whoever was the target of comparison would be rated higher on the dimension. However, if the self is treated differently (perhaps because more is known about the self or because the self, as an accessible reference point, is always used as the referent in comparisons), such differences would not be expected.

STUDY 1

Overview

In Study 1, participants compared themselves with a fabricated person whose description had been idiographically constructed so that the other person either reported doing the same identical religious behaviors as the participant or reported performing the same number of different religious behaviors that were matched on importance with those performed by the participant.

Method

PRETEST SESSION AND CONSTRUCTION OF OTHER PERSON

During a departmental group-testing session, undergraduate students completed a Religious Behaviors questionnaire. Behaviors on the questionnaire were taken from Biernat et al.'s (1997) Religiosity Scale, a 20-item checklist of religious behaviors, including such behaviors as "prayed before or after meals" and "purchased a recording of religious music." In earlier testing, a separate group of research participants had rated the importance of each behavior in terms of religiousness, providing (nomothetic) mean importance ratings for each behavior.

We excluded anyone in the pretesting sample who checked off fewer than two religious behaviors (approximately 35%) and anyone who checked off more than nine behaviors (approximately 12%). In the former case, the maximum number of behaviors that could be shared by the participant and another person was one, which we arbitrarily decided was not enough to constitute "overlapping behaviors." In the latter, checking off nine or more behaviors made it difficult or impossible to construct a description of another person who did not share behaviors with the participant (see below).

The remaining students were eligible to participate in the actual study, and for each of these participants, a personalized other person was constructed, using the same list of religious behaviors. The eligible participants were first sorted by the number of behaviors they checked off. In each group (e.g., students who checked off three behaviors, students who checked off four behaviors, etc.), half of the group was assigned to the overlap condition and the other half was assigned to the no-overlap condition. In the overlap condition, the other (fabricated) person had the exact same behaviors checked off. In the no-overlap condition, the other person had none of the same behaviors checked off, but the behaviors checked off for the other person matched the participant's in terms of importance. Thus, the other person always had the same number of behaviors checked as the participant, as well as having behaviors that were close (or identical) in terms of importance. In the overlap condition, these behaviors were the same, whereas in the no-overlap condition they were different.

The other student's questionnaire was carefully prepared to appear as if the original identification numbers assigned in pretesting had been obscured, ostensibly to protect the other student's anonymity. Furthermore, the other student's questionnaire was completed in one or more ways that differed from the way participants had completed their own questionnaires in pretesting (e.g., if the participant used pencil, the other questionnaire was in ink; if the participants used checkmarks, the other student used an "x").

PARTICIPANTS

Participants included 114 students (79 women and 35 men) who had completed the pretesting questionnaire in exchange for partial fulfillment of a course requirement. The mean number of religious behaviors checked off by students who participated in the actual study was 4.31 ($SD = 1.90$), with a mode of 3.

PROCEDURE

After arriving, participants were told via the consent form that they would participate in a study examining how people make comparisons. Then they completed a set of background questionnaires, which included

Heatherton and Polivy's (1991) State Self-Esteem Scale, along with a short, adjective self-rating scale based on Big-Five dimensions (included to make the self-esteem questionnaire appear to be part of a larger personality battery).

After completing the background questionnaires, participants were reminded that they had completed the religious behaviors questionnaire at the beginning of the term. They were told they would see another student's questionnaire (in actuality, the fabricated questionnaire) and were told to imagine they were going to meet the person who had completed the questionnaire and to use the list of behaviors to form an impression of that person's religiousness.

COMPARISON CONDITIONS

After being given 60 seconds to look over the other questionnaire, participants were assigned to a direction of comparison condition via written instructions (the experimenter was unaware which condition they received). Participants in the self as target condition were instructed as follows:

Please compare YOURSELF to the person who filled out the questionnaire in terms of religiousness. In other words, how religious are you compared to the person who filled out the questionnaire?

Participants in the other as target condition were instructed as follows:

Please compare the PERSON WHO FILLED OUT THE QUESTIONNAIRE to yourself in terms of religiousness. In other words, how religious is the person who filled out the questionnaire compared to you?

All participants then completed the same set of dependent measures, although the order of the questions varied slightly depending on condition. All participants were first asked to rate which of the two people (self or other) was more religious on a 12-point scale (based on measures used by Houston & Sherman, 1995) anchored at 1 (*I'm much more religious*) and 12 (*other is much more religious*). This scale was designed to capture relative differences between the two individuals, with high numbers always indicating that the other person was more religious. Average scores greater than 6.5 indicated that participants viewed the other person as more religious than the self, whereas average scores less than 6.5 indicated that participants viewed the other person as less religious than the self.

