When novel rituals impact intergroup bias: Evidence from economic games and neurophysiology

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Abstract

Long-established rituals in pre-existing cultural groups have been linked to the cultural evolution of group cooperation. Here we test the prediction that novel rituals – arbitrary hand and body gestures enacted in a stereotypical and repeated fashion – can impact intergroup bias in newly formed groups. In four studies, participants practiced novel rituals at home for one week (Experiments 1, 2, 4) or once in the lab (pre-registered Experiment 3), and were divided into minimal ingroups and outgroups. Our results offer mixed support for the hypothesis that novel rituals promote intergroup bias. A modest effect for daily repeated rituals but a null effect for rituals enacted only once suggests that novel rituals can inculcate bias, but only when certain features are present: rituals must be sufficiently elaborate and repeated to impact bias. Taken together, our results offer modest support for the influence of novel rituals on intergroup bias.

Keywords: ritual; intergroup dynamics; intergroup bias; cooperation; neural reward processing

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Collective rituals pervade the social world and define human cultures, religions, and groups (Durkheim, 1915). These behaviors have a distinct evolutionary history and continue to pervade modern social life, with longstanding theory suggesting 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, 2015). However, because rituals exist within a cultural context and bring to mind longstanding 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 create novel rituals and implement them in minimal groups, and then observe their impact on intergroup behavior and underlying neural processes. First, we assess whether even novel rituals – conducted with novel groups – can be sufficient to inculcate intergroup bias. Second, we use the existing literature to identify and then vary 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 ritual is that they have managed to pervade human culture despite the considerable costs incurred from their regular performance. Game-theoretical approaches (e.g., Henrich, 2009) address this puzzle by suggesting that shared rituals help large groups survive by acting as a bulwark against the free-rider problem (Norenzayan et al., 2016). 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 – for example, 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 – marked by increased communal ritual practices and synagogue attendance – show more cooperation towards ingroup members (Sosis & Ruffle, 2003). Field research investigating variations in ritual costliness has shown that more effortful rituals are linked to group cooperation and prosocial behaviors (Xygalatas et al., 2013), with high-intensity rituals resulting 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 ingroup members can have negative consequences for outgroup members. Intergroup competition plays a critical role in the cultural evolution of intragroup cooperation (Gurerk, Irlenbusch, & Rockenbach, 2006); game theory suggests that ingroup altruism is a stable strategy only when coupled with outgroup hostility (Choi & Bowles, 2007; Saaksvuori, Mappes, & Puurtinen, 2011). Supporting these theories, ethnographic studies on religion and ritual show that heightened intergroup conflict and outgroup hatred are linked to greater ingroup commitment to sacred values and engagement in more effortful and costly ritual group displays (Ginges, Atran, Medin, & Shikaki, 2007). For example, the amount of time invested in group ritualistic prayer predicts support for suicide attacks (Ginges, Hansen, & Norenzayan, 2009).

Novel Ritual

This largely correlational research is consistent with our primary hypothesis: rituals galvanize ingroup solidarity, but with costs to members of outgroups. However, because real-life rituals are imbued with culture, history, and preexisting meaning, parsing the causal impact of rituals is challenging. Without controlling for these broad social and group variables, the causal influence of ritual cannot be isolated. Thus, novel rituals – behaviors created in the lab, devoid of historical meaning and culture – allow us to address the psychology of ritual and its effects on intergroup bias. Although rituals can vary widely in their expression (e.g., Whitehouse, 2002), here we operationalize 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 impact of rituals on intergroup bias. Importantly, our experimental design was intended to maximize the "ritual" aspect of the experience while minimizing the "group interaction" aspect, allowing us to control for any impact 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 consider the role of two features common to many group rituals – effort and repetition/time (Durkheim, 1915; Tambiah, 1979; Xygalatas et al., 2013) – by varying each experimentally.

Overview of Experiments

In Experiment 1, we investigate the effects of novel rituals on intergroup bias by assigning some participants to enact a ritual at home for one week before a laboratory session in which they are assigned to novel groups using a minimal groups paradigm (Tajfel, 1974); we use 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 assess whether the level of elaboration and effort moderates the effect of ritual on bias; similarly, in Experiment 3, a pre-registered experiment, we explore the impact of an even more minimal form of novel ritual, one that requires very little repetition and time commitment. Experiment 4 explores neural correlates associated with the impact of ritual on intergroup bias. We assess whether novel rituals influence neural processing related to the evaluation of others' behaviors; this neural system being activated would suggest a candidate proximal mechanism to explain how rituals influence intergroup bias.

Experiment 1—Methods

Participants

One hundred and seven introductory psychology students at the University of Toronto Scarborough participated for course credit; in addition, all participants were informed that they could earn extra bonus money based on their decisions during the experiment; in the end all participants received the same $10 for participating. Participants were randomly assigned to either a ritual condition or a no-ritual control condition. In both conditions, participants underwent a minimal group manipulation (Tajfel, 1974). Seven participants from the ritual condition were excluded from analyses because they failed to complete the assigned at-home portion of the experiment (less than 3 times over the course of the week), which was determined by the number of survey log-ins tracked during the at-home portion and confirmed with participants during the funnel debriefing of the lab portion ($M_{total\ compliance}$ = 4.97). None of the participants expressed suspicion of the role of confederates. The final sample consisted of one hundred participants (25 male, 75 female; M_{age} = 18.8, *SD* = 1.57; ritual condition *n* = 42, no-ritual condition *n* = 58).

Sample size was determined (to ensure sufficient power) 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 effect size of *d* = 0.4 (Richard, Bond, & Stokes-Zoota, 2001), our mixed design could achieve 80% power with as few as 72 participants, assuming a modest (*r* = 0.3) correlation between our repeated dependent measure. We decided to collect data until the end of the term, assuming we collected at least 72 participants.

Procedure

The experiment was comprised of an at-home and in-lab portion, lasting one week (see Figure 1 for an outline of the experimental design). 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). Unlike the classic manipulation, which tends to occur at one time in the lab, participants were not told immediately about the arbitrary grouping assignments (i.e., putting people into groups of "under-estimators" and "over-estimators" based on their responses). 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.

Figure 1.

