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Moralizing gods, impartiality, and religious parochialism across 15 societies

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1 **Abstract**

2 The emergence of large-scale cooperation during the Holocene remains a central problem in the human
3 evolutionary literature. Among several contributing mechanisms, one hypothesis points to culturally
4 evolved beliefs in punishing, interventionist gods that facilitate the extension of cooperative behavior
5 toward geographically distant co-religionists who are unlikely to reciprocate. Furthermore, another
6 hypothesis points to such mechanisms being constrained to the religious ingroup, possibly at the
7 expense of religious outgroups. To test these hypotheses, we administered two behavioral experiments
8 and an extensive set of interviews to a sample of 2,228 participants from 15 diverse populations. These
9 populations included foragers, pastoralists, horticulturalists, subsistence farmers and wage laborers,
10 practicing world religions such as Buddhism, Christianity, and Hinduism, but also different forms of
11 animism and ancestor worship. Using the Random Allocation Game (RAG) and the Dictator Game (DG) in
12 which individuals allocated money between themselves, local and geographically distant co-religionists,
13 and religious outgroups, we found that higher ratings of gods as monitoring and punishing reliably
14 predicted decreased local favoritism (RAGs) and increased resource-sharing with distant co-religionists
15 (DGs). The effects of punishing and monitoring gods on outgroup allocations revealed considerable
16 between-site variability, suggesting that in the absence of intergroup hostility, some religious elements
17 may be implicated in cooperative behavior toward outgroup members as well. These results provide
18 support for the hypothesis that beliefs in monitoring and punitive gods who care about human
19 normative conduct help expand the circle of sustainable social interaction, and open new questions
20 about how different traditions respond to religious outgroups.

21 **Key words:** Cultural Evolution; Impartiality; Punishing Gods; Parochialism; Religion; Supernatural
22 Punishment

23

24 **Introduction**

25 Over the last 12 millennia, the scale of human societies has dramatically expanded from hunter-gatherer
26 networks involving a few hundred individuals to vast nation-states involving millions. Theories explaining
27 the scaling-up of societies have combined insights about evolved cognition with cultural evolutionary
28 models of social norms [1–3], delineating important roles for markets, kinship systems, marriage
29 institutions, and religions [4–7]. Focusing on the role of supernatural beliefs, cultural evolution may have
30 favored the extension of intuitions about social punishment to beliefs in increasingly interventionist
31 gods who, because of their capacity to monitor and punish people for violating norms related to
32 interpersonal conduct, fostered less favoritism toward individuals themselves, their families, and their
33 communities vis-à-vis strangers sharing religious identities, effectively expanding the cooperative circle
34 [8].

35 Historically, competition among communities to control fertile lands dramatically intensified at
36 the onset of the Holocene, favoring larger, sedentary populations capable of communal defense and
37 various forms of collective action [9,10]. Although there are ongoing debates about the precise timing of
38 these historical changes and the nature of the causal relationships, a combination of archaeological and
39 ethnographic data [11,12] suggests that the scaling-up of societies was associated with the gradual

1 evolution of religious beliefs and practices into cultural packages that included more powerful gods who
2 were increasingly motivated and capable of monitoring norms favorable to the emergence of large
3 scale societies. In particular, analyses of ethnographic data revealed the centrality of divine punishment
4 in the evolution of political complexity in the Pacific [13] and robust global relationships between beliefs
5 in punishing gods and various indicators of societal scale and complexity [14–16].

6 Consistent with these observations, global surveys have linked a stronger belief in heaven, hell,
7 and punishing gods with stronger moral disapproval for cheating on taxes, buying stolen goods, and
8 other such public goods [17], and a cross-cultural study of market integration revealed robust
9 correlations between adherence to world religions involving moralizing deities and prosociality in
10 economic games [6]. Complementing these macro-level patterns, a substantial body of literature using
11 priming techniques has shown that among believers, religious reminders can effectively reduce self-
12 favoritism and increase resource-sharing in economic games involving strangers [18–21]. Importantly, to
13 experimentally test the role of belief in punishing and monitoring gods in the expansion of cooperative
14 circle, we previously employed the Random Allocation Game (RAG) among participants from eight
15 diverse field sites, and found that beliefs in monitoring and punishing gods were associated with less
16 favoritism toward the self and local communities when playing with geographically distant co-
17 religionists [8,22].