Participants in the self as target condition then rated both people (self first, other next) separately on a 7-point scale ranging from *not at all religious* (1) to *very religious* (7). These scales could thus capture any effects on the

TABLE 1: Relative and Absolute Ratings of Self and Other, Study 1

	<i>Absolute Rating of Self</i>	<i>Absolute Rating of Other</i>	<i>Relative Ratings</i>
Self as target			
Overlap	3.79 (1.40)	3.86 (1.06)	5.97 (1.35)
No overlap	3.74 (1.38)	4.67 (1.39)	7.58 (2.45)
Mean	3.77 (1.38)	4.25 (1.28)	6.73 (2.09)
Other as target			
Overlap	3.77 (1.22)	4.13 (1.28)	6.70 (1.74)
No overlap	2.79 (1.50)	4.82 (1.44)	8.82 (1.98)
Mean	3.29 (1.44)	4.47 (1.39)	7.72 (2.13)

NOTE: Absolute ratings of self and other were on a 7-point scale, high numbers indicating greater religiousness. Relative ratings were on a 12-point scale; higher numbers indicate other is seen as more religious relative to the self. Standard deviations are in parentheses.

absolute ratings of the self and other, as well as relative differences in the two, by using the self- and other ratings as repeated measures.

Participants in the other as target condition completed the same measures, only they rated the other person first and themselves second. Finally, all participants were asked to rate how similar the self and other were using a 7-point scale ranging from *not at all similar* (1) to *very similar* (7). At the conclusion of the study, all participants completed a questionnaire probing for any confusion or suspicion before they were debriefed and thanked for their participation.

Results

A manipulation check of the overlap manipulation was performed using a 2×2 ANOVA on the similarity ratings. As expected, the self and other were seen as more similar when they had overlapping features ($M = 5.83$, $SD = 1.55$) than when they did not ($M = 4.00$, $SD = 1.76$), $F(1, 110) = 36.20$, $p < .001$. We also explored whether our results replicated other studies that have investigated direction of comparison effects in similarity judgments. Contrary to earlier findings (e.g., Catrambone et al., 1996; Holyoak & Gordon, 1983; Srull & Gaelick, 1983), the self and other were seen as more similar when participants compared self to the other (self as target, $M = 5.30$, $SD = 1.67$) rather than comparing the other to self (other as target, $M = 4.60$, $SD = 2.03$), $F(1, 110) = 5.62$, $p = .02$.

CANCELLATION EFFECTS

The first major question was whether there would be a cancellation effect such that religiousness ratings of the self and other would be lower when they overlapped than when they did not. Participants' absolute ratings of religiousness for themselves and the other person were analyzed in $2 \times 2 \times 2$ within-between ANOVA, with self-ratings versus other ratings as the within-subjects factor and overlap (overlap vs. no overlap) and direction of

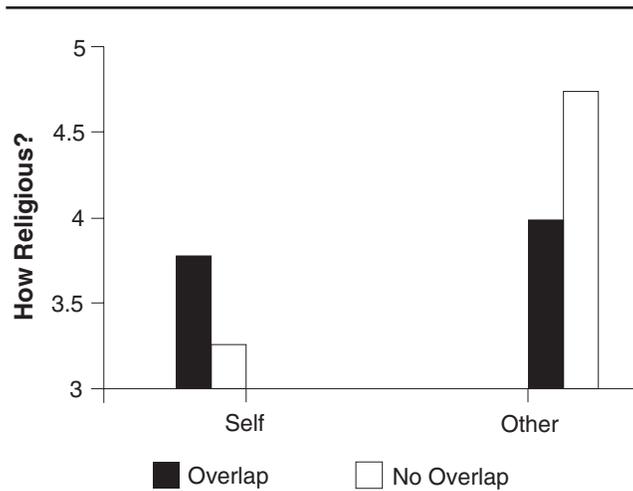


Figure 1 Study 1: Absolute ratings of self and other by overlap condition.

comparison instructions (self as target vs. other as target) as between-subjects factors (see means in Columns 1 and 2 of Table 1). The ANOVA revealed a main effect of the self-other factor, indicating a general trend to rate the other person as more religious than the self, $F(1, 110) = 39.33$, $p < .001$. Cancellation effects were seen for the other person but not the self. This was visible in the two-way interaction of the self-other factor and overlap condition on the absolute ratings of the self and other, $F(1, 110) = 21.74$, $p < .001$ (see Figure 1). Religiousness ratings of the other person were lower in the overlap condition than in the no-overlap condition, but religiousness ratings of the self were actually higher in the overlap condition than in the no-overlap condition.

Analysis of the 12-point relative scale echoed the absolute ratings, revealing a main effect of overlap, $F(1, 109) = 27.03$, $p < .001$ (see Column 3 of Table 1). Scores were lower—that is, relatively closer to the “self is more religious” pole than the “other is more religious” pole—in the overlap condition, indicating that the self was seen as more religious, whereas scores were relatively higher in the no-overlap condition, indicating that the self was seen as less religious.

DIRECTION OF COMPARISON EFFECTS

The second major question was whether there would be a direction of comparison effect, which would be supported if the target of comparison (either self or other, depending on comparison condition) was rated higher than the referent. Consistent with a direction of comparison effect, scores on the 12-point relative scale (anchored with *self is more religious* at the low end and *other is more religious* at the high end) were lower when the self was the target and higher when the other person was the target, $F(1, 109) = 7.60$, $p = .007$ (see last column of

Table 1). Thus, although there was a general trend to always view the other person as more religious than the self (i.e., the overall mean was greater than 6.5), this effect was stronger when the other person was the target and weaker when the self was the target, indicating that whoever was the target of comparison was seen as relatively more religious.

