

Exposure to Analogous Harmdoing Increases Acknowledgment of Ingroup Transgressions in Intergroup Conflicts

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Abstract

A major barrier to the resolution of intergroup conflicts is the reluctance to acknowledge transgressions committed by one's ingroup toward the outgroup. Existing research demonstrates that individuals are generally motivated to justify ingroup conduct and avoid experiencing guilt and shame about ingroup harmdoing. The current work explores the use of an analogy-based intervention to attenuate motivated reasoning in evaluations of ingroup harmdoing. Overall, across six studies, we find support for our hypothesis that considering a case of harmdoing in a removed context increases acknowledgment of an analogous case of ingroup harmdoing. We further explore why, and under what conditions, the analogy is effective in leading to increased acknowledgment of an ingroup transgression. We find that the effect of the analogy is mediated by the endorsement of moral principles specific to the domain of the transgression, suggesting that the mechanism involves a cognitive process of analogical reasoning.

Keywords

analogy, acknowledgment of ingroup harmdoing, psychological intervention, moral judgment, intergroup conflict

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Introduction

Intergroup reconciliation requires perpetrators to acknowledge their harmdoing (Čehajić-Clancy & Brown, 2010), yet people are often resistant to accept that their group has caused suffering (Branscombe & Miron, 2004). Such resistance can be both cognitive—with group members refusing to acknowledge the wrongness of ingroup harmdoing, and emotional—with group members avoiding corresponding feelings of guilt and shame. Individuals derive a sense of self-worth, in part, from group membership, as suggested by social identity theory (Tajfel & Turner, 1986), and are highly motivated to view their group in a positive light (Leach et al., 2007). Acknowledging harm committed by the ingroup, even acts in which the individual was not personally involved, can be highly threatening (Čehajić-Clancy et al., 2011).

When confronted with ingroup harmdoing, individuals find ways to cognitively disengage their moral standards to exonerate their group from wrongdoing (Bandura, 1999; Leidner & Castano, 2012). In the context of intractable conflicts, in particular, people frequently deny ingroup harmdoing (Cohen, 2001) and if undeniable, use many strategies to rationalize or downplay it (Halperin et al., 2010; Waytz et al., 2014). This results in moral hypocrisy, whereby individuals judge their group's wrongdoings more leniently than those of

others (Lammers et al., 2010; Valdesolo & DeSteno, 2007, 2008). Similarly, people rarely experience group-based critical emotions such as guilt and shame toward ingroup conduct (Leach et al., 2013). However, group-based critical emotions play a central role for justice and reconciliation processes (Allpress et al., 2010; Leach et al., 2006; Reifen Tagar et al., 2014, but see Leach & Cidam, 2015, about the two distinct outcomes of shame). It, therefore, remains an important challenge for conflict resolution to promote both cognitive and emotional acknowledgment of ingroup harmdoing.

What, then, can be done to reduce biased processing to increase acknowledgment of ingroup harmdoing? Research on motivated reasoning suggests that the motivation to reach preferred conclusions can be attenuated by making evaluations in a comparative context (Paharia et al., 2013). One way to reduce double standards may be by presenting people

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with analogies (e.g., Uhlmann et al., 2009). We investigate whether analogies can lead to less biased evaluation of ingroup harmdoing. Specifically, we test whether evaluating harmdoing in a different, unrelated, context where there is no motivation to justify the harmdoing, subsequently reduces biased evaluation of an analogous ingroup transgression.

Analogies in Moral Judgment

Analogy is an important tool in reasoning (Gick & Holyoak, 1980). Analogical reasoning involves considering a source and a target that share similarities or higher-order relational structures and transferring knowledge from the source to the target via new insight garnered from considering the source (Gentner, 1983). Analogy can facilitate problem solving and lead to new discoveries (Gentner, 2006). Analogy has also been used in moral decision-making and problem solving, including in the interpersonal domain, as generalizations from solved problems can inform solutions to target problems (Barak-Corren et al., 2017; Blass & Forbus, 2015). Research in moral judgment, primarily focusing on hypothetical moral dilemmas, has found that people are more likely to judge an ambiguous scenario as wrong after first engaging with an analogous scenario that is unequivocally wrong (Lombrozo, 2009; Schwitzgebel & Cushman, 2015; Wiegmann et al., 2012; Wiegmann & Waldmann, 2014). In the political context, Study 2 in Uhlmann et al. (2009) found that participants' political ideology motivated their moral judgments of hypothetical moral dilemmas, but when asked about two similar dilemmas sequentially, participants were consistent with their initial biased judgments when responding to the second dilemma, regardless of the order in which they were presented. This indicates that analogies may be used to foster consistency between judgments that would have otherwise differed due to motivated reasoning.

Recent relevant work that we know of includes an examination of analogies in the intergroup context and showed that White participants were less likely to collectively blame Muslims for terrorism committed by Islamic extremists after first being asked how responsible they think they, themselves, or White people were for hate crimes committed in the name of White identity (Bruneau et al., 2018, 2019). This research highlights the power of analogies in shaping moral judgment in the intergroup context. Yet, it remains to be tested whether analogies can lead people toward acknowledging ingroup harmdoing in intractable conflict.¹ In the context of intractable conflict, in which people's attitudes are particularly rigid and resistant to change (Halperin & Bar-Tal, 2011), analogies might fail to be impactful. Drawing an analogy between harmdoing *toward* the ingroup and harmdoing *of* the ingroup might backfire, as reminders of victimhood may reduce feelings of guilt toward ingroup harmdoing (Wohl & Branscombe, 2008). We therefore propose a subtler approach to encourage less biased evaluation of ingroup harmdoing, in which participants

consider a removed, analogous scenario of harmdoing that is unrelated to their conflict.

The Current Research

We hypothesized that evaluating an analogical case of harmdoing in a removed context, where there is no motivation to justify the perpetrators, would increase acknowledgment of ingroup harmdoing as individuals apply insights gained from the removed case to that of ingroup harmdoing. We tested our hypothesis in the context of the Israeli–Palestinian conflict, focusing on Jewish-Israelis' judgments of home demolition carried out by Israel toward families of Palestinian terrorists. We used home demolition as our target case as it is official policy and therefore unlikely that participants would deny that the harmdoing occurred, or would discount it as the work of a few law-breakers. Although this policy of home demolition is widely supported among Jewish-Israelis—80% support the policy (Peace Index, 2015)—it goes against a strong international norm that prohibits harming one person for another's crime (Darcy, 2010). Interestingly, most Israelis (53%) oppose demolishing the family home of an Israeli who has murdered Palestinians for nationalistic reasons (Peace Index, 2015), suggesting double standards.

We conducted six studies to test whether, why, and under what conditions evaluation of harmdoing in a removed context leads to increased acknowledgment of ingroup harmdoing. In Study 1, we examined the effect of the presence or absence of an analogy on evaluation of ingroup harmdoing. In Studies 2 and 3, we aimed to rule out confounding explanations for the effect. In Study 4, we tested whether the analogy was still effective if anticipated, by presenting the cases of removed and ingroup harmdoing side by side. In Study 5, we tested whether the removed case of harmdoing leads participants to acknowledge ingroup harmdoing if it is more extreme than that of ingroup harmdoing, or rather if a more extreme removed scenario gives participants room to legitimize ingroup harmdoing. In Study 6, we directly tested the mechanism for the effect of the analogy, namely, analogical reasoning. We assessed whether engaging with a removed case of harmdoing leads to increased endorsement of domain-specific moral principles against the harmdoing described, specifically against collective punishment of family members, leading to increased acknowledgment of the ingroup transgression. To rule out an anchoring bias, we tested whether the effect of the analogy holds if participants simply contemplate the case of removed-harmdoing, rather than respond to items evaluating it. The data for all studies is available at <https://osf.io/wv8ch/>.

Study 1

In Study 1, we tested whether first evaluating a case of harmdoing in a removed context leads people to perceive a similar case of ingroup harmdoing as more wrong, expressed both cognitively, in judgment of ingroup harmdoing, and

emotionally, in group-based critical emotions toward the harmdoing.

Method

Participants and procedure. We recruited 185 Jewish-Israeli participants via an online survey company. After dropping 14 participants who failed attention checks, 171 participants remained (demographic details of the sample for each study can be found in the Supplemental Material). In Studies 1 and 2, we did not conduct an a priori power analysis to determine the sample size.

Participants were randomly assigned to one of the two conditions: in the single-evaluation condition, participants read an article, based on a real ingroup transgression, where Israeli authorities demolished the home of Mohammed Allyan, a Palestinian, whose son committed a deadly terrorist attack in Israel and was killed at the scene. The article explained that several family members were now homeless, included an image of Mohammed standing in the ruins of his home, and provided information about the Israeli policy of home demolition. In the analogy condition, participants read the same article, but first, they read and evaluated a similar (fabricated) case of collective punishment in a removed context, describing the Sri Lankan authorities' arrest of an uncle (Myo) and cousins of members of a violent, nationalist, Buddhist group and described the government policy of detaining relatives, without trial (see the Supplemental Material for full texts). In both articles, it was clear that those who were punished were uninvolved with the violent acts. The stories were analogous as they both depicted situations where family members were punished for the political violence of their relatives. Participants in both conditions then answered a short survey, including measures of moral judgment and group-based critical emotions toward ingroup harmdoing.

