

Embodied Remorse: Physical Displays of Remorse Increase Positive Responses to Public Apologies, but Have Negligible Effects on Forgiveness

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Public apologies struggle to communicate genuineness. Previous studies have shown that, in response to public apologies, perceptions of remorse and levels of forgiveness are often low, while skepticism about motive is high. Furthermore, attempts to reduce mistrust of public apologies by manipulating the verbal component of the message have had limited success. Across 6 studies (combined $N = 3,818$), we examined whether people respond more positively to public apologies if the apologies are accompanied by *nonverbal* demonstrations of remorse: operationalized as kneeling (Studies 1 and 6) and crying (Studies 2–5). Overall, embodied remorse had small-to-medium effects on perceived remorse, and through this relationship had reliable effects on perceived likelihood of reoffending, empathy, positive appraisals of the transgressor, and satisfaction with the apology. Positive effects of embodiment emerged regardless of whether transgressions were committed by a collective (Studies 1, 2, and 6) or an individual (Studies 3–5), and were equally strong regardless of whether or not the transgressor issued an apology (Studies 4 and 5). Furthermore, embodied remorse appeared to lie beyond suspicion: if anything, those low in dispositional trust were more positively influenced by embodied remorse than those high in dispositional trust. Despite all these positive effects, embodied remorse did not have a significant effect on forgiveness in any of the studies, and an internal meta-analysis revealed a significant effect that was of negligible size.


Keywords: apologies, forgiveness, trust, embodied

Imagine receiving an apology that is perfect in its verbal composition, but the person delivering the apology *looks* bored, dismissive, or resentful. Now imagine an apology that is verbally garbled or incomplete, but delivered by somebody kneeling in front of you and sobbing uncontrollably. These scenarios under-

score an intuitively obvious point: humans have a wide repertoire of cues to signify remorse that lie outside words, and that sometimes replace the need for them. People do not only have the capacity to *say* they are sorry; they also have the capacity to *embody* their remorse. Drawing on these cues—both verbal and nonverbal—the receiver of the apology can make an intuitive assessment about whether the apologizer is truly sorry and worthy of forgiveness.

This dynamic and emotionally vivid process is one we typically associate with interpersonal (or “one-to-one”) apologies conducted in private between a transgressor and a victim. In the current article, we instead examine *public* apologies, which operate according to a subtly different set of conventions and logics. There are two broad reasons why somebody would make an apology in public. First, the apologizer may be the representative of a group—whether that be of a government, army, corporation, or religious group—that is seeking to redress harms enacted on a group of people (these are referred to in the literature as “many-to-many” apologies). Second, a public apology may occur if somebody is in the public eye—a politician, sports person, or actor, for example—and is trying to restore his or her individual moral integrity in the eyes of the public (a “one-to-many” apology).

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Public apologies frequently obey a consistent set of conventions: the individual makes the apology in front of cameras, one that is deemed accessible to the broader public. It is almost certain that the words have been carefully prepared and tightly scripted, often in collaboration with advisers. As such, the text usually incorporates all the core elements of what is considered to be a complete apology (Blum-Kulka, House, & Kasper, 1989; Scher & Darley, 1997): an acceptance of responsibility, a statement of regret, a promise that the transgression will not happen again, and a statement on how the transgressor plans to make amends. To the best of their ability, speakers may communicate through their voice and actions the heartfelt nature of the apology, but the highly stage-managed and public nature of the communication makes spontaneous nonverbal expressions of emotion unlikely.

Although public apologies are desired and welcomed (Okimoto, Wenzel, & Hornsey, 2015), there is evidence that they struggle to communicate genuineness: perceptions of remorse and levels of forgiveness are often low, whereas skepticism about motive is high (see Shnabel, Halabi, & SimanTov-Nachlieli, 2015). Furthermore, attempts to increase forgiveness by manipulating the verbal component of a public apology have had limited success (Kirchhoff & Čehajić-Clancy, 2014; Philpot & Hornsey, 2008; Steele & Blatz, 2014). The current research is motivated by the possibility that resolving the trust problem with public apologies lies in the non-verbal rather than the verbal elements of the communication.

When surrounded by cameras and microphones it may seem difficult or incongruous to embody the remorse that is being described. Having said that, embodiment of remorse in public apologies is not without precedent. The most iconic gesture of remorse for Germany's WWII aggression is Willy Brandt's *kniefall* moment, where he knelt, head bowed, in front of a shrine commemorating the victims of the Warsaw ghetto uprising. This silent gesture—that does not meet most technical definitions of an apology because it contained no verbal elements—is remembered long after Germany's numerous official apologies have been forgotten (Lind, 2008; Rauer, 2006). More recently—nearly a decade after Stephen Harper's apology to indigenous Canadians—Prime Minister Justin Trudeau issued a follow-up apology in Parliament, an apology that was widely reported as “tearful” (Bartlett, 2017). The current studies examine whether such physical expressions of remorse can do what words struggle to do: communicate sincerity, overcome skepticism, and elicit forgiveness.

Examining whether embodied public apologies overcome the mistrust typically observed following public apologies is important given the role they are presumed to play in the process of forgiveness and reconciliation (Čehajić-Clancy, Effron, Halperin, Liberman, & Ross, 2011; Čehajić-Clancy, Goldenberg, Gross, & Halperin, 2016; Nadler & Shnabel, 2015; Staub, 2006; Tavuchis, 1991). The effectiveness of public apologies is also relevant to corporations and not-for-profit organizations because consumer trust may decline after scandal, which can negatively affect their bottom line (see Coombs & Holladay, 2008; Gillespie & Dietz, 2009). The current studies also help illuminate theoretical questions about why apologies work in the first place, and the nature of the still-mysterious link between these gestures of remorse and forgiveness. Finally, Studies 3–5 help fill a gap in the literature: although there is a long history of examining one-to-one apologies, and a growing frontier of research examining many-to-many apologies, there is surprisingly little experimental research examining apologies from individuals to the public (i.e., one-to-many apologies).

Research on Apologies, Trust, and Forgiveness

One-to-one apologies are well-understood to be powerful facilitators of forgiveness. A meta-analysis of 175 studies showed that an apology was one of the most powerful predictors of interpersonal forgiveness, greater than any demographic characteristic, any personality characteristic (including the dispositional tendency to forgive) and any relationship characteristic (including closeness of the relationship; Fehr, Gelfand, & Nag, 2010). Whether or not somebody apologizes is a stronger predictor of forgiveness than the severity of the transgression, and almost as predictive as whether or not the transgression was intentional.

There is a paucity of experimental research examining one-to-many apologies; that is, apologies made to the public by an individual for his or her personal transgression. Indeed, we could only find one study that manipulated the presence or absence of an apology from an individual to the public, and examined the downstream consequences for forgiveness. In the context studied (a Japanese soldier issuing a public apology for WWII atrocities he had conspired in), there was a modest positive effect of the apology on forgiveness for that soldier (Philpot & Hornsey, 2008, Study 4).

There are many more studies examining the presence or absence of apologies in an intergroup context; that is, where participants are led to believe that a group had apologized or not apologized for a collective transgression. In contrast to the one-to-one context, the modal result in many-to-many contexts is to find no significant relationship between an apology and forgiveness. There are exceptions: there is some evidence that an apology helps promote forgiveness among Canadians for a friendly fire incident conducted by the American military (Brown, Wohl, & Exline, 2008), and among university students who had been insulted by a letter written by a group of professors (Leonard, Mackie, & Smith, 2011). Over a dozen other studies, however, have found no relation between these two variables (see Hornsey & Wohl, 2013; Hornsey, Wohl, & Philpot, 2015, for reviews). Furthermore, studies that have compared apologies with and without offers of reparation have similarly found nonsignificant main effects of forgiveness (Čehajić-Clancy & Brown, 2019; Kirchhoff & Čehajić-Clancy, 2014). Relative to a no-apology control, many-to-many apologies do tend to increase perceptions that the transgressor group is truly sorry, and increase feelings of satisfaction with the transgressors' response. However, these effects often emerge in the context of a broader picture of skepticism: levels of perceived remorse are often below the midpoint, while perceptions that the apologies are driven by ulterior motives (e.g., political pressure or a desire to avoid punishment) are often close to the ceiling of the scale.

This has led theorists to argue that there are intrinsic barriers to trusting a communication in an intergroup context compared with an interpersonal context. For example, the phenomenon of infra-humanization may induce skepticism about the authenticity of expressions of secondary (i.e., distinctly human) emotions like guilt, shame, and remorse from certain outgroup members (Wohl, Hornsey, & Bennett, 2012). It is also likely that the collective nature of the transgression means that there is a greater leap of faith required to trust that, when a representative delivers a collective apology, the group as a whole is truly sorry. Consistent with this, some have found that grassroots apologies issued by ordinary members of the population tend to be more effective than apologies issued by representatives (Okimoto, Hornsey, & Wenzel, 2019) and

that apologies are more effective if a leader's decision to apologize is supported by popular sentiment or by democratic vote (Wenzel, Okimoto, Hornsey, Lawrence-Wood, & Coughlin, 2017). Even here, however, we see many of the problems associated with traditional apologies: relatively modest levels of perceived remorse, and limited effects on measures of forgiveness.

Embodied Remorse: The Theoretical Case for Positive Effects

The clearest case for why embodied gestures of remorse may yield positive responses from the public rests on the folk theory that nonverbal messages are less controllable than words, and so are truer windows into what the communicator is thinking and feeling (van Kleef, 2016). Tears, for example, are not easily controlled (Provine, 2012; Simons, Bruder, van der Lowe, & Parkinson, 2013) and so people tend to use crying as a sign of honesty and reliability (Vingerhoets, 2013). From the perspective of correspondent inference theory (Jones & Davis, 1965), a verbal apology may be discounted because it is a controllable, socially desirable, and normative expression (Okimoto et al., 2015; Takaku, 2001). In contrast, embodied gestures of remorse may appear to the observer as more spontaneous and distinctive, meaning that perceivers are more likely to infer that the apologizer is genuine. From this perspective, nonverbal gestures may help promote forgiveness via their role in communicating the authenticity of the remorse display. In light of genuine remorse, the transgression may be no longer seen as the actions of an immoral person, but rather a moral person who once did an immoral thing, reducing animosity toward the transgressor and alleviating fears that the offense will be repeated (Gold & Weiner, 2000).

Of course, some embodied gestures of remorse (e.g., bowing, kneeling) are perfectly controllable. However, they may also have positive effects because of what these planned gestures signify. In the comparative psychology literature, gestures such as gaze-aversion, crouching, cringing, flinching, and crying are considered signals of appeasement and/or submission; evolved responses designed to inhibit aggression in others (Gračanin, Bylsma, & Vingerhoets, 2018; Hasson, 2009; Strayer & Trudel, 1984). From a symbolic interactionist perspective, such gestures of submission may be effective in promoting the face of the victims, and to have their self-image acknowledged, accepted, and honored (Brown & Levinson, 1987). From the perspective of the needs-based model of reconciliation, embodied gestures of submission may help enhance the power and dignity of the victim group, something that is presumed to be a prerequisite for reconciliation (Nadler & Shnabel, 2015; Shnabel & Nadler, 2008; Shnabel, Nadler, Ullrich, Dovidio, & Carmi, 2009). Finally, it is possible that culturally sanctioned gestures of remorse may help communicate that the transgressor is now prepared to play within societal rules, which helps reassure the victim that the transgressions are unlikely to be repeated (Goffman, 1971). From all these perspectives, even controllable nonverbal gestures may help communicate that the transgressor is reformed and can now be forgiven.

Empirical researchers have been relatively silent on whether the embodiment of remorse leads to more positive responses from the public. One exception is work showing that defendants who display remorseful facial expressions are judged to be less guilty than defendants who have angry or expressionless faces (MacLin,

Downs, MacLin, & Caspers, 2009). Another exception is an archival study examining the facial affect expressed by a senior executive member while apologizing for corporate wrongdoing (ten Brinke & Adams, 2015). This study found that when representatives used deviant facial affect while apologizing (e.g., smiling), stock market returns were significantly lower 3 months later than when representatives used normative facial affect expressions. In follow-up experiments (ten Brinke & Adams, 2015, Studies 2a and 2b), participants watched a video in which a male actor pretended to be the CEO of a hypothetical airline company that had suffered a computer malfunction, inconveniencing customers. Depending on condition, the actor was instructed to appear happy, sad, or unemotional when apologizing. Apologizers who were instructed to communicate sadness nonverbally were considered more remorseful than those who communicated happiness or no affect, and through this effect, participants reported they were more likely to accept their apology and to support the hypothetical company in the future.

Embodied Remorse: The Theoretical Case Against Positive Effects

As described earlier, the case that embodied gestures of remorse may increase the effectiveness of an apology is both intuitive and grounded in theory. However, it is also possible to build a case that embodied remorse will have little-to-no benefit over and above verbal expressions. Perhaps the strongest case for this comes from the literature on intergroup apologies. Previous researchers have explored a number of strategies that theoretically should help promote forgiveness, including increasing the emotionality of the apology (Philpot & Hornsey, 2008, Experiment 4; Wohl et al., 2012), providing an other-focused apology (Berndsen, Hornsey, & Wohl, 2015), showing that the apology is supported by public sentiment (Okimoto et al., 2019), and having direct victims of the outgroup's aggression urge fellow victim group members to accept the apology and move on (Philpot & Hornsey, 2008, Experiment 3). Although some of these strategies were effective in increasing satisfaction with the apology and perceived remorse, these improvements often came against the backdrop of low scores overall, and failed to flow through into improvement in forgiveness. This high level of skepticism toward collective apologies has been interpreted by some as reflecting a lack of *motivation* on behalf of victim group members to forgive (Hornsey et al., 2015; Philpot & Hornsey, 2011).

If we are to assume that victim group members perceive gestures of remorse through a lens of suspicion, then it is possible that even nonverbal gestures might be seen as inauthentic. Bowing and kneeling can be dismissed as affectations or postures; even crying can be seen as fake, or as an act of manipulation (so-called *crocodile's tears*; ten Brinke, MacDonald, Porter, & O'Connor, 2012). Furthermore, although submissive gestures such as kneeling and crying may help communicate that the perpetrator is more sincere and less aggressive, they are also associated with attributions of being less emotionally stable, incompetent, and weak (Hendriks, Croon, & Vingerhoets, 2008; van de Ven, Meijs, & Vingerhoets, 2017; Vingerhoets, van de Ven, & van der Velden, 2016; Zeifman & Brown, 2011). This may have complex downstream effects in terms of making the attribution as to whether the transgressions are likely to be repeated.