A direction of comparison effect also was visible in the absolute ratings of the self and other. This effect was demonstrated by the significant two-way interaction of the self-other factor and order of comparison instructions, $F(1, 110) = 6.75, p = .01$. Ratings of the self were higher when the self was the target than when the other person was the target; ratings of the other person were higher when the other person was the target than when the self was the target.

High-self-esteem participants (based on a median split of this variable) rated everyone (self and other) as marginally less religious on average than low-self-esteem participants, $F(1, 106) = 3.83, p = .053$. However, high self-esteem did not provide a buffer against feature-matching effects. Nine participants reported some suspicion about the similarity of the other person to themselves (interestingly, two of these participants were in the no-overlap condition). Analyses conducted without these participants yielded results virtually identical to those done on the entire sample.

Discussion

Feature-matching models do seem applicable to self-other comparisons under the conditions tested in Study 1, with a couple of interesting twists. The first question addressed by Study 1 was whether shared features cancel each other out in self-other comparisons. There is an egocentric caveat to the answer: It appears they cancel for other people but not for the self. To the extent that another person shares characteristics that are diagnostic of a particular dimension, people lower their ratings of the other person on that dimension. However, when evaluating themselves, people's self-ratings do not reflect the cancellation of shared features. In fact, self-ratings were relatively higher when our participants shared diagnostic characteristics with other people than when they did not share these features. These results are consistent with a model that views feature cancellation as an information-processing tool that may be needed more for processing information about a novel, unfamiliar person than for the highly familiar self.

The answer to our second major question, whether linguistic direction of comparison instructions would affect the evaluations of self and other, was yes. Whoever was the target (self or other) was seen as relatively more religious than the referent, both when the self and other were rated on absolute scales and when a one-dimen-

sional relative scale was used, with the self and other at opposite ends. Given that the feature-matching models predict greater attention to the features of the target of comparison, and all those features in this study were diagnostic of greater religiousness, this result is consistent with what would be predicted by feature-matching models.

It is interesting to note that this result occurred for ratings of the other person both in the no-overlap condition (where it traditionally occurs) and in the overlap condition, where the features of the target of comparison were not unique. This did not appear to be the case for ratings of the self, although the three-way interaction of self-other, direction of comparison instructions, and overlap was not significant, $F(1, 110) = 2.25, p = .137$. For self-ratings, the direction of comparison effect was virtually nonexistent in the overlap condition but pronounced in the no-overlap condition (a post hoc contrast comparing direction of comparison effects for self-ratings in the no-overlap condition was significant, $F[1, 110] = 5.08, p = .026$). Thus, even when the other person had no unique features, focusing on the other person as the target made this person seem more religious, whereas focusing on the self as the target only raised ratings of self-religiousness when the self had unique features.

Our participants viewed themselves and the other person as more, not less, similar when the self was the target (particularly in the no-overlap condition), a result that runs counter to several previous studies that show a pattern of greater similarity ratings when the other person was the target of the comparison. However, Karylowski (1989) has suggested that this pattern is less pronounced for similarity judgments about a specific trait (which we were measuring in this study) than it is for global similarity judgments. Furthermore, Holyoak and Gordon (1983) also found the usual pattern was reduced or even slightly reversed when the other person was a member of a group about which research participants could easily generate a number of attributes. By providing participants in our study with a list of the features of the other person, rather than requiring them to conjure up a description of the other person on their own, generating attributes for the other person would have been easy.

Interestingly, self-esteem did not moderate the results in Study 1. One possibility is that the self-clarity that accompanies self-esteem (see Baumgardner, 1990; Campbell, 1990) is too general to have a big impact on judgments in a specific domain such as religiousness. A measure of individual differences that was more specific to the dimension along which comparisons were being made might moderate the results more powerfully. Thus, in Study 2, in addition to replicating aspects of

Study 1, a measure of religious schematicity also was included. In Study 2, an additional goal was to map the boundary conditions for the direction of comparison results found in Study 1. The results found in Study 1 may occur only under conditions that are strong enough to dislodge the self as a habitual referent and may disappear with a more subtle manipulation of direction of comparison. Thus, in Study 2, order of presentation of the two descriptions was used to manipulate direction of comparison rather than explicit linguistic instructions.

STUDY 2

Method

PARTICIPANTS

Participants included 181 students who participated in the study in exchange for partial fulfillment of a course requirement. Only participants who checked off between 4 and 15 behaviors on the religious behaviors checklist (described below) were included in the analyses, and 2 additional participants were removed due to experimenter errors, leaving a sample of 103 participants (65 women and 38 men).

PRETESTING

In an earlier departmental pretesting session, participants were pretested for religious schematicity. Descriptive statements (based on questions in previous studies of schematicity; e.g., Kendzierski & Whitaker, 1997) such as "My religious beliefs are important to me" and statements that measured how invested one is in religiousness, such as "I spend a lot of time participating in religious activities," made up the measurement for religious schematicity. Participants rated each statement on an 11-point scale as to how strongly they agreed, with 1 = *strongly disagree* and 11 = *strongly agree*. There were eight statements in total (the complete list of items is available from the authors).

