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Having “Been There” Doesn’t Mean I Care: When Prior Experience Reduces Compassion for Emotional Distress

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The current research found that participants who had previously endured an emotionally distressing event (e.g., bullying) more harshly evaluated another person’s failure to endure a similar distressing event compared with participants with no experience enduring the event or those currently enduring the event. These effects emerged for naturally occurring (Studies 1, 3, and 4) and experimentally induced (Study 2) distressing events. This effect was driven by the tendency for those who previously endured the distressing event to view the event as less difficult to overcome (Study 3). Moreover, we demonstrate that the effect is specific to evaluations of perceived failure: Compared with those with no experience, people who previously endured a distressing event made less favorable evaluations of an individual failing to endure the event, but made more favorable evaluations of an individual managing to endure the event (Study 4). Finally, we found that people failed to anticipate this effect of enduring distress, instead believing that individuals who have previously endured emotionally distressing events would most favorably evaluate others’ failures to endure (Study 5). Taken together, these findings present a paradox such that, in the face of struggle or defeat, the people we seek for advice or comfort may be the least likely to provide it.

Keywords: affect, social judgment, hot–cold empathy gap, life events, compassion

Imagine an employee going through a rocky divorce. Struggling to endure the emotional distress of the divorce, the employee decides to request a leave of absence from work. Imagine further that the employee has to make this request to one of two supervisors. One of these supervisors has no experience with divorce, whereas the other supervisor endured a similarly trying divorce early in her career. Whom should the employee approach with this request?

Common sense might suggest that the supervisor who had endured divorce would be more understanding of the employee’s plight. This intuition also has considerable empirical support, with research demonstrating that prior experience with an emotionally distressing event increases sympathy for others facing that same event (Batson et al., 1996; Loewenstein & Small, 2007). In the current research, we propose that rather than being sympathetic, people who have previously endured an emotionally distressing event may be prone to negatively evaluate others who fail to endure a similar event. We argue that constrained memory for the impact of past emotional distress (i.e., “I can’t recall how difficult it was”), combined with knowledge of their own ability to endure the distressing event (i.e., “I did it”), leads people who have endured distressing events to render more negative evaluations (i.e., “Why can’t you do it too?”). Thus, although lay intuition may encourage the employee to approach the supervisor who has lived through divorce, the current research suggests that the employee may ultimately be penalized for doing so.

The Benefits of Experience

The idea that prior experience with an emotionally distressing event can facilitate sympathy is well supported. Research has shown that for a wide range of experiences—from childhood acne to physical abuse—people who have been through a distressing event tend to be more sympathetic toward others facing the same event compared with those with no experience (Barnett, Tetreault, Esper, & Bristow, 1986; Batson et al., 1996; Christy & Voigt, 1994; Clore & Jeffery, 1972; Hodges, Kiel, Kramer, Veach, & Villaneuva, 2010). For example, Batson et al. (1996) found that participants who had acne when they were younger experienced

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1 In this article, we will use the term emotional distress to describe situations in which people must cope with unpleasant affective states in order to achieve their goals. These states refer to a wide range of negative emotions (e.g., anger, fear, sadness), drive, and feeling states (e.g., pain, fatigue; cf. Loewenstein, 2000).
more empathy and compassion when reading about an adolescent’s experience with acne than did those with no such experience. Similarly, Christy and Voigt (1994) found that people who had been abused as a child said that they would be more likely to intervene if they saw a child being abused than were those who had never been abused. These findings suggest that prior experience with a similar event facilitates the ease with which another’s perspective can be adopted, which increases sympathetic responding (e.g., Bandura, 1969; Batson, 1987, 1991).

The literature described here captures situations in which someone who has endured an emotionally distressing event evaluates someone else who is going through the same ordeal. Much less is known about how previously enduring distress affects our evaluation of others who fail to endure a similar event, that is, those who are unable to overcome or appropriately cope with the emotional distress produced by a trying event. Other than cases involving extreme emotional distress, there is a general expectation that people will manage to endure distressing events (Kim, Thomas, Wilk, Castro, & Hoge, 2010; Schwartz, 2000). People recognize that a soldier might be fearful of going on patrol, but there is an expectation that the soldier will complete his or her task. People recognize that being the target of bullying is emotionally challenging, but nevertheless expect a bullied student to cope in normatively appropriate ways and not, for example, resort to physical violence or dropping out of school. Unfortunately, people often fall short of these expectations (e.g., Folkman & Moskowitz, 2004).

This article examines how individuals who have previously endured an emotionally distressing event evaluate those who fail to endure similar distressing events. One possibility is that the shared experience breeds compassion. That is, those who have endured a distressing event in the past will have more compassion for those who fail to endure a similar situation. A second hypothesis, and the one endorsed here, is that those who have endured a distressing event in the past will in fact have less compassion for those who fail to endure a similar distressing event. Research on the hot–cold empathy gap and task completion support our prediction.

The Potential Downside of Enduring Distress

The hot–cold empathy gap suggests that difficulties recalling the impact of past emotional distress may lead people who have endured distress to be less compassionate toward others’ failures to endure. First described by Loewenstein (1996), empathy gaps capture the tendency for people who are not affectively aroused (i.e., in a cold state) to underestimate the impact of affective or “hot” states (Loewenstein, 1996) on their judgments and behaviors. For example, empathy gap research has shown that when dieters are not feeling hungry, they tend to underestimate the extent to which hunger will impact their dietary choices (Nordgren, van der Pligt, & van Harreveld, 2006; Nordgren, van der Pligt, & van Harreveld, 2008; Nordgren, van der Pligt, & van Harreveld, 2009). The empathy gap has been documented for a wide range of affective states, including sexual arousal (Ariely & Loewenstein, 2006), fear (Van Boven, Loewenstein, & Dunning, 2005), hunger (Nisbett & Kanouse, 1969), pain (Nordgren, McDonnell, & Loewenstein, 2011), and fatigue (Nordgren, van der Pligt, & van Harreveld, 2007).

Hot–cold empathy gaps are thought to result from constrained memories for affective states (Loewenstein, 1996; Nordgren, van der Pligt, & van Harreveld, 2006; Robinson & Clore, 2002). Though people can recall the situation that led to an affective state (e.g., “I was distraught because I was going through a divorce”) and can recall the relative strength of the affective state (e.g., “That was the most distraught I have ever been”), they cannot freely bring forth the feeling of the affect (e.g., reexperiencing the agitation and sadness). In other words, although everyone has experienced fatigue, fear, and pain, if they are not currently experiencing an affective state, they will underestimate its influence over behavior.

Constrained memory for affective experiences means that once a distressing event has been overcome, people will have difficulty reliving their original emotional response to the event. Because people instead rely on their immediately accessible feelings in forming judgments, they may believe that their current “cold” state perceptions of the event reflect how they have always felt and reacted (e.g., Read & Loewenstein, 1999; Van Boven, Loewenstein, Dunning, & Nordgren, 2013). For example, the absence of emotional distress in response to a divorce that an individual is “over” will produce the inference that the divorce was less painful than was actually experienced. Because the impact of prior emotional distress is underestimated, divorce may now seem like a life event that can be readily overcome. Support for this account comes from research demonstrating a desensitization bias in emotional perspective-taking (Campbell, O’Brien, Van Boven, Schwarz, & Ubel, 2014). This study found that repeated exposure to an emotion-inducing stimulus (e.g., hearing the same joke a number of times) reduced empathic accuracy for others’ reactions to that stimulus (e.g., predicting how funny others will find the joke). This occurs because people fail to account for how their emotional reactions have diminished over time and therefore mispredict that others who encounter the emotional stimulus for the first time would react less intensely than they in fact do.

Beyond their constrained memory for the impact of past emotional distress, people who have endured distressing events possess the knowledge that they managed to endure the emotional distress. Research has shown that tasks that have been completed seem easier than comparable tasks that have yet to be completed (e.g., Gist, 1987; Gist & Mitchell, 1992). For example, individuals who complete a challenging task tend to subsequently view the task as less difficult, and to predict that they could complete the task again, compared with individuals who have yet to complete the same task (Feather, 1966, 1968; Lenney & Gold, 1982; Manderlink & Harackiewicz, 1984). Having completed the task indicates that the task is manageable and could be completed again. This literature suggests that having previously endured an emotionally distressing event may decrease the perceived difficulty of coping with that event. If past distressing events seem easier to overcome in hindsight, those who have previously endured distressing events may be more likely to penalize individuals who fail to endure a similar distressing event in the present.

In sum, people who have previously endured an emotionally distressing event may utilize two key pieces of information when making assessments of individuals struggling or failing to endure a similar event. First, people who have endured distressing events will be unable to actively experience the motivational force associated with the emotional distress (e.g., Loewenstein, 1996; Nordgren et al., 2006). Second, these individuals possess the knowledge that they managed to endure the event. Both pieces of
information should make the distressing event seem less difficult to overcome in hindsight. We argue that this perception leads individuals to form negative assessments of those who fail to endure the distressing event. In other words, the combined experience of “I can’t recall how difficult it was” and “I did it” leads individuals to render more negative evaluations (i.e., “Why can’t you do it too?”).

Though it may appear to be at odds with research demonstrating that prior experience fosters compassion, our central prediction is fully compatible with these findings. Although prior experience may generally increase compassion for those who have endured or are currently enduring a similar experience (e.g., Batson et al., 1996), we argue that when a target individual fails or struggles to endure an emotionally distressing event, having endured the event will in fact decrease compassion by decreasing the perceived difficulty of overcoming that event. To illustrate, compared with someone who has never endured divorce, a divorcee may be more likely to commiserate with a coworker’s day-to-day efforts while undergoing a divorce, but may also be more prone to condemn the coworker if he succumbs to an emotional outburst at work. Having been there herself, the divorcee can understand the individual’s situation. Yet because divorce seems less difficult to overcome in hindsight, emotional outbursts in the workplace may now seem unacceptable.

Unfortunately, the very conditions under which prior experience is proposed to reduce compassion (i.e., perceivers who have previously endured distressing events, and targets who struggle or fail to endure these events) characterize many real-world situations. Struggling graduate students may seek help from tenured faculty, and members of oppressed communities may appeal for aid from policymakers who overcame structural barriers themselves. Uncovering the potential downsides of overcoming adversity raises novel insights into how people treat those in need.

Current Research

In the current research, we present five studies designed to test the prediction that those who have previously endured an emotionally distressing event will form more negative evaluations of individuals who fail to endure a similar event. In Study 1, we test our predictions outside of the laboratory by exploring how participants at a polar plunge—an event that requires participants to plunge into icy water—evaluate another individual who failed to plunge, either before or after they participated in a plunge themselves. In Study 2, we aim to provide stronger causal evidence for this effect by randomly assigning participants to various stages of completion of a fatiguing exam, and then examining their evaluations of an individual who performs poorly on the exam. Study 2 also serves to rule out a critical alternative explanation—that participants who have endured the event are simply more knowledgeable about the event. Study 3 was designed to explore the mechanism behind this effect by testing whether individuals who have previously endured a distressing event render more negative evaluations of failure because the event seems less difficult to overcome in hindsight. Next, if our reasoning is correct, this effect should only occur when people who have endured a distressing event evaluate others who fail to endure the emotional distress. Study 4 tests this boundary condition. Finally, Study 5 examines whether the people’s lay beliefs match the predicted effect, or whether people instead endorse the belief that those who have previously endured a distressing event would more favorably evaluate failures to endure the distress than would those who have never endured the event.

Studies 1 through 4 have two central conditions: a never-endured condition and a previously endured condition. The never-endured condition comprises individuals who have no experience with the distressing event, whereas the previously endured condition comprises individuals who have endured the event, but are no longer actively experiencing the distress. All participants read about and evaluated an instance in which someone failed to endure a similar emotionally distressing event. To capture the various types of negative evaluations that are associated with such failures, we used three types of measures—emotional reactions to the individual (Studies 1, 3, and 4), general evaluations of the individual (Studies 2 through 4), and behavioral intentions to help (Study 2; Nordgren et al., 2007). Compared with participants who never endured the event, we expected that participants who previously endured the event would form less positive evaluations, feel less compassion and more contempt, and be less willing to help an individual who failed to endure a similar distressing event.

Study 1

In Study 1, we conducted a field experiment to test whether previously enduring a distressing event would lead to more negative evaluations of those who failed to endure that event. Specifically, we examined participants’ emotional reactions toward an individual who failed to complete a polar plunge—an event that requires participants to enter an icy body of water—either before (never-endured condition) or after (previously endured condition) they completed a plunge themselves. Five participants assigned to the never-endured condition indicated that they had previously completed a polar plunge or similar event in previous years, and were excluded from the analyses. We predicted that those who had endured this distressing event would experience less compassion and more contempt in response to another’s failure to endure it.

Method

Participants. Participants were 54 individuals ($M_{age} = 30.69, SD_{age} = 9.95$; 37.5% female; 78% Caucasian, 9% Hispanic, 7% Asian American/Asian, 6% African American/Black) from Chicago, Illinois, and the surrounding area.

Procedure. Participants enrolled in a March polar plunge were approached for participation. Participants were approached in the waiting room of the event, which was an on-site, heated indoor facility. After agreeing to participate, participants were randomly assigned to the never-endured or previously endured conditions. Participants in the never-endured condition completed the study prior to the polar plunge, whereas participants in the previously endured condition provided their contact information and completed the study within 1 week after the polar plunge. In each condition, participants read about an individual named Pat who joins a polar-plunge team and is determined to take the full plunge. When the day approaches, Pat waits in the cold winter air in his swim trunks for his team’s turn. Pat decides that he cannot endure the feeling of cold, despite his motivation to complete the plunge. Pat leaves without completing the polar plunge. After reading the
vignette, participants reported their compassion and contempt toward Pat, and then completed control and demographic measures.

**Measures**

**Emotion.** Participants indicated the extent to which they experienced specific emotional reactions to the polar plunger. In measures adapted from Nordgren et al. (2007), we assessed both positive and negative emotions in the form of compassion (Batson, 1991) and contempt (Rozin, Lowery, Imada, & Haidt, 1999), respectively. The compassion items were “sympathy” and “compassion” (Cronbach’s α = .91), and the contempt items were “contempt,” “anger,” and “disgust” (Cronbach’s α = .83). For each of the five items, we asked participants, “When you think about the polar plunger, to what extent do you feel . . . ?” They responded on a 7-point scale from 1 (not at all) to 7 (extremely).

**Control variables.** Participants in the previously endured condition indicated the number of previous polar plunges they had attended. In addition, participants in the previously endured condition indicated how deep they had entered the water during the polar plunge on a scale from 1 (did not go in) to 7 (full body immersed). Likewise, participants in the never-endured condition indicated the level of submersion they anticipated in the plunge.

**Results and Discussion**

No demographic variables yielded any effects on the dependent measures in this study, nor did the demographic composition of the sample differ between the previously endured and never-endured conditions. Demographic variables will therefore be omitted from further consideration.

As predicted, participants in the previously endured condition felt less compassion toward the failed polar plunger (M = 3.54, SD = 1.80) than participants in the never-endured condition (M = 4.60, SD = 1.43), t(52) = −2.42, p = .019, 95% CI [−1.94, −0.18], d = 0.65. Participants in the previously endured condition also felt more contempt for the failed polar plunger (M = 2.98, SD = 1.58) than did participants in the never-endured condition (M = 1.90, SD = 1.10), t(52) = 2.97, p = .005, 95% CI [0.35, 1.82], d = 0.79. Controlling for participants’ level of submersion (or anticipated submersion) did not alter the results. In examining the previously endured group, six participants had completed more than one polar plunge. Our results were robust to the exclusion of these participants, suggesting that our results are not contingent on a subgroup of polar plunge “experts” who may view the task differently than do those who completed the plunge only once.

This pattern of results suggests that, as expected, having previously endured the polar plunge decreased participants’ compassion and increased contempt toward another who failed to endure that event. One main benefit of Study 1 is external validity—we demonstrate the effect among active polar-plunge participants in the field. To better hone in on causality, Study 2 utilized an experimental paradigm to test the proposed effect.

**Study 2**

In Study 1, we provided initial support for our prediction by finding that participants who had been through the polar plunge felt more negatively about an individual who failed to endure the cold temperatures than did those who had yet to endure the event. Our explanation of these results draws on the hot–cold empathy gap to suggest that constrained memory for the impact of past emotional distress, combined with knowledge of their own ability to endure the distressing event, led people who had endured the event to render more negative evaluations. One alternative explanation of Study 1’s results is an informational account. It is possible that the polar plunge was actually an easier event than one might imagine, and participants in the previously endured condition simply possessed more knowledge. We designed Study 2 to rule out this alternative explanation.

To do so, we randomly assigned participants to one of three conditions (currently enduring vs. previously enduring vs. never endured) in a between-subjects design. The added currently enduring condition allows us to rule out the possibility that the task was simply easier than the participants expected. The currently enduring participants experienced the same distressing event as did participants in the previously endured condition (and thus both had task experience), but only the former made their evaluations while actively experiencing the associated emotional distress. Thus, if the informational account is true, participants in both the previously endured and currently enduring conditions should be less compassionate toward an individual who fails to endure the emotional distress. If our account holds, participants in the previously endured condition will be less compassionate than will those currently enduring the distressing event. Although both groups will have experience with the task, only participants currently enduring the event will be able to appreciate the motivational force of the emotional distress.

The distressing event in question was a strenuous test of mental endurance, which was designed to induce mental fatigue (Nordgren et al., 2006, 2007). We note that although all people—and students in particular—have endured fatigue-inducing examinations broadly, we argue that the knowledge that one has made it through a particular type of event creates the tendency to penalize those who fail to endure the event. The knowledge that one overcame a specific event may not generalize to related, but distinct, events. For example, a law student’s completion of the bar exam may lead her to criticize another who struggles with the bar, but not someone who struggles to complete a medical licensing exam. Thus, to have a never-endured condition, we created a test that represented a unique experience unfamiliar to undergraduate students. Specifically, the test was named the Cognitive Ability and Persistence Battery (CAP-B), a test said to be developed for use by organizations to evaluate current and prospective employees along several dimensions, such as self-control and memory. In addition to being labeled a unique experience, the content of the test was unusual (i.e., memorizing a series of nine-digit number strings) compared with the type of tests that students typically complete.

Participants in all conditions were asked to evaluate an individual who struggles with the test’s fatiguing nature and consequently cannot complete the test. In the currently enduring condition, participants evaluated the target individual while experiencing the fatiguing test. Participants in the previously endured condition
evaluated the individual 1 week after completing the test. Participants in the never-endured condition evaluated the individual without having experienced the test themselves. Given that the test was ostensibly designed to assess employee skill, we also extended our dependent measures in Study 2 to participants’ evaluations of the employee and their beliefs that the employee should be rehired by the organization. Our key prediction was that participants in the previously endured condition would make less favorable evaluations and would be less willing to rehire the employee than would those in the currently enduring and never-endured conditions.

Method

Participants. Participants were 135 students enrolled at a large Midwestern university (M_{age} = 20.68, SD_{age} = 1.64; 64% female; 39% Caucasian, 36% Asian American/Asian, 9% African American/Black, 4% East Indian, 6% Hispanic, 6% Other).

Procedure. Participants completed the study on computers in individual cubicles. Participants were randomly assigned to one of three conditions: currently enduring, previously endured, or never endured. All participants were first informed that the purpose of the study was to examine the use of an employee screening test (i.e., the CAP-B) in a variety of samples.

In the currently enduring condition, participants began the testing procedure upon arrival to the laboratory. In a design adapted from Nordgren et al. (2006, 2007), participants completed a strenuous memory test that lasted for 20 min. The memory test required participants to memorize a series of nine-digit number strings. Each number string appeared for 11 s, after which participants were asked to memorize the numbers for 7 s before they were prompted to enter the number string to the best of their ability. After 20 min of the fatiguing test, participants were asked to complete a questionnaire. At this point, participants read a vignette about an employee who was asked to complete the CAP-B test. The employee was motivated to perform well on the test, but became too fatigued halfway through and could not complete it. Participants then evaluated the employee. Finally, participants indicated their current level of fatigue, completed open-ended questions examining awareness of the hypotheses (e.g., “What do you think the researchers are testing in this study?”), and were debriefed. No participants indicated awareness of our specific research hypotheses.

The previously endured condition was similar to the currently enduring condition, except that participants evaluated the employee 1 week after completing the fatiguing test. After completing the test themselves, participants in the previously endured condition were informed that there would be a follow-up questionnaire to collect additional information about their perceptions of the CAP-B. Upon beginning the follow-up survey, participants were shown a description of the test and asked whether or not they recalled participating in the test. All participants recalled the experience. Participants then completed the vignette portion of the study and were debriefed as in the currently enduring condition.

Finally, participants in the never-endured condition did not complete the fatiguing test. Instead, upon arrival to the laboratory, participants in the never-endured condition viewed a description of the test and sample questions before completing the vignette portion of the study.