18 However, while these initial findings support the hypothesis of intra-religious impartiality (i.e.,
19 extending impartiality to geographically distant co-religionists), the question as to whether these beliefs
20 may also favor more equitable treatment of religious outgroups remains open to study. One cultural
21 group selection account [23] suggested that parochial cooperation should be generally favored
22 (compared to indiscriminate prosociality and uncooperative selfishness), especially during fierce conflict
23 over resources/values [24,25] because it gives a competitive edge to groups with tight cooperative
24 norms [26]. While supernaturally sanctioned norms may stabilize cooperation within a particular
25 religious group (intra-religious impartiality), other groups may have different normative regulations
26 and/or not extend their norms to outsiders [27]. Hence, cooperation between groups with different
27 supernatural commitments is risky and prone to free-riding, predicting parochialism as a baseline
28 relationship between religious groups. However, the cultural evolutionary account put forth by
29 Norenzayan et al. [23] also predicts that in situations where a more inclusive strategy attracts new
30 recruits and enhances cooperative networks, group norms may shift toward more universal application
31 and indiscriminate prosociality to incorporate the members of religious outgroups if they are not in a
32 direct conflict over resources/values [28,29]. To date, evidence appears to be mixed: some studies
33 showed that participants affiliated with religions emphasizing universal morality embrace the extension
34 of cooperation behavior to outgroup members [20,30–32] while other studies indicated that religious
35 participants reveal hostility toward religious outgroups [33,34].

36 In this paper, we present data from 2,228 participants sampled in 15 socio-ecologically and
37 religiously diverse societies (see Tab. 1). We aimed to replicate our previous findings that belief in
38 punishing and monitoring gods helps curbing local favoritism [8,22] by deploying the same protocol in
39 additional societies and, moreover, by deploying a different economic game, namely the Dictator Game
40 (DG). Furthermore, we aimed to extend the previous research by examining the outstanding questions
41 about the role of beliefs in punishing gods in the treatment of religious outgroups. At each site, we used
42 preliminary ethnographic interviews to select one god interested in norms of interpersonal conduct, and

1 assessed those gods' abilities to monitor norm following and punish transgressions. In line with previous
 2 research, we labeled such gods as "moralizing" [22]; however, note that these gods need not be creator
 3 or supreme gods and do not need to care about "morality" as understood in Western philosophy.
 4 Rather, they care about group-specific norms regulating interpersonal conduct [35,36]. As a comparison,
 5 we also selected locally salient gods on the basis of their being relatively less concerned with
 6 interpersonal conduct, less punitive, and less knowledgeable than their "moralizing" counterparts
 7 (except for Hadza, Lovu, and Samburu sites where such gods could not be identified; see SM, section
 8 S2.2 for details).

9 We adapted two experimental games, the Random Allocation Game (RAG) and the Dictator
 10 Game (DG), to measure two distinct facets of cooperative behavior: curbed favoritism and impartial
 11 resource sharing. In the RAG, participants anonymously selected one of two cups designated for
 12 different recipients. They rolled a fair, two-colored die. If it came up one color, participants were
 13 supposed to put the coin into the cup they selected. If the die came up the other color, participants
 14 were supposed to put the coin into the opposite cup. Participants repeated this procedure for 30 coin
 15 allocations and understood that all money will be distributed accordingly [37]. Since the allocations
 16 were made in private, only the participants knew their decisions and they could distribute the
 17 endowment according to their preferences (rather than die rolls), reflecting in- and/or out-group biases.
 18 In the DG, participants anonymously allocated 10 coins between two recipients (also designated by
 19 cups) based purely on their preferences [38]. These games have been widely used in cross-cultural
 20 research and benchmarked against relevant real world behaviors [6,37,39–42]. In different ways, these
 21 experiments permit us to assess the relationship between people's religious beliefs and their biases in
 22 favor of themselves, their families, and their communities.

23 Participants played four rounds of either the RAG or the DG, distributing endowed money
 24 between two cups in each round. To investigate the intra-religious impartiality, two rounds consisted of
 25 contributions to distant co-religionists versus the self (SELF vs. DISTANT) or local co-religionists (LOCAL vs.
 26 DISTANT). These two rounds aimed to replicate prior findings that belief in punishing and monitoring gods

