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Jeer Pressure: The Behavioral Effects of Observing Ridicule of Others

Leslie M. Janes

James M. Olson

University of Western Ontario

Two experiments examined "jeer pressure," which is a hypothesized inhibiting effect of observing another person being ridiculed. Jeer pressure was expected to induce conformity to others' opinions; concern about failing or standing out; and conventional, uncreative thinking. In both experiments, participants observed videotapes containing either other-ridiculing humor, self-ridiculing humor, or nonridiculing or no humor. Participants then completed tasks that assessed conformity, fear of failure, and creativity. Results of both experiments showed that participants who viewed ridicule of others were more conforming and more afraid of failing than were those who viewed self-ridicule or no ridicule. Creativity was not influenced by the humor manipulation. Experiment 2 also included a lexical decision task to assess whether salience of potential rejection mediated the obtained behavioral effects. Salience of rejection mediated the effects of humor on fear of failure but not the effects of humor on conformity.

There is no character, howsoever good and fine, but it can be destroyed by ridicule, howsoever poor and witless.

—Mark Twain (1893)

Disparagement humor is any humor that derogates or provides negative information about someone or something. Most of such humor derogates groups (e.g., Jews, lawyers, women) or individuals based on their group membership (e.g., a Jewish man, a famous lawyer, a beauty queen), often resolving an incongruous situation via a negative stereotype (Suls, 1983). The joke form implies levity, whereas the joke content is abusive. Freud (1905/1960) theorized that our enjoyment of this type of humor derives from its camouflage for the underlying hostile content—we can vent aggressive and hostile feelings in a socially acceptable manner. More recent theorists have suggested that disparagement humor also offers an opportunity to enhance one's subjective well-

being through comparison with a less fortunate other (e.g., Wills, 1981).

Ridicule is one type of disparagement humor—that which is directed at an individual concerning some aspect of his or her behavior or appearance. Compared to other disparagement humor, ridicule is more personal and does not typically focus on group membership as the basis of the humor (Wilson, 1979). Ridicule is ubiquitous in modern North American culture, appearing in movies, advertisements, and television shows, often aimed at teenagers, who learn that it is "uncool" to wear certain clothes or display certain behaviors. Ridicule also is common in schoolyards and playgrounds—so common, in fact, that a recent survey of high school students found that fear of being ridiculed was the most common response that students gave when asked to list their principal fears (Shapiro, Baumeister, & Kessler, 1991).

Despite its prevalence in daily life, little research has examined the effects of ridicule (see Maio, Olson, & Bush, 1997; Olson, Maio, & Hobden, 1999). The aim of the present studies was to begin the investigation of how ridicule affects individuals who observe it. We assumed that ridicule could affect more than its intended target, extending to casual observers. Just as ridicule would be expected to elicit embarrassment and conformity from

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its target, we expected that observers of someone being ridiculed also would exhibit affective and behavioral consequences. Specifically, we hypothesized that seeing another person being ridiculed would have an “inhibiting” effect because it would increase the salience of interpersonal rejection and motivate actions to avoid such a possibility (e.g., conformity to norms). Given the similarity between these predicted effects and the concept of “peer pressure,” we coined the term “jeer pressure” to refer to the pressure felt by observers of ridicule to behave in conventional ways.

Past Research on the Effects of Ridicule

Some researchers have examined the use of ridicule as a behavior modifier. For example, Bryant, Brown, Parks, and Zillmann (1983; see also Bryant, Brown, & Parks, 1981) had children observe videotaped messages discouraging certain actions, where puppet models were corrected with ridicule, commands, or suggestions when they engaged in the behaviors that the experimenters wished to discourage. For 6-year-olds, but not 4-year-olds, watching a model ridiculed was more effective at inhibiting the behavior than was either of the other means of correction. The authors hypothesized that “6-year olds are apparently sufficiently socialized to recognize and appreciate the punishing power of derisive laughter. In contrast, 4-year-olds . . . may lack the experience to recognize derision for what it is” (Bryant et al., 1983, p. 252).

Other researchers have studied intentional embarrassment. Ridicule and teasing comprise the most frequent ways by which people deliberately embarrass others (Sharkey, 1997). Furthermore, intentional embarrassment is typically employed for tactical purposes (e.g., to establish or maintain power and control over the target) and is believed to be very effective at achieving these goals. In one study by Sharkey (1992), 92% of 1,040 embarrassors stated that they had achieved their goals through their use of embarrassment.

Why Is Ridicule Effective?

Ridicule is aversive for the target because it puts him or her in the position of being laughed at by others. The unpleasantness of this situation was noted by Thomas Hobbes (1651/1968): “It is no wonder therefore that men take heinously to be laughed at or derided, that is, triumphed over” (p. 103). The literature on social anxiety attests to the strength of our desire to be liked by others and our fear of being rejected (e.g., Leary, 1984).

Ridicule is also an effective strategy because it makes retaliation inappropriate for the target. If a target becomes angry, then he or she may be perceived as being unable to take a joke. The target who retaliates can be

perceived as a poor sport who is “trying to regain his dignity at the expense of the audience’s pleasure” (Wilson, 1979, p. 204).

The fact that it is often socially acceptable to ridicule people (e.g., among teenagers) is also important. Whereas a person who insults others will probably be perceived in a negative manner (e.g., as rude or hostile), a person who ridicules others may be perceived as witty and clever (Stocking & Zillmann, 1976). Because ridicule has fewer repercussions for the protagonist, it is a frequently chosen technique to control others.