Materials

Manipulation check. To ensure whether participants in the currently enduring condition were distinguishable from those in the previously endured or never-endured conditions in terms of fatigue, participants indicated their current levels of fatigue on a scale from 1 (not at all fatigued) to 7 (very fatigued).

Employee evaluations. To assess participants’ evaluations of the employee, participants rated the employee on two dimensions related to competence (“competent” and “capable”) on sliding scales from 0 (not at all) to 100 (extremely; Fiske, Cuddy, Glick, & Xu, 2002). The mean of these items served as an index of competence (r = .89).

Willingness to rehire. To assess possible behavioral consequences for the employee, we asked participants to recommend whether or not the employee’s contract should be renewed. Participants completed three items related to willingness to rehire (e.g., “I would definitely renew this employee’s contract”) on scales from 1 (strongly disagree) to 7 (strongly agree). The mean of these items served as an index of willingness to rehire (Cronbach’s α = .85).

Results and Discussion

No demographic variables yielded any effects on the dependent measures in this study, and these were omitted from further consideration.

Manipulation check. To examine whether our test was indeed a fatiguing event, we conducted a one-way analysis of variance (ANOVA) examining the effect of condition on current fatigue. The results revealed a significant main effect of condition on fatigue, F(2, 132) = 4.44, MSE = 1.94, p = .014, η² = 0.06. Based on recommendations to use planned contrasts to test specific research questions (Rosenthal & Rosnow, 1985; Steiger, 2004), we conducted a planned contrast to examine whether participants in the currently enduring condition (+2) reported significantly higher levels of fatigue than did participants in the never-endured (−1) or previously endured (−1) conditions. The results revealed that participants in the currently enduring condition reported significantly higher levels of fatigue (M = 4.79, SD = 1.08) than did participants in the never-endured (M = 4.05, SD = 1.57) or previously endured (M = 4.03, SD = 1.47) conditions, t(132) = 2.95, p = .004, 95% CI [0.49, 2.50], d = 0.51, indicating that our manipulation was successful.

Next, to examine the dependent measures, we conducted a series of one-way ANOVAs with planned contrasts. Specifically, planned contrasts examined whether participants in the previously endured condition (−2) gave the employee significantly less favorable evaluations and were less willing to rehire the employee than were participants in the never-endured (+1) or currently enduring conditions (+1).

Employee evaluations. A one-way ANOVA revealed that competence evaluations varied significantly by condition, F(2, 132) = 5.63, p = .005, η² = 0.079. In line with our predictions, the
planned contrast revealed that participants in the previously endured condition rated the employee as significantly less competent ($M = 41.86, SD = 18.84$) than did participants in the never-endured condition ($M = 47.37, SD = 15.97$) and the currently enduring condition ($M = 54.29, SD = 15.63$), $t(132) = 2.67, p = .008, 95\% CI [4.67, 31.22], d = 0.46$.

**Willingness to rehire.** There was also a significant effect of condition on willingness to renew the contract, $F(2, 132) = 3.96, MSE = 0.86, p = .021, \eta^2 = 0.059$. As predicted, the planned contrast revealed that participants in the previously endured condition were significantly less willing to rehire the employee ($M = 4.36, SD = 0.79$) than were participants in the never-endured condition ($M = 4.71, SD = 0.89$) and the currently enduring condition ($M = 4.98, SD = 1.05$), $t(132) = 2.52, p = .013, 95\% CI [0.20, 1.68], d = 0.43$.

Taken together, the results of Study 2 support our predictions. Participants in the previously endured condition were less willing to rehire and provided less favorable evaluations of the employee who struggled with the test than did those in the never-endured and currently enduring conditions. It is important that this difference persisted when including the currently enduring condition because it rules out an information account for this effect; participants in the currently enduring condition completed the same test as did the participants who previously endured the test (and thus possessed similar knowledge about the test), but only the former made their evaluations while actively experiencing the associated mental fatigue.

### Study 3

In the first two studies, we found that, across two different events, people who had previously endured a distressing event evaluated another’s failure to endure that event more negatively than did those who had never endured (or were currently enduring) that event. The goal of Study 3 was to examine the process that drives this effect. Prior research has found that people have a constrained memory for affective states, and once a state is overcome, they can no longer appreciate its motivational force (Nordgren et al., 2006). Moreover, unlike people who have not endured or are currently enduring a distressing event, people who have endured a distressing event might use the knowledge that they managed to endure the event when they make assessments of another’s struggles to endure. We predict that the combination of a constrained memory for the impact of past emotional distress and knowledge of one’s own completion of the event makes the event seem easier to overcome, which, in turn, makes struggling with the event appear more blameworthy (Alicke, 2000). Based on this reasoning, we predicted that the perceived difficulty of overcoming the emotionally distressing event would mediate the effect of enduring the event on evaluations of another’s failure to endure.

We tested these predictions in the context of unemployment, a life event in which a person is actively searching for employment but is unable to find work. A large body of research has accumulated to suggest that unemployment is an emotionally distressing life event (e.g., Grossi, 1999; McKee-Ryan, Song, Wanberg, & Kinicki, 2005). Thus, in Study 3, participants in varying states of unemployment (currently enduring vs. previously endured vs. never endured) read a scenario about a man who begins selling drugs because he is unable to find work. Participants then rated how much compassion they felt for the unemployed individual and provided general evaluations of the individual.

### Method

**Participants.** Participants were 227 American residents drawn from Amazon’s Mechanical Turk ($M_{age} = 33.37, SD_{age} = 11.82$; 44% female; 78% Caucasian, 8% Asian American/Asian, 5% Hispanic, 5% African American/Black, 4% Other).

**Procedure.** Participants were recruited for the study on the basis of their responses to a preselection survey. The currently enduring group consisted of involuntarily unemployed people actively seeking employment. The previously endured group consisted of people who are currently employed, but had previously been unemployed while actively searching for employment. Finally, the never-endured group consisted of people who are currently employed and have never experienced an interruption in their employment since their first job. During the study, participants read about a man struggling to make ends meet. The man had attempted to apply to many jobs, but with no luck. Though he was motivated to persist and find gainful employment, he felt distressed by the constant rejection and lack of progress. One day, the man was approached by a friend who needed help selling small quantities of illegal drugs. Fed up with dealing with unemployment, he immediately took the offer and ended his search for (legal) employment. After reading the vignette, participants then completed the dependent measures and control variables.

### Measures

**Compassion.** Participants indicated the extent to which they experienced compassion toward the man, as in Study 1.

**General evaluations.** Participants also completed measures that assessed their general attitudes toward the unemployed individual. Specifically, participants rated the man on four evaluative dimensions (good, bad; positive, negative; like, dislike; and desirable, undesirable) on 7-point scales (Cronbach’s $\alpha = .91$; Nordgren et al., 2007).

**Perceived difficulty.** Participants completed an item designed to assess the perceived difficulty of unemployment (“It is difficult to overcome being unemployed”) on a scale from 1 (completely disagree) to 7 (completely agree).

**Control variables.** In addition to measuring employment status, we assessed relevant control variables: political orientation, socioeconomic status, geographic location, education, and demographic variables. It is possible that factors such as political orientation are related to both employment status and evaluations of the unemployed. Participants in the previously endured and currently enduring conditions also completed items assessing the length and frequency of the job search (e.g., “How long ago were you unemployed?”/“How long have you been unemployed?”), and the activeness of their job search while unemployed (number of resumes sent, intensity of job search). Participants in the never-endured and previously endured conditions indicated whether they were employed full or part time, and the occupational category of their job.
Results and Discussion

As in Study 2, we conducted a series of one-way ANOVAs, followed by planned contrasts examining whether participants in the previously endured condition (−2) reported significantly less compassion, less favorable evaluations of the individual, and lower perceived difficulty to overcome unemployment than did participants in the never-endured (+1) or currently enduring (+1) conditions.

Compassion. A one-way ANOVA revealed that compassion varied by unemployment group, F(2, 224) = 6.20, MSE = 2.00, p = .002, η² = .052. The planned contrast demonstrated that participants who previously endured unemployment experienced significantly less compassion toward the individual (M = 4.36, SD = 1.54) than did participants who never endured unemployment (M = 4.78, SD = 1.37), and participants currently experiencing unemployment (M = 5.15, SD = 1.31), t(224) = 3.10, p = .002, 95% CI [0.44, 1.98], d = 0.41.

Evaluations. Likewise, ratings of the individual varied significantly by unemployment group, F(2, 224) = 6.99, MSE = 1.34, p = .001, η² = .059. The planned contrast revealed that participants who previously endured unemployment made less favorable evaluations of the individual (M = 3.87, SD = 1.08) than did participants who never endured (M = 4.22, SD = 1.21) and were currently enduring (M = 4.56, SD = 1.19) unemployment, t(224) = 3.27, p = .001, 95% CI [0.41, 1.67], d = 0.43.

Perceived difficulty. There was also a significant effect of unemployment group on the perceived difficulty of overcoming unemployment, F(2, 224) = 3.71, MSE = 2.75, p = .026, η² = .03. As predicted, participants in the previously endured group thought unemployment was less difficult to overcome (M = 4.60, SD = 1.62) than did never-endured participants (M = 5.01, SD = 1.84) and currently enduring participants (M = 5.48, SD = 1.48), t(224) = 2.35, p = .028, 95% CI [0.07, 1.87], d = 0.33.

Mediation. We also tested whether the perceived difficulty of overcoming unemployment mediated the negative effect of enduring the event on compassion. A bootstrapping analysis of mediation (Preacher & Hayes, 2008) with 10,000 resamples with replacement indicated a significant indirect effect, with a point estimate of .07 and a 95% bias-corrected confidence interval of [0.006, 0.196]. This suggests that decreased perceived difficulty of overcoming unemployment mediated the effect of previously enduring on compassion. Participants who previously endured unemployment were less compassionate because they thought unemployment was less difficult to overcome.

Next, focusing on evaluations of the unemployed individual, the bootstrapping analysis estimated the indirect effect to be .15, with a 95% bias corrected interval of [0.06, 0.29]. Again, this interval excluded zero, suggesting that the perceived difficulty of overcoming unemployment also mediated the effect of overcoming unemployment on the negativity of evaluations of the individual.

Control variables. When examining the control variables, only length of unemployment for participants currently enduring unemployment was significantly related to compassion, β = .22, t(74) = 1.96, p = .054, with longer times since last employment predicting increased compassion for the individual. It is likely that the frustrations and hardships associated with unemployment develop over time, increasing its perceived difficulty, and thus increasing compassionate feelings toward an individual struggling with unemployment. We therefore conducted supplementary analyses to investigate whether the effects of unemployment group were limited to participants who had been unemployed for a long time. To do so, we tested whether length of unemployment moderated the effect of unemployment status on compassion and evaluations of the individual. Specifically, we employed moderated regression analyses and regressed the dependent measures onto length of unemployment, unemployment group (+1 = previously endured; −1 = currently enduring), and their interaction. Only unemployment group was a significant predictor of compassion (β = −.21, p = .019) and evaluations of the unemployed individual (β = −.25, p = .006). The other effects were nonsignificant, including the interactions (βcompassion = −.14, p = .22; βevaluations = −.13, p = .262), indicating that our results hold regardless of participants’ length of unemployment.

Taken together, the results of Study 3 further support our predictions. The mediated relationship gives support to our explanation for the effect of enduring the event on the evaluation of another’s failure to endure. We found that individuals who previously endured unemployment believed that unemployment was less difficult to overcome, and this, in turn, predicted reduced compassion and more negative evaluations of an individual who commits illegal acts in response to unemployment. The results of Study 3 again help to rule out an information account. Participants who previously endured unemployment made less favorable evaluations than did currently enduring and never-endured participants. If enduring unemployment simply increased participants’ knowledge that the event was not as difficult as predicted, we would expect both currently and previously unemployed participants to both make less favorable evaluations.

Study 4

Study 4 served two main goals. First, we have proposed that enduring an emotionally distressing event can lead to negative evaluations of individuals who fail to endure a similar event. Integrating the current framework with past research on experience and sympathy (e.g., Batson et al., 1996), we propose that an important boundary condition of the current finding is the perceived failure of another individual to adequately endure a distressing event. Specifically, we predict that past research suggesting that prior experience increases sympathy holds for perceptions of individuals currently enduring the event, whereas the current effect exists for the perceptions of individuals perceived to struggle or fail to endure the event. Having endured distress may facilitate the perspective-taking and self-simulation required for sympathy (e.g., Bandura, 1969; Batson, 1987, 1991), but lead to the underestimation of the impact of the affective states experienced by another who fails to endure the distressing event. To test these predictions, participants who had either never or previously endured bullying evaluated a bullied teen who either violently aggresses against the perpetrators and nearby innocent students (bullying-induced failure condition) or continues to endure the experience (managing-to-endure condition). “Fighting back” and

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3 Only participants in the currently enduring and previously endured unemployment groups (n = 160; 70% of the sample) were included in these analyses, as these were the only groups for whom length of unemployment was an appropriate measure.
lash out against others is largely assumed to be a maladaptive means of coping (Schwartz, 2000; Zapf & Gross, 2001). Consistent with this notion, a pretest of the vignettes revealed that participants rated the teen in the bullying-induced failure vignette as coping significantly worse with the emotional distress compared with the teen in the managing-to-endure vignette.4

Second, Study 4 served to rule out an alternative explanation. We have argued that individuals who have previously endured an emotionally distressing event will form more negative evaluations of failures to endure a similar distressing event. One alternative explanation is that overcoming a distressing event shapes general evaluations of failure to endure (e.g., decreased tolerance for “giving up”) rather than failure induced by the distressing event in particular. In other words, having endured bullying may decrease tolerance for failures to endure, regardless of the cause of that distress, perhaps by increasing the perceived importance of resilience. One way to rule out this alternative explanation is to examine the specificity of this effect. To do so, we added a control condition: grief-induced failure. If enduring bullying only influences the evaluation of a corresponding failure to endure distress (i.e., bullying-induced failure), and does not influence evaluations of unrelated failures to endure (e.g., grief-induced failure), then it would support our explanation.

We tested these predictions by randomly assigning participants to one of three conditions (bullying-induced failure vs. managing-to-endure vs. grief-induced failure) in a between-subjects design. The primary dependent measures were participants’ evaluations of and compassion toward the other. We predicted that, compared with those who never endured bullying, participants who had previously endured bullying would feel more positively about and more compassion toward the bullied teen in the managing-to-endure condition, but feel less positively about and less compassion toward the teen in the bullying-induced failure condition. We predicted that participants’ experience enduring bullying would not influence evaluations of grief-induced failure.

Participants

Participants were 323 individuals (M = 31.63 years, SD = 11.38 years; 42% female; ethnicity not reported) recruited via Amazon’s Mechanical Turk.

Procedure

Prior to the study, participants responded to a questionnaire regarding their history with bullying. Participants were sorted into previously endured and never-endured groups based on their responses to a dichotomous measure assessing history of bullying (“Were you ever bullied or teased?”; McCabe, Antony, Sommerfeldt, Liss, & Swinson, 2003). Participants also indicated the length, severity, and frequency of bullying to provide convergent evidence for this measure.

During the study, participants read a vignette about a high school student. In the bullying-induced failure and managing-to-endure conditions, the student had recently moved to a new high school and had been repeatedly teased and bullied by the other students. The student experienced daily feelings of hurt and humiliation. One day at the cafeteria, a group of bullies began to publicly tease the student in the cafeteria and threw a half-eaten apple at him. In the bullying-induced failure condition, following the provocation, the student lost control over his emotions and behaved violently toward the bullies and other nearby students. Several students were hurt and needed to receive medical attention.

In the managing-to-endure condition, the student was hurt by the experience, but continued to endure the emotional and physical pain associated with bullying. In the grief-induced failure condition, the student commits the same acts of aggression as in the bullying-induced failure condition, but the aggression is said to stem from the grief associated with the loss of a sibling. Following the vignette, participants completed the dependent measures and control variables. Finally, participants answered demographic questions.

Materials

Compassion. Participants completed the compassion measures as in Studies 1 and 3.

General evaluations. Participants also completed measures that assessed their general evaluations of the bullied student as in Study 3.

Control variable. In addition to the primary dependent measures, we assessed the severity, length, and frequency of participants’ experience with bullying. Severity of bullying was measured on a scale from 1 (I wasn’t bullied at all) to 5 (extremely serious), and frequency of bullying was measured on a scale from 1 (never bullied) to 7 (constantly bullied). Participants indicated length of bullying by responding to an open-ended question (“For how long were you bullied?”).

Given that individuals who previously endured bullying, but responded similarly to the struggling protagonist, may not be as punitive as those who responded in a less aggressive manner, we assessed participants’ own reactions to bullying. To do so, participants completed the Retrospective Bullying Questionnaire (Schäfer et al., 2004), which assessed which of 10 strategies they had used to cope with bullying, if any (e.g., “I tried to avoid the situation,” “I got help from family/parents,” “I fought back”). Participants were permitted to select more than one strategy. To ensure that no participants were currently enduring bullying, participants indicated whether or not they were currently being bullied on a dichotomous yes–no item, and the frequency of current bullying from 0 (not bullied) to 5 (daily). Only participants who indicated no current bullying were included in the study.

Results and Discussion

First, to provide support for the validity of our dichotomous measure of bullying, we analyzed whether severity, and frequency of bullying differed depending on participants’ responses to the dichotomous measure (cf. Schäfer et al., 2004). Participants who

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4 A pretest examined whether the teen was viewed as differentially enduring the distress in the failure versus managing-to-endure conditions. Sixty-six online participants read both scenarios and then completed three items assessing perceived coping (e.g., “He is handling the situation as best he can”) on scales from 1 (strongly disagree) to 7 (strongly agree). The results indicated that participants viewed the teen in the failure condition as coping significantly worse with the distress (M = 2.48, SD = 1.12), compared with the teen in the managing-to-endure condition (M = 4.15, SD = 1.10), t(65) = −13.59, p < .001, d = 3.37.
responded “yes” to the dichotomous bullying measure indicated that they were bullied more frequently (M = 3.09, SD = 0.72) than did those who responded “no” (M = 1.35, SD = 0.55), t(320) = 24.07, p < .001, 95% CI [1.59, 1.88], d = 2.69, and also indicated that they were bullied more severely (M = 3.40, SD = 0.73) than did those who responded “no” (M = 2.13, SD = 1.32), t(320) = 10.96, p < .001, 95% CI [1.05, 1.50], d = 1.23.

**Compassion.** A 2 (bullying history: previously endured vs. never endured) × 3 (scenario: bullying-induced failure vs. managing-to-endure vs. grief-induced failure) ANOVA revealed a significant main effect of scenario condition, F(2, 317) = 48.64, MSE = 2.75, p < .001, η² = .235, and no main effect of bullying history, F(1, 319) = 0.10, MSE = 2.75, p = .752, η² < .01. This was qualified, however, by the predicted Bullying History × Scenario interaction, F(2, 317) = 3.22, MSE = 2.75, p = .041, η² = .020 (see Figure 1). Within the bullying-induced failure condition, participants who previously endured bullying were significantly less compassionate toward the student (M = 4.89, SD = 1.35) than were those who never endured bullying (M = 5.43, SD = 1.11), F(1, 319) = –2.16, p = .033, 95% CI [–1.03, –0.05], d = 0.44. By contrast, within the managing-to-endure condition, participants who previously endured bullying were marginally more compassionate toward the student (M = 5.98, SD = 1.02) than were those who never endured bullying (M = 5.68, SD = 1.03), F(1, 319) = 1.66, p = .099, 95% CI [–0.06, 0.65], d = 0.29. As predicted, no significant differences emerged between previously endured participants (M = 4.15, SD = 1.38) and never-endured participants (M = 4.05, SD = 1.81) in compassion toward the student in the grief-induced failure condition, F(1, 317) = 0.31, p = .76, 95% CI [–0.57, 0.78], d = 0.06.

**General evaluations.** A 2 (bullying history: previously endured vs. never endured) × 3 (scenario: bullying-induced failure vs. managing-to-endure vs. grief-induced failure) ANOVA revealed a significant main effect of scenario condition, F(2, 317) = 103.46, MSE = 1.08, p < .001, η² = .395, indicating more positive evaluations of the student who did not aggress against others, and no main effect of bullying history, F(1, 317) = 0.28, MSE = 1.08, p = .595, η² = .001. This was qualified, however, by the predicted Bullying History × Scenario interaction, F(2, 317) = 4.52, MSE = 1.08, p = .012, η² = .028. Within the bullying-induced failure condition, participants who previously endured bullying made marginally less favorable evaluations of the student (M = 4.20, SD = 1.25) than did those who never endured bullying (M = 4.62, SD = 0.91), F(1, 317) = –1.95, p = .054, 95% CI [–0.34, 0.01], d = 0.38. By contrast, within the managing-to-endure condition, participants who endured bullying made significantly more favorable evaluations of the student (M = 5.51, SD = 1.01) than did those who never endured bullying (M = 5.12, SD = 0.98), F(1, 317) = 2.24, p = .027, 95% CI [0.04, 0.83], d = 0.39. Again, no significant differences emerged between previously endured participants (M = 3.36, SD = 1.01) and never-endured participants (M = 3.15, SD = 1.00) in compassion toward the student in the grief-induced failure condition, F(1, 317) = 1.00, p = .321, 95% CI [–0.21, 0.64], d = 0.20.

