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When Novel Rituals Lead to Intergroup Bias: Evidence From Economic Games and Neurophysiology

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Abstract
Long-established rituals in preexisting cultural groups have been linked to the cultural evolution of group cooperation. We tested the prediction that novel rituals—arbitrary hand and body gestures enacted in a stereotypical and repeated fashion—can inculcate intergroup bias in newly formed groups. In four experiments, participants practiced novel rituals at home for 1 week (Experiments 1, 2, and 4) or once in the lab (Experiment 3) and were divided into minimal in-groups and out-groups. Our results offer mixed support for the hypothesis that novel rituals promote intergroup bias. Specifically, we found a modest effect for daily repeated rituals but a null effect for rituals enacted only once. These results suggest that novel rituals can inculcate bias, but only when certain features are present: Rituals must be sufficiently elaborate and repeated to lead to bias. Taken together, our results offer modest support that novel rituals can promote intergroup bias.

Keywords
ritual, intergroup dynamics, intergroup bias, cooperation, neural reward processing, open data, open materials, preregistered

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Collective rituals pervade the social world and define human cultures, religions, and groups (Durkheim, 1912/1915). These behaviors have a distinct evolutionary history and continue to pervade modern social life; long-standing theory suggests that rituals play a role in the maintenance of groups (Rappaport, 1999). Yet these omnipresent behaviors offer an evolutionary puzzle: Why do people willingly engage—often repeatedly and over a lifetime—in a series of effortful, often onerous, behaviors with seemingly no direct payoff to themselves?

Research in experimental psychology, anthropology, and evolutionary biology suggests that rituals are instrumental for the development of cooperative groups (Henrich, 2009; Sosis, 2000). To date, research has generally taken a broad sociocultural approach, investigating preexisting rituals (but see Wen, Herrmann, & Legare, 2016). However, because rituals exist within a cultural context and bring to mind long-standing aspects of group life, an examination of preexisting rituals limits the extent to which researchers can make causal claims about the effect of rituals. To circumvent this problem, we created novel rituals, implemented them in minimal groups, and then observed their effect on intergroup behavior and underlying neural processes. First, we assessed whether even novel rituals—conducted with novel groups—can be sufficient to inculcate intergroup bias. Second, we varied two features of these novel rituals—elaborateness and repetition—to explore which elements are critical for rituals to exert their effects.

Ritual and Group Function

A puzzling feature of rituals is that they pervade human culture despite the considerable costs incurred from their regular performance. Game-theoretical approaches (e.g.,

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Henrich, 2009) address this puzzle by suggesting that shared rituals help large groups survive by acting as a bulwark against the free-rider problem. Theories of costly signaling suggest that rituals serve as a credible public signal, advertising people's beliefs and intentions (Atran & Norenzayan, 2004). Taking the time to master a group's ritual (e.g., enduring the attendant physical and psychological pains and committing time and energy) makes being rejected by the affiliated group particularly undesirable (Alcorta & Sosis, 2005). That is, ritual increases group success through honest signaling of loyalty.

Indeed, rituals observed in the field have been linked to increased group cohesion. For instance, males belonging to religious Israeli kibbutzim, which are marked by increased communal ritual practices and synagogue attendance, show more cooperation toward in-group members than do males belonging to secular kibbutzim (Sosis & Ruffle, 2003). Field research investigating variations in rituals' costliness has shown that more effortful rituals lead to greater group cooperation and more prosocial behaviors (Xygalatas et al., 2013), and that high-intensity rituals result in the synchronization of autonomic physiological activity between performers and observers (Konvalinka et al., 2011). Moreover, ethnographic evidence suggests that collective rituals are associated with effective group functioning, linking ritual to a group's ability to withstand social collapse (Sosis & Bressler, 2003; Tuzin, 2001).

However, these benefits do not come without costs: Greater affiliation with in-group than with out-group members can have negative consequences for out-group members in various group settings. Intergroup competition plays a critical role in the cultural evolution of intragroup cooperation (Gurerk, Irlenbusch, & Rockenbach, 2006); game theory suggests that in-group altruism is a stable strategy only when coupled with out-group hostility (Choi & Bowles, 2007; Sääksvuori, Mappes, & Puurtinen, 2011). Supporting these theories, ethnographic studies on religion and ritual show that heightened intergroup conflict and out-group hatred are linked to greater in-group commitment to sacred values and engagement in more effortful and costly group rituals (Ginges, Atran, Medin, & Shikaki, 2007). For example, the amount of time invested in ritualistic group prayer predicts support for suicide attacks (Ginges, Hansen, & Norenzayan, 2009).

Novel Ritual and Overview of the Experiments

This largely correlational research is consistent with our primary hypothesis: Rituals galvanize in-group solidarity, but with costs to members of out-groups. However, because real-life rituals are imbued with culture, history, and preexisting meaning, parsing the causal effect of rituals is challenging. Without controlling for these broad social and group variables, the causal influence of ritual cannot be isolated. Thus, we used novel rituals—behaviors that were created in the lab and devoid of historical meaning and culture—to investigate the psychology of ritual and its effects on intergroup bias. Although rituals can vary widely in their expression (e.g., Whitehouse, 2002), we operationalized ritual at its most basic level, defining it as a sequence of repeated or stereotypical actions that have no instrumental causal link to a desired outcome.

Thus, the first goal of our research was to create both novel groups and novel rituals to assess the causal effect of rituals on intergroup bias. Our experimental design was intended to maximize the "ritual" aspect of the experience while minimizing the "group interaction" aspect, so that we could control for any effect of group interaction in order to draw causal inferences about the specific effect of rituals on intergroup bias. Our second and equally important goal was to explore which features of rituals are required to instill intergroup bias. We examined the role of two features common to many group rituals—effort and repetition, or time (Durkheim, 1912/1915; Tambiah, 1979; Xygalatas et al., 2013)—by varying each experimentally.

In Experiment 1, we investigated the effects of novel rituals on intergroup bias by assigning some participants to enact a ritual at home for 1 week before a laboratory session in which they were assigned to novel groups using a minimal-groups paradigm (Tajfel, 1974); we used a behavioral measure of economic trust to assess intergroup bias (similar measures have been used in related work on ritual; Xygalatas et al., 2013). In Experiment 2, we assessed whether a ritual's level of elaboration and effort moderates its effect on bias; similarly, in Experiment 3, a preregistered experiment, we explored the effect of an even more minimal form of novel ritual that required very little repetition and time commitment. Experiment 4 explored neural correlates associated with the effect of ritual on intergroup bias. We assessed whether novel rituals influence neural processing related to the evaluation of other individuals' behaviors; activation of this neural system would suggest a candidate proximal mechanism that might explain how rituals influence intergroup bias.

Experiment 1

Participants

One hundred seven introductory-psychology students at the University of Toronto Scarborough participated in this experiment for course credit. All were informed that they could earn additional, bonus money depending on their decisions during the experiment; in the end, all participants received the same $10 bonus. Participants were randomly assigned to either a ritual condition or a no-ritual (control) condition, and participants in both conditions underwent a minimal-group manipulation (Tajfel, 1974). Seven participants in the ritual condition were excluded from analyses because they failed to comply with the assigned at-home portion of the experiment (see the Procedure section). None of the participants expressed suspicion regarding the role of confederates (see the Procedure section). The final sample consisted of 100 participants (25 male, 75 female; mean age = 18.8 years, $SD = 1.57$; ritual condition: $n = 42$; no-ritual condition: $n = 58$).

Our target sample size was determined using an a priori power analysis (G*Power; Faul, Erdfelder, Lang, & Buchner, 2007), which assumed a medium effect size (characteristic of most social psychological findings). Specifically, with an assumed d of 0.4 (Richard, Bond, & Stokes-Zoota, 2001), our mixed design could achieve 80% power with as few as 72 participants, given a modest ($r = .3$) correlation between our repeated dependent measure. We decided to collect data until the end of the term, knowing that we would be able to recruit and collect data from at least 72 participants by that time.

Procedure

The experiment comprised an at-home portion and an in-lab portion over the course of 1 week. At the beginning of the at-home component, participants in both conditions estimated the number of dots in a series of images, as in the classic minimal-group paradigm (Tajfel, 1974). The classic manipulation typically occurs at one time in the lab, but in this case, participants were not told immediately about their arbitrary assignment to groups (i.e., groups of "underestimators" and "overestimators" based on responses in the dot-counting task). Participants were led to believe that their estimates were being recorded in a data file, which was then ostensibly used a week later in the lab to determine the minimal groups.

