

Ascribing Advantages to Social Comparison Targets

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Three experiments examined how people perceive a social comparison target when a dimension important to identity is threatened and a relatively unfavorable social comparison is anticipated. All 3 experiments show that people will perceive or exaggerate advantages in a target, that make the target inappropriate for social comparison, when they anticipate a comparison with the target and are uncertain of the outcome. Experiments 2 and 3 show that reports of some target advantages are moderated by individual differences in self-esteem, such that people with low self-esteem are more likely than people with high self-esteem to perceive that a comparison target enjoys subtle, subjective advantages. Finally, Experiment 3 shows that the report of overt target advantages reflects actual perceptions on the part of the perceiver, and are not merely self-presentational claims intended to manage audience attributions.

How do people know if they are smart, attractive, or skilled? One way people know is if others tell them. Thus, athletes learn that they are skilled, or not skilled, when others tell them so. A second and perhaps more common way of knowing is through social comparison. That is, people come to know their abilities, skills, and attributes through comparisons with others (Festinger, 1954). Others serve as a standard by which people can judge the correctness of their attitudes and opinions and evaluate their skills, competencies, and abilities. Thus, a tennis player can judge her tennis skills by comparing herself with opponents. If she defeats most of her opponents, she can conclude that she is a good tennis player. If she typically loses to her opponents, she can conclude that she is not so good at tennis. But are people objective in their perceptions of opponents and other comparison targets, or might they perceive comparison targets in ways that are self-enhancing? This study examines how people perceive a social comparison target when a dimension important to identity is on the line, and a relatively unfavorable social comparison is anticipated.

MANAGING SOCIAL COMPARISONS

The superior performance of others can have positive consequences for a person's identity and self-esteem (Collins,

1996; Wood, 1989), by providing inspiration (Buunk, Collins, Taylor, VanYperen, & Dakof, 1990) and an opportunity to "bask in his or her reflected glory" (Cialdini et al., 1976; Tesser, 1988). Nevertheless, it also has a downside, insofar as it implies personal deficiency in abilities, traits, behaviors, or attitudes. As such, upward social comparison can be a source of negative affect (Brickman & Bulman, 1977; Buunk et al., 1990; Gibbons, 1986; Pleban & Tesser, 1981), jealousy (Salovey & Rodin, 1984), and anger and resentment (Crosby, 1976; Martin, 1986). To the extent that the negative consequences outweigh the positive consequences, upward social comparisons can be unpleasant.

Given the potential negative identity and esteem consequences of comparing unfavorably with others, it is perhaps not surprising that people are sometimes biased in their selection of social comparison targets, occasionally opting for a target who is inferior. Wills (1981) coined the term *downward social comparison* to describe the strategy of selectively making comparisons with an inferior target. According to Wills (1981, 1987, 1991), people choose less fortunate others for social comparison when their self-esteem is threatened. By so doing, they can increase their subjective well-being. Moreover, if a less fortunate target is unavailable, there is evidence that people will make a target worse off, or in some cases, will create or imagine a target who is worse off. For example, Gibbons (1985) found that mentally retarded adolescents derogated a fellow retarded adolescent, thereby making the target worse off. Other research has shown that women with breast cancer will imagine other women with breast cancer who are faring worse than themselves (Taylor & Lobel, 1989).

SOCIAL COMPARISON IN CONSTRAINED ENVIRONMENTS

The research by Wills and others suggests that comparers can occasionally be thought of as motivated tacticians (Fiske & Taylor, 1991), who choose or create comparison targets with an understanding of the consequences. Unfortunately, people are not always at liberty to choose or manipulate their social comparison targets. Some situations, by their very nature, constrain the individual to make a comparison with a specific person (Wood, 1989). For example, siblings are often compelled to compare themselves to one another. Similarly, in a tennis match, the obvious and most immediate target for social comparison is the opponent. In situations such as these, short of impairing the opponent's performance, downward social comparison may not be an option. Tesser proposed that people may protect their identity by reducing the centrality or relevance of the domain to identity (e.g., an unsuccessful tennis player might take up a different sport; Tesser, 1988; Tesser & Paulhus, 1983). However, some domains form the core of one's identity, making it impossible to reduce the centrality or relevance (e.g., an individual may define him or herself in terms of ability on the tennis court).

The negative consequences of an unfavorable social comparison can be diminished, however, if the comparison target is regarded as dissimilar on a performance-related attribute (Mettee & Smith, 1977). As noted some 20 years ago (Goethals & Darley, 1977), performance is often not a function of ability alone, but rather is influenced by performance-related attributes such as effort, luck, practice, and experience. A difference between two people in one or more of these performance-related attributes can produce a difference in performance, even if the abilities of the two people are identical. From an attribution standpoint, this can work to the advantage of the poorer performer. For example, imagine two tennis players who are similar in performance-related attributes. If the first player beats the second in a tennis match, we can conclude that the first player has greater tennis ability than the second. If, however, the two players are dissimilar in one or more performance-related attributes, in that the first player enjoys an advantage, then a better performance by the first player tells us little about the abilities of the two players. The superior performance of the first player may be due to greater ability, but it may also be due to an advantage on a performance-related attribute. In short, an opponent's advantage on a performance-related attribute can diminish the negative consequences of an unfavorable social comparison by discounting lack-of-ability attributions for the poor performance (Kelley, 1971).

There is some evidence that people will circumvent an anticipated unfavorable social comparison by providing a comparison target with an advantage on an actual performance-related attribute. For example, in a study by Shepperd and Arkin (1991), participants were made uncertain about their ability to outperform an opponent on a forth-

coming ability-linked test. Participants, however, were allowed to select from several varieties of music for their opponent to hear during the test period. Some of the music pieces were ostensibly performance enhancing, and some of the pieces were ostensibly performance obstructing. Participants who anticipated that their test performance would be compared directly to that of their opponent selected performance-enhancing music for their opponent to hear. By so doing, they minimized the extent to which a relatively poor performance could be attributed to personal lack of ability. The opponent's superior performance could be attributed to the advantage (performance-enhancing music) supplied by the participant.

The research just described indicates that under certain conditions people will supply a comparison target with an actual performance advantage. Our research takes the next step and examines whether people will alter their perceptions of a comparison target in advance of a performance, perceiving the target as dissimilar because of a performance-related advantage. For example, prior to an important match a tennis player might imagine that an opponent from a rival school has a better coach, has better shoes, has spent more time practicing during the previous week, or possesses some other advantage. The advantage, although perhaps more imagined than real, makes the opponent inappropriate for social comparison. Should the tennis player lose, he or she can attribute the loss to the opponent's advantage rather than to personal lack of ability.

Our research is distinct from prior research just described in that it represents an *intrapsychic* rather than a *behavioral* strategy (Baumgardner & Arkin, 1987). Rather than supplying a comparison target with a performance advantage, the person draws attention to differences in performance-related attributes or perhaps constructs illusory advantages enjoyed by the comparison target. As such, the advantage conferred on the opponent may exist entirely within the individual's own thinking. In no way, however, is the performance of the opponent actually facilitated. Because it requires no overt behavior, we suspect that the intrapsychic process of imagining or exaggerating a target's advantage on performance-related dimensions should be far more common than behavioral other-enhancement.

