

The Effects of Moral and Pragmatic Arguments Against Torture on Demands for Judicial Reform

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Torture can be opposed on the basis of pragmatic (e.g., torture does not work) or moral arguments (e.g., torture violates human rights). Three studies investigated how these arguments affect U.S. citizens' attitudes toward U.S.-committed torture. In Study 1, participants expressed stronger demands for redressing the injustice of torture when presented with moral rather than pragmatic or no arguments against torture. Study 2 replicated this finding with an extended justice measure and also showed the moderating role of ingroup glorification and attachment. Moral arguments increased justice demands among those who typically react most defensively to ingroup-committed wrongdoings: the highly attached and glorifying. Study 3 showed that the effect of moral arguments against torture on justice demands and support for torture among high glorifiers is mediated by moral outrage and empathy but not guilt.

KEY WORDS: torture, morality, judgment, outrage, empathy, justice, ingroup-committed violence

Following the release of the U.S. Senate Select Committee on Intelligence's study of the CIA's detention and interrogation program in 2014 (Senate Select Committee on Intelligence, 2014), in 2015 the U.S. Senate and the American Psychological Association restated their opposition to torture. Despite this institutional opposition to torture and its prohibition by international declarations and conventions, public opinion still supports torture to a large extent. A poll found that in December 2014, Americans supported torture by a margin of almost 2 to 1 (59% support vs. 31% opposition, *Washington Post/ABC News*, 2015). Similar numbers have emerged in other countries, such as France, the United Kingdom, or South Korea (Lester, 2005). Even more striking, since the Abu Ghraib scandal broke in 2004 (Hersh, 2004) and despite official condemnations of the policies under the Bush and Obama administrations, Americans' support for torture has *increased* and opposition to it has *decreased* (Pew Research Center, 2014).

The public's consistent support for torture appears to be multiply determined by interlinked factors. One prominent factor is that when torture is perpetrated by their own (rather than another)

country, people are motivated to protect their image of their country, which often leads them to use moral disengagement strategies such as dehumanization or blaming the victims (Leidner, Castano, Zaiser, & Giner-Sorolla, 2010; Viki, Osgood, & Phillips, 2013; see also Kelman, 2005). Consequently, people can experience reduced empathy for torture victims and come to see torture not only as *not immoral* but even as *moral* (Tarrant, Branscombe, Warner, & Weston, 2012). This perception of torture as not immoral directly affects its perceived costs and benefits. In a sample of more than 1,800 Americans, stronger beliefs that “forceful interrogation of terrorist suspects” is *not* wrong were associated with stronger beliefs in its effectiveness (Liu & Ditto, 2013, Study 2).

Together, these psychological processes explain why the belief that torture is effective is rather widespread and popular, if erroneous (Janoff-Bulman, 2007; see also Costanzo & Gerrity, 2009). Beliefs in the effectiveness and morality of torture can further explain the longevity of the public’s support for torture. Importantly and problematically, in a representative sample of Americans both beliefs were stronger when people thought that torture is a long-standing (rather than recent) practice (Crandall, Eidelman, Skitka, & Morgan, 2009). Similarly, and similarly problematically, people’s support for torture leads them to see information obtained through torture as more valuable, in turn strengthening their support for torture (Ames & Lee, 2015).

While the research reviewed above furthered our understanding of the public’s support for torture, rigorous empirical research on arguments that may be successful in *eroding* this support is lacking. Thus, in three experiments we investigated the effects of different arguments against torture on support for torture among people with varying degrees of attachment to and glorification of their country. This research contributes to an emerging knowledge base on attitudes toward torture in particular (Ames & Lee, 2015; Liu & Ditto, 2013; Tarrant et al., 2012; Viki et al., 2013) and on ways to develop a culture of peace and respect for human rights in general (Di Lellio & Castano, 2016; Kelman, 2010, 2012; Leidner & Li, 2015; Leidner, Tropp, & Lickel, 2013; de Rivera, Kurrien, & Olsen, 2007). While the studies were conducted in the context of attitudes toward torture, they also speak to our understanding of public opinion more generally and how public opinion can shift as a result of communication (i.e., moral and pragmatic arguments about a societal issue) and identity (i.e., attachment and glorification). In this sense, the present research also takes up the challenge to bridge social psychological research on peace and conflict with mainstream social psychology (see Kelman, 2012; Leidner et al., 2013).

Moral Versus Pragmatic Arguments Against Torture

Arguments against torture featuring in political and societal discourse fall into two main categories. One is pragmatic, stating that torture can lead to mistreatment of or retaliation against “our” soldiers when they are captured by “the enemy” (Costanzo, Gerrity, & Lykes, 2006). The other is moral, stating that torture violates the constitution, human rights, humanitarian law, and ethical values and ideals (Malinowski, 2008). The pragmatic argument does not oppose torture because torture violates universal moral standards or the victims’ rights. Rather, it does so because any possible benefits of torture are not seen as outweighing its costs. These costs include the risk of retaliation against members of the group that perpetrates torture, the risk of torture producing unreliable information, loss of the group’s reputation and credibility in the eyes of its own members and/or outsiders, and the mental and emotional toll on torturers or members of their group (Costanzo & Gerrity, 2009). Regardless of which cost is emphasized by different pragmatic arguments, what unites them is the view that torture should be opposed because it ultimately “does not work”—meaning, it is not useful to “us” (i.e., the *ingroup*). As such, pragmatic arguments may reduce support for torture because they draw attention to the costs or repercussions torture can have for the perpetrator group.

Moral arguments, on the one hand, oppose torture on the basis of deontological morality and ethics, rather than utilitarian-consequentialist morality and cost-benefit analyses. Although it could be

argued that moral arguments entail utilitarian-consequentialist elements and/or self-serving motives, too, they arguably do so less than pragmatic arguments. For instance, a person could use moral arguments out of a motivation to protect or reassert ingroup norms and values, and in this sense endorse moral arguments on utilitarian grounds. Yet, pragmatic arguments can *only* be viewed through a utilitarian-consequentialist lens, whereas moral arguments may *partially* be viewed through a utilitarian-consequentialist lens but always *also* through a deontological lens.

Which Type of Argument Will Be More Effective?

Many factors—from cognitive accessibility to affect to values and individual differences—influence the effectiveness of arguments in changing attitudes or opinion (for a review, see Petty, Wegener, & Fabrigar, 1997). Thus, moral and pragmatic arguments against torture are generally equally likely to change attitudes toward torture. Pragmatic arguments, by definition, do not draw attention to an internalized moral standard that was violated. They do, however, draw attention to benefits (or cost-avoidance) for the ingroup. A large body of research on group processes suggests that people want to provide benefits and reduce costs for the ingroup, especially in intergroup situations (e.g., Brewer, 1979). Thus, pragmatic arguments opposing the idea that torture is effective and therefore does not benefit the ingroup might lead to stronger opposition to torture. Yet, recent research shows that people's belief in the effectiveness of torture *decreases* the more they believe torture is inherently immoral (Liu & Ditto, 2013). This suggests that people's belief in the effectiveness of torture does not only inform attitudes towards torture; attitudes towards torture inform people's belief in its effectiveness (see also Ames & Lee, 2015). In other words, people do not adjust their attitudes in line with torture's purported effectiveness as much as they adjust their beliefs in its effectiveness in line with their (pre-existing) attitudes. In this way, then, preexisting support for torture and beliefs in its effectiveness might actually inoculate people against new, pragmatic arguments about the *ineffectiveness* of torture. If this is true, pragmatic arguments against torture—questioning torture's effectiveness—should have limited effect.