In the ritual condition, participants were given instructions to learn and memorize a set of actions (see Table 1 for the specific steps). To motivate participants to complete the actions over the course of the week, we led them to believe that the actions were thought to be part of an ancient cultural practice and that the purpose of the experiment was to understand the relationship between this action sequence and various cognitive processes. The instructions read as follows:

> *The purpose of this study is to connect people with ancient culture, and to demonstrate the effects of these cultures' various practices. As part of this, you will be asked to learn a shortened version of one of these ancient action sequences and to master it over the course of the week. We will send you daily emails, which will help serve as a reminder for you to perform the action sequence.*

The 2-min-long, ad hoc ritual comprised a series of actions that included raising the hands above the head and in front of the body, bowing the head, and opening and closing the eyes. Our operationalization of ritual was designed to mimic the physical features of real-life ritual behavior, namely, repetitive and highly sequenced movements, clear start and end times, and set rules and guidelines (e.g., participants were told that they should complete the sequences exactly in this way; Norton & Gino, 2013). Note that there was no mention of "ritual" during the weeklong study; the word *ritual* itself is imbued with preconceived notions of the psychological effects of rituals. In addition to receiving the written action sequence, participants were told to watch a video of a model displaying the full set of actions. Participants were asked to learn and memorize the sequence over the course of the week.

All participants were sent e-mail reminders every day during the at-home portion of the experiment. Participants in the ritual condition were reminded to perform the actions and, on the first 3 days, were provided with the video as an aid. Each day, they were asked to complete a survey following the completion of the action sequence. In order to equate time spent, we asked participants in the no-ritual condition to answer filler questions related to cognitive processing for the same amount of time that completing the ritual and filling out the survey was expected to take in the ritual condition. Compliance with the at-home procedure was tracked by accessing participants' surveys and determining whether they had been completed in full. Given that the surveys for participants in the ritual condition included instructions to perform the action sequence, we inferred that participants who completed the surveys had in fact done so. Low-compliance participants (those who completed the actions or survey fewer than three times over the 7 days) were removed from the final analysis ($n = 7$ in the ritual condition, $n = 0$ in the control condition). Low compliance was also confirmed during a funnel debriefing, when participants in the ritual condition were asked to report the number of times they had completed the actions over the course of the week ($M = 4.97$).

At the end of the week, participants completed the in-lab portion of the experiment in groups of 4—3 participants and 1 confederate. They were informed that the experiment was a group experiment; to create the minimal groups, the experimenter—seemingly using the saved data from participants' dot-counting responses earlier in

Table 1. The Three Instructions for Action Sequences That Were Given to Participants in the Ritual Conditions

Simple ritual (Experiment 2)	Intermediate ritual (Experiments 1 and 3)	Elaborate ritual (Experiments 2 and 4)
1. First, close your eyes and take three slow, deep breaths. Upon each exhale, bow your head and make a sweeping motion away from your body using your arms and hands.	1. To start, take five deep breaths with your eyes closed, and bring your focus to rest on the sequences about to be performed. Gently bow your head, close your eyes, and make a wiping motion with your hands away from your body. Finish with your arms resting at your sides.	1. Choose two different coins, either a dime, nickel, or quarter (but NOT a one or two dollar coin). It's best if the two coins you select are different (for example, one a nickel and the other a dime). You will use these two coins throughout the duration of the experiment, over the course of the next week. It is important that you not lose them. Keep them in a safe spot and available.
2. Hold your hands out in front of you, palms facing upwards. Lower your hands slowly down so that they become in line with your hips. Do this movement three times. Close your eyes and bow your head.	2. Hold your hands at waist level, with your arms down by your sides, and have your palms face downward (parallel to the ground), and slowly bring the hands up and down. Do this five times. Bring your arms down. Gently bow your head, close your eyes, and make a wiping motion with your hands away from your body. Finish with your arms resting at your sides.	2. Get a cup or mug of some sort available. Fill it halfway with lukewarm water—being careful that the water isn't too hot or too cold. Gently submerge the two coins in the water. Place the cup down on a surface or on the floor in front of you.
3. Next, close your fingers from each hand to make a tight fist. Hold your fists in front of your chest and bring them together so that your knuckles and thumbs match up. Keeping them in this position, bring your arms straight up over your head. Do this movement three times. Close your eyes and bow your head.	3. Raise your hands about a foot higher with your palms facing away from you. Your elbows will be slightly bent. Spread your fingers with your hands/arms in this position, and then bring your fingers back together, and complete this movement five times. Bring your arms down. Again, gently bow your head, close your eyes, and make the same wiping motion. Finish with your arms resting at your sides.	3. As the coins sit in water, close your eyes and take 5, slow, deep breaths. Afterward, bow your head and make a sweeping motion away holding the cup in your hands.
4. Keeping your fists as is, next bring your fists to either side of your head, so that the knuckles of each hand line up with your temples. Bring your fists together in front of your eyes. Do this movement three times. Close your eyes and bow your head.	4. Bring your hands and palms together in front of your body while raising them above your head. Complete this movement five times. Bring your arms down. Gently bow your head, close your eyes, and make the same wiping motion. Finish with your arms at your sides.	4. Next, gently remove the two coins from the water. Place the smaller coin in your NON dominant hand (left hand if you're right handed) and the larger coin in your DOMINANT hand (right hand if you're right-handed).
5. Bring your fists back down in front of your body and open your hands so that, again, your palms are facing upward.	5. Place your hands on top of your head, with the palm of your dominant hand (writing hand) on the bottom in contact with your scalp. Gently raise your hands just above the head and then bring them back onto the head. Do this five times. Bring your arms down. Bow your head, close your eyes, and make the wiping motion. Finish with your arms resting at your sides	5. Hold your hands out in front of you, palms facing upwards so that the coins don't fall. Lower your hands slowly down so that they become in line with your hips. Do this movement five times. Close your eyes and bow your head.
6. Finish off by closing your eyes and taking three, slow, deep breaths. As you do this bring your full attention, awareness, and focus on your conscious and unconscious mind. You are finished.	6. Bring your arms behind your back with your hands together. Slightly bend at the waist, and complete this movement five times. Bring your arms down. Bow your head, close your eyes, and make the wiping motion. Finish with your arms resting at your sides. Take five breaths. You are finished.	6. Next, keeping the coins in your hand, close your fingers around the coin, making a tight fist. Hold your fists in front of your chest and bring them together so that your knuckles and thumbs match up. Keeping them in this position, bring your arms straight up over your head. Do this movement five times. Close your eyes and bow your head.

(continued)

Table 1. *(continued)*

Simple ritual (Experiment 2)	Intermediate ritual (Experiments 1 and 3)	Elaborate ritual (Experiments 2 and 4)
		7. Keeping your fists as is, next bring your fists to either side of your head, so that the knuckles of each hand line up with your temples. Bring your fists together in front of your eyes. Do this movement five times. Close your eyes and bow your head.
		8. Bring your fists back down in front of your body and open your hands so that your palms are facing upward with the coins resting. Bring both coins together into your DOMINANT hand.
		9. Finish off by closing your eyes and taking five, slow, deep breaths. As you do this bring your full attention, awareness, and focus on your conscious and unconscious mind.
		10. Lastly, return both coins back into the half-filled cup of water for a moment, and remove them.

the week—informed participants (and confederates) that their data showed them to be underestimators and assigned them to the red group. The assignments were revealed at this point to minimize the amount of group experience. Participants were told that the out-group—whose data showed them to be overestimators—had been assigned to the blue group and were completing the same experiment in a neighboring lab space. (In fact, there was no blue group, and all participants were assigned to the red group.) A head shot of each participant (and the confederate) was then taken, and the experimenter led the participants to believe that each person's picture would be uploaded to the main computer to be used later in the experiment; in fact, none of the pictures were uploaded, and the images that were used later in the session had been incorporated into the task prior to the study.

Participants in the ritual condition (along with the confederate) then spent 2 min performing the action sequence one final time: They lined up beside one another, facing a wall so that they could partially see the others to their left and right performing the actions. Given research on the effects of synchronization and mimicry on prosociality and cooperation (Wiltermuth & Heath, 2009), we intentionally staggered the starting time of the participants' performance of the sequence by 10 s to prevent them from coordinating with each other's movements; thus, any effect of condition on the dependent variable could not be attributed to synchrony. To control for the salience of group membership and timing of events, we asked participants in the no-ritual condition to complete another round of dot counting for 2 min in their group, and they were told that their performance confirmed their group assignment. Because participants in the two conditions performed a task for the same amount of time and had their group assignment reinforced, we assumed that all participants had their identity validated equally.