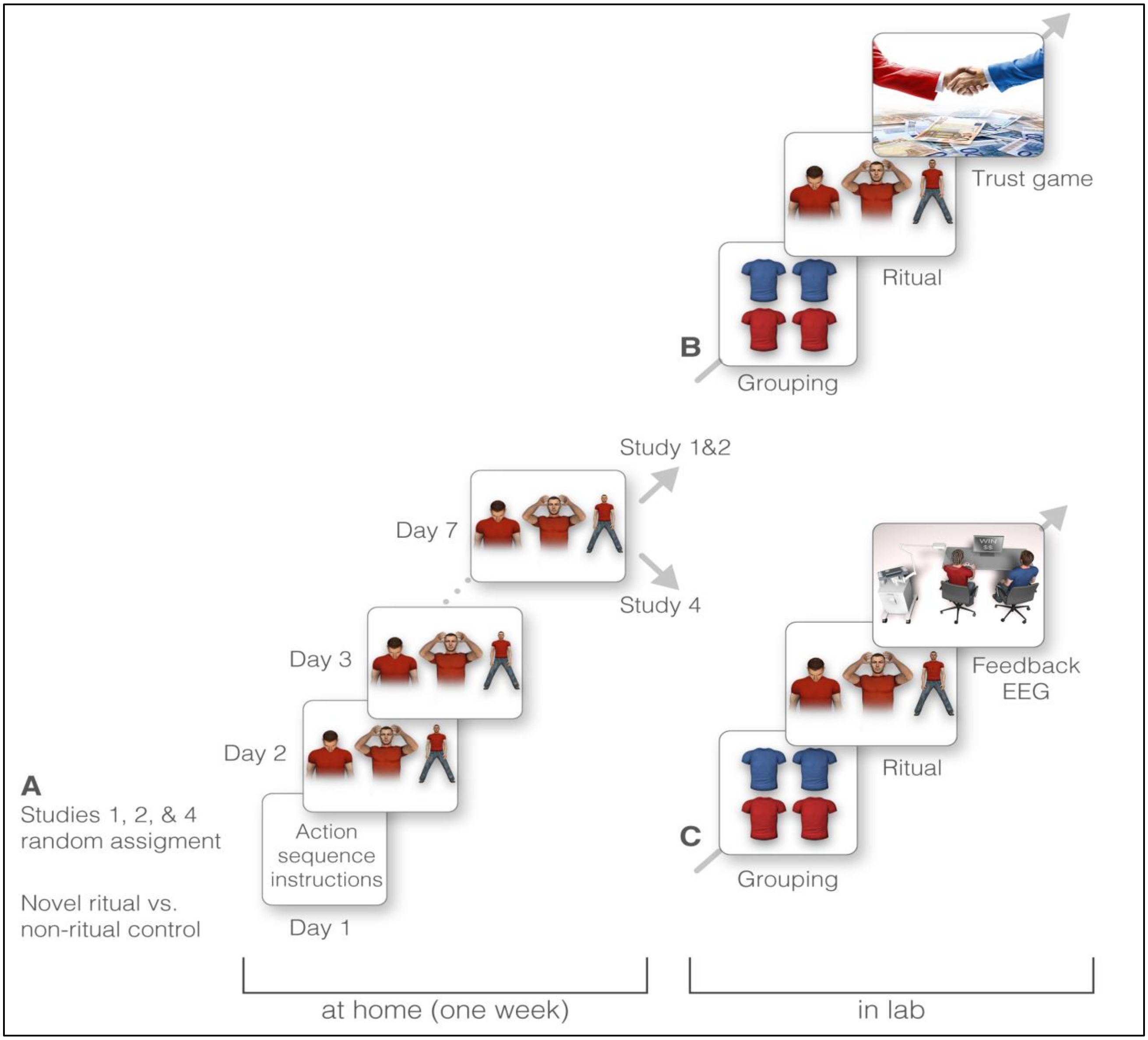

Fig. 1. Schematic diagram of the week-long experimental design for Experiments 1, 2, and 4. The at-home portion (A) lasted one week and the in-lab portion took place on the final day in group assignments. The experimental manipulation consisted of participants practicing and learning a set of specified action sequences, at least once a day over the course of a week, whereas participants in the no-ritual condition underwent a basic minimal group manipulation. In the in-lab portion, a minimal intergroup context was created and participants in the ritual condition performed the actions one last time. In Experiments 1 and 2 (B) participants interacted in multiple rounds of the Trust Game; in Experiment 4 (C) participants observed both ingroup members and outgroup members separately receive performance feedback while measuring participants' EEG.

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 completing the actions over the course of the week, participants were led 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 these action sequences 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.

Table 1.

Simple ritual (Experiment 2)	**Intermediate ritual** (Experiment 1 and 3)	**Elaborate ritual** (Experiment 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	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	2. Get a cup or mug of some sort available. Fill it halfway with lukewarm water – being careful that the water isn't is too hot or too cold. Gently submerge

times. Close your eyes and bow your head.	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.	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.
		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.

Table 1. The three different sets of instructions of sequences that were given to participants in experimental ritual conditions, across all four experiments. The simple-ritual sequence was short, simple, and less demanding; the elaborate-ritual sequence was long, effortful, and had stricter rules; and the intermediate-ritual sequence fell in between.

The two-minute long, ad-hoc ritual was comprised of a series of sequences 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., “ensure you complete the sequences exactly in this way and not in any other”; Norton & Gino, 2013). Note that there was no mention of “ritual” during the week-long study; the word ritual itself is imbued with preconceived notions of the psychological effects of rituals. Along with the written action sequences, participants were also told to watch a video of a model displaying the full set of actions. Participants were asked to learn and memorize the sequences over the course of the week.

All participants were sent email reminders every day during the at-home portion. Participants in the ritual condition were reminded to perform the actions and were provided with the video as an aid for the first three days (the last three days did not include the video). In order to equate time spent, participants in the no-ritual condition completed filler questions related to cognitive processing for the same amount of time. Compliance of the at-home portion (completing the actions and survey) was tracked using the survey page option, which allowed us to see whether participants had logged in to watch the video and complete the action sequences. The experimenter checked this information for each participant, and low-compliance participants (completing the actions/survey less than 3 times out of 7 days) were removed from the final analysis. Low compliance was also confirmed during the funnel debriefing in which participants were asked to report the number of times they completed the actions over the course of the week.

After one week, participants completed the in-lab portion of the experiment in groups of four: three participants and one confederate. Participants were informed that the experiment was a group experiment; to manipulate the group variable, the experimenter – seemingly using the saved data from participants' dot-counting responses earlier in the week – informed participants (and confederates) that their data showed them to be under-estimators, assigning them to the red group. The assignments were revealed at this point to minimize the amount of group experience. Participants were told that the outgroup – whose data showed them to be over-estimators – were assigned to the blue group and were ostensibly 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 headshot of each participant was then taken and they were informed that each person's picture would be uploaded to the main computer to be used later in the experiment; in fact, the only images that were loaded were those of the confederates. Participants in the ritual condition then performed the action sequences one final time: they lined up beside one another, facing a wall such 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/cooperation (Wiltermuth

& Heath, 2009), the timing of the actions was intentionally staggered by ten seconds to prevent participants from coordinating with each other's movements – allowing us to examine the effects of novel ritual controlling for any effect of synchrony. Participants in the no-ritual condition spent the same two minutes completing another round of dot-counting in their group, controlling for group salience and time, and told that this confirmed their previous grouping assignment. By having participants in the control condition complete a survey for the same amount of time, and confirming their group assignments, the assumption was that all participants would have their identity validated equally.

Finally, participants completed a series of rounds with multiple partners of the Trust Game (Berg, Dickhaut, & McCabe, 1995; Sapienza, Toldra-Simats, Zingales, 2013). The Trust Game is an economic game assessing social preferences that has been used extensively to measure trusting and trustworthy behavior between two or more people (e.g., Sapienza, Toldra-Simats, Zingales, 2013). As a result, it is well-suited for testing (economic) trust between members of different groups. In two rounds of the game, participants "interacted" with a member of their ingroup and outgroup separately on a computer. Unknown to participants, the images were of confederates. In each round, participants (always in the role of the sender) were allocated $10 with the option to send any amount (or none) to the receiver. They were instructed that the initial amount sent would be tripled and given to the receiver who then had the option to either keep all of the money or return any amount of it to the sender. The game was explained as follows: If player 1 (sender role) sends player 2 (receiver role) all of their $10 endowment, this $10 amount becomes tripled upon being received by player 2 ($30). In the second exchange (which did not actually occur, but participants were lead to believe that there was a second play), player 2 is then given the option to reciprocate the offer and send any amount of the $30 back to

Player 1. A perfectly cooperative exchange would be player 1 fully trusting player 2 (sending entire \$10) to fairly reciprocate the offer (signaling their trustworthiness) by splitting the \$30, \$15 to each player. Participants understood that in order to gain more than their original endowment, they would need to trust player 2 with a certain amount; the more money sent to player 2, the higher this individual payout, but the greater the risk of the endowment being lost. To make sure that participants understood the logic of the exchange, they were provided with different combinations of payouts and winning/losing contingencies. They continued with the real interactions only once they understood how everything worked. Importantly, to have participants treat the task as an economic exchange with real consequences, they were told that their individual outcomes from each interaction/round would be added and averaged together, and that they would take this amount home in cash. Because there was no actual player 2 in any of these interactions, the participants across both conditions took home the original \$10 regardless of their decisions during the rounds.

