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When Alterations Are Violations: Moral Outrage and Punishment in Response to (Even Minor) Alterations to Rituals

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From Catholics performing the sign of the cross since the 4th century to Americans reciting the Pledge of Allegiance since the 1890s, group rituals (i.e., predefined sequences of symbolic actions) have strikingly consistent features over time. Seven studies (N = 4,213) document the sacrosanct nature of rituals: Because group rituals symbolize sacred group values, even minor alterations to them provoke moral outrage and punishment. In Pilot Studies A and B, fraternity members who failed to complete initiation activities that were more ritualistic elicited relatively greater moral outrage and hazing from their fraternity brothers. Study 1 uses secular holiday rituals to explore the dimensions of ritual alteration—both physical and psychological—that elicit moral outrage. Study 2 suggests that altering a ritual elicits outrage even beyond the extent to which the ritual alteration is seen as violating descriptive and injunctive norms. In Study 3, group members who viewed male circumcision as more ritualistic (i.e., Jewish vs. Muslim participants) expressed greater moral outrage in response to a proposal to alter circumcision to make it safer. Study 4 uses the Pledge of Allegiance ritual to explore how the intentions of the person altering the ritual influence observers' moral outrage and punishment. Finally, in Study 5, even minor alterations elicited comparable levels of moral outrage to major alterations of the Jewish Passover ritual. Across both religious and secular rituals, the more ingroup members believed that rituals symbolize sacred group values, the more they protected their rituals—by punishing those who violated them.

Keywords: ritual, moral, group, norm, commitment

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On November 27, 2011, in an attempt to unify the more than one billion Catholics worldwide, the Catholic Church mandated that all dioceses use the same translation of the Bible, which led to slight alterations in the Mass for English-speaking Catholics. For example, when the priest said, "The Lord be with you," the congregation had responded, "And also with you" but now responded, "And with your spirit." Although many of the alterations were minor, they created an uproar. Church members labeled them an "affront" and warned that they would cause "ritual whiplash" and "fuel a Catholic culture war" (Boorstein, 2011). We suggest that because group rituals come to embody group values, they become sacrosanct, such that even minor—and even beneficial—alterations to group rituals can constitute moral violations, provoking moral outrage. We further examine how group members' moral outrage is influenced by their commitment to their group and belief that the ritual embodies group values, as well as by features of the rituals and the magnitude of the alteration.

The complex series of prayers and behaviors that comprise the Catholic Holy Mass, mostly unaltered for well over 1,000 years, is just one of countless examples of longstanding group rituals—meaningful sequences of actions characterized by rigidity, formality, and repetition. To offer two more examples, the U.S. 21-gun salute honoring military victims was established in the 1800s (U.S. Army Center of Military History, 2019), and male circumcision rituals (i.e., a *brit milah* ceremony) have been practiced in the Jewish faith for over 3,000 years (BBC, 2014). Social scientists have argued that rituals are unique from other group activities in that, once formalized and enacted, they have strikingly consistent features over time (e.g., Bell, 1997; Dulaney & Fiske, 1994; Hayden, 1987; Iteanu, 2016; Norenzayan et al., 2016; Rappaport, 1999; Smith & Stewart, 2011). Indeed, Rappaport (1999) suggests

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that rituals are among "the most perfectly recurrent of cultural events" (p. 178) and that invariance between performances is a defining feature of ritual.

The consistency of ritual features over time suggests that their repeated and specific enactment is deeply important to ingroup members. But by what sociopsychological mechanisms do these behaviors perpetuate with such precision? We examine one possibility: that rituals remain unchanged over time because their alteration evokes moral outrage and subsequent punishment from others who endorse these rituals. Specifically, we explore whether alterations to group rituals are more likely to be perceived as moral violations as compared with seemingly similar group activities that are less ritualistic—because group rituals represent the value system of the group. Moreover, we suggest that moral outrage leads ingroup members to punish those who alter ritualistic activities even when the alterations are minor or benefit the individuals involved.

Conceptualizing Ritual

Consistent with prior conceptualizations, we define *rituals* as (a) predefined sequences of action characterized by rigidity, formality, and repetition that are (b) embedded in a larger system of symbolism and meaning (Hobson et al., 2018; Stein, Hobson, & Schroeder, 2020). Unique from other activities, rituals require both specific physical characteristics (e.g., rigidity) and psychological characteristics (e.g., meaningfulness). For example, the Catholic sign of the cross and the American folding of the flag are two rituals characterized by a specific set of gestures that are deeply meaningful: the sign of the cross symbolizes Jesus' crucifixion, and each of the 13 required flag folds has a separate symbolic meaning. These examples highlight how a simple physical gesture (e.g., a flag fold) in the context of a ritual signifies an important value for a group (e.g., pledging allegiance to the United States). Rituals can also operate at the level of the individual or group. The meaningfulness and symbolism of group rituals, in particular, are linked to a group's value system (e.g., Bell, 1997; Fischer et al., 2013; McCauley & Lawson, 2002; Smith, 1980), whereas individual rituals' meaningfulness may be more idiosyncratic to the performer and the context of the ritual (e.g., athletes' preperformance rituals; Brooks et al., 2016; Hobson et al., 2018).

Critical to our definition of ritual is that similar activities can be more or less ritualistic depending on their physical and psychological features. Habits, for instance, may contain similar physical features (repetitive, rigid behaviors) but typically lack symbolism; moreover, habits do not necessarily serve social functions (e.g., Ouellette & Wood, 1998). To exemplify how an activity can be more or less ritualistic, consider celebrations of the American holiday of Thanksgiving, which tend to be highly ritualistic compared to celebrations of other holidays (e.g., Columbus Day). Thanksgiving celebrations are ritualistic because they require engaging in specified behaviors (e.g., eating turkey, gathering with family) with symbolic meaning (i.e., signaling gratitude; Sezer et al., 2016). Beyond longstanding group traditions, even relatively novel group activities such as a work team performing a warm-up chant before a sales call can be made more ritualistic through added features, such as performing the chant at the same time, in the same way, and with repetitive movements (physically ritualistic features), and imbuing the chant with the organization's values (psychologically ritualistic features).

Rituals typically contain normative elements, but rather than being considered only descriptive norms (what individuals *actually* do) or injunctive norms (what individuals *ought* to do; Cialdini et al., 1991; Reno et al., 1993), rituals are most aligned with a third category of behaviors: institutional norms (what individuals *must* do; Atran & Ginges, 2012; Morris, 2020; Morris, Hong, et al., 2015; Morris & Liu, 2015). Because a defining feature of ritual is that the "actions must be performed in a specific way, and in a specific order" (Boyer & Lienard, 2020, p. 4), group rituals (e.g., the Catholic Sign of the Cross) serve as institutional norms. However, not all institutional norms are group rituals. For example, traffic patterns are examples of institutional norms that are not rituals: different regions have different behavioral standards that "must" be followed (e.g., must yield to pedestrians) but lack the psychological or physical features of ritual (Morris, Chiu, et al., 2015). Moving beyond the prior research that has examined reactions to descriptive and injunctive norm violations (Helweg-Larsen & LoMonaco, 2008; Kam & Bond, 2009; Ohbuchi et al., 2004), here we test novel ways in which group members react to alterations of their institutional norms—specifically, rituals.

In so doing, we also provide a counterpart to research examining the typically beneficial consequences for groups of enacting identical rituals over time (e.g., Fischer et al., 2013; Wen et al., 2016; Xygalatas et al., 2013). We define a *ritual alteration* as any modification to either the physical or psychological features of the ritual. Altering the day or time a ritual is performed, how the elements are performed (including omitting elements or reordering them), the context in which it is performed, who performs it, or even not performing the ritual entirely all fall under our definition of ritual alteration.

Group Rituals Represent Group Values

Although some scholars have questioned the underlying value of rituals and superstitions (see Sax, 2010), noting that individual rituals are more likely to be pathological because they lack broader benefits of shared "cultural priors" (e.g., obsessive–compulsive disorder; Reuven-Magril et al., 2008), the extant literature is clear that *group* rituals often serve a valuable and important function for group wellbeing and survival (Durkheim, 1915; Watson-Jones & Legare, 2016; Whitehouse & Lanman, 2014; Xygalatas et al., 2013).[1] In particular, group rituals serve core affiliative functions Fn1 that address the challenges of group living (Watson-Jones & Legare, 2016) by *promoting*, *protecting*, and *perpetuating* a group's value system (Rossano, 2012).

First, rituals *promote* group values. Group rituals provide a physical manifestation of the group's values, visibly promoting values that the group often considers most sacred (e.g., Bell, 1997; Smith, 1980; Sosis, 2004). To understand how the simple but structured physical gestures in a ritual become symbolically linked to a group value, scholars have proposed the theory of "causal opacity" (Herrmann et al., 2013; Kapitány & Nielsen, 2015; Legare & Souza, 2012, 2014; Legare et al., 2015; Watson-Jones et al., 2014). Rituals appear causally opaque in that they often lack a

[1] Individual rituals, although personally meaningful, often lack cultural history, are not linked to a group's value system, and are less likely to be imbued with sacredness (Hobson et al., 2018; Nielbo & Sørensen, 2016). As a result, our theorizing pertains exclusively to group rituals, both public (e.g., Church service) and private (e.g., praying the Rosary).

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direct observable causal connection between the specific action performed (e.g., shaking one's hand) and the symbolic value it represents (e.g., showing cooperation; Schroeder et al., 2019). Although rituals consist of action sequences that alone can be seen as goal-directed (e.g., washing hands), it is the context of the ritual performance that results in goal demotion (e.g., washing hands many more times than hygiene requires; Liénard & Boyer, 2006). In essence, the causal opacity of ritual leads the physical procedure of the ritual to become less instrumental and more symbolic (Legare & Nielsen, 2015; Rossano, 2012). As an example, in a U.S. military funeral ceremony, the performance of "Taps" involves raising and lowering a bugle in a highly specific way that is not required for proper playing; these actions are instead an end unto themselves and have come to represent U.S. values (i.e., gratitude and respect for veterans; Rossano, 2012). In this sense, the physical enactment of rituals provides evidence of behavioral commitment to ingroup values (especially when rituals are costly to perform; Watson-Jones & Legare, 2016; Xygalatas et al., 2013) and provides an alternative to verbally expressed beliefs and commitments, which can be especially susceptible to deception (Henrich, 2009).

Second, rituals *protect* group values. Groups with stable and consistent values can integrate the activities of large numbers of people, increasing the likelihood of survival (e.g., Chatman & O'Reilly, 2016; Chudek & Henrich, 2011; Cosmides & Tooby, 2013; Harari, 2014; Hofstede, 1993; Thornhill et al., 2009). As a result, rituals can solve adaptive problems associated with forming and maintaining beneficial coalitional alliances because rituals, as physical manifestations of ingroup values, contribute to the stability and consistency of group values (Sosis & Bressler, 2003; Sosis et al., 2007; Watson-Jones & Legare, 2016). Furthermore, per Henrich's (2009) model of social learning, group rituals serve as credibility-enhancing displays that provide evidence of an individual's commitment to ingroup values, allowing ingroup members to differentiate between other ingroup members (who understand and value the group's rituals as a credibility-enhancing display) and outgroup members (who are puzzled by the causal opacity of the group's rituals that have no obvious meaning; Atran & Henrich, 2010; Bulbulia & Sosis, 2011). As a result, rituals identify ingroup members who can be trusted in future interactions, and similarly, they identify outgroup members who may be perceived as a threat to the group (Sosis & Alcorta, 2003; Watson-Jones & Legare, 2016; Wen et al., 2016). Consistent with this theorizing, groups with more established rituals also tend to be more cohesive (Atran & Henrich, 2010; Rappaport, 1999; Rossano, 2012; Sosis & Alcorta, 2003; Tambiah, 1979; Watson-Jones & Legare, 2016).

Finally, rituals *perpetuate* group values. Enacting rituals reinforces group beliefs and narratives, reminding the performers and observers of the meaning behind the ritual: the groups' core values (McCauley & Lawson, 2002; Whitehouse, 2000, 2004). Thus, rituals serve as memory cues (Rossano, 2012), helping to bring to mind ingroup values, which can foster ingroup commitment and cooperation (Fischer et al., 2013; Páez, Rimé, Basabe, Wlodarczyk, & Zumeta, 2015; Stein et al., 2020). Moreover, rituals' specific physical characteristics (e.g., repetition, rigidity) allow rituals to be learned, imitated, and shared with others (Hobson et al., 2018; Kapitány & Nielsen, 2015). Specifically, Legare and Nielsen (2015) argue that rituals are ideal for high-fidelity cultural transmission over time because rituals' causal opacity shifts the focus of the ritual from the desired outcome to the physical procedure, encouraging imitation in children (see also, Clegg & Legare, 2016; Legare et al., 2015; Herrmann et al., 2013; Watson-Jones et al., 2014). Indeed, past research on the vertical transmission of rituals suggests that people have a developmental sensitivity for observing and imitating ritual-like behaviors (Clegg & Legare, 2016; Liberman et al., 2018; Watson-Jones et al., 2016), which makes it easier to transmit rituals—and hence, group values—across generations (Liénard & Boyer, 2006; Rossano, 2012).

Rituals and Group Morality

We suggest that because group rituals embody group values, they come to constitute moral actions. Moral foundations theory (Graham et al., 2011, 2013; Haidt, 2012) proposes that moral intuitions are shaped in part by their development within groups and cultures (Greenwood, 2011). In particular, this theory suggests that there are innate moral "foundations" (e.g., care/harm, loyalty/betrayal) on which morality is constructed within specific groups (Graham & Haidt, 2012). As moral foundations are somewhat broad and abstract (e.g., "harm"), they are translated into idiosyncratic behavioral guidelines that are group-specific and identity-defining (Ellemers, 2017; Ellemers & van den Bos, 2012; Heine, 2005; Leach et al., 2015; Monin & Jordan, 2009; Sachdeva et al., 2011; Tooby & Cosmides, 2010).

For instance, consider the care/harm moral foundation and male circumcision rituals. Groups that practice circumcision rituals (e.g., Jews, Muslims) consider circumcision to be a sign of religious devotion (i.e., care) that influences personal moral convictions (e.g., "Circumcision is a mechanism by which to connect with God"); in contrast, other groups consider male circumcision to be a type of bodily mutilation (i.e., harm) that influences members' personal moral convictions (e.g., "Child circumcision is bodily mutilation"). These examples demonstrate that universal moral norms, such as care and harm, can translate into different group-specific conceptions of what is considered morally correct behavior (Ellemers & van der Toorn, 2015). As a result, behavior that one group considers to be a moral transgression may constitute a meaningful, moral ritual by another (McGraw & Tetlock, 2005; Rai & Fiske, 2011).

Consequences of Ritual Alterations

Taken together, the fact that group rituals serve to promote, protect, and perpetuate group values and that the ingroup encodes its value system as moral leads to our hypothesis that ingroup members will view alterations to group rituals as moral violations. We propose that altering rituals, and thereby compromising the group's sacred values, will elicit *moral outrage*. Moral outrage is a reaction of anger at the violation of a moral standard which manifests in a positive association between judgments of anger and judgments of immorality (e.g., Crockett, 2017; Haidt, 2003; Hechler & Kessler, 2018; Montada & Schneider, 1989; Mullen & Skitka, 2006; Tetlock et al., 2000). Consistent with the sacred value protection model (Tetlock, 2003; Tetlock et al., 1996), people express moral outrage when others broach even the possibility of considering the legitimacy of their sacred values (Baron & Spranca, 1997; McGraw et al., 2012; McGraw et al., 2003; Tetlock et al., 1996). For instance, people respond with moral outrage to a decision-maker who merely *contemplates* trade-offs between sacred values (i.e., human life) and secular values (i.e., money)

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because comparing sacred with secular values can subvert the sacred ones (Tetlock et al., 2000). This suggests that alterations to relatively more ritualistic events (e.g., celebrating Thanksgiving) should provoke more moral outrage than alterations to less ritualistic events (e.g., celebrating Columbus Day), because the former are more closely linked to the group's values.

We further propose that, beyond eliciting moral outrage, individuals who alter group rituals will be punished. Across groups and cultures, individuals who engage in moral transgressions are punished, with social exclusion being a common consequence (Rai & Fiske, 2011; Tooby & Cosmides, 2010). People punish those who violate moral standards, even when facing personal costs for doing so, and anger following moral violations is the proximate mechanism underlying punishment (e.g., de Kwaadsteniet et al., 2013; Fehr & Fischbacher, 2004; Fehr & Gachter, 2000; Lotz et al., 2011; Seip et al., 2014).

Because group rituals represent a group's sacred values, which groups perceive to be non-negotiable principles (Atran, 2010; Atran et al., 2007; Atran & Ginges, 2012; Baron & Leshner, 2000; Tetlock, 2002), even well-intentioned or accidental alterations should elicit outrage and punishment. In a similar vein, the sacred values protection model predicts that sacred values are insensitive to trade-offs (Tetlock, 2002). As a result, individuals should be outraged by the notion that a sacred value has been compromised but relatively insensitive to the degree to which it was compromised (Baron & Spranca, 1997; Sachdeva & Medin, 2008). Thus, as we argue that the level of moral outrage should be more sensitive to the mere presence of an alteration (or not) and relatively less sensitive to the exact magnitude of the alteration, because even minor alterations can subvert a group's sacred values.

Who is most likely to care about the group's values and thus experience outrage and the desire to punish those who alter rituals? In general, ingroup members who are more committed to the group—and who thus adopt group moral values as part of their internal self-standards—are more likely to adhere to those moral imperatives (Blader et al., 2017; Christensen et al., 2004; Leach et al., 2008; Riketta, 2005; Roccas et al., 2008). The internalization of group values is a fundamental element of group commitment; group members who adopt and internalize the values of the group tend to be more committed to the group (Klein et al., 2012; Meyer & Allen, 1991). Some researchers even consider value congruence a definitional component of commitment; for example, Mayer and Schoorman (1992) defined "value commitment" as the "belief in and acceptance of organizational goals and values and a willingness to exert considerable effort on behalf of the organization" (p. 673) and O'Reilly and Chatman (1986) operationalized commitment (internalization dimension) as felt attachment for a group predicated on value congruence. We therefore predict that ingroup members who are more committed to their group and, relatedly, those who perceive a tighter link between rituals and group values, will also be the most likely to experience moral outrage in response to ritual alterations.