Moral arguments against torture, on the other hand, may be more effective in reducing people's support for torture. Making salient the violation of an internalized moral standard, moral arguments against torture should motivate people to demonstrate the ingroup's virtue and morality through opposing torture more strongly (e.g., Batson, Chao, & Givens, 2009; Mullen & Skitka, 2006), demanding to meet and reaffirm moral standards by redressing past and preventing future injustice (e.g., Darley & Pittman, 2003). This hypothesis is in line with research showing that attitudes such as people's support for democracy are related to perceptions of the attitude object (e.g., democracy) as fulfilling a value-expressive rather than ego-defensive function (Gastil, 1992). Further, a wide variety of social psychological literature—on ingroup-committed violence, moral convictions, ingroup dissent, and collective action—shows that people at times oppose their group's group-level behavior when this behavior clashes with internally held values or convictions (for reviews in the different literatures, see Packer, 2008; Skitka & Mullen, 2002; van Zomeren, Postmes, & Spears, 2008).

Such "attitudinal (and sometimes actual/behavioral) rebellion" (Hornsey, 2005) against the ingroup is driven by emotional responses to ingroup-committed transgressions (for a review, see Thomas, McGarty, & Mavor, 2009). For instance, anger or outrage directed at the ingroup, as well as group-based guilt, motivate people to right ingroup-committed wrongs (Doosje, Branscombe, Spears, & Manstead, 1998; Iyer, Schmader, & Lickel, 2007). Similarly, empathy motivates people to end others' suffering, including that of outgroup members (for a review, see Castano, 2012). It is thus possible, even in situations of ingroup-committed wrongdoing in which people are often motivated to defend the ingroup's violation of generally held moral standards, that moral arguments and their focus on violations of these standards decrease support for torture and increase desire to redress its injustice by enhancing group-level emotional responses such as outgroup-directed empathy, ingroup-directed

anger/outrage, and group-based guilt. In the context of political action intentions, however, group-based guilt might be less predictive than anger/outrage or empathy because guilt is more self-reflective (as opposed to other-directed) than empathy and more passive (as opposed to action-oriented) than anger/outrage (Iyer et al., 2007; Waksack, Jost, Tyler, & Chen, 2007). Consistent with this view of the effects of emotional responses to ingroup-committed transgressions on political action intentions, group-based guilt did not predict political action intentions after controlling for anger or other variables (Iyer et al., 2007). Nevertheless, when investigating the emotional processes underlying the effects of arguments against torture on people's opinion on their group's use of torture (Study 3), we included group-based guilt alongside ingroup-directed anger/outrage and outgroup-directed empathy in order to provide a comprehensive test of these processes.

Which Type of People Will Be More Convinced?

Moral arguments critical of the group's actions may be even more powerful to the extent that people are psychologically invested in their group's moral image. This hypothesis is in line with research demonstrating that information processing and attitude change are likely biased when the information is personally relevant and people have a vested interest in it (Petty & Cacioppo, 1979). More specific to group processes, our hypothesis is also supported by findings that morality is a crucial dimension on which the ingroup is evaluated (Leach, Ellemers, & Barreto, 2007) and that people who strongly identify with their group can at times be *more* (rather than less) likely to challenge their own group in an effort to save it from "morally faltering" (for reviews, see Packer, 2008; van Zomeren et al., 2008). From this perspective, yielding to moral arguments against torture can be part of people's attempts to protect their group and self-image. Similarly, different views of the role that dissonance plays in attitude change (for a review, see Petty et al., 1997) suggest that people adjust their attitudes when they observe a gap between behavior and their attitude/belief system (Cooper & Fazio, 1984; Festinger, 1957) or self-concept (Aronson, 1968; Steele, 1988). Thus, moral arguments against torture might increase opposition to torture, perhaps even more so the more invested people are in their group's moral image.

Again, we reasoned that pragmatic arguments would be less likely than moral arguments to change people's opinion on ingroup-committed torture, even among those strongly identifying with their group. This reasoning was based on Ames and Lee's (2015) finding that preexisting support for torture strengthens belief in its effectiveness. If high identifiers are generally more supportive of their countries' policies (including "enhanced interrogation"), and if this preexisting attitude strengthens the belief that "torture works," then exposure to arguments that "torture does *not* work" may not be very effective.

However, group identification is not unidimensional; it encompasses both attachment with and glorification of the group (Roccas, Klar, & Liviatan, 2006). While attachment refers to commitment to and perceived importance of the group, glorification refers to the belief that the group is superior to other groups (especially in terms of morality) and prescribes deference to group norms and authorities. Thus, glorification has a comparative component; it focuses on ingroup superiority and emphasizes loyalty and obedience to the ingroup. Attachment is not comparative in nature; it is ingroup-focused and allows for a more critical evaluation of the ingroup and its actions. Glorification is related to greater use of exonerating cognitions for and moral disengagement from the wrongdoings of the ingroup, whereas attachment typically shows either no effect or the opposite (Leidner & Castano, 2012; Leidner et al., 2010; Roccas et al., 2006). We thus expected high ingroup glorification, but not attachment, generally to be associated with lower demands to redress the injustice of ingroup-committed torture.

Importantly, however, we also expected glorification to moderate the effects of arguments against torture on people's support for or opposition to torture. If high identifiers are more sensitive to moral

than pragmatic arguments—for the reasons just explained—then this sensitivity should be particularly driven by glorification, as the glorified image of the group is predicated on the group's purported morality. This expected moderating effect should be such that low glorifiers should display rather high demands for justice regardless of whether the argument is moral or pragmatic. High glorifiers, on the other hand, should increase their demands in response to a moral argument in particular (as opposed to one that is pragmatic).

Overview of the Studies

Three experiments conducted in 2009 (Study 1) and June/July and November 2010 (Studies 2 and 3, respectively) investigated the effectiveness of moral (as compared to pragmatic) arguments on people's attitudes towards torture, while also testing possible moderators and mediators. The manipulation used in these experiments was tested in a pilot study. Study 1 tested the effects of moral versus pragmatic arguments against torture, Study 2 the moderation by glorification, and Study 3 the (moderated) mediation by empathy, anger/outrage, and guilt.

PILOT STUDY

From a psychological as well as methodological perspective, it is important that a pilot study test whether the arguments we theorized earlier to be pragmatic were actually seen by people as "pragmatic" and whether the arguments we theorized earlier to be moral were actually seen by people as "moral." We thus presented 42 participants recruited through Amazon Mechanical Turk (MTurk) for monetary compensation with a brief statement that U.S. soldiers had allegedly tortured Iraqi detainees, followed by comments from a U.S. General condemning the soldiers' behavior either pragmatically (e.g., "Torturing prisoners is against the interests of our own military") or morally (e.g., "Torturing prisoners is against the ideals of the United States of America and its military"; see the appendix in the online supporting information). Participants reported, on visual analog scales (1–9), how much they *perceived* the arguments as being rather pragmatic (1) or rather moral (9) in nature; agreed or disagreed with the arguments; liked or disliked the arguments; and found the arguments convincing. The latter three dimensions were assessed to ensure that pragmatic and moral arguments did *not* vary on other important dimensions, but only on the dimension of perceived pragmatism versus morality.