PROCEDURE

Participants were run in groups of no more than four. After entering the lab, they were informed that they would be participating in a study that dealt with how people make comparisons. Participants first completed the same checklist of 20 religious behaviors used in Study 1. While participants completed additional background measures,² the experimenter created the description of another person for each participant. Participants were randomly assigned to either the high-overlap or low-overlap condition. Unlike in Study 1 when the self-questionnaire was completed in pretesting, participants in Study 2 completed their own questionnaires during the same session in which they were given the other person's checklist. In the high-overlap condition in Study 2, all of

the behaviors checked off on the other person's questionnaire were the same as those that the participant checked off, except for one. For participants in the high-overlap condition to share at least three behaviors, we excluded from analysis any participants who checked off fewer than four behaviors.³ The participant's behavior ranked as least important according to our pretesting rankings was changed on the other person's questionnaire to a different but similarly ranked behavior.

In the low-overlap condition, five behaviors on the other person's questionnaire differed from the participant's questionnaire.⁴ An exception was made if the participant checked off four behaviors, in which case only four behaviors, all of which differed from the participants' questionnaire, were checked off on the other person's questionnaire. The other person's behaviors that differed were selected with an eye to finding a similarly ranked different behavior that the participant did not perform. Thus, if the participant checked a behavior that ranked 8th out of the 20 behaviors, the experimenter would choose either the behavior ranked 9th or 7th as the corresponding behavior on the other person's questionnaire.

Crossing the high-/low-overlap manipulation, groups of participants also were randomly assigned to an order of comparison condition, with either the self as target or the other person as target. In the self as target condition, participants were first given the other person's questionnaire. Participants were told that the questionnaire had been completed by a previous participant. The experimenter asked them to imagine that they were going to meet that person and to look over the questionnaire carefully so they would be knowledgeable about that other person's religious behaviors. After about 30 seconds, the experimenter retrieved the questionnaire and gave participants their own questionnaire again. Participants in the other as target condition followed the same procedure, only they were first given their own questionnaire to look over and then the other person's questionnaire.

As in Study 1, all participants then completed the same set of dependent measures, although the order of the questions varied slightly and corresponded to the direction of comparison. All participants were first asked to rate which of the two people they had been asked to compare (self or other) was more religious on a 12-point scale anchored with *I'm much more religious* (1) and *other is much more religious* (12). Participants in the self as target condition then rated both people (themselves first and the other person next) on absolute scales ranging from *not at all religious* (1) to *very religious* (7). Participants in the other as target condition completed the same measures, only they rated the other person first and themselves second on the two scales. In other words, all participants were asked first about the person whose checklist

they had seen last, another way of designating the target of comparison.

All participants were then asked to rate how similar the two people that they had read about were, using a 7-point scale ranging from *not at all similar* (1) to *very similar* (7). Once the participants had finished the dependent variable questionnaire and it had been retrieved by the experimenter, the participants were asked to rate on a 7-point scale how important they thought each of the behaviors on the checklist was in terms of religiousness. Finally, participants were asked to recall as many of the other person's behaviors as they could. At the conclusion of the study, all participants completed a questionnaire probing for any confusion or suspicion before they were debriefed and thanked for their participation.

Results

As in Study 1, a manipulation check of the overlap manipulation was performed using a 2×2 ANOVA on the similarity ratings. Once again, there was a strong main effect of overlap on similarity ratings, $F(1, 98) = 58.62, p < .001$, with self and other being rated as more similar when they overlapped on many behaviors ($M = 6.14, SD = 1.04$) than when they did not ($M = 4.04, SD = 1.60$).

The results of Study 2 that follow are divided into three parts. The first two sections parallel Study 1 by addressing the same two major questions: Were there cancellation effects and was there a direction of comparison effect? In the third section, we conduct exploratory analyses looking for possible moderating effects of schematicity.

CANCELLATION EFFECTS

The first major question in Study 2 was whether shared features would be canceled out. As in Study 1, the results showed that shared features cancelled out in the ratings of the other but not for the self. A $2 \times 2 \times 2$ within-between ANOVA was performed on the absolute ratings of self and other, with self- versus other ratings as the within-subjects factor and overlap (high vs. low) and order of comparison (self as target vs. other as target) as between-subjects factors. As in Study 1, there was a main effect of self versus other on the absolute ratings, such that the self was on average seen as less religious than the other, $F(1, 98) = 17.62, p < .001$ (mean religiousness rating for self = 4.50, $SD = 1.46$; mean religiousness rating for other = 5.00, $SD = 1.18$). Consistent with the cancellation results of Study 1, there was also a two-way interaction of self-other and overlap condition such that the self was seen as more religious in the high-overlap condition, whereas the other was seen as more religious in the low-overlap condition, $F(1, 98) = 14.61, p < .001$ (see Figure 2).