Overview of Hypotheses

Our account holds that alterations to group rituals constitute moral violations of sacred group values, such that altering (vs. not altering) a group ritual elicits relatively more moral outrage (H0). We suggest that the more ritualistic the group activity is (that is, the more psychologically meaningful and physically specified it is), the more it will reflect group values, such that altering group activities with more (vs. fewer) ritualistic features will elicit relatively more moral outrage (H1a). Moreover, we predict that altering group activities with more (vs. fewer) ritualistic features will also provoke relatively more punishment of the ritual alterer (H1b).

Our theory contends that altering group rituals constitutes a moral violation because rituals represent group values. As a result, group members who more strongly view the ritual as symbolizing their group's values should experience relatively more moral outrage (H2a); relatedly, moral outrage in response to ritual alteration should be relatively more pronounced among group members who are more committed to the ingroup and therefore care more about the group's values (H2b). The effect of ritual alteration on moral outrage should also be stronger among ingroup than outgroup members because outgroup members do not share the group's values; as a result, ritual alterations do not constitute moral violations to outgroup members (H2c). Last, because even minor alterations to a ritual can undermine group values by questioning the legitimacy of the values it represents, and sacred values are insensitive to trade-offs, we predict that people will be relatively insensitive to the magnitude of the alteration but more sensitive to the presence of an alteration (H2d). Notably, this insensitivity to magnitude would be inconsistent with prior findings on descriptive and injunctive norm violations in which the punishment "fits the crime" (i.e., punishment increases commensurate with the severity of the violation; Fehr & Gachter, 2000, 2002; Forsyth, 1995), highlighting another way that ritual alterations are unique from descriptive and injunctive norm alterations.

We further consider how reactions to a ritual alteration may depend on beliefs about the intentions of the ritual alterer (e.g., whether they made the alteration with benevolent or ill intent). Our theory predicts that altered rituals will elicit moral outrage even when those alterations have benefits (e.g., reducing costs, improving safety) or are well-intentioned (H3) because sacred values are protected against trade-offs, even if the trade-off will result in a beneficial outcome (Baron & Spranca, 1997).

Finally, we test our conceptualization of ritual as an institutional norm, extending beyond prior literature that examines responses to altering descriptive and injunctive norms (Helweg-Larsen & Lo-Monaco, 2008; Kam & Bond, 2009; Ohbuchi et al., 2004). Just as descriptive and injunctive norms overlap in features but can have unique effects on behavior (e.g., Jacobson et al., 2011), we theorize that rituals, as examples of institutional norms, can uniquely affect judgments and behavior.[2] Specifically, we predict (H4) that Fn2

[2] Examining the possibility that people psychologically represent rituals differently than descriptive and injunctive norms, we provided 300 practicing Catholics with the definitions of a ritual, a descriptive norm, and an injunctive norm and asked them to nominate examples of each (see Supplemental Study S1 for study details). Coders then reviewed and categorized the list of examples. Our coding results revealed that some practices (e.g., Baptism, confession) were much more likely to be listed as a ritual than as a descriptive or injunctive norm, others were more likely to be listed as a than a ritual (e.g., showing kindness, donating to than a ritual (e.g., showing kindness, donating to charity), and still others were commonly listed as both (e.g., marriage, prayer; see Supplemental Figure S1 for a Venn diagram depicting the results). These findings indicate that people do have separable representations of descriptive norms, injunctive norms, and rituals.

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altering more ritualistic group activities enhances moral outrage even controlling for the degree that the group activity is a descriptive or injunctive norm.

Overview of Studies

To examine our predictions using an externally valid sample, we conduct Pilot Study A, which presents a collegiate social fraternity sample with new member profiles and tests whether new members who alter more (vs. less) ritualistic activities trigger relatively stronger moral outrage (H0, H1a). Pilot Study B further examines consequences of altering ritualistic activities such as hazing behavior (H1b). Study 1 seeks to establish which aspects of a ritual alteration elicit moral outrage, testing whether alterations to even secular group activities that seem more ritualistic, either via their psychological or physical features, result in relatively more moral outrage (H1a). Study 2 further demonstrates that ritual alterations elicit moral outrage irrespective of the degree to which the ritual is perceived to be a descriptive or injunctive norm (H4). Study 3 presents Jewish and Muslim participants with an objectively positive ritual alteration (mandating safety standards for their religious circumcision ceremonies; H3). Although both Jews and Muslims practice male circumcision, the Jewish circumcision ceremony is more ritualistic than the Muslim ceremony. We test whether altered (vs. unaltered; H0) rituals incite relatively more moral outrage and punishment, especially for Jewish (vs. Muslim) participants (H1a and H1b). Study 4 explores potential scope conditions by experimentally manipulating the ritual alterer's intention for the alteration (H3) using a common U.S. ritual (the Pledge of Allegiance). Finally, Study 5 examines the relation between the magnitude of ritual alteration (i.e., small or large alteration) and moral outrage by using the Seder plate, a Jewish ritual that illustrates the story of Passover (H2d). Study 5 further tests whether outgroup members (non-Jewish participants) experience moral outrage and punishment intent in response to a Jewish ritual alteration (H2c).

To provide examine our theoretical model, Studies 3, 4, and 5 test whether moral outrage mediates the relationship between ritual alteration and punishment of the person who alters the ritual (H1b). Studies 4 and 5 also test whether ingroup members who believe the ritual reflects the group's values (H2a) and feel more committed to the ingroup (H2b) show stronger effects of ritual alteration on moral outrage and punishment.

For all studies, we report how we determined our sample size, all data exclusions, and all measures (Simmons et al., 2011). For all studies except Pilot Studies A and B and Study 5, we followed a stopping rule for 100 participants per between-subjects experimental condition. The sample sizes for Pilot Studies A and B were determined by access to members of a fraternity. In Study 5, we a priori doubled this rule (see relevant study preregistration), aiming to recruit 200 participants per between-subjects experimental condition because we sought to detect an interaction with a nonlinear term. All of our data, code, and survey materials are available in the Open Science Framework (OSF) repository for this project (https://osf.io/6kc3z). Table 1 summarizes each of the studies in the article. T1

Pilot Studies A and B: Altering Fraternity Rituals

Pilot Studies A and B examine alterations to rituals in a real-world, externally valid context: fraternity initiations. Collegiate social fraternities in the United States often require new members (colloquially, "pledges") to complete various initiation activities to attain full member status. These initiation activities vary in how ritualistic they are, ranging from activities that are very repetitive, structured, and meaningful (high-ritualistic) to those that are nonstructured and not symbolic (low-ritualistic). When new members do not perform these initiation activities as expected, they are often punished (i.e., hazed; up to 73% of fraternity and sorority members experience hazing; Allan & Madden, 2008).

Pilot Study A tests fraternity members' reactions to altering high (vs. low) ritualistic activities in their fraternity. We asked 35 members of a collegiate social fraternity to evaluate profiles of four fictional potential new members (which included generic information about the new members, including their names, hometowns, majors, and GPAs) in randomized order (n = 140 data points). Participants then learned that each member had not completed one of four initiation activities that we selected for either being highly ritualistic (performing the Creed and memorizing founders' names, two activities with strong psychological meaning and sequenced physical behaviors) or less ritualistic (attending brotherhood "signature dates" and completing study hours, two activities with relatively weaker psychological meaning and less specific physical behaviors). We hypothesized that participants would show greater moral outrage to the new members who did not complete the more (vs. less) ritualistic activities (preregistered at https://aspredicted.org/8n347.pdf).

Participants subsequently completed a survey with a manipulation check ("How much would you consider it a ritual in your fraternity to do [task]?" 1 = *not at all*, 7 = *extremely*). The survey measured *anger* with four items: how (a) angry, (b) irritated, (c) annoyed, and (d) frustrated they would feel if the new member did not perform the activity (1 = *not at all*, 7 = *extremely*; α = .95; Gino et al., 2012) and perceived *immorality* with four items: how (a) wrong, (b) inappropriate, (c) offensive, and (d) immoral it would be for the new member to not perform the activity (1 = *not at all*, 7 = *extremely*; α = .84; Gino & Galinsky, 2012). Anger and judgments of immorality were positively correlated, r = .41, p < .001, indicating that the participants' anger was moral outrage (e.g., Batson et al., 2009). To control for the possibility that members believe that they are more strongly expected to complete ritualistic activities, the survey asked, "To what extent is it expected for new fraternity members to do [task]?" (1 = *completely optional*, 7 = *completely expected*).

We analyzed results using mixed-effects modeling with random effects for responses within individuals (i.e., random intercept and random slope model; Brauer & Curtin, 2018). Supporting our manipulation, high-ritualistic tasks were rated as more ritualistic (M = 6.21, SD = 1.06) than low-ritualistic tasks (M = 3.57, SD = 1.95), $F(1, 69.44)$[3] = 86.42, p < .001, d = 1.68, even controlling for expectations, $F(1, 40.35)$ = 38.55, p < .001. As hypothesized, not completing a high-ritualistic initiation activity produced more anger (M = 3.67, SD = 1.78) and was perceived as more immoral (M = 2.61, SD = 1.39) than not completing a low-ritualistic activity (M = 2.68, SD = 1.51 and M = 2.11, SD = 1.19, Fn3

[3] Degrees of freedom vary and include decimals because we used Satterthwaite's approximation method for degrees of freedom (due to mixed linear models).

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Table 1
Overview of Studies

Study	Sample	Group ritual	Design	Dependent variable(s)	Moderation
Pilot A (N = 35)	Members of college fraternity	Initiation activities	Ritual (high or low) within-subjects	Moral outrage	None
Pilot B (N = 34)	Members of college fraternity	Initiation activities	Ritual Index (correlational)	Moral outrage and punishment	None
Study 1 (N = 107)	U.S. citizens	U.S. holiday celebrations	Ritual Index (correlational)	Moral outrage	None
Study 2 (N = 803)	U.S. adults	Workplace meeting	Ritual (high or low) × Descriptive norm (high or low) × Injunctive norm (high or low) between-subjects	Moral outrage	None
Study 3 (N = 186)	Jewish and Muslim participants	Religious circumcision	Religious identification (Jewish or Muslim) between-subjects × Ritual alteration (altered or unaltered) within-subjects	Moral outrage and punishment	None
Study 4 (N = 604)	U.S. citizens	U.S. Pledge of Allegiance	Ritual alteration (no-alteration, unknown-intent alteration, benevolent-intent alteration, ill-intent alteration, accidental alteration, or lacking-ability alteration) between-subjects	Moral outrage and punishment	1) Ingroup commitment 2) Belief that ritual represents group values
Study 5 (N = 2,444)	Jewish and non-Jewish participants	Jewish Passover Seder	Religious identification (Jewish or non-Jewish) × Alteration magnitude (zero, one, two, three, four, five, or six alterations) between-subjects	Moral outrage and punishment	1) Ingroup commitment 2) Belief that ritual represents group values

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respectively; anger: $F(1, 63.26) = 22.51$, $p < .001$, $d = 0.60$; immorality: $F(1, 34) = 6.30$, $p = .017$, $d = 0.39$).[4] Fn4

Next examining real alterations to initiation activities, Pilot Study B tests whether recalled alterations to highly ritualistic activities produce more moral outrage and punishment (i.e., hazing behavior) than less ritualistic activities (preregistered at https://aspredicted.org/2im9e.pdf). The same 35 fraternity members from Pilot Study A participated; a problem with the survey not loading left us with only 34 participants. Participants first recalled up to three instances of a new member failing to complete or perform an initiation task (M = 2.38 instances listed on average; n = 81 total observations).

Participants then completed a single-item face valid measure of *ritualism* ("How ritualistic is the task?" 1 = *not at all*, 7 = *extremely*). They also rated the extent to which the task is *physically ritualistic* on a nine-item scale that we developed based on the features of formality, stereotypy, redundancy (proposed by Tambiah, 1979), and specificity (Legare & Souza, 2012) that rituals contain. The scale asked participants to indicate "yes," "somewhat," or "no" to each of the following statements: (a) location specificity ("The task tends to occur at the same time during the new member period"); (b) procedural order specificity ("The task tends to occur in a fixed order"); (c) group member specificity ("The task tends to include the same individuals"); (d) time interval specificity ("The task tends to occur at a fixed interval [e.g., once a week, once every new member period], rather than sporadically"); (e) repetition of procedures ("The task features certain elements that tend to be repeated more than once [for example, high-fiving three times means there is a repetition, but high-fiving just once means there is no repetition]"); (f) stereotypy ("The task includes physical movements and/or utterances [e.g., chants, shouting, making a noise]"); (g) mimicry ("The task features mimicking [e.g., someone engages in a certain behavior and others follow by doing the same behavior]"); (h) synchrony of movements ("The task involves performing certain steps in unison [i.e., everyone completing the step(s) at the same time, in synchrony]"); and (i) communality ("The task feels social [feels like

[4] In addition, the more participants perceived an activity as ritualistic (using the single-item ritual measure), the angrier they felt when it was not completed, $\beta = 0.42$, $SE(\beta) = 0.06$, $t(66.46) = 6.93$, $p < .001$, and the more immoral it seemed not to complete it, $\beta = 0.36$, $SE(\beta) = 0.08$, $t(32.32) = 4.42$, $p < .001$. Participants believed that there were higher expectations to complete the high-ritualistic activities than the low-ritualistic activities, $F(1, 46.88) = 43.34$, $p < .001$, $d = 1.18$. However, controlling for expectations, ritual remained a predictor of anger, $\beta = 0.39$, $SE(\beta) = 0.08$, $t(9.52) = 4.95$, $p < .001$, and immorality, $\beta = 0.37$, $SE(\beta) = 0.11$, $t(16.52) = 3.39$, $p = .004$. In addition, controlling for ritual, expectations neither predicted anger, $\beta = 0.06$, $SE(\beta) = 0.07$, $t(30.36) = 0.84$, $p = .410$, nor immorality, $\beta = 0.00$, $SE(\beta) = 0.08$, $t(16.07) = 0.05$, $p = .959$. Thus, although the ritual manipulation was confounded with expectations, the hypothesized effect is robust when controlling for this confound.

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something that is shared by a group]"). We aggregated constituent items to yield a composite score (*yes* = 3, *somewhat* = 2, *no* = 1), such that higher scores reflect more physically ritualistic features (α = .61). Participants also rated the extent to which the task is *psychologically ritualistic* ("How meaningful is the task?" 1 = *not at all*, 7 = *extremely*).

We measured anger (α = .91) and perceived immorality (α = .82) with the same scales as in Pilot Study A, except we added "mad" to the anger scale (Gino et al., 2012). Anger and perceived immorality were positively related, $r = .49$, $p = .001$, indicating that the participants' anger was moral outrage. To assess the degree to which new members were punished for altering the initiation activity, we asked participants to indicate how psychologically challenging, embarrassing, physically demanding, and harsh the consequences that the new members received were (1 = *not at all*, 7 = *extremely*; α = .76; Keating et al., 2005).

To simplify analyses, we standardized and averaged the three ritual measures (ritualism, physically ritualistic, and psychologically ritualistic; α = .49; although note this index was not preregistered). Participants reported being angrier when more ritualistic tasks were altered, β = 0.23, $SE(\beta) = 0.09$, $t(63.81) = 2.49$, $p = .015$, and punished the newcomer more, β = 0.27, $SE(\beta) = 0.09$, $t(59) = 2.90$, $p = .005$, but did not perceive the alterations as significantly more immoral, β = 0.06, $SE(\beta) = 0.11$, $t(9.73) = 0.52$, $p = .614$.[5] Fn5

In aggregate, Pilot Studies A and B use a collegiate fraternity sample with ecologically valid manipulations to suggest that alterations to more (vs. less) ritualistic group activities can produce greater moral outrage and punishment behavior (although Pilot Study B only found that ritualism affected outrage and punishment, not morality). An implication of these studies is that altering rituals can have significant consequences for new fraternity members, increasing hazing (which can be life-threatening; Nuwer, 2001). Our next studies will address limitations of these pilot studies. For instance, the pilot studies use not participating in a ritual (e.g., not partaking in the Creed) as an instance of ritual violation. In Studies 1–5, we examine cases in which a ritual is clearly altered (e.g., changing the date or location of the ritual) and also use larger sample sizes (at least 100 participants or more per between-subjects condition) to increase the generalizability of our hypothesized effect.

Study 1: Celebrations of U.S. Holiday Rituals

Study 1 compares the effect of altering a more (vs. less) ritualistic event on moral outrage. Specifically, we asked U.S. citizens to rate how ritualistic each of 15 different holiday celebrations is and how morally outraged they would be if the holiday celebrations were altered. Our theory predicts that holiday celebrations that have more perceived ritualistic features would elicit more moral outrage when altered than holiday celebrations with fewer perceived ritualistic features. Because participants differ in ritualistic practices across holiday celebrations (e.g., some people may ritualize Earth Day but not Christmas, and vice versa), we utilize a cross-classified mixed linear model (e.g., Judd et al., 2017) to examine whether perceptions of ritual (regardless of the idiosyncrasies of individual holidays) are associated with moral outrage when the holiday is altered.

Moreover, our theory presumes that the two primary features of rituals—physical features (e.g., sequenced, repetitive) and psychological features (e.g., meaningfulness)—are distinct and independently predict anger and immorality. Thus, we explicitly test whether each of the two primary features of rituals independently predicts anger and immorality. Furthermore, to account for the potential confound that more ritualistic holidays could be more well-known or commonly celebrated, robustness analyses control for how frequently participants engaged in the holidays.

Method

We preregistered our analysis plan and hypotheses (https://aspredicted.org/gy66m.pdf).