Tab. 1. Site Descriptive Statistics

Group	Country	N	DISTANT	OUTGROUP	Prime	Moralizing God	Local God
Cachoeira	Brazil	274	Candomble	Evangelical	MLSC	Christian God	Candomblé God (<i>Ogum</i>)
Coastal Tanna	Vanuatu	178	Christian	Kastom	MLC	Christian God	Garden spirit (<i>Tupunus</i>)
Hadza	Tanzania	201	<i>Hadza</i>	<i>Datoga</i>	-	<i>Haine</i> (Traditional)	<i>Ishoko</i> *
Huatasani	Peru	94	Catholic	Evangelical	MC	Christian God	Mountain Spirits/Christ. Saints
Inland Tanna	Vanuatu	112	Kastom	Christian	MC	<i>Kalpapan</i> (Traditional)	Garden spirit (<i>Tupunus</i>)
Kananga	DRC	200	<i>Non-Luluwa</i> Christ.	<i>Non-Luluwa</i>	MLSC	Christian God	<i>Kadim</i> /Ancestor spirits
Lovu	Fiji	76	Hindu	---	MC	Hindu Bhagwan	None available
Marajó	Brazil	77	Christian	---	MC	Christian God	Virgin Mary
Indo-Mauritians	Mauritius	245	Hindu	Muslim	MSC	Hindu Shiva	Ghost (<i>Nam</i>)
Mysore	India	165	Hindu	Christian	MC	Hindu Shiva	Chamundeshwari
Samburu	Kenya	40	Christ. <i>Samburu</i>	<i>Samburu</i>	-	Christian/Traditional (<i>Nkai</i>)	None available
Sursurunga	Papua New Guinea	163	Christ. <i>Sursurunga</i>	<i>Foreigner</i>	MLC	Christ. God (<i>Káláu</i>)	Spirit (<i>Sírmát</i>)
Turkana	Kenya	247	Christ. <i>Turkana</i>	<i>Turkana</i>	MLSC	Christ. God (<i>Akuji</i>)	Ancestor spirits
Tyvas	Tyva Republic	81	Buddhist	---	MC	Buddha Burgan	Spirit-masters (<i>Cher eezi</i>)
Yasawa	Fiji	75	Hindu	---	MC	Christian God	Ancestor spirits (<i>Kalou-vu</i>)

Notes: *There are no *Ishoko* data in Wave II (see SM, S2.2.3). DRC = Democratic Republic Congo. Primes: M = Moralizing gods prime; L = Local gods prime; S = Secular authority prime; C = Control condition. No Outgroups were selected for sites taking part only in Wave I (see Methods).

1 promotes expansion of rule following toward DISTANT co-religionists (RAG); and to assess whether
2 these beliefs are also associated with increased resource-sharing (DG). The other two experimental
3 rounds collected data on allocations to religious outgroups compared to allocations to the self (SELF vs.
4 OUTGROUP) or to distant co-religionists (DISTANT vs. OUTGROUP), aiming to assess whether belief in
5 punishing and monitoring gods promotes indiscriminate prosociality or whether the cooperative circle is
6 limited only to religious ingroups (religious parochialism). SELF represented allocations that participants
7 made to themselves. LOCAL co-religionist allocations were distributed to randomly selected, anonymous
8 members of the same religion (associated with the selected moralizing gods) in the camp/village/town
9 where we conducted experiments. DISTANT co-religionist allocations were distributed to randomly
10 selected, anonymous individuals in a geographically distant village who practice the same religion as
11 LOCALS. OUTGROUP allocations were distributed to anonymous members of a different religion in a
12 geographically remote village (OUTGROUPS were selected such that they were not in a direct conflict
13 with LOCALS).

14 Finally, to investigate the causal relationship between beliefs in moralizing gods and treatment
15 of other groups, we utilized priming techniques by using locally salient reminders of a) punishing and
16 monitoring moralizing gods; b) relatively less moralistically punitive and knowledgeable local gods; c)
17 secular authorities (e.g., police); and d) a control condition. Although previous research has suggested
18 causal influence of punishing gods on rule-following and resource sharing [21], the use of priming
19 methods has been almost exclusively limited to Western populations and it is not clear whether the
20 reported effects extend also to small-scale, non-industrial societies. By selecting our sites, we aimed to
21 capture a significant portion of the world's human diversity while investigating the individual-level
22 effects of belief in and priming with moralizing gods. We pre-registered two sets of predictions in the
23 Open Science Framework (<https://osf.io/epkbw/>) before data analysis:

24 1. *Expanding the Cooperative Circle to Distant Co-Religionists (DISTANT Games)*

25 Corresponding to the intra-religious impartiality hypothesis, we predicted that participants who
26 (A) reported higher ratings of moralizing gods as punitive and monitoring will show less
27 favoritism toward the SELF and LOCAL co-religionists when playing with DISTANT co-religionists
28 in the RAG and DG, with cups labeled as SELF vs. DISTANT and LOCAL vs. DISTANT. Likewise,
29 participants who (B) were primed with the concept of punitive and monitoring moralizing gods
30 will show less SELF and LOCAL co-religionist favoritism compared to the other conditions.

31 2. *Parochial Religious Norm Adherence (OUTGROUP Games)*

32 According to the religious parochialism hypothesis, allocations in the SELF vs. OUTGROUP RAG and
33 DG should not be influenced by (A) the ratings of, or (B) priming with, punitive and monitoring
34 moralizing gods. That is, participants should bias allocations toward the SELF, irrespective of
35 their religious belief or treatment conditions due to the limited scope of religious norms.

36 Moreover, we predicted that when allocating between DISTANT co-religionists and
37 OUTGROUPS, participants who (C) reported higher ratings of, or (D) were primed with, punitive
38 and monitoring moralizing gods will bias their allocations away from OUTGROUP members and
39 in favor of DISTANT co-religionists in both the RAG and DG (religious parochialism). However, we
40 also explored whether participants at Christian sites (universalistic, proselytizing religion) will
41 taper their bias against OUTGROUPS by virtue of the aforementioned propensity to attract new
42 members (indiscriminate prosociality).