The Present Research

The present studies were designed to examine jeer pressure on observers—the effects of ridicule on people other than the target. As noted earlier, we expected ridicule to have inhibitory effects on observers’ behavior. Such inhibition would, understandably, include a reluctance to engage in the specific behavior that was ridiculed. We expected, however, that jeer pressure would have broader effects (indeed, ridicule is not always directed at a particular behavior). Specifically, we thought that witnessing ridicule would lead observers to avoid engaging in any behavior that could be a target of ridicule. For example, individuals might conform closely to social norms, thereby minimizing their chances of drawing attention to themselves. Because ridicule is so frequent and reactions to it are potentially overlearned, we expected that this generalized “inhibition” would be the typical effect of observing ridicule, even when observers were unlikely to be potential targets themselves.

We should note that this postulated inhibition may not necessarily be due to conscious decision-making processes. People often cannot articulate the causes of their behavior (Nisbett & Wilson, 1977), and they might also be unwilling to admit to themselves or others that their behavior is constrained by ridicule. Nevertheless, we hypothesized that observing ridicule activates thoughts of being rejected, which then motivate careful, conforming behaviors.

We tested this hypothesis by employing dependent measures that reflected conformity, fear of failure, and creativity. We expected the observation of ridicule to increase conformity, increase fear of failure, and decrease creativity.

Conformity. Several theorists have proposed that one function of ridicule is to castigate nonconformity—to punish group members who fail to adhere to social norms (Martineau, 1972; Wilson, 1979). We hypothesized, therefore, that a behavioral effect of observing ridicule would be increased agreement with, or conformity to, other people’s judgments.

Fear of failure. Failure can provoke ridicule. We hypothesized that individuals who observed someone being ridiculed would be anxious to avoid anything that might make them a target of ridicule, including failure. That is, observers of ridicule were expected to be more afraid of failure. In the achievement motivation literature, theorists (e.g., Atkinson & Litwin, 1960; Sorrentino, Hewitt, & Raso-Knott, 1992) have documented that people who are afraid of failing tend to choose either extremely easy tasks where success is guaranteed or extremely difficult tasks where there is no shame in failure (whereas they avoid tasks of moderate difficulty). We expected, therefore, that participants who observed someone being ridiculed would prefer either very easy or very difficult versions of an achievement task, thereby minimizing either the chances or the implications of failure.

Creativity. We also hypothesized that the observation of others being ridiculed would inhibit divergent thinking, considered by many theorists to be a factor in creativity (e.g., Guilford, 1956). Thus, participants exposed to other-ridicule were expected to generate fewer and/or less creative potential uses for an object.

EXPERIMENT 1

The first experiment contrasted the effects of observing others being ridiculed with two other types of humor: self-ridicule and no-target humor. In the other-ridicule condition, participants observed a male, amateur stand-up comedian telling jokes that ridiculed another person. The jokes were directed at this unseen person's physical appearance (e.g., "His acne was so bad as a teenager we used to call him 'pizza face' "); his lack of romantic success (e.g., "He tried to join a lonely hearts club but they said, 'Hey, we're not that desperate!' "); and various other misfortunes, physical and behavioral.

A self-ridicule condition allowed us to test whether the humor would have a different effect if it was self-directed as opposed to directed at another person. The jokes used in this condition were exactly the same, and recited in the same order by the same comedian as in the other-ridicule condition, but they were directed at the comedian himself (e.g., "My acne was so bad as a teenager they used to call me 'pizza face' "). Thus, this condition provided a baseline assessment of the effects of the content of the jokes. Because self-ridicule does not represent true rejection, we expected that it would not have the inhibiting effects of other-ridicule.

A third group observed no-target humor (e.g., "What has two gills, scales, and warns us about the dangers of smoking? The Sturgeon General!") recited by the same comedian. This condition had different jokes than the experimental conditions but provided a baseline assess-

ment of the general effects of humor on the dependent variables. We expected the self-ridicule and the no-target humor conditions to provide similar patterns of responses on the dependent measures, in which case we planned to combine them to form a single comparison group that could assess more powerfully the impact of the other-ridiculing humor.

METHOD

Participants

Participants were 61 undergraduate psychology students at the University of Western Ontario who received course credit for participating. The data from 7 additional participants were dropped from the analyses because their first language was not English (6) or because they did not follow the instructions (1). The final sample consisted of 35 women and 26 men.

Procedure

Each participant was conducted individually by a female experimenter who said that the study was exploring the relationship between hand/eye coordination and humor appreciation; thus, they would assess several types of humor and perform some coordination tasks. First, participants were randomly assigned to view either the other-ridicule, self-ridicule, or no-target humor videotape, which contained the material described earlier. After this 8-minute videotape, participants rated their enjoyment (as a check to ensure that the three videotapes were approximately equally amusing).

Next, participants were told that we were interested in their reactions to another type of humor. They were given four cartoon strips to rate for funniness on a scale from 1 to 7. On each page containing a cartoon were two other ratings; participants were told that these ratings were from previous respondents and that the reason for having all participants use the same page was to prevent unnecessary paper usage. Actually, these two ratings were bogus and were selected by pilot testing to be inaccurate representations of the true humorousness of the cartoons (two funny cartoons received low ratings and two unfunny cartoons received high ratings); we were interested in whether participants would conform to these judgments.

Participants were then taken to another room to perform a ring-toss task, allegedly an assessment of their hand/eye coordination (but actually our measure of fear of failure). The test area was a grid of 15 feet (4.57 m) with a peg at one end and pieces of tape on the floor to mark 1-foot (0.31 m) intervals. As in Atkinson and Litwin (1960), participants were told the following:

You're going to play a ring-toss game. You'll have an opportunity to take 10 shots at the target from any line you wish. Please stand where you feel most comfortable. You may move after each shot or shoot from the same spot—it's up to you. I'll record your shots. We want to see how good you are at this. Please pause for a second after each shot so I can record it.