Overview of the Current Studies

A case can be made for the supposition that embodied gestures of remorse will lead to enhanced forgiveness, enhanced satisfaction, and enhanced appraisals of the perpetrator, all via their effects on perceived remorse. However, there are also reasons to believe that there are trust-based “headwinds” that may mute positive responses flowing from embodied gestures of remorse. This article presents six studies that help referee between these competing sets of predictions. Studies 1 and 6 operationalize embodied remorse through kneeling; Studies 2–5 operationalize it through tears. Furthermore, the studies focus on both many-to-many apologies (Studies 1, 2, and 6) and one-to-many apologies (Studies 3–5). Studies 4 and 5 further manipulate the presence or absence of an apology (allowing us to examine the effects of embodied remorse when communicated in the absence of an apology) and the potential role of dispositional trust in moderating these effects. Study 6 examined the potential moderating roles of entity beliefs and the perceived right to forgive.

In all six studies, we report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the studies. Sample size was prescribed in advance; there was no topping up of data after initial collection. Ethical approval for all studies was obtained by the School of Psychology ethics review committee of the lead author (17-PSYCH-PHD-16-AH). Full materials—including the images used in the manipulations—are available and data can be accessed through the Open Science Framework (https://osf.io/524qe/?view_only=d709389f45a344faa6560fca689b0a55).

Study 1

In Study 1, participants read about a real-world event in which senior executives of the Tokyo Electric Power Company apologized to victims of the Fukushima Daiichi nuclear disaster. Half the participants were exposed to images of the executives kneeling and bowing in front of victims, while the other half did not. On the basis of the theory and research outlined earlier, we tentatively predicted that the embodiment of remorse would increase perceptions that the transgressors were experiencing genuine remorse, and that through this the embodiment condition would invoke higher levels of satisfaction with the response, more positive appraisals of the apologizers, higher levels of forgiveness, and lower perceptions that the transgressors would reoffend in the future. As discussed above, however, it is possible that skepticism about motive might act as a suppressor of the apology-forgiveness link, and so we also included a measure assessing perceived ulterior motives of the apologizer (e.g., the extent to which the executives’ responses was motivated by pressure from outside sources and/or concern about getting into trouble).

To increase the generalizability of the findings, and as an exploratory probe of possible moderator effects, we sampled both Japanese (the victimized group members) and Americans (bystander group members). However, we made no a priori predictions about how nationality would influence results. Although the vast majority of our Japanese participants were presumably not directly affected by the disaster (i.e., they were not “direct” victims), the fact that they share a potent social identity with people who *were* directly affected implies a degree of shared victimhood (Brown et al., 2008; Philpot & Hornsey, 2008). Prior research has demonstrated that bystanders are typically more positive about

apologies than are members of the victimized group (e.g., Blatz, Schumann, & Ross, 2009; Hornsey, Okimoto, & Wenzel, 2017). However, it remains to be seen whether the effects of embodied remorse emerge more or less strongly for members of the victim group relative to third parties.

We also manipulated whether the apology was made publicly (in a press conference) or privately (with only victims present). Typically, one-to-many and many-to-many apologies are communicated in a public forum, with a view to reaching a large number of people. It is possible, however, for apologies to be delivered in private, “closed-door” sessions with representatives of the victimized group. One possible payoff of such an approach is that it minimizes the stage-managed nature of the event, which might reduce people’s skepticism about the genuineness of the gesture. In summary, if there *is* skepticism about the motives for the embodied remorse, we reasoned that this might be more evident in the public condition than in the private condition.

Method

Participants and design. A power analysis using G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) determined that 401 participants would be required for an 85% chance of detecting a small–medium effect ($f = 0.15$). We aimed for this sample size as a minimum target. The original sample comprised 228 American participants and 268 Japanese participants recruited online using Social Survey International (SSI). All participants received an apology that either featured kneeling or did not, and which was either presented in public or private. This resulted in a 2 (American or Japanese participants) \times 2 (embodiment or no embodiment) \times 2 (private or public) between-groups design.

Of the original sample, 71 participants either failed a simple attention check or failed to complete the attention check (“We would like to make sure that you are reading the questions. Please select ‘Disagree’ for this question”). These respondents were excluded, leaving 425 participants for analyses. This final sample comprised 196 American participants ($M = 44.45$ years; 54.1% women) and 229 Japanese participants ($M = 45.43$ years; 59.0% women).¹

Experimental stimuli. For the Japanese sample, all stimuli and materials were translated and back-translated via a professional translation company. After going through the information and consent process, all participants read the following text:

Pictured below are employees of the Tokyo Electric Power Company (TEPCO). A TEPCO power plant was the source of the Fukushima Daiichi nuclear disaster. The disaster occurred after an earthquake and tsunami caused radioactive material to leak out of some of TEPCO’s nuclear reactors, leading to several chemical explosions. The disaster

¹ In Studies 1–3—that all incorporated a manipulation of audience—we also included a measure checking on the effectiveness of the audience manipulation. Participants were asked “In what context was this apology expressed?” and were given two options: public press conference and private meeting. Overall, 49 participants failed this manipulation check in Study 1, 52 failed in Study 2, and 10 failed in Study 3. In every case, however, deleting these participants did not change interpretation of the results. Given this, and given that the audience manipulation was included for generalizability reasons rather than being a critical research question in its own right, we report the results below with the manipulation check fails kept in.

is expected to reduce the life expectancy of thousands of people. A follow-up report concluded that the disaster was partly the result of poor safety regulations, dysfunctional equipment, and a slow and inappropriate response following the meltdown. TEPCO “repeatedly played down the risks and suppressed information about the movement of the radioactive plume, so some people were evacuated from more lightly to more heavily contaminated places,” the report stated.

The remaining text differed depending on condition. In the public apology condition, participants received the following text (here and below, information in brackets was provided in the embodied remorse version of the condition only):

Representatives from the company apologized, [kneeling to the ground,] at a public press conference. They expressed sorrow and regret for their company’s mistakes. The picture below is a still from that press conference.

The subsequent image was taken at an event in which TEPCO representatives issued an apology at a press conference. In the no-embodiment condition, the representatives are sitting at a desk in front of microphones. In the embodiment condition, the representatives are away from their desks, kneeling with their palms pressed to the floor. The accompanying group of journalists, holding microphones and cameras, are visible.

In the private apology condition, participants received the following text:

Representatives from the company apologized, [kneeling to the ground,] in a private meeting with community members affected by the disaster. They expressed sorrow and regret for their company’s mistakes. The picture below was taken by one of the attendees of the private meeting.

The subsequent image was taken at an event in which TEPCO representatives issued an apology to families and friends of victims. In the no-embodiment condition, the image was of TEPCO representatives standing with neutral expressions on their faces. In the embodiment condition, the representatives were kneeling with their faces pressed close to the floor. Note that images and accompanying text for this and the other studies reported here can be accessed through the Open Science Framework (https://osf.io/524qe/?view_only=d709389f45a344faa6560fca689b0a55).

Dependent measures. *Response satisfaction* was measured using a scale by Philpot and Hornsey (2008). Participants rated whether the apology was “adequate,” “sufficient,” and “unsatisfactory” (1 = *strongly disagree*, 7 = *strongly agree*; $\alpha = .85$ after reversing the last item).

Ulterior motives was also measured using the same items used by Philpot and Hornsey (2008). Participants were asked whether they thought the apology was made because of: “pressure from outside sources,” “need to avoid punishment,” “concern for their public image,” and “concern about getting into trouble” (1 = *not at all*, 7 = *very much*; $\alpha = .82$).

Participants were then given the stem question “To what extent do the TEPCO executives seem . . .” and rated the target on 11 traits presented in a randomized order (1 = *not at all*, 7 = *extremely*). Two items—emotional and upset—were combined into a single scale of *emotionality*, $r = .45$, $p < .001$. Nine items related to the extent to which participants had a *positive appraisal* of the target: “likable,” “moral,” “warm,” “cold,” “competent,”

“selfish,” “manipulative,” “authentic,” and “arrogant.” Negatively worded items were reversed, such that high scores on this scale reflect more positive appraisals ($\alpha = .89$).

Perceived remorse was measured with seven items. First, participants rated whether they thought the TEPCO executives felt: “remorseful,” “repentant,” and “truly sorry” (1 = *not at all*, 7 = *extremely*). Second, they rated the extent to which they felt the “apology was made due to remorse for what they’ve done” (1 = *not at all*, 7 = *very much*). Finally, they rated their agreement with three statements: “It is possible for me to trust that this expression of remorse was truly meant,” “I trust this expression of remorse,” and “I am skeptical of this expression of remorse” (1 = *strongly disagree*, 7 = *strongly agree*). After reversing the last item, the seven items were combined into a single, reliable scale ($\alpha = .94$).

Likelihood of reoffending was measured by asking participants to what extent they believed that TEPCO “has learned lessons from what they’ve done” (reversed) and “is likely to repeat their mistakes” (1 = *definitely not*, 7 = *definitely*; $r = .45$, $p < .001$).

Forgiveness was measured using a scale adapted from Wohl and Branscombe (2005). Participants were asked to rate the extent to which they agreed with the following statements: “I forgive TEPCO for the harm done,” “I forgive TEPCO for their role in this incident,” “I don’t hold any negative feelings toward TEPCO for their actions,” and “It is not possible for me to forgive TEPCO’s actions” (1 = *strongly disagree*, 7 = *strongly agree*; $\alpha = .89$ after reversing the last item).

Age, sex, political ideology, and education were measured at the end of the survey. To measure political ideology, we asked respondents to locate themselves in terms of where they would lie “in political matters” on a scale from 1 (*left*) to 9 (*right*) and, on a separate scale, from 1 (*liberal*) to 9 (*conservative*). The two items correlated strongly ($r = .53$) and so were combined into a single scale. We also measured participants’ highest levels of education on a 5-point scale (1 = *less than high school*, 2 = *high school*, 3 = *trade qualification*, 4 = *university degree*, 5 = *postgraduate degree*). None of the effects reported changed depending on whether age, sex, education, and political ideology were controlled for in analyses, and this was true for each of the six studies in this article. Consequently, all the analyses reported below were conducted without controlling for demographics. Correlations among the dependent measures are summarized in Table 1.

Results

We analyzed the data using a series of 2 (American or Japanese participants) \times 2 (embodiment or no embodiment) \times 2 (private or public) between-groups analysis of variances (ANOVAs). The critical comparison was between the embodiment and the no-embodiment conditions, and these means and *SDs* are summarized in Table 2. As can be seen in Table 2, ratings of ulterior motives were relatively high overall, whereas ratings of forgiveness and perceived remorse were relatively low, a pattern that is typical of many previous studies examining group-based transgressions (see Hornsey & Wohl, 2013, for a review).

Effects of embodiment. As seen in Table 2, participants saw the TEPCO executives to be more emotional in the embodied than in the nonembodied condition, $F(1, 414) = 24.51$, $p < .001$, $\eta_p^2 = .06$, an effect that is best interpreted as a manipulation check.

Table 1
Correlations Among Dependent Measures: Studies 1 and 2

Variables	1	2	3	4	5	6	7
1. Emotionality	—	.68***	.53***	.67***	-.22***	-.52***	.50***
2. Positive appraisals	.43***	—	.72***	.79***	-.42***	-.71***	.67***
3. Response satisfaction	.39***	.73***	—	.77***	-.39***	-.71***	.72***
4. Perceived remorse	.49***	.81***	.72***	—	-.39***	-.76***	.76***
5. Ulterior motives	-.06	-.28***	-.29***	-.18***	—	.36***	-.38***
6. Likelihood of reoffending	-.26***	-.60***	-.53***	-.67***	.17***	—	-.66***
7. Forgiveness	.34***	.68***	.73***	.72***	-.26***	-.54***	—

Note. Correlations below the diagonal are for Study 1. Correlations above the diagonal are for Study 2.
*** $p < .001$.

Significant main effects of embodiment also emerged on positive appraisals of the TEPCO executives, $F(1, 414) = 18.14, p < .001, \eta_p^2 = .04$, response satisfaction, $F(1, 414) = 16.34, p < .001, \eta_p^2 = .04$, ulterior motives, $F(1, 414) = 6.88, p = .009, \eta_p^2 = .02$, and perceptions of remorse, $F(1, 417) = 18.03, p < .001, \eta_p^2 = .04$. Participants in the embodiment condition had more positive appraisals of the TEPCO executives, were more satisfied with their response, perceived less ulterior motive, and perceived more remorse than participants in the no-embodiment condition. However, no significant main effect of embodiment emerged on likelihood of reoffending, $F(1, 415) = 3.72, p = .055, \eta_p^2 = .01$, or forgiveness, $F(1, 417) = 2.57, p = .11, \eta_p^2 = .01$.

Moderation by audience. The audience manipulation did not feature in any significant main effects (all $ps > .11$) or two-way interactions with embodiment (all $ps > .08$).

Moderation by nationality. Main effects of nationality emerged across all measures: the American sample was more satisfied with the apology, perceived less ulterior motive, perceived more genuine remorse, appraised the TEPCO executives more positively, saw less likelihood of reoffending, and were more forgiving than the Japanese sample (all $ps < .002$, all $\eta_p^2 > .02$). Of more relevance to the current research question, however, was whether the embodiment manipulation had different effects on the American compared with the Japanese samples. The answer was no: none of the nationality by embodiment interactions approached significance (all $ps > .46$).²

Tests of mechanism. We included measures relating to two possible mechanisms explaining the link between embodiment and the dependent measures: (a) a mechanism that presumed positive effects of embodiment through perceived remorse, and (b) a mech-

anism that presumed flat effects of embodiment through ulterior motives. Because participants perceived *lower* ulterior motives in the embodiment than the no-embodiment condition, the second mechanism was not pursued. To explore the first mechanism, we conducted a series of mediation analyses using Model 4 of Hayes' (2013) PROCESS macro, with 95% confidence intervals (CIs) and 10,000 bootstrap samples. In each case, embodiment was the independent variable, perceived remorse was the mediator, and nationality and audience were covariates. As expected, embodiment had an indirect effect via remorse on positive appraisals, $b = .37, SE = .09, 95\% CI [.195, .553]$, and response satisfaction, $b = .40, SE = .10, 95\% CI [.215, .591]$. In both cases, controlling for remorse rendered the effect of embodiment nonsignificant ($ps > .13$). Embodiment also had indirect effects via remorse on forgiveness, $b = .39, SE = .09, 95\% CI [.217, .579]$, and likelihood of reoffending, $b = -.37, SE = .09, 95\% CI [-.556, -.198]$.