Finally, participants completed two rounds of the trust game (Berg, Dickhaut, & McCabe, 1995; Sapienza, Toldra-Simats, & Zingales, 2013). The trust game is an economic game that assesses social preferences and has been used extensively to measure trusting and trustworthy behavior between two or more people (e.g., Sapienza et al., 2013). As a result, it is well suited for testing (economic) trust between members of different groups. In each round of the game, participants ostensibly interacted with a partner via a computer; participants believed that the image of the partner was a photograph that had been taken earlier in the session. In one round of the game, the partner was a member of the in-group, and in the other round, the partner was a member of the out-group. The in-group partner was the one confederate, whereas the out-group partner was ostensibly one of the members from the other group in the neighboring lab room. Note that across all experimental sessions, we randomly selected the in-group confederate from a pool of six people (three males and three females), to ensure that the results obtained would not be due to a particular individual's appearance. Also, at each session, we ensured that the photograph of the in-group partner matched the actual confederate in that session. The photograph of the out-group partner was randomly selected from a set of four faces. The participants were led to believe that the face that appeared in each round had been randomly chosen from the photographs that were taken previously, when in fact each of the photographs had been preselected beforehand.

In each round, participants (always in the role of the sender) were allocated \$10 and had the option to send any (or none) of this amount to the receiver. They were instructed that the initial amount sent would be tripled and given to the receiver, who would then have the option to either keep all of the money or return any amount of it to the sender. Participants were also told that the receiver would then be given the option to reciprocate the offer and send any of the amount he or she had received back to the sender. (In actuality, this second exchange never occurred.) Thus, in a perfectly cooperative exchange, the sender, fully trusting the receiver to fairly reciprocate the offer, would send the entire \$10 allotment, and the receiver would split the \$30, so that each player would end up with \$15. Participants understood that in order to gain more than their original endowment, they would need to trust the receiver with a certain amount; the more money they sent, the higher the individual payout would be, but the greater the risk of the endowment being lost. To make sure that participants understood the logic of the exchange, we provided them with various scenarios and the resulting payouts.

Participants began the game only once they understood how everything worked. So that participants would treat the task as an economic exchange with real consequences, we told them that their individual outcomes from the two rounds would be averaged together, and that they would take this average amount home in cash. To make sure that participants would treat the two rounds equivalently, we also told them that the receivers' decisions would not be known to them until after both rounds were finished (again, in reality, there was no second exchange in either round, and this was explained to participants after the game finished). Because there was no actual receiver in any of the rounds, the participants in both conditions took home the original \$10 regardless of their decisions during the rounds.

Results

We ran a two-level multilevel model with condition (ritual vs. no ritual) and group status of the receiver (in-group vs. out-group) as predictors of the amount of money entrusted, which was nested within participants. With this model, we estimated a random intercept for each person. We used an unstructured covariance matrix and the between-within method of estimating degrees of freedom. Effect sizes were estimated with semipartial R^2 (Edwards, Muller, Wolfinger, Qaqish, & Schabenberger, 2008).

The model revealed a main effect of group status of the receiver on the amount of money entrusted, $b = -0.32$, $SE = 0.07$, 95% confidence interval (CI) = [−0.46, −0.19], semipartial $R^2 = .167$, $t(99) = -4.86$, $p < .000004$; participants entrusted more money to in-group than to out-group members. Most important, the interaction between condition and group status was significant, $b = -0.19$, $SE = 0.07$, 95% CI = [−0.32, −0.06], semipartial $R^2 = .08$, $t(99) = -2.86$, $p = .005$. As predicted, participants in the ritual condition entrusted significantly less of their money to out-group members (M = \$5.29, SD = 2.80) than to in-group members (M = \$6.30, SD = 2.80), $t(99) = -5.05$, $p = .000002$, whereas participants in the no-ritual condition entrusted comparable amounts to out-group members ($M = 5.82$, $SD = 3.08$) and in-group members ($M = 6.10$, $SD = 3.14$), $t(99) = -1.55$, $p = .12$ (Fig. 1). Examining the between-factors simple effects, we found that the amount entrusted did not differ significantly between the ritual and no-ritual conditions for either in-group or out-group members (both ps > .25). As a result, it is not possible to infer whether the bias caused by the ritual was driven more by in-group trust or by out-group distrust.

This first study demonstrates a link between ritual and intergroup bias. Performing novel, arbitrary action sequences and learning that this experience was either shared by other people (in-group members) or not (out-group members) was enough to lead to biases in the amount of money entrusted to them.

The observed bias may have been a consequence of the level of involvement associated with the ritual-like actions. Costly-signaling theory suggests that the persistence of extravagant, onerous ritual is linked to the stability of cooperation within groups (Xygalatas et al., 2013) and conflict between groups (Sosis, Kress, & Boster, 2007). Bringing this concept into the laboratory for a causal exploration, in a second experiment we tested the idea that variations in ritual effort or elaboration (i.e., "costliness") will modulate intergroup bias. We used the same novel-ritual paradigm as in Experiment 1 but varied whether the ritual was simple or elaborate.

Experiment 2

Participants

One hundred students at the University of Toronto Scarborough participated in this experiment for course credit and the possibility to earn additional money (as in

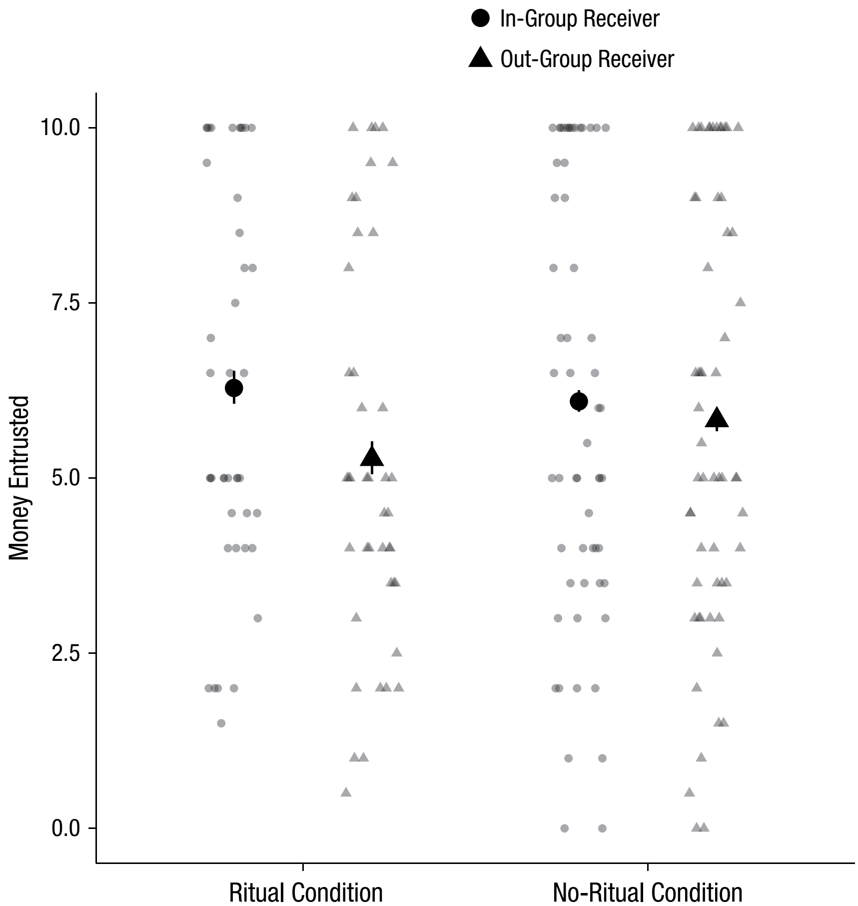

Fig. 1. Results from Experiment 1: amount of money entrusted as a function of condition (ritual vs. no ritual) and group status of the receiver (in-group vs. out-group member). The small symbols represent amounts entrusted on individual trials. The large symbols indicate the least-squares predicted means; error bars represent 95% confidence intervals (in some cases, the error bars are too small to be seen here).

Experiment 1). Participants were randomly assigned to one of three conditions: an elaborate-ritual condition, a simple-ritual condition, or a no-ritual (control) condition. Ten participants from the two ritual conditions were excluded from analyses because they failed to complete the assigned at-home portion of the experiment. None of the participants expressed suspicion regarding the role of confederates. The final sample consisted of 90 participants (27 male, 59 female, 4 with unreported gender; mean age = 20.3 years, *SD* = 2.83; elaborate-ritual condition: n = 29; simple-ritual condition: n = 30; no-ritual condition: n = 31).