Altering perceptions of a comparison target in anticipation of a poor relative performance bears some resemblance to self-handicapping (Berglas & Jones, 1978; Higgins, Snyder, & Berglas, 1990). Self-handicapping is the preemptive claim or creation of a performance impediment that decreases the likelihood of success yet provides a nonability explanation for failure should it occur. Self-handicapping and ascribing advantages to a comparison target are distinct, however, in the focus of attention. With self-handicapping, the focus is on personal performance—for example, the handicapper claims or creates a personal impediment. With the behavior we are examining, the focus is on the advantages enjoyed by a comparison target. Accordingly, people

are likely to ascribe advantages to a target only in a setting in which personal success or failure on a task is defined relative to the target.

People can perceive a comparison target as dissimilar by virtue of an advantage in at least three ways. First, the perceiver can imagine or create advantages in a comparison target. The tennis player, for example, might imagine that her opponent has had more lessons, has spent more time practicing, or is better rested. Second, the perceiver can exaggerate the effect or importance of an advantage. Thus, the tennis player might believe that her opponent's new shoes or new tennis strings enhance performance more than they really do. Finally, the perceiver might believe that attributes of the opponent produce advantages when in fact they do not. For example, the tennis player might believe that her opponent's spiffy new tennis outfit gives her an advantage when the outfit in fact produces no such advantage. In each case, the ability and self-esteem implications of the anticipated poor performance are diminished because a better performance from the opponent can be attributed to the opponent's advantage rather than to personal deficiency.

Preliminary evidence that people will perceive a target as advantaged in anticipation of an unfavorable comparison is provided in a study by Bond (1979). Participants anticipating a competition on an intelligence test rated either their opponent or another participant with whom they were not competing on a variety of dimensions. Opponents were rated as quicker and as more intelligent and competitive than were nonopponents. In our study, we sought to extend the findings from the preliminary study by Bond by examining the conditions under which people will ascribe advantages to a comparison target. Similar to Bond's and other studies examining anticipatory attributional strategies (e.g., Berglas & Jones, 1978; Shepperd & Arkin, 1991), we predicted that people anticipating an unavoidable social comparison on a dimension central to identity are most likely to ascribe advantages to a comparison target when the anticipated outcome of the comparison is either uncertain or negative.

INDIVIDUAL DIFFERENCES IN SELF-ESTEEM

A second purpose of this research was to examine whether ascribing advantages to an opponent is moderated by individual differences in self-esteem. A consistent finding in the attribution literature is that people with high self-esteem tend to take personal responsibility for positive outcomes, but not for negative ones. People with low self-esteem, by contrast, are more even-handed in their attributions, taking equal responsibility for positive and negative outcomes (see Blaine & Crocker, 1993). The self-esteem difference in attribution patterns may reflect self-esteem differences in self-presentational styles (Arkin, 1981; Baumeister, Tice, & Hutton, 1989; Shepperd & Arkin, 1990), or self-esteem differences

in self-certainty (Baumgardner, 1990; Campbell, 1990; Campbell & Lavalley, 1993).

Importantly, the greater self-serving tendency of people high in self-esteem is typically observed only after they receive feedback. That is, people with high self-esteem are more likely than their low self-esteem counterparts to externalize a failure (or internalize a success) that has just occurred. People with low self-esteem, in contrast, are more self-serving prior to performance and feedback, perhaps because they are more likely to expect negative feedback (Shepperd, Ouellette, & Fernandez, 1996; Tice, 1991). The latter research is consistent with the argument offered by Blaine and Crocker (1993) that successful affect regulation on the part of people with low self-esteem involves acknowledging the possibility of future negative outcomes and preparing for them. It is also supported by research on shyness, a construct that is highly correlated with self-esteem. Specifically, before receiving feedback, shy participants are more likely than nonshy participants to dismiss the importance of a self-relevant test by derogating the test's predictive validity. After receiving feedback, nonshy participants receiving negative feedback also come to derogate the test's predictive ability (Shepperd, Arkin, & Slaughter, 1995).

In sum, it appears that people with high self-esteem are more likely than people with low self-esteem to respond defensively to negative outcomes that have already occurred. However, people with low self-esteem are more likely to prepare proactively for potentially negative feedback. Because we examined perceptions prior to an anticipated negative social comparison, we predicted that people with low self-esteem would be more likely than people with high self-esteem to perceive their opponent as advantaged.

OVERVIEW

Our research had three purposes: (a) to examine further the impact of uncertainty on the tendency to ascribe advantages to a comparison target, (b) to examine whether these perceptions are moderated by individual differences in self-esteem, and (c) to examine whether ascribing advantages to a comparison target represents people's true perceptions of the target, or merely reports designed to manipulate audience attributions. To this end, we conducted three experiments. Experiment 1 was a field study examining whether collegiate female swimmers perceive rival swimmers as enjoying nonability advantages when they are competing with the rival and anticipate that their rival may perform better in the upcoming race. Experiments 2 and 3 were conducted in the lab, and examined whether these reports are moderated by individual differences in self-esteem. In addition, Experiment 3 examines whether ascribing advantages to a comparison represents a self-presentational claim, or people's actual perceptions.

EXPERIMENT 1

Experiment 1 was a field study designed to examine whether people in a nonlaboratory setting will actually perceive advantages in an opponent, and the conditions that prompt these perceptions. Collegiate swimmers rated three swimmers from rival schools who were (a) competing with the participant and similar in speed, (b) competing with the participant but slower in speed, or (c) not competing with the participant but similar in speed. We predicted that participants would rate the rival who was competing with the participant and similar in speed as having better coaching and spending more time practicing than the other two rivals.

Method

Participants and procedures. All female collegiate swimmers ($N = 12$) at a small New England College participated voluntarily. Participants completed a three-page questionnaire immediately after swim practice, 2 days prior to the women's regional collegiate championship swim meet. Each page of the questionnaire was tailored specifically for the participant completing the questionnaire, and asked the participant to judge a rival swimmer from another school. For example, one participant read the following:

This weekend you are scheduled to swim the *200 freestyle*. Your fastest time in this event is *1:55.06*. We anticipate that _____ from another New England College will be swimming against you. Her fastest time is *1:54.95*. Answer the questions below with respect to this opponent.

The first page of the questionnaire described a real rival swimmer who was swimming the same race as the participant in the upcoming regional meet and who had actually achieved a best time that was equivalent to (i.e., equal to or a fraction of a second faster than) the participant's best time for that race. The second page of the questionnaire described a real rival swimmer who was swimming the same race but had achieved a best time that was notably slower (i.e., depending on the race, between 1.5 and 14 sec slower) than the participant's best time for the race. The third page described a real rival swimmer who was swimming a race that the participant was not swimming (e.g., butterfly instead of freestyle) but who had achieved a best time that was similar to (i.e., equal to or a fraction of a second faster than) the participant in her best time for that race. For example, one swimmer read the following: "We anticipate that _____ from another New England College will be swimming in the *200 freestyle*. Her fastest time in this race is *2:14.11*. Answer the questions below with respect to this swimmer."

In sum, participants rated a rival who was (a) competing with the participant and equivalent in speed, (b) competing

with the participant but slower in speed, or (c) not competing with the participant but equivalent in speed.

Participants rated the rival swimmer on the following items:

1. "How many hours a week do you estimate that she typically practices in the pool?"
2. "What do you think of the quality of her coach?" (1 = *poor*, 7 = *excellent*).
3. "How rested will she be for this meet?" (1 = *not rested*, 7 = *well rested*).
4. "How likely is it that she will have tapered for this meet?" (1 = *unlikely*, 7 = *likely*).
5. "How many hours a week does she typically spend in dry land training?"¹

Participants also rated how confident they were that they would beat their equally fast opponent and their slower opponent (1 = *not at all confident*, 7 = *very confident*).