Discussion

When the TEPCO executives were portrayed as kneeling, participants were more positive in their appraisals of them and were more satisfied with their response. There was no sign that participants saw the kneeling as fake or manipulative: indeed, participants were less likely to think the apology was driven by ulterior motives if the remorse was embodied. Also, participants saw the TEPCO executives to be more genuinely remorseful if they kneeled, and it was through this mechanism that the embodied remorse had positive effects on other measures. This effect emerged regardless of whether the participants were Japanese (victimized group members) or American (bystander group members). The effect also emerged independently of whether the apology was made in a public press conference or in a private meeting with victims' families.

Table 2
Means (and SDs) Across Levels of Embodiment: Study 1

Measure	Embodiment	No embodiment
Emotionality	4.14 _b (1.32)	3.48 _a (1.38)
Positive appraisals	3.78 _b (1.13)	3.31 _a (1.21)
Response satisfaction	3.53 _b (1.38)	2.98 _a (1.45)
Perceived remorse	3.92 _b (1.37)	3.34 _a (1.57)
Ulterior motives	5.28 _a (1.19)	5.58 _b (1.10)
Likelihood of reoffending	3.88 _a (1.37)	4.13 _a (1.56)
Forgiveness	3.42 _a (1.32)	3.21 _a (1.38)

Note. SDs are in parentheses. Means with different subscripts are significantly different from each other. On likelihood of reoffending, the effect of embodiment is marginal ($p = .055$).

² There was one significant three-way interaction between nationality, embodiment, and audience: on response satisfaction, $F(1, 414) = 4.28, p = .039, \eta_p^2 = .01$. Analysis of simple main effects revealed two effects of embodiment. First, Americans in the public condition were more satisfied in the embodiment condition ($M = 3.85$) than in the no-embodiment condition ($M = 3.09$), $F(1, 414) = 6.93, p = .009$. Second, Japanese participants in the private condition were more satisfied in the embodiment condition ($M = 3.56$) than in the no-embodiment condition ($M = 2.65$), $F(1, 414) = 11.13, p = .001$. No other simple main effects were significant ($ps > .28$). However, given that this is the only significant three-way interaction among seven dependent measures, we do not discuss this effect any further.

However, the embodiment manipulation only had a marginal effect on whether the perpetrators were likely to reoffend and had no effect on participants' willingness to forgive. This represents a nontrivial limitation on the power of embodied remorse to promote reconciliation between victims and perpetrators in intergroup contexts. At this stage, however, it is unclear whether this result accurately captures a tenuous link between embodied remorse and forgiveness, or if it reflects something about the specific operationalization of embodied remorse we used (kneeling) or the specific political and historical context surrounding the Fukushima nuclear disaster. As such, we repeated the design in Study 2 using a different context, a different transgression, and a different operationalization of embodied remorse.

Study 2

In Study 2, American and Korean participants read about an apology made by the South Korean President on behalf of her government for mistakes made before and after the SEWOL ferry disaster. The independent and dependent variables were the same as those used in Study 1, with the exception that embodiment of remorse was manipulated through the presence or absence of tears.

Method

Participants and design. A power analysis determined that 401 participants would be required for an 85% chance of detecting a small-medium effect ($f = 0.15$). As such, this was our minimum targeted sample size. The original sample comprised 255 American and 216 Korean participants, recruited online through SSI. All participants received an apology, which either featured tears or no tears, and which was either presented in public or private. This resulted in a 2 (American or Korean participants) \times 2 (embodiment or no embodiment) \times 2 (private or public) between-groups design.

Of the original sample, 64 participants either failed a simple attention check or failed to complete the attention check. These participants were excluded, leaving 407 participants for analyses. This sample comprised 217 American participants ($M = 44.17$ years; 54.4% men) and 190 Korean participants ($M = 42.65$ years; 48.9% men).

Experimental stimuli. For the Korean sample, all stimuli and materials were translated and back-translated via a professional translation company. All participants read the following text:

Pictured below is the President of South Korea, Park Geun-hye. This image was taken following the SEWOL ferry disaster which killed 304 people, most of whom were young schoolchildren. The South Korean government has been accused of being partly responsible for the disaster because of its push for economic gain at the cost of public safety. Corruption, weak regulation, and lax safety standards have all been said to have contributed to the disaster. The government has also received intense criticism for their slow and ineffective rescue efforts following the disaster.

The remaining text differed depending on condition. In the public apology conditions, participants received the following text (information in brackets was provided in the embodied remorse version of the condition only):

President Geun-hye [tearfully] apologized on behalf of the country in a public press conference, expressing regret and sorrow for the gov-

ernment's mistakes. The image below [of Geun-hye in tears] was taken from the press conference.

In the private apology conditions, participants received the following:

President Geun-hye [tearfully] apologized on behalf of her government in a private meeting with the families of the victims, expressing regret and sorrow for the government's mistakes. The image below [of Geun-hye in tears] was taken by one of the family members at the private meeting.

The subsequent image all featured the same, closely cropped photograph of President Geun-hye's face. Depending on condition, the image was digitally altered to either portray President Geun-hye as in tears or not in tears. Furthermore, in the public conditions a microphone was digitally inserted in front of President Geun-hye's face, whereas in the private condition no microphone was visible.

Dependent measures. Measures of *response satisfaction* ($\alpha = .85$), *ulterior motives* ($\alpha = .84$), *positive appraisal* ($\alpha = .89$), *perceived remorse* ($\alpha = .92$), *likelihood of reoffending* ($r = .48$), and *forgiveness* ($\alpha = .87$) were the same as those used in Study 1. The measure of *emotionality*—that again was treated as a check on the embodiment manipulation—included a third item, *tearful* ($\alpha = .86$). In this case, the measures of *emotionality* and *positive appraisal* were asked with reference to “the South Korean President.” Measures of *perceived remorse*, *likelihood of reoffending*, and *forgiveness* were framed with respect to “the South Korean Government.” Correlations among the measures are summarized in Table 1.

Results

We analyzed the data using a series of 2 (nationality: American or Korean participants) \times 2 (embodiment: tears or no tears) \times 2 (audience: private or public) between-groups ANOVAs. Effects of embodiment are summarized in Table 3. Note that some two-way interactions emerged between nationality and audience: because these interactions are peripheral to the central research question (around embodiment) they are reported in the Open Science Framework (https://osf.io/524qe/?view_only=d709389f45a344faa6560fca689b0a55).

Consistent with the manipulation, participants in the embodiment condition perceived the President to be more emotional ($M = 4.70$) than in the no-embodiment condition ($M = 3.55$), $F(1, 395) = 60.86$, $p < .001$, $\eta_p^2 = .13$. However, this main effect was qualified by an interaction with nationality, $F(1, 395) = 50.27$, $p < .001$, $\eta_p^2 = .11$. Analysis of simple main effects revealed that the manipulation of embodiment only impacted perceptions of emotionality among the American sample, $F(1, 395) = 120.04$, $p < .001$, $\eta_p^2 = .23$, but not the South Korean sample, $F(1, 395) = 0.24$, $p = .63$, $\eta_p^2 < .001$. As shall be discussed in more depth later, the apparent failure of the South Korean sample to internalize the manipulation seems more likely to be a reflection of skepticism than it does of inattention.

Overall, the embodiment manipulation had a significant main effect on response satisfaction, $F(1, 397) = 5.70$, $p = .017$, $\eta_p^2 = .01$, positive appraisals of the President, $F(1, 394) = 12.20$, $p = .001$, $\eta_p^2 = .03$, perceptions of remorse, $F(1, 399) = 7.06$, $p = .008$, $\eta_p^2 = .02$, and likelihood of reoffending, $F(1, 398) = 4.98$, $p =$

Table 3
Means (and SDs) Across Levels of Embodiment, Separately for American and South Korean Participants: Study 2

Measure	American		South Korean	
	Embodiment	No embodiment	Embodiment	No embodiment
Emotionality	5.47 _b (1.34)	3.47 _a (1.50)	3.73 _a (1.29)	3.63 _a (1.25)
Positive appraisals	4.65 _c (1.24)	3.95 _b (1.00)	3.58 _a (1.40)	3.43 _a (1.16)
Response satisfaction	4.17 _c (1.40)	3.76 _b (1.36)	3.32 _a (1.66)	3.02 _a (1.54)
Perceived remorse	4.51 _c (1.35)	3.83 _b (1.21)	3.36 _a (1.50)	3.33 _a (1.37)
Ulterior motives	4.97 _{ab} (1.25)	5.29 _b (1.26)	4.62 _a (1.59)	4.71 _a (1.31)
Likelihood of reoffending	3.67 _a (1.50)	4.03 _a (1.19)	4.74 _b (1.73)	5.03 _b (1.42)
Forgiveness	4.06 _b (1.22)	3.95 _b (1.15)	3.08 _a (1.50)	2.91 _a (1.34)

Note. SDs are in parentheses. Means that do not share a subscript are significantly different according to Duncan's post hoc test ($p < .05$).

.026, $\eta_p^2 = .01$. This reflected the fact that the participants in the embodiment condition were more satisfied with the response ($M = 3.79$), had more positive appraisals of the President ($M = 4.17$), perceived more remorse ($M = 4.00$), and saw less of a likelihood of reoffending ($M = 4.15$) than participants in the no-embodiment condition ($M_s = 3.40, 3.70, 3.58$, and 4.51 , respectively). However, no significant effects of embodiment emerged on ulterior motives, $F(1, 394) = 2.26, p = .13, \eta^2 = .01$, or forgiveness, $F(1, 399) = 0.99, p = .32, \eta_p^2 < .01$.

Moderating effect of audience. Overall, audience had no significant main effects on responses (all $p_s > .067$). Neither did the embodiment manipulation interact with the audience manipulation on any measure, all $p_s > .36$, and there were no three-way interactions, all $p_s > .079$.

Moderating effect of nationality. Robust main effects of nationality emerged across all dependent measures, reflecting the fact that the American sample was more benign in their reception of the apology than was the South Korean sample (all $p_s < .001$, all $\eta_p^2 > .03$). Of more relevance to the current research question, however, was whether the embodiment manipulation had different effects on the American compared with the South Korean samples. Significant interactions between nationality and embodiment did indeed emerge on positive appraisals of the President, $F(1, 394) = 5.25, p = .022, \eta_p^2 = .01$, and perceived remorse, $F(1, 399) = 6.02, p = .034, \eta_p^2 = .01$. Analysis of simple main effects revealed that, in each case, the manipulation of embodiment had no effect on the South Korean sample, both $p_s > .41$, both $\eta_p^2 < .001$. The American sample, in contrast, were more positive in the embodiment than in the no-embodiment condition: they had more positive appraisals of the President, $F(1, 394) = 18.06, p < .001, \eta_p^2 = .04$, and perceived more remorse, $F(1, 399) = 13.99, p = .001, \eta_p^2 = .03$.

Tests of mechanism. As in Study 1, participants perceived lower ulterior motives in the embodiment than the no-embodiment condition. Although this effect was not significant, the trend in the means rules out a "crocodile tears" mechanism, and so this was not pursued further. Instead, we ran a series of analyses to test the possible mediating role of perceived remorse. However, because the effects of embodiment on perceived remorse emerged only within the American sample, we restricted our mediational analyses to this sample.

As in Study 1, embodiment had an indirect effect via remorse on positive appraisals, $b = .40, SE = .10, 95\% CI [.209, .618]$,

response satisfaction, $b = .47, SE = .13, 95\% CI [.225, .745]$, forgiveness, $b = .42, SE = .11, 95\% CI [.214, .645]$, and likelihood of reoffending, $b = -.47, SE = .12, 95\% CI [-.729, -.240]$.

Discussion

Replicating the results observed in Study 1, the collective apology was more effective when the apologizer embodied remorse (through tears) than when they did not. Participants were more positive in their appraisals of the apologizer and more satisfied with her response when she cried. Tears also had a reliable effect on the extent to which participants thought the perpetrators had learned their lessons and would work to make sure the transgression would not happen again.

As in Study 1, there was again no sign that participants were suspicious of the genuineness of the tears or what they represent: perceptions of ulterior motives were statistically equivalent regardless of whether or not tears were present. Furthermore, all effects emerged equally strongly regardless of whether the apology occurred during a private meeting with victims' families or through a public press conference (where one might imagine there would be extra vigilance around the possibility of the manipulative or inauthentic use of tears). Most tellingly, participants perceived the apologizer to be more genuinely sorry when she cried than when she did not, and it was through perceived remorse that the embodiment manipulation had its positive effects.

However, there were major boundary conditions to these effects. First, inspection of Table 3 reveals that all the effects were specific to the American sample. The Korean participants (i.e., members of the victimized group) seemed unmoved by the tears, and even unwilling to acknowledge them. One possible conclusion that can be drawn from this is that members of bystander groups (in this case Americans) may be more influenced by superficial expressions of remorse than are members of the victimized group (the Koreans). However, this same phenomenon did not emerge in Study 1, when Japanese and Americans were equally influenced by the embodied remorse shown by the TEPCO executives. To test the generalizability and robustness of this interaction effect, in Studies 4 and 5 we return to this question of whether members of the victimized group and members of a bystander group differ in their response to embodiments of remorse.

The second boundary condition was that the otherwise positive effect of embodied remorse did not significantly influence partic-

ipant reports of forgiveness. This null result, which was also found in Study 1, is intriguing given that the effects of embodiment on perceived remorse were so reliable, and given the high correlation between remorse and forgiveness. One possible explanation for this is that *collective* forgiveness (as opposed to forgiveness of an individual) is simply a construct that resists short-term manipulation. That is, people may find it difficult to grapple with the nuances of collective forgiveness in the brief duration of the research session. Indeed, collectives are complex and have multiple parts, each with potentially different agendas and different perspectives on the transgression. As such, participants might have found intergroup forgiveness to be abstract and logically challenging (see Chapman, 2007, for a similar argument). Given this, in Studies 3–5 we turned our attention away from the many-to-many apologies used in the first two studies, and toward one-to-many apologies: apologies delivered by an individual for his or her transgressions toward a collective.

Study 3

In Study 3, participants read one of four real-world transgression stories in which an individual apologized to the public. Because it is possible that people respond to emotional tears differently depending on whether the crier is a man or a woman, we used two male and two female transgressors. The decision to use four different transgressions was made for generalizability reasons; no a priori predictions were made with regard to the effect of the particular transgression story on positive responses from the public. As in Study 2, images of the apology were digitally altered to make the apologizer seem tearful or not tearful. Dependent measures were the same as those used in Study 3, with the exception that they were framed in terms of feelings and attitudes toward an individual rather than a group. We also included an extra dependent measure: empathy felt toward the apologizer. This variable was included because research in the interpersonal literature suggests that empathy is a powerful mechanism through which one-to-one apologies promote interpersonal forgiveness (e.g., McCullough, Worthington, & Rachal, 1997).