Participants were given 10 rope rings (diameter = 15 cm) to throw. The experimenter recorded where the participant stood for each throw; success (whether the ring hooked the peg) also was recorded for the purpose of supplementary analyses.

Finally, participants completed a Multiple Uses Task (Torrance, 1962) to assess creativity. A paving stone brick was placed on the table and participants were given 3 minutes to generate as many different and unusual uses for this object as possible. Participants were then thanked and debriefed. Extensive verbal assessment of suspiciousness revealed little suspicion about any aspect of the study.

Dependent Measures

Conformity. Conformity was operationally defined from the cartoon rating task in two ways. First, we counted the number of times, out of four, that the respondent exactly conformed (matched one of the bogus ratings). Second, we calculated the sum of the absolute differences between each respondent's four ratings and the bogus ratings (a match with one of the bogus ratings was scored as 0) (Campbell & Fairey, 1989). In all cases, the two bogus ratings were either the same (e.g., both 6s) or were separated by 1 (e.g., a 5 and a 6).

Fear of failure. A ring-toss task from Atkinson and Litwin (1960) assessed fear of failure. The instructions for this task were intentionally ambiguous so participants could construe the task according to their own motives. Those who stood very close to the target peg to throw were assumed to be minimizing their chances of failure, and those who stood very far away were assumed to be self-handicapping (minimizing the implications of failure) (see Berglas & Jones, 1978). Thus, both strategies were assumed to be indicative of fear of failure, whereas moderate distances were assumed to reflect low fear of failure because the test takers were realistically challenging themselves. Atkinson and Litwin (1960; see also Sorrentino et al., 1992) showed that failure-threatened individuals (low in need for achievement and high in test anxiety) made more throws at both the closest and farthest distances on this task than did success-oriented individuals (high in need for achievement and low in test anxiety).

Fear of failure was operationally defined in two ways. First, we calculated the absolute difference between

each participant's distance from the peg on the first throw and the mean of all first throws (the mean across all participants was assumed to reflect a point that balanced the relevant motives) (see Atkinson & Litwin, 1960). We thought that the first throw would reflect most purely the participant's initial motives on the task, undiluted by later factors such as success or failure on earlier throws. Second, we calculated the average (across all 10 throws) of the absolute differences between each participant's 10 distances from the peg and the mean distance of all participants across all throws.

Creativity. The scoring for the Multiple Uses Task (Torrance, 1962) is based on divergence (the unusualness of the uses generated) and fluency. These measures both reflect divergent thinking—the ability to break out of typical thinking patterns with unconventional ideas. Each participant's proposed uses for a brick were scored for originality/divergence on a scale from 1 to 5; these values were summed to produce a total originality score. In addition, the number of nonbuilding uses was simply counted to produce a fluency score.

RESULTS

Humor Assessment

A 3 (condition) \times 2 (sex) analysis of variance (ANOVA) tested whether the videotapes were equally funny. Participants rated their videotape on a scale from 1 (*not at all funny*) to 7 (*extremely funny*). As expected, ratings of the other-ridiculing ($M = 2.71$, $SD = 1.45$), self-ridiculing ($M = 2.71$, $SD = 1.23$), and no-target humor videotapes ($M = 3.21$, $SD = 1.18$) did not differ significantly ($F < 1$). Men's ($M = 3.08$, $SD = 1.20$) and women's ($M = 2.71$, $SD = 1.36$) ratings also did not differ. Overall, the humor was rated 2.87 on the 7-point scale ($SD = 1.30$), indicating that participants felt it was low to moderately funny, which is typical for laboratory studies of humor (e.g., Maio et al., 1997; Olson et al., 1999).

Tests of the Hypothesis

As expected, the self-ridiculing and no-target humor conditions yielded very similar means on all measures. Therefore, although we always provide the means for all three conditions, we tested the a priori hypothesis about the effects of other-ridicule by planned contrasts of the other-ridiculing condition versus the other two groups combined, using one-tailed tests. Table 1 presents the cell means for the principal dependent measures.¹

Conformity. When conformity was scored as the number of times out of four that the participants exactly conformed to one of the previous ratings, a significant planned contrast was obtained: The mean conformity rating in the other-ridiculing condition ($M = 1.24$) was

TABLE 1: Condition Means on Principal Dependent Measures: Experiment 1

Dependent Measure	Condition		
	Other Ridiculing	Self-Ridiculing	No-Target Humor
Conformity			
Matches*	1.24 (0.83)	0.86 (0.79)	0.90 (0.74)
Sum of deviations	5.14 (2.15)	5.86 (2.54)	6.11 (2.88)
Fear of failure			
First toss*	2.95 (2.13)	1.84 (1.87)	2.19 (1.77)
All throws*	2.55 (1.35)	1.76 (1.14)	1.90 (1.24)
Creativity			
Originality score	22.90 (4.11)	23.27 (4.26)	26.05 (4.75)
Number of uses	7.20 (2.68)	7.23 (2.87)	7.15 (3.70)

NOTE: Standard deviations are in parentheses.

*Planned comparison was significant at $p < .05$.

significantly higher than that in the combined self-ridiculing ($M = 0.86$) and no-target humor ($M = 0.90$) groups (combined $M = 0.88$), $t(60) = 1.71$, $p < .05$. When conformity was scored as the sum of the deviations from the bogus rating, the planned contrast was not reliable, although the pattern of means was again in the expected direction (less difference from the previous ratings in the other-ridiculing condition, see Table 1).