Results

A 2-level multilevel model was used to account for Trust Game trials (i.e., amount sent) nested within participants by estimating a random intercept for each person (see Table 2 for the descriptive data of first three experiments). We used an unstructured covariance matrix and the between-within method of estimating degrees of freedom. Effect sizes were estimated with *semi-partial* R^2 (Edwards, Muller, Wolfinger, Qaqish, & Schabenberger, 2008).

The model revealed a main effect of group status on the amount of money entrusted (b = -0.32, SE = 0.07, $t(99) = -4.86$, $p < .000004$, 95% CI = [-0.46, -0.19], *semi-partial* R^2 = 0.167) such that participants entrusted more money to ingroup than outgroup members. Most importantly, the interaction between ritual and group status was significant (b = -0.19, SE =

0.07, $t(99) = -2.86$, $p = .005$, 95% CI = [-0.32, -0.06], *semi-partial* $R^2 = 0.08$). As predicted, participants in the ritual condition entrusted significantly less of their money to outgroup ($M_{outgroup} = 5.29$, $SD = 2.80$) than ingroup members ($M_{ingroup} = 6.30$, $SD = 2.80$), $t(99) = -5.05$, $p = .000002$ whereas participants in the no-ritual condition entrusted comparable amounts ($M_{outgroup} = 5.82$, $SD = 3.08$; $M_{ingroup} = 6.10$, $SD = 3.14$), $t(99) = -1.55$, $p = .12$ (Figure 2). Examining the between-factor simple effects and comparing across condition, we found no statistical differences either for the ingroup comparisons or the outgroup comparisons (all *p*s > .25). As a result, comparing across conditions it is not possible to infer whether the bias caused by the ritual was driven more by ingroup trust or outgroup distrust.

Figure 2.

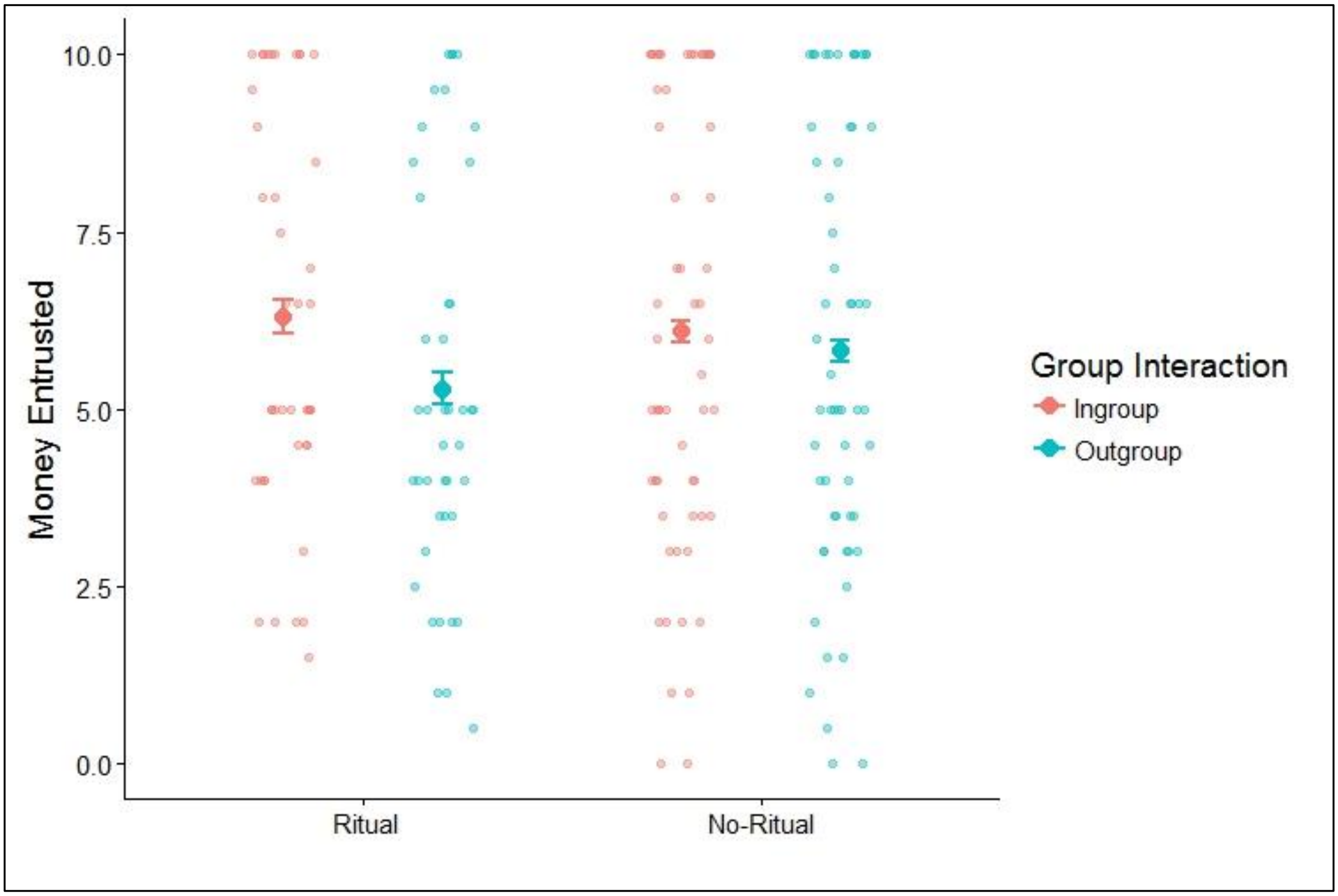

Fig. 2. Evidence that a minimal ritual leads to increased biases in trust behaviors. Least-squares predicted means display the amount of money entrusted as a function of ritual (ritual versus no-ritual control) and group (ingroup or outgroup member; Experiment 1). 95% confidence interval error bars.

Table 2.

Experiment	Ingroup giving	Outgroup giving
Experiment 1		
Ritual condition (intermediate weeklong)	6.30_a (2.80)	5.29_b (2.80)
No-Ritual condition (weeklong)	6.10_a (3.14)	5.82_a (3.08)
Experiment 2		
Ritual condition (elaborate weeklong)	6.38_a (3.09)	5.66_b (3.35)
Ritual condition (simple weeklong)	6.10_a (3.41)	5.79_a (3.36)
No-Ritual condition (weeklong)	6.40_a (3.0)	6.47_a (3.20)
Experiment 3		
Ritual condition (intermediate one-time)	5.34_a (2.97)	4.94_b (3.0)
No-Ritual condition (one-time)	5.23_a (2.67)	5.05_a (2.79)

Table 2. Means (SD) on Ingroup versus Outgroup Giving on the Trust Game for Experiment 1 (intermediate weeklong ritual), Experiment 2 (simple and elaborate weeklong rituals), and Experiment 3 (intermediate one-time ritual). Means across rows with different subscripts differ significantly at $p < .05$ (two tailed).