Participants

We predetermined a sample of 100 participants. In total, 107 U.S. citizens recruited from a West Coast university pool (80 female, 25 male, two unreported; $M_{age} = 20.02$, $SD_{age} = 1.59$) agreed to participate in an in-person laboratory session in exchange for $12. Overall, each of the participants completed the predictor and outcome variables for 15 holidays, yielding a total of 1,605 observations.

Experimental Design

The study was a within-subjects design with 15 (holiday) conditions. AQ: 4

Procedure

We presented participants with 15 U.S. holidays in a randomized order. We selected the list of holidays from the official U.S. Department of State holidays (U.S. Information Agency of the U.S. Department of State, 2000): New Year's Day, Martin Luther King Jr. Day, Abraham Lincoln's Birthday, George Washington's Birthday, Earth Day, Mother's Day, Memorial Day, Flag Day, Father's Day, Independence Day, Labor Day, Columbus Day, Veteran's Day, Thanksgiving, and Christmas. For each holiday, participants first completed the predictor variable (the extent to which each holiday celebration constitutes a ritual). In addition, participants completed a control variable (the frequency of holiday participation). Afterward, we told participants to imagine that the U.S. government "moved celebrations for the holiday one week forward." We chose an alteration of one week (vs. one day) because some holidays are associated with particular days of the week (e.g., Thanksgiving is always on Thursday, Memorial Day is always on Monday) that produce 3- or 4-day weekends. Therefore, the alteration of one week isolates the effect of altering a ritual feature (and not extraneous factors such as vacation

[5] More thoroughly examining each separate measure of ritual (as preregistered), the single-item of ritualism marginally predicted anger, β = 0.16, $SE(\beta) = 0.09$, $t(65.21) = 1.70$, $p = .095$, and punishment, β = 0.32, $SE(\beta) = 0.09$, $t(60.38) = 3.74$, $p < .001$, but not immorality, β = −0.08, $SE(\beta) = 0.10$, $t(68.99) = -0.80$, $p = .426$. The physical features of ritual did not predict anger, β = 0.05, $SE(\beta) = 0.10$, $t(71.52) = 0.56$, $p = .580$, or immorality, β = −0.11, $SE(\beta) = 0.10$, $t(73.17) = -1.12$, $p = .267$, but did marginally predict punishment, β = 0.18, $SE(\beta) = 0.10$, $t(71.94) = 1.79$, $p = .078$. The psychological features of ritual predicted anger, β = 0.24, $SE(\beta) = 0.08$, $t(57.90) = 2.94$, $p = .005$, and immorality, β = 0.31, $SE(\beta) = 0.09$, $t(58.65) = 3.62$, $p < .001$, but not punishment, β = 0.06, $SE(\beta) = 0.09$, $t(56.67) = 0.68$, $p = .497$. There were no interactions between the psychological and physical features of rituals ($ps > .219$).

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convenience) on moral outrage. For instance, for New Year's Eve, participants were asked to imagine the U.S. government moved official celebrations (which traditionally occur on December 31st) to January 6th (i.e., one week forward) instead. Participants then completed outcome variables (self-reported intensity of anger and perceived immorality).

Materials (Survey)

Independent Variable: Ritual Elements. We measured perceptions of how ritualistic each holiday celebration is using the same method described in Pilot Study B. We collected one face-valid item of *ritualism* ("How much does this holiday [and its celebrations] feel like a ritual?"; 1 = *not at all*, 7 = *extremely*). We assessed how *physically ritualistic* the holiday was ($\alpha = .90$), adapting eight of the nine items used in Pilot Study B: (a) location specificity ("Do celebrations of this holiday typically occur at the same location each year?"); (b) time-of-day specificity ("Do celebrations of this holiday typically occur at the same time of day each year?"); (c) procedural order specificity ("Do celebrations of this holiday typically involve activities that occur in a fixed order?"); (d) group member specificity ("Do celebrations of this holiday typically involve the same individuals each year?"); (e) repetition of procedures ("Do celebrations of this holiday typically involve the same activities each year?"); (f) stereotypy ("Do celebrations of this holiday typically include physical movements and/or utterances—such as, chanting, shouting, singing?"); (g) synchrony of movements ("Do celebrations of this holiday typically involve performing certain steps in unison—i.e., everyone completing the step(s) [physical movements and/or utterances] at roughly the same time [such as singing "Happy Birthday," praying together, performing a ceremony together, and so on]?"); and (h) communality ("Are celebrations of this holiday typically social—i.e., they involve more than one person or a group of people?"), and how *psychologically ritualistic* it was ("Overall, how meaningful is the holiday?" 1 = *not at all*, 7 = *extremely*). We standardized and averaged these three measures of ritual (single-item ritualism, physically ritualistic, and psychologically ritualistic) to yield a single *ritual index* ($\alpha = .82$).[6] Fn6

Dependent Variable: Moral Outrage. We measured moral outrage at moving the holiday 1 week forward using the same five-item anger scale ($\alpha = .97$) and four-item immorality scale ($\alpha = .94$) described in Pilot Study B (1 = *not at all*, 7 = *extremely*). Anger and judgments of immorality were positively related, $r = .75$, $p < .001$, suggesting the anger was moral outrage.

Finally, to control for frequency of holiday participation, participants also indicated how often they celebrate each holiday (1 = *never*, 7 = *always*). Lastly, at the end of the survey, we collected demographics (age, gender, race, political orientation, religious belief, and year in college).

Results

To test our predictions, we conducted the same mixed-effects model described in Pilot Study B except that it included crossed random effects for individuals and items (holidays) because our study design involved ratings nested within items and individuals. Supporting our predictions, using the ritual index, participants reported being angrier when more ritualistic holiday celebrations were altered, $\beta = 0.30$, $SE(\beta) = 0.05$, $t(16.24)^7 = 5.77$, $p < .001$, and perceived the alteration as more immoral, $\beta = 0.25$, $SE(\beta) = 0.05$, $t(14.31) = 5.02$, $p < .001$. Figure 1 depicts the results aggregated to the holiday level. Fn7 F1

Further examining how physically and psychologically ritualistic participants perceived the holiday celebrations, we found that the more physically ritualistic, $\beta = 0.12$, $SE(\beta) = 0.04$, $t(32.15) = 3.00$, $p = .005$, and psychologically ritualistic, $\beta = 0.21$, $SE(\beta) = 0.05$, $t(17.48) = 4.39$, $p < .001$, a holiday celebration seemed, the angrier the participant was when the holiday celebration was altered. How physically ritualistic, $\beta = 0.11$, $SE(\beta) = 0.04$, $t(16.83) = 2.49$, $p = .024$, a holiday celebration seemed and how psychologically ritualistic, $\beta = 0.17$, $SE(\beta) = 0.04$, $t(16.52) = 4.24$, $p < .001$, it seemed were also each independently associated with greater perceived immorality when the holiday celebration was altered. Moreover, using the single-item ritual measure, participants reported being angrier when more ritualistic holiday celebrations were moved, $\beta = 0.14$, $SE(\beta) = 0.03$, $t(67.55) = 4.03$, $p < .001$, and perceived the move as more immoral, $\beta = 0.13$, $SE(\beta) = 0.03$, $t(88.79) = 4.48$, $p < .001$.

Furthermore, controlling for frequency of holiday participation, the association between the ritual index and anger was robust, $\beta = 0.23$, $SE(\beta) = 0.04$, $t(37.62) = 6.29$, $p < .001$, as was the association between the ritual index and immorality, $\beta = 0.22$, $SE(\beta) = 0.05$, $t(115.07) = 4.14$, $p < .001$. Additionally, the frequency of holiday participation moderated the association between the ritual index and anger, $\beta = 0.13$, $SE(\beta) = 0.03$, $t(59.38) = 4.57$, $p < .001$, but not the association between the ritual index and immorality, $\beta = 0.05$, $SE(\beta) = 0.03$, $t(72.61) = 1.47$, $p = .146$;[8] however, when including the interaction term, the direct association between the ritual index and anger, $\beta = 0.26$, $SE(\beta) = 0.04$, $t(21.37) = 6.74$, $p < .001$, and between the ritual index and immorality, $\beta = 0.23$, $SE(\beta) = 0.05$, $t(15.66) = 4.30$, $p < .001$, did not change. Fn8

Discussion

In Study 1, holiday celebrations that contained more ritualistic features, whether physical features such as specificity and repetition or psychological features such as meaningfulness, incited more moral outrage when altered. Physically and psychologically ritualistic features of holidays independently predicted moral outrage when altered, suggesting that the primary features of ritual are distinct, and our observed effects are not simply about one component of rituals. Furthermore, the association between ritual and moral outrage remains robust when controlling for frequency of holiday participation, suggesting our measure of ritual is not

[6] We did not preregister that we would combine the three measures of ritual to yield a single ritual index; we decided to create a standardized ritual index to follow our methodology in Pilot Study B. We also report the results individually for the three measures (psychological features, physical features, single-item ritual) as we preregistered in the main text.

[7] As in the pilot studies, the degrees of freedom vary in Study 1 and include decimals because we used Satterthwaite's approximation method for degrees of freedom (due to mixed linear models).

[8] One explanation for this pattern of results is that while anger is often provoked by appraisals of an offense against oneself (Horberg et al., 2011; Lazarus, 1991), judgments of immorality are provoked irrespective of the target of harm (Haidt et al., 1993; Schein & Gray, 2018). People who frequently participate in holiday celebrations may view alterations as immoral and experience anger because they perceive the alteration as an offense against themselves; in contrast, people who do not frequently participate in a holiday lack the experience of personal offense and thus may be less likely to react with anger. We return to differences between effects on anger and immorality in the Study 5 Discussion.

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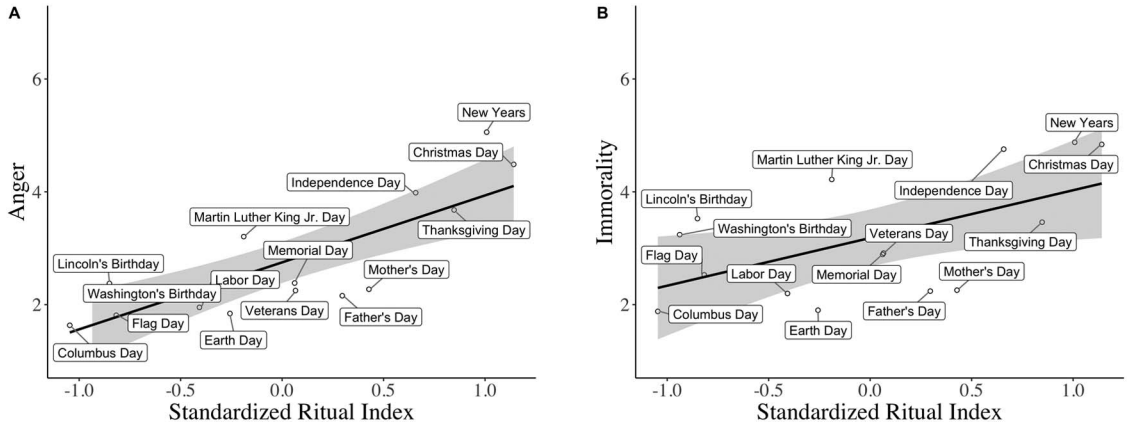
Figure 1

Holiday-Level Visualizations for the Relation Between the Standardized Ritual Index and Anger (Panel A) and Immorality (Panel B) in Study 1



Note. The gray region reflects 95% confidence interval bands.

merely capturing knowledge or popularity of the holiday. Moreover, the analytic approach (treating participants and holidays as random factors) suggests that the association between ritual perceptions and moral outrage is robust taking into account random variation due to the holidays sampled in our study (Judd et al., 2017).

Study 2: Workplace Rituals and Descriptive/Injunctive Norms

Extending from the correlational results of Study 1, Study 2 examines whether altering more (vs. less) ritualistic activities *causally* elicits greater moral outrage. Moreover, Study 2 tests our contention that rituals are more a form of an institutional norm (what individuals *must* do) than a descriptive norm (what individuals *actually* do) or an injunctive norm (what individuals ought to do). We therefore predicted that altering a ritual will have unique psychological consequences compared with altering non-ritualistic descriptive or injunctive norms. Because descriptive and injunctive norms can overlap in practice (e.g., Eriksson et al., 2015; Lindström et al., 2018), it can be difficult to independently manipulate and measure these constructs. As a result, we draw our manipulations from prior studies (e.g., Jacobson et al., 2011; Smith & Louis, 2010; Smith et al., 2012), manipulating the extent to which an activity is perceived as a ritual, a descriptive norm (i.e., how many people actually do it), or an injunctive norm (i.e., how many people believe it ought to be done; Cialdini et al., 1990, 1991; Cialdini & Trost, 1998; Schultz et al., 2007).

Method

We preregistered our analysis plan and hypotheses (https://aspredicted.org/sv83p.pdf).

Participants

We predetermined a sample of 800 participants. In total, 803 participants (358 female, 443 male, two unreported; M_{age} = 38.16, SD_{age} = 11.86) recruited from Amazon's Mechanical Turk agreed to participate in exchange for $0.50.

Experimental Design

The experiment used a 2 (ritual condition: high or low) × 2 (descriptive norm condition: high or low) × 2 (injunctive norm condition: high or low) between-subjects design.

Procedure

After successfully completing two attention checks (as preregistered, we excluded participants who failed one of these attention checks prior to the introduction of the experimental manipulation), participants imagined the following:

> Imagine that your work team engages in an important client meeting every Tuesday morning. You are close with your work team and have a clearly defined set of values in your team (e.g., supporting each other, providing constructive feedback, being honest, and so on). Your team sometimes performs a warm-up chant together before the meeting.

Participants then received our experimental manipulations for ritual (high or low), descriptive norm (high or low), and injunctive norm (high or low). Participants in the high or low *ritual condition* read the following information, respectively: "Each time your team does a warm-up chant, your team engages in a set of [formal, symbolic rites]/[informal, simple actions] that [must be performed in the same way by team members]/[can be performed in random ways by team members]. The warm-up chant is [highly]/[not very] meaningful and [represents your team's values]/[does not particularly represent anything]."

We adopted our descriptive and injunctive norm manipulations from prior research on norms (e.g., Jacobson et al., 2011; Smith & Louis, 2010; Smith et al., 2012). Participants in the high or low *descriptive norm condition* read the following information, respectively: "[Most]/[Few] of your team members (about [70%]/[30%]) actually engage in the warm-up chant before the client meeting."

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Participants in the high or low *injunctive norm condition* read the following information, respectively: "[Most]/[Few] of your team members (about [70%]/[30%]) believe that your team should do a warm-up chant before the client meeting." We presented the ritual manipulation either first or last (randomized), and the descriptive and injunctive norm manipulations in back-to-back randomized order. We opted for this survey design (instead of complete randomization) in order to make salient the differences between the injunctive and descriptive norm manipulations.

Next, participants completed the following five attention checks AQ: 5 in a randomized order: (a) "What percent of your team members believe that your team should do a warm-up chant?" (*injunctive-norm* attention check); (b) "What percent of your team members actually do the warm-up chant?" (*descriptive-norm* attention check); (c) "The warm-up chant: (1) is meaningful, (2) is not meaningful, (3) not sure" (*ritual-psychological-features* attention check); (d) "The warm-up chant: (1) is formal, (2) is informal, (3) not sure" (*ritual-physical-features* attention check); and (e) "The warm-up chant: (1) must always be done the same way, (2) can be done differently every time, (3) not sure" (*ritual-physical-features* attention check). Participants had to answer these questions correctly to advance to the next page; they had unlimited attempts to answer them correctly.

After completing manipulation checks (see below), participants learned that their team warm-up chant activity had been altered: "Imagine that your boss decides to ban team warm-up chants to save time." Participants then completed the rest of the survey.

Materials (Survey)

Manipulation Checks. Our manipulation check on the *ritual condition* read: "Rituals are predefined sequences of behaviors (or a single behavior) characterized by rigidity, formality, and repetition that are embedded in a larger system of symbolism and meaning. How much is a warm-up chant a ritual for your team?" (1 = *not at all a ritual*, 7 = *very much a ritual*). Our manipulation check on the *descriptive norm condition* read: "Descriptive norms are behaviors or attitudes that most people actually do or have in a given situation. How much is a warm-up chant a descriptive norm for your team?" (1 = *not at all a descriptive norm*, 7 = *very much a descriptive norm*). Our manipulation check on the *injunctive norm condition* read: "Injunctive norms are behaviors or attitudes that people believe others are supposed to do or have in a given situation. How much is a warm-up chant an injunctive norm for your team?" (1 = *not at all an injunctive norm*, 7 = *very much an injunctive norm*). We presented these items in randomized order.

Moral Outrage. We used the same scale from Study 1 to measure *anger* (asking how angry, mad, irritated, annoyed, and frustrated participants would be toward their boss; $\alpha = .97$) and *immorality* (asking how wrong, inappropriate, offensive and immoral was their boss's decision to ban team warm-up chants; $\alpha = .92$). Anger and perceived immorality were positively related, $r(801) = .76$, $p < .001$, suggesting the anger was moral outrage.