Measures. All response scales for items across studies ranged from 1 (*not at all*) to 7 (*very much*). In all studies, response scales for evaluation of ingroup harmdoing and removed-harmdoing did not include numbers, but rather labels at the endpoints and midpoint of the scale, to reduce the potential effect of numerical anchoring on subsequent judgments of ingroup harmdoing (see Tversky & Kahneman, 1974, on numerical anchoring). In all studies, participants reported their gender, age, religiosity, and political ideology using one item (How would you define your political stance?) with answers ranging from 1 (*extreme left*) to 7 (*extreme right*).

Wrongness of removed-harmdoing. After reading the removed-harmdoing case and before reading the ingroup-harmdoing case, participants in the analogy condition were asked how wrong they thought the removed-harmdoing was ("How wrong do you think it was that Myo and his sons were arrested?" and "To what extent do you view these types of

methods in response to terrorist attacks as morally wrong?"; $\alpha = .85$).

Empathic emotions toward removed victims. Participants in the analogy condition were asked to rate how much they felt empathy and compassion toward, and sadness and anger for, the victims of removed-harmdoing ($\alpha = .93$).

Main measures. Participants in both conditions responded to the following measures.

Wrongness of ingroup harmdoing. Two items measured the perceived wrongness of ingroup harmdoing: "How wrong do you think it was to destroy the Allyan's (the terrorist's) family home?" and "To what extent do you view these types of methods in response to terrorist attacks as morally wrong?" ($\alpha = .86$).

Group-based critical emotions. Participants were asked to what extent they felt shame and guilt about Israeli forces demolishing the Palestinian family home ($\alpha = .84$).

Moderators. In all studies, we included potential exploratory moderators, such as perception of the Israeli–Palestinian conflict as unique and ingroup identification.²

Results and Discussion

Initial analysis. First, we assessed participants' responses toward the case of Sri Lankan collective punishment to check whether participants indeed viewed it as wrong. We found a near ceiling effect with participants viewing it as morally wrong ($M = 5.93$, $SD = 1.21$) and experiencing high levels of empathic emotions toward the victims ($M = 5.37$, $SD = 1.36$). In contrast, participants in the single-evaluation condition did not view the ingroup-harmdoing case as wrong ($M = 2.30$, $SD = 1.63$). We found further suggestive evidence that evaluations of ingroup harmdoing were motivated as there were significant correlations between political ideology and evaluations of ingroup harmdoing, among participants in the single-evaluation condition, with participants who identified as more right-wing judging ingroup harmdoing as less wrong ($r = -.51$, $p < .001$) and feeling weaker group-based critical emotions ($r = -.39$, $p < .001$), whereas there was no correlation between political ideology and moral judgment of the removed scenario ($r = -.11$, $p = .35$) or empathic emotions toward the victims of removed-harmdoing ($r = -.12$, $p = .32$).

We also tested whether there was a correlation between judgments of removed-harmdoing and judgments of ingroup harmdoing among those in the analogy condition. Indeed, we found a moderate positive correlation ($r = .37$, $p = .001$), which remained unchanged when controlling for political ideology, indicative of analogical reasoning. Correlations

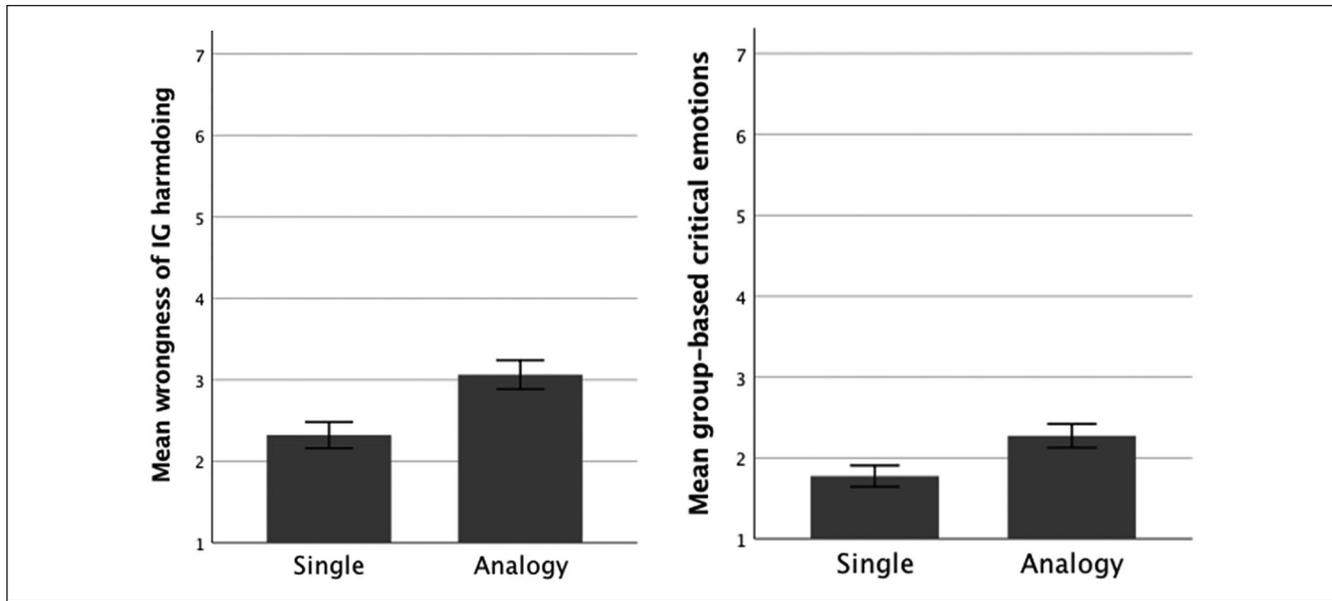


Figure 1. Effect of condition on perceived wrongness of ingroup harmdoing and on group-based critical emotions, controlling for political ideology, in Study 1.

Note. Error bars represent \pm SE of the mean.

across conditions are presented in Table S1 in the Supplemental Material.

Main analysis. Given its centrality in predicting attitudes in the context of the Israeli–Palestinian conflict (Bar-Tal, 2013), across our main analysis in all studies, we controlled for political ideology. To test whether the analogy led participants to perceive ingroup harmdoing as more wrong, we conducted a one-way analysis of covariance (ANCOVA). We found a significant effect of condition on wrongness judgments, $F(1, 168) = 9.57, p = .002, \eta_p^2 = .054$. Participants in the analogy condition judged ingroup harmdoing as more wrong ($M = 3.06, SE = 0.18, 95\%$ confidence interval [CI] = [2.71, 3.41]) than those in the single-evaluation condition ($M = 2.32, SE = 0.16, 95\%$ CI = [2.01, 2.64]; Figure 1). We also found a main effect of condition on group-based critical emotions, $F(1, 168) = 6.39, p = .012, \eta_p^2 = .037$. Those in the analogy condition experienced stronger group-based critical emotions toward ingroup harmdoing ($M = 2.27, SE = 0.15, 95\%$ CI = [1.99, 2.56]) than those in the single-evaluation condition ($M = 1.78, SE = 0.13, 95\%$ CI = [1.51, 2.04]; Figure 1). These effects were not moderated by political ideology, demographic variables, or exploratory moderators included.

Study 1 provided support for our hypothesis that first considering a case of harmdoing in a removed context leads to increased acknowledgment of ingroup harmdoing. However, the psychological process by which the first scenario affected evaluation of the second remained unclear. Although these results could be due to analogical reasoning, there are other aspects of the removed scenario that could have contributed

to the effect. Our next steps, in Studies 2 and 3, were to test whether empathy arousal or increased endorsement of general moral principles or moral anger is sufficient for the removed scenario to impact acknowledgment of ingroup harmdoing, or whether it is necessary that the removed scenario depicts an analogous moral transgression.

Study 2

One explanation for the findings of Study 1 may be that the first scenario depicted human suffering, thereby arousing empathy, which then affected judgments of ingroup harmdoing. The appraisal tendency framework suggests that emotions play a powerful role in shaping cognitive processes and decision-making (Lerner et al., 2015). In the context of conflict, empathy toward outgroup members can lead to more positive attitudes toward them and a desire to help them (Pliskin et al., 2014; Stephan & Finlay, 1999). It is therefore possible that empathy aroused from evaluating removed-harmdoing could account for the observed effect, rather than the evaluation of analogical harmdoing. To rule out this potential confound, we added a condition where participants first evaluated a removed case of suffering that would likely arouse empathy, but did not depict a moral transgression. If the original observed effect can be attributed to the empathy elicited by the removed case, rather than the analogy, then this new case depicting suffering would also increase acknowledgment of ingroup harmdoing.