**Control variables.** In assessing the control variables, gender was a significant predictor of compassion, F(1, 321) = 10.66, MSE = 2.07, p < .001, η² = .032, with females reporting higher levels of compassion for the bullied student. This finding is consistent with some past research (e.g., Batson et al., 1996; Eisenberg & Lennon, 1983). However, an ANOVA examining the effect of gender, bullying history, and condition on compassion revealed no significant interaction with gender, F3-way interaction(2, 311) = 1.02, MSE = 1.57, p = .360, η² = .007, nor did controlling for gender change the pattern of results.

We also conducted supplemental analyses examining the effects of participants’ self-reported coping styles. Specifically, we examined the effects of participants’ own retaliation (fought back = 1; did not fight back = 0), scenario condition, and their interaction on compassion and evaluations of the teen in a two-way ANOVA.5 Only a significant main effect of scenario condition emerged to predict compassion, F(2, 170) = 27.11, MSE = 1.53, p < .001, η² = .242, and evaluations of the teen, F(2, 170) = 42.92, MSE = 1.18, p < .001, η² = .336. The Scenario × Retaliation interactions did not reach significance, Fretaliation(2, 170) = 2.02, MSE = 1.53, p = .136, η² = .023, and Fretaliation × Scenario(2, 170) = 1.69, MSE = 1.18, p = .187, η² = .020, and there were no main effects of retaliation.6 These null effects are perhaps related to the small number of participants who indicated that they fought back across scenario conditions (n = 46). This is a point to which we return in the General Discussion section.

The results of Study 4 provide evidence for an important boundary condition of the effect: the perceived failure of another to endure the distressing event. When evaluating an individual enduring an emotionally distressing event, having endured the event facilitates compassion, likely by facilitating the ease with which the other’s perspective is adopted (“I’ve been there too”; Batson, 1987, 1991; Batson et al., 1996). When evaluating an individual who fails to endure the distressing event, having previously endured the event may reduce compassion because the event seems less difficult to overcome in hindsight. In turn, others’ struggles to adequately endure the event seem unacceptable. Importantly, the

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5 As in Study 3, only participants who had previously endured bullying (n = 176; 55% of the sample) were included in these analyses, as these were the only groups for whom coping strategies was an appropriate measure.

6 None of the other coping strategies yielded significant main effects or interactions on the dependent measures.

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Figure 1. Mean compassion as a function of bullying history and scenario condition (± SE) in Study 4.
results of Study 4 also rule out the alternative explanation that previously enduring distress shapes evaluations of failures to endure broadly, and instead supports our prediction that enduring distress influences the evaluation of a corresponding failure to endure.

**Study 5**

In Study 5, we sought to test whether people’s lay beliefs would match our pattern of data. In other words, would people intuitively understand the results of the current study, or would they mistakenly believe that individuals who have previously endured a distressing event would more favorably evaluate failures to endure? In many cases, shared experiences do foster sympathy, as when perceivers are actively experiencing the emotional distress (Studies 2 and 3), or the target is perceived to endure the distress (Study 4). Because people who have endured emotional distress have “been there, too,” observers may mistakenly predict that people who have endured a distressing event would render more favorable evaluations of failures to endure than would those who have not endured this experience.

To test this possibility, we described the bullying-induced failure scenario from Study 4 to a new group of participants. These participants then predicted which of two teachers would be less likely to penalize the bullied teen’s violence: a teacher who managed to endure bullying at a similar age or a teacher who never endured bullying.

**Method**

**Participants.** Participants were 112 individuals (M_{age} = 31.63 years, SD_{age} = 10.01 years; 67% male; ethnicity not reported) recruited via Amazon’s Mechanical Turk.

**Procedure.** Online participants enrolled in a study ostensibly about interpersonal decision making. Participants were presented with descriptions of scenarios, and were asked three questions about their decision making in these scenarios (e.g., “Which of the following individuals would you be more likely to approach for information about jazz music? Person A is an expert in classical music, and Person B is an expert in folk music”). Embedded in these scenarios was the critical measure. Specifically, participants were provided with the bullying-induced failure vignette from Study 4. Following the vignette, participants were informed that the bullied teen would now face one of two teachers following his actions: Teacher A, who “overcame bullying as a teen,” or Teacher B, who “has had no previous experience with bullying.” Participants were then asked to indicate which teacher the teen should approach for music, and Person B is an expert in folk music”). Embedded in these scenarios was the critical measure. Specifically, participants were provided with the bullying-induced failure vignette from Study 4. Following the vignette, participants were informed that the bullied teen would now face one of two teachers following his actions: Teacher A, who “understood the results of the current study, or would they mistak-}

**Results and Discussion**

As predicted, most participants (99 out of 112) predicted that the teacher who had previously endured bullying would more favorably evaluate the teen’s failure to endure, χ^2(1, N = 112) = 66.04, p < .001, w = .77. Most participants who previously endured bullying (42 out of 45), χ^2(1, N = 45) = 33.80, p < .001, w = .87, and who never endured bullying (57 out of 67) were more likely to select the teacher who previously endured bullying, χ^2(1, N = 67) = 32.97, p < .001, w = .70. There were no significant differences in teacher selection between these groups, χ^2(1, N = 112) = 1.79, p = .181, w = .13. These results suggest that people misinterpret the extent to which those who have endured a distressing event will be more compassionate toward another’s failure to endure distress.

**General Discussion**

When struggling to endure an emotionally distressing event, it would seem that people should turn to those who have endured that same experience. Yet it appears that this intuition may be misguided, as it fails to take into account the psychological consequences of enduring these distressing events. Across five studies, we demonstrated that previously enduring an emotionally distressing event led to more negative evaluations of those who failed to endure a similar emotionally distressing event. In Study 1, people who previously completed a polar plunge felt less compassion and more contempt for an individual who failed to complete the polar plunge. Study 2 ruled out a critical alternative explanation—that participants who endured the distress are simply more knowledgeable about the experience—and found that participants who previously endured a fatiguing test more negatively evaluated another’s failure to endure that test than did participants who had never endured or were currently enduring the test.

Building on these initial results, Study 3 explored the process behind this effect, finding that participants who had previously endured unemployment penalized another’s failure to endure unemployment because they viewed this experience as less difficult to overcome. Moreover, Study 4 revealed that the effect is specific to evaluations of failures to endure: Compared with participants who had never endured an emotionally distressing event, participants who had endured the event made less favorable evaluations of an individual failing to endure, but made more favorable evaluations of an individual who was managing to endure, the event. Finally, in Study 5, observers failed to anticipate this effect of enduring distressing events, instead believing that someone who had endured distress would be most lenient toward failures to endure. By testing our predictions across a wide range of distressing events and instances of struggle and failure (from merely opting out of a polar plunge to engaging in illegal behavior), we have provided evidence for the generalizability of this effect. The current research suggests that experience offers a powerful tool for predicting and understanding who will be the most sympathetic in the face of failure to endure distress.

**Theoretical Implications**

Our results have important theoretical implications for the empathy gap perspective. Typically, research in this paradigm compares participants in either a “hot state” (e.g., experiencing pain) or a “cold state” (e.g., people not experiencing pain) along measures such as empathy toward someone experiencing a similar situation. Such an approach conflates the effects of emotional states (feeling
pain) with knowledge about the experience (knowing what the painful experience was like). Thus, it has remained unclear how previously enduring a hot state (i.e., knowledge without the accompanying emotional state) would affect feelings toward another facing a similar situation. The current research provides initial evidence for the importance of disentangling these factors by suggesting that actively shared emotional experience is critical to the formation of positive interpersonal judgments, whereas knowledge without emotion may, in some cases, have negative consequences for interpersonal judgment. Of course, people who have, for example, overcome unemployment may possess different knowledge than do those who are currently unemployed, and thus future research should continue to explore the orthogonal effects of emotional states and experiential knowledge on sympathy for others’ struggles.

In highlighting the importance of decoupling emotional states from knowledge, the current research also complements recent work proposing a desensitization bias in emotional perspective-taking (Campbell et al., 2014). In Campbell et al.’s (2014) research, repeated exposure to an emotion-inducing stimulus (e.g., hearing the same joke a number of times) reduced empathic accuracy for others’ reactions to that stimulus (e.g., predicting how funny others will find the joke) by attenuating participants’ own emotional reactions. Though examining different aspects of experience (too much experience vs. having endured an experience) and different consequences (empathic accuracy vs. evaluations of failure), both approaches underscore the importance of actively experiencing emotion in forming interpersonal judgments.

Outside of the empathy gap perspective, other literatures examining shared experiences could benefit from exploring the distinction between currently and previously enduring events. Given the impact that shared experiences have on a diverse array of phenomena, such as memory (Hirst, Manier, & Cuc, 2003; Weldon, 2001), cognition (Barsalou, Niedenthal, Barbey, & Ruppert, 2003), and communication in groups (Levine & Higgins, 2001), it may be important for future research to examine the potentially divergent effects of currently and previously shared experiences on other outcomes. For example, on-the-job training programs are shared experiences used to socialize new individuals into organizations. If these programs are led by individuals who have long ago completed the program, these leaders may now underestimate the difficulty of acquiring the skills taught by the program, leading them to produce a deprived learning experience.

Limitations and Issues for Future Research

Despite the advances made by the current studies, there are remaining questions that should be addressed in future research. First, we examined the effect of enduring distress on evaluations among individuals who managed to endure the event (i.e., completed the exam, entered the icy water). It remains unclear how individuals with prior experience, but who failed to endure the distress, would respond. One possibility is that failing to endure the event increases compassion by increasing the perceived difficulty of overcoming that event (e.g., Zuckerman, 1979). Consistent with this suggestion, participants in Study 4 who also responded to bullying with violence were marginally more likely to be compassionate toward the bullying-induced failure. We are, however, reluctant to make conclusions based on these data, given the small number of participants who reported that they had fought back.

A potential counterpoint to this prediction is that even if an individual previously struggled to endure a distressing event, she may recall her ability to endure the event in a more favorable light than reflects reality. Research has accumulated to suggest that people tend to distort negative autobiographical information to become more positive and self-enhancing over time (D’Argembeau & Van der Linden, 2008; Gramzow & Willard, 2006; Mather & Carstensen, 2005). Thus, outside of clear, unambiguous failures to endure (e.g., not completing a comprehensive exam), motivated reasoning may prevent the accurate recall of one’s own difficulty with the event. In turn, reduced compassion for those who fail to endure the event may persist.

As a practical matter, we also note that, in many situations, distinguishing between unsuccessful and successful prior experience groups presents some complications. Namely, those who have failed to endure the distressing event often cannot be distinguished from those currently enduring the event; individuals who fail to overcome unemployment remain unemployed, and those who fail to quit smoking are still smokers and experience the associated cravings. Future research should nonetheless seek to tease apart the effects of prior experience and success on evaluations.

The current research likewise generates questions about how aspects of the experience itself influence evaluations of failures to endure. Given that the perceived ease with which the event can be overcome seems to drive the effect, manipulating aspects of the experience that affect recall of the distress may affect failure evaluations. For example, Kahneman, Fredrickson, Schreiber, and Redelmeier (1993) found that adding a few moments of less intense pain near the end of a distressing event can decrease retrospective evaluations of the overall intensity of the pain. Based on our theorizing, we would anticipate that people who experienced less distress near the end of an event may be more prone to penalize another’s failure to endure compared with people who experienced constant (or increased) distress near the end of an event. Importantly, these results would also suggest that individual differences in habituation versus sensitization to emotional experiences might be an important predictor of compassion toward perceived failure to endure (Campbell et al., 2014).

Finally, the current findings might be used to shed light on an important question in the political sphere: How do people from marginalized groups respond to members of their community once they have achieved success? Previous work addressing this question has suggested that members of marginalized communities (e.g., women, Black Americans) who achieve political or corporate success often espouse a conservative ideology that downplays structural barriers to success for others who share their identity (Zweigenhaft & Domhoff, 1998). Although this phenomenon has been attributed to strategic positioning on the part of elites (e.g., Lewis, 2013), the current research would also suggest that diluted memories of struggling to overcome barriers combined with knowledge of their own success leads successful members of oppressed groups to become more receptive to the conservative individualist ideology that emphasize individual agency over structural constraint. Given the continued importance of this question, future work should explore potential empathy gaps among members of minority groups when they reach the top.
Applications

The current research has important implications for interpersonal dynamics, as well as policies regarding how people treat others facing distressing life events. Many social programs are designed with the input of those who endured distressing events, including rehab programs designed by former addicts, and social welfare programs crafted by individuals who used to live in poverty themselves.7

In this vein, a pressing direction for future work is the exploration of strategies that may combat the negative effects of enduring distress on compassion toward others’ struggles. The critical question is whether there are ways for people to maintain the empathy afforded by the distressing event once they return to a cold state. Perhaps this can be achieved by having people reflect and commit to their evaluations during an experience (e.g., writing down or verbalizing views during the search for employment) or by having people take notice of their own struggles during the experience. This approach aims to increase sympathetic responding to others’ failures by bridging the gap between hot and cold perspectives.

An alternative approach might be to encourage perceivers to place less emphasis on their own subjective experience with the event when assessing others’ failures. For example, a counselor who overcame addiction could consider other struggling patients or the large number of people who relapse when trying to forgive a patient’s transgressions. This approach acknowledges the difficulty inherent in reliving one’s own affective states, and instead facilitates conscious correction against potential bias (e.g., Wegener & Petty, 1995).

Conclusion

At the outset of this article, we posed a question: When struggling with an emotionally distressing event, such as divorce or the loss of a loved one, are people best served by seeking help from others with or without similar experiences? Despite widespread beliefs in the power of shared experience to facilitate compassion, we have shown that for people failing to endure the distress, those who have previously endured a similar experience may represent the toughest critics. This finding has important consequences for how people treat those in need, and who will be most likely to help the distressed. Future research may seek to unveil interventions that combat the negative effects of enduring distress on evaluations of failure.

7 As a caveat to these points, we note that those who have previously endured distressing events are not necessarily inaccurate in their perceptions of the experience itself. In many cases, people who have endured distress are correct in noting what is possible to achieve, such as the veteran polar plunger telling novices that the experience is manageable. Nonetheless, from the perspective of those struggling, it is beneficial to know from whom to seek help or comfort.

References


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The strength to face the facts: Self-regulation defends against defensive information processing

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A B S T R A C T

Five studies examined the impact of self-regulatory capacities on defensive information processing—the tendency to deny, distort, or avoid diagnostic self-threatening information. Across domains, we found that people low in trait (Studies 1 and 5) and state (Studies 2, 3, and 4) self-regulatory capacities were more likely to deny the validity and importance of negative feedback, and were less willing to seek improvement based on this information. Alternative explanations based on self-esteem and competence-based deficits were ruled out. Moreover, we demonstrated a boundary condition of this effect: For participants high in self-improvement motivation, reduced self-regulatory capacities did not affect defensive processing (Studies 4 and 5). Taken together, these results suggest that trait and state levels of self-regulatory capacities are a key factor in determining whether people engage in defensive information processing.

1. Introduction

People regularly receive helpful feedback about their liabilities and weaknesses. However, owning up to one’s shortcomings can be painful, and people often cope with this self-threatening information by engaging in defensive processes that function to reduce the threat (Kunda, 1990; Pyszczynski & Greenberg, 1987). For example, when receiving negative feedback about their intellectual performance, people often respond to this threat by downplaying the seriousness of the feedback or questioning the validity of the information (e.g., Ditto & Lopez, 1992; Kunda, 1987; Major, Spencer, Schmader, Wolfe, & Crocker, 1998). Although defensive processing provides immediate benefits by maintaining positive self-views, it comes at a significant cost. Defensive processing inhibits learning from important (albeit threatening) information and experiences (Aspinwall & Taylor, 1997; Chance, Norton, Gino, & Ariely, 2011; Ditto & Croyle, 1995; Klein & Weinstein, 1997). To illustrate, if an employee derogates or ignores negative feedback from her supervisor, she may avoid the immediate emotional costs associated with the self-threat, but will not take the necessary steps to address her weaknesses. By doing so, she may compromise her long-term job performance.

From this perspective, processing diagnostic negative feedback is akin to a self-control dilemma, in that it represents a tension between gaining information that enables long-term self-improvement versus protecting oneself from short-term emotional and cognitive costs (Banaji & Prentice, 1994; Crocker & Major, 1989; see also Trope, 1986). On the one hand, people are motivated to protect their views of themselves as intelligent, good, virtuous, and able to control important life outcomes (i.e., self-protection motives), and negative feedback casts doubt on this view (e.g., Higgins, 1987; Weiner, 1986). On the other hand, people must seek diagnostic information about themselves in order to learn from this information and adaptively alter their behavior (i.e., self-improvement motives), and defensive processing inhibits this learning (Green, Sedikides, Pinter, & Van Tongeren, 2009; Sherman & Hartson, 2011). Diagnostic negative feedback thus pits the value of long-term self-improvement against the immediate emotional costs of processing self-threats.

How, then, do people resolve this dilemma? In this paper, we address this question by investigating the role of self-regulation—the capacity to override one’s thoughts, feelings, and habitual patterns of behavior (e.g., Baumeister & Heatherton, 1996; Heatherton & Baumeister, 1996) – in the defensive processing of negative feedback. If it is the case that diagnostic negative feedback poses a self-control dilemma, then self-regulatory capacities should play a critical role in facilitating the processing of this information. Across five studies, we tested whether higher levels of self-regulatory capacities (as a trait or a state) predict reduced defensive information processing. We also examined a theoretically and practically important moderator of this effect: self-improvement motivation. We suggest that people who are...
highly motivated to self-improve can overcome deficits in self-regulatory capacities to more openly process negative feedback. Taken together, the current research contributes to our theoretical understanding of the role of self-regulation in defensive processing, and sheds light on recent discussions about the relationship between self-regulatory resources and motivation (Baumeister & Vohs, 2007; Inzlicht & Schmeichel, 2012; Inzlicht, Schmeichel, & Macrae, 2014). These findings also have implications for managers and policy makers interested in decreasing people’s resistance to negative feedback.

1.1. Self-regulation and defensive processing

Self-regulation is the capacity to override one’s thoughts, feelings, and habitual patterns of behavior, and to refrain from acting on undesired, impulsive behavioral tendencies (Baumeister & Heatherton, 1996; Heatherton & Baumeister, 1996; Tangney, Baumeister, & Boone, 2004). This capacity facilitates the enactment of a preferred course of action (e.g., weight loss) in the face of an immediate obstacle (e.g., tempting foods) (Carver & Scheier, 1990; Mischel, 1974, 1984; Mischel, Cantor, & Feldman, 1996; Muraven & Baumeister, 2000).

Self-regulatory capacities can vary both as a trait and as a state (Finkel & Campbell, 2001; Gailliot, Schmeichel, & Baumeister, 2006). Whereas trait self-control is conceptualized as a personality trait capturing the extent to which people can control their impulses across time and situations (e.g., Friese & Hofmann, 2009; Galla & Duckworth, 2015; Mischel, 1974), state self-regulatory strength captures an individual’s in-the-moment ability to control impulses. At the trait level, research has accumulated to suggest that self-control is a relatively stable individual difference over time, with lower trait self-control predicting behavioral and interpersonal problems, including obesity, substance abuse, impulsive buying, and criminality (see de Ridder, Lensvelt-Mulders, Finkenauer, Stok, & Baumeister, 2012 for review). With regard to state self-regulatory strength, one line of research suggests that regulatory capacities are limited (Baumeister & Heatherton, 1996; Muraven & Baumeister, 2000), and can be depleted by activities that require self-regulation, such as intellectual performance, self-presentation, and the control of thoughts, emotions, and behaviors (e.g., Schmeichel, Vohs, & Baumeister, 2003; Vohs, Baumeister, & Ciarocco, 2005; Vohs et al., 2008). In one classic study, participants who resisted the temptation to eat tempting cookies exhibited a decline in persistence on a subsequent mental challenge (Baumeister, Bratslavsky, Muraven, & Tice, 1998).