Several points regarding the procedures of Experiment 1 deserve mention. First, we obtained from the swim coach a roster of swimmers from all schools competing in the meet as well as the best swim time each swimmer had achieved during the season for each event in which they had competed. This made the task of matching each swimmer to an opponent who was equivalent to or slower than the participant for a given race easy. Second, because these were collegiate swimmers we assumed that their general swimming ability would be important to their identity. However, we recognized that the swimmers might regard their ability in some races as more important to identity than their ability in other races. Thus, for the third page of the questionnaire we took great strides to select a race in which the participant was skilled, yet for which she nevertheless was not competing during the regional meet. This task was made easy by the fact that the goal of the swim coach is to win the meet rather than to win specific races. As a result, swimmers are distributed across races in a swim meet so as to maximize the points achieved for finishing among the top eight swimmers for that race. This often results in women swimming some, but not all, of their best races.

Results and Discussion

We predicted that participants would judge their rival as enjoying the greatest advantage when they were competing

¹*Tapering* refers to a reduction in the distance and time spent swimming in the days prior to a race. Tapering reduces the likelihood that the swimmer is tired from practicing when at the swim meet. *Dry land training* refers to exercise out of the water (e.g., weight training, running). Participants also rated the other swimmers in terms of being motivated. However, the item assessing motivation produced a ceiling effect. All swimmers were rated as highly motivated for the regional meet.

with the rival and the rival was equally fast. As evident in Table 1, the pattern of means generally supported the hypotheses. The predictions were tested using two planned contrasts. The first contrast compared ratings of the equally fast opponent to ratings of the slower opponent. As predicted, for three of the five variables (hours practicing, quality of coach, hours of dry land training), participants perceived the equally fast opponent as enjoying a greater advantage than the slower opponent, all $t(11) > 2.80$, $ps < .05$, $R^2 > .42$. The second contrast compared ratings of the equally fast opponent to the ratings of the equally fast nonopponent. Again as predicted, participants rated the equally fast opponent as more advantaged than the equally fast nonopponent for hours spent practicing, $t(11) = 2.69$, $p < .05$, $R^2 = .40$, quality of coach, $t(11) = 5.00$, $p < .05$, $R^2 = .69$, and hours of dry land training, $t(11) = 2.06$, $p < .07$, $R^2 = .28$. In sum, the swimmers generally perceived their rival as enjoying the greatest advantage (e.g., having a better coach, having practiced more hours during the previous week, and having spent more time in dry land training) when they expected to race against her and believed she was similar in speed. Finally, participants reported that they were more confident that they would beat their slower opponent ($M = 4.9$, $SD = 1.3$) than their equally fast opponent ($M = 6.1$, $SD = .90$), $t(11) = 4.31$, $p < .01$, $R^2 = .63$.

The finding that participants rated an opponent in the same race who was similar (as opposed to slower) in speed as having a better coach and as practicing more both in and out of the pool is not particularly surprising. One would expect that part of the reason the faster swimmer is faster is because she has better coaching and trains more. What is more interesting is how the participants rated a swimmer who was similar to themselves in ability but who was swimming a different race than the participant. If the participants had been unbiased in their judgments, they presumably would have rated the equally fast opponent and the equally fast nonopponent similarly. Yet they did not. Instead, participants rated an equally fast swimmer as having a better coach and as practicing more in and out of the pool only when she was an opponent. In sum, participants imputed advantages only to an equally capable fellow swimmer with whom they were anticipating a comparison of performances.

Of course, it is possible that participants did not overestimate the preparedness of their equally fast opponent. Instead, they underestimated the preparedness of their nonopponent. Although this interpretation of the data seems a stretch, we cannot rule it out. The interpretational uncertainty of Experiment 1 led us to conduct a follow-up study in the lab.

EXPERIMENT 2

Experiment 2 had two purposes. The first purpose was to replicate the findings of Experiment 1 in a controlled laboratory setting. The design of Experiment 2 represented a significant departure from Experiment 1. First, the task was performance on an intelligence test rather than performance in a swim meet. Second, the nature of the advantage differed. In Experiment 1, participants were given the opportunity to create or imagine an advantage in a comparison target. In Experiment 2, participants believed that their comparison target received an advantage, an opportunity to practice, that was unavailable to them. Participants then supplied ratings on two dimensions: the proportion of the practice period spent practicing, and their estimates of the effect of practice on test performance.

Although we believe that both dimensions represent ways to ascribe advantages to an opponent, they are nevertheless distinct. Reporting that a comparison target spent most or all of the practice period practicing is an overt, direct approach. It is also an approach that is objective and thus open to challenge by others. Specifically, both the experimenter and the comparison target presumably can monitor how much the coparticipant practices and how many items he or she attempts. Estimating the effect of practice on test performance is a more subtle, indirect approach. It also represents a subjective judgment that is largely unconstrained by objective standards.

A second purpose of Experiment 2 was to examine whether ascribing advantages to an opponent is moderated by individual differences in self-esteem. As noted earlier, people with low self-esteem are more likely than people with high self-esteem to prepare proactively for potentially nega-

TABLE 1
Mean Ratings of the Rival Participant

	<i>Equally Fast Opponent</i>		<i>Slower Opponent</i>		<i>Equally Fast Nonopponent</i>	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Hours practicing in pool	12.8	2.0	12.0	1.7	12.1	1.6
Quality of coach	5.3	0.6	4.5	0.5	4.4	0.7
Rested	5.2	1.8	4.8	1.4	5.2	1.6
Likelihood of tapering	5.4	1.8	5.2	1.7	5.1	1.7
Hours of dry-land training	4.2	1.7	2.8	1.3	2.9	1.3

Note. Higher numbers indicate greater advantage.

tive feedback. Thus, we predicted that people with low self-esteem would be more likely than people with high self-esteem to perceive an opponent as advantaged.

In Experiment 2, male and female participants were induced to feel uncertain that they could outperform a coparticipant on a forthcoming test of cognitive ability. In addition, all participants were led to believe that their coparticipant would have an opportunity to practice prior to the final test. Finally, participants believed either that they were taking the same test as their coparticipant, thus permitting a comparison of test scores, or that they were taking a different test than their coparticipant, permitting no comparison of scores. Participants then estimated how much of the practice period their coparticipant used practicing and what effect they believed practice would have on test performance. We predicted that participants would perceive their coparticipant as practicing more and would perceive practice as more beneficial to performance when they believed that scores would eventually be compared than when they believed that scores would not be compared. However, we predicted that these effects would be attributable primarily to the response of low self-esteem participants.

Method

Participants

Participants were 50 introductory psychology students (14 men, 36 women) run in pairs by a female experimenter. Pairs were randomly assigned to the compared or not compared condition. All participants received course credit for participation. Data from two participants were omitted from analyses because they voiced suspicion about the procedures.

Procedure

On arriving at the laboratory, participants were escorted to separate cubicles where they completed the Rosenberg Self-Esteem Inventory (Rosenberg, 1965). The cubicles permitted participants to view the experimenter, but not each other. Next, the experimenter explained that the purpose of the study was to examine the effect of practice on performance on an intelligence test. The experimenter told participants that commercial test preparation courses were often very effective in improving test scores. However, the reason for the effectiveness of these courses was unknown. Specifically, it was unknown whether people enrolled in commercial courses achieved higher test scores as a result of learning new test-taking skills or merely because they had more opportunity to practice. To examine the practice explanation, participants learned that they would take an initial baseline test, followed by a practice period, and then a final test.