We additionally tested whether individual differences in preference for consistency moderated the effect of the analogy. While the literature on analogical reasoning suggests

that analogies operates via a cognitive learning process, it is possible that a motivation for consistency underlies the observed effects. We considered that people who have a higher preference for consistency may be more affected by the analogy.

Method

Participants and procedure. We recruited 216 Jewish-Israeli participants via an online survey company. After dropping 20 participants who failed attention checks, 196 remained.

The procedure was the same as in Study 1, except for the additional empathy condition. Participants were randomly assigned to one of the three conditions: single-evaluation, analogy, and empathy. Before evaluating ingroup harmdoing, participants in the empathy condition evaluated a fabricated article about a flood in Sri Lanka that left families homeless (see the full text in the Supplemental Material).

Measures. As in Study 1, participants in the analogy condition rated *wrongness of removed-harmdoing* ($\alpha = .86$). Participants in the analogy and empathy conditions rated *empathic emotions toward removed victims*. These questions were asked in order that participants reflect on the removed scenarios. The emotions we compared across these two conditions to check that our empathy manipulation did arouse empathy were empathy and compassion toward the removed victims ($\alpha = .73$).

Main measures. As in Study 1, all participants responded to measures of *wrongness of ingroup harmdoing* ($\alpha = .93$)³ and *group-based critical emotions* ($\alpha = .92$).

Preference for consistency. This six-item scale, comprising items from a measure by Cialdini et al. (1995), was also included after the outcome variables, and measured participants' desire to be consistent, based on self-report ($\alpha = .78$; see the measure in the Supplemental Material).

Results and Discussion

Initial analysis. Table S2 in the Supplemental Material contains correlations between political ideology and judgments of harmdoing per condition and Table S3 in the Supplemental Material contains correlations across conditions. We first tested whether the article depicting floods in Sri Lanka evoked as much empathy and compassion as that depicting collective punishment in Sri Lanka. We ran an independent-samples *t* test and established that both articles evoked the same degree of empathy toward the victims ($M = 5.57$, $SD = 1.16$ vs. $M = 5.58$, $SD = 1.27$), $t(128) = 0.08$, $p = .938$.

Main analysis. A one-way ANCOVA, controlling for political ideology, revealed a significant effect of condition on judgment of ingroup harmdoing, $F(2, 189) = 4.17$, $p = .017$,

$\eta_p^2 = .04$. Compared with those in the single-evaluation condition ($M = 2.27$, $SE = 0.18$, 95% CI = [1.91, 2.63]), those in the analogy condition judged ingroup harmdoing as more wrong ($M = 2.87$, $SE = 0.18$, 95% CI = [2.51, 3.23], $p = .021$). There were no differences in perceived wrongness of ingroup harmdoing between those in the single-evaluation and the empathy condition ($M = 2.17$, $SE = 0.19$, 95% CI = [1.80, 2.55], $p = .724$). Participants in the analogy condition judged ingroup harmdoing as significantly more wrong than those in the empathy condition ($p = .009$; Figure 2). We also found an effect of condition on group-based critical emotions regarding ingroup harmdoing, $F(2, 189) = 2.94$, $p = .055$, $\eta_p^2 = .03$. Compared with those in the single-evaluation condition ($M = 1.83$, $SE = 0.16$, 95% CI = [1.52, 2.14]), those in the analogy condition tended to feel stronger group-based critical emotions, although the effect was marginal ($M = 2.25$, $SE = 0.16$, 95% CI = [1.94, 2.56], $p = .061$). There was no difference in group-based critical emotions between those in the single-evaluation condition and empathy condition ($M = 1.74$, $SE = 0.16$, 95% CI = [1.42, 2.07], $p = .687$). Participants in the empathy condition felt less group-based critical emotions than those in the analogy condition ($p = .026$; Figure 2).

We next tested whether preference for consistency moderated the effectiveness of the analogy condition and found that it was not a moderator ($p = .30$), suggesting that a conscious motivation for consistency is not driving the effect. Again, effects in this study were not moderated by political ideology, exploratory moderators included, or demographic variables.

We replicated the results of Study 1 and showed that evaluating removed-harmdoing led participants to view similar ingroup harmdoing as more wrong, compared with only evaluating ingroup harmdoing and first evaluating an empathy-arousing scenario. The empathy-arousing scenario depicting suffering, rather than a moral transgression, did not affect perceptions of ingroup harmdoing. These results rule out the possibility that our findings were solely due to empathy arousal.

Study 3

Results of Study 2 indicated that the removed case evaluated should depict a moral transgression (i.e., harmdoing) for it to impact judgments of ingroup harmdoing. However, it remains unclear whether the two cases need to depict harmdoing in the same domain (e.g., collective punishment). It may be that first engaging with any transgression increases endorsement of general moral principles against injustice, or its emotional counterpart, moral anger, thereby leading people to view ingroup harmdoing as more wrong. An analogy may, however, only be effective if the two cases of harmdoing are in a similar domain. According to research on analogical mapping, analogies often go unnoticed if the source and target are from different domains (Gick & Holyoak, 1983). To test this, in addition to the single-evaluation condition and the analogy

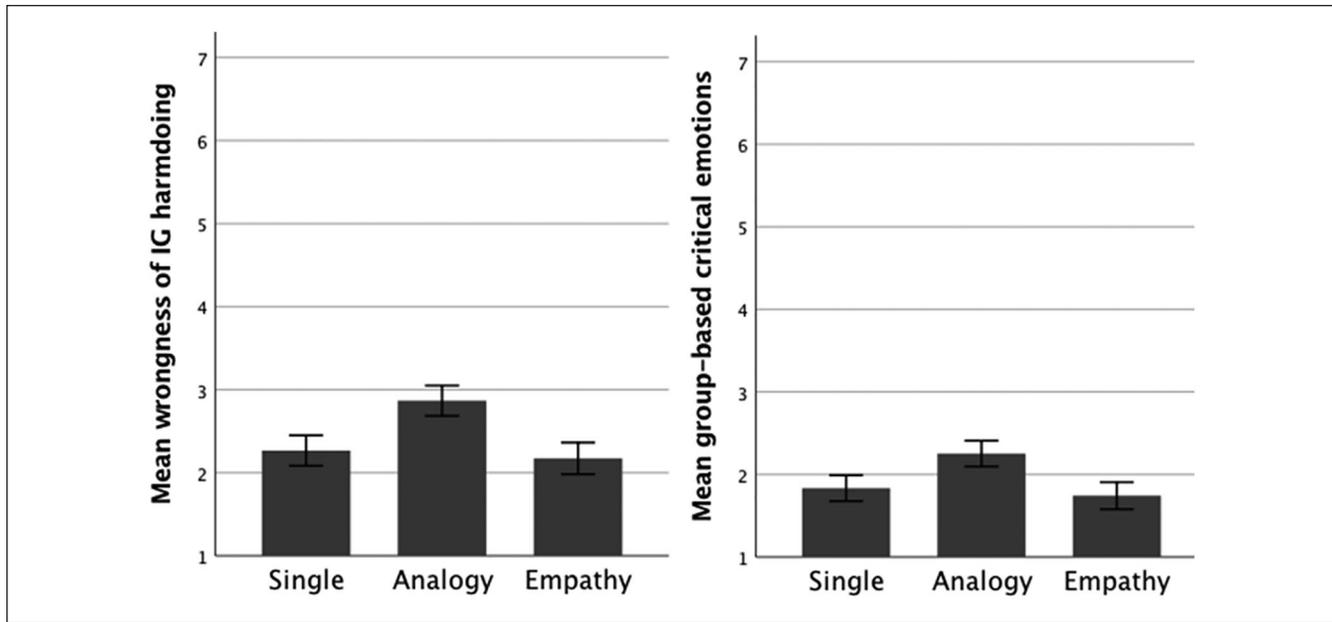


Figure 2. Effect of condition on perceived wrongness of ingroup harmdoing and on group-based critical emotions, controlling for political ideology, in Study 2.

Note. Error bars represent $\pm SE$ of the mean.

condition, we included a condition in which participants first read about a (fabricated) case of harmdoing describing Sri Lankan men being arbitrarily arrested and extorted by the police. This is a case of injustice, but unrelated to collective punishment. We were thus able to test whether this scenario increased endorsement of general moral principles and moral anger, and whether either was sufficient to increase acknowledgment of ingroup harmdoing.

Method

Participants and procedure. We used G*Power to determine an optimal sample size (Faul et al., 2009) based on a small/medium effect size (Cohen's $d = .35$), an α of .05 and power of .80. The power analysis determined that we needed 318 participants in total. We recruited 346 Jewish-Israeli participants to allow for drop out. After screening out 17 who failed attention checks, 329 participants remained.