Fear of failure. We calculated each participant's deviation score for the first throw by taking the absolute difference between the participant's distance from the peg and the grand mean (the average distance of all participants on the first toss). The grand mean was 7.16 feet (2.18 m) from the peg ($SD = 3.06$ ft) on a grid of 15 feet (4.57 m). Thus, on average, participants threw from approximately the midway point on the grid. The planned contrast showed that participants in the other-ridiculing condition ($M = 2.95$ ft [0.9 m]) had significantly larger deviation scores than did those in the combined self-ridiculing ($M = 1.84$ ft [0.56 m]) and no-target humor ($M = 2.19$ ft [0.67 m]) groups (combined $M = 2.02$ ft [0.62 m]), $t(60) = 1.82$, $p < .05$.

Another analysis used each participant's average deviation score for all 10 throws; the grand mean was recalculated (7.4 ft [2.26 m]) based on all tosses by all subjects ($SD = 2.22$ ft). Groups also differed reliably in this analysis: The other-ridiculing group had larger deviations ($M = 2.55$ ft [0.78 m]) than the self-ridiculing ($M = 1.76$ ft [0.54 m]) and no-target humor ($M = 1.90$ ft [0.58 m]) groups (combined $M = 1.83$ ft [0.56 m]), $t(60) = 2.09$, $p < .03$.

The groups did not differ in accuracy (number of successful throws): other-ridiculing $M = 3.38$ ($SD = 1.94$), self-ridiculing $M = 3.52$ ($SD = 1.89$), and no-target humor $M = 3.37$ ($SD = 2.24$, $F < 1$). In addition, participants' average raw distances across the 10 throws (as opposed to

their deviations from the mean) did not differ: other-ridiculing $M = 7.78$ ft (2.37 m, $SD = 2.60$), self-ridiculing $M = 6.96$ ft (2.12 m, $SD = 1.87$), no-target humor $M = 7.46$ ft (2.27 m, $SD = 2.16$, $F < 1$). Thus, throwers in the other-ridiculing condition stood both closer and farther away from the peg (across the 10 trials) than did those in the other groups.

Creativity. Neither participants' total originality scores from the Multiple Uses Task nor their total numbers of nonbuilding uses for a paving brick revealed reliable differences between conditions (see Table 1). Thus, other-ridicule did not appear to decrease creativity on this task.

DISCUSSION

Experiment 1 provided some initial support for the primary hypothesis. Observation of other-ridicule appeared to have inhibiting effects, as indicated by the conformity and fear of failure measures. That these inhibition effects appeared after observing ridicule that was exaggerated and not personally threatening to the viewers indicates the overlearned nature of responses to ridicule; indeed, the ridicule was not even live but videotaped.

Our explanation for these findings is that observing ridicule of others increases the accessibility of possible rejection for the observer's own behavior. Presumably, this expectancy of possible disapproval (see Olson, Roese, & Zanna, 1996) manifests as conformity because people want to avoid behaving in ways that cause them to stand out, thus making them a potential target of ridicule. The fact that self-ridicule did not produce parallel effects showed that the content of the jokes, per se, was not responsible for the results. Rather, the intention of the jokes (other-derogation as opposed to self-effacement) seemed to be critical.

Several features of the first experiment argue for a stronger effect of jeer pressure than was actually obtained. First, real-life other-ridicule is often quite caustic, whereas the ridicule in this experiment was fairly innocuous; many of the jokes were too absurd to be sincere (e.g., "He was so ugly as a child that his parents had to tie a porkchop around his neck to get the dog to play with him") and, therefore, may not have been regarded as abusive. Second, there were no laughing bystanders on the videotape, whereas real-life ridicule often involves an amused audience. Third, the target of the ridicule was not present, whereas in real-life ridicule, an embarrassed target is typically visible. If other-ridiculing humor has inhibiting effects because it makes salient to observers that they also could be rejected, factors such as innocuous jokes, no audience, and an absent target might weaken these effects.

EXPERIMENT 2

Experiment 2 was designed to employ more realistic ridicule than in Experiment 1. First, the ridicule on the videotape was made less exaggerated and more caustic. Also, the target of the ridicule, as well as a laughing bystander, were present on the videotape. It was expected that observing an interaction where one person is ridiculed by another, and laughed at by a third person, would increase the salience of potential rejection in the mind of the observer.

The second experiment again involved measures of conformity, fear of failure, and creativity. The most important change was that we also measured the presumed mediating mechanism underlying the effects of ridicule: fear of rejection. Lexical decision tasks are being used increasingly by social and cognitive psychologists to test the accessibility of constructs. In such a task, respondents read a string of letters that appear on the computer screen and identify, as quickly as possible, whether these letters constitute a word or a nonword. It is assumed that words can be identified more quickly if they are related to an accessible thought. For example, Baldwin and Sinclair (1996) showed that priming participants with success or failure contexts affected the speed of recognition of acceptance and rejection target words for low self-esteem respondents but not for high self-esteem respondents. These results were hypothesized to be due to a chronically accessible schema, whereby low self-esteem individuals associate success and failure with acceptance and rejection, respectively.

A lexical decision task was included in Experiment 2. If observing ridicule of others does, in fact, increase the salience or accessibility of possible rejection, then this increased accessibility should be demonstrable by quicker recognition of words associated with rejection. Although we assume that the inhibiting effects of other-ridicule are not limited to individuals with certain personality characteristics such as low self-esteem, it seemed worthwhile to assess participants' levels of self-esteem in case this variable moderated some of the predicted effects.

METHOD

Participants

One hundred undergraduate students at the University of Western Ontario (26 men and 74 women) received course credit for participating in a study that "assessed different instructional styles." The data from four additional participants were dropped from the analyses because their first language was not English (2) or because they did not follow instructions (2).

Analyses of different measures involved slightly different numbers of participants due to data that were

dropped or missing. The ring-toss data for 7 participants were dropped because they had read about the first study (6) or had already participated in a study employing a ring-toss measure (1) and were aware that tossing rings from the midpoint would reflect favorably on them. They had no suspicions regarding the other dependent measures so we retained the rest of their data.² Word recognition data were missing for 4 participants because the experiment began before the lexical decision task equipment was fully operational.