The experimenter warned participants that the baseline test was difficult and that they should not expect a high score. When the experimenter was certain that participants understood the instructions, she distributed the test with instructions that participants had 10 min to complete it. The baseline test consisted of 20 items typical of those found on the scholastic aptitude test (7 sentence-completion items, 6 analogies, 6 antonyms). After 10 min had elapsed, the experimenter collected the baseline tests and informed the participants that the tests would be scored by a second experimenter. She then left the room with the tests, returning approximately 1 min later.

On her return, the experimenter described the practice period. Participants learned that they would be separated into different rooms for 20 min, with one assigned randomly to the practice condition and the other to the control condition. The participant in the practice condition would receive a list of practice items similar to those appearing on the second test. The participant in the control condition would have no opportunity to practice. Participants also learned that, although the person in the practice condition would have 20 min to practice, he or she could choose to spend all of the time or just a little bit of the time practicing.

To enhance the credibility of the cover story, the experimenter then allowed each participant a few moments to examine a folder consisting of sample items that were described as typical of the practice items received by the person assigned to the practice condition. The sample items were drawn from a GRE practice test. While participants browsed through the sample items, the experimenter excused herself to retrieve the graded practice tests. After approximately 1 min, the experimenter returned with the practice test scores in sealed envelopes. The experimenter explained that the envelopes were sealed to avoid any experimenter bias that might result from her knowing the scores.

Each envelope contained false scores for both participants. Our intention was for participants to be uncertain that they could achieve a favorable social comparison. Based on evidence from the self-handicapping literature on noncontingent feedback (Self, 1989), and our own pilot testing, we concluded that the uncertainty was best accomplished by giving participants noncontingent positive feedback (making participants uncertain that they could replicate their prior high performance on the final test), plus feedback that the coparticipant scored even better.² Thus, the score in the envelope revealed that the participant scored better than expected on the baseline test, solving 13 of 20 items correctly. This score was well above the score participants actually received on the test ($M = 8.6$, $SD =$

²Pilot testing revealed that participants who received the same score as their coparticipant on the practice test consistently believed they would perform better than their coparticipant on the final test. Only when the coparticipant performed better on the practice test were participants uncertain of who would do best on the final test.

3.0) and the score participants estimated they received on the test ($M = 8.5$, $SD = 3.5$). Their coparticipant's score, however, indicated a better performance, with 14 of 20 items solved correctly.

After examining their scores, participants were escorted to separate rooms for the practice period. The experimenter then disclosed privately and separately to each participant that he or she was in the control condition and the coparticipant was in the practice condition. All participants were reminded that their coparticipant would have 20 min to practice and that he or she could spend as little or as much of that time practicing as he or she wanted. Participants were offered several magazines to read while they waited, and then were left alone. After 20 min, the experimenter returned the participants to their cubicles.

Performance comparison manipulation. After the practice period, the experimenter introduced the performance comparison manipulation. In the *comparison* condition, participants learned that they and their coparticipant would take the same final test consisting of analogies, and that they and the experimenter would compare their scores afterwards to see who performed better and to examine the effect of the practice period. In the *no comparison* condition, participants learned that the study was examining many different types of tests. The experimenter explained that test distributions were unequal, and participants would thus be taking different tests. The experimenter disclosed that one of them would take a test consisting of analogies, whereas the other would take a test consisting of antonyms. The experimenter further explained that because they were taking different tests, no comparison of scores was possible. No participant in the no comparison condition voiced suspicion about the fact that they and their coparticipant were now taking different tests.

Next, participants completed a one-page questionnaire containing checks of the manipulation and several distractor items. This questionnaire also contained the primary dependent measures. Specifically, participants estimated the proportion of time their coparticipant spent practicing during the practice period, and the extent to which they believed that performance on standardized tests could be improved by practicing on similar items. After completing the questionnaire, participants were thoroughly debriefed using procedures recommended by Mills (1976).

Results and Discussion

Preliminary analyses comparing the responses of men to the responses of women across conditions revealed no reliable main effects or interactions involving gender of participants. Therefore, gender was excluded as a variable in subsequent analyses. Preliminary analyses also revealed no difference in self-esteem between the two conditions ($Grand M = 39.5$, SD

$= 4.6$), $t(46) = 0.65$, $p > .51$, $R^2 = .01$. Unless otherwise stated, data were analyzed using simultaneous regression procedures within a nested design, in which pairs of participants were treated as a nesting variable, performance comparison condition (compared vs. not compared) was treated as a categorical variable, and self-esteem was centered (Aiken & West, 1991) and treated as a continuous variable. The regression model contained three terms: Self-Esteem, Comparison Condition, and Self-Esteem \times Comparison Condition.

Manipulation checks. Participants responded to all manipulation check items using a 9-step scale ranging from 1 (*strongly disagree*) to 9 (*strongly agree*). Two items assessed the effectiveness of the comparison manipulation. Both yielded a single significant effect of the performance comparison manipulation. Specifically, participants in the comparison condition ($M = 8.4$, $SD = 1.0$) agreed more than participants in the no comparison condition ($M = 2.0$, $SD = 2.0$) to an item that read, "I will be taking the same final test as my coparticipant," $F(1, 23) = 185.86$, $p < .0001$, $R^2 = .80$. Moreover, participants in the comparison condition ($M = 8.0$, $SD = 1.5$) agreed more than participants in the no comparison condition ($M = 3.0$, $SD = 2.6$) to an item that read, "My test score will be compared with the score of my coparticipant," $F(1, 23) = 61.74$, $p < .0001$, $R^2 = .58$.

The comparison manipulation did not affect participants' reports of how important it was to perform well on the test, all $F_s(1, 23) > 1.74$, $p_s > .18$, $R^2 < .04$, or how competitive they felt with their coparticipant, all $F_s(1, 23) < 0.39$, all $p_s > .53$, $R^2 < .01$. These null findings are important, because they suggest that any difference in responses to the primary dependent measures is not due to variations in the importance participants attached to test performance or the extent to which participants were engaged in the task.

Two items assessed whether participants believed their coparticipant might perform better than them on the final test. The first item read, "I anticipate that I will perform well on the test I will take." The second item read, "I anticipate that my coparticipant will perform well on the final test." A mixed model regression analysis of these items, with item treated as a repeated measure, and comparison condition and self-esteem treated as between-subjects measures, revealed a single significant effect of item, $F(1, 44) = 61.56$, $p < .0001$, $R^2 = .57$. Participants were more likely to agree that their coparticipant would perform well on the final test ($M = 6.7$, $SD = 1.3$) than to agree that they would perform well ($M = 5.3$, $SD = 1.5$). Finally, analyses revealed no differences across condition or level of self-esteem in the estimated or actual score received on the test, all $F_s(1, 21) < 2.8$, all $p_s > .10$, $R^2 < .06$.

Perceiving performance advantages. We hypothesized that participants would perceive their coparticipant as

practicing more, and would perceive practice as more beneficial, when they believed that their test scores would be compared than when they believed their test scores would not be compared. Data analyses supported this prediction. Specifically, participants estimated that their coparticipant spent more time practicing in the comparison condition ($M = 79.2\%$ of the time, $SD = 18.1$) than in the no comparison condition ($M = 63.3\%$ of the time, $SD = 26.4$), $F(1, 23) = 5.40$, $p < .03$, $R^2 = .11$.³ In addition, there was a nonsignificant trend for participants in the comparison condition ($M = 7.5$, $SD = 1.2$) to be more likely than participants in the no comparison condition ($M = 6.9$, $SD = 1.1$) to report that performance could be improved by practice, $F(1, 23) = 2.85$, $p < .11$, $R^2 = .06$.