Participants were assigned to a single-evaluation, analogy, or unrelated-harm condition. In the unrelated-harm condition, participants first read about a case of police corruption in Sri Lanka in which people were arrested for no reason and extorted for money (the full text is presented in the Supplemental Material). This article was identical to that first read in the analogy condition, except the reason behind the arrest was different and unrelated to collective punishment.

Measures. Participants in the analogy and unrelated-harm conditions rated *wrongness of removed-harmdoing* ($\alpha = .80$)

and *empathic emotions toward removed victims* ($\alpha = .92$). We were only interested in comparing anger for victims across these conditions to verify that the article in the unrelated-harm condition evoked just as much anger for the victims as that in the analogy condition. We measured preference for consistency to check again whether it may moderate the effect of the analogy.

Main measures. As in previous studies, we measured *wrongness of ingroup harmdoing* ($\alpha = .88$) and *group-based critical emotions toward ingroup harmdoing* ($\alpha = .88$).

General moral principles. This measure, asked at the end of the survey, aimed to capture how much participants endorsed general moral principles against injustice. Participants were asked to think of their moral principles and rate their agreement with the following statements: "It is wrong to punish people for something they did not do" and "It is unfair to punish people for a crime they did not commit" ($\alpha = .91$).

Results and Discussion

Initial analysis. Table S4 in the Supplemental Material contains correlations per condition and Table S5 in the Supplemental Material contains correlations across conditions. We first checked whether participants perceived the corruption case to be as wrong as that of removed collective punishment. A t test revealed that the corruption case was actually perceived as more wrong ($M = 6.78$, $SD = 0.61$) than the removed collective punishment case ($M = 5.75$, $SD = 1.36$),

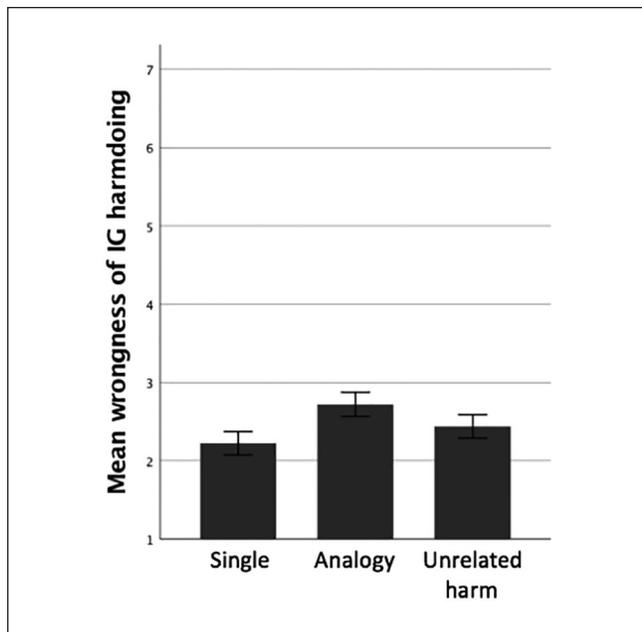


Figure 3. Effect of condition on perceived wrongness of ingroup harming, controlling for political ideology, in Study 3. Note. Error bars represent $\pm SE$ of the mean.

$t(1,214) = 7.21, p = .001, d = .98$, thus constituting a more conservative test. Similarly, those in the unrelated-harm condition were more angry for the victims of corruption ($M = 6.11, SD = 1.47$) than those in the analogy condition were for the victims of removed collective punishment ($M = 4.74, SD = 1.76$), $t(1,207) = 6.15, p < .001, d = .84$. General moral principles were also more strongly endorsed in the unrelated-harm condition. We ran a one-way ANCOVA, controlling for political ideology as this measure was asked at the end of the study and was correlated with political ideology, and found an effect of condition, $F(2,317) = 18.40, p < .001, \eta_p^2 = .104$, on the endorsement of general moral principles. Compared with those in the single-evaluation condition ($M = 4.38, SE = 0.16, 95\% CI = [4.07, 4.70]$), those in the analogy condition endorsed general moral principles more strongly ($M = 4.84, SE = 0.17, 95\% CI = [4.52, 5.17], p = .047$) and so did those in the unrelated-harm condition ($M = 5.74, SE = 0.16, 95\% CI = [5.42, 6.06], p < .001$). Participants in the unrelated-harm condition endorsed general moral principles more strongly than those in the analogy condition ($p < .001$).

Main analysis. We conducted a one-way ANCOVA and found a marginal effect of condition on judgment of ingroup harming, $F(2, 317) = 2.67, p = .071, \eta_p^2 = .017$. Again, those in the analogy condition viewed ingroup harming as significantly more wrong ($M = 2.72, SE = 0.16, 95\% CI = [2.42, 3.02]$), compared with those in the single-evaluation condition ($M = 2.23, SE = 0.15, 95\% CI = [1.93, 2.52], p = .022$). As can be seen in Figure 3, those in the unrelated-harm

condition judged ingroup harming as slightly more wrong ($M = 2.44, SE = 0.15, 95\% CI = [2.14, 2.73]$) than those in the single-evaluation condition and as slightly less wrong than those in the analogy condition, but neither of these differences were significant ($p = .315$ and $p = .190$, respectively). We found no significant differences between conditions for group-based critical emotions toward ingroup harming, $F(2, 317) = 0.15, p = .863, \eta_p^2 = .001$, thus failing to replicate the effect found in previous studies. As in the previous study, preference for consistency did not moderate the effect of the analogy ($p = .30$), nor did political ideology, demographic variables, or other moderators included.

While those in the analogy condition (vs. the single-evaluation condition) acknowledged ingroup harming as more wrong and had stronger endorsement of general moral principles against injustice, these principles were not found to mediate the effect of the analogy on ingroup harming. This suggests that endorsing general moral principles that are not specific to the domain of ingroup harming (i.e., collective punishment) is not a mechanism for the effect of the analogy.

The results of Study 3 provide additional (although only partial) support for the effectiveness of analogy-based interventions to increase acknowledgment of ingroup harming and suggest that analogies work best when the source and target case are within the same domain. The corruption case was judged as more wrong than that of removed collective punishment, and it led to increased levels of anger for victims, and increased endorsement of general moral principles. Despite this, those who first read about corruption did not evaluate ingroup harming as significantly more wrong compared with those in the single-evaluation condition, whereas those in the analogy condition did. Our results indicate that endorsing general moral principles against injustice and experiencing moral anger are not sufficient to explain the effects of the analogy-based intervention. It is important to note, however, that the analogy and unrelated-harm conditions did not significantly differ on wrongness judgments of ingroup harming. While our results indicate that engaging with a removed case of harming in the same domain is most effective in shaping judgments of ingroup harming, these results should be interpreted with caution as there was not a significant difference between the analogy condition and unrelated-harm condition. We return to question the importance of domain specificity in Study 6, when we examine the mechanism of the analogy.

There was no difference between group-based critical emotions toward ingroup harming across conditions, unlike in our first two studies. We were surprised by this null result and continued to explore the effect of the analogy on this outcome in our next studies to assess whether this was simply a type 2 error or represented a more meaningful pattern.

Study 4

In Studies 1 to 3, we found that evaluating a removed case of harming led participants to then judge a similar ingroup

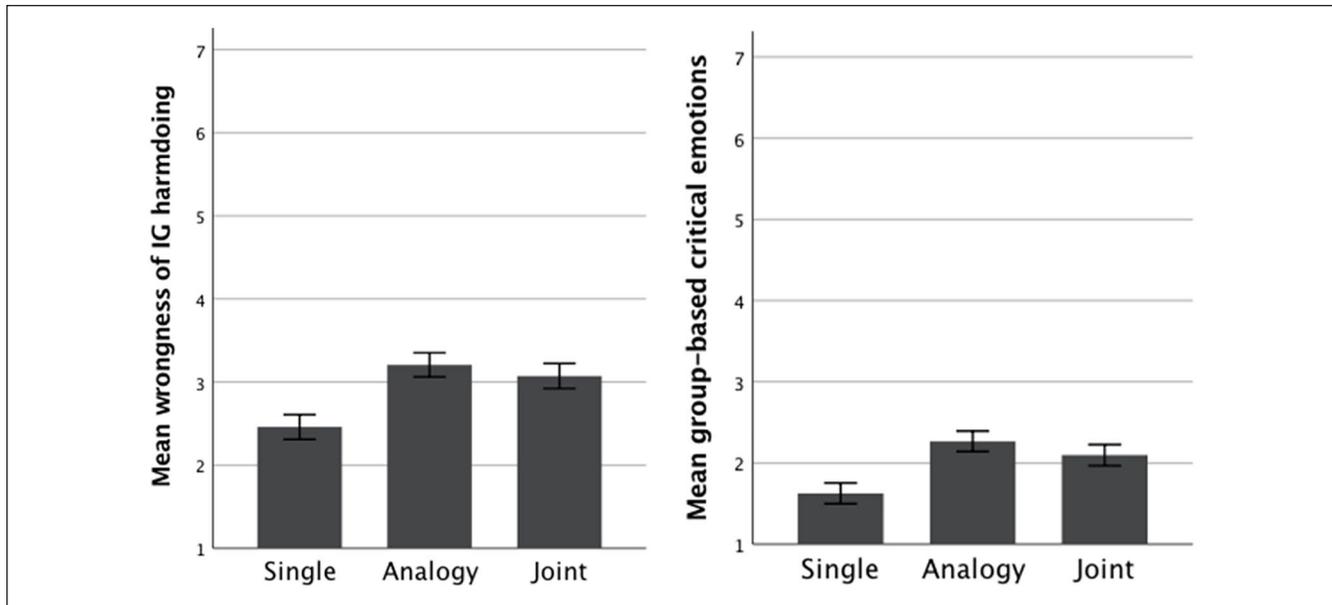


Figure 4. Effect of condition on perceived wrongness of ingroup harmdoing and on group-based critical emotions, controlling for political ideology, in Study 4.