Plausibility Check. We additionally asked participants, "To what extent is the scenario you just read plausible?" (1 = *not at all*, 7 = *very much*).[9] Lastly, we collected demographics (age, gender, and race). Fn9

Results

We first examined whether our ritual, descriptive norm, and injunctive norm manipulations had their intended effect. Supporting our manipulations, the high-ritual team warm-up chant ($M = 6.10$, $SD = 1.23$) was perceived as more ritualistic than the low-ritual team warm-up chant ($M = 4.13$, $SD = 1.83$), $t(801) = 17.86$, $p < .001$, $d = 1.26$; the high-descriptive-norm team warm-up chant ($M = 5.15$, $SD = 1.39$) was perceived as more of a descriptive norm than the low-descriptive-norm team warm-up chant ($M = 3.74$, $SD = 1.62$), $t(801) = 13.21$, $p < .001$, $d = 0.93$; and the high-injunctive-norm team warm-up chant ($M = 5.19$, $SD = 1.31$) was perceived as more of an injunctive norm than the low-injunctive-norm team warm-up chant ($M = 4.39$, $SD = 1.64$), $t(801) = 7.58$, $p < .001$, $d = 0.54$. To be thorough, we further examined whether each manipulation influenced the other manipulation check items. The ritual manipulation also made the activity seem like more of a descriptive norm, $F(1, 795) = 37.57$, $p < .001$, $\eta_p^2 = .05$, and more of an injunctive norm, $F(1, 795) = 68.19$, $p < .001$, $\eta_p^2 = .08$. The descriptive-norm manipulation made the activity seem more like a ritual, $F(1, 795) = 12.22$, $p < .001$, $\eta_p^2 = .02$, although not more like an injunctive norm, $p > .201$. The injunctive-norm manipulation did not make the activity seem more like a ritual or descriptive norm, $ps > .248$.[10] Fn10

Consistent with our primary predictions, the boss's decision to alter the warm-up chant provoked relatively more anger ($M = 2.90$, $SD = 1.81$) and perceived immorality ($M = 2.60$, $SD = 1.62$) when the chant was more ritualistic than when it was less ritualistic ($Ms = 2.23$ and 2.15, $SDs = 1.49$ and 1.43), $ts(795) = 5.92$ and 4.25, $ps < .001$, $ds = 0.40$ and 0.29, respectively. The effect of ritual condition on anger and perceived immorality remained robust when controlling for the descriptive and injunctive norm manipulation in two 2 (high or low ritual) × 2 (high or low descriptive norm) × 2 (high or low injunctive norm) between-subjects ANOVAs (see Table 2). Moreover, the effect of ritual T2, AQ: 6 condition on anger and perceived immorality was not moderated by the effect of descriptive- or injunctive-norm condition, suggesting that the effect of how ritualistic the activity is on anger and immorality is statistically independent of the effects of how descriptively normative and injunctively normative it is (see Figure 2). F2

Additionally, controlling for the descriptive- and injunctive-norm manipulation and perceptions of the injunctive norm and

[9] A 2 (high or low ritual) × 2 (high or low descriptive norm) × 2 (high or low injunctive norm) between-subjects ANOVA on plausibility revealed a main effect of ritual, $F(1, 795) = 6.61$, $p = .010$, $d = -0.18$, such that the high-ritual condition ($M = 4.83$, $SD = 1.71$) was perceived as *less* plausible than the low-ritual condition ($M = 5.13$, $SD = 1.58$). There was also an interaction between descriptive norm and injunctive norm on perceived plausibility, $F(1, 795) = 7.20$, $p = .007$, $\eta_p^2 = 0.01$, such that in the high-descriptive norm condition, the low-injunctive warm-up chant was viewed as more plausible than the high-injunctive, $t(795) = 1.85$, $p = .065$, $d = 0.18$, but in the low-descriptive norm condition, the low-injunctive norm warm-up chant was viewed as less plausible than the high-injunctive, $t(795) = -1.95$, $p = .051$, $d = -0.19$. No other main effects or interactions were significant.

[10] Two interactions also emerged, a Descriptive × Ritual interaction on ratings of ritual, $F(1, 795) = 11.29$, $p = .001$, $\eta_p^2 = .01$, and a Descriptive × Injunctive interaction on ratings of the descriptive norm, $F(1, 795) = 3.56$, $p = .060$. All other interactions were nonsignificant.

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Table 2

Effects of Experimental Condition on Anger and Perceived Immorality in Study 2

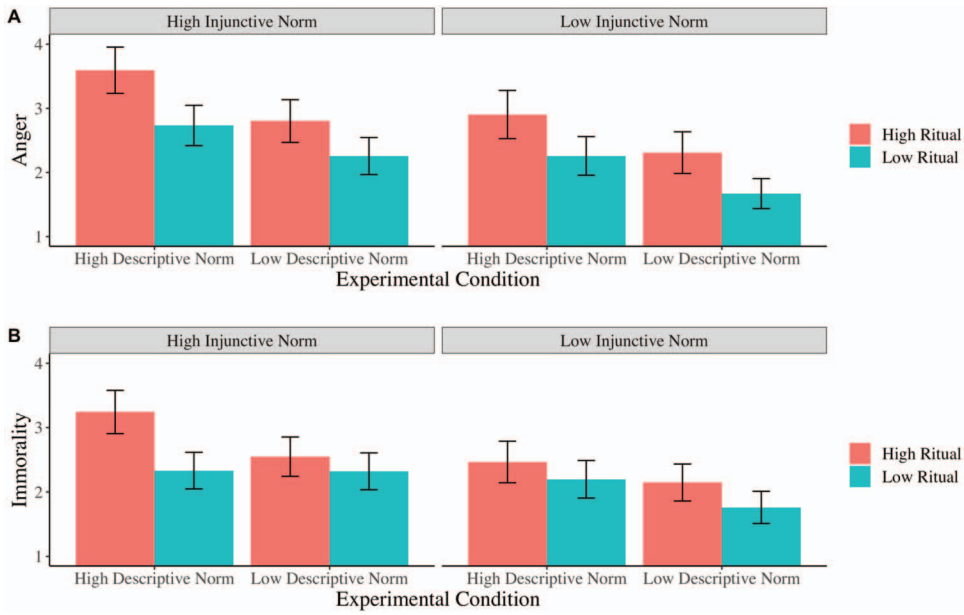
Predictor	Sum of squares	*df*	Mean square	*F*	*p*	η^2	η^2 95% CI [LL, UL]
A) Dependent variable: Anger							
Descriptive	74.92	1	74.92	28.92	.000	.04	[.01, .06]
Injunctive	62.97	1	62.97	24.31	.000	.03	[.01, .06]
Ritual	90.77	1	90.77	35.04	.000	.04	[.02, .07]
Descriptive × Injunctive	0.09	1	0.09	0.04	.850	.00	[.00, .00]
Descriptive × Ritual	1.27	1	1.27	0.49	.485	.00	[.00, .01]
Injunctive × Ritual	0.19	1	0.19	0.08	.784	.00	[.00, .01]
Descriptive × Injunctive × Ritual	1.20	1	1.20	0.46	.496	.00	[.00, .01]
Error	2059.50	795	2.59				
B) Dependent variable: Perceived immorality							
Descriptive	26.53	1	26.53	11.86	.001	.01	[.01, .04]
Injunctive	43.68	1	43.68	19.53	.000	.02	[.01, .05]
Ritual	40.23	1	40.23	17.99	.000	.02	[.01, .05]
Descriptive × Injunctive	0.04	1	0.04	0.02	.896	.00	[.00, .00]
Descriptive × Ritual	3.84	1	3.84	1.72	.190	.00	[.00, .01]
Injunctive × Ritual	2.96	1	2.96	1.32	.251	.00	[.00, .01]
Descriptive × Injunctive × Ritual	8.05	1	8.05	3.60	.058	.00	[.00, .02]
Error	1777.78	795	2.24				

Note. LL and UL represent the lower-limit and upper-limit of the partial η^2 confidence interval, respectively.

descriptive norm (i.e., ratings on the manipulation check items), there were still main effects of the ritual manipulation on anger, $t(797) = 4.54$, $p < .001$, and immorality, $t(797) = 2.99$, $p = .003$, suggesting the effects of the ritual manipulation hold when controlling for the descriptive- and injunctive-norm manipulation *and* ratings of the perceived descriptive and injunctive norm. Even when the group activity was seen as being a low descriptive and low injunctive norm, the boss's decision to alter the warm-up chant provoked relatively more anger ($M = 2.31$, $SD = 1.65$) and marginally greater perceived immorality ($M = 2.15$, $SD = 1.46$)

Figure 2

The Effect of Altering a Group Activity That Seemed More or Less Like a Ritual (High or Low), Descriptive Norm (High or Low), and Injunctive Norm (High or Low) on Anger (Panel A) and Perceived Immorality (Panel B) in Study 2



Note. The *y*-axis represents participants' survey responses on 7-point scales (endpoint labels reported in main text). Error bars represent the 95% confidence intervals around the mean. See the online article for the color version of this figure.

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when the chant was more ritualistic than when it was less ritualistic (Ms = 1.67 and 1.76, SDs = 1.18 and 1.26), ts(795) = 2.82 and 1.84, ps = .005 and .065, ds = 0.44 and 0.28, respectively.

Finally, replicating prior research on descriptive and injunctive norm violations, altering the warm-up chant provoked more anger (M = 2.86, SD = 1.77) and perceived immorality (M = 2.55, SD = 1.61) when it was more rather than less of a descriptive norm (Ms = 2.26 and 2.19, SDs = 1.55 and 1.45), ts(795) = 5.39 and 3.46, ps < .001, ds = 0.37 and 0.24, respectively. And altering the warm-up chant provoked more anger (M = 2.84, SD = 1.69) and perceived immorality (M = 2.61, SD = 1.56) when it was more rather than less of an injunctive norm (Ms = 2.29 and 2.14, SDs = 1.64 and 1.49), ts(795) = 4.94 and 4.44, ps < .001, ds = 0.33 and 0.31, respectively. Altering an activity that was seen as a high (vs. low) ritual produced no different anger and immorality than altering an activity seen as a high (vs. low) descriptive norm, ts(799) = 0.39 and 0.56, ps = .701 and .573, respectively, and no different anger and immorality than altering an activity seen as a high (vs. low) injunctive norm, ts(799) = 0.70 and −0.12, ps = .483 and .899, respectively (see Table 2 and Figure 2).

Discussion

Study 2 finds that altering a group activity with more (vs. fewer) ritualistic features (e.g., symbolism, physical sequence) causally produces more moral outrage. Increasing the degree to which an activity is ritualistic increased moral outrage even when controlling for how descriptively and injunctively normative it is. Thus, Study 2 provides initial support for our contention that ritual alterations are distinct from prior work on descriptive and injunctive norm alterations.

Study 3: Circumcision Ceremony Rituals

While Study 2 prioritized internal validity over external validity, Study 3 instead explores alterations to a meaningful, real-world group ritual: the male circumcision procedure. Study 3 tests whether group members feel greater moral outrage, and have more intent to punish, an ingroup member who recommends altering the circumcision procedure, even when the alteration is objectively beneficial (i.e., having it performed in a sterile hospital). Our theory predicts that even alterations conducted with a good intention (e.g., to improve safety) will provoke outrage.

Specifically, we tested reactions to the circumcision alteration among two groups of perceivers, Jews and Muslims, because they differently view how ritualistic the circumcision ceremony is. Although Judaism and Islam similarly require males to undergo circumcision (World Health Organization, 2008), and similarly see circumcision as being a sacred value, Jews have more sequenced and formal circumcision ceremonies (Brit Milah, also known as Bris) than Mulisms' ceremonies (Khitan), making their ceremonies more ritualistic (Doyle, 2005; Hollender, 2012; Rassbach, 2016). For instance, whereas Jewish circumcision is always performed on the eighth day of the infant's life, in Islam there is no fixed age for circumcision. Our theory predicts that because Jews view circumcision ceremonies as more ritualistic than Muslims do, Jewish (vs. Muslim) group members will feel relatively greater moral outrage and consequently recommend harsher punishment to ingroup members who advocate altering (vs. not altering) circumcision ceremonies.

Method

We preregistered our analysis plan and hypotheses (https://aspredicted.org/3bx6d.pdf).

Participants

We predetermined our sample size to recruit 100 participants in each of two between-subjects conditions. Per our preregistration, we excluded participants who were not religious, operationalized as scoring below a sum of 13 on the Centrality of Religiosity Scale (CRS; Huber & Huber, 2012).[11] In total, 186 adults[12] from Amazon's Mechanical Turk who were adequately religious and identified as Jewish or Muslim (N_{Jewish} = 90, N_{Muslim} = 96) participated in exchange for $1.10 (102 males, 83 females, one unidentified; M_{age} = 33.32, SD_{age} = 9.74). On average, Muslim participants reported higher religious identification (M = 21.68, SD = 2.94) than Jewish participants (M = 18.22, SD = 3.37), t(184) = 7.46, p < .001, d = 1.09.[13] Fn11, 12 Fn13

Experimental Design

The experiment used a 2 (religious identification: Jewish or Muslim) between-subjects × 2 (ritual alteration: altered or unaltered) within-subjects design.

Procedure

Participants rated the extent to which their group's circumcision ceremonies are ritualistic. Then participants learned that the U.S. government was considering passing a law that would require circumcision to be performed in a hospital by a medical professional. Participants viewed two Twitter posts in which an influential ingroup member either recommended passing the new law (altered condition) or not (unaltered condition; see Figure 3 for posts). After reading the post, participants completed our dependent measures as well as two questions to ensure they encoded the proposed alteration as beneficial. We counterbalanced the order of Twitter posts (altered or unaltered) between participants. F3

Manipulation Check: Ritual Elements. As in Pilot Study B and Study 1, to assess how *physically ritualistic* participants perceived the circumcision ceremonies to be, we asked participants to answer "yes," "somewhat," or "no" to each of the following statements: (a) time specificity ("Do [Bris/Khitan] ceremonies typically occur at the same age of each child?"); (b) procedural

[11] The CRS measures the general intensities of five core dimensions of religiosity: public practice ("How often do you take part in religious services?"), private practice ("How often do you pray?"), religious experience ("How often do you experience situations in which you have the feeling that God or something divine intervenes or allows for an intervention in your life?"), ideology ("To what extent do you believe that God or something divine exists?"), and frequency ("How often do you think about religious issues?") on a 5-point scale (1 = *never/not at all*, 5 = *more than once a week/very much so*; α = .72).

[12] Although we posted the MTurk HIT (human intelligence task) seeking 200 participants, we only received 186 responses on Qualtrics after MTurk said the HIT was completed. We believe some participants who did not pass the pre-screen filter submitted the HIT (and requested payment) without being eligible to complete the survey, which preemptively stopped the HIT.

[13] The statistical significance and direction of the results did not change when controlling for individual differences in participant religiosity (see online supplemental materials).

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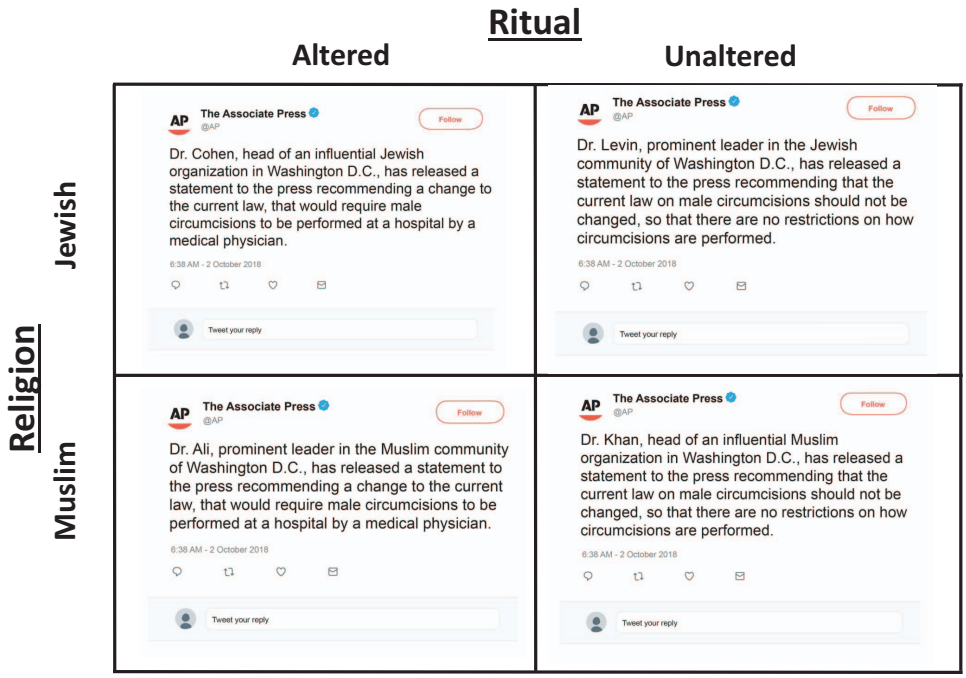
Figure 3
The Stimuli Used in Study 3



Note. We used two different titles and names for generalizability: Dr. [Levin/Cohen or Ali/Khan] is either "head of an influential [Muslim/Jewish] organization in Washington, DC" or a "prominent leader in the [Muslim/Jewish] community of Washington, DC" The titles and names of the target were counterbalanced. For copyright purposes, we changed the label of *The Associated Press* to *The Associate Press*. See the online article for the color version of this figure.

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order specificity ("Do [Bris/Khitan] ceremonies typically involve activities that occur in a fixed order?"); (c) group member specificity ("Do [Bris/Khitan] ceremonies typically involve the same individuals?"); (d) repetition of procedures ("Do [Bris/Khitan] ceremonies typically involve the same activities?"); (e) stereotypy ("Do [Bris/Khitan] ceremonies typically include physical movements and/or utterances—such as, chanting, shouting, singing?"); (f) synchrony of movements ("Do [Bris/Khitan] ceremonies typically involve performing certain steps in unison—i.e., everyone completing the step(s) [physical movements and/or utterances] at roughly the same time [such as praying together, performing a ceremony together, and so on]?"); and (g) communality ("Are [Bris/Khitan] ceremonies typically social—i.e., they involve more than one person or a group of people?"). We aggregated constituent items to yield a composite score (*yes* = 3, *somewhat* = 2, *no* = 1), such that higher scores reflect more ritualistic physical features ($\alpha = .78$).

Dependent Variables: Moral Outrage. We measured *anger* and *perceived immorality* using the scales described in Study 1 (αs = .97 and .93, respectively). Anger and immorality were positively related, $r = .87$, $p < .001$, indicating that participants' anger was moral outrage.