Just as people use self-regulation to avoid tempting foods, we propose that people likewise use self-regulation to process diagnostic negative feedback. Research on automatic egotism and the self-regulation of emotion support this idea. First, research on automatic egotism has demonstrated an automatic positive bias with respect to evaluations involving the self, such that people’s automatic, default information processing orientations are self-serving (Greenwald & Farnham, 2000; Paulhus & Levitt, 1987; see also Ditto et al., 1998). This self-serving information and knowledge is deeply embedded in individuals’ cognitive systems, and thus has a retrieval advantage, dominating self-threatening information processing when information-processing capacities are reduced (e.g., Paulhus, Graf, & Van Selst, 1989). In other words, overriding this automatic tendency requires cognitive resources. Given that the essence of self-regulation is the ability to modify a dominant response tendency (e.g., Schmeichel & Vohs, 2008; Schmeichel et al., 2003), self-regulatory capacities may facilitate the more effortful processing required to process feedback that has negative implications for the self.

Second, to learn from negative feedback, people must also be able to overcome the immediate affective costs associated with self-threats. As suggested by Loewenstein (2006), when deciding whether to seek information, people focus not only on the long-term consequences of possessing that information, but also on their short-term affective responses to that information. Critically, the latter often overwhelms the former for in-the-moment decisions (Kruger & Evans, 2009; Loewenstein, 1996). This theorizing would suggest that, in general, people will favor avoiding the negative affect associated with negative feedback, even at the expense of forgoing the potential long-term benefits. However, we suggest that self-regulatory capacities provide people with the ability to focus on long-term information gains by enhancing their ability to cope with negative affect, frustration, and anxiety in the short-term (e.g., Baumeister, Bratslavsky et al., 1998; Finkel & Campbell, 2001; Gailliot et al., 2006; Gramzow, Sedikides, Panter, & Insko, 2000; Tangney et al., 2004). Research has repeatedly found that depleting self-regulatory capacities produces subsequent failures at emotional control, such as inhibiting emotional reactions to films (e.g., Muraven, Tice, & Baumeister, 1998) or managing frustration and discouragement in the face of task failure (Vohs et al., 2008). Given that people recognize that diagnostic negative feedback will increase negative emotions such as shame and dejection (Trope, 1980; Trope & Brickman, 1973), people low in self-regulatory capacities may avoid the host of negative emotions that accompany attention to self-threatening information in the service of self-protection (e.g., Ashford, 1986; Fishbach & Labroo, 2007; Isen & Simmonds, 1978; Janis & Mann, 1979). Based on these lines of reasoning, we predicted that a high capacity for self-regulation, either as a state or a trait, would decrease the defensive processing of self-threatening information.

1.2. The role of self-improvement motivation

We have proposed that the processing of diagnostic negative feedback represents a self-control dilemma, such that people experience a tension between gaining information that enables long-term self-improvement versus protecting themselves from short-term affective and cognitive costs (Banaji & Prentice, 1994; Crocker & Major, 1989; Trope, 1986). Because people with reduced self-regulatory capacities have fewer psychological resources to overcome the costs associated with processing negative feedback, they may instead prioritize self-protection over self-improvement (Baumeister, Dale, & Sommer, 1998; Crocker & Major, 1989; Kuhl, 2001; Raghunathan & Trope, 2002; Trope & Pomerantz, 1998).

Because of the important role of self-regulation in resolving motivational conflicts (e.g., tempting foods vs. health goals; self-protection vs. self-improvement), scholars have also recently begun to consider the role of the relative strengths of the competing motivations in predicting self-regulatory success (Baumeister & Vohs, 2007; Inzlicht & Schmeichel, 2012). In their process model, Inzlicht and Schmeichel (2012) have proposed that self-regulatory depletion produces temporary shifts in both motivational orientation and attention, which in turn reduces performance on subsequent tasks requiring self-regulation. That is, people with reduced self-regulatory capacities are less motivated to engage in tasks that require effortful control, and are instead more motivated to engage in personally rewarding or enjoyable actions. From this perspective, increasing motivation related to a specific self-regulatory goal may be one way to overcome the effects of reduced self-regulatory resources (Baumeister & Vohs, 2007; Clarkson, Hirt, Jia, & Alexander, 2010; Inzlicht et al., 2014). Consistent with this theorizing, Muraven and Slessareva (2003) discovered that although engaging in self-regulation at
Time 1 did deplete self-regulatory abilities at Time 2, depletion was overcome when performance at Time 2 was incentivized with money.

Similar to cash incentives, the extent to which a person is currently striving toward a given self-regulatory goal may facilitate the motivation to overcome depletion (Baumeister & Vohs, 2007; Fishbach, Friedman, & Kruglanski, 2003). In the context of diagnostic negative feedback, to the extent that people are motivated by self-improvement, they may be able to compensate for reduced self-regulatory capacities in order to gain diagnostic information about their abilities, and better themselves in the future. In other words, at high levels of self-improvement motivation, people will be more likely to make cognitive or emotional sacrifices to achieve self-improvement. We therefore predicted that self-improvement motivation would serve as a boundary condition for the current effect, such that reduced self-regulatory capacities would not enhance defensive processing for participants with high levels of self-improvement motivation.

2. Overview of studies

Five studies tested the hypothesis that people’s self-regulatory capacities predict defensive information processing of negative feedback. In Studies 1 and 3, we examined whether people with higher self-regulatory capacities—either as a trait or a state—would evaluate negative feedback more favorably than would people with lower self-regulatory capacities, while controlling for self-esteem. Whereas Studies 1 and 3 assessed evaluations of feedback, Study 2 explored whether people’s self-regulatory capacities would also impact willingness to seek self-improving over self-protecting feedback. Study 3 then extended these results to behavioral outcomes, testing whether people with higher levels of self-regulatory capacities would also seek out more information about how to improve and demonstrate improved performance following negative feedback. Studies 4 and 5 then tested whether high levels of self-improvement motivation would reduce the effect of self-regulatory capacities on defensive processing.

3. Study 1

Study 1 examined whether individual differences in self-regulatory capacities (i.e., trait self-control) and the defensive processing of negative feedback are negatively related. Given that self-esteem is related to both trait self-control (e.g., Tangney et al., 2004) and reactions to self-threats (see Crocker, Lee, & Harman, 1998), it is possible that the proposed relationship between low self-control and increased defensive processing could be driven by differences in self-esteem. We therefore also examined whether trait self-control negatively predicts defensive processing independent of self-esteem.

3.1. Method

3.1.1. Participants

Participants were 154 individuals ($M_{age} = 32.01$ years; 50% female) recruited online via Amazon’s Mechanical Turk. We aimed for 170 participants ($85$ per cell), but 14 failed an attention check in which they were asked not to fill out a specific question, and two did not complete the study. These exclusion criteria were determined a priori (in this study and in all subsequent studies).

3.1.2. Procedure

Participants who completed the Self-Control Scale and the Rosenberg Self-Esteem Scale one week prior were invited to take part in the study. Upon agreeing to participate, participants completed items ostensibly assessing a fictitious trait called cognitive perceptual integration (CPI; Tesser & Paulhus, 1982), which was said to be predictive of academic and career success. In reality, participants completed open-ended interview questions, such as items from the Snowy Pictures Task (SPT; Ekstrom, French, Harman, & Dermer, 1976), which asks participants to identify a series of images obscured by complex patterns of visual noise. After their scores were ostensibly calculated, participants received feedback indicating that they had scored poorly (bottom 28%) (negative feedback condition) or quite well (top 28%) on the task (positive feedback condition). After receiving this feedback, they completed six items adapted from Tesser and Paulhus (1982) assessing the perceived importance of being high on CPI (e.g., “How important is the CPI test?”) and the validity of the test (e.g., “Do you think the CPI test is representative of your true abilities?”) on a 7-point scale ($\alpha = 0.85$). Scores were combined to serve as an index of CPI evaluations, with higher scores representing higher perceived validity. To see if our feedback manipulation was successful, participants also indicated how well they had performed on the CPI on a scale from −3 (quite poorly) to +3 (very well). Finally, participants completed a post-experiment questionnaire that included demographic measures and open-ended questions intended to gauge suspicion of the hypotheses (e.g., “What do you think the researchers were trying to find in this study?”).

3.2. Materials

3.2.1. Trait self-control

To assess trait self-control, participants completed the brief version of the Self-Control Scale (Tangney et al., 2004). The scale contains 13 items (e.g., “I am good at resisting temptation”) rated on a 5-point scale from 1 (Strongly disagree) to 5 (Strongly agree). After reverse scoring negatively worded items, scores were averaged to produce an index of trait self-control ($\alpha = 0.90$), with higher scores reflecting greater self-control.

3.2.2. Self-esteem

To assess self-esteem, participants completed the Rosenberg Self-Esteem Scale (Rosenberg, 1965). The scale contains 10 items (e.g., “On the whole, I am satisfied with myself”), answered on a scale from 1 (Strongly disagree) to 5 (Strongly agree). After reverse scoring negatively worded items, scores were averaged to produce an index of trait self-esteem ($\alpha = 0.94$), with higher scores reflecting greater self-esteem.

3.3. Results and discussion

3.3.1. Manipulation check

Participants in the positive feedback condition reported performing significantly better on the test ($M = 2.09$, $SD = 0.74$) than did participants in the negative feedback condition ($M = −2.26$, $SD = 0.95$), $t(152) = 27.78, p < 0.001, d = 4.48$, indicating that our feedback manipulation was successful.

3.3.2. Defensive processing

To assess defensive processing, we conducted moderated regression analyses, regressing participants’ ratings of the CPI test onto their centered continuous self-control score, condition (0 = negative feedback; 1 = positive feedback), and the interaction

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1 Specifically, participants for Part 2 of the study were recruited from a pool of 400 participants who completed the trait measures for Part 1. Whether or not participants completed Part 2 was not significantly related to trait self-control or self-esteem ($p = 0.138$ and 0.758, respectively).
between their centered self-control score and condition. Participants’ self-control was a significant, positive predictor of validity ratings, $b = 0.73$, $t(150) = 3.32$, $p = 0.001$, with higher levels of trait self-control predicting higher ratings of the cognitive test’s validity. Consistent with past research (e.g., Ditto & Lopez, 1992), feedback condition was also a significant, positive predictor of validity ratings, $b = 2.15$, $t(150) = 11.24$, $p < 0.001$, with participants in the positive feedback condition rating the test as significantly more valid compared with participants in the negative feedback condition. Consistent with our predictions, these main effects were qualified by a significant, negative interaction between feedback condition and self-control, $b = -0.71$, $t(150) = -2.34$, $p = 0.020$ (see Fig. 1). Feedback condition was more weakly related to validity ratings among participants higher in trait self-control compared with participants lower in trait self-control.

Following the recommendations of Spiller, Fitzsimons, Lynch, and McClelland (2013), we used the Johnson-Neyman technique to identify the range of self-control for which the simple effect of the manipulation was significant. This analysis revealed that participants were significantly more likely to rate positive feedback as more valid than negative feedback if their trait self-control score was 3.67 (out of 5) or lower ($b_{mn} = 1.14$, $SE = 0.47$, $p < 0.05$). At self-control scores greater than 3.67, participants tended to treat positive and negative feedback as more equally valid. A score of 3.67 represented a self-control score in the upper quartile, suggesting that fairly high trait self-control is required for balanced information processing.

Finally, to explore whether the relationship between self-control and defensive processing was independent of self-esteem, we then included participants’ trait self-esteem scores into the moderated regression analyses. These results revealed that all relationships remained significant when controlling for self-esteem ($p < 0.03$). Self-esteem therefore does not seem to account for the relationship between trait self-control and defensive processing.

These results suggest that, at higher levels of trait self-control, participants were less likely to derogate the source of negative feedback. We reason that at higher levels of self-control, people were more likely to prioritize long-term self-improvement over the avoidance of the short-term affective costs of threatening information, which facilitated less defensive processing of that information.

### 4. Study 2

Study 2 served two main goals. First, Study 2 manipulated self-regulatory capacities to demonstrate a causal relationship between decreased self-regulatory capacities and increased defensive processing. Second, in Study 1, we examined the derogation of negative versus positive feedback. Another important aspect of defensive processing is the avoidance of negative feedback (Ashford, Blatt, & Walle, 2003; Trope & Pomerantz, 1998). Study 2 therefore tested whether self-regulatory capacities would also affect people’s willingness to seek diagnostic, but self-threatening feedback.

#### 4.1. Method

**4.1.1. Participants**

Participants were 170 individuals recruited online via Amazon’s Mechanical Turk. Because this study was the first experimental test using a new paradigm, we aimed for 85 participants per cell. Six participants were removed for not completing the depletion manipulation, and two for not completing the study. All subsequent analyses examine responses from the remaining 162 participants ($M_{age} = 35.18$ years; 61% female).

**4.1.2. Procedure**

Participants first completed a self-control depletion manipulation used in prior research (e.g., Baumeister, Dale et al., 1998; Job, Dweck, & Walton, 2010). Specifically, participants were told that they were completing two “stimulus detection tasks”, each lasting 5 min. In the first phase, all participants crossed out each letter $e$ on a page of typed text. This phase of the task is used to establish a behavioral pattern. In the second phase, participants in the no-depletion condition again crossed out every $e$. Participants in the depletion condition instead followed complex rules that required them to inhibit the previously established behavioral pattern (e.g., do not cross out an $e$ followed by a vowel). As a manipulation check, participants responded to four items assessing how much effort they exerted on the previous task on a 7-point scale (e.g., “How much effort did you exert on the previous task?” “How frustrating was the previous task?”) (Muraven, Shmueli, & Burkley, 2006).

Participants were then told to begin the second study, which was about organizational decision-making. In this section of this study, participants imagined that they were about to start a new job, and could choose to work with one of two managers. To aid the decision-making process, participants were provided with reviews of the managers from former employees. One of the managers represented the self-protecting choice, with former employees indicating that the manager provided lots of positive feedback, but very little information about weaknesses. This manager created a positive working environment, but may have limited the potential for growth (e.g., “There was much more of a focus on things I was doing well rather than on areas that needed improvement...[the manager] made us feel like star employees”). By contrast, the other manager represented the self-improving choice, with employees indicating that the manager focused on employees’ weaknesses, but provided little information about strengths. This manager provided a challenging work experience, but afforded opportunities for growth (e.g., “It was a tough 2 years, but I’ve grown”...“Although most of the time I received critical feedback, it was incredibly useful feedback”). These choices captured the tension of theoretical interest between self-protection and self-improvement, respectively. Participants’ choice of manager served as the primary dependent measure. Participants also indicated their preferences on a 7-point scale (“I would definitely...
select Manager X to work with") from 1 (Strongly disagree) to 7 (Strongly agree). We note that although the decision is hypothetical, research suggests that people can readily imagine experiencing the self-threat involved in critical feedback, and reactions to hypothetical self-threats have been found to replicate in the context of real threats (Gurmankin, Baron, & Armstrong, 2004; Gurmankin, Stopfer, Fels, & Armstrong, 2005; Sedikides & Green, 2004).

Finally, to examine whether the managers differentially conveyed self-protecting versus self-improving orientations, participants completed four items assessing their anticipated experience working with each manager (e.g., “I would learn a lot about myself working for Manager X”); “It would be an enjoyable experience to work with Manager X”) on a scale from 1 (Strongly disagree) to 7 (Strongly agree). Participants also completed a post-experiment questionnaire that included demographic measures and open-ended questions intended to gauge suspicion of the hypotheses (e.g., “what do you think the researchers were trying to find?”).

4.2. Results and discussion

4.2.1. Manipulation check

The four manipulation check items were combined to form a single index of depletion (α = 0.70). Participants in the depletion condition reported the letter e task to be significantly more challenging (M = 5.75, SD = 1.06) than did participants in the control condition (M = 5.21, SD = 1.08), t(160) = 3.21, p = 0.002, d = 0.50. These results suggest that the depleting letter e task required more effortful control than did the non-depleting task.

4.2.2. Manager evaluations

Our experimental materials were successful in conveying the self-protecting vs. self-improving orientations of each manager. Paired t-tests indicated that participants anticipated a significantly more positive experience if they worked for the self-protecting manager (M = 6.15, SD = 0.94) compared with the self-improving manager (M = 3.10, SD = 1.47), t(161) = 19.26, p < 0.001, d = 1.52. By contrast, participants anticipated significantly more potential for self-improvement working for the self-improving manager (M = 5.96, SD = 0.93) compared with the self-protecting manager (M = 3.62, SD = 1.28), t(161) = 14.86, p < 0.001, d = 1.17.

4.2.3. Manager selection

While in the control condition 61% of participants chose the self-improving manager, only 41% did so in the depletion condition, χ²(1, N = 162) = 6.27, p = 0.012, OR = 0.45. Likewise, in examining our continuous measure, participants in the depletion condition indicated significantly less interest in working with the self-improving manager (M = 3.90, SD = 2.03) compared with participants in the control condition (M = 4.63, SD = 2.05), t(160) = −2.29, p = 0.023, d = 0.36. Conversely, depleted participants were significantly more interested in working with the self-promoting manager (M = 5.13, SD = 1.84) than were control participants (M = 4.26, SD = 1.92), t(160) = 2.93, p = 0.004, d = 0.46.

In addition to low self-regulatory capacities predicting denial or distortion of negative feedback (Study 1), Study 2 found that participants with experimentally induced self-regulatory decrements were also less likely to pursue self-threatening feedback associated with self-improvement. These participants instead sought feedback that provided self-protection. This study provides further evidence that self-regulatory capacities enable people to put aside self-protective concerns in order to confront diagnostic negative feedback.

5. Study 3

Study 3 served two main goals. First, Studies 1 and 2 relied on self-report methodology. An important behavioral indicator of defensive processing is defensive disengagement from tasks on which one has received negative feedback—engaging in less effort to improve, seeking out less information about how to improve, and making fewer performance improvements (Armitage, Harris, Hepton, & Napper, 2008; Belding, Naufel, & Fujita, 2015; Howell & Shepperd, 2012; Nussbaum & Dweck, 2008; Robins & Beer, 2001). By contrast, people who have accepted the initial negative feedback tend to be more motivated to address their weaknesses and engage in remedial actions. In Study 3, we therefore sought to assess acceptance of negative feedback behaviorally by observing participants’ information-seeking behavior and performance improvements following negative (versus positive) feedback.

As a second aim, we sought to rule out a potential alternative explanation. Because the self-control depletion manipulation requires participants to override predominant response tendencies, it is possible that the manipulation decreases participants’ feelings of competence. This could, in turn, lead people to withdraw from the task to avoid feeling even less competent. We therefore also assessed feelings of competence to address this alternative explanation. We predicted that, among participants who received negative performance feedback, higher levels of self-regulatory capacities would predict more favorable evaluations of the task, increased information-seeking to improve performance, and increased performance on a follow-up test at time 2. By contrast, we predicted that self-regulatory depleted and non-depleted participants in the positive feedback condition would respond similarly because there is no self-threat for positive feedback participants.

5.1. Method

5.1.1. Participants

Participants were 180 undergraduates at a large Midwestern university (M_age = 20.59; 67% female; 41% identified as White; 34% Asian-American/Asian; 16% African-American/Black; 12% Hispanic; 5% other). Considering the size of the effect we found in Studies 1 and 2, we aimed for the recommendation of 50 participants per cell (Simmons, 2014); however, 14 participants did not attend the sessions, six participants could not complete the study due to technical difficulties, and one was suspicious of the feedback. The subsequent analyses were therefore conducted using data from 180 participants.

5.1.2. Procedure

Participants were randomly assigned to one of four conditions in a 2(self-control depletion: depletion vs. no-depletion) × 2(feedback: positive vs. negative) between-subjects design. Upon arrival at the laboratory, participants were informed that they were completing a test of cognitive persistence and logical reasoning called the Cognitive Ability and Persistence Battery (CAP-B), which was predictive of academic and career success. Participants were further told that the CAP-B was being used by various organizations as a part of their employee selection procedures, and that high performance predicted increased likelihood of obtaining an elite job. In reality, all participants completed a 10-item multiple-choice test taken from the logical reasoning portion of the Law School Admissions Test (LSAT) (Ehrlinger, Johnson, Banner, Dunning, & Kruger, 2008).

Participants then completed the depletion manipulation. In the depletion condition, participants were asked to write a story for 5 min without using the letters A and N, whereas in the
no-depletion control condition, participants wrote a story without using the letters X and Z. The letters A and N are more frequently used in writing, thus requiring participants to inhibit their dominant response tendencies in order to complete the story-writing task (Job et al., 2010; Schmeichel, 2007). Following the depletion manipulation, participants viewed their scores from the logical reasoning test. As in Study 1, participants received feedback indicating that they had scored poorly (bottom 28%) (negative feedback condition) or quite well (top 28%) (positive feedback condition) on the test. After participants received this feedback, they completed items designed to assess the perceived validity of the logical reasoning test as in Study 1 (e.g., “Do you think the CAP-B is a useful test of people’s logical reasoning abilities?”) on a 7-point scale. These items had good internal reliability ($\alpha = 0.82$), and were averaged to serve as an index of CAP-B validity ratings, with higher scores representing higher perceived validity.