Note. Error bars represent $\pm SE$ of the mean.

transgression as more wrong. In these studies, participants did not anticipate the analogy while considering the removed case of harmdoing, and therefore their judgments about the removed scenario were unlikely to be motivated. In this study, we tested whether our analogy would still be effective if anticipated. We considered that if participants anticipate the analogy, they might be motivated to judge the removed case of harmdoing less critically, as well. This would then allow them to judge ingroup harmdoing less critically while still being consistent. Alternatively, the removed-harmdoing case may encourage reflection as new insights about collective punishment are formed and, consequently, lead participants to judge ingroup harmdoing as more wrong. To test the effect of anticipating the analogy, we added a condition in which participant saw both articles together before any evaluation.⁴

Method

Participants and procedure. Based on the same power calculation as in Study 3, we recruited 338 Jewish-Israeli participants through a survey company. We dropped 20 participants who failed attention checks, leaving 318 participants.

Participants were assigned to a single-evaluation, analogy, or joint-evaluation condition. In the joint-evaluation condition, participants were presented with the two articles side-by-side and told that they would then read each article in full. They were then asked to type the headlines of both articles before proceeding to read and evaluate each, to ensure that they anticipated evaluation of ingroup harmdoing before evaluating removed-harmdoing. In order not to introduce any confounds, participants evaluated removed-harmdoing and then ingroup harmdoing. The only way this

condition differed from the analogy condition was that both articles were first presented side-by-side, and therefore participants could anticipate the analogy before evaluation.

Measures. Across all conditions, we measured *wrongness of removed-harmdoing* ($\alpha = .92$), *empathic emotions toward removed victims* ($\alpha = .93$), *wrongness of ingroup harmdoing* ($\alpha = .90$), and *group-based critical emotions* ($\alpha = .88$) using the same items as in previous studies.

Results and Discussion

Correlations per condition are presented in Table S6 in the Supplemental Material, and correlations across conditions are presented in Table S7 in the Supplemental Material. We conducted a one-way ANCOVA and found a significant effect of condition on judgment of ingroup harmdoing, $F(2, 310) = 7.23, p = .001, \eta_p^2 = .045$. Compared with those in the single-evaluation condition who evaluated ingroup harmdoing first ($M = 2.46, SE = 0.15, 95\% CI = [2.17, 2.75]$), participants in the joint-evaluation condition ($M = 3.07, SE = 0.15, 95\% CI = [2.77, 3.37], p = .004$) and in the analogy condition ($M = 3.21, SE = 0.15, 95\% CI = [2.92, 3.49], p < .001$) judged ingroup harmdoing as more wrong. There was no significant difference between those in the analogy and joint-evaluation conditions (Figure 4). Next, we found a significant effect of condition on group-based critical emotions toward ingroup harmdoing, $F(2, 310) = 6.77, p = .001, \eta_p^2 = .042$. Compared with those in the single-evaluation condition ($M = 1.63, SE = 0.13, 95\% CI = [1.37, 1.88]$), those in the joint-evaluation condition felt stronger group-based critical emotions ($M = 2.10, SE = 0.13, 95\% CI = [1.84, 2.35]$,

$p = .011$), as did those in the analogy condition ($M = 2.27$, $SE = 0.13$, 95% CI = [2.02, 2.51], $p < .001$). There was no difference in group-based critical emotions, between those in the analogy and joint-evaluation conditions (Figure 4). Consistent with the pattern of results so far, these results show that engaging with the removed-harmdoing case effectively led participants to judge ingroup harmdoing as more wrong. Furthermore, it did not weaken the intervention if participants anticipated the analogy.

Results of Study 4 suggest that a removed scenario is effective in reducing motivated reasoning of ingroup harmdoing if evaluated before or alongside ingroup harmdoing. Participants who anticipated the analogy could have judged removed-harmdoing more leniently and continued to condone ingroup harmdoing, and still remained consistent. Instead, we found that those in the joint-evaluation condition were just as critical of removed-harmdoing as those who evaluated it first. This suggests that the analogy reduces the effect of motivated reasoning on the evaluation of ingroup harmdoing, actually leading people to be more critical of their ingroup's actions, beyond consistency motivation.

Study 5

In Study 5, we tested another important boundary condition for the effectiveness of the intervention, specifically whether evaluating a removed case of harmdoing that depicts a more extreme transgression would be effective in leading participants to acknowledge ingroup harmdoing. On one hand, differences in extremity of the harmdoing might be irrelevant. More than that, evaluating more extreme harmdoing might lead participants to be more critical of the moral transgression and therefore even more critical of similar ingroup harmdoing. This would be consistent with work reviewed earlier showing that after judging a case of harm that is unequivocally wrong, people are more likely to judge a more ambiguous scenario as wrong (Wiegmann et al., 2012). On the other hand, the more extreme case of removed harmdoing might be perceived as less similar to the ingroup-harmdoing case, giving people room to justify ingroup harmdoing while still feeling unbiased. For example, participants could claim that compared with the removed-harmdoing, the case of ingroup harmdoing is minor. This strategy of advantageous comparison, if employed, could render the analogy ineffective (Brown, 2014). To test the effectiveness of first evaluating a more extreme case of harmdoing, we included a condition in which participants first read a (fabricated) article depicting more extreme collective punishment toward relatives of those involved in violent nationalistic crimes.

Method

Participants and procedure. We aimed to recruit 340 participants, based on the same power calculation as in previous studies, via student groups on social media. We ended up recruiting 408 participants, a slightly larger sample than

required as more people completed the study than expected. After dropping 17 participants who failed attention checks, four who were not Jewish, and a participant who completed the survey twice, 386 participants remained.

Participants were randomly assigned to one of the three conditions—single-evaluation, analogy, or extreme. In the extreme condition participants first read about a father and his sons being disappeared and possibly murdered for the crimes of their relatives (full text is presented in the Supplemental Material). This article was the same as that read first in the analogy condition, except that the harmdoing was more extreme (imprisonment in the analogy and being disappeared and possibly murdered in the extreme). We piloted the material before running the study to verify that the removed-extreme case was perceived as more extreme than the removed case used in the analogy condition here and in previous studies. Using snowball sampling, we recruited 16 participants. Each participant evaluated both articles (the order counterbalanced), and to evaluate perceptions of extremity, they rated how fair, radical, and serious each case of harmdoing was, and how much they thought the victims suffered. We found that participants judged the extreme scenario as more extreme ($M = 5.11$, $SD = 0.93$) than the original scenarios ($M = 4.80$, $SD = 0.96$), $t(15) = 2.37$, $p = .03$.

Measures. Participants in the analogy and extreme conditions rated *wrongness of removed-harmdoing* ($\alpha = .62$) and *empathic emotions toward removed victims* ($\alpha = .90$).

Manipulation check. To check whether the extreme removed scenario was perceived as more extreme than the original removed scenario, participants in the analogy and extreme conditions responded to the same four items as used in the pilot study ($\alpha = .77$).

Perceived similarity. To assess whether there were differences in perceived similarity between the two cases of harmdoing evaluated in the analogy condition and the two cases evaluated in the extreme condition, participants answered the following questions: “To what extent, do you think the two articles you read were similar?”; “To what extent do you think the two texts that you read were totally different?” (reversed coded; $\alpha = .89$).