1 Methods

2 *Participants*

3 During two waves of data collection, we recruited a total of 2,228 participants from 15 societies (1,126
4 females; M age = 37.0, SD = 14.8). Specifically, during Wave I, we recruited 591 participants who played
5 the DISTANT RAGs and reported the results of this data collection in several publications [8,22];
6 however, 208 of those participants were contacted again during Wave II to collect the OUTGROUP RAGs.
7 For Wave II, 1637 new participant were recruited, playing either the DISTANT and OUGROUP RAGs or
8 the DISTANT and OUTGROUP DGs (153 participants played both RAGs and DGs). Here, we collapsed both
9 Wave I and Wave II samples to provide robust tests of our hypotheses.

10 We excluded all participants from our analyses whose allocations did not sum to 30 for a
11 particular RAG or 10 for a particular DG. Specifically, we excluded 30 participants from at least one RAG
12 and 33 from at least one DG. Furthermore, we excluded 22 participants who misunderstood the
13 procedure or did not correctly follow procedural steps. At one site, two research assistants
14 counterfeited data, thus all the RAG and DG data collected by these assistants were removed (72
15 participants). The number of participants in each analysis is displayed under specific models. While
16 tables in the main text report only full models (these are missing three sites due to missing some of the
17 co-variates), reduced models including all sites can be found in the SM, section S3. Our protocols were
18 approved by the University of British Columbia's Behavioural Research Ethics Board (BREB) and by the
19 equivalent at each individual researcher's home university. All subjects provided an informed verbal
20 consent for participation before the experiment.

21 *Procedures*

22 Participants were recruited by random sampling from a street or chain sampling, while in smaller
23 communities, researchers randomly sampled households. Upon arrival at a study location, participants
24 were asked to wait before the experiment in a separate area to prevent collusion, and then entered
25 individually into a room/tent/or a secluded area to play the RAGs and DGs. Before playing the games,
26 each participant was given a show-up fee (approximating 1/4 of one day's wage), learned about game-
27 specific rules, and had to demonstrate an understanding of those rules. Afterwards, the participant was
28 left alone to play four rounds of either RAGs or DGs. Both DGs and RAGs were played in a random order.
29 After the gameplay was finished, each participant received the amount contributed to the SELF cups and
30 was escorted into a different area for demographic and religiosity interviews where we asked about
31 moralizing and local gods (see SM, section S2 for more details on procedure).

32 *Materials*

33 We first conducted ethnographic surveys asking roughly 20 participants to list and rank up to five gods
34 and spirits, from which each site selected one monitoring and punitive god/spirit (here labeled
35 'moralizing') and one god/spirit less concerned with the interpersonal aspects of human normative
36 behavior (here labeled 'local'). Confirming our choices of moralizing vs. local gods, the survey showed
37 that the selected moralizing gods were rated on average as more punitive and monitoring than local
38 gods, more concerned with interpersonal norms, and more rewarding. The magnitude of these
39 differences varied by site (SM, Tab. S3). To examine the relationship between belief in punitive and
40 monitoring gods who care about norm transgressions (i.e., moralizing gods) and the RAG and DG

1 allocations, we created a punishment-monitoring score by averaging four binary questions pertaining to
2 the gods' ability and willingness to punish and monitor people. Note that apart from this composite
3 measure, we also planned to examine the monitoring and punishing factors separately as in our
4 previous study [22]; however, there was not enough variation in the monitoring factor to afford such
5 analyses (for discussion see SM 2.1.2). All materials were translated from English into the local
6 languages and back-translated to English to assure translation accuracy.

7 To examine the causal relationship between moralizing gods and impartiality, we adapted
8 priming materials to the specificities of local contexts (see Tab. 1 for an overview of primed concepts).
9 Four of our sites (Cachoeira, Mauritius, Sursurunga, Yasawa) used contextual priming in the RAGs
10 (games were played inside a temple and a control location) while other sites used religious/secular
11 imagery printed on a mat/table cloth (Coastal and Inland Tanna, Kananga, Huatasani, and Mauritius DG);
12 religious/secular material objects (Cachoeira DG, Lovu, Marajó, Mysore, Turkana, Tyva); or verbal
13 priming (Sursurunga DG). See SM, sections S2 and S3 for details. Note that in our correlational analyses,
14 we hold the priming effects constant to account for the fact that not all sites used priming.

15 *Analyses*

16 In our regression models, we used the punishment-monitoring score of moralizing gods as well as our
17 treatment conditions as main predictors of the RAG and DG allocations, controlling for a host of
18 potentially confounding variables. In five modeling steps, we hold site-membership constant as simple
19 fixed effects (i.e., mean site allocations), allowing us to make inferences about the general effect present
20 across our sites while accounting for unmeasured between-site variance. Furthermore, we control for
21 potentially competing explanations by holding constant the ratings of moralizing gods' rewarding
22 abilities, local gods' punishment-monitoring score, and relationship to local secular authorities (e.g.
23 police). Apart from these controls, we also hold constant demographic variables, emotional closeness to
24 LOCAL, DISTANT and OUTGROUPS and game-related variables such as game-order (see SM, section S3
25 for details).