Method

Participants and design. Given that Studies 1 and 2 found a null effect of embodiment on the key outcome measure of forgiveness, in Study 3 we sampled with a view to providing optimal power. A power analysis determined that 768 participants would be required for a 95% chance of detecting a small-medium effect ($f = 0.15$). This again constituted our minimum targeted sample size. Using Amazon's Mechanical Turk, we recruited 814 American residents. Of the original sample, 21 failed a simple attention check or declined to answer the attention check. These participants were excluded, leaving 793 participants for analyses ($M = 37.17$ years; 50.3% women).

Participants were randomly assigned to receive one of four real-world transgression stories in which an individual apologized to the public for something she or he had done. Regardless of which transgression story participants read about, they were led to believe that the target's apology had either occurred in public or private. Participants were also led to believe that the apology was either tearful or there were no tears. This resulted in a 4 (perpetrator) \times 2 (audience) \times 2 (embodiment) between-groups design.

Experimental stimuli. Two of the transgression stories involved examples of public figures being caught out making prejudiced comments on social media. One was a professional swimmer called Stephanie Rice. Participants were told:

Pictured below is Stephanie Rice, a swimmer. She was caught in a public scandal after posting a controversial and offensive Twitter message in which she called a rival football team "faggots." After the team lost to her home team, she tweeted: "Suck on that faggots. Probs the best game I've ever seen!! Well done boys." She faced backlash from the public for her post shortly afterward, with critics labeling the message homophobic.

The second case was a senior public servant (John Huppenthal) who had made negative comments about Hispanic Americans:

Pictured below is John Huppenthal—an American politician who was the Arizona Superintendent of Public Instruction from 2011–2015. He was caught in a public scandal when it was discovered that he had posted anonymous comments on online blogs denigrating welfare recipients and making remarks that were perceived to be racist toward Hispanic Americans. In one blog post, he referred to poor people as "lazy pigs." In another, he wrote: "No Spanish radio stations, no Spanish billboards, no Spanish TV, no Spanish newspapers. This is America, speak English." He was found to have been anonymously posting such comments for years, under multiple pseudonyms.

A third case related to a stem-cell scientist who fabricated her data:

Pictured below is Haruko Obokata, a stem-cell researcher. She was caught in a public scandal when it emerged that she had falsified data on two papers claiming ordinary adult cells could be turned into stem cells by bathing them in acid. She became famous in the scientific community after the research was published in the journal *Nature*. Mistakes were discovered in some data published in two of her papers, photograph captions were found to be misleading, and the work could not be replicated by other scientists. She then failed to reproduce the conversion of adult cells to stem cells.

The fourth case related to a footballer who engaged in lewd and disgusting acts that were made public:

Pictured below is Mitchell Pearce, a footballer. He was caught in a public scandal when a video was released depicting him behaving in a distasteful manner. The video depicted him in a highly intoxicated state in which he drunkenly staggered around a living room, forced an unwanted kiss on a female who rejected his advances, and simulated sex with her dog. His behavior drew widespread criticism and he faced disciplinary action.

After reading each transgression, participants were led to believe that the transgressor had made either a tearful or a nontearful apology, and that the apology was either public or private. For example, in the public condition of the Mitchell Pearce case, participants read the following (text in parentheses only appeared in the embodied remorse conditions): *In a public press conference he [tearfully] apologized, expressing regret for his actions and the damage they had done. The image below is a still from that press conference [showing Pearce in tears].* In the private condition, participants read the following: *In a private meeting with members of the football club, he [tearfully] apologized. Attendees at the closed-door meeting said that he expressed regret for his actions and the damage they had done. The image below was taken from*

one of the club members at the private meeting [showing Pearce in tears].

In all conditions the subsequent image showed a single, head-and-shoulder photograph of the perpetrator. Depending on condition, the image was digitally altered to either portray the target as in tears or not in tears, and to make a microphone visible or not.

Dependent measures. Measures of *emotionality* ($\alpha = .85$), *response satisfaction* ($\alpha = .91$), *positive appraisal* ($\alpha = .91$), *perceived remorse* ($\alpha = .95$), *ulterior motives* ($\alpha = .82$), *likelihood of reoffending* ($r = .72$), and *forgiveness* ($\alpha = .92$) were the same as those used in Study 1, adjusted only to refer to the specific target participants had read about. *Empathy* (toward the apolo-gizer) was measured by asking participants to rate the extent to which they agreed with the following statements: “I feel compassion for this person,” and “I am moved by the thought of what this person is going through” (1 = *strongly disagree*, 7 = *strongly agree*; $r = .76$). All scales were presented in a randomized order.³

Results

Results were analyzed using a series of 4 (perpetrator) \times 2 (embodiment) \times 2 (audience) between-groups ANOVAs. The critical comparison is between the embodiment and the no-embodiment conditions, and these means and SDs are summarized in Table 4. Note that some two-way interactions emerged between perpetrator and audience: because these interactions are peripheral to the central research question they are reported in the Open Science Framework (https://osf.io/524qe/?view_only=d709389f45a344faa6560fca689b0a55).

Effects of embodiment. Confirming the effectiveness of the manipulation, participants in the embodied condition perceived more emotionality than did participants in the no-embodied condition, $F(1, 776) = 189.92, p < .001, \eta_p^2 = .20$. As can be seen in Table 4, main effects of embodiment also emerged on perceived remorse, $F(1, 777) = 12.40, p < .001, \eta_p^2 = .02$, response satisfaction, $F(1, 776) = 4.02, p = .045, \eta_p^2 = .01$, empathy, $F(1, 777) = 4.60, p = .032, \eta_p^2 = .01$, and likelihood of reoffending, $F(1, 776) = 5.09, p = .024, \eta_p^2 = .01$. In every case, impressions of the targets and their apologies were more favorable when the apology was accompanied by tears than when it was not. However, no significant effects emerged on positive appraisals, $F(1, 776) = 0.36, p = .549, \eta_p^2 < .01$, ulterior motives, $F(1, 777) = 2.61, p = .107, \eta_p^2 < .01$, or forgiveness, $F(1, 777) = 0.95, p = .331, \eta_p^2 < .01$.

Table 4
Means (and SDs) Across Levels of Embodiment: Study 3

Measure	Embodiment	No embodiment
Emotionality	5.21 _b (1.37)	3.89 _a (1.42)
Positive appraisals	3.56 _a (1.28)	3.51 _a (1.20)
Response satisfaction	4.22 _b (1.59)	4.01 _a (1.58)
Perceived remorse	4.01 _b (1.60)	3.64 _a (1.53)
Ulterior motives	5.70 _a (1.20)	5.83 _a (1.08)
Likelihood of reoffending	3.41 _a (1.64)	3.66 _b (1.59)
Empathy	3.47 _b (1.66)	3.24 _a (1.50)
Forgiveness	4.48 _a (1.57)	4.38 _a (1.51)

Note. SDs are in parentheses. Means with different subscripts are significantly different from each other.

Moderation by perpetrator. Main effects of perpetrator emerged on every dependent measure (all $ps < .001$, all $\eta_p^2 > .03$). This simply reflected the fact that participants were generally least positive in their impressions of John Huppenthal and his apology, and generally most positive in their impressions of Stephanie Rice and her apology. Given that gender of the speaker was not a focus of the research, it was not manipulated systematically; thus, it is unclear if this pattern is because of more positive reactions to the female target or other features of the specific cases (e.g., race, occupation, offense type, etc.). Of more relevance to the current research question, however, the effect of embodiment was not moderated by perpetrator on any of the measures (all $ps > .19$).

Moderation by audience. No main effects of audience emerged (all $ps > .11$); neither were there any two-way interactions between audience and embodiment (all $ps > .34$). Three-way interactions emerged on positive appraisals of the target, $F(3, 776) = 3.89, p = .009, \eta_p^2 = .02$, and empathy, $F(3, 777) = 3.83, p = .010, \eta_p^2 = .02$. This was driven entirely by significant Audience \times Embodiment interactions that emerged for Haruko Obokata (positive appraisals of the target, $F(1, 776) = 8.75, p = .003$; empathy, $F(1, 777) = 10.62, p = .001$) and nobody else (all other $ps > .21$). When Obokata was apologizing in public, tears led to more positive appraisals, $F(1, 776) = 4.39, p = .036$, and more empathy, $F(1, 777) = 11.93, p = .001$, than no tears. When Obokata was apologizing in private, tears led to less positive appraisals than no tears, $F(1, 776) = 4.37, p = .037$, and there was no effect of embodiment on empathy, $F(1, 777) = 1.31, p = .25$.

Tests of mechanism. As in Studies 1 and 2, the pattern of means on ulterior motives obviated the need to test for a crocodile tears mechanism, and so we only examined the possible mediating role of perceived remorse. As in previous studies, remorse proved to be a reliable mediator: embodiment had significant indirect effects via remorse on positive appraisals, $b = .22, SE = .06, 95\% CI [.098, .344]$, response satisfaction, $b = .26, SE = .08, 95\% CI [.114, .409]$, and empathy, $b = .27, SE = .08, 95\% CI [.115, .424]$. Embodiment also had significant indirect effects via remorse on forgiveness, $b = .25, SE = .07, 95\% CI [.111, .387]$, and likelihood of reoffending, $b = -.26, SE = .08, 95\% CI [-.418, -.114]$.

Discussion

As in Studies 1 and 2, participants believed the perpetrator was more sorry about their transgression when they embodied their remorse, and this tendency flowed through to higher ratings of satisfaction with the perpetrator’s response, and more of a willingness to believe that the transgressor was unlikely to reoffend. On forgiveness, however, there was no effect of embodiment;

³ Also included was a scale that was designed to measure self-pity. The question started: “As described earlier, the person in the photo was caught in a public scandal, during which time they received intense criticism for their actions. In your opinion, does this person believe that the criticism they’ve received is:” Response options were “Not at all fair: definitely fair” (reversed); “Not at all appropriate: Definitely appropriate” (reversed); “Not at all extreme: Definitely extreme”; and “Not at all understandable: Definitely understandable (reversed)” ($\alpha = .87$). A main effect of tears emerged on this scale, such that self-pity was lower in the tears ($M = 3.36$) than the no-tears condition ($M = 3.58$), $p = .047$. Combined with the trends in the means on ulterior motives, this reinforces the overall picture that participants were not feeling skeptical about whether the tears reflected genuine remorse.

neither was there an effect of embodiment on positive appraisals of the perpetrator or ulterior motives.

One consistent pattern across Studies 1–3 is that there were no consistent interactions between embodiment and audience: people's perceptions of kneeling and crying did not differ depending on whether it was a private display of remorse (the private condition) or one that was intended for the media (public condition). Given this, we decided not to manipulate the public versus private nature of the response in subsequent studies, focusing instead just on public responses. Rather, we manipulated a different contextual factor: the presence or absence of an apology.

Specifically, what remains to be seen is how public displays of tears are interpreted in the *absence* of an apology. Perhaps, if the transgressor cries but does not apologize, tears may be perceived as more strategic and less authentic. Studies 4 and 5 examined this question by manipulating both the presence of tears and the presence of an apology. In Study 4, we did so using the Stephanie Rice case (the most positively appraised target in Study 3). In Study 5 we did so using the John Huppenthal case (the least positively appraised target in Study 3).

Another goal of Studies 4 and 5 was to examine the possible role of dispositional trust in moderating the effects of both tears and apologies. Dispositional trust has not been tested before as a moderator of responses to public expression of remorse, but the theoretical case is strong. Several scholars (e.g., Hornsey & Wohl, 2013; Nadler & Liviatan, 2006) have argued that resistance to public apologies flows from a default mistrust of the sincerity of the emotions that are performed in public. Therefore, it stands to reason that people who are predisposed toward trusting others will be more swayed by expressions of remorse than will those who are predisposed toward *mistrusting* others.

Perhaps of more relevance to the current article is the question of whether dispositional trust will moderate the effects of embodiment. There are two possible perspectives on this. If people are prone to seeing embodied displays of remorse through a lens of skepticism (e.g., if people habitually scan tears for signs of inauthenticity and manipulation), then one would expect that the positive effects of embodied remorse would be more pronounced among those high in dispositional trust than among those low in dispositional trust. However, one consistent pattern across Studies 1–3 is that there is little evidence that people perceive the embodiment of remorse as a strategic move or as an inauthentic attempt to manipulate the public. Across all studies, embodiment had a small-to-medium positive effect on perceived remorse. Furthermore, perceptions of ulterior motives trended lower in the embodiment condition; significantly so in Study 1. On the basis of these findings, we tentatively predict that people's responses to embodied remorse may be somewhat trust *insensitive*; that respondents who are less dispositionally trusting will be just as moved by embodied remorse as those respondents who are more dispositionally trusting.

Study 4

In Study 4, we tested our predictions using the Stephanie Rice case described in Study 3. Participants were told that Stephanie Rice had appeared at a press conference in the aftermath of the scandal surrounding her use of homophobic slurs in her tweets about a football game. Participants were led to believe that she did

or did not apologize during the press conference; and that she cried or did not cry during the press conference.

Method

Participants and design. In Study 4, we again increased the sample to minimize the likelihood that any null results could be attributed to low power. A power analysis determined that 819 participants would be required for a 99% chance of detecting a small-medium effect ($f = 0.15$), constituting our minimum sample size target. We sampled American residents (18 years or older) using SSI. Participants were screened based on their demographic characteristics to ensure we had a sufficient number of both heterosexual and nonheterosexual people (i.e., people who self-identified as gay, bisexual or other). Participants were screened out if they reported identifying with a sexual orientation other than what was advertised in the study recruitment ($n = 358$), or if they did not live in the United States ($n = 8$). After screening, 1,233 participants went on to complete the survey.

Of this original sample, 128 failed a check on the manipulation of whether or not an apology was present ("Did Stephanie Rice apologize for her Twitter message?" Yes/No). A further 19 participants dropped out before the manipulation, and 54 failed a simple attention check or declined to answer the attention check. These participants were excluded, leaving 1,032 participants for analyses ($M = 36.7$ years; 56.8% men). Of these, 386 self-identified as heterosexual, and 646 self-identified as gay, lesbian, bisexual, or other (the latter group was oversampled by SSI to ensure adequate numbers).

Participants were led to believe that the public response from the target had either included an apology or had not included an apology, and that their public response was either tearful or there were no tears. This resulted in a 2 (lesbian, gay, bisexual, transgender [LGBT] or straight) \times 2 (apology or no apology) \times 2 (embodiment or no embodiment) between-groups design.

Experimental stimuli. After recording their sexuality, age, and sex, participants read about the incident regarding Stephanie Rice using homophobic slurs in a tweet (the incident was described using the same text reported in Study 3; see also the Open Science Framework: https://osf.io/524qe/?view_only=d709389f45a344faa6560fca689b0a55). The subsequent text was altered depending on condition. In the no-apology condition, participants read the following (text in parentheses only appeared in the embodied remorse version of this condition):

Rice appeared [in tears] in a public press conference. Although she did respond to questions about the incident, she did not offer an apology.