Main measures. As in previous studies, we measured *wrongness of ingroup harmdoing* ($\alpha = .91$) and *group-based critical emotions*.⁵

Results and Discussion

Initial analysis. Table S8 in the Supplemental Material contains correlations per condition and Table S9 in the Supplemental Material contains correlations across conditions. We conducted a one-way ANCOVA and found a significant difference between the analogy and extreme conditions on the manipulation check, $F(1,220) = 6.31$, $p = .013$, $\eta_p^2 = .03$,

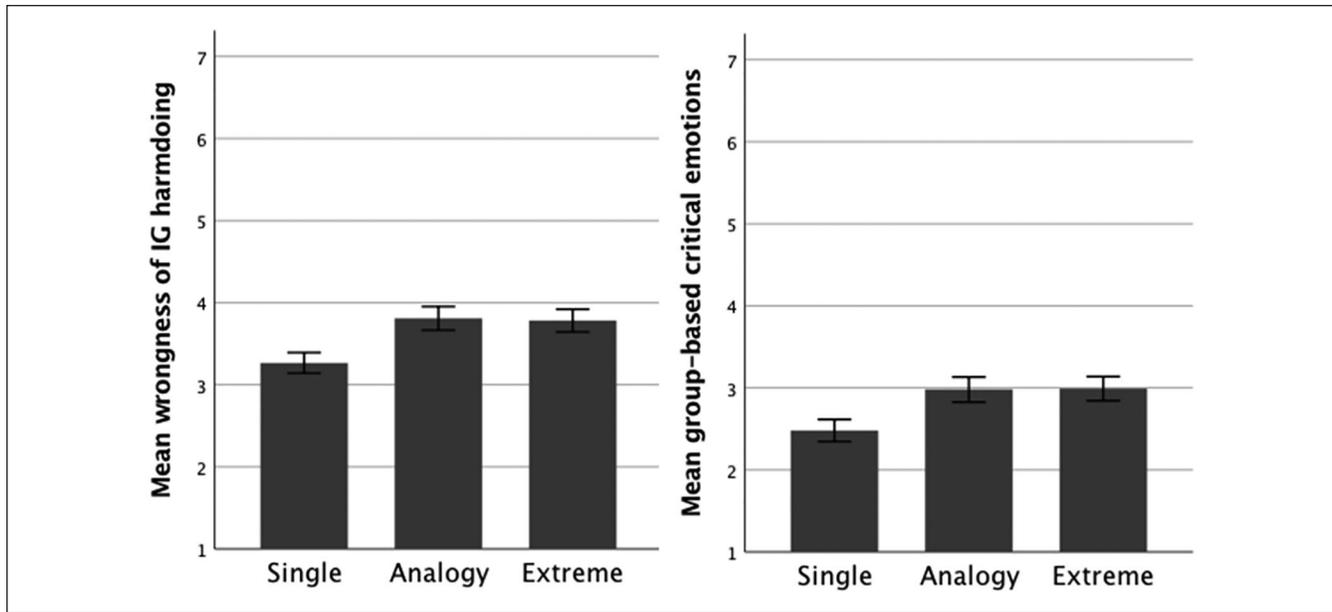


Figure 5. Effect of condition on perceived wrongness of ingroup harmdoing and on group-based critical emotions, controlling for political ideology, in Study 5.

Note. Error bars represent $\pm SE$ of the mean.

suggesting that the removed scenario in the extreme condition was indeed perceived as more extreme ($M = 6.46$, $SE = 0.08$, 95% CI = [6.31, 6.61]) than that in the analogy condition ($M = 6.19$, $SE = 0.08$, 95% CI = [6.03, 6.34]). However, this effect was driven by one item—participants in the extreme condition viewed the case of harmdoing that they read about as more extreme ($M = 6.36$) than those in the analogy condition ($M = 5.86$, $p = .001$), but they did not see it as less fair or as a less serious moral transgression. We next checked whether the extreme case of removed-harmdoing was viewed as less similar to ingroup harmdoing, compared with the original case of removed-harmdoing. We found a marginal effect of condition $F(1,220) = 3.60$, $p = .059$, $\eta_p^2 = .016$, such that those in the extreme condition viewed the extreme removed-harmdoing as slightly less similar to ingroup harmdoing ($M = 4.02$, $SE = 0.16$, 95% CI = [3.71, 4.32]), compared with those in the analogy condition who viewed the original case of removed-harmdoing as more similar to ingroup harmdoing ($M = 4.44$, $SE = 0.16$, 95% CI = [4.13, 4.76], $p = .059$).

Main analysis. A one-way ANCOVA revealed a significant effect of condition on perceived wrongness of ingroup harmdoing, $F(2,359) = 5.43$, $p = .005$, $\eta_p^2 = .029$. As in previous studies, compared with those in the single-evaluation condition, participants in the analogy condition judged ingroup harmdoing as more wrong ($M = 3.81$, $SE = 0.14$, 95% CI = [3.53, 4.09], $p = .005$). Those in the extreme condition also judged ingroup harmdoing as more wrong ($M = 3.78$, $SE = 0.14$, 95% CI = [3.51, 4.05]), compared with those in the single-evaluation condition ($M = 3.27$, $SE = 0.13$, 95% CI = [3.02, 3.51], $p = .006$). There was no

difference between the analogy and extreme conditions (Figure 5). Next, we found a significant effect of condition on group-based critical emotions, $F(2,359) = 4.34$, $p = .014$, $\eta_p^2 = .024$. Participants in the analogy condition felt stronger group-based critical emotions ($M = 2.98$, $SE = 0.15$, 95% CI = [2.68, 3.28]) compared with those in the single-evaluation condition ($p = .015$). Those in the extreme condition also felt stronger group-based critical emotions ($M = 2.99$, $SE = 0.15$, 95% CI = [2.70, 3.28]), compared with those in the single-evaluation condition ($M = 2.48$, $SE = 0.13$, 95% CI = [2.22, 2.74], $p = .011$). There were no significant differences between the analogy and extreme conditions (Figure 5). These effects were not moderated by political ideology or any of the moderators or demographic variables measured.

These results provide additional support for the effectiveness of the analogy intervention. They additionally suggest that engaging with a removed case of harmdoing that is more extreme, but depicts a moral transgression in the same domain (i.e., collective punishment), can increase acknowledgment of ingroup harmdoing. The scenario evaluated in the extreme condition was perceived as more extreme than that evaluated in the analogy condition, but was not perceived as significantly different in moral respects. As the extreme removed scenario was only perceived as slightly different from the standard removed scenario, future research should explore this question further, as we cannot rule out the possibility that a similar, but even more extreme, case of harmdoing evaluated first would either be ineffective or lead to backfire effects. However, results of this study provide initial evidence that analogies of harmdoing

may have limited sensitivity to differences in extremity between the harmdoing depicted.

Study 6

In Study 6, we directly tested the mechanism of the analogy. We hypothesized the following analogical reasoning process: Evaluating harmdoing in a removed context leads to stronger endorsement of moral principles against the harmdoing depicted, guiding subsequent evaluations of similar ingroup harmdoing. This should lead to more objective reasoning and result in more critical evaluations of similar cases of ingroup harmdoing. Specifically, we tested whether considering collective punishment toward relatives of violent extremists in Sri Lanka (vs. not considering this case) would lead participants to judge a similar case of ingroup-committed collective punishment toward outgroup relatives of a terrorist as more wrong, via stronger endorsement of moral principles against collective punishment of family members. We also tested whether the analogy shapes wrongness judgments of ingroup harmdoing via endorsement of general moral principles against injustice,⁶ rather than only via endorsement of principles in the same domain as the harmdoing first depicted (i.e., collective punishment of relatives). We also tested whether increased empathy with outgroup victims is a possible mechanism. In Study 2, we showed that engaging with an empathy evoking scenario was not *sufficient* to increase acknowledgment of ingroup harmdoing. However, this did not rule out the possibility that the analogy increased empathy with the outgroup victims, contributing to the observed effects, and therefore, we tested this directly.

An additional goal of this study was to rule out that the effect of the analogy is due to anchoring. Anchoring occurs when a judgment is influenced by a previously considered and unrelated number that sets an “anchor” value, which is then used as a guide for subsequent evaluations (Tversky & Kahneman, 1974). Although our response scale did not contain numerical values, participants were presented with the same questions and response options in a scale format for both cases of harmdoing. Evaluating removed-harmdoing could have thus anchored subsequent responses. To rule out an anchoring effect, we tested whether the effects hold when only contemplating removed-harmdoing, without evaluating it on a scale.

Our final goal was to test whether perceiving the two cases of harmdoing as similar is a moderator. Theories of analogical reasoning suggest that similarity increases the transfer of principles from a source to a target case (Gentner & Landers, 1985). We have so far tested the impact of domain similarity between cases (Study 3) and similarity in magnitude (Study 5), but not whether individuals who perceive the cases of harmdoing as more similar are more affected by the analogy. We hypothesized that the greater the perceived similarity of the cases, the more impactful the analogy.