26 **Results**

27 We observed high between-site variability in mean allocations in the RAG and DG (see Fig. 1). On
28 average, in both games, participants tended to allocate more coins to the SELF and to LOCAL co-
29 religionists compared to DISTANT co-religionists and OUTGROUPS. The allocations were generally more
30 equitable in the RAG compared to the DG (despite the greater anonymity in the RAG), indicating that the
31 rule-following aspect of the RAG played an important role in participants' decision-making.

32 *Moralizing Gods Promote Allocations to Distant Co-Religionists*

33 To replicate our previous findings from Wave I [8,22], we first analyzed the DISTANT RAGs on the sample
34 from both data collection waves. In the enlarged sample and in line with Wave I, we observed that
35 participants who rated their moralizing god as more punitive and monitoring tended to allocate more
36 money to DISTANT co-religionists. Figure 2 illustrates that going from zero to one in our punishment-
37 monitoring score was associated with an increase in the mean allocations to DISTANT co-religionists. To
38 examine this effect more closely, we regressed RAG allocations on the punishment-monitoring scores
39 using binomial regression models. In all specifications, the estimated punishment-monitoring

1 coefficients were positive, predicting larger DISTANT allocations: rating moralizing gods as punitive and
2 monitoring increased the chances of allocating a coin to the DISTANT cup by 26% [95% CI = 6% – 49%] in
3 the SELF vs. DISTANT RAG and by 22% [95% CI = 3% – 45%] in the LOCAL vs. DISTANT RAG. Crucially, none of
4 the key control variables, including ratings of moralizing gods as rewarding, local gods' punishment and
5 monitoring abilities, or relationship to secular authority, showed stable effects on DISTANT allocations
6 (see Tab. 2, Fig. 3, and SM, section S3.2 for specific modeling steps and Fig. S6 for comparison of Wave I
7 and Wave II).

8 To extend the RAG results to a different economic game measuring a distinct facet of intra-
9 religious impartiality, we included the DISTANT DGs during Wave II. We observed similar effects of
10 moralizing gods on DISTANT allocations as in the RAGs (see Fig. 2). Regressing the DISTANT allocations
11 on the punishment-monitoring measure in a series of five Tobit models revealed that the punishment-
12 monitoring score was associated with an increase in participants' allocations to DISTANT co-religionists.
13 The effects of moralizing gods' ratings predicted up to a 1.25 coin increase [95% CI = 0.25 – 2.24] in
14 allocations to DISTANT co-religionists when playing with the SELF, and up to a 0.89 coin increase [95% CI
15 = 0.07 – 1.70] when playing with LOCAL (maximum allocation was 10 coins). These results held for
16 various model specifications, and none of our key controls predicted DISTANT allocations (Tab. 2, Fig. 3,
17 and SM, section S3.2 for specific modeling steps)

18 To examine whether the effects of punishing gods on behavior can be experimentally
19 manipulated under field conditions, in some sites (see Tab. 1), we randomly assigned participants to be
20 primed with either moralizing gods, local gods, secular authority (the latter in the Dictator Game only),
21 or a control condition. Since we were interested in the strength of effects of the moralizing gods prime
22 compared to the other conditions, we set the moralizing gods prime as a reference category for
23 comparisons in our models (note that this choice does not affect other coefficients of interest; see SM,
24 S2.1.5).

25 We observed priming effects consistent with our hypotheses in the raw data (see SM, Fig. S7),
26 although these patterns weakened after accounting for site fixed effects. Using binomial regression, we
27 did not observe a difference between the moralizing gods and control conditions in the RAG; however,
28 participants in the local gods condition had on average odds of 0.85 [95% CI = 0.70 – 1.01] of allocating a
29 coin into the DISTANT cups compared to the moralizing gods condition (see Tab. 3). Similar patterns
30 were observed for the DISTANT DGs: there were no differences between the moralizing gods and
31 control conditions but the coefficients of the treatment with moralizing gods were higher compared to
32 the local gods and secular authority treatments. While some of these differences were imprecisely
33 estimated, all the coefficients were in predicted directions (ranging from a difference of 0.22 to 0.63
34 coins; see Tab. 3 for 95% CI). We speculate that the lack of difference between the moralizing gods and
35 control conditions occurred because participants in the control condition made allocations close to an
36 equal split; so reminding people of their moralizing god could not make them, on average, any more
37 impartial. However, reminding participants of the local gods could, and did to varying degrees, push
38 people towards favoring SELF and LOCAL over DISTANT co-religionists. Thus, these findings offer only
39 tentative support for prediction #1B and should be interpreted with caution (see SM, section S3.2 for
40 further discussion).