After eliminating four participants (three not born in the United States and one who answered "9" to all questions), we ran analyses on the remaining 38 participants (age: $M = 30.70$, range = 19–56; gender: 16 male, 21 female, one did not indicate gender). Participants saw the moral arguments as more moral ($M = 6.88$) than the pragmatic ones ($M = 3.51$), $F(1, 36) = 16.73$, $p < .001$. To test whether the group mean in each of these two between-subjects conditions differed significantly from the scale midpoint, we ran one-sample t -tests for each condition. Perceived morality of the moral arguments ($M = 6.88$) was significantly higher than the scale midpoint, $t(20) = 3.74$, $p = .001$, and perceived morality of the pragmatic arguments ($M = 3.51$) was significantly lower than the scale midpoint, $t(16) = -2.21$, $p = .042$. Importantly, the arguments only differed in perceived morality, not in agreement, liking, or conviction, $F_s < 0.64$, $p_s > .430$. Furthermore, the difference in perceived morality remained significant even when controlling for agreement, liking, or conviction, $F(1, 33) = 15.10$, $p < .001$. These results strongly supported the validity of our conceptualization and operationalization of moral and pragmatic arguments.

STUDY 1

Study 1 compared the effects of moral and pragmatic arguments against torture to a baseline without arguments. Rather than focusing on attitudes towards torture in the abstract, we focused on it in the concrete, assessing people's demands to redress the injustice of several instances of torture reminiscent of Abu Ghraib.

Method

Participants. We recruited 215 people via the internet website Craigslist. After excluding 14 people for whom the United States was not the ingroup, and 10 people who failed to remember the identity of the soldiers who perpetrated torture, 191 participants remained (age: $M = 35.94$, $SD = 13.41$, range = 18–78; gender: 54 male, 132 female, 5 did not indicate gender; conservatism: $M = 3.35$, $SD = 2.00$, range = 1–9). Participants in the different conditions did not significantly differ in age ($M_s = 35.95$, 35.03 , and 36.70 for moral, pragmatic, and no-argument condition, respectively), $F(2, 186) = 0.24$, $p = .787$, conservatism ($M_s = 3.34$, 3.43 , and 3.16 , respectively), $F(2, 185) = 0.26$, $p = .771$, or gender, $\chi^2(2) = 0.96$, $p = .617$, indicating that participants were demographically comparable across conditions.

Procedure. The study was conducted online. Participants were randomly assigned to read one of three fictitious news articles reporting on four cases of U.S. soldiers torturing Iraqi prisoners in a Baghdad jail. The article was comparable to real events and presented in the layout of online articles of *The New York Times*. Depending on condition, the article concluded with criticism of the ingroup's actions on pragmatic or on moral grounds, with excerpts identical to the ones used in the pilot study. In the third, no-argument condition, the article did not criticize the ingroup's actions.

Measures

Demands for justice. After reading the article, participants answered eight items measuring demands for retributive (e.g., "Independently from any other kind of punishment, these U.S. soldiers should be fired by the army") and restorative justice (e.g., "The families of the victims should receive an apology by the U.S. government"; "The families of the victims should receive financial compensation"), using visual analog scales (1–9; Leidner et al., 2010). The first retributive justice item asked whether the soldiers should be sent to prison, and participants answered "yes" or "no." If participants answered "yes," they were then asked to indicate the recommended sentence on a second item (1 = minimum by law; 9 = maximum by law); if they answered "no" on the first item, they scored zero on this second item. All subsequent items were answered by everyone, regardless of the responses to the first or second item, on visual analog scales.

Manipulation checks. Finally, in open-ended questions, participants were asked "Where did the prisoners come from? (which country)"; "Where did the soldiers come from? (which country)"; and "In which country or city was the prison located the newspaper article was about?"

Results and Discussion

We first factor analyzed all demands for justice items but the first, dichotomous item. Factor analysis indicated that the items loaded on one factor (97% of the variance explained; $\alpha = .85$). We thus averaged them into a composite score; before doing so, however, we had to standardize the items due to the different scale for the second item.¹ The resulting score was used as a dependent variable

¹ The results reported below did not change when only using the six items that shared the same scale (and thus did not necessitate standardization when being used without item No. 2).

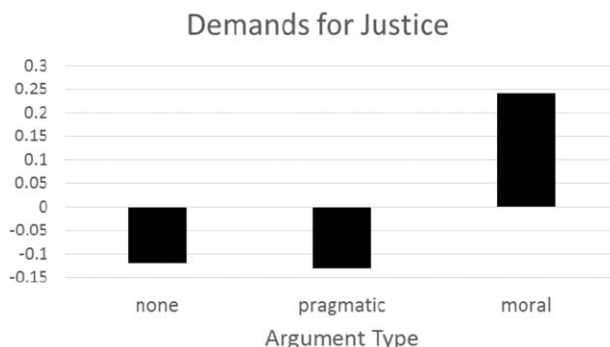


Figure 1. Demands for justice as a function of argument type (none vs. pragmatic vs. moral) in Study 1.

($M = -.01$, $SD = .75$) in an analysis of variance with the experimental manipulation as the independent variable (IV: no-argument vs. pragmatic vs. moral). We performed all analyses in SAS (Statistical Analysis System), utilizing its general linear model (GLM) procedure for t -tests, analyses of variance as well as moderated regressions. As the GLM procedure outputs F instead of t values (but is equivalent to using a regression procedure with effect coding of the dichotomous variable[s]), we report F s below. The corresponding t values can be determined according to $F = t^2$.

The effect of argument was significant, $F(2, 188) = 5.15$, $p = .007$, $\eta^2 = .05$. Participants in the moral-argument condition ($M = .24$, $SD = .47$) scored significantly higher than participants in the no-argument ($M = -.12$, $SD = .89$), $t(188) = 2.80$, $p = .006$, $d = .04$, and pragmatic-argument conditions ($M = -.13$, $SD = .77$), $t(188) = 2.76$, $p = .006$, $d = .04$, while the latter two did not differ, $t(188) < 0.01$, $p = .947$ (see Figure 1). Additional analyses are reported in the online supplementary information. Altogether, results suggested that moral but not pragmatic arguments against torture change people's opinion on torture.

STUDY 2

In Study 1, it remained unclear whether or not the observed patterns hold among people who strongly identify with their group. This moderating factor was thus tested in Study 2. We predicted that high glorifiers would be particularly swayed by *moral* arguments because morality is the most important domain people focus on when evaluating their group (Leach et al., 2007), and high glorifiers are particularly invested in the moral image of their group (Roccas et al., 2006). Thus, the expected moderation of effects of arguments by glorification should be such that low glorifiers should display rather high demands for justice regardless of (the type of) argument, whereas high glorifiers should increase their demands in response to moral arguments. In other words, the effects of moral arguments found in Study 1 should only emerge among high glorifiers.

