# The Moderating Effect of Trivial Triggering Provocation on Displaced Aggression

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Two studies examined the interaction between the presence or absence of (a) an initial provocation and (b) a subsequent minor triggering action on the part of the target of displaced aggression. Consistent with the triggering event being seen by participants as indeed trivial when administered by itself without prior provocation, exposure to it literally had no impact on aggression toward its source. When previously provoked, however, this subsequent triggering event strongly increased displaced aggression, causing it to reliably exceed both that displayed when there was no antecedent provocation and that elicited by provocation alone. Mediation analyses showed that for participants who had been provoked, subjective feelings of displeasure concerning the triggering event mediated the effect of the trigger on aggression.

Displaced aggression is thought to occur when a person who is initially provoked cannot retaliate directly against the source of that provocation and, instead, subsequently aggresses against a seemingly innocent target (e.g., Mosher & Proenza, 1968; S. Worchel, Hardy, & Hurley, 1976). Thus, in terms of the matching rule (Axelrod, 1984), it reflects a level of aggression that incommensurably exceeds the level ordinarily warranted by the behavior of its target.

Dollard, Doob, Miller, Mowrer, and Sears (1939) identified two sources of constraint against direct, retaliatory aggression. First, the source of a provocation sometimes is unavailable, as when the provocateur has left the immediate situation, or alternatively, when the provocation is produced by an intangible, nonhuman source. Bad weather, heat, or a foul odor are examples of the latter type of provocation (e.g., Konecni & Doob, 1972; Rotton, Barry, Frey, & Soler, 1978). Second, an actor may fear further retaliation from the provocateur were he or she to emit a direct retaliatory aggressive response. When a provocateur has greater power, such as a boss in an employment setting, such fear may preclude well-justified retaliation (e.g., Bandura, 1973; R. A. Baron, 1971; Taylor, Schmutte, & Leonard, 1977). Under these types of circumstances, aggression is sometimes redirected toward displacement targets (Dollard et al., 1939).

The notion of *displaced aggression* attained prominence with the publications of the related monographs by Dollard and colleagues (Dollard et al., 1939) and by Hovland and Sears (1940) some 60 years ago. Although it may be important to our understanding of human aggression, as well as being socially troublesome, nevertheless, contemporary social psychology researchers only occasionally appear to evidence any continued interest in the concept (e.g., Hepworth & West, 1988; Mullen, 1986). We cannot identify any experimental studies on displaced aggression published within the last decade. And taking Geen's (1990) recent mainstream textbook on aggression as an example, he devotes 40 words to displaced aggression, solely defining it in accord with the frustration-aggression theorists (Dollard et al., 1939) and not referencing it in the subject index.

As discussed in Marcus-Newhall, Pedersen, Carlson, and Miller (2000), when we inspected the contents of a convenience sample of 17 social psychology textbooks published since 1985, most seemingly viewed displaced aggression as a conceptually obsolete phenomenon, and those that did discuss it typically emphasized its controversial empirical status. This impression led us to undertake a broader historical examination of a convenience sample of 122 social psychology textbooks. We tallied the number of sentences in each text devoted to the topic of displaced aggression. To test the impression that Dollard et al. (1939) stimulated a brief interest in the concept that persisted for only a short period after its publication, we divided the textbook data into three groups (viz., 1900-1939, 1940-1945, and 1946-present). In response to the unequal variances among groups, we applied Welch's method (Wilcox, 1996). Post hoc analysis of the obtained difference among the three groups (p < .001) showed that for the interval of 1940–1945 there was more extensive discussion of the topic by comparison with other periods (i.e., a mean of 22.0 sentences for the 1940-1945 interval compared with 0 and 4.9 for the 1900-1939 and the 1946-present intervals, respectively). This suggests that there was a flurry of activity and interest in displaced aggression following the publication of Dollard et al. (1939) that, shortly thereafter, died off. Replication of this analysis with more discrete subdivisions of the modern era (viz., 1900-1939, 1940-1945, 1946-1963, 1964-1981, and 1982-present) yielded identical results. Again, post hoc tests showed that coverage in the 1940-1945 interval exceeded that of the other intervals, with no other differences among periods. (See Appendix A of Ensari & Miller, 1998, for texts sampled and the number of sentences devoted to the topic in each text.) We interpret these results as indicating that contemporary social psychology researchers view the concept of displaced aggression as

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obsolete, and do so at least in part because they view its conceptual validity as suspect.

In striking contrast to this picture presented by contemporary textbooks, meta-analytic examination of the experimental research on displaced aggression (Marcus-Newhall et al., 2000) yields a very different conclusion. The obtained mean effect size of +.55 (95% confidence interval = +.48/+.62; fail-safe n > 4,000) shows that it is both a highly reliable phenomenon and one of moderate magnitude (Cohen, 1977). In addition, theoretically relevant moderator variables, such as the intensity of initial provocation, the similarity between the provocateur and target, and the negativity of the setting in which the participant and the target of displaced aggression interacted, altered its magnitude of effect in directions consistent with theoretical expectation.

### Triggered Displaced Aggression

Perhaps even more important than the notion of displaced aggression, however, is the relatively unexplored concept of *triggered* displaced aggression (Dollard, 1938). Its defining feature is the provision of a second provocation, a *triggering event*, by the target of displaced aggression. Triggered displaced aggression manifestly appears to have greater ecological validity compared with nontriggered displaced aggression (Miller & Marcus-Newhall, 1997). When displaced aggression seemingly is observed in everyday life, it typically appears to have occurred because its target has done something (however trivial) that, from the actor's point of view, justifies the observed aggressive reaction. In summary, a trigger has the following characteristics: (a) it is emitted by the eventual target of aggression (not the original provocateur), (b) it is by itself a provocation, and (c) it has differential effects depending on its intensity.

The interesting feature of triggered displaced aggression, from both a theoretical as well as an applied perspective, is that it can produce an interactive effect in which the magnitude of aggressive responding exceeds the sum of the independent or unique effects of both the initial provocation and the subsequent triggering action from the potential target of displaced aggression (Miller & Marcus-Newhall, 1997). That is, after an initial provocation in a setting wherein retaliation is precluded, the aggressive response to a subsequent low-intensity triggering provocation can exceed that which is implicit in the additive combination of the effects of the initial provocation and the triggering event.

There are several reasons for this latter expectation. Lowintensity triggers are more ambiguous with respect to whether they constitute a provocation. Consequently, priming effects from the initial provocation can cause such ambiguous stimuli to be more readily noticed (e.g., Higgins & King, 1981). In addition, such prior priming is likely to have attributional consequences, leading to an interpretation of the minor triggering event as having been intentionally provoking (Duncan, 1976). Thus, when preceded at an earlier time by a strong provocation that precluded retaliatory action, minor negative experiences, such as another person's irritation, a somewhat disapproving comment, or a bump from another person on a crowded bus as it rounds a turn, can elicit an aggressive response that in its intensity and destructiveness is strikingly incommensurate with the level of this second minor provocation. That is, there is a violation of Axelrod's (1984) matching rule, in that the magnitude of response to the minor

triggering event far exceeds the ordinary magnitude of retaliatory aggressive escalation.

In contrast, strong triggering events are unlikely to yield this interesting interactive effect. They are likely to be readily noted and accurately seen as provoking irrespective of the presence or absence of prior priming by an antecedent provocation. Likewise, there is less likelihood of differential attributional distortion of the intentionality of the triggering event as a consequence of the presence or absence of a prior provocation.

As argued above, to increase the likelihood of an aggressive response that exceeds the additive effect of both the initial provocation and the subsequent trigger, the triggering event must be of low intensity. First, this necessitates that the trigger should be of lesser magnitude than the initial provocation. Second, because of its trivial nature, a trigger in the absence of prior provocation would not be expected to produce an increase in aggressive responding.

In the experimental literature on displaced aggression, merely a handful of studies examine triggered displaced aggression. Of the four we could locate, only two orthogonally manipulated both an initial provocation and a subsequent triggering event (R. A. Baron & Bell, 1975; P. Worchel, 1966). Although both Carver and Glass (1978) and Geen and Berkowitz (1967) manipulated initial provocation (by asking participants to complete an unsolvable puzzle), their designs excluded a trigger-only cell. Hence, they precluded comparisons that separately assess the impact of either the provocation or the trigger alone and thereby permit clear interpretation of their combined effect.