In the apology condition, participants read the following:

Rice apologized [in tears] in a public press conference, expressing regret for her actions and the damage they had done.

The subsequent image all featured the same, closely cropped photograph of Stephanie Rice's face as used in the public conditions of Study 3 (i.e., either in tears or not in tears, and with a microphone in the foreground).

Measures of *emotionality* ($\alpha = .84$), *positive appraisal* ($\alpha = .88$), *ulterior motives* ($\alpha = .85$), *response satisfaction* ($\alpha = .87$), *empathy* ($r = .76$), and *forgiveness* ($\alpha = .89$) were the same as those used in Study 3. The measure of *perceived remorse* was the

same as that used in Study 1, except the last three items were not included, leaving a four-item scale ($\alpha = .92$). All of these scales were presented in a randomized order. The likelihood of reoffending measure used in Studies 1–3 was not included in Studies 4 or 5.

Dispositional trust was measured by asking participants to rate their agreement with five statements taken from Schuessler's (1982) five-item scale of dispositional propensity to trust. Example items include "Most people can be trusted" and "I find it hard to figure out who you can really trust these days" (1 = *strongly disagree*, 7 = *strongly agree*; $\alpha = .76$ after reversing three negatively worded items).

Results

We analyzed the data using a series of 2 (LGBT or straight) \times 2 (apology or no apology) \times 2 (embodiment or no embodiment) between-groups ANOVAs. Means and SDs across levels of embodiment and apology are summarized in Table 5.

Main effects of embodiment. Confirming the success of the manipulation, participants in the embodiment condition perceived the target to be more emotional ($M = 4.59$) than did participants in the no-embodiment condition ($M = 2.91$), $F(1, 1021) = 303.27$, $p < .001$, $\eta_p^2 = .23$. This large main effect was qualified by a much weaker interaction between embodiment and the apology manipulation, $F(1, 1021) = 7.12$, $p = .008$, $\eta_p^2 = .01$. Tests of simple main effects showed that the embodiment manipulation was successful in both the apology, $F(1, 1021) = 102.30$, $p < .001$, $\eta_p^2 = .09$, and the no-apology conditions, $F(1, 1021) = 215.18$, $p < .001$, $\eta_p^2 = .17$, but that it was stronger in the latter.

The embodiment manipulation also had significant main effects on positive appraisals of the target, $F(1, 1020) = 33.60$, $p < .001$, $\eta_p^2 = .03$, response satisfaction, $F(1, 1017) = 10.86$, $p < .001$, $\eta_p^2 = .01$, perceptions of remorse, $F(1, 1018) = 34.68$, $p < .001$, $\eta_p^2 = .03$, and empathy, $F(1, 1024) = 12.54$, $p < .001$, $\eta_p^2 = .01$. This reflected the fact that participants in the embodiment condition (vs. participants in the no-embodiment condition) had more positive appraisals of the target ($M = 3.31$ vs. 2.89), were more satisfied with the response ($M = 3.42$ vs. 3.10), perceived more remorse ($M = 3.05$ vs. 2.49), and were more empathic ($M = 3.03$ vs. 2.69). Consistent with Studies 1–3, no significant main effect of embodiment emerged on forgiveness, $F(1, 1020) = 2.65$, $p = .104$, $\eta_p^2 < .005$.

Main effects of apology. Main effects of apology emerged on emotionality, $F(1, 1021) = 16.61$, $p < .001$, $\eta_p^2 = .02$, positive

appraisals of the target, $F(1, 1020) = 54.28$, $p < .001$, $\eta_p^2 = .05$, response satisfaction, $F(1, 1017) = 208.46$, $p < .001$, $\eta_p^2 = .17$, perceptions of remorse, $F(1, 1018) = 240.65$, $p < .001$, $\eta_p^2 = .19$, empathy, $F(1, 1024) = 45.87$, $p < .001$, $\eta_p^2 = .04$, and forgiveness, $F(1, 1020) = 36.14$, $p < .001$, $\eta_p^2 = .03$. Participants in the apology condition (vs. participants in the no-apology condition) perceived more emotionality ($M = 3.98$ vs. 3.57), had more positive appraisals of the target ($M = 3.40$ vs. 2.85), were more satisfied with the response ($M = 3.99$ vs. 3.62), perceived more remorse ($M = 3.55$ vs. 2.09), were more empathic ($M = 3.21$ vs. 2.55), and were more forgiving ($M = 4.46$ vs. 3.85) than participants in the no-apology condition.

Apology \times Embodiment interactions. On ulterior motives, main effects of apology, $F(1, 1018) = 103.70$, $p < .001$, $\eta_p^2 = .09$, and embodiment, $F(1, 1018) = 17.61$, $p < .001$, $\eta_p^2 = .02$, were qualified by a significant embodiment by apology interaction, $F(1, 1018) = 35.09$, $p < .001$, $\eta_p^2 = .03$. In the absence of an apology, participants reported relatively high conviction that the transgressor was motivated by ulterior motives when she cried than when she did not, $F(1, 1018) = 54.54$, $p < .001$, $\eta_p^2 = .05$. However, when an apology was present (as it was in Studies 1–3) the means trended in the opposite direction, $F(1, 1018) = 1.41$, $p = .236$, $\eta_p^2 < .005$. No other Apology \times Embodiment interactions were significant.

Effects of sexuality. LGBT participants credited the target with less emotionality, appraised the target less positively, were less satisfied with the apology, perceived less genuine remorse, and were less forgiving than the straight sample (all $ps < .001$, all $\eta_p^2 > .01$). Of more relevance to the current research question, however, none of the sexuality by embodiment interactions approached significance (all $ps > .32$). Neither was there any significant sexuality by apology interactions or three-way interactions (all $ps > .11$).

Moderation by dispositional trust. When deciding where to position our potential moderator in the questionnaire, we were faced with two options. One option would be to place the measure of dispositional trust before the manipulation, but with the risk that by priming trust we would contaminate people's responses to the apology and embodiment manipulations. The second option—that is the one we chose—was to place the moderator after the dependent measures, but with the risk that the manipulations would influence dispositional trust. The results of 2 (heterosexual or LGBT) \times 2 (apology or no apology) \times 2 (embodiment or no

Table 5
Means (and SDs) Across Conditions: Study 4

Measure	Apology		No apology	
	Embodiment	No embodiment	Embodiment	No embodiment
Emotionality	4.66 _c (1.56)	3.25 _b (1.50)	4.52 _c (1.50)	2.62 _a (1.38)
Positive appraisals	3.63 _c (1.26)	3.15 _b (1.23)	3.02 _b (1.10)	2.67 _a (1.08)
Response satisfaction	4.18 _c (1.44)	3.79 _b (1.56)	2.73 _a (1.51)	2.51 _a (1.36)
Ulterior motives	5.57 _c (1.28)	5.74 _c (1.30)	5.19 _b (1.52)	4.15 _a (1.89)
Perceived remorse	3.85 _a (1.69)	3.23 _c (1.70)	2.32 _b (1.28)	1.86 _a (1.10)
Empathy	3.37 _d (1.65)	3.04 _c (1.62)	2.72 _b (1.46)	2.39 _a (1.34)
Forgiveness	4.54 _b (1.49)	4.37 _b (1.55)	3.92 _a (1.47)	3.79 _a (1.63)

Note. SDs are in parentheses. Means that do not share a subscript are significantly different according to Duncan's post hoc test ($p < .05$).

embodiment) between-groups ANOVAs suggested that we made the right choice: there were no significant main effects or interactions on dispositional trust (all $ps > .14$). This meant that it was plausible to run regressions in which trust was included as a predictor. As such, regressions were conducted in which dispositional trust (centered around the mean) was entered at Step 1, along with the manipulation of embodiment (0 = *no embodiment*, 1 = *embodiment*) and the manipulation of apology (0 = *no apology*, 1 = *apology*). Participant sexuality (0 = *heterosexual*, 1 = *LGBT*) was included as a covariate. Step 2 included predictors representing the interaction between embodiment and trust; apology and trust; and embodiment and apology. The 3-way interaction term was entered at Step 3. Main effects of apology, embodiment, and sexuality simply reflected what has been reported in the ANOVAs and so will not be discussed further.

Main effects of dispositional trust emerged on all six dependent measures: the higher participants were in dispositional trust, the less they perceived ulterior motives ($\beta = -.08, p = .007$) and the higher they scored on measures of positive appraisals of the target, perceived remorse, satisfaction with the response, empathy, and forgiveness (all $\beta s > .06$, all $ps < .04$). However, Step 2 revealed that these main effects were qualified by significant trust by apology interactions on positive appraisals of the target ($\beta = .09, p = .027$), perceived remorse ($\beta = .10, p = .007$), satisfaction with the response ($\beta = .09, p = .023$), and empathy ($\beta = .09, p = .032$). In each case, analysis of simple slopes (calculated through Model 1 of PROCESS) revealed the same pattern. Among those high in dispositional trust, there were significant effects of the apology on positive appraisals of the target, $b = .70, SE = .10, p < .001, 95\% CI [.500, .893]$, perceived remorse, $b = 1.72, SE = .13, p < .001, 95\% CI [1.464, 1.979]$, satisfaction with the response, $b = 1.56, SE = .13, p < .001, 95\% CI [1.316, 1.812]$, and empathy, $b = .84, SE = .13, p < .001, 95\% CI [.587, 1.097]$. Among those low in dispositional trust, there were still significant (but weaker) effects of the apology on positive appraisals of the target, $b = .38, SE = .10, p < .001, 95\% CI [.183, .577]$, perceived remorse, $b = 1.27, SE = .13, p < .001, 95\% CI [1.008, 1.524]$, satisfaction with the response, $b = 1.16, SE = .13, p < .001, 95\% CI [.910, 1.408]$, and empathy, $b = .44, SE = .13, p < .001, 95\% CI [.188, .700]$.

There was only one two-way interaction between dispositional trust and embodiment, which emerged on ulterior motives ($\beta = -.08, p = .044$). Analysis of simple effects revealed that participants generally perceived more ulterior motives in the tears versus the no-tears condition, but that this tendency was stronger among those low in dispositional trust, $b = 1.25, SE = .16, p < .001, 95\% CI [.936, 1.569]$, than among those high in dispositional trust, $b = .87, SE = .16, p < .001, 95\% CI [.548, 1.186]$. On perceived remorse, there was also a significant three-way interaction ($\beta = -.11, p = .037$). Analysis of simple effects (conducted using Model 3 of PROCESS), revealed a significant Trust \times Embodiment interaction when there was an apology, $b = -.26, SE = .12, p = .031, 95\% CI [-.502, -.024]$, but not when there was no apology, $b = .08, SE = .11, p = .471, 95\% CI [-.137, .296]$. Tests of simple effects within the apology condition revealed that embodiment had a positive effect on perceived remorse among those low in dispositional trust, $b = .91, SE = .19, p < .001, 95\% CI [.539, 1.272]$, but not among those high in dispositional trust, $b = .33, SE = .18, p = .069, 95\% CI [-.027, .694]$.

Discussion

When the transgressor cried, she was appraised more positively, won over more empathy, and was seen to be more genuinely remorseful than when she did not cry. The transgressor's response was also seen to be more satisfying when she cried than when she did not. As in Studies 1–3, however, the embodiment of remorse had no reliable effect on forgiveness.

As anticipated from previous research (Philpot & Hornsey, 2008, Study 4), the one-to-many apology increased not just how much participants liked and empathized with the transgressor, but also the extent to which they were prepared to forgive her. Furthermore, as expected on the basis of previous research and theory, the positive effects of apologies were particularly pronounced among those high in dispositional trust.

Of interest, though, the effects of embodiment were mostly unmoderated by whether it was accompanied by an apology. On positive appraisals of the target, response satisfaction, perceptions of remorse, and empathy, the beneficial effects of tears emerged equally strongly in the no-apology condition as the apology condition. Furthermore, there was very little evidence that people responded to tears differently depending on whether they were high or low in dispositional trust. On measures relating to the transgressor—the extent to which she aroused feelings of empathy, won over positive regard, and convinced participants of the genuineness of her remorse—tears had an equally strong effect among the chronically untrusting as it did among the chronically trusting. Those low in dispositional trust were more likely than those high in trust to see tears as reflecting ulterior motives, but this suspicion did not flow through into other outcome measures. Indeed, when accompanied by an apology, tears increased perceptions of remorse among participants *low* (but not high) in dispositional trust.

Study 5

Study 5 had the same aims and the same design as Study 4, with the exception that it was applied to the John Huppenthal case rather than the Stephanie Rice case. John Huppenthal received the most negative evaluations of the four targets used in Study 3, which is perhaps unsurprising: his disparagement of Hispanics was both secretive (in the sense that he used aliases to disguise his identity) and protracted (it occurred over many years). Unlike Stephanie Rice, his transgression cannot be excused as a crass mistake, as a misunderstanding, or as a one-off error of judgment. As such, this provides a particularly strict test of the power of embodied remorse in winning over public sentiment.

A secondary aim of Study 5 is to diversify the measurement of forgiveness used in this program of research. The measure we used in Studies 1–4 is well-established and proved sensitive to the apology manipulation, so we do not believe that the null results of embodiment on forgiveness can be explained away as a problem with invariance in the measure. However, to minimize the possibility that the null result on forgiveness is a Type II error, we used two forgiveness measures in the current research: the measure used in Studies 1–4, as well as a measure adapted from an established forgiveness scale that has a focus on revenge intentions.

Method

Participants and design. Originally, 1,053 participants responded to the survey. However, despite being invited to respond on the basis of their national and ethnic characteristics, 9 participants indicated that they were not Americans, 83 participants who were included in the Hispanic sample did not include being Hispanic in their self-reports of ethnicity, and 2 participants who were included in the White sample did not include being White in their self-reports of ethnicity. These participants were routed out of the survey at the beginning. Of the participants that remained, five admitted to not reading the questions properly in an open-response feedback question before debriefing. A further eight dropped out before the manipulation, and 132 failed a manipulation check asking about the presence or absence of an apology (“Did John Huppenthal apologize for his blog posts?”; Yes/No). After deleting these cases, the sample comprised 409 White participants ($M = 37.9$ years; 50.1% men) and 405 Hispanic participants ($M = 33.2$ years; 49.9% men).⁴

Participants were randomly assigned to receive an apology or not, which either featured tears or did not. This resulted in a 2 (White or Hispanic participants) \times 2 (apology or no apology) \times 2 (tears or no tears) between-groups design.