We also predicted that reporting the opponent as advantaged in the comparison condition would come primarily from low self-esteem participants. Analyses revealed only partial support for this second hypothesis. Specifically, for the more overt measure of estimates of time spent practicing, analysis revealed no main effect or interaction involving self-esteem, all $F_s(1, 21) < .08$, $p > .79$, $R^2 = .01$. However, for the more subtle measure involving judgments of the effect of practice, analysis revealed a significant interaction of comparison condition and self-esteem, $F(1, 21) = 5.47$, $p < .03$, $R^2 = .10$.

³Participants also responded to a second overt measure by supplying estimates of how many problems their coparticipant attempted. However, this item proved to be a poor dependent measure in both Experiments 2 and 3 because of tremendous within-cell variability. In both studies, all $F_s < 1.40$, all $p_s > .25$.

The regression lines corresponding to the estimated effectiveness of the practice period for the comparison and no comparison participants are plotted in Figure 1, using procedures recommended by Aiken and West (1991), for points one standard deviation above and below the mean self-esteem score ($M = 39.5$, $SD = 4.6$). As predicted, participants who anticipated a comparison of test scores were more likely to report that practice improves performance if they were low in self-esteem than if they were high in self-esteem, $t(21) = 2.27$, $\beta = -.10$, $p < .05$, $R^2 = .20$. In the no comparison condition, where the coparticipant's test performance was irrelevant to the participant's own test performance, neither high nor low self-esteem participants were particularly inclined to agree that practice was beneficial to performance, $t(21) = 1.21$, $\beta = .07$, $p > .23$, $R^2 = .06$.

Finally, the correlation between the measure of time spent practicing and reports of the effect of practice (after partialing out the effect of the manipulated variable) was quite low, $r(45) = -.001$, suggesting that these two measures were regarded as distinct by participants.

In sum, the results provide partial support for the predictions. Participants reported that their coparticipant enjoyed a greater performance advantage when they anticipated a comparison of performances than when they anticipated no such comparison. Moreover, self-esteem moderated perceptions of the opponent's advantage, but only for one of the two measures. That is, in the comparison condition, participants with low self-esteem rated practice as more beneficial than did participants with high self-esteem. This finding is consistent with prior theorizing and evidence noted earlier that people

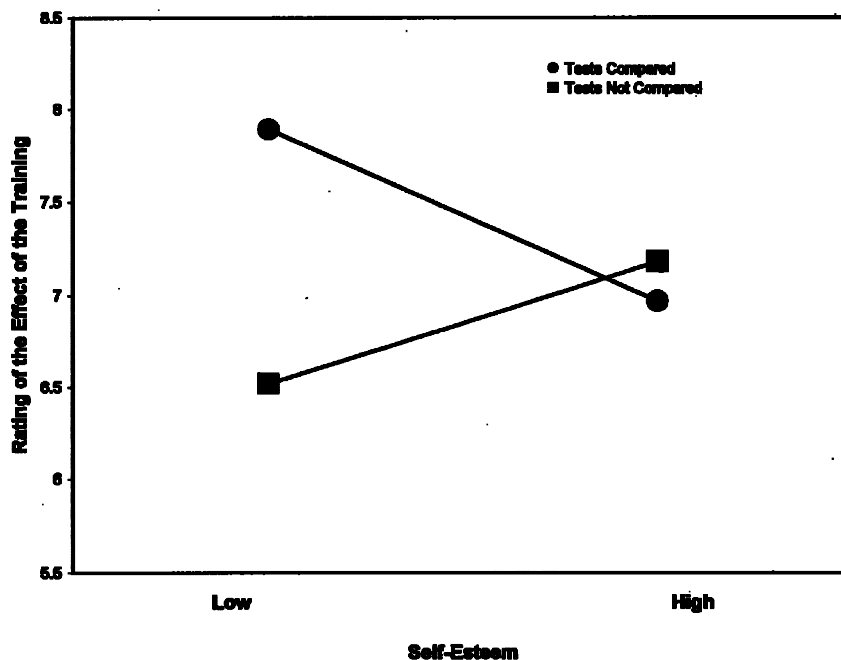


FIGURE 1 Study 2: Ratings of training effectiveness.

with low self-esteem are more likely to prepare in advance for potentially negative feedback.

It is important to note, however, that high and low self-esteem participants did not differ in their estimates of how much time their coparticipant spent practicing. The absence of a significant condition by self-esteem interaction for the time estimates was unexpected. Perhaps the manipulation of performance expectations—participants felt it was more likely that their coparticipant would perform well than they would perform well—coupled with the conspicuous nature of their coparticipant's practice advantage, made this more overt approach an obvious and attractive strategy to high and low self-esteem participants alike.

EXPERIMENT 3

Experiments 1 and 2 reveal that people will rate another person as advantaged when they anticipate a comparison with that person. Experiment 1 further reveals that the advantage is reported only when the possibility exists that the comparison target will perform better. Experiments 1 and 2 do not tell us, however, whether the advantages ascribed to the target reflect a perception—namely, people's true beliefs about the comparison target—or merely a report. That is, do participants privately believe that the target is advantaged, or are they merely reporting an advantage to manage public perceptions?

To examine this question, Experiment 3 manipulated whether the experimenter would know the outcome of the participant's test performance relative to the comparison target, and the participant's ratings of the coparticipant. If the advantages attributed to the coparticipant reflect solely an attempt to manage public perceptions, then participants will rate their coparticipant as advantaged only when they believe the experimenter will see their score and know their ratings of the coparticipant. If, however, the advantages attributed to the coparticipant reflect private perceptions, then participants will perceive the coparticipant as advantaged regardless of whether the experimenter will or will not know how they performed on the test and how they rate the comparison target.

Finally, part of the design of Experiment 3 represents a partial replication of Experiment 2, thereby permitting an examination of the reliability of the findings of Experiment 2.

Method

Participants

Participants were 132 introductory psychology students (58 men, 74 women) randomly assigned to conditions in a 2 (Performances Compared vs. Not Compared) \times 3 (Public Feedback vs. Private Feedback vs. Private No Feedback) be-

tween-subjects factorial design. Participants were run in pairs by a female experimenter, and received course credit for participation.

Procedure

The procedures of Experiment 3 were similar to those of Experiment 2, with one important exception. After participants were returned to their cubicles following the practice period, and after they learned that they would take the same test (Performances Compared) or a different test (Performances Not Compared), participants learned one of three things from the experimenter. In the *public feedback* condition, the experimenter informed participants that she would privately tell each of them how they did on the final test, and that she would provide the participants with test norms so that they could evaluate their performance relative to other students. This condition most closely resembled Experiment 2. Similar to Experiment 2, this condition was public, in that the experimenter was privy to both participants' scores. However, unlike Experiment 2, participants would not learn each other's scores. Instead, they could only learn how they performed relative to the norms. This minor difference in procedure excluded the other participant from the audience, and provided a more stringent test of the hypothesis.

In the *private feedback* condition, participants learned that they would learn individually how they did on the final test, but, for privacy reasons, the experimenter would not. In addition, participants learned that the experimenter would not see their responses on the final questionnaire, on which they supplied their estimates of how much their coparticipant practiced and what effect the practice had on test performance. Private feedback participants were told that they would receive an answer key after they completed the final test, so that they could self-score their test and learn how they did before they placed their test in the envelope and delivered it to the mail drop box. They also were told that they would receive norms so they could see how they did relative to the average student. Finally, the experimenter instructed these participants, for confidentiality reasons, not to share their scores with each other after they self-scored their tests.