Our target sample size was determined using an a priori power analysis (G*Power software; Faul et al., 2007). We partially based this analysis on the results from Experiment 1, in which the correlation between our repeated measures was .89, and the effect size (semipartial R^2) was .08. To be conservative, we used a smaller effect size, d = 0.3 (smaller than the average effect in social psychology), and a correlation of .80 for the repeated dependent variables. Given these values, G*Power indicated that with a mixed design with three groups, we could achieve 80% power to detect the omnibus interaction effect with 48 participants. We decided to collect data until the end of the term, knowing that we would be able to recruit and collect data from at least 48 participants by that time.

Procedure

The procedure and design were similar to those of Experiment 1. Again, the experiment comprised a weeklong at-home portion and a minimal-group manipulation, but there were a few key changes. To increase the credibility of the group assignments, we included five confederates at each lab session; two acted as members of the minimal in-group (which included the one real participant) and three acted as members of the minimal out-group. (We wanted to ensure that the two groups had equal numbers of members.) The confederates' group assignment (i.e., in-group or out-group) was varied and randomly chosen beforehand. After the group assignments, the three out-group confederates were taken to a neighboring room. That is, participants began the experiment in the presence of both in-group and out-group members. In addition, we altered the experiment's framing. Whereas in the first experiment the action sequence was framed as having some level of ancient or cultural meaning, in Experiment 2 the framing was as vague and neutral as possible. Participants were informed that the experiment was related to physical movement and its effects on cognitive processing. In other words, we wanted participants to perform the sequence without any preconceived notion of ritual. Specifically, they were told:

> *Research has found a link between different types of bodily actions and cognitive functions. The purpose of the current study is to therefore extend these findings to a representative Canadian sample. As part of this, you will be asked to learn a set of short physical sequences and eventually master it over the course of the following week. We will send you daily emails, which will help serve as a reminder for you to perform the action sequence.*

The actions in the elaborate-ritual and simple-ritual conditions had the same ritual-like features as in Experiment 1, but varied in their level of complexity and length. Specifically, the simple-ritual sequence was shorter, was less stringent in its rules and guidelines, and involved less repetition; the elaborate-ritual sequence was longer and more involved, had stricter rules, and utilized coins and water as ritual instruments (Table 1). In a pretest, a separate sample (N = 54) rated these rituals and the intermediate ritual in Experiment 1 for their level of perceived effort and elaborateness, using scales from 0 (*no effort* or *very simple*) to 100 (*extreme effort* or *very elaborate*). Analyses confirmed that the simple ritual was judged as least effortful (M = 47.9) and least elaborate (M = 51.4), the elaborate ritual was judged as most effortful (M = 63.7) and most elaborate (M = 73.5), and the intermediate ritual fell in between (effort: M = 55.2; elaborateness: M = 58.8)—effort: $F(2, 53) = 19.37$, $p < .001$, η_p^2 = .27; elaborateness: $F(2, 53) = 22.32$, $p < .001$, $\eta_p^2 = .30$.

As in Experiment 1, participants who completed the at-home actions or survey fewer than three times over the course of the week were removed from the final analysis (n = 4 in the simple-ritual condition, n = 6 in the elaborate-ritual condition, n = 0 in the no-ritual condition). Low compliance was also confirmed during a funnel debriefing, when participants in the two ritual conditions were asked to report the number of times they had completed the actions over the course of the week (M = 4.73).

As in Experiment 1, participants completed the trust game at the lab at the end of the week, but instead of two interactions, there were four: two interactions with each in-group confederate and two interactions with each of two out-group confederates. Participants were led to believe that the computer randomly chose the two out-group members who were their partners. However, as in Experiment 1, all the photographs of ostensible partners were preloaded into the task.

Results

As in Experiment 1, we used a two-level multilevel model with independent variables of condition (elaborate ritual vs. simple ritual vs. no ritual) and group status of the receiver (in-group vs. out-group) to estimate a random intercept for the amount entrusted by each person. The model revealed a significant main effect of group status on money entrusted, b = 0.16, SE = 0.08, 95% CI = [0.01, 0.31], semipartial $R^2 = .05$, $t(267) = 2.11$, $p = .035$: Participants entrusted more money to in-group than to out-group members. We performed a chi-square test of independence to determine whether the omnibus model with the interaction term or a model with the interaction removed provided a better fit to the data. For this test, the degrees of freedom is reported as the difference in the degrees of freedom between the models (e.g., West, Aiken, & Krull, 1996). This test revealed a small effect, $\chi^2(2) = 4.97$, $p = .08$, partially supporting the inclusion of the interaction term as providing the better fit, despite the nonsignificant test result.[1]

Although the omnibus interaction was nonsignificant, we conducted pairwise comparisons, given the findings from Experiment 1 and our predictions regarding trust-game allocations in the elaborate- and simple-ritual conditions. We found that participants in the elaborate-ritual condition entrusted significantly less money to out-group members (M = \$5.66, SD = 3.35) than to in-group members (M = \$6.38, SD = 3.09), $t(267) = 2.58$, $p = .01$, an effect that was absent in the simple-ritual condition (out-group: M = \$5.79, SD = 3.36; in-group: M = \$6.10, SD = 3.41), $t(267) = 1.52$, $p = .13$, and the no-ritual condition (out-group: M = \$6.47, SD = 3.20; in-group: M = \$6.40, SD = 3.0), $t(267) = 0.50$, $p = .62$ (Fig. 2). Finally, looking at the between-factors simple effects, we found that the amounts entrusted did not differ significantly between any pair of conditions for either in-group or out-group members (all ps > .25).

These results offer partial evidence that differences in the effort invested in ritual, which was varied at a basic level in our paradigm, produce different levels of intergroup bias. Although we did not find a significant interaction between condition and group status on the amount entrusted in the trust game, the simple effects offered modest support for our predictions. Although the amount of effort for the elaborate ritual registered a minor cost for the individual, especially compared with the onerous

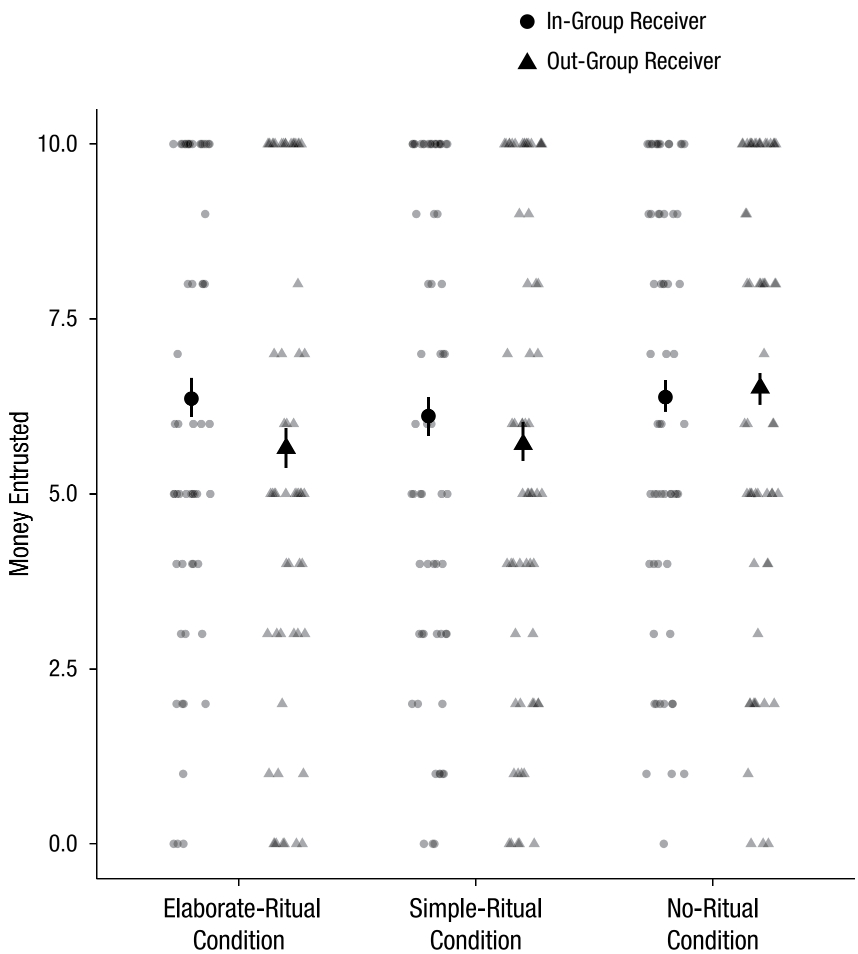

Fig. 2. Results from Experiment 2: amount of money entrusted as a function of condition (elaborate ritual vs. simple ritual vs. no ritual) and group status of the receiver (in-group vs. out-group member). The small symbols represent amounts entrusted on individual trials. The large symbols indicate the least-squares predicted means; error bars represent 95% confidence intervals.

behaviors of real-life rituals, it was sufficient to elicit intergroup bias. We did not observe intergroup bias in the simple-ritual condition, which suggests that rituals that are too minimal—requiring too little investment and involvement—may not cause intergroup bias.