Experimental Conditions

Other-ridiculing humor. The videotape in this condition involved three adults (two males and a female) changing a flat tire on a bicycle. One of the men performed the task (e.g., gluing on a patch) while the other man assisted him (e.g., handing him materials). The woman was an off-camera narrator who explained the steps involved in the task. The man who completed the task made a number of mistakes (e.g., pinching his finger in the pump), and the assisting man made ridiculing jokes about these mistakes (e.g., "I guess that's why they call it a foot pump") as well as other, more general jokes directed at the man completing the task (e.g., "If a loser like him can fix a tire, you can too"). The narrating woman laughed at the ridicule off-camera.

Self-ridiculing humor. The videotape in this condition contained the same scenario as the other-ridiculing condition. However, in this condition, the man who completed the task engaged in self-ridicule about his mistakes and personality. In both cases, the ridicule was exactly the same (e.g., "If a loser like me can fix a tire, you can too"), thereby controlling for the content of the jokes. As in the other-ridiculing condition, the female narrator laughed off-camera at the jokes.

No humor condition. In this condition, the videotape showed the same tire-changing task with the same individuals but without any ridicule or humor. We used a no humor rather than a no-target-humor control condition because the tire-changing task and the alleged purpose of the study (assessing instructional styles) made incidental humor seem awkward and forced.

Dependent Measures

Conformity. Respondents rated the tire-changing videotape on four dimensions: clarity, enjoyability, explicitness, and overall educational value. Participants recorded their ratings of these dimensions on four rating sheets that were posted on the wall of the laboratory room. The first 4 spaces, out of 10, were already filled out on each sheet; the participant added a fifth rating. For each dimension, the bogus ratings had two different scores, each allegedly given by two of the four previous

participants. The dimension of clarity, for example, had two bogus ratings of 1 on a scale of 1 to 7 and two ratings of 2. As in Experiment 1, the bogus ratings were selected to be inaccurate (as established by pretesting, which also showed that the three different versions of the videotapes did not differ among themselves on these dimensions).

Conformity was assessed by how closely participants' responses matched those of the alleged previous four respondents. As in Experiment 1, this was calculated (a) by counting how many times, out of four, the participant exactly conformed to either one of the bogus ratings and (b) by summing across the four items the absolute differences between each participant's ratings and the bogus ratings (a match with either of the bogus ratings was scored as 0).

Fear of failure. The same ring-toss task as in Experiment 1 was used to assess fear of failure, with the scoring methods identical to those described earlier.

Salience of rejection. The accessibility of rejection-related thoughts was measured by instructing participants to indicate, as quickly and accurately as possible, whether a string of letters displayed on a computer screen for 500 milliseconds was a word or a nonword. Participants responded by pressing the "yes" or the "no" button on a button box. There were 40 nonword trials and 40 word trials, which were randomly ordered for each participant. Ten of the word trials were rejection-oriented words (e.g., humiliated, mocked, disapproval, rejected), 10 were acceptance-oriented words (e.g., admired, valued), and 20 were neutral words (e.g., estimate, write). Nonwords were generated by changing one letter of common words (e.g., commert, invute).

Our primary interest was in respondents' speed of recognition of the rejection-oriented words. Acceptance-oriented words were included to ascertain whether both negative and positive outcomes become salient following other-ridicule. Neutral words provided a baseline estimate of participants' reaction speeds; we subtracted the mean reaction time for neutral words from the mean reaction times for rejection- and acceptance-oriented words (Fazio, 1990).

Creativity. Creativity was assessed by a Multiple Uses Task (Torrance, 1962), as in Experiment 1. A paper clip was the target object, however, instead of a paving stone brick.

Procedure

Participants were conducted individually by a female experimenter who explained that the study was testing the effect of different instructional styles on memory for the material. They learned that they would watch one of

several educational videotapes, each teaching the same lesson but with a different instructional style.

Participants first completed Rosenberg's (1965) 10-item self-esteem scale and then watched an 8-minute videotape demonstrating how to change a flat tire on a bicycle—either the other-ridiculing, self-ridiculing, or no humor tape. After the video, participants completed a 7-item memory questionnaire, which tested recall for the information presented (these items maintained the cover story and also ensured that the three videos were equally comprehensible).

After the memory questions, participants rated the videotape on four dimensions (e.g., clarity), each on a separate sheet taped to the wall of the laboratory. Each sheet contained 10 rating scales, and the first 4 scales were already filled out, ostensibly by previous participants. The experimenter said that participants in the same condition were using the same rating sheets so that there were not "hundreds of sheets of paper, with only one rating on each." Participants were asked to fill out each rating sheet in the fifth spot.

After these ratings, participants were informed that the first experiment was completed. They were asked if they would be willing to complete some unrelated tasks because the first study was so brief (approximately 20 min); all agreed to continue. The purpose of these tasks, allegedly, was to determine the role that hand/eye coordination plays in speed of word recognition. Participants were asked to perform the ring-toss task and the word recognition task. The order of these two tasks was counterbalanced across participants in case one task influenced the other or the effects of other-ridiculing humor diminished over time.

The ring-toss task was the same as in Experiment 1. The lexical decision task was completed in a separate room containing a computer. Each participant was briefed on how to perform the task and then read explicit instructions displayed on the screen. To familiarize participants with the task, they first completed 10 practice trials where they were shown a letter string and had to decide if it was a word or nonword. After these practice trials, participants completed the 80 critical trials. This task took approximately 10 minutes to complete.