In addition, however, in all four of these studies the provocation intensity of the triggering event was essentially equivalent to, if not stronger than, that of the initial provocation. For example, in P. Worchel (1966), the initial provocation was the announcement of a pop quiz, whereas the triggering second provocation consisted of interrupting and insulting participants and thereby denving them an adequate amount of time to complete a test, which ostensibly predicted their likelihood of academic success. Thus, although the initial provocation undoubtedly aroused anxiety, the triggering second provocation appears to have been of even greater intensity, containing anger-arousing insult, eliciting frustration, and, in its interference with participants' performance on a measure predicting their attainment of an important step toward career goals, producing ego threat. Examination of the procedures of the other studies discussed above (i.e., R. A. Baron & Bell, 1975; Carver & Glass, 1978; Geen & Berkowitz, 1967) yields a similar conclusion. As previously argued, under conditions in which the triggering event equals or exceeds the provocation intensity of the initial provocation, the theoretically interesting interactive effect discussed above will not occur. And in confirmation of this expectation, additive effects were obtained both in P. Worchel (1966) and in R. A. Baron and Bell (1975).

In sum, to assess whether the combination of provocation and a subsequent triggering event can synergistically increase displaced aggression, rather than merely produce an additive effect that combines displaced aggression from the initial provocation with the direct retaliatory aggression elicited by the triggering provocation, the ideal study will include two key features. In addition to containing a triggering second provocation that is manifestly trivial in both its intensity and capacity to elicit an aggressive response, the study must also have the appropriate comparison conditions within a single experimental design. Specifically, it must fully cross the presence or absence of an initial provocation with the presence or absence of a subsequent triggering event. This allows assessment of the separate effects of both provocation and trigger by themselves, as well as their interaction. As previously indicated, although only P. Worchel (1966) and R. A. Baron and Bell (1975) included all four of these conditions in a single experiment, they each used a triggering event that was not of trivial magnitude and thereby undermined the likelihood of an interactive augmentation of the separate effects of the initial provocation and the subsequent triggering provocation.

Thus, by combining a trivial trigger with all four necessary cells, our own designs are unique. We made two major predictions. First, and more important, we expected an interaction between the initial (Time 1) provocation and the subsequent (Time 2) minor triggering provocation. Consistent with successfully designing the triggering event to be trivial in nature, when there is no prior provocation its presence or absence will not augment aggression. Under conditions of initial provocation, however, its presence or absence will dramatically affect displaced aggression, strongly increasing it in the presence of a trigger.

Second, we expected a "contrast effect" under conditions of provocation and no trigger compared with a no-provocation/notrigger condition. Specifically, we expected less displaced aggression after participants experience an initial provocation compared with the condition in which there is no such antecedent provocation. Although this may seem counterintuitive, contrast effects have been reliably demonstrated in a wide range of stimulus categories including the sensory modalities of vision (e.g., Curran & Johnston, 1996; Raymond & Isaak, 1998; Suzuki & Cavanagh, 1998), taste (e.g., Schifferstein & Oudejans, 1996), touch (e.g., DeCarlo, 1994), and smell (e.g., Lawless, 1991; Madigan, Ehrlichman, & Borod, 1994). Judgmental contrasts effects are also seen in a variety of other domains, including self-evaluations following exposure to highly attractive same-sex stimulus persons (Thornton & Maurice, 1997; Thornton & Moore, 1993), ratings of hyperactivity in twins (Simonoff et al., 1998), ratings of a target person's performance (Sumer & Knight, 1996), judgments of ethical marketing practices (Kellaris, Dahlstrom, & Boyle, 1996), and ratings of the emotional content of faces (Underwood, 1994). Moreover, they form the core of theoretical formulations of fundamental processes of human judgment, such as adaptation level theory (Helson, 1964). More relevant, however, is that this contrast prediction is consistent with some specific instances of prior experimental research on aggression (e.g., Berkowitz & Knurek, 1969). Even more important, however, is that in our meta-analytic analysis of (nontriggered) displaced aggression (Marcus-Newhall et al., 2000), we found support for this contrast effect within the context of the overall main effect of supportive evidence for displaced aggression. Specifically, the stronger the provocation, the smaller the magnitude of displaced aggression.

Given this strong evidence in support of a contrast effect, one might next question why displaced aggression occurs at all, or why we expect minor triggering events to augment displaced aggression to a degree that exceeds an additive model. We have already explained the latter expectation by having pointed to the interactive potential of priming effects and attributional distortion in augmenting the aggression arousing impact of ambiguous Time 2 triggering events. With respect to why any displaced aggression is found in the absence of Time 2 triggering events, it is likely that the same factors are at work. Recall that the negativity of experimental settings and similarity between provocateur and target were important moderators of displaced aggression in our metaanalysis. We suspect that the ambient levels of these factors (as found on average within the studies comprising the displaced aggression literature) probably served these same priming and attributional functions for nontriggered displaced aggression.

In sum, we expected a contrast effect in our experimental studies. Compared with a strong initial provocation and its resulting negative affect, a subsequent nontriggering neutral social interaction with a target person will appear markedly more positive than it does in a situation in which that same neutral interaction was not preceded by an unpleasant provocation.

## Study 1

## Method

## Participants and Design

Participants were 48 women, 14 men, and 2 persons who did not indicate their gender. All were undergraduate students at the University of Southern California who participated in a 2 (provocation: yes or no)  $\times$  2 (trigger: yes or no) between-subjects design in exchange for class credit. They were randomly assigned to the four cells of the design, with the constraint that one full replication of the design be completed prior to starting the next.

## **Provocation Procedure**

Participants were told that they would be taking part in two studies concerned with problem-solving ability. They were taken into a room that contained a tape recorder, headphones, a timer, a television, and a VCR, and they were asked to complete a short questionnaire requesting information about their gender, class standing, race, desire to perform well, and their performance expectations on the problem-solving tasks. They were then told that the first study examined the effect of distraction on problemsolving ability. They received 15 anagrams, along with an answer sheet, and were told to solve as many of them as possible within 5 min. The experimenter then turned on music and exited the room. After 5 min, he or she re-entered, turned off the music, took the answer sheet (ostensibly to grade the answers), and gave the participant a handout indicating how a sample of engineering students had performed on the same anagram task during the previous semester. After leaving again, the experimenter subsequently re-entered after approximately 4 min to give the participant his or her score.

In the provocation condition, participants listened to mildly irritating music (Stravinsky's Rites of Spring) and were assigned difficult anagrams (e.g., tophhapogr = photograph). When the experimenter returned with their scores, participants were first told that they had scored below average compared with the sample of engineering students. Then, the experimenter insulted them about their problem-solving ability and effort by telling them that their performance was really poor and that the anagram portion of the experiment should be done again. However, the experimenter then added in an exasperated and irritated tone that it would be a waste of his or her own time to rerun the session and, therefore, they should just proceed to the second part of the study. In the no-provocation condition, participants listened to mildly positive music (a jazz version of the opening movement of Bach's Brandenberg Concerto No. 3), solved easy anagrams (e.g., meit = time or item), were told that they received an average score compared with engineering students, and were not insulted.

## Trigger Procedure

Participants were then told that the second problem-solving study investigated the distracting effects of attending both to audio and visual cues. It was explained that an assistant on a videotape would ask them 15 trivia-game questions. The trivia questions would be spoken aloud by the assistant and the multiple-choice foils for each would be presented on a card that the assistant displayed. Participants were to answer as many as possible, using the response form provided by the experimenter. The experimenter then turned on the videotape and exited the room. The videotape began immediately. On it, an African American female research assistant read trivia questions aloud while holding up multiple choice responses for each question. After the conclusion of the tape, the experimenter re-entered the room, retrieved the participant's answer sheet, again provided a summary sheet indicating the average score obtained by engineering students from the previous semester, and then exited to grade the participant's responses.

After about 5 min, the experimenter re-entered the room and told the participant that the research assistant on the videotape has applied for a paid position for the following semester and the supervising faculty member had requested feedback on her performance. Participants were informed that their feedback was completely anonymous and confidential. They were provided with a form that requested ratings on the research assistant's motivation, intelligence, work ethic, and professionalism. In addition, participants were to indicate how strongly they recommended the assistant for the position. The experimenter left the room while the participant completed these forms. To create a perception of response anonymity, participants were explicitly asked to not put any identifying information on their response forms, to seal them in the envelope provided, and to place their sealed envelope in a large box containing many other such envelopes. After completing their assessment of the research assistant, participants were given manipulation check measures regarding the anagram task and their mood. Finally, they were thanked for their participation and debriefed.