In addition to moderating factors, Study 2 also included a more comprehensive scale measuring justice demands: willingness to provide immediate help and relief as well as sustained financial support for victims; the necessity to express remorse and ask for forgiveness—two important dimensions of symbolic compensation that affect conflict resolution positively (Noor, Brown, Gonzalez, Manzi, & Lewis, 2008); and, most importantly, demands for structural and policy reforms to prevent torture from recurring in the future.

Study 2 also differed in terms of sampling, using Amazon's Mechanical Turk (MTurk) instead of Craigslist. MTurk samples are more representative of the U.S. population than standard Internet samples and more diverse than college samples (Buhrmester, Kwang, & Gosling, 2011). Furthermore, MTurk data tends to be of the same quality and reliability as data from traditional samples

(Buhrmester et al., 2011; Paolacci & Chandler, 2014) and replicates effects found in traditional samples, be it in survey designs (Goodman, Cryder, & Cheema, 2013), diary studies (Boynton & Richman, 2014), or bogus partner interactions (Summerville & Chartier, 2013). Importantly, online samples have been used in both correlational and experimental studies in clinical, personality, social, and political psychology (Andover, 2014; Campbell & Kay, 2014; Craemer, 2010; Leidner, 2015; Leidner, Castano, & Ginges, 2013), including research on attitudes (e.g., Adelman, Leidner, Ünal, Nahhas, & Shnabel, 2016; Rovenpor, Leidner, Kardos, & O'Brien, 2016).

Compared to traditional research, however, participants in online research tend to be less attentive to the research material (Goodman et al., 2013). This likely poses problems in research studies like ours, where processing of the information presented in the manipulation (e.g., the alleged newspaper article) is critical for the theorized attitude change and its underlying psychological processes to emerge. We thus recorded the time participants spent processing the information. Since even fast readers cannot read over 600 words per minute without considerable loss of comprehension (Carver, 1985), participants who read at a rate faster than 600 words per minute (i.e., low reading time) were excluded from data analysis. In our research context of attitudes toward torture, the advantages of online samples (heterogeneity, diversity, age range, etc.) outweigh this disadvantage—that is, that some participants may pay less-than-ideal attention to stimulus/manipulation materials—especially given that laboratory studies with college student samples would have more serious disadvantages in this context (e.g., threat to ecological validity, generalizability).

Method

Participants. We recruited 324 participants via MTurk. Seventy-one were eliminated for spending less time reading the article than the minimum for comprehension indicated above. Seven more participants were eliminated for spending a disproportionately high amount of time reading the article (according to univariate outlier analysis; Tabachnick & Fidell, 2007), indicating they were likely interrupted. After dropping another 10 participants who reported not being born in the United States and three who failed manipulation checks (see below; two remembered the victims' nationality incorrectly, one remembered the perpetrators' nationality incorrectly), the final sample comprised 233 participants (age: $M = 31.13$, $SD = 11.58$, range = 18–81; gender: 78 male, 152 female, 3 did not indicate gender; conservatism: $M = 4.29$, $SD = 2.25$, range = 1–9). Participants in the different conditions did not significantly differ in age ($M_s = 31.02, 31.86$, and 30.68 for moral-, pragmatic-, and no-argument condition, respectively), $F(2, 227) = 0.19$, $p = .826$, conservatism ($M_s = 4.18, 4.17$, and 4.19 , respectively), $F(2, 227) = 0.52$, $p = .600$, or gender, $\chi^2(2) = 0.40$, $p = .819$, indicating that participants were demographically comparable across conditions.

The deletion of participants with below-threshold reading time for the manipulation material resulted in the elimination of a hefty 22% of the sample. Inclusion of these participants in our analyses did not produce the same pattern of results reported below. However, this discrepancy is *consistent* with the rationale behind the selection; namely that these participants could not be affected by the manipulation because they did not attend to it and did not adequately process the information for attitude change to occur. Further, an exclusion rate of 22% is within normal range for online studies and deemed necessary to ensure high data quality (Chandler, Mueller, & Paolacci, 2013).

Procedure. Participants were randomly assigned to read one of the three news articles used in Study 1, and they filled out the measures described below, followed by demographics.

Measures. All answers were given on visual analog scales ranging from 1 (*no, absolutely not*) to 9 (*yes, absolutely*). Factor analyses suggested that all scales were unidimensional, as indicated by scree plots and the eigenvalue-greater-than-one criterion, with factor loadings $> .40$, explaining 75% or more of the variance for each scale.

Manipulation checks were the same as in Study 1.

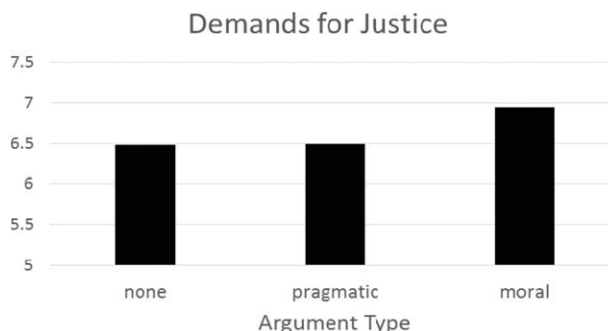


Figure 2. Demands for justice as a function of argument type (none vs. pragmatic vs. moral) in Study 2.

Ingroup glorification (“Other nations can learn a lot from us”; “It is disloyal for Americans to criticize the U.S.”; $\alpha = .83$, $M = 4.72$, $SD = 1.44$) and *attachment* (e.g., “It is important for me to serve my country”; “Being an American is an important part of my identity”; $\alpha = .94$, $M = 6.71$, $SD = 1.79$) were measured with the eight-item scales by Roccas et al. (2006).

Demands for justice were measured with six statements regarding the individual ingroup perpetrators (e.g., “These U.S. soldiers should be punished for the things they have done”); four items measuring willingness to provide immediate help for victims (e.g., “The U.S. should make sure the basic needs of the victims’ family members are met, so they can mourn in peace”); three items about financial reparations (e.g., “The families of the victims should receive financial compensation from the U.S.”); five items measuring demands for apology, remorse, and forgiveness (e.g., “The families of the victims should receive an apology by the U.S. government”); and five items measuring demands for reform (e.g., “To prevent prisoner abuse in the future, the U.S. governmental and military structures should be reorganized”). All but one item with a low factor loading (about capital punishment for the U.S. soldiers) were averaged into a composite score ($\alpha = .94$, $M = 6.66$, $SD = 1.42$).