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 others (ingroup members) or not (outgroup members), was enough to lead to biases in economic giving behavior.

The observed biases may be 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 (Xygalatas et al., 2013) and conflict between (Sosis, Kress, & Boster, 2005) groups. Bringing this concept into the laboratory for a causal exploration, in a second experiment we test the idea that variations in ritual effort or elaboration (i.e., actions that are "costly") will modulate intergroup bias. We used the same novel ritual paradigm – but varied the extent to which the ritual was simple or elaborate – to explore a moderating role for effort in the effect of rituals on intergroup bias.

Experiment 2—Methods

Participants

One hundred students at the University of Toronto Scarborough participated for course credit with the possibility to earn additional money (as in Experiment 1). Ten participants from the two ritual conditions were excluded from analyses because they failed to complete the assigned at-home portion of the experiment (less than 3 times over the course of the week; $M_{total\ compliance}$ = 4.73). None of the participants expressed suspicion of the role of confederates. Participants were randomly assigned to one of three conditions: an elaborate-ritual condition (n = 29), a simple-ritual condition (n = 30), and a control no-ritual condition (n = 31). The final sample consisted of ninety participants (27 male, 59 female, 4 unreported; M_{age} = 20.3, SD = 2.83).

The final sample size was determined using an a priori power analysis with *G*Power* software (Faul et al., 2007). Specifically, we partially based our power analysis on the results from Study 1, where the correlation between our repeated measures was r = 0.89 and the effect size was semi-partial R^2 = .08. To be conservative, we used a smaller effect size of d = 0.3 (smaller than the average effect in social psychology) and a correlation of r = 0.8 for the repeated dependent variables. With these as inputs into G*Power with a mixed design with three groups, we could achieve 80% power in detecting the omnibus interaction effect with 48 participants. We decided to collect data until the end of the term, assuming we collected at least 48 participants.

Procedures

The procedure and design was similar to that of Experiment 1, with the same week-long at-home portion and minimal group manipulation, with a few key changes. To increase the credibility of the grouping assignments, we included four extra confederates who acted as the minimal outgroup members and who, after being designated the outgroup, were taken to the neighboring room. That is, participants began the experiment in the presence of both ingroup and outgroup members. In addition, we altered the experiment’s framing. Whereas in the first experiment the action sequences were 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 sequences without any preconceived notion of ritual, including any reference to ancient cultural practices.
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 were made up of 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, less stringent in its rules and guidelines, and involved less repetition; the elaborate-ritual sequence was longer and more involved, stricter in its rules, and also utilized coins and water as ritual instruments (Table 1). Using a piloted pretest, the three rituals (intermediate ritual in Study 1; simple and elaborate ritual in Study 2) were independently rated by a separate sample (N=54) on their level of perceived effort and elaborateness using a 0 (no effort, very simple) to 100 (extreme effort, very elaborate) scale. Analyses confirmed that the simple ritual was judged as least effortful and most simple (M_{effort} = 47.9; $M_{elaborateness}$ = 51.4), the elaborate ritual as most effortful and most elaborate (M_{effort} = 63.7; $M_{elaborateness}$ = 73.5), and the intermediate ritual as falling in between (M_{effort} = 55.2; $M_{elaborateness}$ = 58.8) (effort: F (2,53) = 19.37, p < 0.001, $\acute{\eta}^2_p$ = 0.27; elaborateness: F (2,53) = 22.32, p < 0.001, $\acute{\eta}^2_p$ = 0.30). The same at-home portion and in-lab minimal group manipulation was used as in Experiment 1. Participants completed the Trust Game like before, but instead of two interactions, there were four – two ingroup confederate interactions and two outgroup confederate interactions. As in Experiment 1, there was a week-long at-home portion and a final in-lab testing session.

Results

Similar to Experiment 1, a 2-level multilevel model was used to account for Trust Game trials (i.e., amount sent) nested within participants by estimating a random intercept for each person. The model revealed a significant main effect of group status on money entrusted (b = 0.16, SE = 0.08, t(267) = 2.11, p = .035, 95% CI = [0.01, 0.31], *semi-partial* R^2 = 0.05): participants entrusted more money to ingroup than outgroup members. A model comparison test was performed with a chi square test of independence, in which the test statistic degrees of freedom is reported as the difference in the degrees of freedom between the omnibus and interaction-removed models (e.g., West, Aiken, & Krull, 1996). This revealed a small effect, $\chi^2(2) = 4.97$, $p = .08$, partially supporting the inclusion of the interaction term as the better fitting model, despite the non-significant test result.[i]

Despite the non-significant omnibus interaction ($p = .08$), we examined the condition pairwise comparisons, given the findings from Experiment 1 and our predictions regarding trust game allocations for the elaborate versus simple ritual conditions. From this, we found that participants in the elaborate-ritual condition entrusted significantly less money to outgroup members ($M_{outgroup} = 5.66$, $SD = 3.35$) than ingroup members ($M_{ingroup} = 6.38$, $SD = 3.09$), $t(267) = 2.58$, $p = .01$, an effect that was absent in the simple-ritual condition ($M_{outgroup} = 5.79$, $SD = 3.36$; $M_{ingroup} = 6.10$, $SD = 3.41$), $t(267) = 1.52$, $p = .13$, and no-ritual control condition ($M_{outgroup} = 6.47$, $SD = 3.21$; $M_{ingroup} = 6.40$, $SD = 3.0$), $t(267) = 0.50$, $p = .62$. Finally, looking at the between-factor simple effects and comparing across condition, we found no statistical differences either for the ingroup comparisons or outgroup comparisons (all *p*s > .25).

Figure 3.

Fig. 3. Evidence that variation in ritual costliness modulate biases in trust behaviors. Least-squares predicted means display the amount of money entrusted as a function of ritual type (elaborate ritual versus simple ritual) and group (ingroup or outgroup member; Experiment 2). 95% confidence interval error bars.

These results offer partial evidence that differences in ritual effort – varied at a basic level in our paradigm – produce different levels of intergroup bias. Although we did not find a significant interaction between type of ritual and the amount entrusted in the Trust Game, the simple effects offered modest support for our predictions. Although the amount of relative effort for the elaborate ritual registers a minor cost for the individual, especially compared to the onerous behaviors of real-life rituals, it is sufficient to elicit intergroup bias. Critically, we did not observe intergroup bias in the simple ritual condition, suggesting 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: only those with an intermediate or elaborate amount of effort generate bias, suggesting that rituals must include certain features to inculcate bias.

The novel rituals in the first two studies possessed 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.

Experiment 3—Methods

Participants

One hundred and twenty three students at the University of Toronto Scarborough participated for course credit with the possibility to earn additional money (as in the previous two experiments). Four participants were excluded from the analyses because they saw the confederates from the previous study slot and were suspicious of their role as participants. Participants were randomly assigned to one of two conditions: a one-off ritual condition (n = 66) and a control no-ritual condition (n = 53). The final sample consisted of one hundred and nineteen participants (40 male, 79 female, M_{age} = 18.35, SD = 1.25). The final sample was determined by assuming a similar effect size as the previous two experiments. Data collection was stopped when a similar number reached. In line with open science practices, we preregistered the current experiment on Open Science Framework (osf.io/82bnc), where we outline in detail the methods, design, and hypotheses that were considered prior to the start of data collection.