Experiments 1 and 2 thus provide mixed evidence that novel rituals inculcate intergroup bias: It appears that only those that require an intermediate or elaborate amount of effort generate bias. These results suggest that rituals inculcate bias only if they include certain features.

The novel rituals in the first two experiments required varying degrees of effort, but all involved a weeklong regimen of daily, repeated practice. We next examined whether a novel ritual requiring intermediate effort but enacted only once would produce intergroup bias. Given previous research suggesting that that a one-time ritual can be sufficient to affect emotional states (e.g., Norton & Gino, 2013), we predicted that a one-time, moderately effortful ritual would produce bias. We tested this prediction in Experiment 3, a preregistered experiment.

Experiment 3

Participants

One hundred twenty-three students at the University of Toronto Scarborough participated in this experiment for course credit and the possibility to earn additional money (as in the previous two experiments). Participants were randomly assigned to one of two conditions: a one-time-ritual condition or a no-ritual (control) condition. Four participants were excluded from the analyses because they had seen the confederates at the previous lab session and were suspicious of their role as participants. The final sample consisted of 119 participants (40 male, 79 female; mean age = 18.35 years,

SD = 1.25; one-time-ritual condition: n = 66; no-ritual condition: n = 53).

The target sample size was determined by assuming an effect size similar to what we observed in the previous two experiments. Given that we were testing whether very minimal rituals could be effective in shaping intergroup dynamics, we used G*Power (Faul et al., 2007) to conduct an a priori power analysis with conservative values, so that we would have more certainty in the robustness of our results. For this analysis, we used a small effect size, d = 0.26, and a correlation between dependent variables that was smaller than what we uncovered in Experiments 1 and 2, r = .7. This power analysis revealed that we could achieve 95% power to detect an omnibus interaction between condition (one-time ritual vs. no ritual) and group status of the receiver (in-group vs. out-group) in our mixed-design study with a total sample of 118. We decided to collect data until the end of the term, knowing that we would be able to recruit and collect data from at least 118 participants by that time.

Procedure

The minimal-group manipulation used in the previous two experiments was used in this experiment as well. As in Experiment 2, each session included two in-group confederates and three out-group confederates. Unlike in the other two experiments, however, there was no at-home portion: Ritual-condition participants learned and performed the ritual (the action sequence from Experiment 1) during their one and only lab visit. Thus, although participants were assigned to minimal groups, they did not repeat the ritual over a series of days. Following the group assignments, participants in the one-time-ritual condition performed the action sequence standing shoulder to shoulder with the confederates and staggering their start times (as in Experiments 1 and 2). Each person followed the instructions for the sequence, which were printed on a piece of paper on the wall. The intermediate ritual from Experiment 1 was used to ensure that the actions were sufficiently elaborate. To control for the salience of group membership and for time, we asked participants in the no-ritual condition to complete a round of personality items (which took 2 min, the same time as performing the action sequence) and told them that the results confirmed their group assignment. Finally, participants played four rounds of the trust game, two with each in-group member and two with each of two out-group members (as in Experiment 2).

Results

To analyze the data, we used the same multilevel model as in the other experiments. The model revealed a main effect of group status on the amount of money entrusted, b = −0.15, SE = 0.06, 95% CI = [−0.25, −0.04], semipartial R^2 = .02, t(356) = −2.63, p = .009; participants entrusted more money to in-group than to out-group members. Contrary to our original predictions, however, the interaction between condition and group status was not significant, b = −0.06, SE = 0.06, 95% CI = [−0.16, 0.05], semipartial R^2 = .002, t(356) = −1.01, p = .31; participants in the one-time-ritual condition were no more biased in their trust (in-group: M = \$5.34, SD = 2.97; out-group: M = \$4.94, SD = 3.0) than were participants in the no-ritual condition (in-group: M = \$5.23, SD = 2.67; out-group: M = 5.05, SD = 2.79; Fig. 3).

A one-time novel ritual does not cause heightened intergroup bias, even when that ritual is relatively effortful. These results are consistent with the notion that repetition and time are additional factors necessary for rituals to inculcate bias. In addition, given that asynchronous group activity has been shown to be associated with lower levels of affiliative behavior compared with synchronous activity, it is possible that in order for bias to be generated, in-group members need to experience an element of interpersonal synchronization.

Taken together, Experiments 1 through 3 reveal that not all novel rituals promote group bias. The most minimal form of rituals appeared not to be sufficient, which suggests that effort, repetition, and time are critical elements for ritual to produce bias. Because this interpretation is based in part on a null finding, however, further confirmatory testing is essential.

Why do rituals create intergroup bias? The presence of a neural system attuned to processing the actions and outcomes of other individuals—to such an extent that individuals represent the actions of others as their own—offers one possible underlying brain mechanism. The first three experiments offer modest support for the notion that, under some conditions, collective rituals can influence behavior toward members of in-groups and out-groups, suggesting that rituals heighten sensitivity to evaluating other people as members of in-groups or out-groups. As a result, rituals may be represented in brain systems that underlie the evaluative observation of other individuals (e.g., de Bruijn, Miedl, & Bekkering, 2008). If that is the case, this neural circuitry should be modulated as a function of whether the ritual experience has been shared with other individuals (in-group) or not (out-group). In Experiment 4, we leveraged recent neuroanthropological advances in exploring the neural basis of group ritual behaviors (e.g., Schjoedt et al., 2013). We examined the effect of ritual on intergroup reward processing; specifically, we assessed rapid changes in participants' neural performance monitoring while they observed in-group members and out-group members receive rewarding and punishing outcomes, comparing

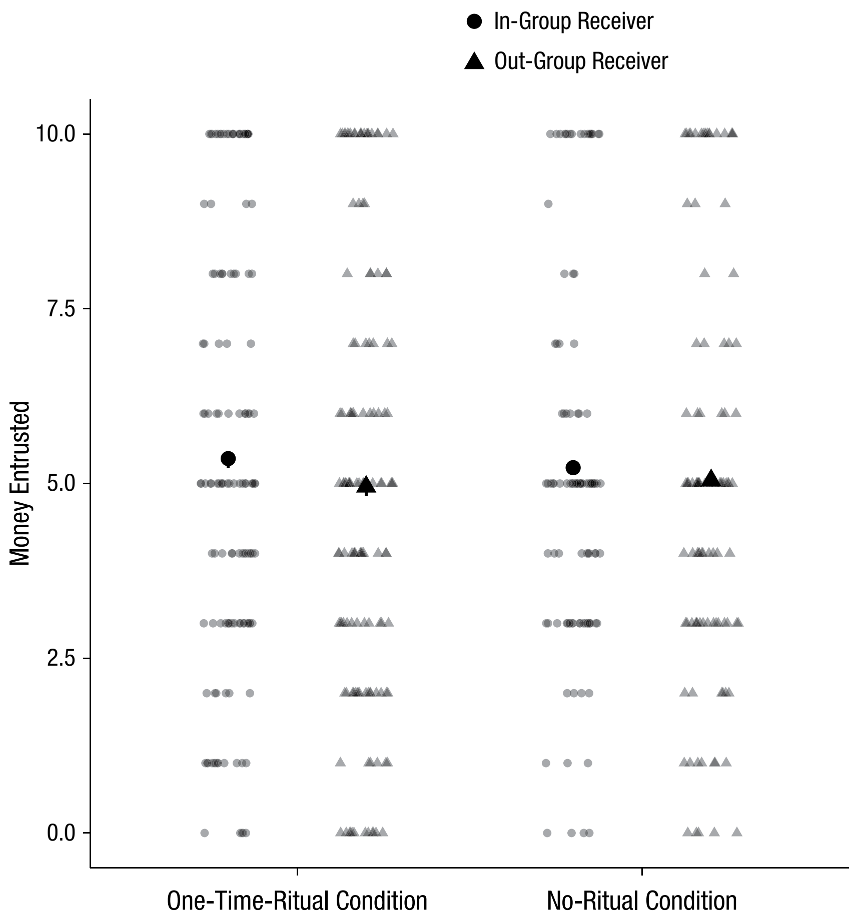

Fig. 3. Results from Experiment 3: amount of money entrusted as a function of condition (one-time ritual vs. no ritual) and group status of the receiver (in-group vs. out-group member). The small symbols represent amounts entrusted on individual trials. The large symbols indicate the least-squares predicted means (error bars for 95% confidence intervals are too small to be seen here).

the neural data between participants whose minimal-group membership was based on ritual and those whose minimal-group membership was not based on ritual.