Results

When running the same analysis as in Study 1, results by and large reproduced (see the online supplementary information; see Figure 2). To test for the focal moderation hypothesis of Study 2, we computed a moderated regression with condition, glorification, attachment, and their interactions as predictor terms. Before doing so, we ensured that neither glorification nor attachment was itself affected by argument type, $F_s(2, 229) = 0.06$ and 0.91 , $p_s = .939$ and $.402$, respectively, and then centered both to allow for their use as moderators in our regression model. Glorification was strongly negatively associated with demands for justice, $\beta = -.29$, $F(1, 220) = 11.73$, $p < .001$, $\eta^2 = .05$. The only other significant effect was the three-way interaction, $F(2, 220) = 4.87$, $p = .003$, $\eta^2 = .06$. The effect of condition at low and high levels of attachment ($\pm 1 SD$) and glorification ($\pm 1 SD$) was thus computed. Only for individuals high in glorification *and* attachment, demands for justice differed between conditions. Among this set of individuals, justice demands differed significantly between the moral condition ($M = 6.95$) and the no-argument condition ($M = 6.17$), $t(220) = 2.30$, $p = .023$, $d = .36$, but not between the pragmatic condition ($M = 6.42$) and the no-argument condition, $t(220) = .67$, $p = .504$, nor between the pragmatic and the moral condition, $t(220) = -1.42$, $p = .159$. Among any other set of individuals (i.e., individuals high in *either* glorification *or* attachment, or low in both), none of the conditions differed from each other. In other words, only for individuals who were strongly attached to and strongly glorifying their country, moral arguments against torture increased justice demands (compared to no argument; see Figure 3). These results remained virtually the same when adding age or gender as covariates.

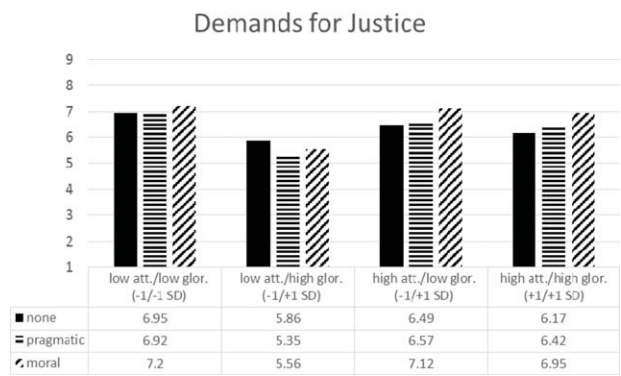


Figure 3. Demands for justice as a function of argument type (none vs. pragmatic vs. moral), ingroup attachment, and glorification in Study 2.

Discussion

Study 2 largely reproduced (or even fully reproduced, when adding age or gender as covariates) the findings of Study 1 using a more comprehensive measure of demands for justice. Compared to participants exposed to no argument or pragmatic arguments, participants exposed to moral arguments voiced (somewhat) stronger demands for justice. While the omnibus test and one of the preplanned contrasts reached only marginal statistical significance, the effect sizes of both the overall effect of argument and the comparisons between moral versus neutral/pragmatic argument were small but meaningful—suggesting the effects observed in Study 1 and 2 are consistent and reproducible (see Cohen, 1994; Fisher, 1925; Kirk, 1996, 2003). Importantly, the pattern of results in the three conditions and their three pair-wise comparisons matched the pattern in Study 1.

Extending Study 1, the effect of argument on demands for justice was moderated by glorification and attachment. While we expected glorification alone to be a significant moderator, the expected moderation of the effect of argument type by glorification was further moderated by attachment. In other words, the expected effect of argument type occurred only among high glorifiers who were strongly attached to their group (but not among those who were weakly attached to their group). As this result was partially unexpected given previous findings showing that attachment often has the opposite effect of glorification (but see Leidner, 2015, Study 2), we explored it further in Study 3.

STUDY 3

Study 3 expanded our earlier operationalization of attitudes towards torture in the concrete (i.e., demands to redress its injustice), adding a direct measure of support for opposition to torture in general. Second, it investigated potential mediators of the effects observed in Studies 1 and 2: *moral outrage*, *empathy*, and *guilt*. Because in Study 1 and Study 2 the pragmatic condition did not differ from the no-argument condition, Study 3 focused on the moral argument condition (compared to no argument) only.

Method

Participants. We recruited 234 participants through MTurk. Thirty-three were eliminated for spending less time reading the article than the minimum for comprehension, as in Study 2. Five participants were identified as univariate outliers (Tabachnick & Fidell, 2007) for taking significantly longer

than average to read the article—possibly being interrupted during the study. Five other participants not born in the United States, and one participant who specified the identity of the perpetrators incorrectly, were also eliminated. One hundred and ninety participants were thus retained for subsequent analyses (age: $M = 33.34$, $SD = 12.32$, range = 18–72; gender: 50 male, 133 female, 7 did not indicate gender; conservatism: $M = 4.37$, $SD = 2.12$, range = 1–9; education: one reported less than high school, 21 reported high school/GED, 67 reported some college, 68 reported to have a college degree, 23 reported to have a Master's degree, 3 reported to have a postdoctoral degree, 7 did not indicate; religion: 24 self-identified as agnostic, 26 as atheist, 3 as Buddhist, 36 as Catholic, 5 as Jewish, 1 as Muslim, 37 as Protestant, 50 as “other,” and 8 did not indicate; 94 reported ties through family or friends to the U.S. Army, 89 reported not to have any such ties, 7 did not indicate). The conditions did not significantly differ in terms of participants' age ($M_s = 33.68$ and 33.02 for moral and no-argument condition, respectively), $F(1, 180) = 0.13$, $p = .720$, conservatism ($M_s = 4.46$ and 4.29 , respectively), $F(1, 181) = 0.29$, $p = .591$, gender, $\chi^2(1) = 1.57$, $p = .210$, education, $\chi^2(5) = 6.83$, $p = .234$, religion, $\chi^2(7) = 7.16$, $p = .412$, or ties to the U.S. Army, $\chi^2(1) = 1.19$, $p = .275$. Again, this indicated that participants were demographically comparable across conditions.

Procedure. The experiment was conducted online. Participants were randomly assigned to read one of the two articles (moral vs. no-argument).

Measures. Participants completed measures, on visual analog scales (1–9), of *empathy* (six items, e.g., “I am moved by the plight of the prisoners”; $\alpha = .93$, $M = 5.93$, $SD = 1.96$), *collective guilt* (five items, e.g., “As an American, thinking about what these prisoners endured makes me feel guilty”; $\alpha = .98$, $M = 5.35$, $SD = 2.52$), *moral outrage* (three items, e.g., “I am morally outraged by the events I read about in the news report”; $\alpha = .97$, $M = 7.03$, $SD = 2.11$), and *support for torture* (five items, “To what extent the torture of the prisoners was... “professional,” “appropriate,” “understandable,” “fair,” and “justifiable”; $\alpha = .93$, $M = 2.19$, $SD = 1.54$). Measures of *demands for justice* ($\alpha = .95$, $M = 6.62$, $SD = 1.55$), *attachment* ($\alpha = .92$, $M = 6.79$, $SD = 1.71$), and *glorification* ($\alpha = .85$, $M = 4.86$, $SD = 1.49$) were identical to Study 2. The composite score for demands for justice was computed with all but one item; the same item excluded in Study 2 again had a poor factor loading, below .40.

Manipulation check. As in Studies 1 and 2, participants were asked to recall the prisoners' and soldiers' country of origin. All participants passed the manipulation check.