In the trigger condition, the research assistant read the questions too quickly, mispronounced some of the words and names (e.g., Leonardo da Vinci was pronounced Leon de Vinsky), and occasionally mixed up the multiple choice responses (reading Question 9, for instance, while presenting the answers to Question 10). In the trigger condition, participants were told they did poorly compared with engineering students, but were not insulted about their performance. In the no-trigger condition, the video-taped assistant read the questions slowly, made no pronunciation errors, and correctly matched the questions with the appropriate multiple choice answers. Subsequently, they were told that their score was about the same as that of the engineering students' average. We expected that, compared with its presence in the provocation condition, the absence of insult by the experimenter in the trigger condition would ensure a lower intensity of negative experience.

#### Results

#### Manipulation Checks

**Provocation.** Five items assessed the effectiveness of the provocation manipulation: (a) The anagram task was fun (reverse coded), (b) the anagrams were difficult, (c) I was frustrated by doing the anagrams, (d) there was enough time to solve the anagrams (reverse coded), and (e) I was pleased with my performance (reverse coded). Each item was assessed on an 8-point scale ranging from 1 (*intensely disagree*) to 8 (*intensely agree*). Internal consistency for these variables was acceptable (Cronbach's  $\alpha = .76$ ). Participants in the provocation condition indicated stronger agreement with this composite (M = 23.93, SD = 4.27) than those in the no-provocation condition (M = 18.31, SD = 5.2), t(64) = 4.71, p < .001.

*Trigger.* Four items assessed the effectiveness of the trigger manipulation: (a) There was enough time to answer the questions

(reverse coded), (b) the research assistant read the questions too quickly, (c) the research assistant was responsible for my performance, and (d) the research assistant spoke clearly (reverse coded). Each item was assessed on a 6-point scale ranging from 1 (*strongly disagree*) to 6 (*strongly agree*). Internal consistency for these variables was acceptable (Cronbach's  $\alpha = .77$ ). Participants in the trigger condition indicated stronger agreement with this composite (M = 17.40, SD = 4.0) than those in the no-trigger condition (M = 13.31, SD = 4.13), t(64) = 4.01, p < .001. In an analysis constrained to only no-provocation participants, the directional difference of higher means for those triggered compared with the no-trigger participants, although in the predicted direction (16.06 vs. 14.12, respectively), was not reliable, t(30) = 1.31, p = .20, two-tailed.

### Comparison of Provocation and Trigger Intensities

As indicated, our conceptual analysis led us to attempt to ensure that the intensity of the Time 1 provocation exceed that of the subsequent triggering event. To assess our success in inducing this difference we compared manipulation check effect sizes produced by the two manipulations. Under conditions of no initial provocation, the effect size of the composite score of the 4 trigger manipulation-check items discussed above was moderate in strength (d = .46). In contrast, the effect size for the composite of the 5 provocation manipulation-check items under conditions of no subsequent trigger was nearly twice as large (d = .89). This difference is consistent with our intent of making the triggering event relatively trivial compared with the initial provocation.

#### Aggression

To assess aggression towards the target, 5 items were combined into a composite measure. Four of them reflected the subjective ratings of the research assistant on adjectives concerning jobrelated attributes (hardworking, professional, smart, and motivated), with higher numbers equating to less favorable evaluations. The bipolar 6-point scales for each item ranged from very lazy to very hardworking (reverse coded), very professional to very unprofessional, very intelligent to very unintelligent, and very motivated to very unmotivated. The fifth item was the overall recommendation for the research assistant position (i.e., I recommend this research assistant be a paid assistant next semester). This item was assessed on a 6-point scale ranging from 1 (strongly disagree) to 6 (strongly agree) and was subsequently reverse coded so that higher numbers corresponded to more aggression. Each item was individually standardized, with scales reversed when necessary, and items subsequently combined to form a composite measure on which a higher score reflected less positive evaluations and therefore greater aggression. Internal consistency of the composite measure was acceptable (Cronbach's  $\alpha = .78$ ).

A 2 × 2 analysis of variance (ANOVA) revealed a Provocation × Trigger interaction, F(1, 62) = 4.97, p < .029, as well as a main effect of trigger, F(1, 62) = 6.32, p < .015, but no main effect of provocation, F(1, 62) = .69, p > .10. Inspection of Figure 1 shows an interaction that perfectly parallels the predicted form of interaction. When a participant had not been previously provoked, triggering behavior by the research assistant had little moderating effect on aggressive responses toward her (M = -0.047,



Figure 1. Effect of initial provocation and a subsequent triggering event on the intensity of displaced aggression (Study 1). The dependent measure reflects mean standardized scores across all five response items. Scores were standardized because the descriptive endpoints on the four evaluative items differed from that of the behavioral measure. Error bars represent the standard error for that condition.

SD = 0.75 for the no-provocation/no-trigger cell compared with M = 0.002, SD = 0.72 for the no-provocation/yes-trigger cell). Again, when participants were initially provoked but did not experience the triggering event, they displayed the least amount of aggression in the four cells (M = -0.285, SD = 0.64), whereas those who were both provoked and triggered displayed the greatest amount of aggression (M = 0.52, SD = 0.59).

The simple effect of provocation for participants who were subsequently triggered was significant, F(1, 60) = 4.69, p < .05. Specifically, the presence of prior provocation increased aggressive responding for participants who later experienced a trivial triggering event. In addition, although the simple effect of provocation for nontriggered participants was not statistically significant, F(1, 60) = 0.97, p > .10, the directional findings were consistent with the hypothesized judgmental contrast effect (i.e., lower aggression in the yes-provocation/no-trigger condition than in the no-provocation/no-trigger condition).

As previously indicated, the five-item composite aggression measure was composed of four evaluative ratings and one behavioral assessment-the hiring recommendation. In additional analyses, separate ANOVAs were performed on each of these two types of ratings. Both independently showed interactions of the same form as that seen on the overall five-item composite measure. For the evaluative ratings, a  $2 \times 2$  ANOVA again revealed a Provocation  $\times$  Trigger interaction, F(1, 54) = 4.67, p < .035, as well as a main effect of trigger, F(1, 54) = 5.16, p < .027, but no main effect of provocation, F(1, 54) = .71, p > .10. The means for the no-provocation and ves-provocation cells under trigger were 4.94 and 5.44, respectively, whereas under no trigger they were 4.50 and 4.00, respectively. For the hiring recommendation, a 2  $\times$  2 ANOVA revealed a marginally reliable Provocation  $\times$ Trigger interaction, F(1, 60) = 2.82, p < .098. And again, there was a main effect of trigger, F(1, 60) = 9.94, p < .003, but no main effect of provocation, F(1, 60) = 0.00, p > .10. The means for the no-provocation and yes-provocation cells under trigger were 3.98 and 4.55, respectively, whereas under no trigger they were 3.96 and 3.66, respectively. Thus, for both measures, all four means were on the negative side of the scale midpoint of 3.5.

## Mediational Analyses

Mediational analyses were conducted to assess whether subjective ratings of the research assistant mediated the effect of the manipulation of trigger on aggression for provoked participants. Regression of participants' composite subjective negative rating of the research assistant on the experimental manipulation of the assistant's triggering behavior confirmed the manipulation-check data showing that mistakes made by the assistant resulted in more negative ratings, F(1, 30) = 22.27, p < .001 ( $R^2 = .42$ ). Next, regressing the amount of aggression on the trigger manipulation confirmed stronger aggression in response to the manipulated triggering action by the research assistant, F(1, 30) = 13.58, p < 100.001 ( $R^2 = .31$ ). Mediation was again explored by regressing aggression toward the research assistant on the subjective negative ratings elicited by the trigger manipulation. After the manipulation of the research assistant's triggering behavior was controlled for, participants' subjective negative ratings determined their level of aggression toward the research assistant, F(1, 29) = 4.90, p < .035 $(\Delta R^2 = .10)$ . However, when the effect of subjective negative ratings is removed, the manipulation of triggering action by the confederate no longer predicted aggression, F(1, 29) = 2.32, p > 2.32 $.10 (\Delta R^2 = .05)$ . These analyses show that among participants who had been previously provoked, subjective negative ratings of the research assistant at least partially mediated the effect of her manipulated triggering behavior on subsequent aggression toward her.

## Discussion

#### Predictions

Study 1 supported our major hypothesis that experiencing a prior provocation markedly augments subsequent aggressive responses to a minor triggering event. Participants who experienced both provocation and trigger were more aggressive than either those who experienced the triggering event alone or those who experienced the initial provocation without the subsequent triggering event. Further, this triggered displaced aggression was greater than that predicted by an additive model. Thus, the combination of provocation and a second minor triggering provocation had a synergistic effect on displaced aggression. Mediational analyses confirmed that the negative affect elicited by the trivial trigger importantly contributed to the differential effects that were found as a function of whether it was preceded by a strong initial provocation.