Participants then returned to the original testing room and completed a Multiple Uses Task, generating as many uses for a paper clip as they could in 3 minutes. Participants then rated how funny they found the videotape; we expected that the two videotapes involving ridicule (other- and self-ridicule) would both be rated as funnier than the no humor videotape. Finally, participants were debriefed and thanked. As in Experiment 1, verbal probing revealed no suspicion about the purpose of the study other than the 7 participants noted in Note 2.

RESULTS

Humor Assessment

Participants rated the videotapes on a scale from 1 (*not at all funny*) to 7 (*extremely funny*). A Condition \times Sex ANOVA indicated that men ($M = 2.70, SD = 1.54$) and women ($M = 2.77, SD = 1.58$) rated the videotapes equivalently ($F < 1$), whereas the three conditions received different ratings: other-ridiculing $M = 2.67 (SD = 1.51)$, self-ridiculing $M = 3.72 (SD = 1.51)$, and no humor $M = 1.97 (SD = 1.18)$, $F(2, 98) = 13.23, p < .001$. Participants in the no humor condition found the videotape less funny than did those in the combined other-ridiculing and self-ridiculing conditions, $t(98) = 4.21, p < .001$. Unexpectedly, the two ridiculing groups also differed significantly, $t(63) = 3.02, p < .01$, with participants in the self-ridiculing condition rating the videotape as more amusing than those in the other-ridiculing condition. To ensure that these differences between conditions in funniness did not account for any significant findings on the principal dependent measures, all tests of the hypothesis reported in the next section also were conducted with funniness ratings treated as a covariate. All significant effects remained significant when funniness ratings were held constant statistically.

Analysis of scores on the 7-item memory questionnaire revealed no differences between groups: Participants in the other-ridiculing ($M = 5.94, SD = 1.09$), self-ridiculing ($M = 5.93, SD = 1.25$), and no humor conditions ($M = 5.86, SD = 1.12$) did equally well ($F < 1$). Thus, participants appeared to pay equal attention to the various videotapes.

Tests of the Hypothesis

As in Experiment 1, the means for the self-ridiculing and no humor conditions were very similar on all measures; therefore, the hypothesis that other-ridicule is inhibiting was tested by planned contrasts of the other-ridiculing condition versus the other two groups combined. Table 2 presents the cell means on each principal dependent measure except the lexical decision task. Preliminary analyses showed that the personality variable of self-esteem had no significant main or interaction effects on any measure. Therefore, it will not be discussed further.³

Conformity. When conformity was defined as the number of times out of four that participants' ratings matched one of the bogus ratings, the planned comparison was significant: Those in the other-ridiculing condition conformed significantly more often ($M = 1.56$) than did those in the self-ridiculing ($M = 1.09$) or no humor ($M = 1.26$) groups (combined $M = 1.18$), $t(98) = 2.07, p < .03$. When conformity was defined as the sum of the

TABLE 2: Condition Means on Principal Dependent Measures: Experiment 2

Dependent Measure	Condition		
	Other Ridiculing	Self-Ridiculing	No Humor
Conformity			
Matches*	1.56 (1.29)	1.09 (1.35)	1.26 (1.36)
Sum of deviations	5.25 (3.03)	6.06 (2.12)	5.80 (3.38)
Fear of failure			
First toss*	2.26 (1.29)	1.60 (1.35)	1.88 (1.36)
All throws	1.96 (1.20)	1.77 (0.91)	1.56 (1.19)
Creativity			
Originality score	25.30 (4.04)	24.97 (4.11)	26.36 (4.53)
Number of uses	8.55 (3.28)	8.22 (3.30)	8.44 (3.08)

NOTE: Standard deviations are in parentheses.

*Planned comparison was significant at $p < .05$.

absolute deviations from the bogus ratings, the planned comparison was not reliable.

Fear of failure. Our principal measure of fear of failure was the deviation score for participants' first ring toss. The grand mean (average distance of first tosses for all participants) was 6.91 feet (2.11 m, $SD = 2.35$). The planned contrast of the other-ridiculing condition ($M = 2.26$ ft [0.69 m]) with the combined self-ridiculing ($M = 1.60$ ft [0.49 m]) and no humor ($M = 1.88$ ft [0.57 m]) conditions (combined $M = 1.74$ ft [0.53 m]) was significant, $t(96) = 1.79, p < .05$, reflecting larger deviations (more fear of failure) in the other-ridiculing condition.

A second analysis used average deviation scores across all 10 throws based on an overall grand mean of 6.49 feet (1.98 m, $SD = 2.03$). In this case, the planned comparison was not reliable. When only the ring-toss data that preceded the lexical decision task (the order of the two tasks was counterbalanced) were included, however, the planned comparison across all 10 throws was reliable, with the other-ridiculing group ($M = 2.19$ ft [0.67 m]) showing larger deviations than the combined self-ridiculing ($M = 1.55$ ft [0.47 m]) and no humor ($M = 1.43$ ft [0.44 m]) groups (combined $M = 1.49$ ft [0.45 m]), $t(48) = 2.23, p < .02$. To the extent that the ring-toss data from these participants were purer in that they were not contaminated by the lexical decision task, it makes sense that this analysis yielded better support for our predictions.

As in Experiment 1, an analysis of accuracy (number of successful throws) revealed no differences between the other-ridiculing ($M = 4.65, SD = 2.30$), self-ridiculing ($M = 3.80, SD = 2.09$), and no humor ($M = 3.70, SD = 2.21$) conditions, $F(2, 88) = 1.77, ns$. Participants' average raw distances from the peg also did not differ: other-ridiculing $M = 5.96$ feet (1.82 m, $SD = 2.22$), self-ridiculing $M = 6.85$ feet (2.09 m, $SD = 1.90$), and no humor $M = 6.68$ feet (2.04 m, $SD = 1.89$), $F(2, 90) = 1.67, ns$.