Method

Participants and procedure. We recruited 529 Jewish-Israeli participants via a survey company. As we hypothesized an interaction, we calculated our required sample with a smaller effect size compared with previous studies, $d = .025$, an α of .05 and power of .80. This showed that 505 participants were required. We dropped 43 participants who failed attention checks and another nine who completed the entire survey in less than 3 min.⁷ This screening was consistent with that in previous studies, where no participants completed the study in such a short time. There remained 477 participants.

Participants were randomly assigned to one of the three conditions—single-evaluation,⁸ analogy, or a contemplating condition—in which they were presented with the case of removed-harmdoing prior to evaluating ingroup harmdoing but were asked only to contemplate the case of removed-harmdoing and did not see or respond to any items about it. We used the same manipulation materials depicting removed and ingroup harmdoing, as in previous studies.

Measures. Participants in the single-evaluation and analogy conditions rated *wrongness of removed-harmdoing* ($\alpha = .92$) and *empathic emotions toward removed victims* ($\alpha = .92$). Participants in the contemplating condition were instead presented with these instructions:

Think about the article that you just read. What do you think about what happened to Myo and his sons? What feelings did reading this article elicit? Please consider this case from a moral perspective. After 20 seconds you will be able to continue to the next page.

Across all conditions, we measured *wrongness of ingroup harmdoing* ($\alpha = .91$), *group-based critical emotions* ($\alpha = .87$), and endorsement of *general moral principles* ($\alpha = .91$) with the same items as in previous studies.

To test endorsement of *domain-specific moral principles* as a mechanism, participants were asked to consider their moral principles and indicate their agreement with the statements, “It is morally wrong to punish family members for crimes of their relatives that they were not involved in” and “It is unfair to punish family members for crimes of their relatives which they did not support” ($\alpha = .92$). We considered that simply asking participants about their principles may affect responses to ingroup harmdoing beyond the effect of the analogy. We therefore measured endorsement of principles after evaluation of ingroup harmdoing. We also included a measure of *empathy toward the outgroup victims* ($\alpha = .92$), in which participants rated the extent of empathy and compassion they felt toward the Allyan family ($\alpha = .92$). We included a measure of *perceived similarity* ($\alpha = .88$) between the cases of harmdoing, as in Study 5, at the end of the study.

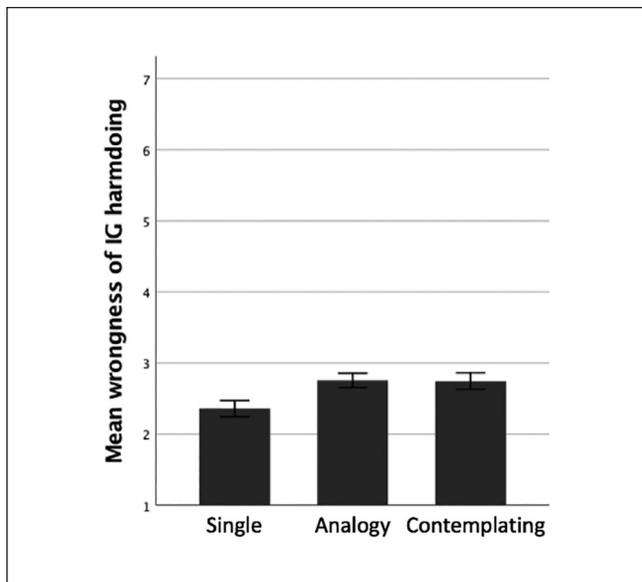


Figure 6. Effect of condition on perceived wrongness of ingroup harming, controlling for political ideology and perceived similarity, in Study 6.

Note. Error bars represent $\pm SE$ of the mean.

Results and Discussion

Main analysis. Table S10 in the Supplemental Material contains correlations per condition and Table S11 in the Supplemental Material contains correlations across conditions. As well as controlling for political ideology across all analysis, we also controlled for perceived similarity, as there were marginal differences between conditions. Those in the analogy condition perceived the two cases of harming as less similar than those in the contemplating condition ($p = .061$). We tested whether perceived similarity moderated the effect of the analogy but found that it did not ($p = .833$).

We then tested the effect of condition on evaluations of ingroup harming. A one-way ANCOVA revealed an effect of condition on wrongness of ingroup harming, $F(2,469) = 4.06$, $p = .018$, $\eta_p^2 = .017$. As in previous studies, compared with those in the single-evaluation condition ($M = 2.36$, $SE = 0.12$, 95% CI = [2.13, 2.58]), participants in the analogy condition judged ingroup harming as more wrong ($M = 2.76$, $SE = 0.10$, 95% CI = [2.56, 2.96], $p = .010$), as did those in the contemplating condition ($M = 2.75$, $SE = 0.12$, 95% CI = [2.52, 2.98], $p = .018$).⁹ As expected, there was no significant difference between the analogy and contemplating conditions ($p = .95$; Figure 6). Contrary to our hypothesis and results in most of the previous studies, there was no significant effect of condition on group-based critical emotions, $F(2,469) = 0.307$, $p = .736$, $\eta_p^2 = .001$.

We then tested the effect of condition on potential mechanisms. First, we tested the effect of condition on domain-specific moral principles against collective punishment, and found a significant effect, $F(2, 469) = 5.93$, $p = .003$,

$\eta_p^2 = .025$. Compared with those in the single-evaluation condition ($M = 3.79$, $SE = 0.14$, 95% CI = [3.51, 4.07]), those in the analogy condition endorsed domain-specific moral principles more ($M = 4.43$, $SE = 0.13$, 95% CI = [4.18, 4.68], $p = .001$), as did those in the contemplating condition ($M = 4.27$, $SE = 0.14$, 95% CI = [3.99, 4.55], $p = .018$). There was no significant difference between the analogy and contemplating conditions on domain-specific moral principles ($p = .41$). We next tested the effect of condition on general moral principles and found no significant effect ($p = .20$), ruling this out as a candidate for mediation. Finally, we tested whether evaluating removed-harming increased empathy toward the Allyn family members and found no such effect ($p = .771$), ruling out empathy as a mechanism.

Next, we tested whether endorsing domain-specific moral principles mediated the effect of analogies on perceived wrongness of ingroup harming using Hayes's (2013) PROCESS bootstrapping command with 5,000 iterations (model 4). We collapsed the analogy and contemplating conditions together, as there was no significant difference between these conditions and both differed significantly from the single-evaluation condition. We found that the addition of domain-specific principles in the model reduced the relative total effect of the analogy (vs. single evaluation) on perceived wrongness of ingroup harming, and the indirect effect through domain-specific moral principles (effect = .20, $SE = 0.06$, 95% CI = [0.08, 0.32]) was significant. The model (Figure 7) confirms our hypothesis that considering a case of removed harming, leads to greater endorsement of domain-specific moral principles, in turn leading to increased condemnation of similar ingroup harming that violates those principles.

In Study 6, we identified a mechanism for the effect of analogies. Our results demonstrated that the analogy operates via a cognitive process whereby the removed scenario encourages endorsement of moral principles specifically against the moral violation described, which are then applied to the evaluation of the similar case of ingroup harming. Our findings also showed that the analogy was not effective due to the endorsement of general moral principles, but rather only endorsement of principles specifically related to the type of harming depicted. We also found that contemplating removed-harming, without responding to any items regarding it, was equally effective in leading participants to judge ingroup harming as more wrong, ruling out an anchoring bias.

While the effect of the analogy on moral judgment was replicated, we again did not observe an effect on group-based critical emotions, suggesting that the analogy increases cognitive acknowledgment of ingroup harming more consistently than emotional acknowledgment. Finally, we found that perceived similarity was not a moderator, suggesting that the effect does not rely on conscious perception of similarity. Previous research has shown that analogical inference from a source to a target case can occur implicitly, outside of

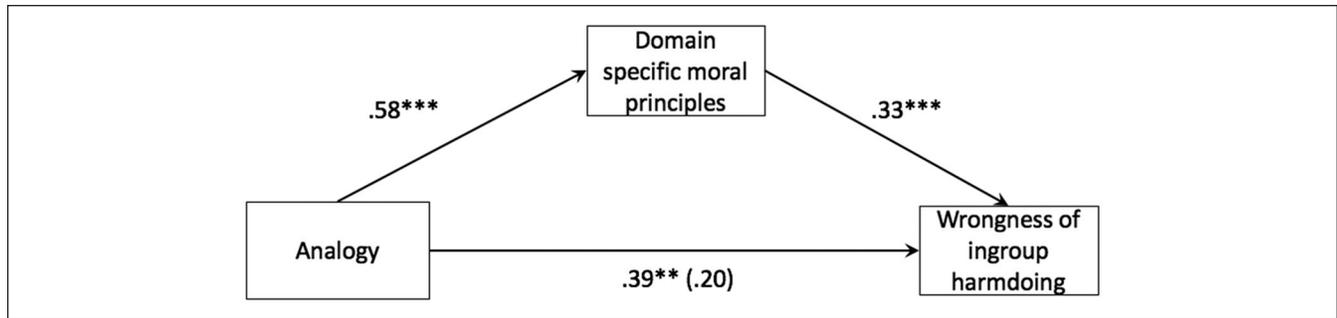


Figure 7. Model testing the effect of condition (analogy vs. no analogy) on perceived wrongness of ingroup harmdoing through endorsement of domain-specific moral principles against collective punishment, for Study 6.