Participants were then informed that they would complete an additional version of the test, but that they would first have the opportunity to learn more about how to perform well on the CAP-B. All participants received one page of introductory information about the test and its content. At this point, participants were asked if they would like to learn more about how to perform well on the test, and could select yes or no. If they selected yes, participants were provided with an additional page of information that explained how to best solve one of the question types (e.g., missing assumption, inference, strengthen/weaken arguments), and were provided with examples. If they selected no, participants were directed to complete the second test, which was similar in form to the first test (also items from the LSAT), but with new items. Participants had the option to read three more pages of information about the test in total before completing the second test. Finally, all participants completed a post-experiment questionnaire that included the depletion manipulation check used in Study 2, and a measure of feelings of competence invoked by the manipulation (“The story-writing task made me feel capable” and “The story-writing task made me feel competent”, $r = 0.88$, on 7-point scales). Participants also completed demographic measures, indicated whether or not they had practiced taking the LSAT, and answered an open-ended question intended to gauge suspicion of the hypotheses (e.g., “What do you think the researchers were trying to find in this study?”).

2 We first conducted a pretest on a separate sample of 107 participants to determine whether our feedback manipulation sufficiently induced self-threat. We assessed self-threat separately as a pretest rather than in the main study to avoid induced by the depletion manipulation offer an alternative account for our results. In analyzing the effect of condition on feelings of competence induced by the depletion manipulation check to be significantly more challenging ($M = 4.95$, $SD = 1.14$) than did participants in the control condition ($M = 3.20$, $SD = 0.88$), $t (176) = 11.39$, $p < 0.001$, $d = 1.72$.

5.2. Results and discussion

5.2.1. Manipulation check

Participants in the depletion condition rated the story-writing task to be significantly more challenging ($M = 4.95$, $SD = 1.14$) than did participants in the control condition ($M = 3.20$, $SD = 0.88$), $t (176) = 11.39$, $p < 0.001$, $d = 1.72$.

5.2.2. Evaluations of the diagnostic test

Consistent with the results of Study 1, the results from a 2-way ANOVA revealed a significant main effect of feedback condition, $F (1,173) = 9.51$, $p = 0.002$, $\eta^2 = 0.052$, indicating lower ratings of the diagnostic test’s validity among participants who received negative feedback, and no main effect of depletion condition, $F(1,173) = 0.18$, $p = 0.675$, $\eta^2 = 0.001$. The feedback main effect was qualified, however, by the predicted depletion $\times$ feedback interaction, $F(1,173) = 6.50$, $p = 0.012$, $\eta^2 = 0.036$. Within the negative feedback condition, depleted participants were significantly more likely to derogate the validity of the diagnostic test, rating the test as less valid than did non-depleted participants. In the positive feedback condition, depleted and non-depleted participants rated the diagnostic test as equally valid (see Table 1 for all Study 3 dependent measures, test statistics, and effect sizes).

5.2.3. Information-seeking

Because information-seeking behavior reflected a bimodal distribution (i.e., most participants either selected no (47%) or all of the additional tips (34%), we examined information seeking as a binary dependent measure (where information seeking = 1, and no information seeking = 0) with chi-square analyses (though see Table 1 for means and alternate analyses). The results revealed that, while in the negative feedback condition depleted participants were marginally less likely to seek extra information than were non-depleted participants, no significant differences emerged in the positive feedback condition.

5.2.4. Performance

The results from a 2-way ANOVA revealed no significant main effects of feedback condition or depletion condition, $F s < 0.658$, $p s > 0.418$. We again found the predicted depletion $\times$ feedback interaction, $F(1,175) = 10.79$, $p = 0.001$, $\eta^2 = 0.058$. Within the negative feedback condition, depleted participants performed significantly worse on the second test than did non-depleted participants. In the positive feedback condition, depleted participants scored marginally better than did non-depleted participants.

5.2.5. Additional analyses

We also examined whether reduced feelings of competence induced by the depletion manipulation offer an alternative account for our results. In analyzing the effect of condition on feelings of competence invoked by the writing-task, there was no effect of feedback condition or feedback $\times$ depletion, $F s < 0.810$, $ps > 0.369$, $\eta^2 s < 0.005$, but there was a main effect of depletion condition, $F (1,174) = 29.96$, $p < 0.001$, $\eta^2 = 0.147$. Participants in the depletion condition reported that the writing task made them feel less competent ($M = 3.54$, $SD = 1.47$) than did those in the control condition ($M = 4.66$, $SD = 1.24$), $t (176) = -5.48$, $p < 0.001$, $d = 0.88$. Critically however, these competence ratings did not predict any of the dependent measures ($ps > 0.157$), nor did including the competence measures in analyses of covariance affect any of the previously reported results ($p s (interactions) < 0.012$). Thus, changes in

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2 We first conducted a pretest on a separate sample of 107 participants to determine whether our feedback manipulation sufficiently induced self-threat. We assessed self-threat separately as a pretest rather than in the main study to avoid generating demand effects or otherwise affecting participants’ responses to the dependent measures (e.g., Brown, 1990). These participants came from an online subject pool of undergraduates from the same Midwestern university as in Study 3. In the pretest, participants completed the CAP-B items and then received either the positive or negative feedback. These participants then completed two measures of self-threat. One measure from Park and Maner (2009) asked participants to complete three items (e.g., when receiving this feedback, how did you feel about yourself?) on a 7-point scale ($\alpha = 0.90$). The other measure was an adapted version state self-esteem scale, which asked participants to indicate their level of agreement or disagreement with 10 items (e.g., “I am worried about whether I am regarded as a success or failure”) on a 5-point scale from 1 (Not at all) to 5 (Extremely) (Heatherton & Polivy, 1991) ($\alpha = 0.91$). We removed items from the original scale pertaining to appearance because these items lacked relevance to the task at hand. For both scales, higher scores reflect less self-threat. The results revealed that participants in the negative feedback condition reported less positive views on the self-threat scale ($M = 2.36$, $SD = 0.94$) than did those in the positive feedback condition ($M = 4.46$, $SD = 1.52$), $t (105) = -8.59$, $p < 0.001$, $d = 1.67$. Similarly, participants in the negative feedback condition had lower state self-esteem ($M = 3.05$, $SD = 0.70$) than did those in the positive feedback condition ($M = 3.75$, $SD = 0.74$), $t (105) = -4.42$, $p < 0.001$, $d = 0.85$.

3 Two participants indicated that they had practiced items from the LSAT. Removing these participants does not affect the study results.

4 We follow the recommendations of MacCallum, Zhang, Preacher, and Rucker (2002) who recommend dichotomizing count variables when they are highly skewed.
feelings of competence following the depletion manipulation do not seem to drive our results.

Taken together, the results of Study 3 paint a clear picture: Across multiple measures, participants with reduced self-regulatory capacities were more likely to engage in defensive processing of negative feedback than were their non-depleted counterparts. This effect was found not only with self-report measures, but also with two behavioral measures. This provides important convergent evidence for our predictions. Moreover, because depletion did not negatively affect participants' information-seeking and performance following positive feedback, the results suggest that rather than creating general deficits in participants' ability to perform a cognitive task, reduced self-regulatory capacities harm the processing of information that contains self-threats.

6. Study 4

Study 4 tested our prediction that self-improvement motivation would moderate the effect of reduced self-regulatory capacities on defensive processing. To test this prediction, we manipulated both self-regulatory capacities and self-improvement motivation. We predicted that self-regulatory depletion would reduce participants’ willingness to seek valuable, but self-threatening information, but not when self-improvement motives were active. Such a pattern would suggest that increasing self-improvement motivation is one means by people can overcome the negative effects of depletion on defensive processing.

6.1. Method

6.1.1. Participants

Participants were 179 individuals recruited via Amazon’s Mechanical Turk (M_{age} = 34.83 years; 51% female). We aimed to collect 200 participants (50 per cell), but five participants were removed for failing an instructional manipulation check, in which they entered ‘Decision-Making’ into a text box if they read the instructions (Oppenheimer, Meyvis, & Davidenko, 2009), and 16 did not complete the study.

6.1.2. Procedure

Participants were randomly assigned to 1 of 4 conditions in a 2 (self-control depletion: depletion vs. no-depletion) × 2 (motive: self-improvement vs. control) between-subjects design. Participants first completed the self-control depletion manipulation from Study 3. Participants then moved onto a second, ostensibly unrelated task, which served to induce either a self-improvement motive (self-improvement condition) or no motive (control condition). As in prior research (Fishbach & Labroo, 2007), participants in the self-improvement condition listed five activities that they would do in order to become a better person. In the control condition, participants listed and described five types of furniture. Participants in the self-improvement condition listed activities such as “learn other languages,” “take classes,” and “be more patient.” This manipulation increases the accessibility of the self-improvement motive, which has been found to increase its motivational strength (e.g., Green et al., 2009; Srull & Wyer, 1979). Participants next completed the dependent measure used in Study 2, in which participants chose to work with either a manager who focuses on negative feedback (the self-improving manager) or positive feedback (the self-protecting manager). They again also indicated their preferences on a 7-point scale (“I would definitely select Manager X to work with”) from 1 (Strongly disagree) to 7 (Strongly agree). Finally, all participants completed a post-experiment questionnaire that included the depletion manipulation check used in the previous studies, and open-ended questions intended to gauge suspicion of the hypotheses (e.g., “What do you think the researchers were testing in this study?”).

6.2. Results and discussion

6.2.1. Manipulation check

Participants in the depletion condition rated the story-writing task to be significantly more challenging (M = 5.20, SD = 1.30) than did participants in the control condition (M = 3.58, SD = 1.09), t (177) = 9.01, p < 0.001, d = 1.35.

6.2.2. Manager selection

To examine the effects of the depletion and self-improvement manipulations, we conducted a binary logistic regression in which we regressed manager selection (0 = self-improving manager; 1 = self-protecting manager) onto depletion condition (0 = depletion; 1 = no-depletion) and self-improvement condition (0 = self-improvement; 1 = control). The results revealed the predicted interaction between depletion and self-improvement motivation, \( b = -1.25, \chi^2(1, N = 179) = 4.13, p = 0.042 \). There was also a significant main effect of self-improvement motivation, \( b = 0.93, \chi^2(1, N = 179) = 4.47, p = 0.035 \), such that participants in the self-improvement motivation condition were more likely to choose the self-improving manager, but no main effect of depletion, \( p = 0.733 \). Decomposing the significant interaction, within the control condition, 54% of non-depleted participants chose the self-improving manager, whereas only 28% of depleted participants did so, \( b = -1.11, \chi^2(1, N = 87) = 6.28, p = 0.012 \). By contrast, within the self-improvement condition, depleted and non-depleted participants were equally likely to select the self-improving manager (50% and 46%, respectively), \( b = 0.15, \chi^2(1, N = 92) = 0.12, p = 0.733 \).

Results from the continuous measures reflect a similar pattern. We conducted a 2-way ANOVA examining the effects of the

Table 1

<table>
<thead>
<tr>
<th>Condition</th>
<th>Negative feedback</th>
<th>Positive feedback</th>
</tr>
</thead>
<tbody>
<tr>
<td>Depletion</td>
<td>No-depletion</td>
<td>Test statistic</td>
</tr>
<tr>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>Validity ratings</td>
<td>3.00 (1.17)</td>
<td>3.57 (1.41)</td>
</tr>
<tr>
<td>% Seeking information</td>
<td>33.3</td>
<td>52.3</td>
</tr>
<tr>
<td>Number of tips</td>
<td>0.89 (1.26)</td>
<td>1.36 (1.4)</td>
</tr>
<tr>
<td>Correct answers (Time 2)</td>
<td>1.68 (1.14)</td>
<td>2.32 (0.98)</td>
</tr>
<tr>
<td>M (SD)</td>
<td>M (SD)</td>
<td>Test statistic</td>
</tr>
<tr>
<td>Depletion</td>
<td>No-depletion</td>
<td>M (SD)</td>
</tr>
<tr>
<td>4.08 (1.21)</td>
<td>3.67 (1.29)</td>
<td>1.54</td>
</tr>
<tr>
<td>61.7 (1.11)</td>
<td>61.4 (1.11)</td>
<td>0.001</td>
</tr>
<tr>
<td>1.44 (1.35)</td>
<td>1.46 (1.33)</td>
<td>(-0.027)</td>
</tr>
<tr>
<td>2.09 (1.11)</td>
<td>1.66 (1.05)</td>
<td>1.87</td>
</tr>
</tbody>
</table>

Note. The test statistics and effect sizes are \( t \) and \( d \) values except for the self-seeking information variable, for which \( \chi^2 \) and odds ratios are used, respectively.

\(^1 \ p < 0.10.\)

\(^2 \ p < 0.05.\)

\(^3 \ p < 0.01.\)
depletion and self-improvement conditions on willingness to work with the self-improving manager. The results revealed a depletion × self-improvement motivation interaction, $F(1,175) = 3.74, p = 0.055$, $\eta^2 = 0.021$, and no main effects (see Fig. 2). Decomposing the interaction, within the control condition, depleted participants were again significantly less willing to work with the self-improving manager ($M = 3.52, SD = 1.87$) than were non-depleted participants ($M = 4.61, SD = 1.80$), $t(90) = -2.84, p = 0.006$, $d = 0.59$. However, within the self-improvement condition, depleted and non-depleted participants did not significantly differ in their willingness to work with the self-improving manager ($M = 4.35, SD = 2.15$ and $M = 4.29, SD = 2.07$, respectively), $p = 0.904$, $d = 0.03$. Likewise, there was a significant interaction effect between depletion and self-improvement conditions in predicting willingness to work with the self-promoting manager, $F(1,175) = 4.84, p = 0.029$, $\eta^2 = 0.027$, and no significant main effects. As anticipated, within the control condition, depleted participants were significantly more willing to work with the self-protecting manager ($M = 5.43, SD = 1.53$) than were non-depleted participants ($M = 4.37, SD = 1.92$), $t(90) = 2.94, p = 0.004$, $d = 0.61$. By contrast, when a self-improvement goal was made active, no significant differences emerged between depleted and non-depleted participants ($M = 4.50, SD = 1.93$ and $M = 4.63, SD = 1.88$, respectively), $p = 0.744$, $d = 0.07$.

Consistent with Studies 1–3, Study 4 demonstrated that reductions in self-regulatory capacities increased defensive processing. Furthermore, this study demonstrated that an active self-improvement motive following depletion can negate this effect, suggesting that people’s self-improvement motivation is an important boundary condition.

7. Study 5

Study 5 served three main goals. First, we sought to enhance external validity by testing whether our predictions would hold in the context of entrepreneurs receiving feedback about their creativity—a domain of high importance to this population. Second, we aimed to gather additional support for the role of self-improvement motivation by examining the moderating effects of trait self-improvement motivation. As with self-control, self-improvement motivation varies both as a trait and a state (Breines & Chen, 2012; Fishbach & Labroo, 2007; Sedikides & Hepper, 2009). Third, the feedback choice in Studies 2 and 4 was hypothetical. Importantly, Study 5 tested whether the effects from Studies 2 and 4 would generalize to participants’ real choice behavior.

7.1. Participants

Participants were 157 American adults recruited via an online panel of entrepreneurs ($M_{age} = 48.43$ years; 41% women; 32% had some college, 53% had a bachelor’s degree, 10% had a Master’s degree, and 6% had a PhD). We aimed for 160 participants (80 per cell), but two participants did not complete the study, and one was not an entrepreneur and/or self-employed and was thus removed from the dataset. These participants came from a range of industries, including online retail, non-profit, design, and consulting. This sample indicated that creativity was an attribute highly important to their careers ($M = 6.09, SD = 1.18$, on a scale from 1 = Not at all important to 7 = Extremely important).

7.2. Procedure

Participants were invited to complete an online survey. After following a link to the study website, they were informed that they would complete a well-validated test designed to examine their current level of creativity: the Remote Associates Test (RAT; Mednick, 1962). To manipulate anticipated feedback negativity, we used a manipulation from prior research in which participants completed either difficult or easy items from the RAT (Brown, 1990; Molden & Higgins, 2008). This manipulation leverages the tendency for people to assume they have performed worse than others after completing difficult rather than easy tests (Moore & Healy, 2008; Windschitl, Kruger, & Simms, 2003). After completing the test, participants were told that they could receive in-depth feedback about their performance, including how it compared to successful entrepreneurs. Participants then indicated whether or not they would like to receive the feedback (yes or no). Participants then indicated the extent to which they preferred to receive feedback, how positive they would feel after receiving the feedback, and how much they could learn from the feedback on 7-point scales (1 = Not at all to 7 = Very much).

After completing filler items, participants then completed the trait self-control and self-improvement measures, and manipulation checks (“How many questions do you think you answered correctly?” and “What do you think your feedback will be like?” on a scale from 1 = Very negative to 7 = Very positive). Participants also reported a number of control variables including their industry category, length of time in an entrepreneurial role, perceived job security, job satisfaction, and demographic variables.

7.3. Materials

7.3.1. Trait self-control

As in Study 1, we assessed trait self-control with the brief version of the Self-Control Scale (Tangney et al., 2004) ($x = 0.86$).

7.3.2. Trait self-improvement

To assess self-improvement, participants completed 11 items (e.g., “I want to learn and improve myself”), answered on a scale from 1 (Strongly disagree) to 5 (Strongly agree) (adapted from Fishbach & Labroo, 2007). Again, scores were averaged to produce an index of trait self-improvement, with higher scores reflecting greater levels of chronic self-improvement motivation ($x = 0.85$). Trait self-improvement scores positively correlated with trait self-control scores, $r = 0.31, p = 0.001$, yet moderately, indicating discriminant validity (Cohen, 1992).

5 To control for factors such as curiosity, all participants knew that they would see the answers to the RAT at the end of the study. However, the additional feedback would allow them to receive detailed feedback about their performance, including how it compared to their peers.
7.4. Results and discussion

Of the demographic and control variables, only higher levels of job security positively predicted preference to receive feedback, \( b = 0.20, \ p = 0.031 \). However, job security did not significantly interact with any of the other predictor variables, nor did controlling for security affect any of the results. Thus, the control variables are not discussed further. We also note that participants’ trait self-control and self-improvement scores were not affected by the anticipated feedback manipulation (\( p = 0.861 \) and 0.548, respectively). In addition, neither trait self-control nor trait self-improvement influenced participants’ performance on the Remote Associates Test itself (\( p = 0.277 \) and 0.973, respectively).

7.4.1. Manipulation checks

Participants in the anticipated negative feedback condition (i.e., completed the difficult version of the test) answered fewer questions correctly (\( M = 2.36, SD = 2.04 \)) than did participants in the anticipated positive feedback condition (i.e., completed the easy version of the test) (\( M = 7.44, SD = 2.77 \)), \( t(155) = -13.04, p < 0.001, d = 2.09 \). They also guessed that they had answered fewer questions correctly (\( M = 3.59, SD = 2.41 \)), and thought that they would receive more negative feedback (\( M = 2.80, SD = 1.42 \)) than did those in the anticipated positive feedback condition (\( M = 8.41, SD = 1.63 \) and \( M = 5.13, SD = 1.46 \)), \( t(155) = -14.77, p < 0.001, d = 2.37, and t(153) = 10.08, p < 0.001, d = 1.63 \). Therefore, participants in the anticipated negative feedback condition did indeed anticipate getting more negative feedback than did those in the anticipated positive feedback condition.

Consistent with the intended psychological consequences of the manipulation, participants in the anticipated positive feedback condition also thought that receiving the feedback would make them feel significantly more positive (\( M = 6.11, SD = 1.04 \)) than did those in the anticipated negative feedback condition (\( M = 5.54, SD = 1.60 \)), \( t(155) = 2.67, p = 0.008, d = 0.48 \). By contrast, participants in the anticipated positive feedback condition thought there was significantly less potential to learn from the feedback (\( M = 4.05, SD = 1.18 \)) than did those in the anticipated negative feedback condition (\( M = 4.97, SD = 1.76 \)), \( t(155) = -3.89, p < 0.001, d = 0.62 \).

7.4.2. Feedback-seeking

To assess feedback-seeking, we conducted binary logistic regression analyses, regressing participants’ feedback choice (1 = seeking feedback; 0 = avoiding feedback) onto their centered continuous self-control score, their dummy-coded condition (1 = negative feedback; 0 = positive feedback), and the interaction between their centered self-control score and condition. The results revealed a significant effect of feedback condition, \( b = -6.41, z = -2.40, p = 0.016 \), indicating less willingness to seek feedback in the anticipated negative feedback condition, and a marginal effect of trait self-control, \( b = -1.02, z = -1.70, p = 0.089 \). Critically, there was a significant interaction between anticipated feedback condition and trait self-control, \( b = 1.63, z = 2.19, p = 0.024 \). We again used a floodlight analysis to determine the range(s) of self-control for which the effect of the manipulation was significant. The results revealed that participants were significantly more likely to seek out feedback when it was positive rather than negative if their trait self-control score was 3.24 (out of 5) or lower (representing the 47th percentile of self-control scores), \( b_m = -1.14, SE = 0.58, z = -1.96, p = 0.050 \). At self-control scores greater than 3.24, participants were equally likely to seek out feedback, regardless of whether the anticipated feedback was positive or negative.