41 *Varying Effects of Moralizing Gods on Allocations to Outgroups*

1 The OUTGROUP games further expanded our Wave I design, pitting the SELF against an OUTGROUP
2 individual (SELF vs. OUTGROUP), or a DISTANT co-religionist against an OUTGROUP individual (DISTANT vs.
3 OUTGROUP). Exploring the raw data displayed in Fig. 2 suggested that the effects of punishing and
4 monitoring gods on OUTGROUP allocations are highly variable and may be site-dependent. To test the
5 OUTGROUP hypotheses (indiscriminate prosociality vs. religious parochialism), we used the same set of
6 regression models as in the DISTANT games.

7 First, consistent with hypothesis #2A, we observed that the punishment-monitoring score
8 showed no reliable effect on players' contributions to OUTGROUPs in the SELF vs. OUTGROUP RAG (see
9 Tab. 2). In the DG, moralizing gods' punishment and monitoring was associated with increased
10 contributions to OUTGROUPs at the players' expense up to an increase of 1.23 coins [95% CI = 0.08 –
11 2.39]. However, this estimate was fairly imprecise and confidence intervals tightened to exclude zero
12 only when holding the emotional closeness to and similarity with the OUTGROUP constant, suggesting
13 that allocations to OUTGROUP members depend on pre-existing relationships (see Tab. 2, Fig. 3, and
14 SM, Tab. S19).

15 Testing whether the punishment-monitoring score is associated with favoritism toward DISTANT
16 co-religionists at the expense of OUTGROUPs (hypothesis #2C), the results from both the DISTANT vs.
17 OUTGROUP RAG and DG revealed variable effects of the punishment-monitoring score. The general
18 coefficients were in predicted directions, indicating that the punishment-monitoring score was
19 associated with the odds of 0.9 [95% CI = 0.66 – 1.24] of allocation to the OUTGROUP cup in the RAG
20 and with 0.33 [95% CI = -1.20 – 0.54] lower coin allocations in the DG. However, the confidence intervals
21 suggested high-between site variation (see Tab. 2 and Fig. 3). Breaking down the variation by the type of
22 moralizing gods' religion at each site (Christian vs. Other) suggested that the absence of the main effect
23 may be explained by the fact that at Christian sites, gods' punishment-monitoring score was on average
24 associated with higher allocations to OUTGROUPs, while the reverse applied to the other sites (see SM,
25 section S3.3 for discussion).

26 Regarding experimental manipulation in the OUTGROUP games, we employed the same priming
27 techniques as in the DISTANT games (see SM, Fig. S10). For the SELF vs. OUTGROUP RAG, we compared a
28 control condition with the moralizing gods treatment, observing the odds ratio of 0.91 [95% CI = 0.84 –
29 0.99] for allocations to OUTGROUPs in the control condition compared to the moralizing gods treatment
30 (see Tab. 3). In the SELF vs. OUTGROUP DG, we added treatments with local gods and local secular
31 authority, observing that the moralizing gods treatment predicted an increase of 0.36 coins [95% CI = -
32 0.06 – 0.79] compared to the control condition, 0.68 coins [95% CI = 0.21 – 1.14] compared to the local
33 gods treatment, and 0.75 coins [95% CI = 0.20 – 1.31] compared to the secular authority treatment (see
34 Tab. 3). For the DISTANT vs. OUTGROUP games, there were no differences between the moralizing gods and
35 control conditions in the RAG, and only weak negative differences between the moralizing gods and
36 other treatments in the DG. In sum, the priming results in the OUTGROUP games do not provide support
37 for our predictions (#2B and #2D), and suggest that priming may instead promote indiscriminate
38 prosociality in the SELF vs. OUTGROUP games (see SM, section S3.3 for discussion).

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Tab 2. Coefficients and 95% Confidence Intervals from Binomial and Tobit Regressions Estimating Allocations to the DISTANT/OUTGROUP Cups. Abbreviated Results From Full Models.