With regard to our secondary hypothesis, we expected a judgmental contrast effect under conditions in which a benign nontriggering second person was evaluated against the background of a strong prior provocation from another person. Although in the anticipated direction, the findings of Study 1 did not yield a reliable contrast effect. As previously indicated, this expectation had been based not only on a wide array of supporting research, as well as theory, but more specifically on aggression research (Berkowitz & Knurek, 1969) and a meta-analytic result found within the literature on displaced aggression (Marcus-Newhall et al., 2000). The issue of a contrast effect in the context of nontriggered displaced aggression, however, is perhaps more complex than it appears from a straightforward reading of our contrast effect prediction. That is, the obtained meta-analytic contrast effect of decreased (nontriggered) displaced aggression as a function of stronger Time 1 provocation (Marcus-Newhall et al., 2000) was found within the context of an overall positive main effect of displaced aggression.

This suggests that the likelihood of a contrast effect would have been greater had we used a Time 1 provocation of stronger intensity. At the same time, a reliable contrast effect is also more likely to have been found had we used a Time 2 target that did not elicit the negative priming effects that were likely to have been induced by the out-group membership of this target (even in the absence of any minor provoking action on his or her part). We incorporated these changes in Study 2.

#### Relative Strengths of Initial Provocation and Trigger

Every experiment contains paradigm-specific features that reflect the specific ways in which its key conceptual variables are manipulated. Often, their presence (unknowingly) may be critical for obtaining the reported results. A key feature of our conceptual analysis of triggered displaced aggression was that triggering events of trivial intensity are key to the occurrence of disjunctively escalated aggressive responding. In contrast with the few prior studies of triggered displaced aggression, Study 1 attempted to implement that requirement by making the initial (Time 1) provocation discernibly stronger than the subsequent triggering provocation. Thus, participants who received the provocation experienced the frustration of extremely difficult anagrams, the disrupting effect of loud cacophonous music during their task performance, a consequential failure to complete the anagram task, a self-diminishing comparison with the performance of engineering students, and, finally, insult from the experimenter concerning their intellectual competence.

In contrast, triggered participants encountered only some of these ingredients. Like the initial provocation, the triggering triviagame task was frustrating and produced a sense of failure, both in an absolute sense, and, as with the Time 1 provocation, as a result of the comparison information provided about the performance of engineering students. However, there was neither loud disrupting music, nor any direct interpersonal insult, either from the experimenter or any other person. Consequently, we expected the provocation to be stronger than the trigger because of the added component of insult contained in the provocation manipulation.

The comparison of effect sizes resulting from each manipulation suggests that we did in fact successfully implement the intended differential intensity between the Time 1 provocation and the triggering event. Nevertheless, two potential concerns arise. First, the specific items used to calculate the provocation/no-provocation and the trigger/no-trigger effects sizes were not identical. Specifically, three of the five items used to assess provocation intensity concerned the participant's perception of the anagram task (i.e., the anagrams were difficult, the anagrams were fun, there was enough time to complete the anagram task), whereas the remaining two items were concerned with the participant's affective reaction to the task (i.e., frustration with the task, being pleased with one's performance). In contrast, the four items underlying the trigger/ no-trigger effect size were concerned with the participant's perception of the performance of the research assistant. Consequently, there is little overlap between the content of the items used to calculate the effect sizes, making an interpretation of the obtained difference in their relative magnitudes problematic. Thus, the obtained difference in manipulation strength, though seemingly supportive of our intent, may have reflected instead a happenstance use of "easier items" in the provocation set compared with the trigger set.

Second, the person who provided the triggering experience was an African American woman. For our experimental participants, all of whom were non-African American, the use of a person who was manifestly an out-group member might have primed negative and aggressive thoughts because of her race. Therefore, although the provocation manipulation contained the additional component of a direct, interpersonally delivered insult, it is conceivable that by incorporating the additional aggressive prime of an African American out-group target, the trigger manipulation was not in fact discernibly weaker in its intensity. Instead, the obtained smaller effect size for the trigger manipulation may have reflected an overlay of politically correct responding (Vanman, Paul, Ito, & Miller, 1997) on the trigger manipulation-check items, when in fact the triggering experience was as intense as the initial provocation.<sup>1</sup>

Of course, if the triggering event in Study 1 matched the intensity of the initial provocation, it is difficult to explain why we obtained the interactive effect that we did, rather than a mere replication of the prior additive outcomes of P. Worchel (1966) and R. A. Baron and Bell (1975). Nevertheless, to the extent that these concerns have validity, they question our conceptual argument concerning the importance of low-intensity triggering events. Therefore, Study 2 addressed this issue.

## Study 2

In Study 2 we examined the robustness of the effects obtained in Study 1 by conceptually replicating it. Consequently, within the context of the same experimental design, we incorporated a rather different set of procedures. In addition, we assessed displaced aggression with a different response measure. Thus, we made the following specific changes in Study 2.

First, in Study 1 provocation was primarily experienced in a face-to-face manner. Although this increases the personalization of the provocation (Alicke, Klotz, Breitenbecher, Yurak, & Vredenburg, 1995), it precluded experimenter-blind procedures. Greater uniformity can be achieved if the provocation is administered in a standardized manner. In Study 2, unknown to the participant, the provocation was taped and delivered by means of an intercom. Although the experimenter was not blind to the provocation or trigger manipulations, he or she had no contact with the participant after that point in the procedure at which the experimental conditions were imposed. This was made possible by the fact that all subsequent instructions were given by means of a computer.

Second, target attributes can be expected to moderate aggressive responding (Miller & Marcus-Newhall, 1997). In Study 1, the displaced aggression target was an African American woman. We

<sup>&</sup>lt;sup>1</sup> Our speculations here about the possibility of politically correct responding are antithetical to the potential implication of the triggering person's African American identity raised above in our discussion of our failure to have obtained the expected contrast effect. Both cannot be correct. The point here is that features of the implementation of Study 1 precluded any certainty about whether one, the other, or neither are correct.

have already discussed the potential problems this may have created with respect to both our intent to ensure that the triggering event was less provoking than was the Time 1 provocation and our attempt to assess their relative intensities. In Study 2, we sought to overcome these potential problems. First, unlike Study 1, in which the manipulation-check items used to assess the relative intensities of the provocation and trigger manipulations differed, we used an identical manipulation-check item. Second, we sought to generalize our finding by replicating the same overall effect with a Caucasian male target. This change avoids any priming of aggressive constructs, as might have been induced by the use of an out-group female African American target. At the same time, it precludes any possibility that an evaluative overlay of politically correct responding might artifactually confirm our intended difference in the relative strength-of-manipulation inductions in our comparison of manipulation-check effect sizes. In Study 2, instead of an out-group target as the source of the triggering event, we inserted a shared cognitive link between provocation and trigger-an explicit connection between the participant's year in school and the provoking aspect of both the initial provocation and the subsequent triggering event.

As previously indicated, we also used a different measure of aggression in Study 2. In other studies we have argued for the functional equivalence of various measures of aggression (Carlson, Marcus-Newhall, & Miller, 1989). Nevertheless, some aggression researchers (e.g., Tedeschi & Quigley, 1996) have argued that there are important conceptual distinctions among different types of measures of aggression. Although general evaluative assessments were included among the dependent measures of Study 1, that study also contained a measure that clearly reflected imposition of harm to the person who provided the triggering event. Specifically, participants were made to believe that they were influential with respect to a research job coveted by the target and that a negative evaluation of her would diminish her chances of obtaining it.

In Study 2, we used only a hypothetical behavioral measure of aggression. Participants had no expectation that they could in fact influence any outcomes important to the person who provided the triggering event. Although affect, belief, and intention typically are viewed as antecedents of aggressive (or any other) action, at the same time, there are motivating consequences that can stem from the knowledge that there clearly will be an opportunity for completing an aggressive response. Such motivational effects may function to extrematize responding on measures of affect, belief, and intention. Specifically, in Study 1, participants had the perception that they did indeed have the power to harm the triggering person who was (partially) responsible for their poor score on the trivia game (by reducing her chances of getting the job). In turn, the opportunity for completing this aggressive act may have had feedback consequences that extrematized their ratings on the evaluation items, making them more negative. Moreover, although costs that might be incurred as a result of behavioral action might deter it, the anonymity of responding in Study 1 along with the absence of opportunity for future interaction with, or retaliation by, the target, should have eliminated any meaningful costs. In Study 2, we examined the effects of the independent variables under circumstances that precluded such motivational feedback effects. By using only a hypothetical measure of aggression, we provided no true feasibility of aggressive action. Thus, Study 2 had

the potential to extend the range of circumstances (or types of response measures) in which the effects found in Study 1 can be expected.