Punishment. We measured punishment with two measures. First, participants wrote a response tweet to the social media post using up to 240 characters: "Now, imagine that you have an opportunity to respond to the tweet on Dr. [Ali's/Cohen's/Levin's/Khan's] recommendation. Dr. [Ali/Cohen/Levin/Khan] recommended that the current law on male circumcisions [not] be changed, so that there are [no] restrictions on how circumcisions are performed. What would you tweet? Please respond within 240 characters." Two research assistants (blind to hypotheses) independently coded for agreement with the alteration recommendation along a 5-point scale (2 = *strong agreement*, 1 = *mild agreement*, 0 = *indifference*, −1 = *mild disagreement*, −2 = *strong disagreement*).[14] Responses that did not follow instructions were not coded; we excluded these observations from this analysis (35 of 372 responses, 9.4%). The two sets of coding showed reasonable reliability, $r(333) = .88$, $p < .001$, and we aggregated and reverse-scored them to make a measure of *disagreement* with the speaker. Second, participants rated the likelihood that they would reply to the social media posts with the following three *punitive messages*: "This recommendation is completely offensive"; "Inexcusable. Shame on this recommendation"; "I am horrified by this terrible recommendation" (1 = *not at all likely*, 7 = *extremely likely*; $\alpha = .95$). Fn14

Beneficial Alteration Check. To confirm that participants appraised the alteration as beneficial, we asked them to complete each of the following statements with either "at a hospital/clinic by

[14] This coding scale was not preregistered and was determined after the study was complete.

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a medical doctor" or "at a nonhospital setting by a nonmedical doctor": (a) safety ("It is safer for the child to be circumcised"); and (b) less risk ("There is less risk of complications from the circumcision").[15]

Lastly, we collected demographics (age, gender, race, [Jewish/Islamic] denomination, and political orientation).

Results

As expected, the Jewish participants rated their circumcision ceremony as more ritualistic ($M = 2.61$, $SD = 0.35$) than the Muslim participants ($M = 2.13$, $SD = 0.50$), $t(184) = 7.59$, $p < .001$, $d = 1.11$. Supporting our assertion that the alteration is beneficial, the majority of participants indicated the altered ceremony would be safer (83.9%), $\chi^2(1, N = 186) = 84.01$, $p < .001$, and less risky for the child (84.95%), $\chi^2(1, N = 186) = 89.47$, $p < .001$, than the unaltered ceremony. Importantly, statistically similar proportions were obtained when separately examining Muslim participants (safer: 83.33%; less risky: 87.50%) and Jewish participants (safer: 84.37%; less risky: 82.22%), indicating that *both* groups saw the alteration as objectively beneficial (i.e., no statistical difference between groups in assessments) of safety, $\chi^2(1, N = 186) = 0.00$, $p = 1.00$, or risk, $\chi^2(1, N = 186) = 0.64$, $p = .420$).

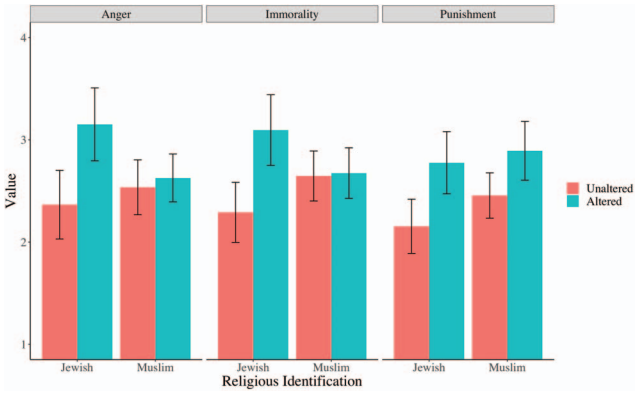
We analyzed the effects of the ritual alteration condition (altered or unaltered) and religious identification condition (Jewish or Muslim perceivers) on anger, perceived immorality and punishment (i.e., likelihood of sending punitive messages and disagreement with target) with 2 × 2 ANOVAs with random factors for participant. We found main effects of ritual alteration condition on anger, immorality, and sending punitive messages but not on disagreement (Fs = 6.15, 5.82, 14.55, and 1.24, ps = .014, .017, <.001, and .266, η_p^2 = 0.02, 0.02, 0.06, and 0.00, respectively) and no effects of religious identification condition on any of the four measures (Fs = 0.32, 0.02, 1.34, and 0.21, ps = .566, .878, .247, and .648, η_p^2 = 0.00, 0.00, 0.01, and 0.00, respectively). Testing our primary hypotheses, the interactions between alteration (altered or unaltered) and religious identification (Jewish or Muslim) were marginally significant on anger, significant on perceived immorality, nonsignificant on the likelihood of sending punitive messages, and significant on disagreement (Fs = 3.86, 5.04, 0.44, and 16.88, ps = .051, .026, .507, and <.001, η_p^2 = 0.01, 0.02, 0.00, and 0.05, respectively; see Figure 4).

Because our primary interest was in whether Jewish participants, as compared with Muslim participants, had stronger moral outrage and punished more in the altered versus unaltered conditions, we next decomposed the interactions. Among Jewish participants, altering the ceremony incited more anger, perceived immorality, disagreement, and sending of punitive messages (Ms = 3.12, 3.06, 0.10, and 2.74; SDs = 1.88, 1.82, 1.44, and 1.88, respectively) than not altering it (Ms = 2.33, 2.26, −0.77, and 2.12; SDs = 1.68, 1.60, 1.43, and 1.62, respectively), ts(184) = 3.09, 3.24, 3.65, and 3.11[16], ps < .002, d = 0.44, 0.47, 0.61, and 0.36. In contrast, among Muslim participants, the effect of altering the ceremony did not differ in anger and perceived immorality (Ms = 2.66 and 2.71; SDs = 1.92 and 2.00, respectively) from not altering it (Ms = 2.57 and 2.68; SDs = 1.76 and 1.70, respectively), ts(184) = 0.37 and 0.12, ps > .710, ds = 0.05 and 0.02. Altering the ceremony actually created *more agreement* (less disagreement) with the target ($M = -0.51$, $SD = 1.59$) than not altering it among Muslims ($M = -0.01$, $SD = 1.63$), $t(331) = -2.14$, $p = .033$, $d = -0.31$. Finally, altering the ceremony still increased Muslims' likelihood of sending punitive messages (altered vs. unaltered: Ms = 2.92 and 2.49; SDs = 2.09 and 1.77, respectively), $t(185) = 2.26$, $p = .025$, $d = 0.23$.

To test the pathway by which altering the ritual increased punishment, we computed two mediation models using the experimental manipulation as our predictor (altered = 1 vs. unaltered = 0), moral outrage (composite of anger and perceived immorality) as our mediator, and our two punishment measures as the dependent measures. Given the within-subjects nature of our data, we computed cluster-robust standard errors. Providing support for our predicted pathway, the models revealed an indirect effect through moral outrage (disagreement: $b = 0.293$, $SE = 0.113$, 95% CI [0.072, 0.514]; punitive messages: $b = 0.304$, $Boot\ SE = 0.127$, 95% CI [0.056, 0.552]).

Figure 4

The Effect of Religious Identification (Jewish or Muslim) and Ritual Alteration (Unaltered or Altered) on Anger, Perceived Immorality, and Participants' Reported Likelihood of Sending Punitive Messages (i.e., Punishment) in Study 3



Note. The *y*-axis represents participants' survey responses on 7-point scales (endpoint labels reported in main text). Participants' disagreement with the speaker is not depicted in this graph because it was measured on a different scale. Error bars represent the 95% confidence intervals around the mean. See the online article for the color version of this figure.

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Discussion

Using externally valid stimuli, Study 3 shows that religious Jews, who highly ritualize the circumcision ceremony, experience relatively more moral outrage toward and are more likely to punish—via stronger disagreement and more frequent punitive written messages—an

[15] We had participants complete the 44-item Big Five Inventory (John et al., 2008) as a filler between the moral outrage/punishment questions and the beneficial alteration check.

[16] The degrees of freedom for pairwise comparisons involving the disagreement dependent variable is 331 because (a) 35 of 372 self-written comments could not be coded because the participant did not follow the instructions (the decision was made by research assistants blind to condition and hypotheses); and (b) the model fit is singular (i.e., model overfitted), suggesting that the random factor for participant is not supported by the data (variance of random factor is nearly zero).

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ingroup member who advocates altering the circumcision ceremony than an ingroup member who advocates no alteration. In contrast, Muslims, who ritualize the circumcision ceremony less than Jews, did not experience greater moral outrage or intent to punish an ingroup member who advocated change. The stronger moral outrage among Jews emerged even though both Jewish and Muslim participants acknowledged the alteration was beneficial (i.e., safer and less risky for the child) at similar rates. These results suggest that even alterations to group activities that are seen as beneficial may still produce moral outrage and punishment when the activities contain ritualistic features, supporting our theoretical account.

The results further suggest that it is not simply that violating sacred values enhances moral outrage, but rather altering the ritualistic activities that are related to sacred values elicits moral outrage. Specifically, Jews and Muslims both consider circumcision to be a sacred value in their respective groups,[17] but because Jews (vs. Muslims) more strongly ritualize the circumcision ceremony, altering the ceremony elicits greater moral outrage among Jews than Muslims. Fn17

A remaining question from Study 3 is whether ingroup members would broadly endorse a wider and more generalizable set of punishments for a ritual alterer (beyond the punitive messages tested in Study 3). To examine this, we conducted Supplemental Experiment S2 in which Catholic participants ($N = 305$) watched a video of an ingroup member alter (vs. not alter) a Catholic ritual (the Sign of the Cross). After viewing the video, participants reported higher intent to make the ritual alterer complete unpleasant tasks on behalf of the church, such as clean toilets or scrub the kitchen floor (full details are available in the online supplemental materials). This suggests that perceivers are willing to endorse a wider set of punishments for someone who alters a ritual (at least compared with someone who does not alter it).

Study 4: U.S. Pledge of Allegiance Ritual

Study 4 examines how beliefs about different reasons *why* a person alters a ritual affect moral outrage in reaction to the alteration. We recruited a sample of U.S. citizens to tell us their reactions to an ingroup member altering the U.S. Pledge of Allegiance by not standing. The Pledge is an important U.S. ritual with predefined physical behaviors (i.e., standing up, facing the U.S. flag, putting one's right hand over one's heart, and publicly stating the pledge in unison) and symbolism (i.e., symbolizing U.S. values such as freedom and liberty). We provided four different reasons for why the ingroup member altered the pledge: trying to help the United States (benevolent-intent condition), trying to harm the United States (ill-intent condition), not being able to stand due to a medical condition (lacking-ability condition), and forgetting to stand (accidental condition). We also included two baseline comparison conditions: one in which the participant learned about the alteration but not the reason why it was altered (unknown-intent condition) and another in which there was no alteration.

Our theory predicts that, because alterations compromise groups' sacred values, any alteration—regardless of the intention behind it—will incite relatively greater moral outrage compared to no alteration. As a result, even a benevolent-intent alteration or an accidental alteration should provoke relatively more outrage than no alteration. To further examine the consequences of ritual alteration, we explore whether moral outrage also creates an intent to punish by assessing group members' intentions to ostracize people who alter their rituals.

Lastly, we assessed two factors that our account suggests should moderate the relation between ritual alteration and moral outrage: participants' ingroup commitment and beliefs that the ritual reflects their group's values. Because we contend that rituals reflect group values, we theorize that group members may perceive the ritual alteration as an infringement on and alteration to those group values. Therefore, people higher in group commitment should experience relatively more outrage when the Pledge is altered as compared with individuals who are less committed to the U.S. because more committed group members care more about the group's values which are being altered. Moreover, individuals who have stronger beliefs that the Pledge symbolizes U.S. values should experience relatively more outrage when the Pledge is altered as compared with individuals who have weaker beliefs because the former individuals are most likely to view the alteration as an affront to the group's values.

Method

We preregistered our analysis plan (https://aspredicted.org/7im7h.pdf).[18] Fn18

Participants

We predetermined our sample size to recruit 100 participants in each of the six experimental conditions; in total, 604 adults from Amazon's Mechanical Turk who identified as U.S. citizens (280 females, 324 males; $M_{age} = 36.89$, $SD_{age} = 11.64$) participated in exchange for $0.50.

Experimental Design

The experimental design was six between-subjects conditions: no-alteration, unknown-intent alteration, benevolent-intent alteration, ill-intent alteration, accidental alteration, and lacking-ability alteration.[19] Fn19

[17] A post-test on Prolific Academic of 99 Jewish males and 97 Muslim males ($M_{age} = 29.81$, $SD_{age} = 12.02$; preregistration: https://aspredicted.org/xf3db.pdf) showed that most Jews rated circumcision as sacred (92/99), as did most Muslims (93/97), with no differences between the groups, $\chi^2(1, N = 196) = 0.34$, $p = .558$. Controlling for individual differences in religiosity with the Centrality of Religious Scale (Huber & Huber, 2012) did not change the results, $t(193) = 0.49$, $p = .624$. To measure sacredness, we adopted a measure from Berns et al. (2012): "Imagine that another country with a different value system from [Jews/Muslims] offered to pay [Jews/Muslims] to stop practicing circumcision because they found circumcision to be distasteful. Do you think there is any dollar amount that [Jewish/Muslim] people would accept if it meant that they could never practice circumcision again and would need to disavow it and what it means entirely?" Participants were given two response options: "no" or "yes (specify amount in U.S. dollars)." We coded "no" as an affirmation that circumcision is a sacred value.

[18] Note that we only preregistered an analysis plan (no hypotheses).

[19] Our preregistration contained slightly different labels for the experimental conditions: unknown-intent alteration was called "no intent," benevolent-intent alteration was called "good intent alteration," ill-intent alteration was called "bad intent alteration," accidental alteration was called "mistake condition," and lacking-ability alteration was called "justified condition."

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Procedure

After completing an eligibility prescreen (to filter out non-U.S. citizens, per our preregistration), participants read a vignette about the United States President's State of Union address:

> Imagine that you are watching the U.S. President's State of the Union address on TV. The camera hovers over another U.S. citizen just like yourself. They are on a live video stream, as is everyone else in the room. As is typical, the ceremony starts with everyone in the room standing up to recite the Pledge of Allegiance.

In the no-alteration experimental condition, participants then read, "As no surprise, the U.S. citizen stands up while reciting the pledge, choosing to stand with everyone else in the room." In all of the alteration conditions, participants instead read, "Much to your surprise, the U.S. citizen stays seated while reciting the pledge, choosing not to stand with everyone else in the room." In the unknown-intent alteration, participants read no further information; in the benevolent-intent, ill-intent, accidental, and lack-ability alteration conditions, they were given a reason to explain why the U.S. citizen altered the ritual (see Table 3 for full text of reasons provided in these four experimental conditions). T3

Materials (Survey)

Manipulation Check. To measure perceived intention for the ritual alteration, participants in the five alteration conditions answered the following question: "To what extent did the U.S. citizen have a good intention when sitting down during the Pledge of Allegiance?" (1 = *very bad intention*, 7 = *very good intention*).

Moral Outrage. As in previous studies, we measured moral outrage with self-reported intensity of anger and perceived immorality. To measure anger, we asked participants how (a) angry, (b) mad, (c) irritated, (d) annoyed, and (e) frustrated they would feel toward the U.S. citizen (1 = *not at all*, 7 = *extremely*; $\alpha = .98$). To measure perceived immorality, we asked participants how (a) wrong—right, (b) inappropriate—appropriate, (c) immoral—moral, and (d) inoffensive—offensive the target's decision during the Pledge of Allegiance was (7-point bipolar scales, e.g., 1 = *very wrong*, 7 = *very right*; $\alpha = .92$). We reverse-scored these items such that higher numbers indicated stronger perceived immorality. Unlike in prior studies, in Study 4, we transformed the perceived immorality measure into a bipolar scale (continuum between two opposite end points) to reduce demand effects because it is possible that a negative unipolar scale could prompt participants to think that there is something wrong with the target's behavior. Anger and perceived immorality were positively related, $r = .61$, $p < .001$, indicating that the anger was moral outrage.

Punishment. To measure intent to ostracize, we asked participants to indicate the extent to which they would do the following if they saw the U.S. citizen in their community: (a) avoid—meet them, (b) ignore—acknowledge them, and (c) keep them at a distance—keep them close (7-point bipolar scales, e.g., 1 = *definitely avoid them*, 7 = *definitely meet them*; $\alpha = .92$; Ferris et al., 2008). We reverse-scored these items such that higher numbers indicated stronger intent to ostracize.

Individual Differences. To measure our predicted moderator, participants' commitment to the ingroup, we asked participants to rate their agreement with following four items: (a) "I identify as an American," (b) "I see myself as an American," (c) "I am glad to be an American," and (d) "I feel strong ties with Americans" (1 = *not at all*, 7 = *extremely*; $\alpha = .93$; Ellemers et al., 1997). To measure our other predicted moderator, beliefs that the ritual symbolizes the group's values, we asked participants to rate their agreement with the following two items: (a) "The Pledge of Allegiance stands for U.S. values" and (b) "The Pledge of Allegiance is symbolic and highly meaningful" (1 = *not at all*, 7 = *extremely*; $r = .86$, $p < .001$). Participants' commitment to their group and beliefs that the ritual symbolized group values were positively related, $r = .67$, $p < .001$.

Lastly, we collected demographics (age, gender, race, political orientation, household income) and years of U.S. citizenship ("How many years have you been a U.S. citizen?").

Results

The means and standard deviations for each experimental condition and dependent measure are shown in Table 4. T4, AQ: 8

We first examined whether our intentionality manipulation had its intended effect. The benevolent-intent and lack-ability conditions did not differ, $t(497) = 1.35$, $p = .176$, $d = 0.20$, and were

Table 3
Reasons for the Ritual Alterations Provided in Four of the Experimental Conditions in Study 4

Experimental condition			
Benevolent-intent alteration	Ill-intent alteration	Accidental alteration	Lack-ability alteration
You later hear from a friend that the U.S. citizen stayed seated during the Pledge because they are part of a new movement of citizens who are trying to make the pledge more inclusive to Americans with disabilities who may not be able to stand. The intent of the movement is to help America become a stronger nation.	You later hear from a friend that the U.S. citizen stayed seated during the pledge because they are part of a new movement of citizens who are protesting the pledge because they think that the U.S. has a problematic value system. The intent of the movement is to make America a weaker nation.	You later hear from a friend that the U.S. citizen stayed seated during the pledge because they forgot to stand.	You later hear from a friend that the U.S. citizen stayed seated during the pledge because they recently injured a leg, and the doctor ordered them to stay seated.