In the *private no-feedback* condition, participants learned that neither they, their coparticipant, nor the experimenter would learn how they performed on the final test. Specifically, the experimenter explained that, for confidentiality reasons, she was not permitted to see participants' responses on anything they completed (including the final questionnaire on which they supplied their responses to the primary dependent measures). The experimenter then gave participants a large manila envelope with instructions to place their final test (after completion) and any questionnaires they completed inside. The manila envelope was addressed for delivery to the campus coding office. The experimenter explained that at the end of the experiment, participants were to

place the sealed envelope in the campus mail drop box located in the psychology main office. The purpose of the private no-feedback condition was to examine whether merely taking the same test as the coparticipant, even with no feedback anticipated, would lead participants to perceive their coparticipant as advantaged.

In sum, in the private no-feedback condition, neither the experimenter nor the participants would know how the participants scored on the test. In the private feedback condition, participants would learn how they individually scored on the test relative to the norms. The experimenter, however, would not see participants' scores. In the public feedback condition, participants would learn how they individually scored on the test relative to the norms. In addition, the experimenter would know how the two participants performed relative to each other.

After these instructions, which were administered after the practice period, the experimenter announced that participants would now take the final test, but first they needed to complete a brief questionnaire. The questionnaire was identical to the final questionnaire in Experiment 2, with the exception that it contained a few additional items testing the effectiveness of the privacy manipulation. After completing the questionnaire, participants were thoroughly debriefed using procedures recommended by Mills (1976).

Results and Discussion

Preliminary analyses comparing the responses of men to the responses of women across conditions revealed no main effects or interactions involving gender of participants. Therefore, gender was excluded as a variable in subsequent analyses. Preliminary analyses also revealed no difference across conditions in self-esteem (Grand $M = 40.0$, all $F_s < 2.36$, all $p_s > .14$, $R^2 < .02$). Unless otherwise indicated, all data were analyzed using simultaneous regression procedures within a nested design, in which pairs of participants were treated as the nesting variable, and performance comparison condition (Compared vs. Not Compared) and feedback (Public Feedback vs. Private Feedback vs. Private No Feedback) were treated as categorical variables. Only analyses directly testing predictions regarding self-esteem included self-esteem (after centering; Aiken & West, 1993) and the interaction of self-esteem with the manipulated variables as predictors. The analyses involving self-esteem included seven terms: Self-Esteem, Privacy, Comparison Condition, Self-Esteem \times Privacy, Self-Esteem \times Comparison Condition, Privacy \times Comparison Condition, and Self-Esteem \times Privacy \times Comparison Condition.

Manipulation checks. Participants responded to all manipulation check items using a 9-point scale, ranging from 1 (*strongly disagree*) to 9 (*strongly agree*). As with Experi-

ment 2, participants in the comparison condition ($M = 8.4$, $SD = 1.3$) agreed more than participants in the no comparison condition ($M = 1.7$, $SD = 1.7$) that they would be taking the same final test as their coparticipant, $F(1, 60) = 530.19$, $p < .0001$, $R^2 = .69$, and that their final test score would be compared with that of their coparticipant ($M = 7.8$, $SD = 1.9$ vs. 3.8 , $SD = 3.1$), $F(1, 60) = 74.60$, $p < .0001$, $R^2 = .34$. Analysis of the second item also yielded a significant effect of feedback privacy, $F(2, 60) = 4.46$, $p < .02$, $R^2 = .02$. Participants in the private feedback condition ($M = 6.4$, $SD = 3.1$) and private no-feedback condition ($M = 6.4$, $SD = 2.9$) agreed more than participants in the public condition ($M = 5.0$, $SD = 3.6$) that their test score would be compared with that of their coparticipant, both $t_s(93) > 2.57$, $p_s < .05$, $R^2 = .07$.⁴ Although unexpected, this latter effect is considerably smaller than (and in no way qualifies) the comparison condition main effect.

Consistent with Experiment 2, analysis of the two items used to assess perceptions of performance on the final test again revealed that participants were more likely to agree that their coparticipant would perform well on the test ($M = 6.5$, $SD = 1.1$) than to agree that they would perform well ($M = 5.2$, $SD = 1.5$), $F(1, 118) = 114.55$, $p < .0001$, $R^2 = .44$. Because we felt it pertinent, we also included self-esteem and the interactions of self-esteem and the manipulated variables as terms in the analysis examining responses to these items. The results revealed a significant interaction of item and self-esteem, $F(1, 118) = 22.32$, $p < .0001$, $R^2 = .09$. Examination of the slopes for each item revealed that self-esteem was unrelated to estimates of how the coparticipant would perform on the test ($\beta = -.01$), $t(128) = .55$, $p = .58$, $R^2 < .01$. However, participants with high self-esteem agreed more than participants with low self-esteem that they would perform well on the test ($\beta = .09$), $t(128) = 4.04$, $p < .0001$, $R^2 = .11$. This finding is consistent with prior evidence that people with low self-esteem have lower performance expectations than people with high self-esteem (Shepperd et al., 1996).

Three items assessed the effectiveness of the feedback privacy manipulation. All three yielded a significant effect of privacy, all $F_s(2, 60) > 10.00$, $p < .0001$, $R^2 > .07$, demonstrating that the privacy manipulation was quite successful. Specifically, private feedback condition ($M = 7.8$, $SD = 2.3$) and private no-feedback participants ($M = 7.9$, $SD = 1.8$) agreed more than public participants ($M = 5.8$, $SD = 2.9$) that questionnaire responses were confidential and would not be seen by the experimenter, both $t_s(83) > 3.90$, $p < .001$, $R^2 > .15$. In addition, public participants ($M = 7.4$, $SD = 2.5$) agreed more than private feedback ($M = 3.4$, $SD = 2.8$) and private no-feedback participants ($M = 2.7$, $SD = 2.5$) that the experimenter would learn how they scored on the final test, both $t_s(83) > 7.30$, $p < .001$, $R^2 > .39$.

⁴Here, and in other places, we dropped the nesting variable from the analysis because the nesting variable had no effect, and because dropping it simplified the statistical analyses.

Finally, public ($M = 8.7, SD = .89$) and private ($M = 8.1, SD = 1.8$) feedback participants agreed more than private no-feedback participants ($M = 1.9, SD = 2.2$) that they would learn how they performed on the final test, both $t(83) > 16.80, p < .001, R^2 = .77$. This final item also yielded an unexpected main effect of the comparison manipulation, $F(1, 60) = 4.04, p < .05, R^2 > .01$, Comparison participants ($M = 6.4, SD = 3.5$) agreed more than no comparison participants ($M = 5.9, SD = 3.6$) that they would learn their final test score. This effect was unexpected but not particularly troubling, because it raises no interpretational problems.

Analyses revealed no significant effect for participants' scores (Grand $M = 8.5, SD = 2.67$) on the baseline test, all $F_s(60) < 0.33, p > .72, R^2 < .003$. Analysis did reveal a marginally significant effect of privacy for the score participants estimated receiving (Grand $M = 8.6$) on the baseline test, $F(2, 60) = 2.47, p = .09, R^2 = .02$. However, this effect was small, and the manipulation occurred well after participants took the baseline test and made their estimates. We thus believe this marginal effect is spurious.