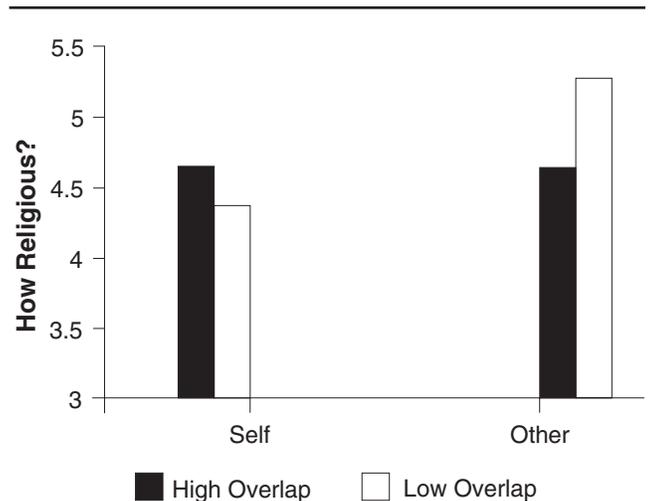


Figure 2 Study 2: Absolute ratings of self and other by overlap condition.

As in Study 1, a 2×2 ANOVA on the relative scale ratings revealed a main effect of overlap. The other person was rated as relatively more religious than the self (e.g., higher numbers on this scale) in the low-overlap condition, whereas the self was relatively more religious than the other (e.g., lower numbers on this scale) in the high-overlap condition, $F(1, 99) = 11.80, p < .001$ (mean for low-overlap condition = 7.75, $SD = 2.12$; mean for high-overlap condition = 6.54, $SD = .93$; higher numbers indicate that the other person is seen as relatively more religious than the self).

Study 2 also allowed us to see if canceling the other person's shared features meant that participants were less able to recall these behaviors or, as in previous studies, if shared features were actually better recalled. Because participants who listed more behaviors themselves also had to remember more behaviors for the other person, we covaried out the number of behaviors in these analyses, producing a significant covariate, $F(1, 98) = 14.98, p < .001$. There was a main effect of overlap, with participants recalling more of the other person's behaviors when they overlapped more with the participant's own behaviors, $F(1, 98) = 23.45, p < .001$ (mean percentage of behaviors recalled in high-overlap condition = 77.1%, $SD = 18.7$; mean percentage recalled in low-overlap condition = 59.7%, $SD = 20.1$).

DIRECTION OF COMPARISON EFFECTS

Unlike Study 1, there was no evidence of a direction of comparison effect in Study 2. On the 12-point relative scale, the main effect for order of comparison was not significant, $F = 2.06, p = .155$ (although the means were in the same direction as Study 1, self as target $M = 6.82, SD = 1.72$ and other as target $M = 7.33, SD = 1.75$). Using the absolute ratings, a direction of comparison effect would

be represented by a two-way interaction of self-other and order of comparison, with whoever was the target (i.e., either the self or the other person) being seen as more religious; however, this interaction was not significant, indicating no direction of comparison effect in the absolute ratings either.

Interestingly, in Study 2, both the self and the other person received higher religiousness ratings when participants saw the other person's questionnaire first (self as target) than when they saw their own questionnaire first (other person as target), $F(1, 98) = 4.71, p = .04$. An examination of the means suggests that participants in the high-overlap condition were largely responsible for producing this result, as evidenced by the two-way interaction of order of comparison and overlap condition, $F(1, 98) = 4.61, p = .034$. Collapsing across the ratings for self and other in the high-overlap condition, the mean religiousness rating was 5.19 when the self was the target ($SD = 1.31$) and only 4.12 when the other person was the target ($SD = 1.16$), whereas in the low-overlap condition, the mean was 4.83 when the self was the target ($SD = 1.11$) and a nearly identical 4.82 when the other person was the target ($SD = 1.05$). In other words, when participants in the high-overlap condition saw the other person first (i.e., the self was the target), average ratings of both the self and other went up. When they saw their own questionnaire first (i.e., the other person was the target), average ratings of both the self and other went down. Participants in the low-overlap condition were not affected in this way by direction of comparison.

One speculative interpretation of these results is that in the high-overlap condition, where the two sets of features were highly similar, once participants arrived at their judgment of the target (the last description they saw and the first person they rated), they adjusted their rating of the referent to match it accordingly. Thus, after canceling out the shared features of the other person when that person was the target, participants also lowered their ratings of themselves, given that there was so much overlap between self and other. When the self was the target (and thus not subject to cancellation), participants raised their ratings of the other person to bring it in line with the highly similar self. This pattern of results was not found in Study 1, and similarity ratings were fairly similar across the two studies. However, the pressure to assimilate the judgment of the referent to the target may have been greater in Study 2 when the degree of overlap between the two sets of features may have been highlighted, given that participants saw them in rapid succession.