Lexical decision task. Each participant's mean reaction times were calculated for rejection-related, acceptance-related, and neutral target words. Table 3 presents the condition means. To control for individual differences in baseline reaction times, we subtracted neutral word recognition times from rejection-related word recognition times. The planned contrast of the other-ridiculing condition versus the other two groups on reaction times for rejection-related minus neutral words was significant: other-ridiculing $M = .05$, self-ridiculing $M = .08$, no humor $M = .07$ (combined $M = .075$), $t(85) = 2.03$, $p < .03$. Thus, compared to the other conditions, participants in the other-ridiculing condition were significantly faster in recognizing rejection-related words. A similar contrast for acceptance-related words was not significant.

Creativity. As in Experiment 1, neither measure of creativity (originality ratings of proposed uses for a paper clip; total number of nonclip uses) revealed reliable differences between conditions (see Table 2).

Mediation Analyses

We hypothesized that the effects of observing ridicule were due to heightened salience of rejection. Therefore, we conducted the mediation analyses recommended by Baron and Kenny (1986) to test whether participants' speed of recognition of rejection-related words minus their speed of recognition of neutral words (i.e., our measure of the salience of rejection) mediated the impact of other-ridicule on the two dependent measures that yielded reliable effects (the number-of-matches conformity measure and the first-toss fear of failure measure).

Conformity. For the conformity measure, it turned out that salience of rejection did not correlate with the number-of-matches conformity measure, $r(86) = .07$, which is a necessary condition for mediation.

Fear of failure. In contrast, participants' first-toss deviations correlated significantly with the measure of salience of rejection, $r(87) = -.25$, $p < .02$. Thus, we tested whether the independent variable (humor condition) was still a significant predictor of the deviations measure when the salience of rejection was held constant statistically. As expected, the planned contrast of the other-ridiculing versus the remaining two conditions fell almost to 0 when salience of rejection was covaried out ($t < 1$). Finally, because the deviation and salience of rejection measures were correlated, we also tested the (unlikely) possibility that fear of failure mediated the effect of the independent variable on the salience of rejection (i.e., that the humor manipulation affected fear of failure, which then affected salience of rejection). A planned contrast, however, showed that even when

TABLE 3: Condition Mean Reaction Times (in seconds) for Rejection-Related, Acceptance-Related, and Neutral Target Words: Experiment 2

Word Category	Condition		
	Other Ridiculing	Self-Ridiculing	No Humor
Rejection-related	0.60 (0.10)	0.63 (0.15)	0.63 (0.11)
Acceptance-related	0.57 (0.09)	0.59 (0.14)	0.59 (0.12)
Neutral	0.55 (0.09)	0.55 (0.12)	0.56 (0.10)

NOTE: Standard deviations are in parentheses.

fear of failure was held constant statistically, the other-ridiculing condition exhibited greater salience of rejection than did the combined self-ridiculing and no humor conditions, $t(84) = 2.03$, $p < .03$.

DISCUSSION

Using different stimuli to portray other-ridicule and self-ridicule, Experiment 2 replicated almost exactly the findings of Experiment 1. Observation of other-ridicule again resulted in significantly more conformity and fear of failure than did observation of self-ridicule or no humor. Experiment 2 also provided two pieces of evidence that the salience of potential rejection played a role in these effects. First, on the lexical decision task, participants in the other-ridiculing condition recognized rejection-related words more quickly than did participants in the self-ridiculing and no humor conditions. Thus, other-ridicule appeared to increase the accessibility of rejection. More significantly, mediation analyses indicated that salience of rejection mediated the impact of ridicule on participants' ring-toss behavior, although it did not appear to mediate the conformity result. The mediation data provide interesting evidence for the hypothesized role of the accessibility of rejection in the effects of other-ridicule on fear of failure. That is, heightened accessibility of rejection appeared to be the psychological mechanism that was influenced by other-ridicule and that, in turn, influenced fear of failure.

Why did salience of rejection mediate the findings on the ring-toss task but not on the conformity measure? The ring toss was probably the most threatening of our tasks in that "failure" was relatively clear and unambiguous. Consequently, the accessibility of possible rejection (as a result of the humor stimuli) affected participants' behavior on this task. In contrast, conformity might have reflected a wish not to stand out rather than fear of rejection per se. Thus, perhaps exposure to other-ridiculing humor increased both the accessibility of failure (producing the effect on the ring-toss task) and the desire for deindividuation (producing the effect on the conformity task).

GENERAL DISCUSSION

Our goal in these experiments was to test the hypothesis that observing someone being ridiculed has an inhibiting effect on the observer. This phenomenon of jeer pressure was expected to involve conformity, fear of failure, and stifled creativity. In both studies, each with a different manipulation of ridicule, participants in the other-ridiculing condition were significantly more conforming and more afraid of failing (as indicated by standing either very close to or very far from the peg in the ring-toss task) than were participants in the self-ridiculing and no-ridicule conditions. Thus, other-ridicule increased participants' behavioral manifestations of concern about rejection (assuming that conformity and avoiding failure reduce one's chances of being rejected). The role of concern about rejection was directly supported in Experiment 2, where other-ridicule increased the speed with which rejection-related words were recognized; thus, observation of ridicule made participants more perceptually vigilant for rejection-related stimuli. Moreover, the salience of rejection (as indicated by reaction times) mediated the impact of the ridicule manipulation on fear of failure.

The number-of-matches measure of conformity yielded stronger results in both studies than did the sum-of-deviations measure of conformity. Perhaps participants felt that by exactly matching just one or two of the bogus ratings, they could safely diverge from the other ratings, which rendered the sum measure less sensitive. On the ring-toss task, the first-throw measure yielded stronger results in both studies than did the all-throws measure. The first throw presumably constituted the purest assessment of participants' motives because subsequent throws were potentially influenced by preceding successes or failures.