Similar to Experiment 2, the manipulated variables had no effect on how important participants regarded a good test performance, all $F_s > 1.52, all p_s > .22, R^2 < .02$. Of note, the feedback privacy manipulation affected how competitive participants felt with their coparticipant, $F(2, 60) = 2.97, p < .06, R^2 = .02$. Post hoc comparisons using the Bonferroni procedure revealed that private feedback participants ($M = 5.3, SD = 2.4$) felt more competitive than public feedback participants ($M = 4.1, SD = 2.3$), $t(83) = 2.47, p < .05, R^2 = .07$, but no more competitive than private no-feedback participants, $M = 4.5, SD = 2.3, t(83) = 1.57, p > .12, R^2 = .03$. This latter

finding notwithstanding, as with Experiment 2, the results of the importance and competitiveness items reveal that any difference in responses to the primary dependent measures is not due to variations in the importance participants attached to test performance or the extent to which participants were engaged in the task.

In sum, the manipulation checks revealed that the comparison and privacy manipulations, and the manipulation of participants' expectations regarding the final test, were successful. We now turn to the primary dependent measures.

Time spent practicing. The primary purpose of Experiment 3 was to determine whether rating a comparison target as particularly advantaged reflects a private perception or a public report designed to manage audience impressions. Simultaneous regression analysis of participants' practice time estimates yielded a single significant Privacy \times Performance Comparison interaction, $F(2, 60) = 3.54, p < .04, R^2 = .03$. Similar to Experiment 2, self-esteem in Experiment 3 did not influence estimates of how much time participants estimated that their coparticipant practiced, all $F_s(1, 58) < 2.15, p_s > .12, R^2 < .02$.

Figure 2 presents, by condition, participants' estimates of the proportion of time the coparticipant spent studying during the practice period. Means were compared using a series of a priori contrasts. Consistent with Experiment 2, participants in the public feedback condition estimated that their coparticipant spent more time practicing ($M = 76.1\%$ of the time, $SD = 14.7$) when performances presumably could be compared than when they could not be compared ($M =$

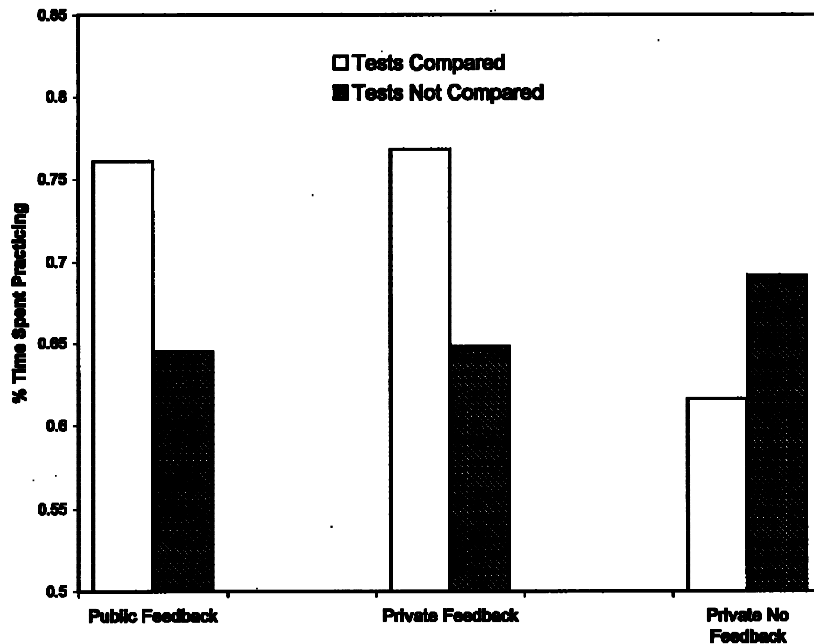


FIGURE 2 Study 3: Estimates of the amount of time the coparticipant practiced.

64.5%, $SD = 17.4$), $t(124) = 2.12$, $p < .05$, $R^2 = .03$. More importantly, participants in the private feedback condition also estimated that their coparticipant spent more time practicing when performances presumably could be compared ($M = 76.8\%$, $SD = 16.3$) than when they could not be compared ($M = 64.7\%$, $SD = 23.8$), $t(124) = 2.08$, $p < .05$, $R^2 = .03$. This latter finding reveals that participants were not ascribing greater advantages to their opponent merely to manipulate public attributions. Rather, they truly believed that their coparticipant was more advantaged in the comparison condition than in the no comparison condition. In the private no-feedback condition, participants in the comparison condition ($M = 61.6\%$, $SD = 20.5$) and the no comparison condition ($M = 69.2\%$, $SD = 15.8$) did not differ in their estimates of how much their coparticipant practiced, $t(124) = 1.39$, $p > .16$, $R^2 = .015$. Finally, participants in the public and private feedback conditions who anticipated a comparison of performances did not differ in their estimates of how much time their coparticipant spent practicing, $t(124) < .05$, $p > .90$, $R^2 = .006$. However, private feedback participants estimated that their coparticipant spent more time practicing than did private no-feedback participants, $t(124) = 2.71$, $p < .01$, $R^2 = .06$.

Similar to Experiment 1, the correlation between the measure of time spent practicing and participants' reports of the effect of practice (after partialing out the effect of the manipulated variable) was quite low, $r(127) = -.09$, $p > .32$, suggesting that these two measures were regarded as distinct by participants.

The public and private feedback conditions differed both in whether the experimenter would see the participants' scores on the final test, and in whether the experimenter would see the participants' responses to the questionnaire

items. In addition, as noted earlier, participants reported feeling more competitive in the private feedback condition than in the public feedback condition. Had the pattern of means come out differently for the primary dependent measure, these differences between the public and private feedback conditions would raise interpretational difficulties. However, participants in the public and private feedback conditions did not differ from each other in their estimates of how much time their coparticipant practiced. The only difference in practice time estimates were between the compared and not compared conditions and within the compared conditions, between the two feedback conditions and the no-feedback condition.

In sum, the results reveal that the reports of greater opponent advantages in the comparison condition do not represent an attempt to manipulate audience attributions about an anticipated poor performance relative to a comparison target. Rather, participants anticipating test feedback truly perceived their coparticipant as more advantaged when they anticipated a comparison of scores than when they anticipated no such comparison.

Effect of practice. A second purpose of Experiment 3 was to examine whether the self-esteem effect found in Experiment 2 would replicate. Analysis of the item asking about the effect of practice yielded a marginal effect of self-esteem, $F(1, 58) = 3.76$, $p < .06$, $R^2 = .03$, and a significant effect of privacy, $F(1, 60) = 3.78$, $p < .03$, $R^2 = .03$. These main effects, however, were qualified by a significant Self-Esteem \times Privacy \times Comparison Condition interaction, $F(2, 58) = 7.07$, $p < .002$, $R^2 = .05$. Further analysis revealed that the interaction was attributable entirely to the responses of participants in the

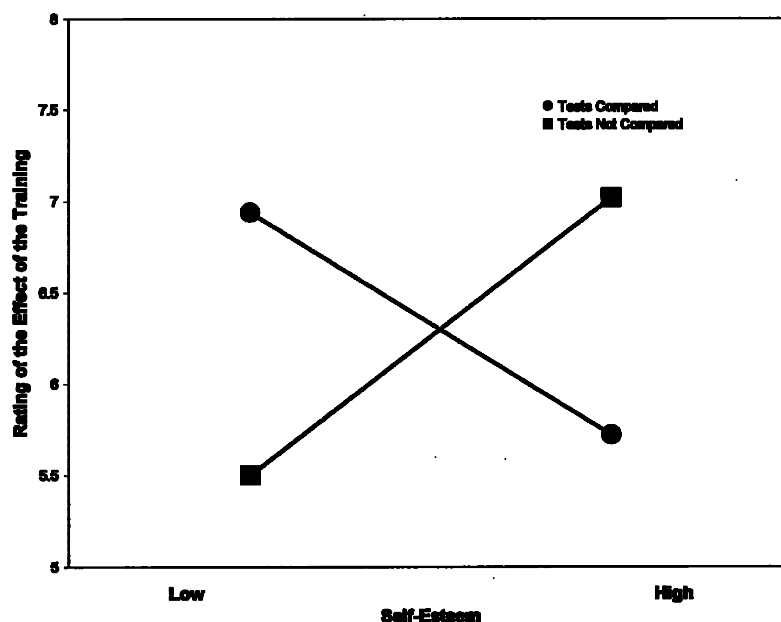


FIGURE 3 Study 3: Ratings of training effectiveness.

public feedback condition, the condition most similar to Experiment 2. Specifically, when we analyzed the data separately within the three privacy conditions, the Self-Esteem \times Comparison condition interaction was significant in the public condition, $F(1, 20) = 10.36, p < .005, R^2 = .20$, but not in either of the two private feedback conditions, both $F_s(1, 20) > 2.67, p_s > .11, R^2 < .06$.