Results

Neither glorification nor attachment was affected by argument type, $F_s < 1$. We centered them and included them as independent (continuous) variables in subsequent analyses, together with argument (moral vs. no-argument) and their interaction term. Age, education, religion, and ties to the U.S. Army had no effects on any dependent variables (DVs), nor did they change the results reported below when entered as covariates in the analyses. Gender and conservatism, however, did have effects on all DVs and did change the results reported below when entered as covariates in the analyses. Importantly, the use of gender and conservatism as covariates did not decrease but increased the key interaction effect (of argument type by glorification) on all DVs (in the predicted direction). Below, we first and foremost report the analyses without covariates, as those were what we had planned in our a priori data-analytical strategy. Analyses where the addition of gender and conservatism made a difference in conventional significance levels compared to the analyses without covariates are reported in the online supplementary information.

Demand for justice. Glorification had a significant main effect, $F(1, 178) = 29.84$, $p < .001$, $\eta^2 = .14$. More glorification was associated with weaker justice demands ($\beta = -.47$). The interaction between glorification and argument was also significant, $F(1, 178) = 5.02$, $p = .026$, $\eta^2 = .027$. Low glorifiers' justice demands did not differ between the moral ($M = 7.13$, $SE = .25$) and the no-

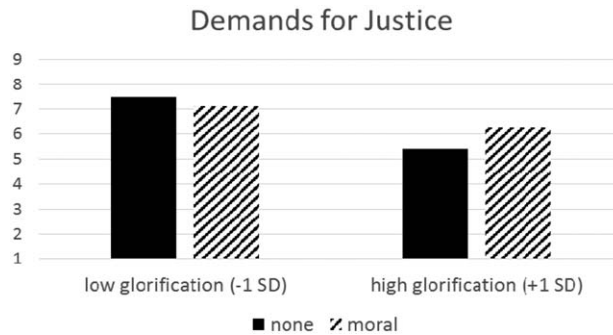


Figure 4. Demands for justice as a function of argument type (none vs. pragmatic vs. moral) and ingroup glorification in Study 3.

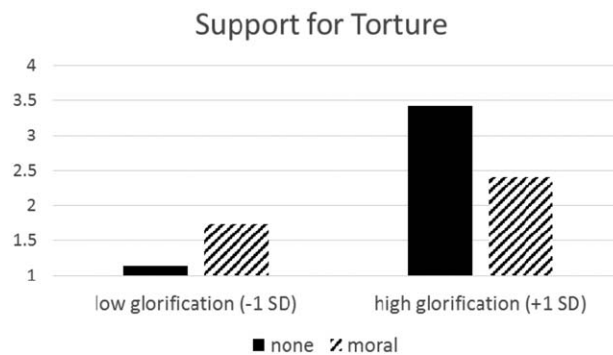


Figure 5. Support for torture as a function of argument type (none vs. pragmatic vs. moral) and ingroup glorification in Study 3.

argument ($M = 7.47$, $SE = .24$) condition, $t(178) = -1.00$, $p = .321$. High glorifiers, however, demanded significantly more justice in the moral ($M = 6.26$, $SE = .28$) than in the no-argument condition ($M = 5.40$, $SE = .26$), $t(178) = 2.26$, $p = .025$, $d = .34$ (see Figure 4). All other effects were non-significant, $F_s < 2.4$, $p_s > .13$.

Support for torture. Glorification and attachment had opposite effects, $F(1, 178) = 32.52$, $p < .001$, $\eta^2 = .15$, $\beta = .48$, and $F(1, 178) = 3.57$, $p = .060$, $\eta^2 = .02$, $\beta = -.17$, respectively. The interaction between glorification and condition was significant, $F(1, 178) = 9.69$, $p = .002$, $\eta^2 = .05$. Low glorifiers did not differ in their support for torture in the moral ($M = 1.74$, $SE = .24$) versus no-argument condition ($M = 1.15$, $SE = .23$), $t(178) = 1.77$, $p = .079$. High glorifiers, however, supported torture significantly less in the moral ($M = 2.41$, $SE = .26$) than in the no-argument condition ($M = 3.42$, $SE = .25$), $t(178) = -2.79$, $p = .006$, $d = .41$ (see Figure 5). No other effect reached significance.

Empathy. Glorification had a significant main effect, $F(1, 178) = 24.04$, $p < .001$, $\eta^2 = .12$, $\beta = -.43$, and so did attachment, $F(1, 178) = 5.91$, $p = .016$, $\eta^2 = .03$, $\beta = .23$. The interaction of glorification and condition was significant, $F(1, 178) = 5.44$, $p = .021$, $\eta^2 = .03$. Low glorifiers showed no difference in empathy between the moral ($M = 6.35$, $SE = .32$) and the no-argument condition ($M = 6.80$, $SE = .31$), $t(178) = -1.00$, $p = .319$, whereas high glorifiers displayed significantly more empathy in the moral ($M = 5.46$, $SE = .35$) than in the no-argument condition ($M = 4.31$, $SE = .33$), $t(178) = 2.39$, $p = .018$, $d = .35$ (see Figure 6). No other effect reached significance.

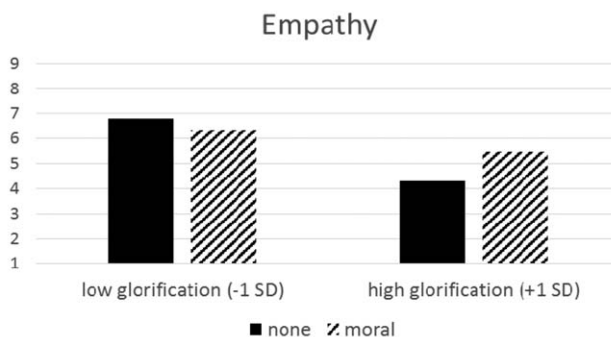


Figure 6. Empathy as a function of argument type (none vs. pragmatic vs. moral) and ingroup glorification in Study 3.

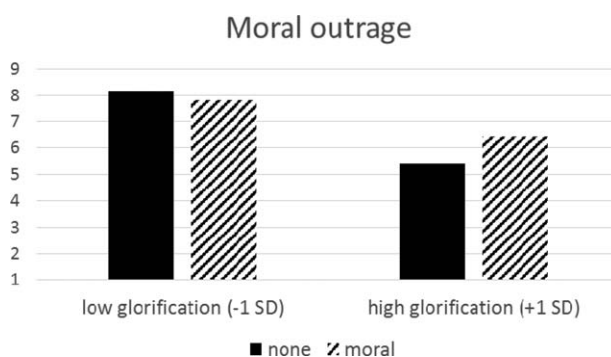
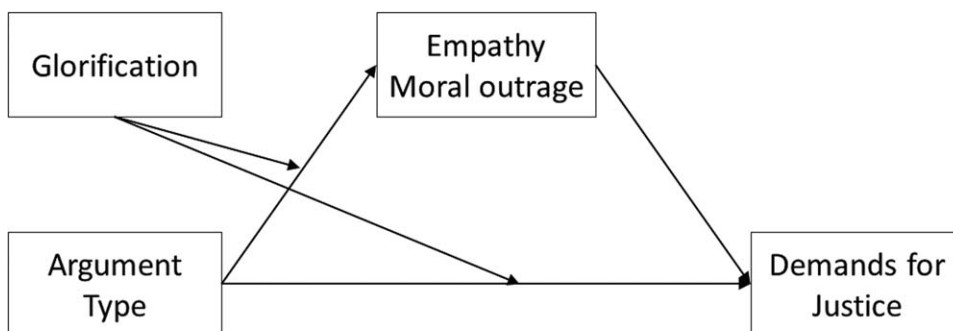


Figure 7. Moral outrage as a function of argument type (none vs. pragmatic vs. moral) and ingroup glorification in Study 3.