	DISTANT Games				OUTGROUP Games			
	Random Allocation Game		Dictator Game		Random Allocation Game		Dictator Game	
	SELF vs. DISTANT	LOCAL vs. DISTANT	SELF vs. DISTANT	LOCAL vs. DISTANT	SELF vs. OUTGROUP	DISTANT vs. OUTGROUP	SELF vs. OUTGROUP	DISTANT vs. OUTGROUP
	<i>OR</i>	<i>OR</i>	β -Est.	β -Est.	<i>OR</i>	<i>OR</i>	β -Est.	β -Est.
MG Pun-Mon	1.26** (1.06, 1.49)	1.22* (1.03, 1.45)	1.25* (0.25, 2.24)	0.89* (0.07, 1.70)	1.08 (0.79, 1.49)	0.90 (0.66, 1.24)	1.23* (0.08, 2.39)	-0.33 (-1.20, 0.54)
LG Pun-Mon	1.07 (0.94, 1.21)	0.99 (0.87, 1.12)	0.57 (-0.13, 1.28)	-0.04 (-0.62, 0.54)	0.76* (0.61, 0.94)	1.18 (0.94, 1.47)	-0.05 (-0.88, 0.79)	-0.37 (-1.00, 0.25)
MG Reward	0.98 (0.86, 1.11)	0.96 (0.84, 1.09)	-0.49 (-1.13, 0.15)	-0.27 (-0.80, 0.25)	0.88 (0.73, 1.06)	1.14 (0.94, 1.38)	-0.34 (-1.05, 0.37)	0.03 (-0.50, 0.57)
Police	1.03† (1.00, 1.07)	1.00 (0.97, 1.04)	0.01 (-0.16, 0.17)	-0.08 (-0.22, 0.05)	1.01 (0.96, 1.06)	1.02 (0.97, 1.07)	0.02 (-0.17, 0.21)	-0.06 (-0.20, 0.08)
Constant	0.81* (0.66, 0.98)	0.94 (0.77, 1.15)	4.43*** (3.24, 5.62)	3.54*** (2.50, 4.58)	1.30 (0.94, 1.79)	0.94 (0.68, 1.29)	2.65*** (1.29, 4.02)	4.75*** (3.71, 5.79)
N People	602	598	802	792	261	255	636	630
N Sites	12	12	9	9	7	7	8	8

Notes: *OR* = Odds ratio (exponentiated coefficients) from binomial regression; β -Est. = Beta Estimate from Tobit regression; MG = Moralizing Gods; LG = Local Gods; Pun-Mon = Punishment-monitoring. The full models hold constant site-specific mean allocations, treatment, age, sex, number of children, household size, material insecurity, emotional closeness and practice similarity to local and distant co-religionists and to outgroup, and police evaluation. Site means are modeled as simple fixed effects with Mysore as the reference category (see SM, section S3.1). Note that these models exclude three sites due to the lack of local god beliefs (Hadza, Lovu, and Samburu). Reduced models including all sites are displayed in the SM (see Models 2 for Tab. S7-10; Tab. S17-20).

† $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$

Tab 3. Coefficients and 95% Confidence Intervals from Binomial and Tobit Regressions Estimating Allocations to the DISTANT/OUTGROUP Cups according to Treatment Groups.

	DISTANT Games				OUTGROUP Games			
	Random Allocation Game		Dictator Game		Random Allocation Game		Dictator Game	
	SELF vs. DISTANT	LOCAL vs. DISTANT	SELF vs. DISTANT	LOCAL vs. DISTANT	SELF vs. OUTGROUP	DISTANT vs. OUTGROUP	SELF vs. OUTGROUP	DISTANT vs. OUTGROUP
	<i>OR</i>	<i>OR</i>	β -Est.	β -Est.	<i>OR</i>	<i>OR</i>	β -Est.	β -Est.
Control Prime	1.01 (0.96, 1.07)	0.97 (0.92, 1.03)	-0.24 (-0.66, 0.17)	-0.02 (-0.36, 0.32)	0.91* (0.84, 0.99)	0.99 (0.91, 1.08)	-0.36† (-0.79, 0.06)	-0.04 (-0.38, 0.31)
Local God Prime	0.89 (0.74, 1.06)	0.85† (0.70, 1.01)	-0.63** (-1.08, -0.17)	-0.22 (-0.60, 0.15)	--	--	-0.68** (-1.14, -0.21)	-0.36† (-0.74, 0.01)
Secular Authority Prime	--	--	-0.45 (-0.99, 0.10)	-0.29 (-0.73, 0.16)	--	--	-0.75** (-1.31, -0.20)	-0.22 (-0.68, 0.23)
Constant	0.98 (0.87, 1.10)	1.05 (0.93, 1.17)	4.82*** (4.08, 5.57)	3.50*** (2.90, 4.11)	1.1 (0.96, 1.26)	0.97 (0.84, 1.12)	3.77*** (3.02, 4.53)	4.99*** (4.37, 5.60)
N People	689	690	997	987	312	309	991	988
N Sites	9	9	9	9	4	4	9	9

Notes: *OR* = Odds ratio (exponentiated coefficients) from binomial regression; β -Est. = Beta Estimate from Tobit regression. Moralizing gods prime is the reference category, hence the coefficients report differences in the DISTANT and OUTGROUP allocations between the moralizing gods treatment compared to the other treatments. The models hold constant site-specific means with Mysore as the reference category; age; sex; number of children; size of household; and material insecurity.