Experimental stimuli. After recording their ethnicity, age, and sex, participants read about the incident regarding John Huppenthal making anti-Hispanic statements on social media (the incident was described using the same text reported in Study 3). The subsequent text was altered depending on condition. In the no-apology condition, participants read the following (text in parentheses only appeared in the embodied remorse version of this condition):

Huppenthal appeared [in tears] in a public press conference. Although he did respond to questions about the incident, he did not offer an apology.

In the apology condition, participants read the following:

Huppenthal [tearfully] apologized at a public press conference, stating: “I’m here today to apologize for my blog comments. I realized fervently and more powerfully than ever that all of my actions both in my official role and privately need to bring honor. . . . Obviously my hurtful blog comments didn’t do that.”

At the end of the passage, all participants read “*The photograph below was taken at this press conference*” and participants in the no-embodiment conditions saw a head-and-shoulders photograph of Huppenthal at the press conference. Participants in the embodiment conditions saw a head-and-shoulders photograph of Huppenthal at the press conference apparently wiping away tears.

Measures of *emotionality* ($\alpha = .80$), *positive appraisal* ($\alpha = .87$), *perceived remorse* ($\alpha = .93$), *ulterior motives* ($\alpha = .84$), *response satisfaction* ($\alpha = .79$), *empathy* ($r = .73$), and *forgiveness* ($\alpha = .84$) were the same as those used in Study 3, except rephrased to refer specifically to Huppenthal. Finally, *revenge* was measured by adapting the revenge-related items from the Transgression-Related Interpersonal Motivations Scale-12 (TRIM-12; McCullough et al., 1998). Participants rated the extent to which they agreed with the following statements: “Someone should make Huppenthal pay,” “I wish that something bad would happen to Huppenthal,” “I want Huppenthal to get what he deserves,” “I

want Huppenthal to be hurt and miserable,” and “I wish I could get even with Huppenthal” (1 = *strongly disagree*, 5 = *strongly agree*; $\alpha = .88$). The dependent measures were presented in a randomized order. As in Study 4, a measure of *dispositional trust* ($r = .71$) was included after the dependent measures.⁵

Results

We analyzed the data using a series of 2 (White or Hispanic) \times 2 (apology or no apology) \times 2 (embodiment or no embodiment) between-groups ANOVAs. Means and *SDs* across levels of embodiment and apology are summarized in Table 6.

Main effects of embodiment. Participants in the embodiment condition perceived the target to be more emotional ($M = 4.40$) than did participants in the no-embodiment condition ($M = 3.17$), $F(1, 633) = 101.63$, $p < .001$, $\eta_p^2 = .14$. The embodiment manipulation also had significant main effects on positive appraisals of the target, $F(1, 633) = 15.54$, $p < .001$, $\eta_p^2 = .02$, and perceptions of remorse, $F(1, 630) = 9.78$, $p = .002$, $\eta_p^2 = .02$. This reflected the fact that participants in the embodiment condition appraised the target more positively ($M = 3.24$) and perceived him to be more remorseful ($M = 3.04$) than did participants in the no-embodiment condition ($M = 2.86$ and 2.71 , respectively). However, no significant main effects of embodiment emerged on response satisfaction, $F(1, 628) = 2.32$, $p = .13$, $\eta_p^2 < .01$, empathy, $F(1, 634) = 2.18$, $p = .14$, $\eta_p^2 < .01$, forgiveness, $F(1, 634) = 0.04$, $p = .84$, $\eta_p^2 < .01$, or revenge, $F(1, 639) = 1.91$, $p = .17$, $\eta_p^2 < .01$. As in Study 4, there was a main effect of embodiment on ulterior motives, such that participants perceived more ulterior motives in the embodiment condition ($M = 5.55$) than in the no-embodiment condition ($M = 4.95$), $F(1, 630) = 32.29$, $p < .001$, $\eta_p^2 = .05$.

Main effects of apology. Main effects of the apology manipulation emerged on emotionality, $F(1, 633) = 16.72$, $p < .001$,

⁴ As can be seen in the Results, there is a gap between the overall N and the d/dfs used for the analyses. This is because of a programming error in the randomization function, such that participants were exposed to only four of five blocks of dependent measures. The “missing” block was randomized, meaning that the error did not introduce systematic skew into the data. However, it does mean that each measure (including the attention check) was only completed by 80% of the sample. Given this—and given the reduced power associated with the programming error—we included participants who failed the attention check in the analysis. It should be noted, though, that the results did not change regardless of whether the attention check fails were excluded or included in the analyses. Furthermore, a post hoc power analysis determined that the study still had at least a 96% chance of detecting a small-medium effect ($f = 0.15$) even with the reduced numbers in each analysis.

⁵ Also measured in Study 5 was a question about the extent to which Huppenthal felt “sorry for what he has done” and whether he felt “sorry for himself.” Scores on these items were positively correlated ($r = .18$, $p < .001$) and the same effects emerged on both: scores for both items were significantly higher in the apology condition, the embodiment condition, and among the White sample. Finally, after the measure of dispositional trust, we included a measure of entity beliefs as a potential moderator (i.e., whether participants felt as though people’s moral character can change). Unfortunately, there was a significant main effect of apology on entity beliefs such that participants in the apology condition reported higher entity beliefs than did those in the no-apology condition, $p = .006$. Given this, we felt as though entity beliefs could no longer be considered a truly independent variable, and so the moderation effects are not reported. In Study 6 we revisited this question, but this time using a premeasure of entity beliefs toward groups.

Table 6
Means (and SDs) Across Conditions: Study 5

Measure	Apology		No apology	
	Embodiment	No embodiment	Embodiment	No embodiment
Emotionality	4.58 _a (1.65)	3.78 _b (1.57)	4.19 _c (1.57)	2.85 _a (1.40)
Positive appraisals	3.62 _c (1.25)	3.12 _b (1.25)	2.81 _a (1.15)	2.57 _a (1.17)
Response satisfaction	3.85 _b (1.42)	3.73 _b (1.37)	2.80 _a (1.34)	2.62 _a (1.43)
Ultior motives	5.73 _c (1.15)	5.70 _c (1.13)	5.38 _b (1.38)	4.13 _a (1.94)
Perceived remorse	3.76 _c (1.78)	3.49 _c (1.84)	2.45 _b (1.29)	1.92 _a (1.30)
Empathy	3.53 _b (1.58)	3.25 _b (1.60)	2.77 _a (1.56)	2.67 _a (1.56)
Forgiveness	4.32 _b (1.48)	4.28 _b (1.33)	3.82 _a (1.47)	3.79 _a (1.54)
Revenge	2.45 _a (1.05)	2.57 _a (0.95)	2.46 _a (0.83)	2.55 _a (0.92)

Note. SDs are in parentheses. Means that do not share a subscript are significantly different according to Duncan's post hoc test ($p < .05$).

$\eta_p^2 = .03$, positive appraisals of the target, $F(1, 633) = 51.55, p < .001, \eta_p^2 = .08$, response satisfaction, $F(1, 628) = 97.78, p < .001, \eta_p^2 = .14$, perceptions of remorse, $F(1, 630) = 139.00, p < .001, \eta_p^2 = .18$, empathy, $F(1, 634) = 27.19, p < .001, \eta_p^2 = .04$, and forgiveness, $F(1, 634) = 18.54, p < .001, \eta_p^2 = .03$. Participants in the apology condition (vs. those in the no-apology condition) perceived more emotionality ($M = 4.06$ vs. 3.54), had more positive appraisals of the target ($M = 3.38$ vs. 2.69), were more satisfied with the response ($M = 3.79$ vs. 2.71), perceived more remorse ($M = 3.60$ vs. 2.12), were more empathic ($M = 3.39$ vs. 2.72), and were more forgiving ($M = 4.30$ vs. 3.80) than participants in the no-apology condition. However, no significant main effects emerged on revenge, $F(1, 639) = 0.02, p = .89, \eta_p^2 < .01$.

Effects of participant ethnicity. Consistent with the results of Studies 1–3, participants who were part of the group that was directly offended by the transgressor's actions (in this case Hispanic Americans) were significantly more negative across all the dependent measures than were participants who were not part of the group that was directly offended by the transgressor's actions (in this case White Americans; all $ps < .03$, all $\eta_p^2 > .01$). Of more relevance to the current research question, however, was whether the embodiment manipulation had different effects on the different samples. As in Studies 1 and 4, there was no evidence that this was the case (all $ps > .052$). Neither were there any significant ethnicities by apology interactions (all $ps > .06$).

Apology \times Embodiment interactions. As in Study 4, participants perceived more ulterior motives in the apology condition ($M = 5.72$) than in the no-apology condition ($M = 4.78$), $F(1, 630) = 72.75, p < .001, \eta_p^2 = .10$, but this was qualified by a significant embodiment by apology interaction, $F(1, 630) = 31.03, p < .001, \eta_p^2 = .05$. The pattern was the same as for Study 4: there was a strong tendency for participants to perceive more ulterior motives in the embodiment than in the no-embodiment condition, but this only emerged in the no-apology condition, $F(1, 630) = 62.19, p < .001, \eta_p^2 = .09$, not in the apology condition, $F(1, 630) = 0.01, p = .936, \eta_p^2 < .01$. This interaction was in turn qualified by the sole significant three-way interaction, $F(1, 630) = 5.59, p = .018, \eta_p^2 = .01$. The two-way interaction described above, while statistically reliable for both White Americans, $F(1, 630) = 4.81, p = .029$, and Hispanic Americans, $F(1, 630) = 27.90, p < .001$, was more pronounced for the latter (i.e., the victimized group). These were the only cases in which the em-

bodiment and apology manipulations featured in significant interactions.

Moderation by trust. We first tested whether the manipulations of apology and embodiment had significant effects on dispositional trust. The results of 2 (Hispanic Americans or White Americans) \times 2 (apology or no apology) \times 2 (embodiment or no embodiment) between-groups ANOVA on dispositional trust revealed only a main effect of participant ethnicity, $F(1, 789) = 12.57, p < .001, \eta^2 = .02$. White American participants ($M = 3.65$) were more trusting than Hispanic American participants ($M = 3.39$). Given that the regressions controlled for ethnicity, this was not a problem for our analyses. No other significant main or interaction effects emerged on trust (all $ps > .11$). Given this, we conducted the same set of regressions that we conducted in Study 4. Again, main effects of apology, embodiment, and ethnicity simply reflected what has been reported in the ANOVAs and so will not be discussed further.

As in Study 4, the higher participants were in dispositional trust, the higher they rated on positive appraisals of the target ($\beta = .11, p = .002$) and perceived remorse ($\beta = .11, p = .002$). Step 2 revealed only one significant trust by apology interaction, on response satisfaction ($\beta = .12, p = .022$). Consistent with Study 4, tests of simple effects revealed the apology had a positive effect on satisfaction among those high in dispositional trust, $b = 1.37, SE = .19, p < .001, 95\% CI [1.003, 1.731]$, and a smaller but still significant positive effect among those low in dispositional trust, $b = .87, SE = .19, p < .001, 95\% CI [.494, 1.245]$.

There was also a significant trust by embodiment interaction on revenge ($\beta = .16, p = .009$). Among those low in dispositional trust, participants were significantly less likely to want to engage in revenge when the remorse was embodied, $b = -.30, SE = .11, p = .004, 95\% CI [-.510, -.097]$. Among those high in dispositional trust, however, this effect was nonsignificant, $b = .08, SE = .10, p = .430, 95\% CI [-.123, .289]$.

Discussion

As in all previous studies, participants attributed more emotionality and more genuine remorse to the transgressor when he embodied his remorse. Consistent with Studies 1, 2 and 4, participants also appraised the transgressor more positively when he embodied the remorse expressed. However, unlike the previous

studies, this effect did not result in an improvement on any other outcome measure: the transgressor was not empathized with more when he cried, and participants were no more satisfied with the response when it involved tears. The unusually flat effect of embodiment in this study can probably be best explained by the nature of the transgression: the protracted nature of the transgression speaks to a fundamental failing in the transgressor's moral essence, so superficial displays (such as tears) may not have been sufficient to change participants' assessments of his character. As in all the previous studies, embodiment had no effect on forgiveness, regardless of which way it was operationalized. It is notable, though, that the verbal apology *was* sufficient to reliably improve impressions of the transgressor: this emerged on all outcome measures, including forgiveness.

As in the previous four studies, there was a lack of evidence that participants were explicitly negative about the transgressor's tears. The one exception, however, was in the no-apology condition: here, as in Study 4, there was a sizable tendency for participants to presume more ulterior motives when the transgressor cried than when he did not cry. However, there is no evidence that this suspicion about motive flowed through into other evaluations, and in the apology conditions there were no effects of tears on ulterior motives at all.

As in Study 4, dispositional trust was not highly influential in terms of shaping participants' responses to the embodiment manipulation. The one exception was revenge: tears significantly reduced feelings of vengeance toward Huppenthal among those low in dispositional trust, but this was offset by a null effect of tears among those high in dispositional trust. This is broadly consistent with Study 4: dispositional trust was relatively unimportant in shaping responses to the tears, but to the extent that it was, the effects of tears were most positive among people *low* in dispositional trust.

Study 6

Studies 1–5 showed a convergence of evidence that embodied displays of remorse have a small positive effect on how audiences respond to an apologizer and their comments. However, there is no consistent evidence that embodied remorse increases forgiveness. Study 6 incorporated a number of methodological innovations designed to explore some reasons for the previously observed null effects on forgiveness, and to further interrogate the boundary conditions under which embodiments of remorse may or may not have an effect. In doing so, we returned to the many-to-many context used in Studies 1 and 2, this time focusing on the iconic moment in which the former German Chancellor Willy Brandt dropped to his knees at a site memorializing the slaughter of Jewish citizens in Poland in WWII.

One possible reason why people might hold on to unforgiveness in intergroup contexts is that they do not feel as though they have the right to forgive. For example, there is qualitative evidence from Northern Ireland that withholding forgiveness can be seen as a way of "keeping faith" with the dead and the injured (McLernon, Cairns, & Hewstone, 2002). Similarly, it has been proposed that when someone who is close to you is injured, relinquishing anger can feel disloyal and disrespectful (Exline, Worthington, Hill, & McCullough, 2003). These insights underscore the difference between *direct victims* (people who are directly affected by the

transgression) and *indirect victims* (people who share an identity with those who have been directly affected, but who were personally unharmed). Although indirect victims may have a positive response to embodied remorse, it could be that they do not feel that they have the right to offer forgiveness. If so, this could potentially explain the unusual pattern of results described in Studies 1–5; movement across conditions on every variable *except* forgiveness.