Figure 3 presents the plot of the regression lines for the comparison and no comparison conditions for points 1 *SD* above and below the mean self-esteem score ($M = 40.0, SD = 5.7$). Once again, procedures recommended by Aiken and West (1991) were used. In the comparison condition, the slope of the regression line in Experiment 3 ($\beta = -.11$), $t(21) = 2.62, p < .05, R^2 = .25$, is virtually identical to the slope in Experiment 2 ($\beta = -.10$). The slope indicates that participants with low self-esteem in the comparison condition reported that they believed that practice could improve test performance. In the no comparison condition, the slope appears steeper in Experiment 3 ($\beta = .13$), $t(20) = 2.07, p < .06, R^2 = .17$, than in Experiment 2 ($\beta = .07$). The slope in Experiment 3 indicates that high self-esteem participants in the no comparison condition also reported that practice can improve test performance. This unexpected self-esteem effect in the no comparison condition is intriguing. However, because it did not appear in Experiment 2, we urge caution in interpreting it.

In sum, analysis of judgments of the effectiveness of practice revealed that, just as in Experiment 2, self-esteem moderated the more indirect, subtle perceptions of the performance context. Specifically, in the comparison condition, participants with low self-esteem were more likely than participants with high self-esteem to regard practice as beneficial to performance. However, the fact that the three-way interaction was due entirely to participants in the public condition indicates that responses to participants' judgments of the effectiveness of practice, unlike their judgments of the amount of time spent practicing, were sensitive to self-presentational concerns.

GENERAL DISCUSSION

The results of three separate studies reveal that people will perceive a target as enjoying performance advantages, or will exaggerate their reports of the impact of a target's existing performance advantages, if they anticipate a comparison with the target and suspect an unfavorable outcome from that comparison. Whereas some of the claims of target advantage appear to represent the comparer's true beliefs about the performance setting, Experiment 3 revealed that other claims represent a self-presentational ploy used by people with low self-esteem to manipulate audience attributions.

Regarding the more overt approach of estimating the amount of time the coparticipant spent practicing, Experiments 2 and 3 revealed that high and low self-esteem par-

ticipants alike believed that their coparticipant spent more time practicing when anticipating a comparison of performances than when anticipating no such comparison. Moreover, Experiment 3 revealed that when it came to estimates of time spent practicing, the advantage ascribed to the coparticipant did not vary as a function of the publicity of the performance or the claim. Instead, participants truly believed their reports that the target enjoyed a greater performance advantage. That is, regardless of whether they anticipated public or private performance feedback, participants anticipating feedback reported that their coparticipant spent more time practicing in the comparison condition than in the no comparison condition.

Regarding the more subtle approach of judging the effect of practice, the results were quite different. Specifically, Experiments 2 and 3 revealed that, in the comparison condition, participants with low self-esteem were more likely than participants with high self-esteem to rate practice as beneficial. By so doing, they were in a stronger position to attribute the possibly superior performance of their coparticipant to the advantage of additional practice, rather than to superior ability. In line with Blaine and Crocker (1993), we proposed that this self-esteem difference may stem from a tendency for low self-esteem participants to acknowledge more readily the possibility of a superior performance from the coparticipant, and to be more thorough in preparing for an unfavorable social comparison. Consistent with this reasoning is the finding that low self-esteem participants were more likely than high self-esteem participants to report that they would perform poorly on the final test relative to their coparticipant.

Finally, Experiment 3 revealed that the effect of self-esteem on estimates of the effectiveness of practice occurred only in the public condition. In the two private conditions, the interaction of self-esteem and the comparison condition was not significant. The implication is that this more subtle approach to ascribing advantages to an opponent represents an attempt by people with low self-esteem to manipulate the audience attributions for an anticipated poor performance relative to the comparison target.

It might be argued that the differential ratings of how much the target practiced were determined entirely by participants' perceptions of the importance of the test. That is, participants may have perceived the test as more important to identity when they were taking the same test as their coparticipants and when they anticipated test feedback, perhaps because they felt a greater sense of competition with their coparticipant. Two findings, however, argue against this alternative interpretation of the data. First, whereas analyses revealed an effect for the item measuring feelings of competitiveness, the pattern of means was inconsistent with this alternative hypothesis. Specifically, analysis revealed a main effect rather than an interaction for the competitiveness item. Moreover, the main effect that did emerge revealed that participants felt just as competitive in the private feedback condition as in the private no-feedback condition, and more

competitive in the private feedback condition than in the public feedback condition. Second, responses to the manipulation check items revealed no difference across conditions in how important participants rated the test. Thus, how competitive participants felt and how important they rated the task were unrelated to their estimates of how much time their coparticipant practiced.

We are quick to point out that the world is often not fair. Decisions about jobs, promotions, and admissions to selective graduate programs are all too often influenced by criteria other than ability and competency. As such, the perception that a comparison target enjoys an advantage may in fact be accurate. A tennis opponent from a rival school may actually have a better coach or more opportunity to practice prior to a match. Nevertheless, our findings suggest that people may occasionally see advantages in comparison targets that do not exist, or exaggerate the effect of existing advantages.

We believe that people will lean toward imagining or exaggerating advantages in another anytime they anticipate a relatively unfavorable social comparison on a dimension that is central to identity. We also suspect that this defensive process may be useful for explaining some instances of perceived favoritism, prejudice, and discrimination. For example, some preemptive cries of discrimination (or reverse discrimination) by applicants for jobs, promotions, awards, and admission to competitive graduate programs may have little grounding in reality, instead representing a defensive misperception of what criteria will be applied by evaluators making decisions about important outcomes. Thus, an applicant to a competitive graduate program may reason in advance that the playing field is biased in favor of those who enjoy an advantage due to race, age, or sex, or because they went to a prestigious undergraduate program. Such a preemptive belief can shield the applicant from the unsettling conclusion that a rejection is indicative of personal deficiencies. In such cases, the claim of discrimination may stem less from an objective appraisal of the decision criteria than from an attempt to dodge the ability and identity implications (and ultimately, their consequences for self-esteem) of an unfavorable outcome.

ACKNOWLEDGMENTS

We thank Laura Whalen for her assistance in setting up and conducting Experiment 1; Holly Siegel and Julie Katz for serving as experimenters in Experiment 2; and Laura Cunningham, Nicole Glaros, Angela Orthwein, and Ligia Ortega for serving as experimenters in Experiment 3. We also thank Marie Helweg-Larsen for her helpful comments on an earlier draft of this article.

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