Next, because we expected the relationship between self-control and feedback-seeking to be moderated by self-improvement motivation, we modeled the interaction between self-control and anticipated feedback condition at varying levels of self-improvement motivation (i.e., we considered a three-way interaction between self-control, self-improvement, and condition). The results suggested that the two-way interaction between self-control and anticipated feedback condition depended on participants’ level of self-improvement motivation, \( b = 2.31, SE = 0.46, p = 0.006 \). The interaction between trait self-control and condition was significant for self-improvement motivation scores of 4.03 (out of 7) or lower, \( b_m = -0.38, SE = 0.20, z = -1.96, p = 0.050 \). At high levels of self-improvement motivation, participants’ willingness to seek out feedback was less contingent on their self-control.

7.4.3. Feedback preference

A similar pattern of results emerged for the continuous measure of participants’ preference to receive the feedback. There was a significant effect of trait self-control, \( b = -0.50, t(155) = -2.25, p = 0.026 \), a significant effect of condition, \( b = -4.42, t(155) = -4.10, p < 0.001 \), and a significant feedback condition by self-control interaction, \( b = 1.25, t(155) = 3.95, p < 0.001 \). Participants with self-control scores of 3.16 (out of 5) or below preferred receiving feedback more in the anticipated positive than in the anticipated negative condition (\( b = -0.46, p = 0.050 \)). However, among participants with self-control scores higher than 3.16, there was no difference in feedback preference ratings by condition (i.e., whether the feedback was anticipated to be positive or negative). In fact, participants with self-control scores greater than 4.02 (the 85th percentile) demonstrated a preference for feedback in the anticipated negative compared to positive feedback condition (\( b = 0.63, p = 0.050 \)).

We next explored moderation by self-improvement motivation. The results suggested that the two-way interaction between self-control and anticipated feedback condition depended on participants’ level of self-improvement motivation, \( b = -0.57, SE = 0.25, t = -2.30, p = 0.022 \). The interaction between trait self-control and condition was significant for self-improvement motivation scores of 4.41 (out of 7) or lower, \( b_m = 0.43, SE = 0.22, t = 1.98, p = 0.050 \) (see Fig. 3). Again, participants’ self-control scores did not predict feedback preference at high levels of self-improvement motivation.

Consistent with Studies 1–4, Study 5 demonstrates that lower self-regulatory capacities increase defensive processing. Here captured by an increased avoidance of negative feedback on an attribute rated as highly important. This study further demonstrated that high levels of self-improvement motivation can negate this effect, suggesting that people can overcome lower self-regulatory capacities and avoid engaging in defensive information processing when they are sufficiently motivated to do so. Finally, we demonstrated these effects in an organizationally relevant context. Given the importance of both creativity and feedback-seeking for entrepreneurial success (e.g., Baron & Ward, 2004; Frese, 2009), these results suggest the importance of this phenomenon for the entrepreneurial workforce.

8. General discussion

The ability to receive beneficial (but threatening) information with an open mind is critical to adaptive functioning. It is also a difficult feat. The present research suggests that self-regulatory capacities play a key role in determining whether people respond to threatening information openly or defensively. Across multiple domains, participants with higher levels of trait or state self-regulatory capacities were more likely to seek, accept, and utilize negative feedback than were their counterparts with lower
self-regulatory capacities. When participants were high in self-improvement motivation, however, self-regulatory capacities no longer predicted defensive processing. Overall, the current research suggests that trait and state capacity for self-regulation offers a powerful tool for predicting and understanding who will be most susceptible to defensive information processing.

8.1. Theoretical contributions and practical implications

The present research provides important theoretical contributions. First, although scholars have suggested that confronting self-threatening information presents a self-control dilemma (e.g., Trope, 1986), previous empirical tests of this assertion were indirect. For example, Trope and Neter (1994) examined reactions to negative feedback following boosts in positive affect, and found that people were more receptive to negative feedback following a positive experience (i.e., previous success on a similar task). The authors inferred that positive experiences increase people's 'psychological resources' for coping with negative feedback. However, positive experiences (i.e., mood) and self-regulatory capacities are orthogonal constructs, and the direct effects of self-regulatory capacities on defensive processing cannot be directly inferred from these existing studies (e.g., Fujita, Trope, Liberman, & Levin-Sagi, 2006; see Schmeichel & Inzlicht, 2013 for review). Our work provides direct empirical evidence of such a link. By systematically exploring the effects of self-regulatory capacities on defensive processing, we provide a critical piece in the defensive information-processing puzzle.

Second, our results contribute to the self-regulation literature by shedding light on recent discussions about the relationship between self-regulatory resources and motivation (Baumeister & Vohs, 2007; Inzlicht & Schmeichel, 2012; Inzlicht et al., 2014). Though self-control theories have been recently updated to include considerations of motivation, our results provide novel empirical evidence for the notion that increasing self-improvement motivation may reduce the effects of depleted self-regulatory capacities. Importantly, these results also support the process model of depletion proposed by Inzlicht and Schmeichel (2012). The process model suggests that self-regulatory depletion produces temporary shifts in both motivational orientation and attention, which in turn reduces performance on a subsequent task requiring self-regulation. In Study 4, self-regulatory depletion increased defensive processing in the control condition, but not in the self-improvement motive condition. It may be that reduced self-improvement motivation following depletion among those in the control condition drove increased defensive processing (Inzlicht & Schmeichel, 2012; Spencer, Zanna, & Fong, 2005).

The current results also have important managerial and organizational implications. A major problem faced by organizations is that, despite increased provision of employee feedback (Prewitt, 2007), defensive processing often prevents that feedback from being internalized and acted upon. One clear prescription offered by the current work is that feedback may be more likely to be internalized if it is provided when recipients have higher self-regulatory capacities. For example, feedback should be provided at the beginning rather than at the end of a work day, as research suggests that self-regulatory capacities decline as the day progresses (Danziger, Levav, & Avnaim-Pesso, 2011; Hofmann, Baumeister, Foerster, & Vohs, 2012). However, the results also suggest that activating self-improvement motivation in employees (e.g., by having them list the reasons why self-development at the workplace is important to them) may be an effective means of increasing receptiveness to negative feedback—even in the face of reduced self-regulatory capacities. Finally, understanding employees' trait level self-control and self-improvement motivation might allow managers to have a better understanding of how specific employees will react to diagnostic negative feedback in the workplace.

8.2. Limitations and future directions

Despite the advances made, it should be noted that the current work contains some limitations that should be addressed in future research. For one, all five studies examined individuals outside of their organizational context. Examining these findings at an organizational level could extend the research questions in interesting ways. For example, in Studies 4 and 5, higher levels of self-improvement motivation buffered people from the costs of low self-control. Extending beyond individual-level motivation, it is possible that different organizational cultures may operate similarly to increase the relative importance of self-improvement over self-protection motives. For instance,
employees working in more achievement-oriented cultures (e.g., Cooke & Rousseau, 1988) that prioritize self-improvement may be more likely to pursue self-improvement, and show receptiveness to potentially threatening feedback, even in the face of reduced self-regulatory capacities.

Second, the current research supports recent theorizing suggesting that the effects of self-regulatory depletion are driven by shifts in motivation and attention that undermine future self-control efforts (Inzlicht & Schmeichel, 2012; Job et al., 2010). It is possible, however, that once people are extremely taxed, they may not be able to engage in the effortful control required for the accurate processing of negative feedback, even when self-improvement motivation is high (Baumeister & Vohs, 2007; Vohs, Baumeister, & Schmeichel, 2012). Future research is needed to examine this possibility.

Future research may seek to explore the interrelations between the current findings and other factors that have been found to affect the processing of negative information. For example, one important line of literature has considered the role of regulatory focus in predicting reactions to negative feedback. According to regulatory focus theory, people can be oriented toward either promotion goals (i.e., attaining positive aspirations) or prevention goals (i.e., avoiding feared outcomes and potential failures) (Higgins, 1998). Research has found that prevention focus is related to self-protective strategies, such as defensive reactions to negative feedback (Hepper, Gramzow, & Sedikides, 2010; Molen & Higgins, 2008). Integrating these findings with our current framework, it would be interesting to examine whether promotion-oriented people are less likely to demonstrate defensive reactions because of their increased focus on self-improvement.

We also note that our perspective converges with Shani and colleagues’ Search for Unpleasant Truths (SUT) framework (Shani, Tykocinski, & Zeelenberg, 2008; Shani & Zeelenberg, 2007; see also Loewenstein, 2006). The SUT framework explores situations in which people do prefer to seek out negative information, even when that information is predictably harmful and offers no utility. Examples that capture this tension include someone deciding whether or not to find out if the lottery ticket they forgot to send in was a winner, or whether or not to view pictures of a cheating spouse (Kruger & Evans, 2009; Shani, Igou, & Zeelenberg, 2009).

We believe that Shani and colleagues’ research captures a related, but distinct phenomenon. First, in the SUT scenarios, it is the lack of information that is concerning. In the current research, people already possess relevant knowledge (e.g., positive baseline views of one’s competence), and self-threatening information contradicts those pre-existing beliefs (e.g., Ditto & Lopez, 1992; Dunning, 2007). Second, in the SUT studies, the scenarios are largely unrelated to self-improvement, as people can no longer remember the situations (Kruger & Evans, 2009; Shani & Zeelenberg, 2007; Shani et al., 2009). Thus, in Shani and colleagues’ paradigm, people are driven to resolve their curiosity in the short-term, even when the information offers no instrumental value, and might be psychologically harmful in the long-term. By contrast, in the current research, people are driven to avoid the short-term affective costs of processing self-threatening information at the expense of the potential long-term gains of incorporating self-developmental feedback. However, these perspectives converge in the capturing the tension between choices that are affectively desirable in the short-term, but come at a long-term cost, versus those that might be less desirable in the short-term, but offer long-term gains. In both cases, we suggest that self-regulatory capacities will be an important predictor of people’s abilities to overcome the temptation of information consumption options that are desirable in the short-term, but worse in the long-term.

9. Conclusion

An old proverb suggests that “good medicine tastes bitter.” Often, the most helpful information to receive can be the most difficult to swallow. The current research proposes that people’s self-regulatory capacities dictate receptiveness to diagnostic negative feedback. Given the many demands on self-regulatory capacities in the workplace—from quickly approaching deadlines to interpersonal conflicts with coworkers—our work suggests the importance of considering the timing and context in which feedback is provided. Our work also offers a silver lining. Invoking employees’ self-improvement motivation can provide an effective buffer against defensiveness in the face of these workplace strains.

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References


Value Corruption: Instrumental Use Erodes Sacred Values

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ABSTRACT
Sacred values like environmental-protection, patriotism, and diversity are increasingly leveraged by organizations, marketers, and individuals to yield profits and favorable reputations. In the current research, we investigated a potentially perverse consequence of this tendency: that observing values used instrumentally (i.e., in the service of self-interest) will negatively impact observers’ perceptions of those values. Six studies demonstrate support for this value corruption hypothesis. Following exposure to the instrumental use of a sacred value, people perceived that value to be less sacred as assessed by both explicit (Studies 1-6) and implicit (Study 4) measures, and were less willing to act in value-consistent ways (Studies 4 and 5). Study 3 found that the value corruption effect was specific to sacred values, and was attenuated when a non-sacred value was used instrumentally. These results have important implications: People and organizations that use values instrumentally may ultimately undermine the values from which they intend to benefit. The results also suggest that introducing market norms into previously untouched domains will ultimately come at a cost.

Keywords:
Values; sacred values; self-interest; morality
Value Corruption: Instrumental Use Erodes Sacred Values

The National Football League (NFL) has long been a strong supporter of the military and patriotism. Football games are full of patriotic displays, including camouflage jerseys, national flags placed throughout the stadiums, and special appearances by military personnel. The NFL has created a strong association between its organization and patriotism. An interesting element of this association between the NFL and patriotism is that it does not come for free. The term “paid patriotism” was coined in a recent report documenting that the U.S. Department of Defense paid professional sports leagues across America at least $53 million to embed military-themed programs into the game-day experience. These acts, done under the guise of the teams’ voluntary expressions of patriotism, were actually performed to yield a substantial profit.

Acts like paid patriotism are not unusual. Sacred values like environmental-protection, diversity, and patriotism, are frequently employed by organizations, marketers, and individuals to yield profits and favorable reputations. That observers often react negatively to situations in which values are explicitly used in the service of self-interest is well captured by existing research (McGraw, Schwartz, & Tetlock, 2012; Tetlock, Kristel, Elson, Green, & Lerner, 2000; Turco, 2012). Indeed, reports of paid patriotism were met with criticism of both professional sports leagues and the Department of Defense. What remains unclear is how people react to the sacred values themselves in the aftermath of instrumental use. In the current research, we predicted that people exposed to a value used instrumentally will subsequently view the value itself as less sacred and will demonstrate less commitment to that value compared to those who have not. We argue that because these sacred values are believed to be pursued as ends in themselves—kept separate from utilitarian concerns, such as profitability (Durkheim, 1915; Tetlock et al., 2000)—the instrumental use of values undermines the values’ status as sacred.
This sentiment is well-captured by Senator John McCain’s fear that paid patriotism would only serve to “cheapen” other, more genuine acts of patriotism (Bryant, 2015).

The Instrumental Use of Sacred Values: Shaping the Nature of Values

Sacred values are those for which a group or groups of people believe that their commitments to these values are absolute and inviolable, and that these values should not be traded off for secular values, such as profitability or efficiency (Tetlock, 2002, 2003; Tetlock et al., 2000). A value is deemed sacred when people believe that the value (and the objects, persons, or places associated with it) cannot be valued along a metric common with economic goods, such as when land becomes “Holy Land” and is beyond dollar valuations (Rozin & Wolf, 2008). Common examples of sacred values include honor, patriotism, environmental protection, equality, fairness, and individual health and well-being (Baron & Spranca, 1997; Tetlock, 2003).

According to Tetlock’s (2002; 2003) sacred-value-protection model (SVPM), people strive to keep sacred values separate from encroachment by the secular (e.g., concerns over profit, efficiency, and the calculus of capitalism). As a consequence, people consider it “taboo” to, for example, put a price on saving an endangered species, or to justify racial profiling on the basis of cost-benefit analyses. The SVPM holds that people react to these taboo tradeoffs with moral outrage, an aversive arousal state that comprises feelings of anger, disgust, and contempt, and a desire to punish the violator (Ginges & Atran, 2009; Ginges et al., 2007; Tetlock et al., 2000). Exposure to taboo tradeoffs is also said to be morally contaminating, leading observers to want to engage in acts of moral cleansing, such as demonstrating increased commitment to the violated value. In one illustrative study, Tetlock et al. (2000) exposed participants either to a taboo tradeoff, in which a hospital administrator decides between saving the life of a young boy

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1 Consistent with Tetlock (2003), we consider these values to represent an expressed, but not a revealed preference, in that people often make choices that undermine these values (e.g., Tetlock et al., 2000).
who needs an organ transplant or saving the hospital $500,000 for other organizational purposes, or a tragic tradeoff, in which the administrator must decide which of two boys to save. The authors found that participants exposed to the taboo tradeoff demonstrated more moral outrage, and a greater desire to punish the administrator, than did those exposed to the tragic tradeoff. Consistent with the moral cleansing hypothesis, participants exposed to the taboo tradeoff also reported being more likely to volunteer for a campaign that would increase organ donations.

Taboo tradeoffs comprise a specific type of value violation in which a sacred value is sacrificed in exchange for secular concerns (e.g., for profit). As highlighted in the paid patriotism example, there are other ways in which values can be violated. In the current research, we are interested in instrumental uses of values, in which a sacred value is used in the service of self-interest (e.g., for profit), but is not directly harmed or compromised. What happens to perceptions of sacred values following this type of instrumental use remains unclear. We propose that once sacred values are used instrumentally their meaning becomes corrupted. Research on marketization and crowding out effects supports this proposition.

First, marketization is the process through which market norms, which emphasize profit and self-interest, are introduced into aspects of life previously governed by nonmarket norms. Despite strong preferences to keep the sacred and secular separate, the reality of market-based societies is that almost anything can be used to further self-interest, and collisions between these two realms are quite common (Bell, 1976; Calabresi & Bobbit, 1978). Scholars of marketization suggest that market norms have the potential to corrupt non-market values, in part because of the attitudes markets express toward the values (e.g., Appadurai, 1986; Douglas, 1966; Fiske, 1992; Koptyoff, 1986; Sandel, 2012). Typically, a sacred object or value is beyond price and serves as an end in itself. Marketization, however, ties the object or value to secular concerns, such as
yielding a profit. Once marketized, the sacred is no longer above utilitarian concerns, rendering it secular (Durkheim, 1915; Eliade, 1959). In other words, the sacred and market norms are viewed as “hostile worlds” with the former “contaminating” the latter (Zelizer, 2005, pp. 20-21). To illustrate, the ability to purchase “naming rights” of sacred places, such as parks and civic spaces might increase funding for those spaces, but this practice also suggests that these spaces are up for sale (Sandel, 2012). Although the literature on marketization is largely theoretical, crowding out effects provide empirical evidence suggesting that ties to self-interest might undermine sacred values.

Sacred entities and values are perceived to be intrinsically valuable, meant to be pursued as ends in themselves. Research on crowding out effects provides empirical evidence that introducing instrumentality to intrinsically valuable tasks can erode the value of those tasks (e.g., Ariely, Bracha, & Meier, 2009; Deci, Koestner, & Ryan, 1999; Frey & Oberholzer-Gee, 1997). Crowding out effects typically describe the tendency for the introduction of extrinsic rewards to decrease individuals’ intrinsic motivation to complete tasks. For instance, students who previously enjoyed completing puzzles lost interest in playing for free once they were paid to solve the puzzles (Deci, 1971). Extrinsic incentives can also crowd out moral norms. In their study of Israeli day care centers, Gneezy and Rustichini (2000) found that imposing fines on parents for picking up children late ironically increased tardiness. The authors argued that the fines transformed punctuality from a moral duty to a market transaction governed by price.

Research also suggests that observers use the presence of incentives to infer the intrinsic value of a task, even in the absence of their direct participation. Wild, Enzle, Nix, and Deci (1997) found that participants who observed a paid target complete a task inferred that the task would be less intrinsically enjoyable than did participants who observed a volunteer complete the
same task. We argue that observing sacred values used instrumentally may similarly suggest that the value’s primary purpose is utilitarian in nature, undermining its sacred status.

Though it may appear at odds with research demonstrating that people strive to protect their values in the face of threat, our central prediction is fully compatible with these findings. Although people may react to threats to values by engaging in acts of moral cleansing that reaffirm the values, we argue that instrumental use may not represent a sufficiently severe threat to evoke value protective responses. The scenarios used in existing research on the sacred-value-protection model tend to involve both the introduction of instrumentality and the active harm of the value. For example, when the hospital administrator lets the child die in order to save the hospital $500,000, there is a clear violation of the sanctity of human life in exchange for monetary benefit (Tetlock et al., 2000). We cover a distinct, yet widespread, type of instrumental use in which the value is used in the service of self-interest, but used in a manner that does not directly compromise or harm the value itself. In fact, introducing benefits to self-interest arguably helps values-based causes (Pallotta, 2008). Supporting our prediction, research has demonstrated the ironic tendency for mildly negative events to cause more harm than more intensely negative events by failing to activate the psychological processes that attenuate distress (Gilbert, Lieberman, Morewedge, & Wilson, 2004; Gilbert, Pinel, Wilson, Blumberg, & Wheatley, 1998). We suggest that milder instrumental uses may similarly create greater changes in perceived sacredness by failing to induce this psychological immune system. For example, whereas letting an oil spill continue in order to earn a profit is a clear violation and threat to the value of environmentalism, an oil company’s attempt to benefit from promoting environmentalism is not inherently wrong, but might suggest that environmentalism has entered the world of markets and self-interest. Tetlock et al. (2000) echo this point by suggesting that
strong value protective responses will be most likely to emerge when the “observed normative violation is so egregious (as ours usually were) that it severely undercuts the moral order” (p. 869). In the current research, we document the harmful downstream effects of an increasingly commonplace practice.

**Overview of Studies**

Six studies using diverse approaches test the value corruption hypothesis. Study 1 provides an initial test of hypothesis, testing whether the instrumental use of a sacred value decreases the perceived sacredness of that value. Study 2 seeks to demonstrate specificity around this effect, by examining whether the value corruption hypothesis is specific to sacred values, and is attenuated when secular values are used toward instrumental gain. Study 3 enhances external validity by testing the hypothesis with real campaigns. After documenting the basic effect that instrumentality can desacralize values, we move to behavioral markers of this shift in sacredness. Specifically, Studies 4 and 5 examine whether instrumental use reduces donation to value-relevant causes. Studies 4 and 5 also offer evidence that the corruption effect is specific to the value that has been used instrumentally, and does not affect perceptions of other sacred values. Next, if our reasoning is correct, the value corruption should only occur for events that do not clearly represent a threat to the value. Study 5 tests this boundary condition. Finally, Study 6 examines whether participants who witnessed a value being used instrumentally would subsequently endorse ambiguously instrumental uses of the value as being value-consistent, indicating a relaxing of standards for use of the value.