Experiment 4

Feedback event-related potentials

We assessed continuous electroencephalographic (EEG) data, focusing on two related evoked brain potentials, or event-related potentials (ERPs), known to be associated with rapid, on-line performance monitoring and outcome processing related to rewards and punishments: the feedback-related negativity (FRN) and the feedback-P300 (f-P300). The FRN, peaking at frontocentral sites, is an early negative-going waveform occurring 200 to 300 ms after participants experience punishing or rewarding feedback (Gehring & Willoughby, 2002). The f-P300, labeled for its positivity and latency, and peaking mostly at posterior sites, is a slow-wave component that typically emerges between 200 and 500 ms after receipt of feedback and often codes the representations of feedback or reward magnitude. An increased f-P300 is thought to reflect the allocation of attention and enhanced stimulus processing in response to any motivationally salient features that are encountered in the environment.

Observers witnessing other people's reward and punishment outcomes exhibit analogous waveforms with a similar latency and morphology: The observer FRN (oFRN) is thought to underlie the automatic and motivational evaluation of other people's performance (van Schie, Mars, Coles, & Bekkering, 2004), and the amplitude of the observer f-P300 has been shown to track outcomes differentially according to the interpersonal context (Picton, Saunders, & Jentzsch, 2012). The oFRN, as an index of a system of automatic motivational reward evaluation, may be too early to code group membership. We expected that group membership would not modulate the oFRN but would modulate the controlled neural appraisal indexed by the later occurring observer f-P300.

Design

Thus, in Experiment 4, we examined the effect of novel ritual on observer-related ERPs in a minimal intergroup context. We assigned participants to a ritual or no-ritual condition and tracked their neural processing while they watched an in-group member and an out-group member (defined by a minimal-group manipulation) receive both punishing and rewarding feedback on a task. The full design was a 2 (condition: elaborate ritual vs. no ritual) × 2 (feedback type: reward vs. punishment) × 2 (group status of the performer: in-group vs. out-group), with repeated measures on the final two factors.

Participants

Fifty-nine students at the University of Toronto Scarborough participated in this experiment for course credit and bonus pay. Nine participants were excluded from all analyses because of computer or hardware malfunction (n = 4), high EEG artifact rates (> 30% artifacts; n = 2), personal knowledge of the confederates (n = 2; see the Procedure section), or awareness of the experiment's purpose (n = 1). This left a final sample of 50 participants (35 female, 15 male; mean age = 21.4 years, *SD* = 5.67). All participants completed the at-home portion of the experiment (see the Procedure section), so none were dropped from the analyses because of low compliance. Participants were randomly assigned to one of two conditions, an elaborate-ritual condition (n = 25) or a no-ritual condition (n = 25).

The smaller sample size relative to our first three experiments is typical of most neurophysiological and neural imaging studies, in which the reliability of measures is gained through multiple trials that are highly correlated and through using a mixed experimental design that relies on at least one within-subjects factor. A power analysis using G*power (Faul et al., 2007) indicated that given a small effect size, d = 0.3, and a correlation of .8 (i.e., within the range observed in the previous experiments—from .7 to .9), we could achieve 80% power to detect an omnibus interaction effect with 38 participants. We decided to collect data until the end of the term, knowing that we would be able to recruit and collect data from at least 38 participants by that time.

Procedure

The procedure for creating the minimal groups was the same as in Experiment 2 except as noted here. All participants were compliant with the instructions for the at-home component (i.e., they completed the action sequence or the survey at least 3 times over the week. This was also confirmed during a funnel debriefing, when participants in the ritual condition were asked to report the number of times they had completed the actions over the course of the week (M = 6.24). In this experiment, each lab session involved a single participant and three confederates; the participant and one confederate were assigned to the red group (in-group), and two confederates were assigned to the blue group (out-group).

The experimenter told participants that they would have the chance to earn bonus money in a "cognitive/perception timing" task and that in separate, alternating rounds, they would perform the task (and receive their own feedback) and observe other people performing the task (and see them receiving feedback). The experimenter told the combined groups that everyone would participate in the task (both performing and observing), but that there was only enough time for one person's EEG to be recorded, and that a name would be randomly drawn to determine who that person would be. The name of the participant was drawn in each case. If the participant was in the elaborate-ritual condition, he or she then performed the ritual in the lab; if the participant was in the no-ritual condition, he or she completed the dot-counting manipulation for the same amount of time as the ritual took, ostensibly as confirmation of the group assignment.

We used continuous EEG to measure participants' ERPs while they (a) received feedback on a task and (b) observed in-group and out-group members receive similar feedback. We report only the results for the observation rounds here, given the hypotheses under consideration. Each participant completed four separate rounds: observe an in-group member perform, observe an out-group member perform, perform with an in-group member observing, and perform with an out-group member observing. The order of the rounds was counterbalanced across participants.

The task we used to provide feedback to participants was a time-estimation task (Miltner, Braun, & Coles, 1997): On each trial, a central fixation cross was presented for 250 ms and followed by a blank screen; participants were instructed to press the space bar when they believed exactly 1 s had passed since the appearance of the fixation cross. Visual feedback was provided 2 to 3 s after the initial fixation cue, so there was approximately a 1-s interval between response and feedback. The feedback remained on the screen for 1 s and was followed by an intertrial interval varying between 1 and 2 s. Participants were provided with written instructions and completed 20 practice trials. They were told that their bonus pay would increase with the number of correct responses they provided while in the performer role, and that their task while in the observer role was to pay close attention to the feedback being given to the performer. To ensure that participants were engaged while observing, we tested their recall intermittently (e.g., "Was the last trial you observed correct or incorrect?").

Participants received rewarding feedback ("win money") when their response fell within a predefined time window centered around 1 s after the appearance of the fixation cross, and they received punishing feedback ("lose money") when their response did not fall within this window. Participants were unaware that this time window was adaptively calibrated over the course of each round, such that it decreased after a correct response and increased after an incorrect response. As a result, rewarding and punishing stimuli occurred roughly equally often in each round.

Continuous EEG was recorded during the four rounds of the time-estimation task using a stretch Lycra cap embedded with midline electrodes (Electro-Cap International, Eaton, OH). Recordings used average ear and forehead channels as reference and ground, respectively. The continuous EEG was digitized using a sample rate of 512 Hz, and electrode impedances were maintained below 5 kΩ during recording. Off-line, EEG was analyzed with Brain Vision Analyzer 2.0 (Brain Products GmbH, Munich, Germany). EEG data were corrected for vertical electrooculogram artifacts (Gratton, Coles, & Donchin, 1983). An automatic procedure was employed to detect and reject artifacts. An artifact was defined as a voltage step of more than 25 μV between sample points, a voltage difference of 150 μV or more within a 150-ms interval, voltage above 85 μV or below −85 μV, or a maximum voltage difference of less than 0.50 μV within a 100-ms interval. Intervals with one or more of these artifacts were rejected on a channel-by-channel basis in order to maximize data retention.

To account for the unique spectral features present in the oFRN and observer f-P300 (e.g., Başar-Eroglu, Demiralp, Schürmann, Başar, 2001; Cavanagh, Zambrano-Vasquez, & Allen, 2012), we applied two digital off-line filters to the EEG data: First, to analyze the oFRN, we isolated the range of theta activity with a band-pass filter set between 4 and 8 Hz. The oFRN was defined as the maximum (most negative) peak occurring between 250 ms and 350 ms at the frontocentral electrode site, Fz. Second, a broad-range digital filter (between 0.1 and 30 Hz) was applied to look at effects on the f-P300. The f-P300 was defined as the mean amplitude of the waveform at the posterior-central electrode site, Pz, between 240 ms and 440 ms.

Results

oFRN analyses. A 2 (condition: elaborate ritual vs. no ritual) × 2 (feedback type: reward vs. punishment) × 2 (group status of the performer: in-group vs. out-group) mixed-factor analysis of variance (ANOVA) revealed a significant main effect of condition, $F(1, 48) = 5.53$, $p = .02$, $\eta_p^2 = .10$; overall, participants in the elaborate-ritual condition showed significantly higher oFRN amplitudes than participants in the no-ritual condition (see Fig. 4 for the oFRN components). There was also a significant two-way interaction between condition and feedback type, $F(1, 48) = 8.90$, $p = .004$, $\eta_p^2 = .16$. Planned pairwise comparisons of oFRN amplitudes indicated that participants in the elaborate-ritual condition differentiated between witnessing other people receive punishing feedback ($M = -1.30$ μV, $SD = 0.60$) and witnessing other people receive rewarding feedback ($M = -1.11$ μV, $SD = 0.59$), $t(48) = 2.74$, $p = .009$; this difference mirrors the typical loss-gain differentiation that occurs in the FRN when people experience their own rewarding feedback and punishing feedback. In contrast, participants in the no-ritual condition showed equivalent oFRN amplitudes in response to other people's punishment ($M = -0.78$ μV, $SD = 0.60$) and reward ($M = -0.88$ μV, $SD = 0.59$), $t(48) = 1.49$, $p = .14$.[2] As predicted, the three-way interaction of condition, feedback type, and group status was nonsignificant, $F(1, 48) = 0.61$, $p = .44$, $\eta_p^2 = .01$, which suggests that the difference between oFRN amplitudes when participants observed reward versus punishment was not modulated by group status in either condition.