To a degree, this explanation has been anticipated in previous studies, and with unpromising results. Philpot and Hornsey (2008, Study 3) conducted a study in which a collective apology was accompanied by a direct victim who advocated forgiveness. Although it was predicted that this would give participants permission to *unlock* their forgiveness, no such effect emerged. Furthermore, it could be argued that this explanation was addressed indirectly in Studies 4 and 5 of the current article. Here, members of the majority were compared with members of a minority group in terms of whether they were influenced by embodied remorse after a discriminatory act against the minority group. It could be argued that the minority group members have more of a right to forgive than members of the perpetrator group, and yet their response to the manipulation of embodiment was statistically equivalent. However, it could also be argued that these tests are somewhat tangential ways of exploring the hypothesis, and so we designed Study 6 to test it directly.

In Study 6, American participants were exposed to images of Willy Brandt either standing or kneeling in front of the Polish war memorial. A week before the manipulation, we measured the extent to which participants perceived a right to forgive Germany for their WWII aggression. After the manipulation, we supplemented the forgiveness measure with a measure of what we labeled *projected forgiveness*: To what extent did participants think they would or should forgive *if they were direct victims*? If the null result on forgiveness was caused by the fact that people did not feel as though they had a right to forgive perpetrators for events that did not directly affect them, this might be observable in two ways. First, we might see moderation by the right to forgive measure, such that the embodied remorse manipulation would have a positive effect on forgiveness for those high (but not low) in perceived right to forgive. Second, we might see a positive effect of embodied remorse on the projected forgiveness measure (in which participants are asked to respond as though they were direct victims).

Another goal of Study 6 was to examine whether implicit theories of groups moderate the effect of embodied remorse. Entity beliefs refer to the extent to which people feel as though humans are fixed and unchangeable entities (Chiu, Hong, & Dweck, 1997). Although typically measured with respect to individuals, this notion has been extended to apply to groups, such that an implicit entity belief reflects a predisposition to believe that the character of a group is fixed (Halperin, Russell, Trzesniewski, Gross, & Dweck, 2011). Theoretically, this implicit belief should impact the effectiveness of apologies. According to some (e.g., Gold & Weiner, 2000), estimations of reoffense likelihood are impacted by inferences about the fundamental personality of the offender and attributions of the cause of the event, and it is a utilitarian concern for the future that primarily mediates the effectiveness of apologies. It might be predicted, then, that people high in entity beliefs will be unmoved by gestures of remorse, which implicitly (or explicitly) promise change. This prediction is

borne out by data: People high in entity beliefs are less impacted by interpersonal apologies when deciding whether to forgive (Hornsey, Schumann, et al., 2017), are less open to compromise with historical enemies (Halperin et al., 2011), and are less positively influenced by collective apologies (Wohl et al., 2015). As such, it is possible that the effects of embodied remorse might be more positive among people low in entity beliefs than among people high in entity beliefs.

Finally, an additional benefit of using the kniefall context is that it explores a context in which the display of remorse is unexpected and culturally nonnormative. One theory about why Willy Brandt's kniefall moment was able to cut through is that it was surprising, spontaneous, and nonnormative (unlike the Japanese context used in Study 1, kneeling is not an orthodox part of the display rules associated with contrition among Western leaders). Although it is usually considered desirable to stay within normative prescriptions in public behavior, some have argued that the apology context might be an exception. Okimoto et al. (2015) showed that participants who are led to believe collective apologies are relatively common *desire* an apology more than those who are led to believe they are rare, but are also less impressed by the apology once they receive it. The authors referred to this phenomenon as *normative dilution*, making the case that the ubiquity of the gesture has reduced its power as a genuine signal of remorse. Arguably, the contexts we used in Studies 1–5 might have suffered from this process of normative dilution. Study 6 gave us the opportunity to examine a many-to-many context in which the display still has the power to surprise and capture attention.

Method

Participants and design. A power analysis determined that 352 participants would be required for an 80% chance of detecting a small-medium effect ($f = 0.15$). Data collection occurred across two waves through Prolific. In Wave 1, we collected data on demographics, as well as on the two proposed moderators: right to forgive and entity beliefs. Wave 2 (collected a week later) comprised the manipulation and the dependent measures. Of the 358 participants who completed Wave 1, 347 also completed Wave 2, and these were the final sample used for analysis ($M = 33.09$ years; 170 women, 168 men, 9 “other”). Of this sample, just five self-identified as Jewish. Results of the analyses did not change regardless of whether the Jewish participants were included or excluded from analysis; consequently, the analyses reported below were conducted with the Jewish participants included.

All participants were exposed to an image of Willy Brandt at the Polish war memorial, which was photoshopped to show him either kneeling (embodiment condition) or standing upright (no embodiment condition). As described below, the proposed moderators (right to forgive and entity beliefs) were measured a week before the manipulation.

Wave 1. Wave 1 incorporated the same demographics used in Study 2, as well as two new moderators: right to forgive and entity beliefs. *Right to forgive* was measured with three items: “I believe I have the right to forgive Germany for the persecution of Jewish people in WWII,” “It is not my place to forgive Germany for the persecution of Jewish people in WWII,” and “Only the victims of the Holocaust themselves have the right to forgive Germany for what happened in WWII” (1 = *not at all*, 7 = *very much*). After

reversing the last two items, the three items formed a reliable scale ($\alpha = .79$). *Entity beliefs* was measured using a four item scale adapted from Halperin et al. (2011): “Groups can do things differently but the important things of who they are can't really be changed,” “Groups that are characterized by violent tendencies will never change their ways,” “As much as I hate to admit it, you can't teach an old dog new tricks—groups can't really change their basic characteristics,” and “Every group or nation has basic moral values and beliefs that can't be changed significantly” (1 = *not at all*, 7 = *very much*; $\alpha = .88$).

To help minimize hypothesis awareness, we also measured a number of constructs that were unrelated to remorse or forgiveness (intended for a separate project). These comprised measures of conspiratorial ideation, disgust sensitivity, reactance, anxiety, trust in science, belief in precognition, and attitude toward pseudosciences. All scales—including the distractor scales—were presented in a randomized order.

Experimental stimuli. All participants received the following information (text in parentheses only appeared in the embodied remorse version of this condition):

The picture below was taken in 1970. It features Willy Brandt, who at the time was the German Chancellor. The photo was taken when Brandt was visiting a memorial for The Warsaw Ghetto Uprising in Poland. The uprising occurred following Jewish people's attempts to resist their movement to an extermination camp. In response, the Nazis systematically burnt each block in the Ghetto, killing more than 13,000 Jewish people. Brandt stood in silence (kneeled with his head bowed) in front of the memorial, a move that was interpreted as a gesture of remorse for Germany's World War II history.

The subsequent image that participants saw was a picture of Willy Brandt in front of the shrine. To assist the manipulation of embodiment, the image was altered to make Willy Brandt appear to be either standing (in the no-embodiment condition) or kneeling (in the embodiment condition). In both of the photographs, journalists can be seen in the backdrop of the photograph, recording the event.

Dependent measures. Measures of *response satisfaction* ($\alpha = .85$), *emotionality* ($r = .39$), *positive appraisal* ($\alpha = .89$), *perceived remorse* ($\alpha = .92$), *likelihood of reoffending* ($r = .58$), and *forgiveness* ($\alpha = .89$) were the same as those used in Study 1. The measure of *ulterior motives* was also the same as that used in Study 1, except we did not include in the questionnaire the item “concern about getting into trouble,” which seemed like a poor fit for the WWII context ($\alpha = .73$). In this case, the measures of *emotionality*, *perceived remorse*, and *positive appraisal* were asked with reference to Willy Brandt. Measures of *likelihood of reoffending* and *forgiveness* were framed with respect to “Germany.”

The new measure of projected forgiveness comprised the items: “If I were Jewish then I would forgive Germany for the harm done,” “I think that Jewish people should forgive Germany for the harm done,” “It is time for Jewish people to move past their negative feelings toward Germany for what happened in WWII,” and “Germany deserves to be forgiven for the persecution of Jewish people in WWII” (1 = *not at all*, 7 = *very much*). Although the correlations among the items were very high ($\alpha = .91$) it should be noted that the first item is the one that most proximally measures the extent to which participants think they would forgive

if they were direct victims, whereas the other three items could arguably be driven by concerns about intergroup relations (i.e., not telling victimized groups what to do) rather than projected forgiveness per se. The suspicion that there is a subtle qualitative distinction between these items is reinforced by the fact that, as will be seen below, the effects of embodied remorse on the two sets of items are somewhat different. In the interests of transparency, then, we report both sets of analyses below: for the first question as a stand-alone item, and then again for the four-item scale.

Results

Effects of embodiment. We first examined the effects of the embodiment manipulation on emotionality. Consistent with previous studies, the manipulation had a strong effect on the “emotional” item, $F(1, 345) = 10.48, p = .001, \eta_p^2 = .03$, such that participants in the embodied condition perceived more emotionality than did participants in the no-embodied condition. Unlike previous studies, however, there was no effect of the manipulation on the “upset” item, $F(1, 345) = 0.00, p = .978, \eta_p^2 < .01$. On reflection, it seems likely that the upset item is a more appropriate check on the manipulation that involves crying than it is of the more somber and restrained act of kneeling with head bowed. Given this, we viewed the single emotional item as the more valid check of the embodied remorse manipulation.

As can be seen in Table 7, participants also had more positive appraisals of Willy Brandt, $F(1, 345) = 5.71, p = .017, \eta_p^2 = .02$, and greater response satisfaction, $F(1, 345) = 5.32, p = .022, \eta_p^2 = .02$, in the embodiment condition than in the no-embodiment condition. However, no effects emerged on ulterior motives, $F(1, 345) = 1.89, p = .171, \eta_p^2 = .01$, perceived remorse, $F(1, 345) = 1.88, p = .171, \eta_p^2 = .01$, likelihood of reoffending, $F(1, 345) = 2.09, p = .149, \eta_p^2 = .01$, or forgiveness, $F(1, 345) = 1.78, p = .183, \eta_p^2 = .01$.

Of particular relevance to this study, there was also no effect of embodiment on projected forgiveness, $F(1, 345) = 2.42, p = .120, \eta_p^2 = .01$. However, there was an effect of embodiment on the single item: “If I were Jewish then I would forgive Germany for the harm done,” $F(1, 345) = 3.96, p = .047, \eta_p^2 = .01$. Participants in the embodiment condition ($M = 4.24$) endorsed this item more than did those in the no-embodiment condition ($M = 3.88$).

Moderation by right to forgive. Main effects of right to forgive emerged on emotionality, $b = .13, SE = .05, 95\% CI$

[.043, .222], response satisfaction, $b = .12, SE = .05, 95\% CI$ [.023, .209], forgiveness, $b = .31, SE = .04, 95\% CI$ [.225, .388], and projected forgiveness, $b = .34, SE = .04, 95\% CI$ [.257, .416]. This reflected the fact that participants who felt a stronger right to forgive at Wave 1 perceived Willy Brandt to be more emotional at Wave 2, were more satisfied with the response, were more forgiving, and thought others should be more forgiving. Of more relevance to the current research question, however, the effect of embodiment was not moderated by right to forgive on any of the measures, including both the single-item and four-item version of projected forgiveness (all $ps > .21$).

Moderation by entity beliefs. Main effects of entity beliefs emerged on emotionality, $b = .12, SE = .05, 95\% CI$ [.012, .218], positive appraisal, $b = -.07, SE = .04, 95\% CI$ [-.141, -.001], likelihood of reoffending, $b = .10, SE = .04, 95\% CI$ [.180, .010], and perceived remorse, $b = -.11, SE = .04, 95\% CI$ [-.192, -.025]. This reflected the fact that participants who had stronger entity theories of groups at Wave 1 perceived Willy Brandt to be more emotional at Wave 2, but were less positive in their appraisals of him, more likely to believe Germany would reoffend, and less likely to perceive remorse. Again, however, the effect of embodiment was not moderated by entity beliefs on any of the measures, including both the single-item and four-item version of projected forgiveness (all $ps > .07$).

Discussion

As in previous studies, the effect of embodied remorse was either positive or neutral. When he was presented kneeling, participants were more likely to see Willy Brandt as emotional, were more positive in their appraisal of him, and more satisfied with his response than when he was presented standing. However, on likelihood of reoffending, ulterior motives, and perceived remorse, there was no reliable difference between the two conditions.

In line with Studies 1–5, there was also no significant effect of the manipulation on forgiveness. Furthermore, there was no evidence that the effects of embodied remorse would emerge more strongly if participants were low in entity beliefs about groups. We speculated that one possible reason for the stubborn lack of movement on the forgiveness measure was that participants might not have felt as though they had the right to forgive Germany for events that did not directly affect them. However, there was only mixed support for such a process: the null result on forgiveness was consistent regardless of whether participants felt a strong or weak sense that they had a right to forgive. Participants did endorse the single item “If I were Jewish then I would forgive Germany for the harm done” more in the embodiment condition than the no-embodiment condition, which provides tentative support for the notion that effects on forgiveness would be more likely to emerge if participants placed themselves in the shoes of direct victims (“projected forgiveness”). However, we emphasize that this effect was weak, and did not emerge on the full four-item scale.

Internal Meta-Analysis

To help gain a bird’s eye view of the effects across studies, we conducted a random-effects analysis of the effects of embodiment across Studies 1–6. For these analyses, participants in Studies 4

Table 7
Means (and SDs) Across Levels of Embodiment: Study 6

Measure	Embodiment	No embodiment
Emotionality	4.84 _b (1.34)	4.35 _a (1.52)
Positive appraisals	5.19 _b (.95)	4.94 _a (1.00)
Response satisfaction	4.86 _b (1.49)	4.49 _a (1.47)
Perceived remorse	5.12 _a (1.10)	4.95 _a (1.22)
Ulterior motives	4.26 _a (1.32)	4.07 _a (1.33)
Likelihood of reoffending	2.32 _a (1.12)	2.50 _a (1.24)
Forgiveness	4.75 _a (1.45)	4.55 _a (1.33)
Projected forgiveness	4.07 _a (1.39)	3.84 _a (1.37)

Note. SDs are in parentheses. Means with different subscripts are significantly different from each other.

and 5 who were in the *no apology* condition were excluded. Distinct groups within studies were treated as independent samples. This resulted in 13 samples: the United States and Japan samples in Study 1; the United States and Korea samples in Study 2; the four perpetrator conditions in Study 3; the LGBT and straight samples in Study 4; the Hispanic American and White American samples in Study 5; and the United States sample in Study 6.