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Table 4

Participants' Ratings for Each Dependent Measure by Experimental Condition in Study 4

Dependent variable	Experimental conditions					
	No-alteration	Unknown-intent alteration	Benevolent-intent alteration	Ill-intent alteration	Accidental alteration	Lacking-ability alteration
Perceived (benevolent) intentions	N/A	3.97 (1.50)[a]	5.92 (1.42)[b]	3.89 (1.97)[a]	3.87 (1.30)[a]	5.62 (1.52)[b]
Anger	1.41 (0.99)[a]	2.55 (1.79)[b]	2.18 (1.91)[b]	3.11 (2.07)[c]	2.45 (1.72)[b]	1.64 (1.49)[a]
Perceived immorality	2.56 (1.25)[a]	4.08 (1.34)[c]	3.34 (1.56)[b]	4.40 (1.51)[c]	4.43 (1.30)[c]	2.49 (1.35)[a]
Ostracism	3.41 (1.04)[a,b]	3.73 (1.28)[a,c]	3.41 (1.34)[a,b]	4.25 (1.63)[d]	3.97 (1.24)[c,d]	3.21 (1.22)[b]

Note. The mean (standard deviation) are presented for each experimental condition. The superscripts reflect whether the value is statistically significantly different (at the $p = .05$ level) from the other values in the same row.

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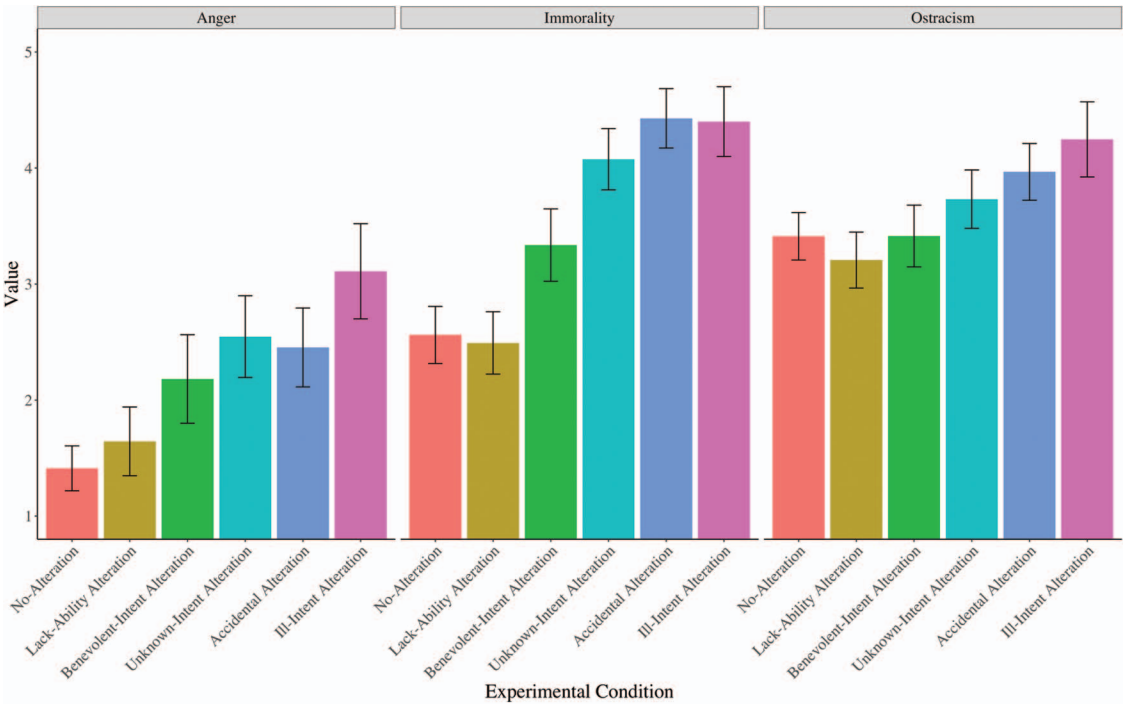
perceived as more positive than the ill-intent, unknown-intent, and accidental-alteration conditions, $ts(497) > 7.52$, $ps < .001$, $ds > 1.09$. The ill-intent, unknown-intent, and accidental-alteration conditions did not differ, $ts(497) < 0.45$, $ps > .650$, $ds < 0.07$.

We next examined the patterns of ratings across each dependent variable for the six experimental conditions (see Figure 5; one-way ANOVAs of experimental condition on anger, perceived immorality, and punishment: $Fs(5, 598) = 13.60, 40.77, \& 9.13$, $ps < .001$, $\eta_p^2 = 0.10, 0.25$, and 0.07, respectively). For anger, the ill-intent alteration condition produced the most anger, more so than any other experimental condition, $ts(598) > 2.36$, $ps < .019$, $ds > 0.29$. The unknown-intent alteration, benevolent-intent alteration, and accidental alteration conditions produced the next most anger (none significantly different from each other, $ts(598) > 1.52$, $ps > .128$, $ds < 0.20$), compared with the remaining conditions (no-alteration and lacking-ability alteration conditions, $ts(598) > 2.23$, $ps < .026$, $ds > 0.32$). The no-alteration and lacking-ability alteration conditions produced the least anger (no different from each other, $t(598) = -0.97$, $p = .331$, $d = -0.18$).

For perceived immorality, the ill-intent alteration, accidental-alteration, and unknown-intent alteration conditions were seen as most immoral as compared to all other conditions, $ts(598) > 3.77$, $ps < .001$, $ds > 0.51$, and no different from each other, $ts(598) < 1.80$, $ps > .071$, $ds < 0.27$. The benevolent-intent alteration condition was considered significantly less immoral than the three aforementioned conditions, but still more immoral than the no-alteration and lacking-ability alteration conditions, $ts(598) > 3.95$, $ps < .001$, $ds > 0.55$, which were considered

Figure 5

The Effect of Experimental Condition on Anger, Perceived Immorality, and Ostracism Intent in Study 4



Note. The *y*-axis represents participants' survey responses on 7-point scales (endpoint labels reported in main text). Error bars represent the 95% confidence intervals around the mean. See the online article for the color version of this figure.

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least immoral and no different from each other, $t(598) = 0.35$, $p = .726$, $d = 0.05$.

Finally, for ostracism likelihood, participants were most likely to ostracize targets in the ill-intent alteration condition (as compared to all other conditions, $ts(598) > 2.81$, $ps < .005$, $ds > 0.35$), yet similarly highly likely to do so in the accidental-alteration condition (not different from the ill-intent alteration condition, $t(598) = 1.52$, $p = .128$, $d = 0.19$). The likelihood of ostracizing in the accidental-alteration condition did not significantly differ from the unknown-intent alteration condition, $t(598) = 1.28$, $p = .200$, $d = 0.19$. As compared with the accidental-alteration condition, participants were relatively less likely to ostracize those in the benevolent-intent alteration, lacking-ability alteration, and no-alteration conditions, $ts(598) > 3.00$, $ps < .003$, $ds > 0.43$ (no different from each other, $ts(598) < 1.12$, $ps > .261$, $ds < 0.16$).

Mediation

To test the pathway by which altering the ritual increased punishment, we computed mediation models using the experimental manipulation as our predictor, moral outrage as the mediator (composite of anger and perceived immorality), and ostracism as the dependent measure. In particular, we created four indicator variables (ill-intent alteration = 1 vs. no-alteration = 0, accidental alteration = 1 vs. no-alteration = 0, unknown-intent alteration = 1 vs. no-alteration = 0, benevolent-intent alteration = 1 vs. no-alteration = 0) and conducted one mediation model per indicator variable with 5,000 bootstrapped samples. Providing support for our predicted pathway, the models revealed an indirect effect through moral outrage for ill-intent alteration versus no-alteration ($b = 1.058$, *Boot SE* = 0.181, LLCI = 0.730, ULCI = 1.425), accidental alteration versus no-alteration ($b = 0.655$, *Boot SE* = 0.153, LLCI = 0.396, ULCI = 0.978), unknown-intent alteration versus no-alteration ($b = 0.708$, *Boot SE* = 0.131, LLCI = 0.493, ULCI = 1.007), and benevolent-intent alteration versus no-alteration ($b = 0.353$, *Boot SE* = 0.103, LLCI = 0.167, ULCI = 0.559).

Moderation

The effect of intentionality on anger, perceived immorality, and ostracism intent was moderated by commitment to the United States, $Fs(5, 592) = 6.48$, 11.10, and 6.53, $\eta^2_{ps} = 0.05$, 0.09, and 0.05, such that higher commitment (+1 *SD*) was associated with a stronger effect of ill-intent alteration ($\beta s = 1.62$, 1.92, and 1.33, $ps < .001$), accidental alteration ($\beta s = 1.04$, 1.89, and 0.98, $ps < .001$), unknown-intent alteration ($\beta s = 1.01$, 1.56, and 0.74, $ps < .001$), benevolent-intent alteration ($\beta s = 0.79$, 1.00, and 0.42, $ps < .026$), and lack-ability alteration ($\beta s = 0.34$, 0.43, and 0.22, $ps = .064$, .010, and .252), whereas the effect of alteration was eliminated or attenuated for lower commitment (−1 *SD*; ill-intent alteration: $\beta s = 0.30$, 0.41, and −0.06, $ps = .089$, .009, and .745; accidental alteration: $\beta s = 0.12$, 0.46, and −0.14, $ps = .510$, .004, and .440; unknown-intent alteration: $\beta s = 0.28$, 0.37, and −0.23, $ps = .119$, .020, and .204; benevolent-intent alteration: $\beta s = 0.04$, −0.03, and −0.43, $ps = .836$, .843, and .027; lack-ability alteration: $\beta s = -0.06$, −0.46, and −0.48, $ps = .733$, .004, and .007; see Figure 6, Panel A).

Likewise, the effect of intentionality on anger, perceived immorality, and ostracism intent was also moderated by beliefs that the Pledge of Allegiance symbolizes U.S. values, $Fs(5, 592) = 13.07$, 20.33, and 9.19, $\eta^2_{ps} = 0.10$, 0.15, and 0.07, such that stronger beliefs that the Pledge of Allegiance symbolizes U.S. values (+1 *SD*) was associated with a stronger effect of ill-intent alteration ($\beta s = 1.75$, 2.07, and 1.40, $ps < .001$), accidental alteration ($\beta s = 1.16$, 1.97, and 0.96, $ps < .001$), unknown-intent alteration ($\beta s = 1.31$, 1.77, and 0.87, $ps < .001$), benevolent-intent alteration ($\beta s = 0.98$, 1.15, and 0.43, $ps < .017$), and lack-ability alteration ($\beta s = 0.35$, 0.40, and 0.10, $ps = .028$, .007, and .575), while the effect of alteration was eliminated or attenuated with weaker beliefs (−1 *SD*; ill-intent alteration: $\beta s = 0.20$, 0.22, and −0.14, $ps = .223$, .138, and .440; accidental alteration: $\beta s = -0.02$, 0.29, and −0.18, $ps = .915$, .056, and .333; unknown-intent alteration: $\beta s = 0.09$, 0.18, and −0.33, $ps = .597$, .235, and .070; benevolent-intent alteration: $\beta s = -0.22$, −0.30, and −0.50, $ps = .199$, .061, and .010; lack-ability alteration: $\beta s = -0.09$, −0.52, and −0.43, $ps = .600$, <.001, and .020; see Figure 6 Panel B).

Discussion

Study 4 demonstrates that altering a ritual for any reason other than lacking the ability to perform it properly led to relatively greater moral outrage—but not necessarily more punishment—as compared with performing the ritual correctly. Consistent with our theory, even well-intentioned or accidental alterations triggered relatively more outrage than no alteration. However, intention did matter, as an ill-intentioned alteration elicited greater moral outrage than a benevolent-intentioned alteration. These results indicate that lacking the ability to perform a ritual is a scope condition of our effect, while simultaneously highlighting the ubiquity of moral outrage for any other reason.

Beyond shedding light on how exactly the presumed reason for a ritual alteration influences reactions to the alteration, Study 4 also examined two theoretically motivated moderators of the effect of ritual alteration on moral outrage. First, participants who were more committed to their group (here, the United States) felt relatively more outraged by the alteration. Second, participants who believed that the ritual symbolizes the group's values (here, believing that the Pledge of Allegiance symbolizes U.S. values) were also relatively more outraged by the alteration. These two results support our theory that altering rituals creates moral outrage because the alteration compromises the ritual's perceived reflection of the group's values.

Study 5: Jewish Passover Ritual

Our theory holds that ritual alterations are construed as moral violations because rituals physically embody sacred group values. As a result, because sacred values are treated as non-negotiable and absolute (Baron & Spranca, 1997; Tetlock, 2003), even a minor ritual alteration can violate sacred values. Specifically, previous theorizing suggests that sacred values are insensitive to trade-offs, making individuals relatively attuned to whether or not a sacred value has been compromised and relatively insensitive to the degree to which the value is compromised (Baron & Spranca, 1997; Sachdeva & Medin, 2008). Building on this previous re-

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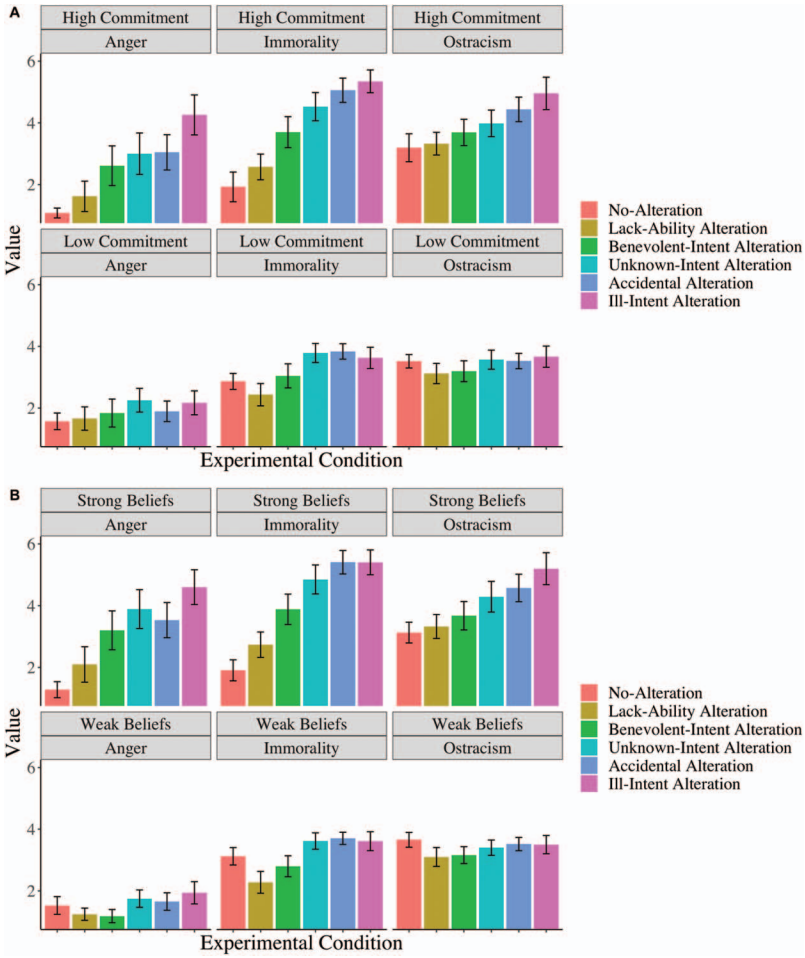
Figure 6

The Effect of Experimental Condition and Commitment to the U.S. (High or Low; Panel A) and Beliefs That the Ritual Symbolizes U.S. Values (Strong or Weak; Panel B) on Anger, Perceived Immorality, and Ostracism Intent In Study 4



Note. The *y*-axis represents participants' survey responses on 7-point scales (endpoint labels reported in main text). The moderators (continuous variables) were transformed into categorical variable (high or low commitment to the U.S., strong or weak beliefs) via the median split method for ease of visualization. Error bars represent the 95% confidence intervals around the mean. See the online article for the color version of this figure.

search, we predicted that the presence of an alteration (i.e., the first alteration) may be more consequential for moral outrage than each additional alteration (the "diminishing-marginal-impact" hypothesis). In Study 5, we compare our hypothesis against an alternative possibility that individuals will be linearly sensitive to the magnitude of the alteration, punishing commensurately more with each altered feature (i.e., the "constant-marginal-impact" hypothesis).

Specifically, we vary the extent to which a host of a Passover dinner, or Seder, alters the Seder plate ritual. Seder is a ritualistic meal conducted annually by Jewish people to retell the story of Passover. Each of six items on the Seder plate symbolizes one component of the story. In this study, the host either alters zero, one, two, three, four, five, or all six of the Seder items, thereby manipulating the magnitude of alteration to the ritual. To test between the aforementioned competing predictions, we examine whether the relation between the number of altered features and moral outrage follows a reciprocal functional form (i.e., the marginal effect of alteration *decreases* as the number of features altered increases, which would support our diminishing-marginal-impact hypothesis) or linear functional form (i.e., the marginal effect of alteration *remains at a constant rate* as the number of features altered increases, which would support the alternative constant-marginal-impact hypothesis).

In addition to examining ingroup members' (i.e., Jewish participants') reactions to the ritual alteration, we also examine the reactions of outgroup members (i.e., non-Jews). While we predict the relation between altered features and moral outrage follows a diminishing marginal impact relation for ingroup members, there

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should be no relation between ritual alteration and moral outrage for outgroup members who do not care about the ritual or group values at all.