Collective guilt. Glorification and attachment had opposite effects, $F(1, 178) = 16.81, p < .001$, $\eta^2 = .09$, $\beta = -.37$, and $F(1, 178) = 10.22, p = .002$, $\eta^2 = .05$, $\beta = .31$, respectively. The interaction of glorification and argument was not significant, $F(1, 178) = 2.02, p = .157$, $\eta^2 = .01$. No other effect reached significance.

Moral outrage. Glorification and attachment had opposite, significant effects, $F(1, 178) = 31.53, p < .001$, $\eta^2 = .15$, $\beta = -.49$, and $F(1, 178) = 21.97, p < .001$, $\eta^2 = .11$, $\beta = .43$, respectively. The interaction of glorification and argument was marginally significant, $F(1, 178) = 3.63, p = .058$, $\eta^2 = .02$. Among low glorifiers, outrage did not differ between the moral ($M = 7.80, SE = .34$) and the no-argument condition ($M = 8.16, SE = .33$), $t(178) = -.76, p = .447$. High glorifiers, however, reported significantly more outrage in the moral ($M = 6.44, SE = .37$) than in the no-argument condition ($M = 5.41, SE = .35$), $t(178) = 2.00, p = .047$, $d = .30$ (see Figure 7). No other effect reached significance.

Mediation analyses. We tested whether the effect of moral arguments among high glorifiers, on both demands for justice and support for torture, was mediated by empathy and outrage (as guilt was not affected by argument in the first place), running a moderated multiple mediation analysis with 5,000 bootstrap samples and 95% confident intervals (Hayes, 2012). To be consistent with our prediction and our above tests of a two-way interaction of condition by glorification, and because we needed now to test for the indirect effects at low and high levels of one moderator (glorification) across the levels of a second moderator (attachment) while still accounting for this second moderator, we used Hayes's "model 8" with added control terms of attachment and its interactions with condition and glorification. Hayes's "model 10," while allowing for a more straightforward way to include a



High glorifiers

Indirect effect through empathy: boot coefficient = .137, lower CI = .059, upper CI = .393

Indirect effect through outrage: boot coefficient = .119, lower CI = .091, upper CI = .367

Low glorifiers

Indirect effect through empathy: boot coefficient = -.085, lower CI = -.234, upper CI = .039

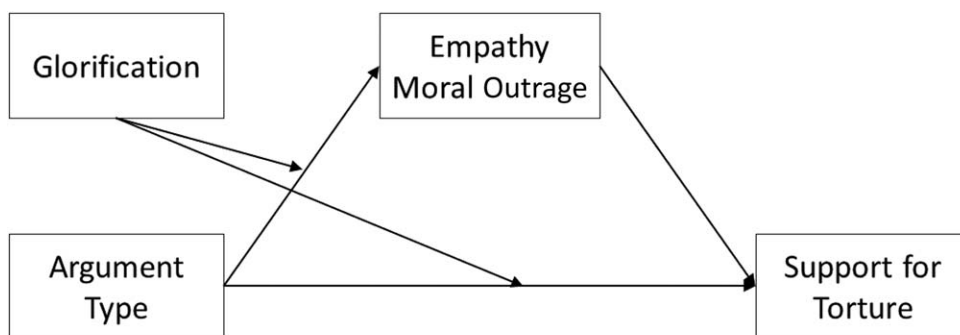
Indirect effect through outrage: boot coefficient = -.066, lower CI = -.214, upper CI = .067

Figure 8. Indirect effects of argument type (none vs. moral) and ingroup glorification through empathy and moral outrage on demands for justice (Study 3).

second moderator, in this case would not have allowed us to test indirect effects at low and high levels of glorification *across* levels of attachment (i.e., among high glorifiers low *and* high in attachment) while still accounting for attachment, nor does it provide an index of moderated mediation. Thus, using model 8, we introduced argument (coded as “−1” for participants in the no-argument and “+1” for participants in the moral condition) as independent variable, empathy and outrage as mediators, justice as DV, glorification as moderator, and attachment and its interaction terms as covariates. Collective guilt was not entered as mediator because it was not jointly affected by condition and glorification to begin with in the analysis without covariates (and even with covariates it was only marginally affected). As hypothesized, empathy and outrage only mediated the effect of argument among high glorifiers but not among low glorifiers, controlling for attachment and its interactions with condition and glorification. For high glorifiers, the indirect effects through empathy (CI95% = [.059, .393]) and outrage (CI95% = [.091, .367]) were both significant. For low glorifiers, neither indirect effect was significant (empathy: CI95% = [−.234, .039]; outrage: CI95% = [−.214, .067]; see Figure 8). Further, the difference between these (significant) indirect effects at high levels of glorification and these (nonsignificant) indirect effects at low levels of glorification was also significant (empathy: CI95% = [.040, .273]; outrage: CI95% = [.065, .265]).

The same mediational model with support for torture as the DV (instead of demands for justice) revealed a similar pattern: Empathy and outrage mediated the effect of argument among high but not low glorifiers, controlling for attachment and its interactions with condition and glorification. For high glorifiers, the indirect effects through empathy (CI95% = [−.277, −.035]) and outrage (CI95% = [−.408, −.013]) were both significant. For low glorifiers, neither indirect effect was significant (empathy: CI95% = [−.018, .166]; outrage: CI95% = [−.065, .255]; see Figure 9). Again, the difference between these (significant) indirect effects at high levels of glorification and these (nonsignificant) indirect effects at low levels of glorification was also significant (empathy: CI95% = [−.192, −.026]; outrage: CI95% = [−.300, −.003]).²

² For both demands for justice as well as support for torture, the moderated mediation through empathy and outrage remained significant when entering gender and conservatism as additional covariates.

**High Glorifiers**

Indirect effect through empathy: boot coefficient = $-.119$, lower CI = $-.277$, upper CI = $-.035$

Indirect effect through outrage: boot coefficient = $-.188$, lower CI = $-.408$, upper CI = $-.013$

Low Glorifiers

Indirect effect through empathy: boot coefficient = $.053$, lower CI = $-.018$, upper CI = $.166$

Indirect effect through outrage: boot coefficient = $.070$, lower CI = $-.065$, upper CI = $.255$

Figure 9. Indirect effects of argument type (none vs. moral) and ingroup glorification through empathy and moral outrage on support for torture (Study 3).