Importantly, we also include the output of an a priori power analysis from G*Power (Faul et al., 2007). Given that we are testing if *very* minimal rituals could be effective in shaping intergroup dynamics, we conducted an a priori power analysis with conservative values so that we had more certainty in the robustness of our results, including a small effect size of $d = 0.26$ and a correlation between dependent variables that was smaller than was uncovered in studies 1 and 2, namely $r = 0.7$. Such a power analysis reveals that we can achieve 95% power for detecting the omnibus interaction in our mixed design study with $N = 118$. We decided to collect data until the end of the term, assuming we collected at least 118 participants.

Procedures

The same procedures were used as in the previous two experiments for the minimal group manipulation. Unlike the other two experiments, however, there was no at-home portion: Ritual participants learned and performed the ritual during their one and only lab visit. Thus, while participants were assigned to minimal groups, like the previous experiments, they did not repeat the rituals over a series of days. Participants in the one-time ritual condition then performed the action sequences, standing shoulder-to-shoulder and staggering their start times like before. Each participant followed along to the instructions of the sequence steps, which were printed on a piece of paper on the wall. The intermediate ritual sequence from Experiment 1 was used to ensure that the actions were sufficiently elaborate. Controlling for group salience and time, participants in the no-ritual control condition spent the same two minutes completing a round of personality items, and told that the results confirmed their grouping assignment. They finished with four rounds of the Trust Game.

Results

The same multilevel model was used 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, t(356) = -2.63, p = .009, 95% CI = [-0.25, -0.04], *semi-partial* R^2 = 0.02) such that participants entrusted more money to ingroup than outgroup members. Contrary to our original predictions, however, the interaction between ritual and group status was not significant (b = -0.06, SE = 0.06, t(356) = -1.01, p = .31, 95% CI = [-0.16, 0.05], *semi-partial* R^2 = 0.002), suggesting that participants in the one-time ritual condition were no more biased in their money entrusting ($M_{ingroup}$ = 5.34, SD = 2.97; $M_{outgroup}$ = 4.94, SD = 3.0) than participants in the no-ritual control ($M_{ingroup}$ = 5.23, SD = 2.67; $M_{outgroup}$ = 5.05, SD = 2.79; Figure 4).

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

Taken together, Studies 1 – 3 reveal that not all novel rituals impact group bias. The most minimal form of rituals appear not to be sufficient, suggesting that effort, repetition, and time are critical ritual elements for producing bias. Because this interpretation is based in part on a null finding, however, further confirmatory testing is essential.

Figure 4.

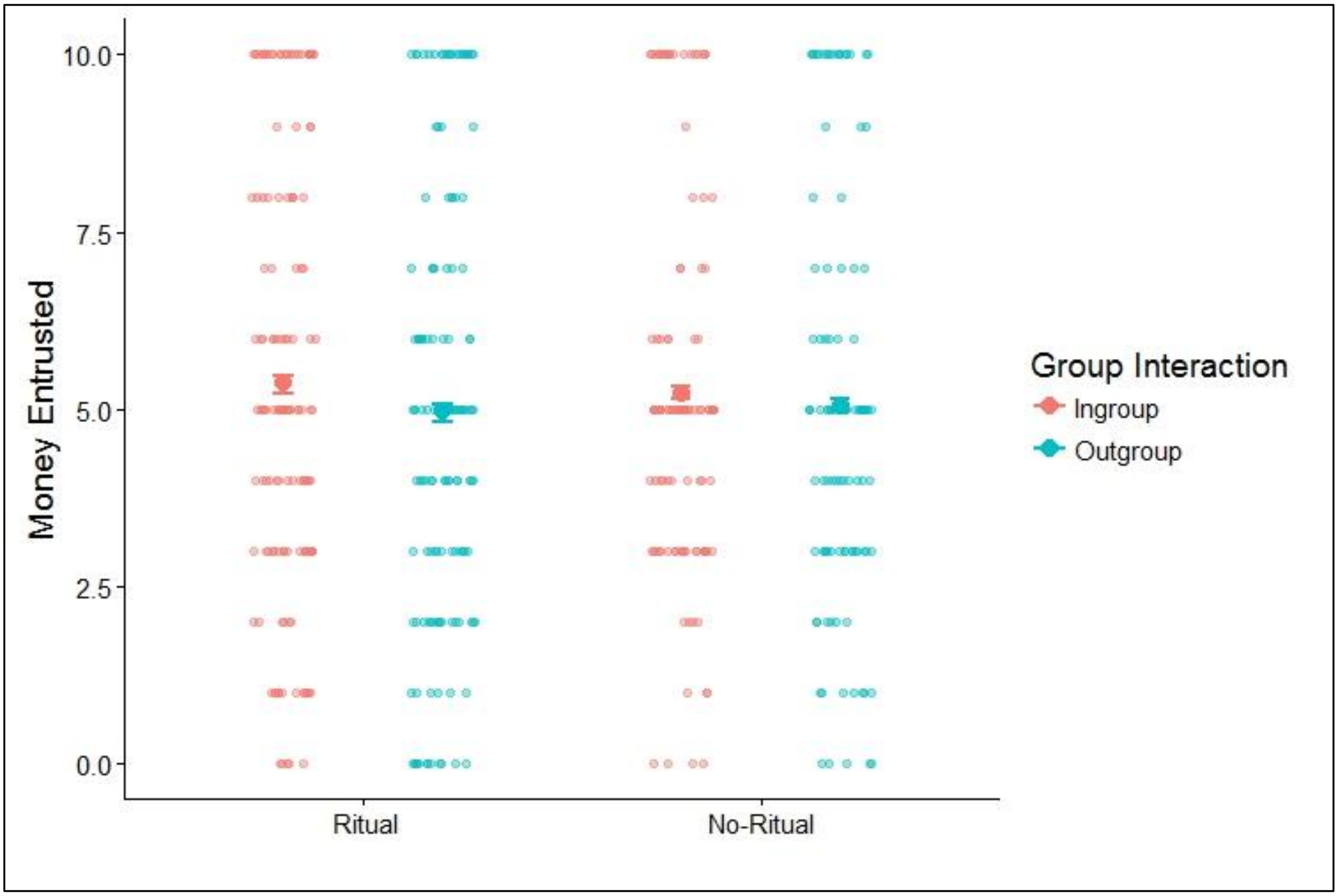

Fig. 4. Least-squares predicted means display the amount of money entrusted as a function of ritual type (one-time ritual or no-ritual control) and group (ingroup or outgroup member; Experiment 3). 95% confidence interval error bars.

Why do rituals create intergroup bias? The presence of a neural system attuned to processing the actions and outcomes of others – to such an extent that individuals represent the actions of others as their own – offers one underlying brain mechanism. The first three experiments offer modest support for the notion that, under some condition, collective rituals can influence behavior toward members of ingroups and outgroups, suggesting that rituals heighten sensitivity to evaluating others as members of ingroups or outgroups. As a result, rituals may be represented in brain systems that underlie the evaluative observation of others (e.g., de Bruijn, Miedl, & Bekkering, 2011). In particular, this neural circuitry should be modulated as a function of whether the ritual experience has been shared with others (ingroup) or not (outgroup). Experiment 4 leverages recent advances in neuroanthropology exploring the neural basis of group ritual behaviors (e.g., Schjoedt et al., 2013). Experiment 4 thus examines the effect of ritual on intergroup reward processing; specifically, assessing rapid changes in neural performance monitoring while participants observed ingroup members and outgroup members receive rewarding and punishing outcomes.