In Study 2, we also sought to increase the likelihood of obtaining a reliable contrast effect in the comparison of the noprovocation/yes-provocation cells under the no-trigger conditions. Thus, in addition to eliminating our previous use of an African American Time 2 triggering target, we attempted to strengthen the intensity of the Time 1 provocation.

A final difference between the two studies was the use of a male experimenter in Study 2.

### Method

#### Participants and Design

Forty-nine participants from an introductory psychology course (42 women, 7 men) with a mean age of 18.96 years (SD = 1.29) volunteered in exchange for extra course credit in a 2 (provocation: yes or no)  $\times$  2 (trigger: yes or no) between-subjects design. The four resulting cells each contained 12 participants, with the exception that the no-provocation/no-trigger cell contained 13.

## Procedure

After arriving at the site of the experiment and being seated close to a computer monitor, each participant was asked to complete a consent form while waiting for the arrival of a second participant (who was actually a confederate). One minute later, the male confederate arrived and asked if he was at the correct place. He completely ignored the participant, making no eye contact. The experimenter acknowledged that it was the correct room, produced a form containing the true participant's student ID number along with a fake one for the confederate, and asked both for their ID numbers and year in school. To uniformly enable the confederate to indicate that he was 1 year ahead of the participant.

The experimenter then stated that (a) the experiment used two separate rooms, (b) prior to their arrival it had been decided that the person with the participant's ID number would remain in the room in which they were currently located, whereas the confederate would be relocated with another experimenter in a separate room on the same floor, and (c) the confederate would be instructed to return to the current room to exchange materials during the course of the study. The exchanges would be made by slipping the materials under the door so as to avoid face-to-face contact that might affect their subsequent impression formation of one another.

Biographic sketch and designation of dissimilarity. About 20 s after the confederate departed, the experimenter explained that he needed to check the computer monitor in the other room because his research assistant recently had experienced problems with it. He then asked the participant to fill out a biographic sketch that later would be exchanged with the confederate. The sketch presented 10 group categories (political orientation: liberal/conservative; smoking status: smoker/nonsmoker; position on abortion: pro-life/pro-choice; social class: low/middle/upper; major: letters, arts, and sciences (LAS)/business; stance on the death penalty: for/ against; Greek member: yes/no; stance on marijuana legalization: for/ against; year in school; transportation status: resident/commuter student; and California native: yes/no) for which participants were to indicate their group memberships and rate the importance of the categories with respect to "your identity or the sense of who you are as a person." To provide evidence of dissimilarity between the participant and the confederate, the confederate constructed and later exchanged with the participant a biographic sketch that differed on 7 of the 10 categories. The three shared memberships were determined ideographically by designating them as the 3rd, 7th, and 8th most important categories listed by the participant.

When the participant completed the form, the experimenter returned to present instructions stating that the experiment primarily concerned impression formation in the context of a hiring decision and that three items would be mutually exchanged with the confederate: their answers to anagram problems, their biographic sketches, and an evaluation by each participant of the other's responses to the anagram test. The accompanying rationale indicated that hiring decisions are often based on limited information. It went on to explain that the experiment allowed each participant to see a small sample of the other's work (i.e., the anagram answers), that each participant should get a feel for how the other person evaluates and interacts with potential coworkers (i.e., their respective evaluations of the other participant's anagram answers), that each participant should get to know something about the "other applicant" as a person (i.e., the biographic sketch), and that these three bits of information would help them form an impression of each other.

Provocation manipulation. The participant was then left to complete a series of 14 anagrams presented on the computer screen. Each was displayed for 5 s, followed by a screen prompt for the answer. The participant was instructed to write each response on an answer sheet, to state it aloud, and, in instances in which they did not know the answer, to state aloud "I don't know." The experimenter, who could both hear and communicate with the participant by means of an intercom that was positioned about 5 ft (1.5 m) away, further indicated that after a 10-s period for writing and stating the answer, the correct answer would appear on the screen for 5 s. Then, 5 s would be made available for the participant to use each word in a first-person sentence prior to the appearance of the next anagram.

The provocation condition contained three central features. First, the anagrams were fairly difficult, consisting of words like *environment*, *lieutenant*, and so forth. Second, as a distraction, the storm passage from Beethoven's Sixth Symphony was played loudly in the background. Third, and most important, following the paradigm of Stemmler (1989), at three separate times a tape recording of the experimenter's voice was played over the intercom, telling the participant to speak louder. During the first interruption, the participant heard the experimenter say, "Look, I can barely hear you. I need you to speak louder please." Roughly 1 min later, the experimenter said in a slightly louder and more frustrated voice, "Hey, I still need you to speak louder." Finally, about 2 min into the anagram task, the experimenter uttered in a very frustrated voice, "Look, this is the third time I have had to say this! Can't you freshman [sophomores, juniors, etc.] follow directions?" The reference to the participant's year in school was ideographically matched to their prior self-report.

In the no-provocation condition, the 14 anagrams were much easier (e.g., *test, acted, time, dress*). The background music was the sound of falling rain, which is typically judged as relaxing. Finally, the tape-recorded interruptions containing the experimenter's voice were neutral in nature, simply informing the participant that he or she had just completed the 4th, 8th, and 12th anagrams.

At the conclusion of the anagram task, the participant was informed on the computer screen that the confederate was being sent to exchange answer sheets to the anagram problems and that the participant should place envelopes containing his or her biographic sketch and anagram answer sheets under the door. Because the experimenter and confederate could hear the participant's anagram responses over the intercom, the confederate could complete his answer sheet to indicate correct answers for all items answered correctly by the participant and for three additional anagrams. Consequently, the confederate always appeared more competent. This made the subsequent trigger manipulation more believable.

The computer then instructed the participant to first look at the confederate's biographic page, followed by the anagram answer sheet, and then to evaluate his or her anagram answers using the provided form containing 7-point scales for rating (a) the confederate's overall performance, (b) how much it appeared that the confederate concentrated on the task, and (c) how likely it was that the confederate would perform well in a class at the University of Southern California (USC) that required good verbal skills. The form also contained several blank lines for indicating an overall impression of the confederate and any additional feedback or evaluation.

Trigger manipulation. After the participant completed this form, the computer instructed him or her to place it in an envelope, wait for the confederate, and then exchange evaluations. The computer then asked the participant to read the confederate's evaluation of their own work. In the trigger condition, the confederate's evaluation of the participant ranged from neutral to negative on the three items. However, the key feature of the trigger condition occurred in the section in which the confederate narratively wrote his or her overall evaluation. Specifically, the confederate wrote, "Although the task was difficult, I would have thought that a college [freshman, sophomore, etc.] would have performed better on this task." The choice of year in school matched that provided by the participant at the beginning of the experiment and that alluded to in the provocation manipulation, thereby linking the insulting features of the provocation and trigger manipulations. In the no-trigger condition, the confederate's evaluation was neutral on the three items, with a concluding comment of, "Although the task was fairly difficult, I think he [or she] did a fairly good job."

Dependent measures. After inspecting the confederate's evaluation, the participant completed dependent measures. The first series of items consisted of six 11-point scales assessing attitude toward the confederate: liking, friendliness, competence, intelligence, enthusiasm for being a partner with the confederate in a future experiment, and enthusiasm for hiring him, were the participant the personnel director for a company.

Additional measures included a Mood Adjective Checklist (MACL; Nowlis, 1965), which served as the manipulation check for the provocation variable, and three manipulation-check questions concerning the trigger variable. To give the pretense of anonymity, the participant was told not to write his or her name or student ID number on any of these pages. In addition, to further augment the illusion of anonymity, after completing the dependent measure packet, the participant was instructed to place it in an envelope and set it in a box containing other response envelopes that ostensibly had been completed by previous participants.

If the participant exhibited any agitation after completing the dependent measure packet, he or she was given a "cooling-down period" during which he or she listened to relaxing music and read magazines. Then, he or she was probed for suspicion and debriefed. Although the experimenter was not blind to the provocation or trigger manipulations, he had no contact with the participant after the point in the procedure at which the experimental conditions were imposed. This was made possible by administering all subsequent instructions by means of a computer.

#### Results

#### Manipulation Checks

**Provocation.** To assess the effectiveness of the provocation manipulation, participants were asked to recall how they felt when they completed the anagram task (viz., the provocation manipulation). They indicated their mood by responding to the MACL (Nowlis, 1965). On an a priori basis, six adjectives (i.e., *angry, irritable, defiant, annoyed, grouchy,* and *frustrated*) that describe an irritated or angry mood were analyzed. Internal consistency among these items was high (Cronbach's  $\alpha = .90$ ). The summed composite score could range from 0 (*no anger*) to 18 (*maximum anger*), because the scores of each of the six separate items were on a 4-point scale ranging from 0 to 3. The provocation condition mean (M = 10.33, SD = 5.32) exceeded that of the no-provocation group (M = 4.64, SD = 3.94), t(47) = 4.27, p < .001, indicating an effective manipulation.