INDIVIDUAL DIFFERENCES IN SELF

To address the question of whether participants who spent a lot of time thinking about religiousness would

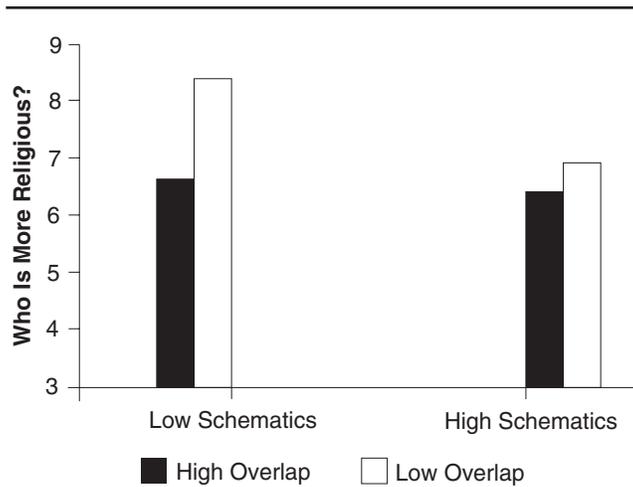


Figure 3 Study 2: Relative scale ratings.

NOTE: High numbers mean other is more religious; low numbers mean self is more religious.

show the same contextual effects in self-other comparisons as those who did not, we added schematicity for religiousness as an exploratory variable to our analyses. A reliability analysis of the schematicity items yielded a Cronbach's alpha of .918 for seven of the eight items; therefore, the mean of these seven items (the other item was dropped) was used as the measure of schematicity. Adding the median split of schematicity as a dichotomous variable⁵ to the analyses using the 12-point relative scale produced a main effect of schematicity, $F(1, 93) = 6.01, p = .016$. High schematics tended to rate the self as relatively more religious than the other person ($M = 6.70, SD = 1.46$), whereas low schematics tended to rate the other as relatively more religious than the self ($M = 7.65, SD = 1.93$; higher numbers indicate the other is seen as relatively more religious than the self). Schematicity also interacted with overlap condition, indicating that low schematics were more affected by the overlap condition than high schematics, $F(1, 93) = 3.98, p = .049$. In other words, low schematics viewed themselves as more religious in the high-overlap condition and the other person as more religious in the low-overlap condition, whereas high schematics viewed themselves and others in relatively the same way regardless of how much they overlapped (see Figure 3).

In Study 2, we also asked all participants to rate how important each of the religious behaviors on our list was, as well as to list from memory all those behaviors checked off by the other person. High schematics generally rated all behaviors performed by the self and other as more important (mean importance = 4.67, $SD = .99$) than did low schematics (mean importance = 4.17, $SD = 1.06$), $F(1, 92) = 4.39, p = .039$, possibly because the behaviors high schematics did were generally consid-

ered more important, as were the behaviors assigned to the other person (these behaviors were, after all, matched on importance, based on pretesting ratings of importance). In addition to this finding, unique behaviors performed by the self were viewed as more important ($M = 4.43$, $SD = 1.66$) than unique behaviors performed by the other person ($M = 2.80$, $SD = 1.40$), $F(1, 91) = 79.27$, $p < .001$. High schematics were particularly likely to view unique behaviors performed by the self as more important than unique behaviors performed by the other person, $F(1, 91) = 8.47$, $p < .005$ (high schematics rated the importance of self-unique behaviors as 4.76 and the importance of other unique behaviors as 2.67, whereas low schematics gave ratings of 4.10 and 2.99, respectively).⁶

Discussion

Study 2's results replicated the cancellation effect but failed to demonstrate the direction of comparison effect reported in Study 1. The self-other asymmetry in canceling out the shared features of the other person but not the self was replicated: Ratings of the other person were lower and self-ratings were higher when self and other overlapped a lot. A measure of religious schematicity moderated this pattern of results when the relative scale was used, suggesting that low schematics were more affected by the overlap manipulation than high schematics.

Participants recalled more of the other person's behaviors in the high-overlap condition than in the low-overlap condition. As in past feature-matching studies (e.g., Hodges, 1997; Houston et al., 1989), canceling shared features does not appear to mean they are forgotten. In the present study, participants had an additional reason for being able to recall the shared features: Not only were the shared features presented twice (in the two checklists) but they were also features of the self. Self-referencing (e.g., S. B. Klein & Kihlstrom, 1986; Rogers et al., 1977) would presumably facilitate recall of these features.

One alternative explanation of the asymmetrical cancellation for self and other is that participants raised their ratings of the other person in the low-overlap condition because they were particularly impressed when the other person engaged in religious behaviors that they themselves did not perform (in essence, the definition of unique features for the other person in this study). Because we equated the self's and the other's unique features in terms of religiousness, this interpretation would mean that participants were weighting the idiographic uniqueness of a feature more heavily than its nomothetic importance for religiousness when evaluating other people, still an interesting result from a feature-matching perspective. However, the importance ratings collected in Study 2 speak against this interpreta-

tion: Participants generally rated the unique behaviors performed by the other person as less important than the unique behaviors they themselves performed.