Experiment 4—Methods

Feedback event-related potentials (ERPs)

We assessed continuous electroencephalographic (EEG) data, focusing on two related evoked brain potentials or event-related potentials (ERPs) known to be associated with rapid, online performance monitoring and reward processing: the feedback-related negativity (FRN) and the feedback-P300. The FRN, peaking at fronto-central sites, is an early negative-going waveform occurring 200-300 ms after the occurrence of punishing and 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 200-500 ms after receiving feedback and often codes the representations of feedback or reward magnitude. 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 others' outcomes exhibit analogous waveforms with a similar latency and morphology as when experiencing those outcomes themselves: The observer-FRN (oFRN) is thought to underlie the automatic and motivational evaluation of others' performance (van Schie, Mars, Coles, & Bekkering, 2004); the observer f-P300 amplitude has also been shown to track outcomes as a function of varying interpersonal contexts (Picton, Saunders, & Jenzsch, 2012). The oFRN, as a system of automatic motivational reward evaluation, may be too early to code group membership, and so we might only expect to see modulations as a function of member interaction in the controlled appraisal system of the later occurring f-P300.

Participants

Fifty-nine students at the University of Toronto Scarborough participated for course credit and bonus pay. Nine participants were excluded from all analyses due to computer/hardware malfunction (n = 4), high EEG artifact rates (>30% artifacts; n = 2), and either personally knowing the confederates (n = 2) or being aware of the experiment's purpose (n = 1), leaving a sample of 50 participants (35 females, 15 males; M_{age} = 21.4, SD = 5.67).[ii] All participants completed the at-home portion of the experiment (at least 3 times over the week; $M_{total\ compliance}$ = 6.24) and so none were dropped from the analyses due to 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 relatively smaller sample size is typical of most neurophysiological and neural imaging studies, where 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. Specifically, when conducting a power analysis with G*power inputting a small effect d=0.3 and a correlation of r = 0.8 (justified in data, where correlations ranged between r = 0.7 and r = 0.9), we could achieve 80% power for the omnibus interaction effect with 38 participants. We decided to collect data until the end of the term, assuming we collected at least 38 participants.

Procedure

The same procedures were used as in Experiment 2, including the at-home and in-lab portions of the experiment. During the in-lab portion (which followed the at-home, repeated ritual, portion), the participant and one confederate were assigned to the red group (ingroup) and two confederates were assigned to the blue group (outgroup). The experimenter told the group that there was only enough time to complete the EEG set up for one person, and that a name would be randomly drawn to determine who would complete the session. The name of the participant was drawn in each case. Participants in the elaborate-ritual condition then performed the ritual in the lab, while those in the no-ritual condition completed the dot-counting manipulation for the same amount of time, ostensibly as confirmation of their group assignment. The full design was a 2 (ritual: elaborate-ritual or no-ritual) X 2 (group: ingroup or outgroup) X 2 (outcome: reward or punishment), with repeated measures on the final two factors.

Punishment and reward observation. We used continuous EEG to measure participants' ERPs while (i) receiving feedback on a task and (ii) while observing ingroup and outgroup members receive similar feedback. The experimenter told participants that they would have the chance to earn extra bonus money in a cognitive/perception timing task where they would alternate in separate blocks as either the one doing the task (i.e., as the performer receiving their own feedback) or the one watching (i.e., as the observer viewing other members receive feedback). To clarify, only one participant was hooked-up for EEG, and this participant sometimes performed the task and other times observed others perform the task. We present only the observation-rounds data in the current paper, in line with the hypotheses we set out to test. The performance-rounds data were analyzed separately and discussed in another paper.

We used a time-estimation task in order to provide feedback to participants (Miltner, Braun, & Coles, 1997): on each trial a central fixation cross was presented for 250 ms, followed by a blank screen, and participants were instructed to press the spacebar when they believed exactly 1 s had passed since the appearance of the fixation cross. Visual performance feedback was provided 2 s after the initial fixation cue, resulting in an approximately 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 while in the performer role, the more correct responses they provided, the more bonus pay they would earn, while in the observer role their task was to pay close attention to feedback being given to the performer. In order to ensure that participants were engaged during the observer role, they were tested with intermittent recall (e.g., “Was the last trial you observed correct or incorrect?”).

Participants received either rewarding (“win money”) or punishing (“lose money”) feedback depending on whether their response fell within a predefined time window centered around 1 s after the appearance of the fixation cross. Unknown to participants, this time window was adaptively calibrated over the course of each block, such that the window was decreased after a correct response and increased after an incorrect response. As a result, the occurrence of rewarding and punishing stimuli was roughly equal in each performance/observer block.

Neurophysiological recording and analysis. Continuous EEG was recorded during the four blocks of the time-estimation task using a stretch Lycra cap embedded with midline electrodes (Electro-Cap International, Eaton, OH). Recordings used average ear and a 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. Offline, EEG was analyzed with Brain Vision Analyzer 2.0 (Brain Products GmbH, Munich, Germany). EEG data was corrected for vertical electro-oculogram artifacts (Gratton, Coles, & Donchin, 1983). An automatic procedure was employed to detect and reject artifacts. The criteria applied were a voltage step of more than 25 µV between sample points, a voltage difference of 150 µV within 150 ms intervals, voltages above 85 µV and below -85 µV, and a maximum voltage difference of less than 0.50 µV within 100 ms intervals. These intervals were rejected from individual channels in each trial.[iii]

Results

oFRN Analyses. For oFRN activity, a 2 (condition: ritual vs. no-ritual) X 2 (feedback type: reward vs. punish) X 2 (interaction: ingroup vs. outgroup) mixed-factorANOVA revealed a significant main effect of ritual, F (1,48) = 5.53, p = .02, $ή^2_p$ = 0.10; across feedback type and ingroup/outgroup observation, participants in the elaborate-ritual condition showed significantly higher oFRN amplitudes than participants in the no-ritual condition (see Figure 4a for the oFRN components and corresponding head-maps). There was also a significant two-way interaction between ritual and feedback type, F (1,48) = 8.90, 95% CI = [0.10, 1.24], p = 0.004 .01, $ή^2_p$ = 0.16. Planned pairwise comparisons indicated that participants in the ritual condition differentiated between witnessing others receive punishing feedback (M = -1.30 µV, SD = 0.60) and rewarding feedback (M = -1.11 µV, SD = 0.59), t(48) = 2.74, p = .009, mirroring the typical loss-gain differentiation in receiving feedback oneself. In contrast, participants in the no-ritual condition showed equivalent oFRN amplitudes in response to others' punishments (M = -0.78 µV, SD = 0.60) and rewards (M = -0.88 µV, SD = 0.59), t(48) = 1.49, p = .14.[iv] However, going against our predictions, there was a non-significant three-way interaction between condition, feedback type, and group interaction, F (1,48) = 0.61, 95% CI = [0.10, 1.24], p = 0.44, $\acute{\eta}^2_p$ = 0.01, suggesting the loss-gain differentiation of the oFRN was not modulated by group status in either condition.

Together, rituals increased the early, automatic monitoring of the outcomes of others, a pattern not evident for participants who did not perform rituals, suggesting that rituals heighten sensitivity to social outcomes and make group membership more salient, although contrary to our predictions there was no difference in oFRN across group status in either condition.

Figure 5.