Taken together, the current research contributes to our understanding of how instrumentality can shape values, how the meaning of values can change over time, and sheds light on the potential consequences of the introduction of market norms into previously
untouched spheres of life. Determining which values are held as sacred or not by a given group or culture is of great importance, given that perceived sacredness predicts willingness to act on those values (in the absence of external incentives), a stated unwillingness to compromise those values, and aggressive responses to conflict involving those values (see Atran, 2010 for review).

**Pretest**

To ensure that the values we examined were viewed by participants (on aggregate) as being sacred, we conducted a pretest. To do so, 100 participants were recruited on Amazon’s Mechanical Turk. Six were removed for not completing the study, and the analyses were conducted on the remaining 94 ($M_{age} = 34.60, SD_{age} = 11.30$; 36.2% female). For this study and all subsequent studies, we determined sample size in advance and ended data collection before analyzing the results.

We pretested several sacred (and secular) values discussed in prior research, using an established paradigm. Specifically, participants indicated their position (agree or disagree) on a value-relevant issue (e.g., “People of all races should be treated equally,” “I am proud to be an American”). Participants were then asked how much money it would take for them to change their position on the issue. Critically, participants had the option to “opt-out” if they would never change their position, for any amount of money. Participants were considered to hold the value as sacred if they chose this opt-out option, and we considered a value sacred if the median decision was to opt-out of the transaction (Berns et al., 2012). In total, participants completed 16 questions, covering a wide range of values.

The results revealed substantial variance in the degree to which values were held as sacred. The values of environmentalism, patriotism, religion, equality, human health and well-being, and human life were held as sacred by participants, with the median bid being to opt-out
of the transaction. In order of descending bid prices, loyalty, conflict resolution, honesty, political orientation, maintaining rules, creativity/innovation, economic development, and taste and product preferences were not rated as sacred, with median bid prices ranging from $50,000 to $500.

**Study 1**

In Study 1, we provided an initial test of the value corruption effect. Specifically, we examined whether instrumental use of a sacred value (patriotism) would decrease the perceived sacredness of the value, as compared with a value-consistent use or a neutral control condition.

**Method**

**Participants.** Participants were 155 American adults (mean age = 33.4 years; 61% female) recruited on Amazon’s Mechanical Turk. An additional 14 adults were recruited, but failed an attention check (“Please skip this item—do not select an answer on this line”; Oppenheimer, Meyvis, & Davidenko, 2009), and were excluded from the analyses according to a decision made prior to conducting the study.

**Procedure.** Participants were randomly assigned to one of three conditions. In the instrumental use condition, participants then read about an organization that used the American flag in a recent campaign because their analyses indicated that it would be a profitable approach. In other words, the organization used patriotism to serve their self-interest. In the value-consistent use condition, participants likewise read about an organization that used the American flag in a recent campaign, but their primary aim was to promote national pride. Thus, in both the instrumental use and value-consistent use conditions, a national symbol was used, but to different ends. Participants in the control condition read neutral text and then proceeded to the measures.
In all conditions, participants then completed filler items, which comprised items adapted from Byrne’s (1971) Attitudes Scale (e.g., “I like classical music”; “I prefer practical jokes to verbal humor”). For the key dependent measure, participants then completed a measure of sacred values adapted from Hanselmann and Tanner (2008). Specifically, participants indicated their level of agreement or disagreement on a scale from 1 (Strongly disagree) to 7 (Strongly agree) with four statements assessing how sacred national symbols were perceived to be (e.g., “The flag and other national symbols are sacred”; “The flag and other national symbols should never be sacrificed, no matter what the benefits”). The items were highly correlated (Cronbach’s alpha = .90), and were averaged to create a single measure of sacredness. Participants also completed a manipulation check regarding the intent of the organization in the instrumental use and value-consistent use conditions (i.e., “The organization launched the campaign to increase profits”), and completed demographic measures.

Results and Discussion

Manipulation Check. Participants in the instrumental use condition were significantly more likely to report that the campaign was intended to yield profits ($M = 6.32, SD = 1.07$) than were those in the value-consistent use condition ($M = 4.08, SD = 1.79$), $t(102) = 7.80, p < .001, d = 1.53$, indicating that the manipulation was successful.

Sacredness of National Symbols. Perceptions of sacredness differed significantly by condition, $F(2, 152) = 4.27, p = .016, \eta^2 = .053$. Pairwise comparisons revealed that participants perceived national symbols to be less sacred in the instrumental use condition ($M = 3.82, SD = 1.63$) than in either the value-consistent use condition ($M = 4.61, SD = 1.45$), $t(102) = -2.60, p = .011, d = 0.51$, or the control condition ($M = 4.52, SD = 1.35$), $t(102) = -2.32, p = .022, d = 0.45$. 
No significant difference in perceived sacredness emerged between the value-consistent use and control conditions, \( t(100) = 0.30, p = .769, d = 0.06 \).

Viewing the instrumental use of a sacred value led participants to subsequently perceive that value to be less sacred compared with viewing the value used in a value-consistent manner or simply reading about a value-relevant object. These results provide initial support for the value corruption hypothesis.

**Study 2**

In Study 2, we sought to demonstrate the specificity of the value corruption effect by demonstrating that value corruption occurs for sacred, but not more secular values. To the degree that sacred values are corrupted by instrumentality because this act constitutes a violation of the properties of sacred values, this effect should be attenuated for values that are more secular in nature, as instrumental use is consistent with the properties of secular values (Durkheim, 1915; Tetlock, 2003).

**Method**

**Participants.** Participants were 236 American adults (mean age = 32.7 years; 62% female) recruited on Amazon’s Mechanical Turk. An additional 24 adults were recruited, but 15 failed an attention check and 9 did not complete the study, and were therefore excluded from the analyses according to a decision made prior to conducting the study.

**Procedure.** Participants were randomly assigned to 1 of 4 conditions (value: sacred vs. non-sacred) x 2 (instrumentality: instrumental use vs. value-consistent use) between-subjects design. Participants were informed that they would participate in a study about interpersonal communication, and would view a message that was written by another individual named Jamie. In all conditions, Jamie was soliciting donation requests. In the instrumental use condition, he
was a being paid $0.25 for every $1 collected. In the value-consistent condition, Jamie was a volunteer. In the sacred value condition, Jamie was collecting donations for the Wildlife Conservation Network, which supports environmentalism. In the non-sacred value condition, Jamie was collecting donations for Accel Innovations², which supports innovative and creative projects.

After completing filler items, participants completed measures of value sacredness for both environmentalism and innovation. As in Study 1, the sacredness items were from Hanselmann and Tanner (2008) (“Environmentalism is sacred”; “Environmentalism should not be sacrificed, no matter the benefits”; “I feel committed to environmentalism”; “There are principles involved in this topic that I would defend under any circumstances”). Participants then completed a manipulation check regarding the intent of the organization in the instrumental use and value-consistent use conditions (i.e., “Jamie is seeking to increase his profits”), and completed demographic measures. Participants also answered the open-ended question, “What do you think the researchers are testing in this study?”

Results and Discussion

Manipulation Check. Entering the value and instrumentality conditions into a two-way analysis of variance revealed a significant effect of instrumentality condition on perceived profit motivation, $F(2, 232) = 190.72, p < .001, \eta^2 = .438$. Participants in the instrumental use conditions were significantly more likely to report that the campaign was intended to yield

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² In another pretest using a separate sample of 56 participants, a paired t-test revealed that participants rated environmentalism as significantly more sacred ($M = 4.66, SD = 1.27$) than innovation ($M = 3.46, SD = 1.67$), $t(55) = 6.20, p < .001$, though these values were rated as similarly important ($M_{\text{enviro}} = 6.23, SD = 1.04$; $M_{\text{innovation}} = 6.20, SD = 0.92$), $t(55) = -0.306, p = .761$. 
profits ($M = 6.14, SD = 0.78$) than were those in the value-consistent use conditions ($M = 2.45, SD = 1.56$), $t(115) = 16.25, p < .001, d = 2.95$.

**Sacredness.** A two-way analysis of variance revealed a significant interaction between instrumentality (instrumental use vs. value-consistent use) and value (sacred vs. non-sacred) conditions in predicting the perceived sacredness of environmentalism, $F(1, 232) = 7.37, p = .007, \eta^2 = .031$, a significant main effect of instrumentality condition, $F(1, 232) = 13.39, p < .001, \eta^2 = .055$, and no main effect of value condition, $F(1, 232) = 0.019, p = .890, \eta^2 = .000$ (see Figure 1). Breaking down this significant interaction revealed that participants who witnessed the sacred value (environmentalism) used instrumentally rated environmentalism as significantly less sacred ($M = 3.66, SD = 1.32$) than did those who witnessed environmentalism used in a value-consistent manner ($M = 4.72, SD = 1.09$), $t(115) = -4.72, p < .001, d = 0.87$. Participants who observed the secular value (innovation) used instrumentally did not differ in their ratings of the sacredness of environmentalism ($M = 4.08, SD = 1.44$) from those who observed innovation used in a value-consistent manner ($M = 4.23, SD = 1.12$) conditions, $t(117) = -0.64, p = .522, d = 0.12$.

Assessing the effect of instrumentality and value conditions on the perceived sacredness of innovation yielded no significant interaction, $F(1, 232) = 1.28, p = .259, \eta^2 = .006$, no main effect of value condition, $F(1, 232) = 1.08, p = .302, \eta^2 = .005$, and a marginal effect of instrumentality condition, $F(1, 232) = 3.43, p = .065, \eta^2 = .015$. Participants viewed innovation as similarly sacred across the non-sacred value/instrumental use ($M = 3.42, SD = 1.34$), non-sacred value/value-consistent use ($M = 3.55, SD = 1.18$), sacred value/instrumental use ($M = 3.41, SD = 1.43$), and sacred value/value-consistent use conditions ($M = 3.93, SD = 1.41$).

[Insert Figure 1 about here]
Taken together, the results of Study 2 support the value corruption hypothesis: When a sacred value was used toward instrumental gain, participants subsequently viewed that value as less sacred as compared to when they observed that value used toward value-consistent purposes, or viewed a non-sacred value used for instrumental or value-consistent purposes. Study 2 thus demonstrates discriminant validity: The value corruption effect is attenuated when a value that is perceived as more secular in nature is used toward instrumental gain.

**Study 3**

One critique of Studies 1 through 3 is that the organizations’ instrumental versus value-consistent motives were made explicit to participants. Although this approach facilitates internal validity and is consistent with prior manipulations of instrumentality (e.g., Newman & Cain, 2014), external validity is compromised as most individuals and organizations would not freely announce their instrumental motives. Study 3 was designed to address this issue by using real campaigns from Earth Day. Earth Day is an annual event celebrated on April 22nd to demonstrate support for environmental protection. While pure in its origins, many organizations (whose motives have been treated with suspicion; Yoon, Gurhan-Canli, & Schwarz, 2006) have become involved in Earth Day campaigns (Verhovek, 2000). To test the value corruption prediction, participants viewed real Earth Day campaigns that varied in perceived instrumentality (or no campaign in the baseline control condition), and then completed measures of the perceived sacredness of environmental protection and demographic measures.

**Method**

**Participants.** Participants were 223 American adults (mean age = 33.4 years; 64% female) recruited online on Earth Day 2015. An additional 17 adults were recruited, but failed an
attention check or did not complete the study, and were excluded from the analyses according to a decision made prior to conducting the study.

*Procedure.* Participants were invited to participate in a study ostensibly designed to assess their beliefs and attitudes. Participants were randomly assigned to one of three conditions: instrumental use vs value-consistent use vs baseline control. In the instrumental and value-consistent use conditions, participants were first asked to evaluate and recall details from public Earth Day campaigns. Participants in the instrumental use condition viewed a real campaign pretested\(^3\) to be high on perceived instrumentality (an online post from *Nascar* featuring an image of the Earth reading “Happy Earth Day”), while participants in the value-consistent use condition viewed a campaign pretested to be low on perceived instrumentality (an online post from the *Wildlife Conservation Society* featuring an image of the Earth also reading “Happy Earth Day”). Control participants did not view a campaign.

After viewing the campaign, participants completed filler items about the aesthetics of the campaign (e.g., “The image was engaging”). Participants then completed measures assessing perceived sacredness and the manipulation check. Participants also indicated their age, gender, ethnicity, education level, and political orientation (on a scale from 1 = *Extremely liberal* to 5 = *Extremely conservative*). Finally, participants also answered the open-ended question, “What do you think the researchers are testing in this study?”

*Measures*

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\(^3\) To identify real Earth Day campaigns associated with instrumentality, 49 participants rated, on 7-point scales, a series of Earth Day campaigns on the extent to which they were associated an intention to “primarily increase profits”. The campaign selected as instrumental was associated with significantly higher intentions to pursue profits \((M = 5.98, SD = 1.09)\) than was the campaign selected as the value-consistent campaign \((M = 2.51, SD = 1.31)\), paired \(t(48) = 9.10, p < .001\). These campaigns did not significantly differ in terms of engagement, ease of reading, and aesthetic appeal, \(ps > .376\).
**Manipulation Checks.** Participants indicated their agreement or disagreement with the statements “The campaign primarily intended to increase profits” and “The campaign primarily intended to support the environmental cause” on scales from 1 (*Strongly Disagree*) to 7 (*Strongly Agree*).

**Sacredness.** Using items from prior research (Chakroff et al., 2013; Gino, Kouchaki, & Galinsky, 2015), participants were asked to rate the value of environmentalism along six dimensions (“pure,” “tainted” (reverse-coded), “clean,” “sacred,” “should not be sacrificed, no matter the benefits”; “involves principles that I would defend under any circumstances”; $\alpha = .85$). This measure draws on models of moral psychology which suggest that purity is an inherent element of sacred entities, and the derogation of sacred entities is associated with contamination or impurity (Graham & Haidt, 2011; Uhlmann & Zhu, 2014; Shweder et al., 1997).

**Results and Discussion**

In analyzing the control variables, only political orientation significantly predicted perceived sacredness, $b = -.22, p = .005$, with more conservative participants perceiving environmental protection to be less sacred. This finding is consistent with past research (e.g., Dunlap, Xiao, & McCright, 2001; Feinberg & Willer, 2013). Because political orientation did not significantly interact with the predictor variable, nor did controlling for this variable affect the results, political orientation will not be discussed further.

Examining the critical dependent measure, an ANOVA examining the effect of condition on sacredness revealed a significant effect of condition, $F(2, 220) = 5.94, p = .003, \eta^2 = .051$. Participants in the instrumental use condition ($M = 5.18, SD = 1.16$) rated environmentalism as significantly less sacred than did those in the value-consistent ($M = 5.71, SD = 0.96$) and control
conditions \((M = 5.65, SD = 1.04)\), \(ts > -2.55, ps < .012, ds > 0.40\). The value-consistent and control conditions did not significantly differ from each other \((p = .778)\).

Study 3 again found support for the value corruption hypothesis: Participants who observed a real Earth Day campaign pretested to score high on instrumentality subsequently viewed environmentalism as less sacred compared with participants who viewed an Earth Day message rated low on instrumentality or participants who observed no message.

**Study 4**

Study 4 had two three goals. The first was to test whether mere contemplation of instrumental use would be sufficient to demonstrate the current effect. According to circular models of values and goals, different values have motivational aims that are consistent with each other, oppose each other, or are orthogonal to each other (Schwartz, 1992, 1994; Maio, Pakizeh, Cheung, & Rees, 2009). Instrumental goals, such as profit, tend to be in opposition to what are called universalism values, such as equality and environmentalism. It is therefore possible that merely activating a motivationally incompatible goal (e.g., profit motives) decreases the motivational power of the sacred value (e.g., environmentalism). In Study 4 we sought to compare this priming account with our account by exploring the effects of instrumental use on value sacredness before and after it occurs. We predict corruption effects will only occur after the sacred value has been used in an instrumental manner, when the violation of the value’s sacred status has occurred. In the case of mere contemplation, the value has not yet been tied to instrumental aims (e.g., Baron & Leshner, 2000; Baron & Spranca, 1997; Lichtenstein, Gregory, Irwin, 2007; Mandel & Vartanian, 2008). Second, participants completed an implicit measure of sacredness in addition to explicit measures. This served to rule out possible alternative explanations based on demand characteristics. Finally, we assessed an important behavioral
outcome: participants’ willingness to donate to value-relevant (or less relevant) charities. If the corruption effect is specific to the value that has been used instrumentally, participants’ general willingness to support values-driven causes will not be affected; rather, they will be less willing to donate to causes relevant to the corrupted value.

To test these predictions, participants were randomly assigned to 1 of 3 conditions in a between-subjects design: instrumental-use vs. contemplated instrumental-use vs. value-consistent use. After reading about an environmental campaign promoted for instrumental or value-consistent reasons, participants completed measures of value sacredness and their willingness to donate to an environmental cause or other causes.

**Method**

**Participants.** Participants were 128 undergraduates at a Midwestern university ($M_{age} = 20.59$ years; 59% female). We sought to collect 150 participants (i.e., 50 per cell), but some participants did not show up to their appointments, and we ended data collection at the end of the spring (final) academic quarter.

**Procedure.** Participants first read initial instructions indicating that they were participating in a study assessing evaluations and recall of various communications. In all conditions, they then read a transcript between a CEO and a board of directors. In the instrumental-use condition participants read the following scenario:

The CEO of a company spoke to the company’s board of directors and said, “We are thinking of starting a new environmental campaign.

The chairman of the board replied, “Good, green sells. Let’s make as much profit off of this as we can.”
After deliberating, the board members all agreed, and the company went forward with the environmental campaign.

The contemplated instrumental-use condition was similar, except that instead of going forward with the campaign, participants read that “the board members will deliberate the issue.” In the value-consistent use condition, participants read a similar dialogue, but chairman of the board will instead reply, "Good, let's increase our commitment the environmental cause as much as we can." Participants then completed the implicit measure of the purity component of sacredness. This measure draws on the tendency for purity (impurity) to be grounded in the perceptual experience of the color white (black) (Sherman & Clore, 2009; Sherman, Haidt, & Clore, 2012). Specifically, under the guise of a study on image recall task, participants were exposed to a series of neutral (e.g., “table,” “forecast”) and value-relevant words (e.g., “green,” “environmental”) in fonts varying in color on the white-black spectrum (adapted from Meier, Robinson, Crawford, & Ahlvers, 2007). After viewing each word, participants viewed a blank screen for 11 seconds before being asked to indicate the shade of the previous word on a 5-point gray shades scale (see Appendix for sample materials). The degree to which participants associate environmentalism with darker as opposed to lighter shades served as an index of perceived impurity.

After completing the implicit measure, participants completed explicit measures of the perceived sacredness of environmentalism (as in Study 3), and of other values (fairness, health, social order, pleasure, and equality). Participants then completed the manipulation checks and demographic measures. To assess donation behavior, participants were told that they have been provided with an additional $3, which they could either keep or donate part or all of to a charity. Participants then indicated their desired donation amount (from $0 to $3), and were provided
with three charities that they could select from, which captured different sacred values: The Sierra Club (environmental protection), Get Fit Foundation (health/well-being), and Teaching Tolerance (diversity) (see the Appendix for materials).

**Results and Discussion**

*Implicit Measure.* Consistent with Meier et al., we analyzed participants’ number of correct choices by word color. To the degree that participants associate a given concept with darkness rather than lightness, they will be less accurate on trials in which the concept appeared in a lighter font. Participants’ lightness-matching judgments were accurate on average 43.16% (SD = 15.77%) of trials (chance accuracy = 20%). Thus, although the task was challenging, perceptual judgments were more accurate than chance, t(127) = 16.62, p < .001, d = 2.95. Performance on the light and medium trials was significantly correlated with the explicit measure of sacredness, rs > .27, ps < .002. Performance on the dark trials was not, r = .00, p = .99.

There was a main effect of condition on the correct categorization of light trials, F(2, 125) = 3.41, p = .036, η² = .052. Participants in the instrumental-use condition answered significantly fewer questions correctly (M = 0.81, SD = 0.82) than did those in the contemplation condition (M = 1.40, SD = 1.19), t(83) = -2.67, p = .009, d = 0.58, and marginally fewer than did those in the in the value-consistent use condition (M = 1.18, SD = 1.11), t(84) = -1.76, p = .083, d = 0.38. The contemplation and value-consistent conditions did not significantly differ (p = .385). There was also a main effect of condition on the correct categorization of medium trials, F(2, 125) = 3.71, p = .027, η² = .056. Participants in the instrumental-use condition answered significantly fewer questions correctly (M = 1.00, SD = 0.87) than did those in the contemplation condition (M = 1.50, SD = 0.99), t(83) = -2.47, p = .016, d = 0.53, and significantly fewer than
did those in the value-consistent use condition \((M = 1.52, SD = 1.10)\), \(t(84) = -2.39, p = .019, d = 0.51\). The contemplation and value-consistent conditions did not significantly differ \((p = .959)\). No significant differences emerged on the dark trials, or for the neutral words.