Trigger. Three 11-point scales ranging from 1 (strongly agree) to 11 (strongly disagree) assessed the effectiveness of the trigger manipulation. The high-aggression portion of each scale was expanded, with the described midpoint being 8 for the item angered or upset and 4 for the positively worded items. When Welch's test (Wilcox, 1996) was used (because the assumption of homogeneity of variance was violated), participants in the trigger condition indicated being more angered or upset by the confederate's evaluation (M = 5.71, SD = 3.42) compared with those not triggered (M = 9.68, SD = 1.65), t(32.87) = 5.14, p < .001 (note that smaller numbers correspond to more agreement with the given statement). The second and third questions asked participants whether they were happy with the confederate's evaluation of their work and whether it was fair and reasonable. Nontriggered participants indicated greater happiness (M = 3.68, SD = 1.93) than their triggered counterparts (M = 8.13, SD = 2.01), t(47) = 7.90, p < .001, and perceived the confederate's evaluation as more fair and reasonable (M = 3.56, SD = 2.62 vs. M = 6.21, SD = 3.11), t(47) = 3.23, p < .01.

Analysis of the trigger manipulation-check data for the subset of participants in the no-provocation conditions produced similar results. Specifically, separate t tests for each of the three trigger manipulation-check questions showed reliable differences (ps < .05) in the predicted direction, indicating that under conditions of no prior provocation, participants in the trigger condition were significantly more angry, less happy, and thought the evaluation was less fair and reasonable than did those who were not triggered. Thus, the effect obtained on the overall analyses of the manipulation check for the trigger manipulation was not due to those participants who had experienced prior provocation.

### Comparison of Provocation and Trigger Intensities

As with Study 1, we wanted to create an initial provocation that was more intense than the trigger. However, unlike Study 1, the present study included some overlap in the items used to assess the relative effectiveness of the provocation and trigger manipulations, thus allowing for a more appropriate comparison of the respective effect sizes. Specifically, both the provocation and trigger manipulation checks assessed the participant's degree of anger following each manipulation. Under conditions of no initial provocation, the trigger/no-trigger effect size for angry was d = 0.99. In contrast, the provocation/no-provocation effect size under conditions of no subsequent trigger was larger (d = 1.28). As with Study 1, this evidence is consistent with our contention that the initial provocation was perceived as subjectively more intense and irritating to the participants than was the triggering event.

#### Aggression Measure

Triggered displaced aggression. To assess aggression toward the target (the confederate), six items were combined to form a composite score: four were evaluative ratings of the confederate (viz., liking, competence, friendliness, and intelligence), two were more behavioral (i.e., the participant would be enthusiastic about having the confederate again as a partner in another experiment and the participant would have a favorable opinion about hiring the confederate if, as personnel director of a company, he or she were in the position to do so). This last item in particular buttressed the cover story of "assessing impression formation in the context of making hiring decisions in business settings." The 11-point scales ranged from 1 (strongly agree) to 11 (strongly disagree) and again used an expanded high aggression range with a described scale midpoint of 4. Internal consistency among items was high (Cronbach's  $\alpha = .88$ ), and perfectly parallel reliable effects were obtained when evaluative and more behavioral sets were separately analyzed. A 2 × 2 ANOVA of the composite scores (i.e., adding all six items) yielded the expected Provocation × Trigger interaction (see Figure 2), F(1, 45) = 9.11, p < .005, as well as a main effect of trigger, F(1, 45) = 10.28, p < .005, but, as anticipated, no effect of provocation, F(1, 45) = .70, p > .10.

Inspection of Figure 2 shows that when a participant had not been previously provoked, triggering behavior by the confederate had no effect on aggressive responses to him (M = 22.54, SD = 9.54 for the no-provocation/no-trigger cell vs. M = 23.00, SD = 6.18 for the no-provocation/yes-trigger cell). However, when a participant was initially provoked (in the context of the anagram task), the impact of subsequent triggering action by the confederate was sizable. Specifically, when initially provoked but then not triggered, participants displayed the least aggression among the four cells (i.e., M = 17.17, SD = 4.20), whereas when provocation was followed by subsequent triggering behavior by the confederate, it resulted in the most aggression (i.e., M = 32.50, SD = 12.19).

As in Study 1, the simple effect of provocation for participants who were subsequently triggered was statistically significant, F(1, 22) = 5.80, p < .05, indicating that the combination of prior provocation with a triggering event dramatically increased aggressive responding. Second, the simple effect assessing the judgmental contrast effect was marginally significant, F(1, 23) = 3.22, p =.086. Specifically, participants who were initially provoked but did not experience a subsequent triggering event displayed even less



Figure 2. Effect of initial provocation and a subsequent triggering event on the intensity of displaced aggression (Study 2). In contrast to Figure 1, there was no need to standardize the scores, because the descriptive endpoints of the evaluative measures and the hypothetical behavioral responses were identical. Error bars represent the standard error for that condition.

aggression compared with control participants who were neither provoked nor triggered.<sup>2</sup>

As stated above, the composite aggression measure was composed of four evaluative ratings and two behaviorally orientated questions. Separate 2 × 2 ANOVAs were performed on the evaluative ratings and the behavior-like composites. Results for both were identical to those of the overall six-item composite measure, with each exhibiting an interaction that reflected the same ordering of means (i.e., F(1, 45) = 8.25, p < .01, for the evaluative rating composite, and F(1, 45) = 6.25, p < .05, for the behavioral composite).

Contrast effect. As discussed previously, Study 1 produced only directional evidence for the predicted contrast effect in which the nontriggering confederate seemed comparatively nicer when the experimenter had previously provoked the participant than when he had not (cf. Berkowitz & Knurek, 1969). The procedures of Study 2 were altered to make a contrast effect more likely. The primary change was to increase the intensity of the initial provocation in Study 2 relative to Study 1. Although both studies used different manipulation-check items to assess the effectiveness of the provocation manipulation, a comparison of the resulting provocation/no-provocation effect size for each study is germane to this point. Consistent with the notion that the intensity of the provocation was stronger in Study 2 relative to the first study, the composite provocation/no-provocation effect size for Study 2 (d = 1.28) exceeded that found for Study 1 (d = 0.89). Nevertheless, we obtained only a marginally reliable contrast effect of more favorable evaluation in the yes-provocation/no-trigger condition compared with the no-provocation/no-trigger condition, F(1, 23) = 3.22, p = .086.

The present study, however, provided a second, alternative measure of the contrast effect. As stated previously, participants were given the opportunity to evaluate the confederate prior to the trigger manipulation. Specifically, participants rated the confederate's performance on the anagram task before they received the confederate's evaluation of their own work (i.e., the trigger manipulation). Because participants had not yet been divided into trigger and no-trigger conditions, this second measure is based on twice as many participants and therefore adds substantially greater power to the analysis. It should be noted that participants were fully aware that these evaluations would be given to the confederate.

Participants rated the confederate on three questions using a 7-point scale ranging from 1 (very poor) to 7 (excellent). The questions asked for ratings on the confederate's (a) overall performance, (b) apparent concentration level used during the task, and (c) likelihood of performing well in a USC class that requires good verbal skills.

Despite the high intercorrelation among these three items ( $\alpha = .86$ ), a 2 (provocation: yes or no)  $\times$  3 (item) repeated measures ANOVA was performed. It yielded a main effect of provocation, F(1, 47) = 2.13, p < .05, with provoked participants rating the confederate more favorably than nonprovoked participants. As anticipated, neither the repeated measure variable (item) nor the interaction approached statistical significance. Whereas our other analyses had failed to provide consistent evidence of a reliable contrast effect, these results show that participants responded more favorably (i.e., displayed less aggression) toward the confederate under conditions of initial provocation.

## Mediational Analysis

Following R. M. Baron and Kenny (1986), mediational analysis examined data from the provocation conditions. Regression of participants' composite subjective affective response to the evaluation received from the confederate on the experimental manipulation of the confederate's triggering action confirmed the manipulation-check data, which showed that receipt of a negative evaluation from the confederate induced a more negative affective state, F(1, 22) = 28.70, p < .001,  $(R^2 = .57)$ .