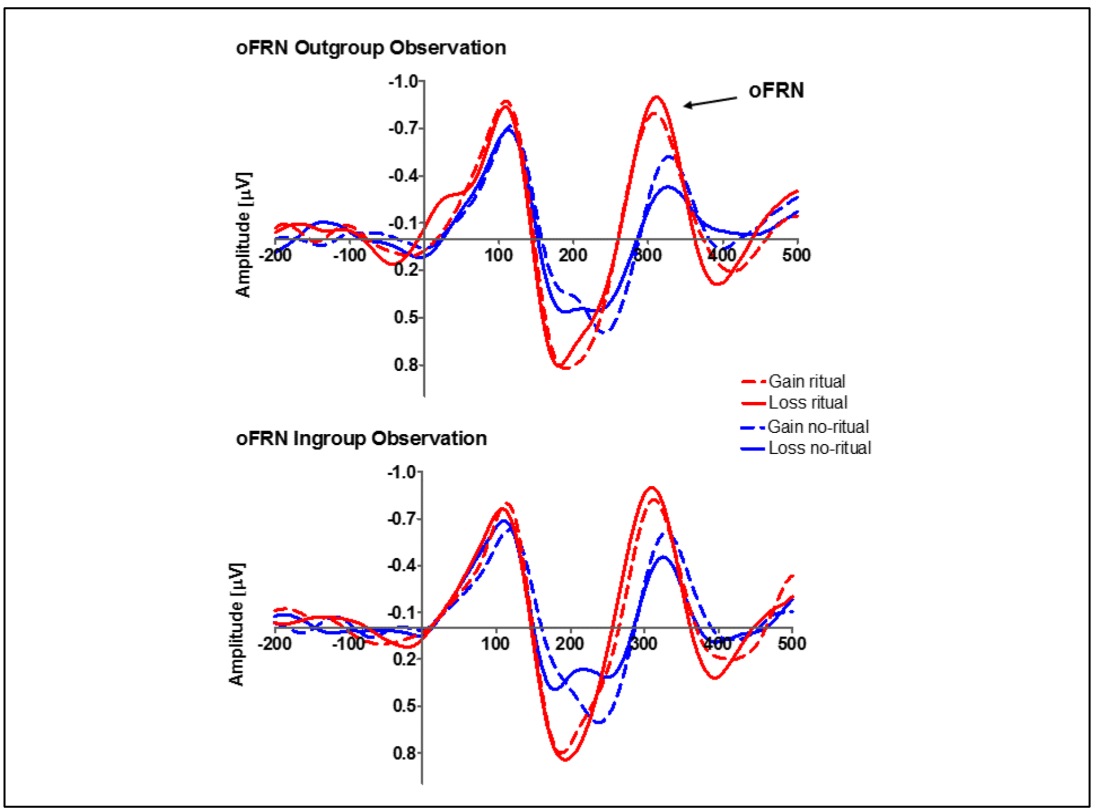

Fig. 5. Feedback-locked waveforms at Fz illustrating oFRN following losses (“lose money”) and gains (“win money”) feedback, while observing outgroup members (top panel) and ingroup members (bottom panel).

f-P300 Analyses. All analyses were conducted on the f-P300 difference-wave (gain trials – loss trials; Δf-P300), with larger positive Δf-P300s indicating a motivated attention to reward relative to punishment. In this case, a difference-wave approach provides a more reliable estimate of the effects compared to looking at the raw ERP estimates. A 2 (condition: ritual vs. no-ritual) X 2 (interaction: ingroup vs. outgroup) mixed-factor ANOVA for the Δf-P300 revealed no main effects (*ps* > 0.25), but a significant two-way interaction between condition and group, *F* (1,48) = 4.41, 95% CI = [0.02, 1.15], *p* = .04, $ή^2_p$ = 0.08. Simple effects tests revealed that participants in the ritual condition showed opposite patterns of differentiated activity across group member observation, *t* (48) = 2.04, *p* = .047, such that they elicited more positive Δf-P300 processing while observing ingroup members (*M* = 0.23, *SD* = 1.26), but a mirrored (more negative) Δf-P300 pattern while observing outgroup members (*M* = -0.73, *SD* = 2.18). Participants in the no-ritual condition showed comparable Δf-P300 activation during ingroup member (*M* = -0.10, *SD* = 1.53) and outgroup member observation (*M* = 0.34, *SD* = 1.83), *t* (48) = 0.93, *p* > .25. Further, pairwise comparisons across conditions revealed a marginal effect for outgroup observation, *t*(48) = 1.87, *p* = .07, but no effect for ingroup observation, *t* (48) = 0.85, *p* > .25, indicating that the mirrored Δf-P300 pattern in the ritual condition, though marginal and perhaps inconclusive, is consistent with the behavioral findings that the source is at the level of the outgroup. See Figures 6a (ritual condition) and 6b (no-ritual condition) for the Δf-P300 components and corresponding head-maps.

Interestingly, self-generated Δf-P300 activation typically reveals a greater relative positivity when calculating gains – losses (Leng & Zhou, 2010); given that this neural component reflects a system of generic reward processing and conscious appraisal of gains to the self (as reflected in more positive Δf-P300s), these results are consistent with the idea that ritual participants tracked rewards to ingroup members in a manner consistent with personal gains to the self, while showing the opposite pattern specifically for outgroup punishment. Taken together with the behavioral effects in the above experiments, the f-P300 findings are consistent with the idea that rituals instill a motivation to see both rewards to ingroup members *and* punishments to outgroup members as motivationally salient (i.e., schadenfreude) (Cikara, Bruneau, Van Bavel, & Saxe, 2014).

We add a final note regarding these analyses: In our original hypotheses, we had stronger predictions that the group differentiation in observed-based ERPs would begin at the oFRN; the fP300 effects were more exploratory. The data reveal that although other-person monitoring indicated in the oFRN was heightened for participants in the ritual condition, the expected differentiation between group members were only observed in the fP300 and not in the oFRN as initially predicted. Still, this is somewhat consistent with the idea that the brain's responses to outcome evaluations may be divided into an early, semi-automatic process (oFRN) and a later, more top-down appraisal process (fP300), with the contextual cues of group dyanmics only encoded later in the fP300 (e.g., Leng & Zhou, 2010). Admittedly, given the exploratory nature of some of these questions (e.g., the effects with the fP300), confirmatory future research will need to be done to test the reliability and robustness of the findings. Until then, we urge caution in the interpretation of the findings.

The two ERP findings offer preliminary and exploratory evidence of a candidate neural mechanism, which could help to explain ritual's various group-based outcomes. Specifically, rituals operate by recruiting the early, automatic monitoring of others, making membership and affiliation, especically in this case of intergroup contexts, a much more salient social cue brought to mind. At the same time, it is important to note that this is only one possible mechanism that offers insight into the effect of rituals. While we provide evidence of the involvement of lower level brain-based processes, it is likely that there are multiple joint operating mechanisms, which implicate both bottom-up neural and top-down psychological systems.

Figure 6.