**Explicit Ratings of Sacredness.** There was a main effect of condition on ratings of value sacredness, \(F(2, 125) = 17.02, p < .001, \eta^2 = .214\). Participants in the instrumental-use condition rated environmentalism as significantly less sacred \((M = 3.07, SD = 0.98)\) than did those in the value-consistent use condition \((M = 4.22, SD = 0.96)\), \(t(84) = -5.46, p < .001, d = 1.18\), or in the instrumental-use contemplation condition \((M = 4.00, SD = 0.96)\), \(t(83) = -4.44, p < .001, d = 0.96\). Participants in the value-consistent use and control conditions did not differ significantly \((p = .562, d = 0.22)\). Participants’ ratings of the sacredness of other values (fairness, health, social order, pleasure, and equality) did not differ by condition \((ps > .332)\).

**Donation Behavior.** There was a main effect of condition on whether or not participants donated to the environmental cause, \(\chi^2 = 7.44, p = .024\). While only 2\% of participants in the instrumental-use condition donated to the environmental cause, 24\% did so in the contemplation condition, and 26\% did so in the value-consistent use condition. There was also a significant effect of condition on the amount they donated to the environmental cause, \(F(2, 125) = 3.34, p = .039, \eta^2 = .051\). Participants in the instrumental-use condition donated significantly less money to the environmental cause \((M = $0.02, SD = 0.15)\) than did participants in the contemplation condition \((M = $0.33, SD = 0.82)\), \(t(83) = -2.45, p = .017, d = 0.53\), and participants in the value-consistent use condition \((M = $0.35, SD = 0.35)\), \(t(84) = -2.68, p = .009, d = 0.58\). Participants in the contemplation and value-consistent use conditions did not differ in donation amount \((p = .929)\). There was no effect of condition on overall donation amount, nor were there any differences between conditions in donations to other causes.
Study 4 again found support for the value corruption hypothesis: Participants who observed the instrumental use of environmentalism subsequently viewed that value as less sacred, as captured by both implicit and explicit measures, and were less willing to donate to the environmentalism cause. Moreover, the instrumental use had to have taken place for the value corruption effect to occur, suggesting that merely activating a motivationally incompatible value does not reduce the perceived sacredness of sacred values.

**Study 5**

The main goal of Study 5 was to reconcile the current effect with prior research on taboo tradeoffs and value protection effects (e.g., Ginges et al., 2007; Tetlock et al., 2000). Although our results are consistent with the theorized effects of marketization (e.g., Sandel, 2012), at first glance they may appear inconsistent with previously documented value protection effects. For example, Tetlock and colleagues (2000) found that people react to “taboo” uses of sacred values with a desire to punish the perpetrator and by demonstrating an increased commitment to the value, which ostensibly serves to protect their sacred values. We propose that the current research captures a distinct phenomenon. Namely, the instrumental uses in the current research are less severe than what has been examined in the existing literature. In the current research, observers witnessed a sacred value used instrumentally, but used in a manner that did not directly compromise or sacrifice the value itself. In contrast, in the taboo tradeoffs literature, the values are compromised for the sake of economic value.

We propose that the milder instrumental uses of interest in the current research ironically create greater changes in perceived sacredness by inducing less defensive reactions (Gilbert et al., 2004). To the extent that instrumental use does not elicit sufficient value threat, people will not engage in the psychological process to defend it (e.g., by donating to value-relevant causes).
We test this prediction by randomly assigning participants to observe an instrumental values use, a value-consistent use, or a classic taboo tradeoff (instrumentality + value compromise).

**Participants.** We recruited 150 participants (i.e., 50 per cell) on Amazon’s Mechanical Turk. An additional twelve participants enrolled. Eleven participants were then removed deleted for spending less than 5 seconds reading the vignette, leaving a total of 151 participants ($M_{age} = 32.30$, $SD_{age} = 9.62$; 45% female).

**Procedure.** Participants first read initial instructions indicating that they were participating in a study assessing their evaluations and recall of various images. As in Study 4, all participants then read a transcript between a CEO and a board of directors. The instrumental- and value-consistent use conditions were the same as in Study 4, but this time in the context of diversity. In the taboo tradeoff condition, we adapted a scenario from Hanselmann and Tanner (2008) in which a CEO must decide whether to prevent discrimination or save the money for other organizational purposes (e.g., marketing, research and development). Participants then completed the measure of value sacredness (as in Study 4), and a measure of moral outrage. The outrage measure came from Tetlock et al. (1999), and asked participants to indicate how disgusted, upset, saddened, outraged, angry, and offended they felt on a scale from 1 (*Not at all*) to 7 (*Very much so*) ($\alpha = .97$). Participants then completed the manipulation checks and demographic measures.

Similar to Study 4, participants were told that they have been provided with an additional $0.25, which they could either keep or donate part or all of to a charity. Participants then indicated their desired donation amount (from $0$ to $0.25$), and were provided with three charities that they could select from, which captured different sacred values: The Sierra Club...
(environmental protection), Get Fit Foundation (health/well-being), and Teaching Tolerance (diversity).

**Results and Discussion**

**Sacredness.** There was also a main effect of condition on sacredness judgments, $F(2, 149) = 13.67, p < .001, \eta^2 = .155$. Participants in the instrumental use condition rated diversity as significantly less sacred ($M = 3.40, SD = 1.03$) than did participants in the value-consistent use condition ($M = 4.07, SD = 1.15$), $t(105) = -3.20, p = .002, d = 0.62$, or the taboo tradeoff condition ($M = 4.52, SD = 1.09$), $t(99) = -5.30, p < .001, d = 1.05$. Participants in the taboo tradeoff condition rated diversity as marginally more sacred than did participants in the value-consistent use condition, $t(94) = 1.95, p = .054, d = 0.40$.

**Outrage.** There was also a main effect of condition on feelings of outrage, $F(2, 149) = 38.06, p < .001, \eta^2 = .338$. Participants in the taboo tradeoff condition experienced significantly more outrage ($M = 4.22, SD = 1.65$) than did participants in the instrumental use condition ($M = 2.86, SD = 1.83$), $t(99) = 3.90, p < .001, d = 0.78$, or the value-consistent use condition ($M = 1.44, SD = 1.09$), $t(94) = 9.85, p < .001, d = 2.01$. Participants in the instrumental use condition experienced significantly more outrage than did participants in the value-consistent use condition, $t(105) = 4.81, p < .001, d = 0.92$.

**Donation Behavior.** There was no effect of condition on willingness to donate to charity in general, $F(2, 149) = 0.03, p = .971, \eta^2 = .000$. There was, however, an effect on willingness to donate to the diversity cause, $F(2, 149) = 3.32, p = .039, \eta^2 = .043$. Participants in the instrumental use condition donated significantly less to the diversity cause ($M = \$0.01, SD = 0.05$) than did participants in the taboo tradeoff condition ($\$0.05, SD = 0.10$), $t(99) = -2.64, p = .010, d = 0.54$, and marginally less than in the value-consistent use condition ($M = \$0.04, SD = \ldots$.)
0.09), $t(104) = -1.94, p = .055, d = 0.38$. The taboo tradeoff and value-consistent use conditions did not significantly differ, $t(93) = .71, p = .482, d = 0.15$.

Study 5 again found support for the value corruption hypothesis. Study 5 further reconciled the value corruption effect with value protection effects found previously. Compared with exposure to the instrumental use of diversity, exposure to a taboo tradeoff led to significantly more outrage, and no drop in perceived sacredness or willingness to donate to a value-relevant cause. Participants in the taboo tradeoff condition were even slightly more likely to perceive diversity as sacred compared to those exposed to a pro-diversity message.

**Study 6: Shifting Standards**

In Study 6, we examined whether participants who witnessed a value being used for instrumental gain would subsequently be more willing to endorse ambiguous uses of the value as being value-consistent. One key marker of the sacred is people’s unwillingness to blend elements of the secular with the sacred, and exert effort to restrict the meaning of that sacred object or value (Belk et al., 1989; Ginges & Atran, 2009; Tetlock, 2002, 2003). To the extent that a value becomes less sacred, people may hold less stringent criteria for what comprises a value-consistent use. To test this idea, participants completed the instrumentality manipulation as in Study 3, and then rated three organizations which were pretested to vary in people’s willingness to describe them as pro-environmental. Participants indicated the degree to which they believed the organizations were pro-environmental.

**Method**

**Pretest.** To select the stimuli for Study 7, 58 participants recruited on Amazon’s Mechanical Turk rated the degree of pro-environmentalism (on a scale from $1 = \text{not at all}$ to $7 = \text{a great deal}$) of ten organizations that have made “green” claims, but range in the degree to
which they might be perceived as driven by environmentalism (versus profit). On the basis of these results, we selected three organizations that ranged in ratings of pro-environmentalism for use in Study 6. The less pro-environmental organization (Toyota, an automotive manufacturer) was rated as significantly less pro-environmental ($M = 4.10, SD = 1.60$) than was the moderately pro-environmental organization (West Paw, a sustainable pet store) ($M = 5.55, SD = 1.26$), $t(57) = -6.46, p < .001, d = -1.71$, or the highly pro-environmental organization (Conservation International, a nonprofit environmental organization), $t(57) = -8.67, p < .001, d = -2.30$. The moderately environmental organization was rated as less pro-environmental than was the highly pro-environmental organization, $t(57) = -4.66, p < .001, d = -1.22$.

**Participants.** Participants were 142 American adults recruited on Amazon’s Mechanical Turk. An additional 23 participants were recruited, but failed an attention check, and were excluded from the analyses according to a decision made prior to conducting the study.

**Procedure.** Participants were randomly assigned to 1 of 3 conditions (instrumental use vs. value-consistent use vs. pure instrumentality) in a between-subjects design. As in Study 3, participants were informed that they would participate in a study about interpersonal communication, and would view a message that was written by another individual named Jamie. In all conditions, Jamie was soliciting donation requests. In the instrumental use condition, he was a being paid $0.25 for every $1 collected. In the value-consistent condition, Jamie was a volunteer, collecting donations for the Wildlife Conservation Network, supporting the environmentalism cause. In the pure instrumentality condition, Jamie was working for Accel Innovations for profit. The pure instrumentality condition was again included to rule out the possibility that mere exposure to self-interested behavior might make participants less concerned
about the sacredness of the environmental cause (e.g., Evans, Maio, Corner, Hodgetts, Ahmed, & Hahn, 2013).

After completing filler items, participants were asked to rate each of the three organizations from the pretest in a randomly generated order. For each, participants completed a 5-item scale designed to assess the degree to which the organizations were rated as pro-environmental (e.g., “To what extent would you rate this organization as pro-environmental?”) on a scale from 1 (Not at all) to 7 (A great deal). Participants then completed a manipulation check regarding the intent of the organization in the instrumental use and value-consistent use conditions (i.e., “Jamie is seeking to increase his profits”), and completed demographic measures. Participants also answered the open-ended question, “What do you think the researchers are testing in this study?”

Results

Manipulation Check. There was a main effect of condition on perceived profit motivation, $F(2, 139) = 113.01, p < .001, \eta^2 = .619$. Participants in the instrumental use condition ($M = 6.04, SD = 1.38$) and in the pure instrumentality conditions ($M = 6.38, SD = 1.00$) were significantly more likely to report that Jamie was motivated by profit than were those in the value-consistent use condition ($M = 2.43, SD = 1.77$), $t(93) = 12.43, p < .001, d = 2.58$, and $t(93) = 13.44, p < .001, d = 2.79$, respectively. The instrumental use and pure instrumentality conditions did not significantly differ, $t(93) = 1.34, p = .489, d = 0.28$. Participants in the instrumental use conditions were significantly more likely to report that the campaign was intended to yield profits ($M = 6.14, SD = 0.78$) than were those in the value-consistent use conditions ($M = 2.45, SD = 1.56$), $t(115) = 16.25, p < .001, d = 2.95$. 
**Pro-environmental Ratings.** There was a main effect of condition on ratings of the less pro-environmental organization, $F(2, 139) = 7.01, p = .001, \eta^2 = .092$, but no main effect of condition on the ratings of the moderately $F(2, 139) = 0.64, p = .527, \eta^2 = .009$, or highly pro-environmental organizations, $F(2, 139) = 0.989, p = .379, \eta^2 = .014$, (see Table 1 for all means and standard deviations). For the less pro-environmental organization, participants in the instrumental use condition were significantly more likely to rate the organization as pro-environmental than were participants in the value-consistent use condition, $t(93) = 3.39, p = .003, d = 0.70$, and the pure instrumentality condition, $t(93) = 2.57 = .030, d = 0.53$. There were no significant differences between the value-consistent and pure instrumentality conditions, $t(93) = 0.83, p = .684, d = 0.17$.

Study 6 again finds support for the value corruption hypothesis, this time using a different indicator of value sacredness: a relaxing of standards for what constitutes value-consistent use.

**General Discussion**

As sacred values like environmental-protection, patriotism, and diversity have become increasingly employed by organizations, marketers, and leaders to yield profits and favorable reputations, it becomes increasingly important to understand how this tendency shapes the meaning of the values themselves. In the current research, we present six studies demonstrating that instrumentality can desacralize values. Across values and value uses, participants who observed a sacred value used instrumentally subsequently rated that value as less sacred compared to participants who had not observed the value used in this manner. As a result, observers of instrumental uses of values were less willing to donate to causes relevant to the value, and held less strict criteria for what constitutes a value-consistent use.
The present research provides important theoretical contributions. First, existing research has primarily focused on taboo tradeoffs, in which a sacred value is explicitly traded off in exchange for secular concerns, such as profit (Baron & Spranca, 1997; Ginges et al., 2007; Tetlock et al., 2000). In the current research, we identify and examine another means by which values violations can occur, a more commonplace type of violation in which sacred values are used toward self-interest. Our findings speak to discussions surrounding the negative effects of markets on human values and social institutions. For example, Sandel’s (2012) *What Money Can’t Buy* raises provocative questions around practices like paying to hunt endangered species and to skip lines at amusement parks. Sandel argues that “when we decide that certain goods may be bought and sold, we decide, at least implicitly, that it is appropriate to treat them as commodities, as instruments of profit and use. But not all goods are properly valued in this way” (p. 9). The current research offers empirical evidence supporting this assertion, and also suggests the conditions under which instrumentality is likely to undermine our sacred values.

In a related vein, the current findings shed light on how culture and values might shift over time. The psychological and organizational behavior literatures have often treated human values as primarily stable (e.g., Feather, 1971; Rokeach, 1973; Schwartz, 1997; cf., Chatman, 1991). Indeed, much of the research on values relies on the assumption that values are akin to personality characteristics. It is therefore unsurprising that existing research has primarily separately examined how relatively stable individual differences (e.g., political orientation, group membership; Ginges et al., 2007; Graham & Haidt, 2012) predict the perceived sacredness of values. We extend scholars’ understanding of human values by suggesting how situational influences (e.g., values used for monetary or reputational gain) can influence perceptions of values.
The current results also have important managerial and organizational implications. Namely, organizations should take great care when communicating initiatives relevant to sacred values (e.g. Corporate Social Responsibility initiatives), lest they undermine the very values from which they intend to benefit. An increasingly common narrative in organizations is “doing well by doing good”, which captures the belief that the pursuit of social values and profit is a win-win. Implicit in this approach is the subordination of values to profit, such that “doing good” is a subordinate goal whose use is determined by the degree to which it facilitates “doing well” (Knobe, 2010; Monin & Kreps, 2011). Corporations that publish corporate citizenship reports are often quick to note that the intent of their actions is primarily instrumental. For instance, Wells Fargo & Co.’s 2009 Corporate Social Responsibility (CSR) report stated “this is not about charity”—serving as a signal to customers, clients, and shareholders to generate revenue. One way in which this subordination is problematic is that it might signal a fragile commitment to values, such that values-driven behaviors would be abandoned if their profitability came into question (Margolis & Walsh, 2001, 2003; Vogel, 2005). Our results suggest that communications that prioritize values as ends in themselves might be more effective (i.e., “doing well and doing good”).

Just as profit-driven organizations can be penalized for making values-based claims, organizations based in sacred domains can face negative consequences for attempting to make a profit. For example, churches and pharmaceutical organizations (which people believe should prioritize the sacred values of spirituality and health) are penalized for leveraging market-based tactics (e.g., rebranding, value based pricing; McGraw, Schwartz, & Tetlock, 2012), and may corrupt values when they do so. At the same time, these organizations also need financial resources in order to function optimally (Pallotta, 2008). Future research could examine whether
phrasing instrumentality as a means to values-based ends may help to preserve the values, and reduce backlash against nonprofits and other value-based organizations drawing on market norms.

Future studies might examine whether the phenomenon identified here also occurs in samples outside of the United States, a place of prominent marketization and of relatively high corporate distrust (Lam, 2014). Would the results be stronger or weaker in nations where marketization is less common? In addition, all measures were assessed relatively soon after the experimental manipulations; how long do these effects last? Finally, the existing studies do not speak to how people who strongly hold a given sacred value (e.g., environmental activists) might seek to protect the value from instrumentality threats. One interesting possibility that we plan to explore is that people who strongly hold a given sacred value engage in a process of value constriction—more narrowly defining which actions and objects are value-consistent following instrumental use. This constriction could serve to protect the sacredness of the value. For example, a wide range of behaviors could be classified as patriotic: wearing a flag pin, buying only American products, involvement in local government, chanting “USA” during the Olympic Games, posting “Happy 4th of July” to social media, or enlisting in the army to defend the country. Some actions are ambiguous with regard to whether or not the actions reflect sincere patriotism. If patriotism has been perceived to have been undermined by instrumental use, people for whom patriotism is important may subsequently view only less ambiguous behaviors as being consistent with the value of ‘patriotism’, in order to maintain the value’s sacredness. Future research may seek out activists or other highly value-driven populations to study this proposed value constriction effect.
Conclusion

As corporations increasingly interface with our sacred values—be it through advertisements, product offerings, or Corporate Social Responsibility initiatives—it becomes increasingly important to understand the effects of blending the worlds of markets and values on perceptions of and commitment to values. The results of the current research suggest that instrumentality can undermine the sacredness of values, and suggests that organizations and leaders should take great care in how they communicate their values and values-based initiatives.
References


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<thead>
<tr>
<th>Environmentalism Ratings</th>
<th>Instrumental Use</th>
<th>Value-consistent Use</th>
<th>Pure Instrumentality</th>
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<td>Low</td>
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<td>4.64 (0.96)</td>
<td>4.79 (0.84)</td>
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<td>5.48 (1.21)</td>
<td>5.30 (1.14)</td>
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<tr>
<td>High</td>
<td>5.78 (1.10)</td>
<td>5.45 (1.42)</td>
<td>5.49 (1.23)</td>
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TABLE 1
Means and standard deviations by condition in Study 6.
FIGURE 1

The perceived sacredness of environmentalism by condition ($\pm SE$).
APPENDIX

Study 4 Donation Behavior

Thank you for your participation.

As a part of doing behavioral research relevant to judgments and values, a large mission for our lab is to make donations available to various nonprofits. In most of our studies, we like to give people the option to participate in these charitable causes.

As such, you have been provided with an additional $3.00 (on top of your base pay), which you may now choose to contribute to charity, or will receive after the study is complete. You can donate as much or as little of the money as you want ($0-3.00).

If you choose to donate, you will be provided with a list of charities to choose from on the next page.

How much would you like to donate today? ______

If you chose to donate:

Thank you for your support. Please select which of the following you choose to donate to:

_____ The Sierra Club: A non-profit geared toward the preservation of land and forest, clean air and water, and a host of other issues (see www.sierraclub.org).

_____ Get Fit Foundation: A non-profit geared toward promoting active, healthy lifestyles (see more at getamericafit.org).

_____ Teaching Tolerance: A non-profit dedicated to promoting diversity and creating equitable school experiences for our nation's children (see more at tolerance.org).
Study 4 Implicit Measure Instructions
Some Instructions Before You Begin Part 2:

This experiment is concerned with your ability to evaluate and recall words. A word will briefly appear on the center of the screen. You should try to remember this word and its meaning for a later memory test. The font color of this word will vary slightly from one of 5 possible shades.

You should **first attend to the meaning of the word**. After you process the meaning of the word, there will be a 10 second pause before you see a color scale with different shades of darkness.

Your task is to decide which color shade matches the font color of the word you have just viewed. You should use the mouse to select the color shading that you believe matches that of the word.