Second, in line with the ANOVA results on aggressive responses to the confederate's evaluation, regressing the amount of aggression upon the trigger manipulation confirmed stronger aggression in response to triggering action by the confederate, F(1, 22) = 16.97, p < .001,  $(R^2 = .44)$ . Finally, mediation was explored by regressing aggression toward the confederate on subjective affective responses elicited by the trigger manipulation. After controlling for the manipulation of the confederate's triggering evaluation, participants' subjective negative affective responses to it determined their level of aggression toward the confederate, F(1, 21) = 8.59, p < .01,  $(\Delta R^2 = .16)$ . However, when the effect of subjective affective response is removed, the manipulation of triggering action by the confederate no longer predicted aggression, F(1, 21) = 0.89, p > .10,  $(\Delta R^2 = .02)$ .

These analyses are consistent with the idea that among participants who previously had been provoked, subjective negative affective reactions to the confederate's triggering evaluation mediated the effect of his triggering action on the participants' subsequent aggression toward him.

Separate mediational analyses examining the independent role of each of the three trigger manipulation-check items show that the two affective measures-anger/upset and happiness-carry the mediational effect shown with the composite measure. In contrast, the cognitive appraisal of whether the triggering evaluation was fair and reasonable did not contribute to the mediation of subsequent aggressive responding. Specifically, on this latter manipulation-check item, although the effect of the perception of the fairness of the evaluation predicted aggression when controlling for trigger, F(1, 21) = 7.03, p < .02, ( $\Delta R^2 = .14$ ), if perceptions of fairness are controlled, then the trigger variable still predicted aggression, F(1, 21) = 8.11, p = .01, ( $\Delta R^2 = .16$ ). This indicates that perceptions of the fairness of the evaluation did not fully mediate aggression for previously provoked participants. Put more abstractly, data are more consistent with the view that the affective arousal elicited by the triggering event seemed to mediate the effects that it produced in combination with prior provocation. The cognitive appraisal it elicited seemed to play a limited role in mediating its effect.

#### Discussion

The results of Study 2 replicated the main finding of Study 1, namely, a triggering event has a differential impact when combined with the presence or absence of an initial provocation. In

 $<sup>^{2}</sup>$  Both of these simple effect analyses used the error term specific to their individual one-way ANOVAs and not the error term from the overall factorial analysis, because the assumption of homogeneity of variance was violated (Keppel, 1991).

addition, the results showed that subjective feelings of anger regarding the triggering event, and not cognitive appraisals, at least partially mediated the effect of the triggering event on aggression by previously provoked participants. Finally, countering the failure to find reliable support for a contrast effect in Study 1, Study 2 yielded a marginally reliable contrast effect on one set of response measures and a reliable effect on a second set. These differential findings might be due in part to the two paradigmatic changes discussed earlier that made Study 2 a more sensitive test of the contrast hypothesis—namely, an initial provocation of higher intensity and a target unencumbered by the negative priming effects of racial out-group membership.

The findings of Study 2 are conceptually important for several other reasons. First, we replicated the triggered displaced aggression effect of Study 1 under conditions in which the triviality of the trigger relative to the initial provocation was of unambiguously lower intensity. Second, given that any differential behavior by the experimenter as a function of experimental condition was essentially precluded in Study 2, it seems unlikely that the results of Study 1 can be attributed to experimenter bias. Third, the results are not constrained to circumstances in which aggression is directed toward an African American out-group target. In Study 2, the target was Caucasian. Finally, the overall results of both studies were highly similar despite key differences in the response measures. Furthermore, the same pattern of results was obtained in each study for both evaluative ratings and more behaviorally orientated measures.

Although the findings in these studies are theoretically novel, one might quibble with the measures of aggression that were used. Specifically, in Study 2, do the six items that comprised the composite dependent measure actually represent "aggression"? The aggression measure in Study 2 was hypothetical in nature. Consequently, participants are less likely to believe that their responses can potentially harm the target. Nevertheless, several issues warrant notice. First, on the basis of meta-analytic evidence, Carlson, Marcus-Newhall, and Miller (1989) argued for the functional equivalence of physical (e.g., electric shocks) and verbal measures of aggression, such as those used in this research. Evidence for this claim was provided by reliable positive relationships between written measures of aggression and physical measures, such as shock intensity and duration. In addition, both physical and written measures of aggression were influenced in a similar fashion by precipitating factors, such as frustration and anger (Carlson et al., 1989). Second, along with trait evaluations, the composite aggression measure used in Study 2 included more behaviorally oriented items concerning a hypothetical hiring decision, denial of which could constitute intention of harm-a common component of modern definitions of aggression (Geen, 1990). More important, separate analyses that were performed on the four "evaluative ratings" and the two "behavioral measures" (e.g., partnering and hiring) yielded identical patterns of means. These parallel effects add further confirmation to Giancola and Chermack's (1998) rebuttal of Tedeschi and Quigley (1996), who argued that different dependent measures of aggression do not tap the same overarching construct. However, the key dependent measure of aggression used in Study 1 consisted of a recommendation regarding hiring a person for a research assistant job. A recommendation against hiring manifestly qualifies as harmful behavior. This measure of aggression yielded the same pattern of effects as those of Study 2. In light of these arguments, the dependent measure of aggression in Study 2, although more hypothetical in nature, shares both functional and conceptual similarities with more traditional measures of aggressive responding.

#### General Discussion

The obtained results support our primary hypotheses. First, our major prediction received strong support in both studies. Under conditions of no prior provocation, the presence or absence of a minor triggering event had virtually no effect, confirming that its intensity was indeed trivial. In fact, the level of aggression exhibited under these two conditions was almost identical (see the two data points on the left portions of Figures 1 and 2). This essential equivalence was obtained in the face of reliable differences on the items that assessed the trigger manipulation-differences showing that participants subjectively did experience the confederate's behavior (the triggering event) as upsetting. This pattern of results, however, dramatically differed under conditions of initial provocation. Then, as discussed above, in the absence of a subsequent triggering event, aggression toward the second person (the confederate) was reduced. When, instead, the initial provocation was succeeded by a minor triggering event, it produced substantially more aggression than when either provocation or trigger was presented alone or when their independent effects were additively combined.

The results are important because this is the first instance in which triggered displaced aggression has been investigated in a circumstance wherein both of the following characteristics have been simultaneously imposed: (a) the initial provocation and a subsequent triggering event were orthogonally manipulated in the same experimental design, thus providing all the appropriate comparison cells, and (b) the intensity of the triggering event was minor and insignificant compared with the provocation. They are all the more striking in light of the numerous differences between the two studies in terms of implementation of the Time 1 provocation, the triggering event, the target, the cover stories, and the response measures.

Our second hypothesis, considerably less central to our major theoretical concern, is the predicted contrast effect. In Study 2, we obtained some evidence antithetical to the notion of (nontriggered) displaced aggression, both on the primary aggression measure (p = .086) and on the composite evaluation measure of the target that was administered before the trigger manipulation (p < .05). Specifically, under conditions of no triggering action by a second person, that person received less aggression (or more favorable evaluation) when the participant had been subjected to an initial provocation. This result is consistent with both the meta-analytic findings of Marcus-Newhall et al. (2000) and with the experimental outcomes of Berkowitz and Knurek (1969). Relative to the experimenter, who had provided both a frustrating and insulting experience, a target person who had treated the participant in a neutral manner elicited a more favorable reaction.

Nevertheless, the comparison between the two cells that are germane to this contrast effect only yielded a directional effect on the primary measure of aggression in Study 1 that was not significant. To assess the overall finding across both studies, the data from the aggression measure were pooled across studies after being appropriately standardized. The data set selected from Study 2 structurally matched that of Study 1. Each set was drawn from half of the design and each used response measures collected after the participant had completed his or her interaction with the target. This resulted in a 2 (study: 1 vs. 2)  $\times$  2 (condition: provocation/no-trigger vs. no-provocation/no-trigger) design. The main effect for condition, F(1, 49) = 4.87, p < .05, obtained in the absence of both an effect for study, F(1, 49) = .002, p > .10, and a significant Condition  $\times$  Study interaction, F(1, 49) = .128, p >.10, confirms a reliable contrast effect. Aggression was reliably lower across studies when the participant received an initial provocation without a subsequent triggering event compared to the no-provocation/no-trigger condition.

This finding is important because it points to a process that counters the effects of variables ordinarily associated with reliable (nontriggered) displaced aggression effects. In this sense, then, it highlights the theoretical importance of actions and attributes of the displaced aggression target that can function to justify displaced aggression. Note that the weaker contrast effect was obtained in Study 1, in which the nontriggering target was an African American out-group member. One can only speculate whether the nonsignificant, but directional counter-displaced-aggression effect obtained in Study 1 would be reversed in a noncollege setting in which the politeness norms that constrain public negative reactions toward this racial out-group (e.g., Vanman et al., 1997) are not as potent.