Method

We preregistered our analysis plan and hypotheses (https://aspredicted.org/yq9vq.pdf).

Participants

We predetermined our sample size to recruit 200 participants in each of 14 experimental conditions. However, after running the experiment for 2 weeks, we encountered difficulties recruiting sufficient Jewish subjects from the Amazon Mechanical Turk subject pool. Without analyzing any data except for the question on participants' religious identification, we posted an amendment to our preregistration (https://osf.io/95nr7) and changed our stopping rule to recruit as many Jewish subjects as feasible during the following seven weeks. In total, 2,444 adults (N_{Jews} = 731, $N_{Non\text{-}Jews}$ = 1,713) from Amazon's Mechanical Turk (1,107 females, 1,324 males, 13 unidentified; M_{age} = 35.41, SD = 10.81) participated in exchange for $0.50.

Experimental Design

The experiment used a 2 (religious identification: Jewish or non-Jewish) × 7 (alteration magnitude: zero, one, two, three, four, five, or six) between-subjects design.

Procedure

First, participants read information about the Seder plate ritual and answered basic comprehension questions. Specifically, they read:

> The Jewish Seder retells the ancient story of Israel's redemption from bondage in Egypt. The word "Seder" means "order" in Hebrew, referring to the specific sequence of events in the Seder ritual, which centers around the Passover Seder meal. The Seder ritual serves to teach the lesson of Exodus, God's saving of the Jewish people from slavery. Tell us (in your own words) what you know about the six items on the Passover Seder plate: Maror, Z'roa, Charoset, Chazeret, Karpas, Beitzah.

We provided participants with a free response box and required them to write at least 25 characters about the Passover Seder in order to make salient their own understanding of the Jewish ritual. We told participants, "It is completely acceptable if you are unfamiliar with these items" and "There is no right or wrong answer" to this question. Next, participants imagined the following scenario:

> Imagine that you recently moved to an area and your neighbors invite you to attend a Passover Seder. You do not need to be Jewish to attend a Passover Seder—individuals of all faiths are welcome to attend and enjoy the festive meal. When you arrive at the house for Seder, you are greeted by the host, who will be leading the Seder. The host walks with you to the Seder table.

Depending on the assigned condition, participants then learned about the items on the host's Seder plate. In the zero-alteration condition, all of the items on the Seder plate were consistent with the Passover Seder ritual. Participants in the zero-alteration condition read the following (which shows the correct Seder plate items):

> The host's Seder plate contains the following items:
>
> 1. A roasted bone (Zeroah)
> 2. A hard-boiled egg (Beitzah)
> 3. Horseradish root (Marror)
> 4. Mixture of chopped apple, walnuts, and red wine (Charoset)
> 5. Sprigs of parsley (Karpas)
> 6. Romaine lettuce (Chazeret)

In the other alteration conditions, the host's Seder plate included one, two, three, four, five, or six altered items. We randomly selected common party food items to replace the traditional Seder plate items. Participants in the six-alteration condition read the following (which shows all of the alterations that we selected):[20] Fn20

> The host's Seder plate contains the following items:
>
> 1. Peanut butter (Zeroah)
> 2. Spaghetti (Beitzah)
> 3. Yogurt (Marror)
> 4. Chocolate chip cookie (Charoset)
> 5. Ice Cream (Karpas)
> 6. Cupcake (Chazeret)

To ensure that our effects would not be unique to the specific item altered, we created conditions for all possible combinations of alterations (i.e., zero-alteration = one condition, one-alteration = six conditions for each of the six possible items, two-alterations = 15 conditions, three-alterations = 20 conditions, four-alterations = 15 conditions, five-alterations = six conditions, six-alterations = one condition). For instance, in the two-alteration condition, the 15 conditions included all possible combinations of two alterations (e.g., one combination was peanut butter for Zeroah and spaghetti for Beitzah, another combination was yogurt for Marror and chocolate chip cookie for Charoset, and so on). Participants then completed several measures and were debriefed.

Materials (Survey)

Manipulation Check. We assessed the perceived magnitude of alteration with the following question: "How different was the host's Seder plate from the traditional Seder plate?" (0 = *com-*

[20] In Jewish tradition, there is a prohibition against mixing dairy and meat. We note that our dairy replacements (e.g., yogurt, ice cream) violate this tradition when the Zeroah is in its unaltered form (a roasted bone). This was an unintended oversight. To ensure that our effects would not be unique to the specific item altered, we created conditions for all possible combinations of alterations. Critically, even when the alterations do not include dairy items, we continue to observe our predicted effects, suggesting that our results are not solely due to concerns about dairy.

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pletely the same [*i.e., 0 items altered*], 6 = *completely different* [*i.e., 6 items altered*]).

Moral Outrage and Punishment. We measured anger and perceived immorality using the scales described in Studies 1–3 (αs = .98 and .96, respectively). Anger and judgments of immorality were positively related, $r(2,442) = .87$, $p < .001$, indicating that the anger was moral outrage. To measure intent to ostracize, we used the same scale from Study 4, but changed the Likert scale options to increase the variation in participant responses. Specifically, participants indicated the extent to which they would do the following if they saw the target in their community: (a) avoid them, (b) ignore them, (c) keep them at a distance, and (d) have nothing to do with them (1 = *not at all*, 7 = *extremely*; α = .98).

Individual Differences. We measured ingroup commitment (α = .91) and the perceived extent to which the ritual symbolizes the group's values, $r(728)$[21] = .61, $p < .001$, with scales described in Study 4, except we substituted "American(s)" or "United States" with "Jew(s)" or "Jewish." Although both Jewish and non-Jewish participants completed these measures, we are only interested in the responses from the ingroup members (i.e., Jewish participants) because outgroup members are not attached to ingroup values. Participants' commitment to Judaism and perceptions of the extent to which the Seder ritual represents Jewish values were positively correlated, $r(729) = .40$, $p < .001$.

Lastly, we collected demographics (gender, age, race, political orientation, religious belief) and asked an exploratory question on past Seder attendance ("Have you attended a Passover Seder before?"; yes, no).

Results

We first examined whether our alteration manipulation had its intended effect. There was a positive, linear relation between the number of items altered and the manipulation check item (perceived alteration) for both Jewish participants, β = 0.20, *SE*(β) = 0.03, $p < .001$, and non-Jewish participants, β = 0.12, *SE*(β) = 0.02, $p < .001$, but as expected, the relation was stronger for Jewish participants (interaction: β = 0.08, *SE*(β) = 0.04, $p = .040$). Furthermore, the reciprocal items altered term as well as the interaction between reciprocal items altered and Jewish dummy variable were not significant (*p*s > .135), suggesting that participant perceptions of the alteration manipulation followed a linear functional form (as we intended) rather than a reciprocal functional form.

To test between whether the relation between alteration magnitude and moral outrage followed a linear (i.e., x) or reciprocal (i.e., 1/x) functional form, we conducted the following models shown in Table 5. First, in three models (one for each respective dependent variable of anger, immorality, and ostracism, Table 5 Models 1, 4, and 7, respectively), we tested whether the linear (x) term for alteration magnitude predicted moral outrage, and whether the linear term was stronger for Jewish (vs. non-Jewish) participants. Next, in three models (Table 5 Models 2, 5, and 8), we tested whether the reciprocal (1/x) term for alteration magnitude predicted moral outrage, and whether the reciprocal term was stronger for Jewish (vs. non-Jewish) participants. Lastly, we tested a combined model (including both the linear and reciprocal terms and their respective interaction with Jewish identification; Table 5 Models 3, 6, and 9) to test whether the reciprocal term predicts moral outrage, controlling for the linear term. We hypothesized that the relation between alteration magnitude and moral outrage would follow a reciprocal (1/x) functional form (i.e., supporting the diminishing-marginal-impact hypothesis) and that this relation would be robust, controlling for the linear term.

In the three linear models, the results showed that the linear effect of ritual alteration on anger, immorality, and ostracism is stronger for Jewish participants, βs = 0.12, 0.14, and 0.06, *p*s < .002, than for non-Jewish participations, βs = 0.01, 0.04, and 0.00, *p*s = .340, <.001, and .789 (interactions: βs = 0.10, 0.10, and 0.06, *p*s < .011, respectively). In the three reciprocal models, the reciprocal effect of ritual alteration on anger, immorality, and ostracism is stronger for Jewish participants, βs = −0.87, −1.03, and −0.53, *p*s < .001, than non-Jewish participants, βs = −0.02, −0.24, and −0.01, *p*s = .821, .005, and .8892 (interactions: βs = 0.85, 0.79, and 0.52, *p*s <.001, respectively). Lastly, in the three combined models, the Jewish × Reciprocal interaction term is significant for anger, perceived immorality, and ostracism, βs = −0.75, −0.80, and −0.69, *p*s = .012, .006, and .023, respectively, whereas the Jewish × Linear interaction term is not significant for anger, perceived immorality, and ostracism, βs = 0.02, 0.00, and −0.03, *p*s >.511, respectively, which supports our hypothesized reciprocal form (i.e., the diminishing-marginal-impact hypothesis) and suggests that participants' sensitivity to the magnitude of the ritual alteration is relatively low.

Because nonlinear coefficients are difficult to interpret, Table 6 presents the average marginal effect (i.e., instantaneous rate of change) of the reciprocal alteration term at a range of plausible values. The marginal effects in Table 6 reveal that the average marginal effect of the reciprocal alteration variable is large and positive for Jewish participants, but rapidly decline as the number of alterations increases (e.g., for anger, the mean marginal effect begins at 0.53 and ultimately declines to 0.02). Thus, as the magnitude of the ritual alteration increases, Jewish participants experience moral outrage and punishment intent at a decreasing rate. In comparison, among non-Jewish participants, the mean marginal effects begin at 0.02 (anger), 0.24 (immorality), and 0.01 (ostracism) and decline to zero. Thus, non-Jewish participants experience minimal anger and punishment intent irrespective of the magnitude of the ritual alteration (see Figure 7). We note that non-Jewish participants do perceive the alteration as immoral but at a much lower rate than Jewish participants; this is something we did not predict and we consider further in the study's Discussion.

Mediation

To test the pathway by which altering the ritual increased punishment, we computed a mediation model using the experimental manipulation as our predictor (altered = 1, unaltered = 0), moral outrage as our mediator (composite of anger and perceived immorality), and ostracism as the dependent measure. Specifically, the alteration conditions (items altered: 1, 2, 3, 4, 5, 6) were collapsed into a single alteration condition ("1"), and we included both ingroup and outgroup members. Providing support for our predicted pathway, the model revealed an indirect effect through

[21] We did not force responses to these questions; as a result, two of the Jewish participants only answered one of the two questions.

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Table 5
OLS Regression Models in Study 5

	Dependent Variable								
	Anger			Immorality			Ostracism		
	(Model 1)	(Model 2)	(Model 3)	(Model 4)	(Model 5)	(Model 6)	(Model 7)	(Model 8)	(Model 9)
Jewish identification	−0.089 (0.096)	0.660*** (0.071)	0.554** (0.273)	0.095 (0.094)	0.780*** (0.070)	0.785*** (0.268)	0.020 0.097)	0.435*** (0.072)	0.611** (0.277)
Linear items altered	0.011 (0.012)		0.034 (0.023)	0.045*** (0.012)		0.062*** (0.023)	0.003 (0.012)		0.007 (0.023)
Linear items altered × Jewish identification	0.108*** (0.021)		0.017 (0.042)	0.096*** (0.021)		−0.001 (0.041)	0.056** (0.022)		−0.028 (0.043)
Reciprocal items altered		−0.019 (0.085)	0.188 (0.164)		−0.235*** (0.083)	0.141 (0.161)		−0.012 (0.086)	0.030 (0.166)
Reciprocal items altered × Jewish identification		−0.849*** (0.152)	−0.747** (0.298)		−0.794*** (0.149)	−0.801*** (0.292)		−0.515*** (0.154)	−0.686** (0.302)
Constant	−0.147*** (0.053)	−0.095** (0.039)	−0.308** (0.149)	−0.322*** (0.052)	−0.057 (0.038)	−0.443*** (0.147)	−0.085 (0.054)	−0.068* (0.040)	−0.111 (0.151)
Observations	2,444	2,444	2,444	2,444	2,444	2,444	2,444	2,444	2,444
R^2	0.042	0.043	0.045	0.078	0.077	0.081	0.017	0.019	0.019
Adjusted R^2	0.041	0.042	0.043	0.077	0.076	0.079	0.015	0.018	0.017
Residual std. error	0.979 (*df* = 2440)	0.979 (*df* = 2440)	0.978 (*df* = 2438)	0.961 (*df* = 2440)	0.961 (*df* = 2440)	0.959 (*df* = 2438)	0.992 (*df* = 2440)	0.991 (*df* = 2440)	0.991 (*df* = 2438)
F Statistic	35.806*** (*df* = 3; 2440)	36.542*** (*df* = 3; 2440)	22.798*** (*df* = 5; 2438)	69.078*** (*df* = 3; 2440)	68.200*** (*df* = 3; 2440)	43.172*** (*df* = 5; 2438)	13.670*** (*df* = 3; 2440)	15.833*** (*df* = 3; 2440)	9.581*** (*df* = 5; 2438)

Note. Jewish identification is a dummy variable (1 = Jewish, 0 = otherwise). We computed reciprocal items altered by taking the reciprocal (1/x) of items altered. We added one (+1) to items altered (0, 1, 2, 3, 4, 5, 6) such that the variable is as follows: 1, 2, 3, 4, 5, 6, 7. We shifted this variable upwards by one because, for the reciprocal (1/x) term, the 0 items altered condition results in an undefined number (when x = 0, then 1/x = 1/0). Standard errors are in parentheses. Values are standardized.
* $p < .1$. ** $p < .05$. *** $p < .01$.

moral outrage (b = 0.380, *Boot SE* = 0.078, LLCI = 0.224, ULCI = 0.530).

While the above mediation test was preregistered, we also conducted an exploratory moderated mediation analysis. Specifically, to test the pathway by which altering the ritual increased punishment especially for ingroup (vs. outgroup) participants, we computed a moderated mediation model using the alteration manipulation as our predictor (altered = 1 vs. unaltered = 0), religious identification as our moderator (Jewish = 1, non-Jewish = 0), moral outrage (index of anger and perceived immorality) as our mediator, and ostracism as the dependent measure. Providing support for our predicted pathway, the 95% confidence level for the estimate of the index of moderated mediation did not include zero, indicating a significant moderated mediation effect (Index = 0.990, *Boot SE* = 0.157, LLCI = 0.690, ULCI = 1.314). Consistent with our prediction, the conditional indirect effect of alteration on intent to ostracize was mediated through moral outrage among Jewish participants (b = 1.08, *Boot SE* = 0.116, LLCI = 0.847, ULCI = 1.30) but not among non-Jewish participants (b = 0.081, *Boot SE* = 0.107, LLCI = −0.130, ULCI = 0.288).

Moderation

To test whether Jewish participants who reported having a stronger commitment to Judaism and stronger beliefs that the Passover Seder is symbolic of group values were more likely to punish the target who altered their ritual, we regressed our outcome measures on commitment to Judaism, experimental alteration manipulation (altered = 1, unaltered = 0), and the interaction term (see Table 7). As in the mediational analyses, the alteration conditions (items altered: 1, 2, 3, 4, 5, 6) were collapsed into a single alteration condition ("1"). As expected, interactions emerged for anger, perceived immorality, and ostracism, Fs(1, 727) > 4.61, ps < .032; (see Table 7 for full models), suggesting commitment to Judaism and beliefs about symbolic value moderated the relation between ritual alteration condition and anger, perceived immorality, and ostracism.

Follow-up analyses using simple slopes (Aiken & West, 1991) clarified the nature of the interactions. Supporting our first moderation prediction, there was a stronger effect of alteration among persons higher (+1 *SD*) on commitment to Judaism (anger: β = 0.86, $p < .001$; immorality: β = 0.99, $p < .001$; ostracism: β = 0.71, $p < .001$) while the effect was attenuated among persons lower (−1 *SD*) on commitment to Judaism (anger: β = 0.40, p = .002; immorality: β = 0.46, $p < .001$; ostracism: β = 0.24, p = .063). Supporting our second moderation prediction, there was a stronger effect of alteration among persons who more strongly (+1 *SD*) believe the Passover ritual represents Jewish values (anger: β = 0.96, $p < .001$; immorality: β = 1.03, $p < .001$; ostracism: β = 0.68, $p < .001$) than among persons with weaker (−1 *SD*) beliefs (anger: β = 0.30, p = .021; immorality: β = 0.41, p = .001; ostracism: β = 0.25, p = .067).

Discussion

Study 5 demonstrates that the relation between the magnitude of ritual alteration and moral outrage and punishment follows a diminishing marginal impact relation for ingroup (but not outgroup) members. In other words, ingroup members' moral outrage was more sensitive to the first alteration (which violates sacred

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Table 6
Mean Marginal Effects in Study 5

Condition	Dependent variable: Anger		Dependent variable: Immorality		Dependent variable: Ostracism	
	Jewish participants	Non-Jewish participants	Jewish participants	Non-Jewish participants	Jewish participants	Non-Jewish participants
Zero alterations	0.86	0.02	1.03	0.23	0.53	0.01
One alteration	0.22	0.00	0.26	0.06	0.13	0.00
Two alterations	0.10	0.00	0.11	0.03	0.06	0.00
Three alterations	0.05	0.00	0.06	0.01	0.03	0.00
Four alterations	0.03	0.00	0.04	0.01	0.02	0.00
Five alterations	0.03	0.00	0.03	0.01	0.01	0.00
Six alterations	0.02	0.00	0.02	0.00	0.01	0.00

Note. Marginal effects measure the expected instantaneous rate of change in the dependent variable as a function of a change in the explanatory variable. Conceptually, marginal effects provide an approximation of the amount of change in the dependent variable that is expected to be produced by a very small change in the explanatory variable. Analytically, the marginal effect of an explanatory variable is the partial derivative with respect to explanatory variable of the prediction function f.