Thus, the early, automatic monitoring of the outcomes of other people was increased in participants who performed a ritual, compared with those who did not perform a ritual. This result suggests that rituals heighten sensitivity to observer-related outcomes in general.

Observer f-P300 analyses. All analyses of the observer f-P300 were conducted on difference waves, reward trials – punishment trials (Δf-P300); the Δf-P300 was defined as the mean amplitude of the difference waveform at the posterior-central electrode site, Pz, between 240 ms and 440 ms. A larger positive Δf-P300s indicated greater motivated attention to reward relative to punishment. A difference-wave approach provided a more reliable estimate of the effects than did looking at the raw ERP estimates. A 2 (condition: ritual vs. no ritual) × 2 (group status of the performer: in-group vs. out-group) mixed-factor ANOVA revealed no main effects on the Δf-P300 (ps > .25), but a significant two-way interaction between condition and group status, $F(1, 48) = 4.41$, $p = .04$, $\eta_p^2 = .08$. Simple-effects tests revealed that participants in the elaborate-ritual condition showed opposite patterns of differentiated activity when observing in-group and out-group members, $t(48) = 2.04$, $p = .047$; the Δf-P300 was positive while they observed in-group members ($M = 0.23$, $SD = 1.26$), but negative while they observed out-group members ($M = -0.73$, $SD = 2.18$). Participants in the no-ritual condition showed comparable Δf-P300s during observation of in-group members ($M = -0.10$, $SD = 1.53$) and out-group

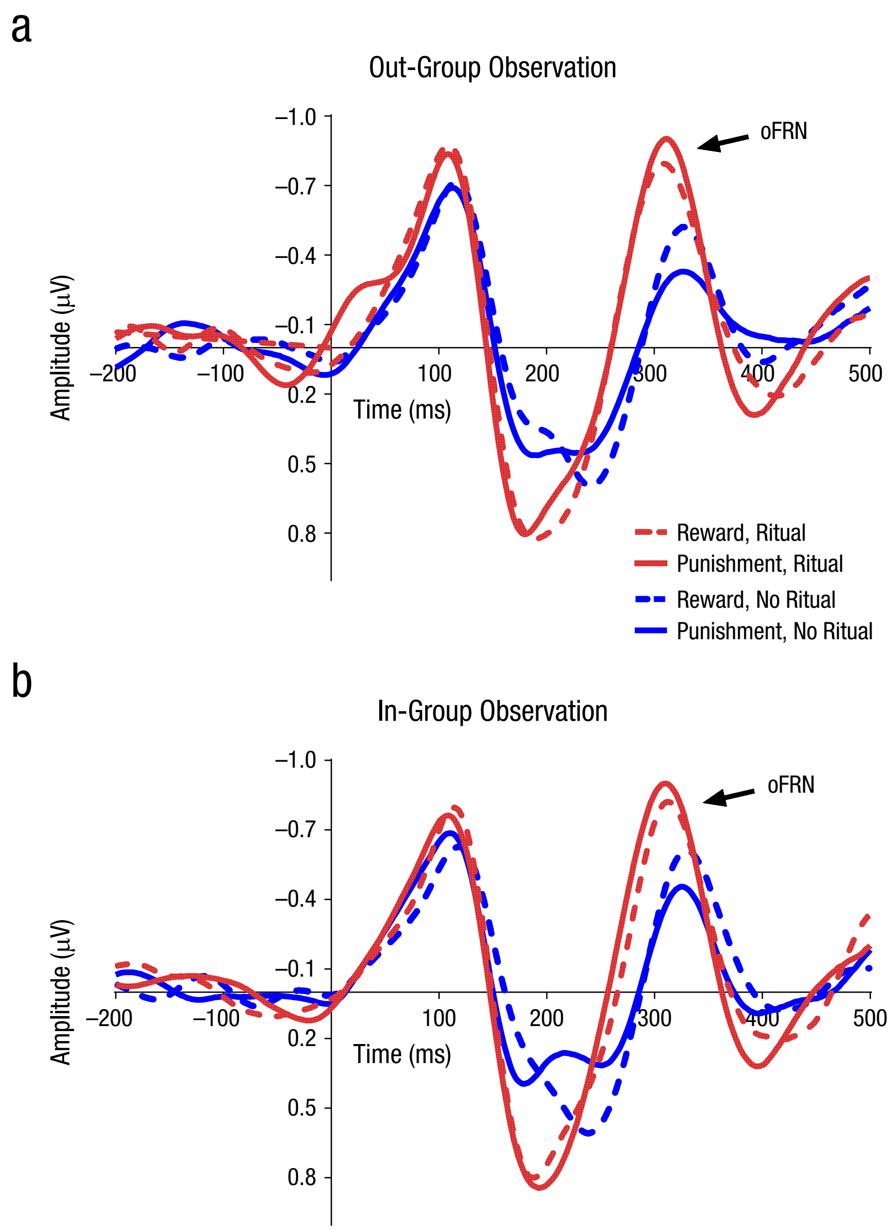

Fig. 4. Grand-average, feedback-locked waveforms illustrating the observer feedback-related negativity (oFRN) following (a) out-group observation and (b) in-group observation in Experiment 4. Waveforms at electrode Fz are shown separately for observation of reward and punishment for participants in the ritual and no-ritual conditions.

members (M = 0.34, SD = 1.83), $t(48) = 0.93$, $p > .25$.[3] Further, between-condition pairwise comparisons revealed a marginal effect of condition for out-group observation, $t(48) = 1.87$, $p = .07$, but no effect for in-group observation, $t(48) = 0.85$, $p > .25$. Figure 5 shows the Δf-P300 waveforms, as well as the raw ERP waveforms, for in-group and out-group observation, separately for the elaborate-ritual and no-ritual conditions.

The Δf-P300 typically reveals greater positivity for gains than for losses when participants monitor their own outcomes (Leng & Zhou, 2010); thus, our results are consistent with the idea that participants in the elaborate-ritual condition tracked rewards to in-group members in a manner consistent with how they would track gains to themselves, while showing the opposite pattern specifically for out-group members. Taken together with the behavioral effects observed in Experiments 1 through 3, the observer f-P300 findings are consistent with the idea that rituals instill a motivation to see both rewards to in-group members and punishments to out-group members