Neither study found that other-ridicule had any effect on participants' creativity. It is possible, however, that fear of ridicule inhibits people from publicly expressing creative ideas, as opposed to generating them privately. If participants had been asked to contribute ideas in a group setting, perhaps those who had observed other-ridicule would have been more reluctant to volunteer highly divergent or unusual ideas. It is also possible that the null results for creativity reflected, at least in part, the fact that this measure was always the last one obtained from participants, by which time the effects of other-ridicule may have dissipated.

The fact that self-ridicule did not have any inhibiting effects in either study suggests that the intention of the jokes to belittle someone was a critical feature of the other-ridiculing humor. The self-ridiculing stimuli consisted of the same joke content as the other-ridiculing

stimuli but without genuine rejection. It seems that self-ridiculing humor was perceived as good-natured.

An important caveat to our conclusions about other-ridicule should be noted: Our findings did not document unequivocally that humor, per se, played a necessary role in the effects of other-ridicule. Because we did not include an insult condition involving derogation without any humor, we cannot disentangle the effects of disparagement versus humor in other-ridicule. For example, we cannot discount the possibility that similar results would have been obtained if the speaker had simply reviled the target. Of course, given that ridicule is more socially acceptable than insult, it is important to document ridicule's effects even if they parallel those of insults. Indeed, we would not be surprised if observing someone being insulted also produced conforming, conventional behavior, for the same reasons that other-ridicule is inhibiting. The important point is that even when it is softened or rendered more palatable by levity, the disguised derogation involved in other-ridicule affects observers.

We should also acknowledge that our reported tests of the hypothesis consisted of comparisons between the other-ridiculing condition and a pooled comparison group—the self-ridiculing condition and either the no-target humor (Experiment 1) or no humor condition (Experiment 2). For exploratory purposes, pairwise comparisons also were conducted. The other-ridicule versus self-ridicule comparisons were almost always statistically significant, whereas the other-ridicule versus no-target/no humor comparisons tended not to be significant. Thus, the added power provided by the combined comparison group strengthened our tests of the hypothesis. Although it would have been nice if pairwise comparisons had uniformly supported our predictions, the pooled comparisons are themselves noteworthy given the artificial laboratory settings in which our studies were conducted.

In this context, it is important to note that participants in the current experiments were not potential targets of ridicule themselves, yet exposure to other-ridicule had inhibiting effects nonetheless. The videotaped comedian or videotaped assistant could not ridicule participants next. In real-life ridicule, observers are often potential targets of ridicule from the instigator, which, if anything, should increase the inhibiting effects beyond those exhibited in our research. Thus, real-life ridicule may be more powerful than suggested by the current studies.

Future research should examine limiting conditions to the effects of other-ridicule. For example, attributions about the intent of the source may be important; friendly kidding might have less impact than deliberate mocking

designed to hurt another's feelings. Whether the ridicule focuses on objectively true characteristics of the target (e.g., clothing) or constitutes obvious exaggeration (e.g., failure to meet impossible standards) also may be important.

It is also likely that exposure to other-ridicule sometimes produces positive affective reactions to the target, such as sympathy, and/or negative affect toward the instigator. Indeed, behavior toward either the target or the source could be influenced. The conditions that facilitate these possible reactions to other-ridicule need to be articulated in future research.

Conclusions

Jeer pressure is alive and well. Our findings show that exposure to someone being ridiculed can have inhibiting effects on college students, even when the ridicule is presented on videotape. Other-ridicule appears to make observers aware of their own vulnerability to ridicule and rejection. This enhanced awareness can limit and constrain subsequent behavior.

Of course, the inhibition produced by ridicule may not always be a negative thing. For example, ridicule is sometimes used to minimize nonconformity to institutionalized social norms (Wilson, 1979), such as in the armed forces to create obedience in the training of soldiers. Unfortunately, the ridicule that occurs in real life is often not directed at negative behaviors. On the contrary, ridicule often is directed at socially positive behaviors, such as wearing a bicycle helmet or refusing to smoke a cigarette. Among adolescents, particularly, ridicule often is reserved for those who act in a responsible manner. Our data clearly show that even the observation of others being ridiculed can create conformity and fear of failure. Arming young people with the strength to resist jeer pressure is a worthy social goal.

NOTES

1. Preliminary analyses showed that there were no reliable sex differences (or interactions of sex and condition) on any of the measures.

2. Prior to running Experiment 2, the authors were interviewed about Experiment 1 by a media liaison person from the university. A story about Experiment 1 appeared in a campus newspaper just after Experiment 2 began. The only thing recalled by the participants who had read of the first study was that they should stand at the midpoint of the grid when completing the ring-toss task. Thus, the analyses reported in the results section excluded these participants for the ring-toss task but included them for the other measures. Nevertheless, for exploratory purposes, all tests of the hypothesis reported in the Results section were calculated with these 7 participants excluded, and the results were identical.

3. Preliminary analyses also showed that participants' sex did not interact with the manipulation on any of the measures. Two main effects for sex were revealed. On the sum-of-deviations conformity measure, men ($M = 7.31$) conformed less than women ($M = 5.14$), $F(1, 93) = 12.20$, $p < .001$. On the ring-toss measure across all 10 trials, men ($M = 2.46$ ft [0.75 m]) deviated more from the grand mean than did women ($M = 1.52$ ft [0.46 m]), $F(1, 87) = 15.18$, $p < .001$. Because nei-

ther of these sex differences qualified the predicted findings in any way, this factor will not be discussed further.

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