Note. Unstandardized coefficients displayed.

* $p < .05$. ** $p < .01$. *** $p < .001$. (two-tailed tests)

conscious awareness (Day & Gentner, 2003; Gross & Greene, 2007), as may be the case here.

General Discussion

Confronting people with transgressions committed by their group in the context of intergroup conflict is often met with resistance, as group members strive to combat the threat this poses to their moral image (Bandura, 1999). Ingroup members commonly justify ingroup transgressions and avoid corresponding feelings of guilt and shame (Leach et al., 2013). We examined whether analogies could be used to reduce the effect of biased processing on evaluations of ingroup harmdoing in the context of intractable conflict. Specifically, we developed and tested the effectiveness of an intervention to increase acknowledgment of an ingroup transgression by evaluating an analogous case of harmdoing in a removed context, where there is no motivation to justify the conduct. We found, across six studies, that considering a case of removed-harmdoing led participants to perceive ingroup harmdoing as more morally wrong. In four of these studies, group-based critical emotions toward the ingroup transgression were stronger among those who were exposed to the analogy. These results provide converging evidence for the effectiveness of analogy in increasing acknowledgment of ingroup harmdoing in the context of intergroup conflict.

After initially testing the intervention (Study 1), we explored boundary conditions and mechanisms for the effect. We proceeded to show that this effect is not solely due to empathy arousal, increased endorsement of general moral principles, or moral anger (Studies 2, 3, and 6). We additionally ruled out two important factors as boundary conditions, showing that the analogy held even when anticipated (Study 4) and when the removed case of harmdoing depicted a more extreme transgression, albeit it was perceived as only slightly more extreme (Study 5). Finally, we tested the mechanism and showed that the analogy led to increased endorsement of moral principles against collective punishment of family members, in turn leading to increased condemnation of

ingroup harmdoing. We also ruled out that the effect was due to numerical anchoring (Study 6).

These findings lay initial foundations for mapping the psychological processes involved in analogy-based interventions. They build on previous work demonstrating that the effect of motivated reasoning can be constrained when making judgments sequentially or alongside another case (e.g., Bruneau et al., 2018, 2019; Paharia et al., 2013). However, to the best of our knowledge, this is the first time that an analogy-based intervention has been shown to increase acknowledgment of ingroup harmdoing in intergroup or intractable conflict. We tested our main hypothesis several times and with different samples and found the same pattern of results among participants recruited via an online survey company and via student groups on social media (Study 5). Our findings are particularly noteworthy given that in the context of intractable conflict, individuals are particularly ready to defend the ingroup (Bar-Tal, 2013), conflict-related attitudes are extremely rigid and difficult to change (Halperin & Bar-Tal, 2011), and attempts to change attitudes are commonly met with resistance (Saguy et al., 2013). In such a context, there is arguably also less risk that results are due to demand characteristics.

A risk of analogy-based interventions is that they may lead to defensiveness. Drawing an analogy between ingroup actions and conduct that is generally perceived as immoral could be threatening for ingroup members and entrench existing attitudes. Perhaps surprisingly, the interventions we tested did not backfire, even when the analogy was anticipated or when the removed case of harmdoing was more extreme. Furthermore, we ran these studies over the course of 1 year, thus at times of varying intensity of the conflict, and were able to replicate results even during violent escalation. In each study, we tested several individual difference moderators, but none moderated the effect, suggesting the robustness of the intervention.

We found that our intervention promotes more objective reasoning, as principles endorsed after considering a removed case where one is impartial then inform evaluation of ingroup conduct, which is normally biased. The mere desire to be

consistent is unlikely to be the driving force behind the effect, for two reasons. First, consistency motivation alone cannot explain why the participants in Study 4, who saw both cases of harmdoing side-by-side, and had the opportunity to condone both scenarios (while remaining consistent), chose instead to be as critical of ingroup harmdoing as those who first evaluated removed-harmdoing. Second, those with a stronger preference for consistency were not more affected by the manipulation (Studies 2 and 3). Taken together, this suggests that considering the case of removed-harmdoing changes one's way of thinking about ingroup harmdoing beyond consistency motivation, and that it actually leads to less motivated evaluation of the ingroup transgression.

Some questions remain as to how similar the source and target case must be in order for the analogy to be effective. In this work, we test the importance of similarity in domain (Study 3), in magnitude (Study 5), and perceived similarity between cases as a moderator (Study 6). We found that perceived similarity did not moderate the effect of the analogy, suggesting that the analogy may work beyond a conscious perception of the cases being similar. While evaluating whether a more extreme removed scenario affected wrongness judgments of ingroup harmdoing, it was seen as only slightly more extreme than the less extreme removed scenario, and it was not viewed as less moral. Thus, future work should investigate how sensitive the intervention may be to larger differences in extremity between the source and target cases. Finally, our results indicate that the analogy works best when the cases are in the same domain (Study 3). This is in line with the mechanism that we identified, namely, that the analogy is effective via stronger endorsement of principles, specifically regarding the domain of the harmdoing depicted.

Future work should continue to test the boundary conditions for the effect of analogies in this context. In our studies, we drew analogies between harmdoing in a *removed* context and ingroup harmdoing. It is possible that an analogy between conduct of the *enemy* group and that of the ingroup would raise defenses and produce a backlash effect, as it may serve as a reminder of victimhood (Wohl & Branscombe, 2008) or lead people to be particularly motivated to justify ingroup conduct to demonstrate their group's moral superiority. It is also possible that the analogy could have led to defensiveness if the comparison was more explicit, for example, if participants were asked to list similarities between the two cases of harmdoing. In addition, we focused on a type of harmdoing that is an official government policy and is therefore difficult to deny or dismiss as the acts of individuals. It remains to be tested whether analogy-based interventions could increase acknowledgment of types of harmdoing that governments rarely admit to, such as torture, where denial may be more prevalent. In our work, we found that analogies shape moral judgments of the particular case of ingroup harmdoing and associated group-based critical emotions. Future work should also test whether the effects of analogies on judgments of ingroup harmdoing could extend to additional downstream

outcomes, such as support for apologies and reparations. Finally, work is needed to establish whether the effects of analogy-based interventions persist across time.

Final Thoughts

Acknowledging ingroup harmdoing is pivotal in reducing ingroup transgressions and ultimately for intergroup reconciliation processes. It is therefore a key social goal for human rights and peace activists and educators. Analogy-based interventions have the potential not only to expand our theoretical understanding of conflict-related psychological processes and attitude change, but to assist practitioners who contend with the challenge of the many ingenious ways in which people avoid and disengage from their group's transgressions.

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Supplemental Material

Supplemental material is available online with this article.

Notes

1. One unpublished field study (Lustig, 2002, cited by Solomon, 2004) indicated that exposure to an analogous conflict can improve aspects of intergroup relations among those involved in a different intractable conflict. Despite a small sample and mixed results, these findings imply that analogies may be beneficial in this context, but further research is warranted.
2. A list of moderators included in all studies for exploratory purposes is presented in the Supplemental Material.
3. In Studies 2, 3, and 4, we added one reverse-coded item to the measure of wrongness of ingroup harmdoing. Reliability always improved with this item deleted, and therefore, we did not include it in the final measure. We find a similar pattern of results across studies with this item included.
4. An additional goal of this study was to test whether after evaluating ingroup harmdoing as acceptable, as participants did in studies so far, participants would then judge removed-harmdoing as more acceptable. To test this, participants in the single-evaluation condition evaluated the removed-harmdoing case *after* the ingroup-harmdoing case. This would provide us with

an indication of the impact of the removed scenario if evaluated second (see the Supplemental Material for results and discussion regarding this).

5. In this study alone, due to human error, guilt toward ingroup harmdoing was not measured.
6. Endorsement of general moral principles was not found to be a mediator in Study 3. We tested this again to gain further clarification regarding endorsement of general versus domain-specific principles as a mechanism.
7. The pattern of results remains the same and significant effects hold with these nine participants included in the analyses.
8. In the single-evaluation condition, participants first evaluated ingroup harmdoing and then removed-harmdoing so that perceived similarity could be measured across all conditions. See the Supplemental Material for results for the effect of condition on judgments of removed-harmdoing.
9. Without controlling for perceived similarity, we found the same pattern of results for the effect of analogies on judgment of ingroup harmdoing; however, the difference between the single-evaluation condition and analogy condition became marginal ($p = .076$), whereas the difference between the single-evaluation condition and contemplating condition remained significant ($p = .021$).

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