Now that we have developed an experimental paradigm that confirms our theoretical arguments about the consequences of triggered displaced aggression, it can be extended to related research domains. For instance, two obvious areas of application are intergroup aggression and alcohol-related aggression. Previous theories have argued that triggering effects will be stronger at the intergroup level (e.g., Insko & Schopler, 1998; Miller & Marcus-Newhall, 1997) and under the influence of alcohol (e.g., Steele & Southwick, 1985). As interesting and theoretically important as such extensions may be, however, an understanding of the mechanisms or processes that underlie our effects must also be addressed.

# Underlying Process

Our mediational analyses, although informative, only begin to address the underlying processes that might explain (triggered) displaced aggression. We next discuss some conceptual approaches to a process understanding and raise relevant issues with respect to them.

# Sensitization Theory

P. Worchel (1960, 1966) advocated a "sensitization" model of displaced aggression. He argued that the initial blocking of an aggressive action produces a hostile drive that compromises an individual's ability to endure further frustration. Under conditions of no initial provocation, a subsequent minor incident will tend to be ignored because it falls below a threshold for aggressive responding. Under the heightened state of agitation caused by initial provocation, however, the same triggering event will exceed the ensuing lowered threshold, making an aggressive reaction more likely. Given the relatively mild nature of the new target person's triggering action, the actor's aggressive response to it will seem incommensurate with its triviality. Buss (1961) discussed a similar threshold view.

Although a threshold model is intuitively appealing, its confirmation and utility is compromised by the empirical difficulties that underlie the measurement of thresholds. An alternative approach is to assess the model less directly. Its underlying drive notion implies an amplification or energization of any response, suggesting, for instance, a more rapid counting of the number of times the letter C appears on a page. Compared with those allowed to retaliate against the initial provocateur, those prevented from doing so should exhibit a faster pace. Likewise, it implies a reduced pace of counting by those provided with the opportunity to display triggered displaced aggression, compared with those initially provoked but given no subsequent opportunity for triggered displaced aggression.

## Priming

Priming provides an explanation from a purely cognitive perspective. Research has established that negative affect, such as anger, has a priming function that directs attention toward negatively valenced stimuli (Higgins & King, 1981). Berkowitz (1993), in his cognitive neoassociation theory of aggression, proposed that aversive events produce negative affect, which in turn activates various thoughts, memories, physiological responses, and motor reactions. An initial provocation will prime aggression-related constructs that make future aggressive responding more likely. Such connectionist models conceptualize memory as a network of interconnected nodes and links, with activation spreading through a series of connected concepts in direct proportion to their strength of interconnection (e.g., McClelland & Rumelhart, 1988; Read & Miller, 1993). A priming explanation makes different predictions than those outlined above with respect to the sensitization account. Specifically, it draws attention to a different set of response measures. It thus predicts, for instance, better recall or faster reaction times to specific words used by the provocateur that were key components of the provocation.

## Rumination

An important feature of the experimental paradigms that we used for studying triggered displaced aggression, as well as those used by other researchers before us, concerns the temporal interval between the strong initial provocation and the second minor triggering provocation. It was short. Likewise, for all of the 82 effect sizes examined in our meta-analysis of (nontriggered) displaced aggression (Marcus-Newhall et al., 2000), the temporal interval between the provocation and the measurement of displaced aggression never exceeded 20 min or so. Thus, both the reliable triggered displaced aggression effects reported herein and the meta-analytically reliable (nontriggered) displaced aggression effect reflected in the mean effect size of +.54 can be attributed to lingering arousal produced by the initial provocation. Although the obtained reliable findings within these temporal constraints have both theoretical and practical importance, much of the everyday explanatory appeal of the concept of displaced aggression applies to situations with much more extended temporal gaps between an initial provocation and the display of displaced aggression. For these latter situations an adequate explanatory process requires mechanisms that can function over a duration well beyond the typical 20-30-min interval over which a temporarily induced mood state or provocation lasts. The concept of rumination (Martin & Tesser, 1989; Rusting & Nolen-Hoeksema, 1998) is a good candidate for such an explanatory process.

# A Model of Triggered Displaced Aggression

We can now summarize several types of factors that are likely to moderate the effect of a Time 2 triggering event on aggressive responding in the context of an initial provocation. First, there are features of the Time 1 provocation itself that may increase the probability that an individual will engage in ruminative activity, thus making subsequent aggression more likely. These features include the subjective importance and intensity of the provocation and the social environment within which it occurred. For example, a particularly intense provocation, or one that occurs in view of other people and thereby causes embarrassment (Tedeschi & Felson, 1994), may result in enhanced brooding and rumination on the part of the provoked individual. Such brooding can function as a prime that increases the probability of an aggressive response to a subsequent minor triggering event, making it exceed the level expected under the matching rule (Axelrod, 1984).

A second consideration is the nature of the events that occur during the interval between the initial provocation and the trigger. Activities that are either distracting or change the social environment by directing attention away from thought about the initial provocation are likely to reduce the interactive effect of a subsequent minor triggering event. In contrast, activities that remind an individual about the initial provocation, such as a phone call from the secretary of the initial provocateur, will enhance the synergistic effect of a minor trigger on aggression.

A third factor is the nature of the Time 2 triggering event. As argued earlier, triggers that either are of low intensity or ambiguous with respect to whether they reflect intended aggression are more subject to attributional distortion. Such distortion may result in a trivial triggering event being perceived as an intentionally hostile one that warrants an aggressive response. In addition, other factors are likely to augment the interactive impact of the Time 2 trigger on aggressive responding: the similarity between the triggering event and the original provocation, the similarity between the individual responsible for the trigger and the initial provocateur, and the occurrence of a triggering event in the context of alcohol ingestion or other factors that reduce the inhibitions that normally restrain aggressive responding.

Finally, personality characteristics may serve as moderators of the situational variables described above. For instance, an array of personality dimensions may be associated with individual differences in the propensity to ruminate. Cognitive propensities, as reflected in need for closure (Webster & Kruglanski, 1994) or personal need for structure (Thompson, Naccarato, Parker, & Moskowitz, 1993), need for cognition (Cacioppo & Petty, 1982), "linkers/nonlinkers" (McIntosh & Martin, 1992), or features of obsessive-compulsive character, might promote ruminative thought about the initial provocation. Likewise, personality traits associated with emotional reactivity, such as narcissism (Raskin & Terry, 1988), chronic anger (Buss & Durkee, 1957), or low and/or unstable self-esteem (Baumeister, Smart, & Boden, 1996) may function to heighten sensitivity to a Time 2 triggering event.

# The Relation Between Triggered Displaced Aggression and Excitation Transfer Theory

Our theoretical analysis of triggered displaced aggression appears to share conceptual similarities with the excitation transfer theory of Zillman (1971, 1979), in which an initial provocation is followed by a subsequent manipulation of arousal, by means of noise (e.g., Donnerstein & Wilson, 1976; Konecni, 1975), an erotic film (e.g., Cantor, Zillman, & Einsiedel, 1978; Donnerstein, Donnerstein, & Evans, 1975; Zillman, 1971), or physical exercise (e.g., Zillman, Katcher, & Milavsky, 1972). The subsequent manipulation of arousal corresponds in some sense to what we have called a Time 2 triggering provocation. However, as discussed in Marcus-Newhall et al. (2000), there are two key conceptual differences between studies of triggered displaced aggression and research on excitation transfer theory: (a) the target of aggression (studies in excitation transfer theory mainly examine direct retaliation against the original provocateur, whereas the present research is concerned with a displacement target), and (b) the nature of the Time 2 triggering event (the present studies deliberately design the trigger to be trivial and very low in its arousal qualities in contrast to the erotica or strenuous physical exercise typically used in excitation transfer research, which elicits moderate to high levels of arousal).

## Conclusion

Why are the procedures and results of these studies important? First, we have proposed that triggered displaced aggression is more valid ecologically than are the typical published investigations of displaced aggression. The latter examine aggression against an "innocent" target. Although firm descriptive data is lacking, we think it likely that most real-world instances of displaced aggression reflect responses to minor triggering events, such as the ones implemented in these studies, rather than responses to totally innocent victims. Second, from a more analytical perspective, we have argued that a better understanding of the dynamics of triggered displaced aggression requires an experimental design that contains (a) orthogonal manipulations of both provocation and trigger and (b) a minor or trivial triggering event. To our knowledge, the present studies are the first to explicitly combine both of these features. Finally, the primary insight that they provide is to highlight the significant moderating role that triggering can play in displaced aggression. Specifically, in cases of initial provoking events, the occurrence of subsequent minor provocations can dramatically augment the magnitude of displaced aggression.

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