The importance results pose another riddle. Given the human inclination toward self-serving bias, participants' general tendency to view the other person as more religious than the self (in both studies) suggests that religiousness may not be such a desirable trait among our sample of college students. However, participants also thought that their own unique behaviors were more important in terms of religiousness, which suggests that there was some self-serving bias toward viewing the self as more religious. Kruger's (1999) finding that the "better-than-average" effect holds only in domains in which absolute performance tends to be high may help explain why participants viewed themselves as less religious than the other person, despite the importance ratings. If the group of college students in our sample tend to be fairly low in religiousness, then we should not expect a better-than-average effect for the self. Furthermore, Kruger's work has demonstrated that people sometimes even see themselves as lower than average in domains where absolute performance levels tend to be low, as may be the case for the somewhat abstract, hard-to-clearly-achieve dimension of religiousness.

Still, it is intriguing that participants generally viewed behaviors performed by the self as more important in terms of religiousness and yet their rating of the other person was inversely related to the relative number of these behaviors the other person performed. Clearly, a simple linear weighting of the importance of each of the behaviors does not provide a good model of participants' religiousness ratings. Furthermore, the results show more than a simple context effect (such as when a globally good option makes other options look worse by comparison). Instead, the context in this case is created during the feature-matching process, based on whether individual features are shared.

Study 2 showed no direction of comparison effect. Unlike Study 1, the target was not seen as relatively more religious than the referent in Study 2. Direction of comparison in Study 2 was manipulated by varying the order in which the two checklists of behavior were presented as well as the order in which the evaluation questions were asked. Although order alone has been used as the sole manipulation in studies with other stimuli (e.g., Agostinelli, Sherman, Fazio, & Hearst, 1986), this manipulation may have been too weak to affect self-other judgments. In addition, in Study 2, the lists of features for both the self and other were seen within minutes of making the religiousness ratings, whereas in Study 1, participants were not reminded of what they had listed for their own behaviors on the pretesting questionnaire some weeks earlier. Houston and Sherman

(1995) found that simultaneous presentation of stimuli reduced direction of comparison effects. Even though the self and other lists were not presented simultaneously in Study 2, they were presented in close sequence. In the absence of a successful manipulation of direction of comparison, all other things being equal, the self will be used as a habitual referent (Catrambone et al., 1996), making the other person the target of comparison. Under these circumstances, given the nature of the unique features, the other person should have been seen as more religious than the self, as was the case.

GENERAL DISCUSSION

Across two studies, we found that when people were asked to compare the religiousness of themselves and another person, they rated someone with whom they shared many religious behaviors lower than someone with whom they shared fewer behaviors. This result is consistent with feature-matching and cancellation results found for a variety of stimuli. However, in our studies, we found that for one special class of stimuli—the self—shared features do not appear to cancel each other out. Evaluations of the self actually tended to be higher when the self and the other person shared features. These results were found consistently even though the two studies varied the extent to which the high- and low-overlap conditions differed. The extreme version of the manipulation was used in Study 1, with no overlap versus total overlap. In Study 2, both the self and other always had at least one unique behavior, even in the high-overlap condition, and the number of shared behaviors in the low-overlap condition was relative to the number of behaviors our participants checked off for themselves.

Why was there an asymmetry in judgments of the self and judgments of the other? Feature-matching strategies appear to be used more in judgments that are made feature by feature than in global judgments (Sanbonmatsu, Kardes, & Gibson, 1991). Furthermore, matching and canceling shared features is a potentially effortful way to process information about a stimulus (Hodges, 1997). People may expend the effort only for stimuli for which they have no stored information relevant to a global evaluation. To the extent that people already have some understanding of their own religiousness, they may not have to resort to matching up shared features to figure out how to rate themselves. Instead, encountering another person who does the same religious behaviors may provide a kind of social proof, boosting their ratings of their own religiousness, as we found in these two studies. By canceling out shared features for others, but not themselves, our participants were highlighting the differences in other people. By not canceling shared features for the self, their self-judgments instead highlighted the extent to which they were similar to others. It

is as if the participants' judgments of others reflected contrast, whereas their judgments of themselves reflected assimilation.

The other pattern of results associated with feature-matching models, a direction of comparison effect, was found in the first study but not the second. The direction of comparison manipulation may have been too subtle in Study 2, or perhaps the features of the self were too highly accessible (or some combination of these factors). Of the two processes identified in feature-matching models—matching and canceling, and direction of comparison—direction of comparison is the one that appears to be more easily manipulated and modified (Hodges, 1998; Houston & Sherman, 1995; but see also Dhar, Nowlis, & Sherman, 1999). Researchers have found a number of ways to moderate direction of comparison effects, both in evaluative judgments of stimuli not involving the self (Hodges, 1998; Houston & Sherman, 1995; Sanbonmatsu et al., 1991) and in similarity judgments that do involve the self (Holyoak & Gordon, 1983; Karylowski & Skarzynska, 1992; Srull & Gaelick, 1983). By contrast, matching and canceling effects may be less mutable, at least in situations where people have no stored judgments. Indeed, the alignment of shared dimensions may be fundamental to all comparative judgments (Gentner & Markman, 1994). The fact that our overlap manipulation had the most consistent effect on participants' evaluations across our two studies fits with these theories.