As can be seen in Table 8, effects of embodiment were strongest on perceived remorse ($d = .28$) and positive appraisals ($d = .28$), followed by response satisfaction ($d = .22$), likelihood of reoffending ($d = -.18$), and ulterior motives ($d = -.10$). Despite not reaching significance in any of the six studies, the extra power offered by the meta-analysis meant that the effect on forgiveness reached significance ($d = .10$). However, with pooled sample sizes as large as this, it is important to focus more on the effect sizes than on the significance level per se (Borenstein, 2000), and on this count the effect on forgiveness appears to be trivial.

General Discussion

The primary goal of the research reported in this article was to answer the following question: Does saying sorry receive a more positive response from the public when the apologizer displays some physical embodiment of their remorse? On the basis of six experiments, the short answer to this question appears to be “yes,” but with substantial caveats.

The Case for Embodiment Having Positive Effects

People who embodied their remorse were seen to be more genuinely sorry than people who simply *stated* their remorse. This effect was small-to-medium in size, and highly significant in Studies 1–5. Furthermore, these perceptions of remorse flowed through to improve other perspectives on the apologizers and their comments: when transgressors embodied remorse, participants appraised them more positively, were more satisfied with their response, were more empathic toward them, and perceived them to be less likely to reoffend.

One noteworthy aspect of the observed effects is their consistency across a number of contexts and multiple types of transgressions. Positive effects of embodiment emerged regardless of whether participants were appraising disastrous mistakes that led

to catastrophic loss of life (Studies 1 and 2), genocidal acts (Study 6), academic malpractice (Study 3), acts of public indecency (Study 3), the use of homophobic slurs (Study 4), or protracted and systematic acts of racism (Study 5). Embodiment had similar effects regardless of whether the display occurred in a public press conference or in a private meeting with victims (Studies 1–3). Positive effects of embodiment emerged regardless of whether the transgressions were committed by a collective (Studies 1, 2, and 6) or an individual (Studies 3–5); by a male (Studies 1, 5, and 6) or by a female (Studies 2 and 4). Perhaps most strikingly, embodiment of remorse had similarly positive effects regardless of whether or not the transgressor issued an apology (Studies 4 and 5). Finally, we showed positive effects using more than one operationalization of embodiment: kneeling (Studies 1 and 6) and crying (Studies 2–5).

At no stage in any experiment was there evidence that embodiment of remorse damages an apology. In Studies 4 and 5 people expressed high skepticism about the motives behind a tearful performance that was *not* accompanied by an apology, but even here perceptions of ulterior motive did not flow through into a reliable “backfire” effect of tears on forgiveness, positive appraisals of the target, empathy, or likelihood of reoffense. Put simply, embodiment of remorse mostly helped the transgressor; it never hurt them.

It is also revealing that, in Studies 4 and 5, the effects tended to emerge independently of whether participants were low or high in dispositional trust. This lack of moderation elucidates the psychological underpinnings of our effects. At the outset, we speculated that people may question the integrity of embodied remorse; that the apology may be perceived as insincere or self-pitying. However, if this were the case, the effects of embodied remorse would be stronger for people who had a general predisposition toward trusting others. This did not occur: Dispositional trust rarely featured in interactions with the embodiment manipulation, and when it did, the effects of embodiment tended to be stronger for *low-trust* participants (this was true on perceived remorse in Study 4; revenge in Study 5). This suggests that participants worked from the assumption that tears are spontaneous and cannot be controlled; that they provide a transparent window into the inner state of the transgressor. From this perspective, tears help because they are seen as an authentic signal as to the genuineness of the remorse,

Table 8
Random-Effects Meta-Analyses Testing the Effects of Embodied Remorse Across Studies 1 to 6

Measure	d	k	p	Q	I^2	T^2	CI
Positive appraisals	.28	13	<.001	26.76*	55.66	.02	[.16, .39]
Response satisfaction	.22	13	<.001	9.20	.00	.00	[.15, .30]
Perceived remorse	.28	13	<.001	18.22	34.17	.01	[.18, .37]
Ulterior motives	-.10	13	.046	19.68	38.87	.01	[-.19, -.002]
Likelihood of reoffending	-.18	9	<.001	3.33	.00	.00	[-.27, -.09]
Forgiveness	.10	13	.009	6.60	.00	.00	[.03, .17]

Note. Participants in Studies 4 and 5 who were in the *no apology* condition were excluded from this analysis. Distinct groups within studies were treated as independent samples: namely the United States and Japan samples in Study 1; the United States and Korea samples in Study 2; the four perpetrator conditions in Study 3; the lesbian, gay, bisexual, transgender (LGBT) and straight samples in Study 4; and the Hispanic and White samples in Study 5. d = pooled estimate of the standardized mean difference (M1 = Mean score in the embodied condition; M2 = Mean score in the nonembodied condition); k = the number of samples; Q = total variance where an asterisk represents significant variability in the outcome between studies; I^2 = proportion of variability because of heterogeneity between studies rather than sampling error, T^2 = between-study variance; CI = 95% confidence interval around d .

one that is beyond manipulation and public relations concerns and, therefore, does not require a “benefit of the doubt.”

It seems unlikely, however, that all acts of embodiment are reducible to an uncontrollable genuineness signal. The most obvious case for this is that, in Studies 1 and 6, the (perfectly controllable) act of kneeling had similarly positive effects. However, future research may be necessary to identify the mechanisms that might explain why kneeling had positive effects. One possibility is that it is culturally constrained; that because kneeling is a normative gesture of remorse in Japan, *failing* to kneel was seen as a mindful signal that the TEPCO executives did not accept that they should feel remorse. It should be noted, however, that the kniefall context in Study 6 was an outlier in the sense that it was the one study in which embodiment of remorse had no reliable effect on perceived remorse. This opens up the possibility that kneeling may promote positive responses through pathways other than communicating remorse. Perhaps, as speculated by Goffman (1967, 1971), the normative display of remorse signaled that they were humbled enough to play along with society’s rules, with everything that implies in terms of future behavior. It may also be that this gesture of remorse is particularly effective in signaling humility and a preparedness to accept a low-power role, which speaks to victims’ underlying need to have their power and dignity restored (Nadler & Shnabel, 2015).

Caveats Regarding the Positive Effects of Embodiment

Despite the overall positive picture of embodied remorse presented by the reported studies, it is clear that embodiment of remorse is not a panacea for the trust problems surrounding public apologies. The first caveat is that the effects of embodiment—although reliable—are modest in size (see Table 8). Furthermore, for most measures there was one study where the effect was so small that it did not reach conventional levels of significance (on positive appraisals this occurred in Study 3, on satisfaction and empathy it occurred in Study 5, on perceived remorse it happened in Study 6, and on likelihood of reoffending it occurred in Studies 1 and 6). In Studies 4 and 5, the effect sizes of the embodiment manipulation were dwarfed by the effect sizes of the apology manipulation.

Another major caveat is that the embodiment manipulation failed to have a substantial effect on forgiveness. This was true regardless of whether the participants were members of the victimized group or bystanders. Although the meta-analysis revealed a significant effect, the effect size was extremely small, and was not significant in any of the individual studies. It is hard to dismiss these null results as a measurement issue, in part because the manipulation of apology in Studies 4 and 5 *did* have effects on forgiveness. Furthermore, the weak results emerged across six highly powered studies, and in the context of consistently significant effects on other measures. It seems, then, that there is something meaningful and diagnostic of reality about these null results: in terms of whether the public forgives you—or even whether they want to inflict vengeful punishment on you—crying or kneeling does not help.

Close analysis of the data suggests a paradox: given that embodiment had a large effect on perceived remorse, and given that remorse was highly correlated with forgiveness (see Table 1), why did embodiment have no effect on forgiveness? One possibility is

that the positive effects of remorse are being suppressed by a countervailing effect on an as-yet unidentified suppressor variable, a possibility that should be pursued in future research. For now, however, it is worth raising the question of whether forgiveness should indeed be considered the “gold standard” to which the success of an apology is held. Apologies have tremendous value in their own right, independently of their effectiveness in promoting forgiveness. One of the most powerful functions of an apology is that they validate the victims’ experiences: they acknowledge that the transgression happened, that the transgression had painful consequences for the victims, and that these consequences were the transgressors’ fault. Whether this gesture translates to forgiveness is ultimately up to the victims to decide. Indeed, research suggests that the decision to forgive is based on factors more complex than whether the transgressor is sorry in the moment (Wohl, Kuiken, & Noels, 2006). For victims, an apology is not typically seen as the end of a process, but rather the beginning of a process: a statement of intent that behavior will change in the future. The outcome of this process may or may not lead to forgiveness depending on whether the implicit promises carried within the apology are followed through (Wohl, Hornsey, & Philpot, 2011; Wohl, Matheson, Branscombe, & Anisman, 2013).

Future Directions

From an applied point of view, one major benefit of embodied gestures of remorse is that they are easily communicated and disseminated. In a time-poor and attention-challenged world, it is rare for people to process a speech in depth. However, an image of a person kneeling or crying can be processed and understood in an instant. One direction for future research is to examine not just the effectiveness of these images, but also the extent to which the images are remembered and disseminated relative to verbal apologies. It would seem likely that images of embodied remorse (e.g., Willy Brandt’s kniefall) would be easier to remember and disseminate than text, but this remains an empirical question.

Although four out of the six studies in this article examined crying, we obviously do not advocate that apologizers attempt to *turn on* tears during a public statement. Crying is a difficult thing to control; it typically can only be done by upregulating negative emotions, but even then, tears are not easy for most to produce on command (Simons et al., 2013). Although there was a general lack of skepticism toward the crying targets in the current study, simulated crying would seem a high-risk strategy, and one that would easily invite allegations of emotional manipulation should it be transparent (ten Brinke et al., 2012). However, there is a vast array of nonverbal cues that apologizers can use to signify remorse. We examined kneeling in Studies 1 and 6, but there are many other nonverbal cues an apologizer could deploy (e.g., head down, gazing toward the ground). There exists a void in understanding the utility of nonverbal regret. We call for research that examines the nonverbal spectrum of regret to match the empirical attention that the verbal spectrum has received.

One methodological strength of Studies 2–4 was the use of digital technology to add and remove tears to the image of the transgressor. The result was perfectly controlled stimuli. However, a downside to this strategy is that the cues for regret were narrowed to the tears alone. We were unable to incorporate, among other signifiers of distress, knitted eyebrows, a red face, bloodshot eyes, or a trembling

lip. As research in this area grows, it would be ideal if the highly controlled experimental methods were complemented by more vivid real-world displays of remorse, encompassing both still and moving images. It may be particularly powerful, for example, if participants were exposed to real-life transgressions in the moment, accompanied by real-life remorse displays from confederates.

Another direction for future research would be to examine the cultural specificity of nonverbal gestures of remorse. It was interesting that kneeling was somewhat effective within our Japanese sample (Study 1), whereas crying was ineffective for the Korean sample (Study 2). There are many things that differ across these two studies that could have contributed to this discrepancy, but one possible reason is that kneeling is culturally normative within Japan (*dogeza*) whereas it is unclear whether crying has the same normative support within Korea. To date, however, the empirical literature on nonverbal communications of remorse is thin. For research in this domain to have genuinely global currency, it is important that researchers progress with a close eye on the role of culture.

Future research may benefit from interrogating more closely how different audience members respond to the same gesture. In the current studies, we incorporated participants who were part of the group that was transgressed against (i.e., Japanese in Study 1, Koreans in Study 2, LGBT participants in Study 4, and Hispanics in Study 5) as well as participants who were bystanders to the transgression. However, what is missing from our analysis is the voices of people who experienced intense and direct harm as a result of the transgression (e.g., family and friends of people who lost their lives in the Fukushima or SEWOL disasters). Typically, the number of direct victims of a transgression is much smaller than the number of indirect victims, so the propensity for forgiveness of the latter may be more predictive of the health of intergroup relations going forward. However, the forgiveness process of direct victims carries with it enormous applied importance for those affected individuals, and should be a focus for future research.

Such research would also have interesting theoretical implications, given that it seems likely that the forgiveness process is qualitatively distinct for direct versus indirect victims. For example, it has been speculated previously (e.g., Hornsey et al., 2015) that direct victims might have greater ambivalence around the forgiveness process than indirect victims. On one hand they may find it more *difficult* to forgive, given that the level of injury is relatively intense. However, they also might have more *motivation* to forgive: there are documented negative consequences of unforgiveness for physical and mental health (e.g., Wade, Hoyt, Kidwell, & Worthington, 2014), and so “moving on” might be seen as a process of self-compassion and/or self-protection. Another qualitative distinction between direct and indirect victims is that the former may feel as though they have more permission or “right” to forgive than the latter. In Study 6 we speculated that the feeling that indirect victims might not have a right to forgive could be one reason why there were flat effects of embodiment on forgiveness, despite positive effects of embodiment on constructs that are theoretically and empirically associated with forgiveness. On the whole, Study 6 revealed little support for this notion, given that effects were not moderated by participants’ perceived right to forgive, and given that the effects of embodiment did not emerge on a scale of projected forgiveness. However, there was a significant effect of embodiment on a face valid, single-item proxy for projected forgiveness, which offers enough promise to suggest this

construct should be a focus for more systematic research in the future.

Also missing is an analysis of how group identification might moderate effects. Brown et al. (2008) found that an American apology for a friendly fire incident was accepted more strongly by weakly identified Canadians than by strongly identified Canadians. It would be interesting to see whether group identification also plays a role in moderating responses to embodiments of remorse.

Finally, future research may benefit from examining more carefully the interactive effects of verbal and nonverbal components of an apology. In the current studies, the manipulation of the nonverbal of the apologizer occurred against the backdrop of a single apology statement. It is possible, however, that more fine-grained designs—ones that manipulate embodiment as well as manipulating different verbal components of the apology, such as offer of reparations—could uncover nuances that the current studies were not equipped to detect.

Conclusions

To date, most of the research examining the effectiveness of public apologies has focused on the verbal elements. Across six studies, we provide evidence for the importance of the “visual language” of an apology. Specifically, results suggest that apologies cut through with the public more when they are accompanied by physical demonstrations of remorse than when they are not. Furthermore, nonverbal expressions of remorse appeal to people in a way that does not seem to be overtly contaminated by skepticism of motive or concern for manipulation, which may be one reason that their effectiveness is equally strong (if not stronger) among the chronically mistrusting (compared with the chronically trusting).

Despite all the observed positive effects of nonverbal expressions of remorse, we did not find much evidence that embodied remorse increases forgiveness. Kneeling and crying promoted empathy and respect for the transgressor’s motives, but this in itself was not sufficient to promote the affective, cognitive, and behavioral shifts that define forgiveness. Rather than seeing this as an experimental failure or an empirical paradox, this null result may be better thought of as a common-sense stance from our participants; a reminder that after people have violated the trust of the public (whether it be through gross neglect, racism, sexual inappropriateness, or cheating), it takes more than a one-off emotional display to win it back.

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