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values) than to the magnitude of alteration, consistent with our theory that alterations to group rituals provoke outrage because rituals represent sacred group values. These results help to explain why group rituals have strikingly consistent features over time: because even small alterations to rituals are met with outsized punishments.

Supporting results from prior studies, moral outrage statistically accounted for the relationship between ritual alteration and punishment intent among ingroup members. Commitment to the ingroup and the belief that the ritual symbolizes ingroup values moderated the relation between ritual alteration and moral outrage and punishment, such that individuals who were more committed to Judaism and strongly believed that the Passover ritual symbolizes Jewish values experienced the most outrage. These results further support our theory that ritual alterations are moral violations because they challenge the group's sacred values.

Finally, Study 5 demonstrates that the effect of ritual alteration on moral outrage is stronger among ingroup than outgroup members, supporting our theory that ritual alterations pose a unique moral violation to ingroup members, not outgroup members, because outgroup members do not share the group's values. Surprisingly, outgroup members did perceive the ritual alteration as immoral, although to a lesser degree than ingroup members did. We observed a similar disconnect between the effect of a ritual alteration on anger and immorality in Study 1; one possible explanation for this pattern of results is that anger stems from perceived offense against oneself (Horberg et al., 2011; Lazarus, 1991), whereas moral judgments stem from perceptions of harm—irrespective of the target of harm (Haidt et al., 1993; Schein & Gray, 2018). Thus, outgroup members may perceive altering another group's ritual as harm directed at that group, invoking judgments of immorality, without experiencing anger or the desire to punish because the moral violation is not relevant to them or their ingroup.

General Discussion

Group rituals abound across the world. As times change, people sometimes try to update these rituals, whether it be the Catholic Church modifying the language used in Mass (as in the opening example of the current paper) or American athletes kneeling on one knee during the U.S. national anthem (as famously exemplified by American football player Colin Kaepernick).[22] We demonstrate in seven studies that people who advocate alterations to more (vs. less) ritualistic group activities—including minor alterations that are accidental, well-intentioned, or beneficial to the group (e.g., increasing safety)—provoke relatively more moral outrage and punishment from ingroup (but not outgroup) members than people who advocate alterations to less ritualistic group activities. These more negative reactions to attempts to alter group rituals help to explain why group rituals have strikingly consistent features over time. Fn22

Our article uses a variety of different rituals as stimuli, from cultural rituals (e.g., U.S. holidays) to religious rituals (e.g., Catholic, Jewish, and Muslim rituals) to organizational rituals (e.g., collegiate fraternity initiation rituals). In all of our studies, when group activities contained more ritualistic elements, such as being more symbolic, rigid, or repetitive, their alterations elicited more moral outrage and provoked more punishment than when they contained fewer ritualistic elements. Moreover, moral outrage was particularly pronounced among individuals who were more strongly committed to the group in which the ritual originated and among those who more strongly believed that the ritual symbolizes group values. Such individuals are likely the stalwarts of the groups' rituals, applying the most punishment to people who attempt to alter them.

Theoretical Contributions

Our research makes at least four theoretical contributions. First, our research broadens the scientific understanding of the many forms that social norms can assume and the consequences of altering such norms. Prior research shows that people react negatively when others alter descriptive norms (how others typically act) or injunctive norms (how they ought to act; Chudek & Henrich, 2011; Helweg-Larsen & LoMonaco, 2008; Kam & Bond,

[22] https://web.archive.org/web/20180905033553/https://www.nytimes.com/2018/09/04/sports/colin-kaepernick-nfl-anthem-kneeling.html.

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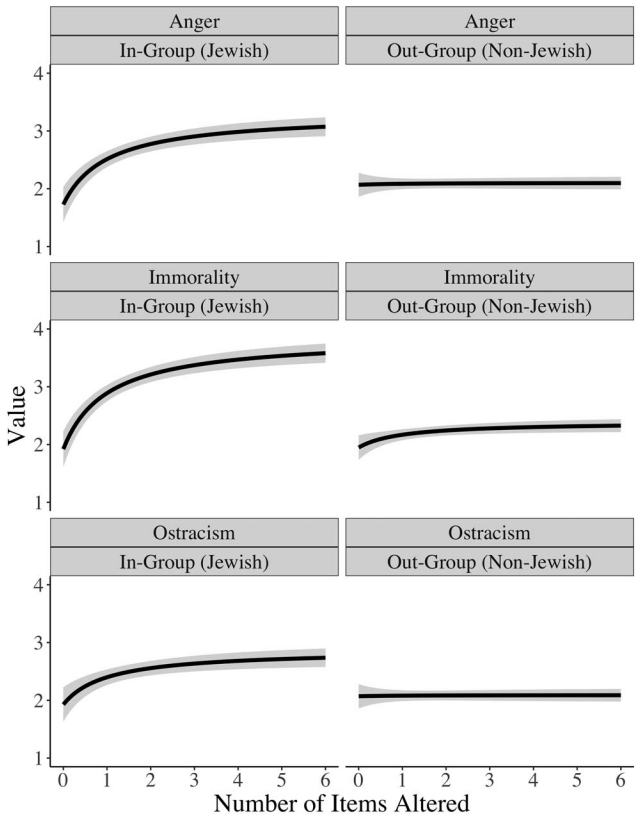
Figure 7

The Effect of the Experimental Alteration (Seven Conditions: 0, 1, 2, 3, 4, 5, or 6 Item(s) Altered) and Religious Identification (Two Conditions: Ingroup [Jewish] or Outgroup [Non-Jewish]) on Anger, Perceived Immorality, and Ostracism Intent in Study 5



Note. To visualize the effect of the reciprocal (1/x) term, we smoothed the linear model with the following formula: y ~ I(1/x). The *y*-axis represents participants' survey responses on 7-point scales (endpoint labels reported in main text). The gray regions are 95% confidence bands for the regression lines.

2009; Ody-Brasier & Vermeulen, 2014; Rakoczy & Schmidt, 2012; van Kleef et al., 2015). We extend this previous research by examining reactions to altering rituals, which are institutional norms (how people *must* act; Atran & Ginges, 2012; Morris, 2020; Morris, Chiu, et al., 2015; Morris & Liu, 2015). We empirically demonstrate one form in which institutional norms are manifested: sacrosanct group rituals, wherein the physical features of ritual *must* be performed the same way whenever the ritual is performed. Unlike altering descriptive and injunctive norms, altering rituals induces not only negative reactions but also moral outrage because they physically represent sacred group values.

Moreover, our theory and findings highlight many other ways in which ritual alterations differ from descriptive and injunctive norm alterations. Previous work has theorized that individuals punish norm violators to sustain cooperation in large groups (e.g., Chudek & Henrich, 2011; Gelfand et al., 2011; Rakoczy & Schmidt, 2012) or because norm-violating ingroup members harm the group's image (e.g., Marques et al., 1998), but does not explain why alterations that bring benefits to the group are punished (Studies 3–4) or why the relation between the magnitude of alteration and outrage follows a nonlinear pattern (Study 5). If anything, prior theories predict the opposite: that well-intentioned norm-violators can be viewed positively (e.g., Bellezza et al., 2014; Gino, 2018; Sijtsema et al., 2009; van Kleef et al., 2012; van Kleef et al., 2011;) and that the punishment of norm-violators increases commensurately with the severity of the violation (e.g., Eriksson et al., 2017; Fehr & Gachter, 2000, 2002; Gürerk et al., 2006). Uniquely explaining the current article's results, our theoretical account proposes that (a) even ritual alterations that benefit the group can trigger relatively more moral outrage because rituals represent sacred group values and (b) even minor alterations to rituals will be viewed as consequential violations of the rituals' sacredness.

Second, we contribute to growing empirical interest in understanding the psychological consequences of performing rituals. Whereas prior research demonstrates that individual rituals can causally enhance feelings of control (Norton & Gino, 2014; Tian et al., 2018), alleviate anxiety (Brooks et al., 2016), and enhance consumption experiences (Vohs et al., 2013), relatively less experimental research examines the consequences of group rituals. Performing group rituals together (usually synchronously) is generally associated with benefits for the individuals who perform them as well as for the group itself (Fischer et al., 2013; Reddish et al., 2014; Reddish et al., 2013; Sezer et al., 2016; Watson-Jones & Legare, 2016; see Hobson et al., 2018, or Stein et al., 2020, for recent reviews), even when the rituals are extreme or involve pain (Xygalatas et al., 2013). Certain types of group rituals, such as greeting rituals, have also been found to signal and consequently produce cooperation (Schroeder et al., 2019). Moving beyond these prior findings, the current paper instead examines consequences of a group ritual being performed "incorrectly"—that is, with deviations from its usual performance.

Third, our work contributes to the understanding of "sacred values" (values that a community treats as possessing transcendental significance; Tetlock, 2003). Previous research has noted that what is deemed sacred can vary dramatically across groups (e.g., Atran et al., 2007; Ginges et al., 2007; Morris, Chiu, et al., 2015; Tetlock & Fincher, 2015) and that people protect their sacred values in at least three ways (taboo trade-offs, forbidden base rates, and heretical counterfactuals; Tetlock et al., 2004; Tetlock et al., 2017). Here, we integrate prior theorizing suggesting that rituals represent group values (Rossano, 2012; Watson-Jones & Legare, 2016) and that group values are sacred (Atran & Ginges, 2012; Ellemers, 2017; Sheikh et al., 2012; Sosis & Alcorta, 2003) to extend the sacred value protection model. Specifically, we find that ritualizing mundane group behaviors—via embedding them in a social system with behavioral specificity—can make them take on sacred properties. Because rituals represent sacred group values, our work suggests group members view ritual alterations as an alteration to the very values the ritual serves to represent. Thus, altering group activities become increasingly "forbidden" the more that group activities are ritualized.

Fourth, we add to research examining the impact of group membership on the psychology of moral judgment (e.g., Ellemers, 2017; Ellemers et al., 2019; Gao et al., 2016; Mitkidis et al., 2017; Pagliaro et al., 2011). Although the importance of morality has been emphasized for the functioning of social communities at a

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Table 7

Regression Results for the Moderating Role of Jewish Commitment and Beliefs That the Passover Ritual Symbolizes Jewish Values in Study 5

	Dependent variable					
	Anger		Immorality		Ostracism	
	(1)	(2)	(3)	(4)	(5)	(6)
Jewish commitment	−0.073 (0.086)		−0.048 (0.084)		−0.134 (0.087)	
Ritual symbolic of Jewish values		−0.129 (0.090)		−0.057 (0.088)		−0.107 (0.092)
Alteration	0.627*** (0.101)	0.630*** (0.100)	0.723*** (0.099)	0.723*** (0.098)	0.475*** (0.103)	0.462*** (0.102)
Jewish Commitment × Alteration	0.230** (0.094)		0.264*** (0.092)		0.235** (0.096)	
Ritual Symbolic × Alteration		0.327*** (0.098)		0.309*** (0.096)		0.215** (0.100)
Constant	−0.539*** (0.094)	−0.541*** (0.092)	−0.622*** (0.092)	−0.619*** (0.090)	−0.411*** (0.095)	−0.397*** (0.094)
Observations	731	731	731	731	731	731
R^2	0.070	0.084	0.106	0.121	0.038	0.038
Adjusted R^2	0.066	0.081	0.102	0.117	0.034	0.034
Residual Std. Error (df = 727)	0.966	0.959	0.948	0.940	0.983	0.983
F Statistic (df = 3; 727)	18.266***	22.341***	28.623***	33.343***	9.481***	9.472***

Note. Regression results for the moderating role of Jewish commitment and beliefs that the Seder ritual symbolizes Jewish values on the relation between experimental condition (unaltered = 0 vs. altered = 1) and anger, perceived immorality, and ostracism in Study 5. Standard errors are in parentheses. Regression values are standardized.

* $p < .1$. ** $p < .05$. *** $p < .01$.

theoretical level (e.g., Graham & Haidt, 2010; Haidt & Kesebir, 2010), empirically, the intragroup context is often not considered in the study of morality. Studies often examine isolated phenomena by relying on a specific paradigm, such as the trolley problem (e.g., Greene et al., 2001), to assess a particular behavior (e.g., cheating in laboratory; Bazerman & Gino, 2012), trait-level characteristics (e.g., empathy, honesty), or emotion (e.g., guilt). In particular, research on how group-level variables such as group values impact individual moral judgment and behavior is scarce; in a comprehensive review of morality research published from 1917 to 2017, Ellemers et al. (2019) found that only 5.6% of published studies on moral judgments examined intragroup dynamics. Here, we study intragroup contexts, and their moral ramifications, using group rituals.

Limitations and Future Directions

Our current studies are not without limitations, which we think also point to directions for future research. First, we obtained our results from controlled experiments conducted in the laboratory or online. Although this approach enables us to make strong inferences about causality, future research may benefit from exploring the consequences of altering rituals in more naturalistic settings. Relatedly, levels of moral outrage measured in our experiments were relatively low (often below the scale midpoint). It could be that participants imagine ritual alterations that they assume are not real and that hence do not elicit strong reactions; in real-world settings, people may experience greater moral outrage. Future work could alter rituals in field settings and measure anger in other ways beyond self-reports (e.g., physiological signals).

Second, future research could identify additional moderators and boundary conditions of the effect of ritual alteration. Identifying conditions in which altering a ritual does not trigger moral outrage and punishment carries practical utility for leaders of groups and organizations who desire to alter their group rituals. We consider multiple moderators in the current article: commitment to one's group, beliefs about rituals symbolizing group values, ingroup versus outgroup membership, and the ritual-alterer's intentions. There are many potential moderators we did not test that might be interesting to study. For one, our studies included instances in which the ritual alteration was instantiated as both changing a ritual and as not performing a ritual, finding that both not performing and changing a ritual provoke more outrage compared to correctly performing or not changing the ritual, respectively. Future research could examine whether altering some aspect(s) of a ritual provokes a different emotional reaction than not performing a ritual at all. A second potential moderator that we did not test is whether the effect of ritual alteration on moral outrage is larger for religious versus secular group rituals. In related work, Legare and Souza (2012) found that the presence of a religious icon in a ritual can increase the perceived efficacy of the ritual. Our theory would suggest that the effect of alteration is larger when the ritual represents group sacred values (which is often the case with religious rituals).

Third, future research could examine which specific features of ritual amplify or mitigate outrage when the ritual is altered. One promising avenue would be based on Whitehouse's (2002) theory that religious rituals systematically vary in their expression via different "modes" (see Whitehouse & Lanman, 2014). The imagistic mode is characterized by low frequency-high arousal rituals (e.g., violent initiation rituals), while the doctrinal mode is characterized by high frequency-low arousal rituals (e.g., weekly religious prayer). Across our studies, we find alterations to both low frequency-high arousal rituals (e.g., Pilot Studies A and B fraternity initiation; Study 3, Jewish circumcision ceremony) and high frequency-low arousal rituals (e.g., Study 4, U.S. Pledge of Allegiance) can trigger moral outrage, but did not directly compare the magnitude of the outrage. Future research could test whether group members are relatively more outraged when someone alters their

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low frequency-high arousal rituals or high frequency-low arousal rituals. As the repetitive nature of high frequency-low arousal rituals leads people to turn "on autopilot" and "go through the motions" (Whitehouse, 2002, pp. 299–300), reducing the likelihood that group members attach the meaning of their groups' values to the ritual, our theory predicts high frequency-low arousal (vs. low frequency-high arousal) rituals will produce relatively less outrage when altered.

Fourth, future research could also explore whether and why ritual alterations by ingroup leaders versus followers invoke different levels of moral outrage. As a preliminary test of whether the hierarchical rank of the ritual-alterer influences moral outrage, we conducted Supplemental Experiment S3 (N = 599). This experiment manipulated whether the U.S. Pledge of Allegiance ritual was altered by a high-status group member (U.S. senator) or low-status member (U.S. citizen), and showed that the ritual-alterer's status did not influence moral outrage, nor did it interact with the effect of ritual alteration on outrage (full details available in the online supplemental materials). However, we think this question warrants further investigation: It still may be possible that someone who is not only high status but also considered a ritual expert in the group (e.g., the Catholic Pope) would be afforded greater ability to alter the group's rituals without provoking the same level of negative reaction from ingroup members. It is also likely that authority figures (e.g., the U.S. government; Study 1) have greater moral standing to alter some rituals (e.g., Memorial Day) more than others (e.g., Christmas).

A final direction for future research could be to consider practical implications for group and organizational leaders who attempt to alter their group's unique rituals. Indeed, it has been well-documented that change and innovation in organizations can be difficult to implement for a host of reasons (Blau & Scott, 1962; Crozier, 1964; Merton, 1957). Our research offers one explanation for why people become attached to the status quo, pointing to the possibility that employees may view change as morally outrageous when organizational rituals and traditions reflect organizational values. In such a way, our studies further indicate a possible idea for making change more palatable: Making organizational activities feel less ritualistic may decrease the moral outrage provoked when they are altered. For instance, leaders may be able to dissociate the physical performance of an organizational activity from its values, which would then make the activity feel less ritualistic.

Conclusion

Rituals are ubiquitous and long-lasting in groups, despite regular and consistent changes to society, emerging technologies, and economic activity. To date, little experimental research has examined the psychological underpinnings of this unique group behavior. The strikingly consistent features of group rituals over time suggest there might be something psychologically distinct about rituals that keeps them immutable. Across seven studies, we show that group members who alter group rituals invoke stronger moral outrage and punishment from other group members than those who do not alter the rituals, even when the alterations are minor or beneficial. Furthermore, altering group activities with more (vs. fewer) ritualistic features provokes more moral outrage and punishment. This outrage is amplified among individuals who are most committed to their group and see the ritual as most strongly symbolizing the group's values. Our work highlights the sacrosanct nature of rituals, demonstrating when ritual alterations become moral violations.

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