Caveats and Further Directions

We purposefully chose religiousness as the comparison dimension for these studies because we wanted a dimension for which our participants would be unlikely to have objective or stored information regarding their standing in relation to other people. Other dimensions such as intelligence or athleticism may have more objective markers that might circumvent the use of feature matching and thus might produce a different pattern of results (e.g., "It does not matter how many intelligent features she has, I know my IQ is higher"). Furthermore, we may have found different results with a dimension that was valued more among college students, such as healthiness (e.g., Bruininks & Hodges, 2000), sociability, or intelligence.

The tendency to use feature-matching models of comparison may not only vary across dimensions but also across people within a dimension. We explored whether people who have greater self-clarity associated with higher self-esteem may be less susceptible to the effects of feature matching. Self-esteem had no effect on our results, but the schematicity measure did moderate the cancellation effect in the predicted direction (high schematics were less affected by the overlap manipulation)

on the relative measure. However, our designation of high and low schematics was to divide our sample using a median split on the mean score for seven items. Thus, we may actually have been looking not at high and low schematics but at low and lower schematics. Another possibility, given the questions in our schematicity measure, is that the group we labeled low schematics may actually be highly schematic for “unreligiousness” (i.e., a trait that is not only defined merely by the absence of religiousness, in the way that “evil” is not merely the absence of “good”). As a nonmanipulated variable, schematicity could easily have been correlated with how positively a participant viewed religiousness; we know it was correlated with how many religious behaviors they (and thus how many religious behaviors the other person) performed ($r = .61, p < .001$). Furthermore, students who reported the greatest and smallest numbers of religious behaviors were not included in the studies reported here because it was impossible to manipulate overlap for these people. Even with this limit on our sample, our findings are still important, given that the group in the middle of the distribution is likely to be most representative of the population. However, we cannot say whether people at the extreme ends of the scale use the same strategies or arrive at the same conclusions.

The present studies differed from other studies that have applied Tversky’s (1977) original model of comparison to the self and other by providing an explicit list of features that described the self and the other. It could be argued that in many naturalistic comparison settings, we are not often handed a list of features describing another person, and we do not think of ourselves as merely a list of attributes. However, there are a number of contexts when people do become their features, such as in employment selection. Even in less structured settings, referring to specific features is certainly not unheard of (e.g., “Why is she dating HIM? I drive a convertible, play in a band, and can bench press 200”). Paradigms that allow for the study of comparative judgments in ongoing social interactions would add a valuable new dimension to our work.

Given that this was the first application of feature-matching models to trait evaluations, we kept things simple by making all of our features (the religious behaviors) diagnostic of the dimension. Further research should investigate what happens when features that indicate the absence of a trait (e.g., “does not believe in any form of God”) are included in the mix. In past feature-matching studies investigating evaluative judgments, results have not always been symmetrical for positive and negative features (Dhar et al., 1999; Tucker, 1998).

In conclusion, the work presented here provides a novel finding, demonstrating that overlap affects judgments of the self differently from judgments of other

people. If another person shares a greater number of features with oneself that are diagnostic of a particular dimension, that other person will receive lower ratings on that dimension. Ratings of the self, on the other hand, appear to go up (if they move at all) when a greater number of features are shared with another person. Although this particular pattern of results is novel, the results are also consistent with a number of basic tenets in social and cognitive psychology. The self has long been recognized as having special status as a reference point, and aligning shared features and dimensions appears to be a fundamental component of comparison. Perhaps not surprisingly, we are quite pleased with the extent that our findings overlap with these other past results.

NOTES

1. Tversky called the thing being compared the “subject of comparison,” but our experience is that “target of comparison” is a more intuitive and less confusing term.

2. Participants also completed self-esteem measures, as in Study 1, but because self-esteem did not interact with any of the other variables, these measures will not be discussed.

3. Note that this means that participants in the low-overlap condition who checked off many behaviors sometimes shared more behaviors than those in the high-overlap condition who checked off few behaviors (e.g., a participant in the low-overlap condition who checked off 12 behaviors would share 7 behaviors; a participant in the high-overlap condition who checked off 6 behaviors would only share 5). However, the design of the study reflected a belief that percentage of shared behaviors was as important, if not more important, than absolute number of shared behaviors.

4. This necessitated excluding participants who checked off more than 15 behaviors, because there were only 20 behaviors, and if a participant did 16 or more of them, it limited the extent to which we could manipulate overlap, giving us too few unique behaviors to work with.

5. We follow the custom set in other studies of schematicity by treating it as a dichotomous variable. However, we ran regression analyses using schematicity as a continuous variable too. The beta weight on the schematicity term was not significant ($t = 1.44, p = .15$), but the beta weight on the Schematicity \times Overlap interaction term was significant ($t = 2.44, p = .016$).

6. These analyses were run on the entire sample, including participants in both the high- and low-overlap conditions, even though in the low-overlap condition, there was only one unique self-behavior and one unique other behavior. Not surprisingly, unique behaviors were considered generally more important in the low-overlap condition than in the high-overlap condition, $F(1, 91) = 14.14, p < .001$, a result most easily explained by the fact that there were more unique behaviors in the low-overlap condition than the high-overlap condition. Overlap condition did not interact with any other factor, and the pattern of results looked very similar when only participants in the low-overlap condition were included in the analyses.

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