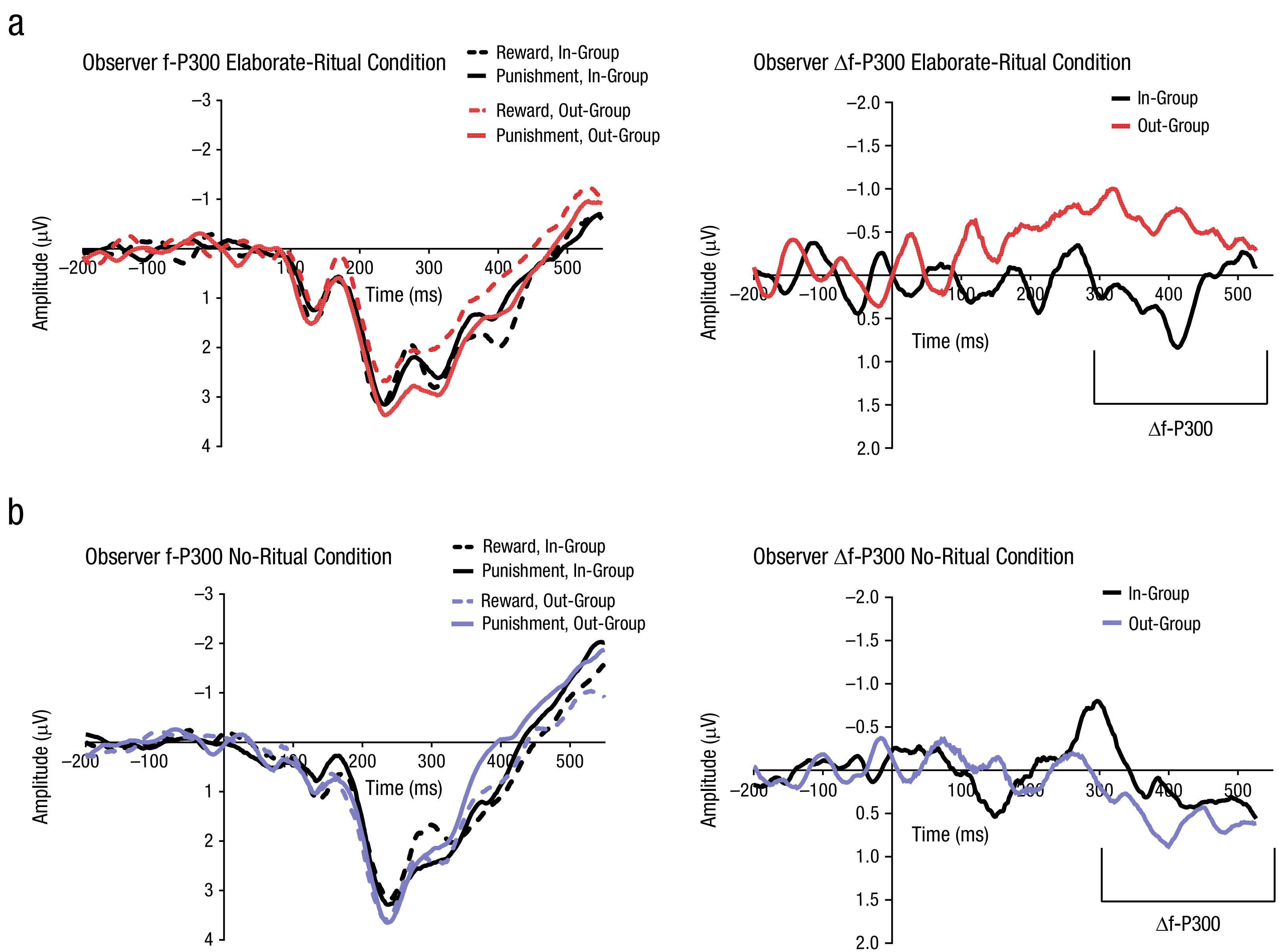

Fig. 5. Grand-average, feedback-locked waveforms illustrating the observer f-P300 in the (a) elaborate-ritual and (b) no-ritual conditions of Experiment 4. Raw event-related potential (ERP) waveforms at electrode Pz are on the left. Separate waveforms are shown for observation of reward and punishment of out-group members and in-group members. Difference-wave ERPs (reward trials – punishment trials) are shown on the right.

as motivationally salient (i.e., schadenfreude; Cikara, Bruneau, Van Bavel, & Saxe, 2014).

Taken together, the data revealed that although other-person monitoring indicated in the oFRN was heightened for participants in the elaborate-ritual condition, the differentiation between observing in-group and out-group members occurred only in the observer f-P300 and not in the oFRN. These results are consistent with the idea that the brain's processing of reward and punishment may be divided into an early, semiautomatic process (oFRN) and a later, more top-down appraisal process (f-P300), and that the contextual cues of group dynamics are encoded only in the later process (e.g., Leng & Zhou, 2010). However, given that these findings remain preliminary, confirmatory research will be needed to test the reliability and robustness of the results. Until then, we urge caution in the interpretation of our findings.

Our ERP findings offer preliminary evidence that rituals may operate by recruiting the early, automatic monitoring of other individuals, and thereby increasing the salience of membership and affiliation as important social cues, especially in intergroup contexts. At the same time, it is important to note that this is only one possible mechanism underlying the effect of rituals. Although we have provided evidence of the involvement of lower-level brain-based processes, it is likely that there are multiple joint mechanisms involving both bottom-up neural and top-down psychological systems.

Discussion

These four experiments offer modest and mixed support for the notion that novel rituals—devoid of cultural meaning and history—can induce intergroup bias. Existing theory proposes that rituals serve a social function, facilitating effective group living by galvanizing in-group loyalty. We used novel rituals in an effort to document that rituals alone—stripped of any broader cultural resonance—lead to intergroup bias. We found only partial support for this hypothesis; our mixed evidence is consistent with the notion that the presence of certain features is critical for rituals to inculcate bias. The lack of an effect in the simple-ritual condition of Experiment 2 suggests that rituals must be sufficiently effortful and costly to lead to bias, and, contrary to our preregistered prediction, the null effect of the one-time ritual in Experiment 3 suggests the importance of repetition and time.

Thus, our original hypotheses need to be revised: It appears that only certain novel rituals generate bias; in other words, when rituals are too minimal—lacking elements, such as repetition, time, and effort, that are present in real-world rituals—they fail to exert an influence on intergroup functioning. Given the modest support for our hypothesis in Experiment 2 and our failure to confirm our preregistered hypothesis in Experiment 3, many of our conclusions are preliminary: Future research is needed to systematically determine which features are and are not required for rituals to influence group processes. Our results from the trust game in Experiments 1, 2, and 3, are ambiguous as to the cause of the bias created. It is unclear whether it was the product of in-group liking or out-group antagonism. Costly-signaling theory posits that extravagant rituals strengthen in-group ties, but does not make specific predictions about out-group hostility. Although the current investigation does not offer conclusive evidence, our results suggest that out-group derogation may also play a role in producing the effects of ritual on intergroup bias. Future research is needed to explore how group rituals may differentially affect bias toward in-groups and against out-groups, especially when in-groups and out-groups perform competing rituals, as, for example, do certain religious communities that are similar but different (Brewer & Pickett, 1999).

We note that across the three behavioral studies we failed to find a basic minimal-group effect in the no-ritual (control) condition, a null finding that is potentially at odds with the established literature on minimal groups. However, whereas minimal-group studies have typically used point-allocation matrices to measure bias (e.g., Tajfel, 1974), our studies relied exclusively on the trust game.

Taken together, our experiments provide modest evidence of ritual's effect on intergroup bias. Cultural stabilization of ritual began in human evolution when fast-growing groups began to experience elevated intergroup competition, which necessitated in-group cooperation (Norenzayan et al., 2014). The current results partially support the idea that rituals offer a strategy for the regulation of in-group behavior—but at a cost to the out-group.

Action Editor

Wendy Berry Mendes served as action editor for this article.

Author Contributions

N. M. Hobson, F. Gino, M. I. Norton, and M. Inzlicht designed the experiments. N. M. Hobson carried out the experiments and statistical analyses, and N. M. Hobson, F. Gino, M. I. Norton, and M. Inzlicht wrote the manuscript.

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Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

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Open Practices

All data and materials have been made publicly available via the Open Science Framework and can be accessed at https://osf.io/tyjpc/. The design and analysis plan for Experiment 3 was preregistered at https://osf.io/82bnc/#. The complete Open Practices Disclosure for this article can be found at http://journals.sagepub.com/doi/suppl/10.1177/0956797617695099. This article has received badges for Open Data, Open Materials, and Preregistration. More information about the Open Practices badges can be found at http://www.psychologicalscience.org/publications/badges.

Notes

1. Because we were examining an interaction between two categorical variables (condition and group status), one of which had more than two levels (i.e., three experimental conditions), we tested for significance by comparing models. The standard interaction terms that are in the output are ignored in such cases because they do not account for the variance included in the three-level categorical variable of the nested model (see West et al., 1996; Judd & McClelland, 1989).

2. For a more conservative test of effects on the oFRN, we conducted an additional analysis using a base-to-peak method, taking the difference between the maximum positive value between 250 ms and 350 ms after presentation of feedback and the most negative point between this maximum and 350 ms after the onset of feedback. We found a nonsignificant main effect of condition, $F(1, 48) = 3.30$, $p = .08$, $\eta_p^2 = .06$. As in the original analysis, however, there was a significant two-way interaction between condition and feedback type, $F(1, 48) = 5.57$, $p = .02$, $\eta_p^2 = .10$. The simple-effects tests revealed the same pattern as the original analysis.

3. Results of analyses including the 3 participants who were initially excluded because of personal knowledge of the confederates or awareness of the experiment's purpose were similar to those reported in the main text. For oFRN amplitude, the main effect of condition, $F(1, 51) = 4.01$, $p = .05$, $\eta_p^2 = .07$, and the interaction between condition and feedback type, $F(1, 51) = 6.96$, $p = .01$, $\eta_p^2 = .12$, remained significant. For the observer f-P300 difference wave, the two-way interaction between condition and group status remained significant, $F(1, 51) = 4.57$, $p = .037$, $\eta_p^2 = .08$.

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