Discussion

Study 3 replicated the effect of moral arguments against torture on demands for justice and extended previous results on a measure of support for torture. While Study 2 found moral arguments to affect only high glorifiers who were also high in attachment, Study 3 found this effect among high glorifiers across levels of attachment (i.e., among high glorifiers high *or* low in attachment). Most importantly, Study 3 investigated three potential mediators: empathy, outrage, and guilt. The first two showed very similar patterns. Namely, while high glorifiers responded generally less outraged and empathic to ingroup-committed torture, moral arguments increased outrage and empathy compared to no argument against torture. Both increases explained the effect of moral arguments on demands for justice and support for torture among high glorifiers.

General Discussion

In 1987, the United Nations Convention against Torture and Other Cruel, Inhuman or Degrading Treatment or Punishment (UNCAT) came into force. Article 2 of the convention prohibits use and justification of torture without exceptions and commands all participating countries to prevent the use of torture in their territories. Yet, public opinion in the United States and worldwide supports the use of torture in large numbers, even majorities. We thus investigated which arguments against torture are most likely to resonate with the public and lead to stronger demands to redress the injustice of torture and to weaker support for torture. Specifically, we compared the effectiveness of moral and pragmatic arguments against torture, the moderating role of glorification of (and attachment to) the nation, and, in an effort to unveil the factors underlying this process, the mediating role of empathy, moral outrage, and guilt.

Findings suggest that only moral arguments are effective in prompting people to oppose torture by redressing its injustice. This finding is consistent with Ames and Lee (2015), who found that rather than the lack of benefit of torture reducing support for torture, preexisting support for torture increases the perceived benefit of torture. As such, it may then not be surprising that preexisting support for torture

and beliefs in its effectiveness inoculate people against new, pragmatic arguments about the *ineffectiveness* of torture. Study 2 further demonstrated that the effect of moral arguments is only evident among people who strongly glorify the ingroup (and are strongly attached to it). Again, pragmatic arguments were ineffective, regardless of whether people were high or low in glorification or attachment. Study 3 replicated this effect for all high glorifiers (across both low and high levels attachment) and found that they support torture less strongly after moral arguments *because* these arguments elicit more outrage over torture as a violation of moral standards and more empathy towards victims (but not more guilt).

What Makes Moral Arguments Effective?

H. G. Wells wrote in *The Rights of Man — What Are We Fighting For?* that the allied soldiers in WWII “had been stirred profoundly by those outrages upon human dignity perpetrated by the Nazis” (Wells, 1940, p. 31). Moral outrage motivates people to pursue justice and oppose perceived injustice. Oftentimes, however, when the same cruelty is committed by ingroup members, this outrage and its concomitant motivation are missing—especially among people who glorify the ingroup (Leidner et al., 2010). Our findings show that the presentation of a moral critique of the ingroup’s actions can increase outrage over violated moral standards even among high glorifiers. This finding is also in line with research on moral mandates; if people’s moral convictions are violated, they react with anger (Mullen & Skitka, 2006). It appears this moral mandate effect can even extend to situations in which people would otherwise disengage from the moral violation (i.e., ingroup-committed wrongdoings), as long as it is combined with moral arguments against the standard violation.

Moral arguments also exert their effect by enhancing empathy toward the victims. Empathy, a ubiquitous and automatic response that humans have towards other humans (Decety & Ickes, 2009; Rifkin, 2010), is often curtailed as a consequence of social categorization processes that separate “us” from “them” (Castano, 2012) and by motivational processes that serve maintenance of a positive group and self-image (Castano & Giner-Sorolla, 2006; Leidner et al., 2010; Leidner & Castano, 2012). A moral critique of such atrocities likely functions as a reminder of the victims’ humanity and their place in the moral circle (rather than outside of it; Singer, 1981) and thus of the connection that ingroup and outgroup members have as humans despite any group boundaries on other dimensions such as nationality, ethnicity, or religion.

Limitations and Future Research

Our findings are of both theoretical and practical importance. Practical, because they identified which type of arguments against torture are most effective in curbing public support for torture, and thus should perhaps be used more in the public debate over torture. Theoretical, because they illuminate communication and persuasion processes that are usually influenced by bias (i.e., self-relevant attitudes), and how and why attitudes can be changed even among those people who are usually most biased (i.e., high glorifiers). Both advances highlight the great potential that our understanding of communication, identity, and attitudes of the public’s opinion has for the establishment of a culture of peace and respect for human rights. Future research is needed to assess whether our findings for the effectiveness of moral but not pragmatic arguments against torture generalize to other norm violations. Economic exploitation of third-world countries or support for violent authoritarian regimes are but two examples which lend themselves to the same analysis we presented here for the case of torture.

While the effects were remarkably robust across studies and nonnegligible in size, it is unclear whether people’s attitudes truly changed. From a design perspective, a within-participants design with attitudes measured pre- and postargument might render further evidence for attitude change. But ultimately the between-participants experimental designs we employed yield the same evidentiary value. More importantly, a within-participants design would further compound the real issue at hand: Did

participants' self-reports reflect actual attitude change or pressure or social desirability concerns? While our data is not able to speak to this question, it does not critically restrict the interpretability of our data with respect to public opinion on torture. First, when people report their attitudes in public opinion polls, social pressure or social desirability concerns are likely to operate similar as in our self-report measures—perhaps even to a greater degree. Second, in the case of public opinion and its consequences for policy and decision making at the group level, it matters first and foremost what people report their attitudes to be; whether or not such self-reports match the “real attitude” is secondary in this context.

A factor our studies did not account for is the perceived likelihood of possible (positive or negative) consequences of torture. The effectiveness of pragmatic arguments in particular might depend on people's subjective assessments of questions such as “How likely will our use of torture put our own troops or our country at risk?” Thus, it is possible for pragmatic arguments to be effective in reducing support for torture under more specific circumstances than the ones explored in our studies (e.g., when the costs of torture are particularly high and likely to be incurred). Yet, given the more general circumstances of our studies, it seems safe to say that *without* special circumstance, moral arguments against torture yield more promise to reduce public support for torture than pragmatic ones.

Another open question concerns the potency of moral arguments. Is it specific to condemnation of torture, or does it generalize to the endorsement of torture as well? More specifically, can moral arguments only reduce high glorifiers' support for torture when condemning its immorality, or could they also increase high glorifiers' support for torture when embracing its morality? While this question needs to be answered by future research, there is reason to believe that moral arguments *for* torture will be less potent than moral arguments against torture. This is because the effects we observed were unique to high glorifiers—that is, those group members who usually defend morally questionable ingroup behavior “by default” (Leidner & Castano, 2012; Leidner et al., 2010; Roccas et al. 2006). This default naturally restricts the space and likelihood for moral arguments *for* torture to increase high glorifiers' support for torture.

To conclude, moral arguments against ingroup-perpetrated torture seem to increase demands for justice, and decrease support for torture, precisely for those people who are most problematic from the perspective of social justice and peaceful intergroup relations—that is, high glorifiers. Rather than naïve or hopelessly idealistic, appealing to the moral and good in human nature may be worthwhile and effective after all and help strengthen the normative power of international humanitarian law.

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Supporting Information

Additional supporting information may be found in the online version of this article at the publisher’s website:

Appendix

Study 1

Study 2

Study 3