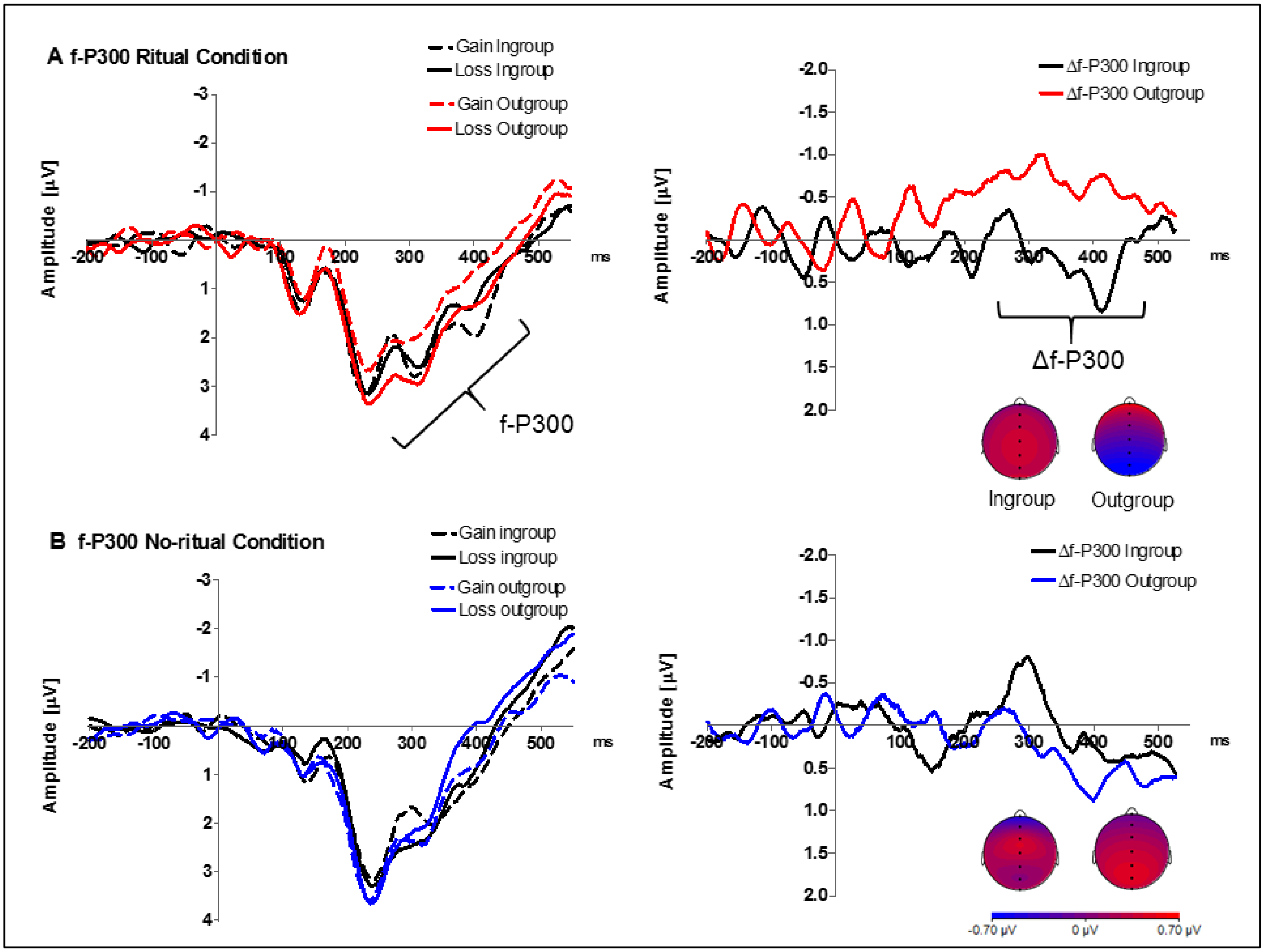

Fig. 6. Feedback-locked waveforms at Pz illustrating f-P300. The waveforms during ingroup observation across both conditions are depicted in black lines, and the waveforms during outgroup observation are depicted in different colors (red and blue) for the ritual condition (A) and no-ritual condition (B) groups. Loss and gain-related ERPs (left panels) and difference-wave ERPs (gain trials – loss trials; right panels) are shown independently (Experiment 4). Spline head maps: Scalp distributions of the Δf-P300 (mean activity 301 – 467 ms) for ingroup observing and outgroup observing in both conditions (Experiment 4).

Discussion

Four experiments offer modest and mixed support for the notion that novel rituals – devoid of cultural meaning and history – can induce intergroup bias. Results across the experiments suggest that novel rituals only lead to bias when they necessitate sufficient effort and repetition over time. Existing theoretical arguments propose that rituals serve a social function, facilitating effective group living through galvanizing ingroup loyalty. We utilized novel rituals in an effort to document that rituals alone – stripped of any broader cultural resonance – impact intergroup bias. We find only partial support, with our mixed evidence consistent with the notion that the presence of certain features is critical for rituals to impact bias. In Experiment 2 we fail to find an effect in the simple ritual condition, suggesting that rituals must be sufficiently effortful and costly, and, contrary to our pre-registered predictions, the null effect in Experiment 3 with one-time rituals suggests the importance of repetition and time.

Revising our original hypotheses, it appears that it is the case that only certain novel rituals generate bias; in other words, when rituals are too minimal – lacking elements present in real-world rituals such as repetition, time, and effort – 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 pre-registered hypothesis in Experiment 3, many of our conclusions are preliminary: future research is needed to systematically determine which features are required (and which are not) 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 this bias. It is unclear whether they are products of ingroup liking or outgroup antagonism. Costly signaling theory posits that extravagant rituals strengthen ingroup ties, but does not make specific predictions about outgroup hostility. Although the current investigation does not offer conclusive evidence, our initial results suggest that outgroup 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 ingroups and against outgroups – especially when ingroups and outgroups perform competing rituals, as, for example, with members of different religious faiths who are similar but different (Brewer & Pickett, 1999).

We note that across the three behavioral studies we fail to find a basic minimal group effect in the non-ritual control condition, a null findings that is potentially at odds with an established social identity literature. However, whereas the traditional methods used in minimal group studies typically use point allocation matrices (e.g., Tajfel, 1974), the present set of studies relied exclusively on the Trust Game as the measure of bias.

Taken together, we find modest evidence of the psychological and neural basis of novel ritual and their effect on intergroup bias. Cultural stabilization of ritual began in human evolution when fast-growing groups began to experience elevated intergroup competition, necessitating ingroup cooperation (Norenzayan et al., in press). In line with these theoretical claims, the current results partially support the claim that rituals offer a strategy for the regulation of ingroup behavior – but at a detriment to the outgroup.

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 paper.

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Notes

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[i] Because we are examining an interaction between two categorical variables, one of which has more than two levels (i.e., 3 experimental conditions), the way to test for significance is by using a model comparison approach as we did here. The standard interaction terms that are in the output are ignored in such cases because they do not consider the variance accounted for by including the 3-level categorical variable in the nested model (see Aiken, West, & Krull, 1996 and Judd & McClleland, 1989).

[ii] Analyses for oFRN and the fP300 including these three participants were similar. For the oFRN, the main effect of condition, F (1,51) = 4.01, p = .05, $ή^2_p$ = 0.07, and the interaction between condition and feedback type, F (1,51) = 6.96, p = .01, $ή^2_p$ = 0.12, remained significant. For the fP300 model, the two-way interaction between condition and group remained significant, F (1,51) = 4.57, p = .037, $ή^2_p$ = 0.08.

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

[iv] As a more conservative test on the oFRN and attesting to the robustness of these effects, we conducted an additional analysis using a base-to-peak method, taking the difference between the maximum value between 250 ms and 350 ms following feedback presentation and the most

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negative point between this maximum and 350 ms following feedback onset. We find a non-significant main effect of ritual, *F* (1,48) = 3.30, *p* = .08, $ή^2_p$ = 0.06 across all feedback types and ingroup/outgroup observation.. As before, however, there was a significant two-way interaction between ritual and feedback type, *F* (1,48) = 5.57, 95% CI = [0.10, 1.24], *p* = 0.02, $ή^2_p$ = 0.10. The simple effects tests reveal the same pattern as the original analysis.