† $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$

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2

1 Discussion

2 Through four iterations of both the Random Allocation Game (RAG) and the Dictator Game (DG), as well
3 as a priming battery, we examined the effects of beliefs related to supernatural monitoring and
4 punishment on impartial treatment of various receivers. Controlling for a host of potentially
5 confounding factors, our results show that higher ratings of moralizing gods as punitive and monitoring
6 were associated with larger allocations to DISTANT co-religionists when playing with both the SELF and
7 LOCAL co-religionists in the RAGs and DGs, supporting the intra-religious impartiality hypothesis. The
8 experimental priming with moralizing gods typically produced larger contributions to DISTANT co-
9 religionists compared to the treatments with local gods and secular authority, but not compared to the
10 control condition. The lack of difference between the moralizing gods and control conditions resulted
11 from the fact that the majority of participants committed to moralizing gods played close to 50/50 split
12 in the control condition, hence the primes had low variation to act upon (in both the DG and RAG,
13 giving half of the endowment is generally at ceiling on allocations to strangers [6,37]).

14 These findings support the idea that the cultural evolution of supernatural agents into punishing
15 and monitoring gods who care about interpersonal, normative conduct may have played a role in the
16 extension of the cooperative circle beyond kin-networks and local ingroup interests. In small-scale
17 societies, supernatural agents are often confined to local ecologies with limited spatio-temporal
18 relevance for people where they may promote parochial cooperation by demanding collective activities
19 [43,44]. However, since some local gods are tied to a specific place, they do not necessarily provide an
20 overarching identity for geographically distant communities and are not as interested in normative
21 conduct of anonymous members of different communities (29; see also SM, Tab. S3). In line with this
22 proposition, our correlational results showed that belief in moralizing—but not local— gods reduced
23 local favoritism. Moreover, in the analyses of the priming conditions, the average allocations to DISTANT
24 co-religionists across all the games were smaller in the local gods treatment compared to the moralizing
25 gods treatment. While these results are consistent with what the theory predicts, we note that co-
26 religionist recipients in these games were associated with the “moralizing” deity traditions and future
27 cross-cultural work should investigate impartiality among “local” deity traditions.

28 The negligible effects of another key control variable— the reward ratings of moralizing gods—
29 suggest that it is specifically the belief in the punishing and monitoring (not rewarding) aspects of
30 supernatural agents that promote higher DISTANT allocations [45]. Similarly, a different body of
31 literature has suggested that in some cultural and historical circumstances, secular institutions may
32 outcompete the policing functions provided by belief in punishing and monitoring gods [46,47]. Again,
33 our results in the DISTANT RAGs and DGs hold even when controlling for relationship to local secular
34 authority in this particular sample. Consistent with this finding, the secular authority prime led to lower
35 allocations to DISTANT co-religionists compared to the moralizing gods prime in the DGs, although
36 confidence intervals for these estimates were quite wide, suggesting considerable variability for which
37 we have not accounted.

38 The cultural evolutionary approach developed above further suggests that the increased
39 impartiality shown toward DISTANT coreligionists in the RAG and DG should not necessarily extend to
40 religious outgroups (religious parochialism; prediction #2A). Indeed, while self-favoritism was
41 constrained by commitments to moralizing gods in the SELF vs. DISTANT RAG (above), the punishment-

1 monitoring score did not play a role in curbing self-favoritism when playing the RAG with OUTGROUP.
2 This result points to the parochial effects of moralizing gods, who may be concerned with normative
3 conduct toward co-religionists but not necessarily toward outgroups. The result of SELF vs. OUTGROUP DG
4 was more complicated. In this DG, we observed a positive correlation between punishment-monitoring
5 ratings and allocations to OUTGROUPS; however, this coefficient was not robust across different model
6 specifications and only emerged as significant at conventional levels when both people's relationship
7 with, and similarity to, the OUTGROUP were held constant (c.f. [48]). This relationship suggests that
8 when groups are sufficiently similar and maintain prior favorable relationships, belief in punishing and
9 monitoring gods may deter extremely selfish treatment of outgroup members in the DG (i.e., zero
10 allocations; see SM, section S3.3 for discussion). In line with this interpretation, priming moralizing gods
11 increased OUTGROUP allocations in both the SELF vs. OUTGROUP RAG and DG compared to the control
12 condition (but never raised them above the 50/50 split).

13 Finally, we hypothesized that individuals would make a distinction between DISTANT co-
14 religionists and OUTGROUPS (#2C), promoting the competitiveness of one's own religious group by
15 increasing allocations to DISTANT co-religionists at the expense of OUTGROUPS. While the coefficients in
16 both the RAG and DG were in the predicted direction, the confidence intervals of those effects
17 suggested considerable between-site variability. We explored a hypothesis stating that in the absence of
18 intergroup hostility (as shown by our OUTGROUP emotional closeness measure, see SM, Tab. S4),
19 religions appealing to universal norms— which may ultimately foster proselytizing— would put
20 emphasis on indiscriminate prosociality manifested in higher OUTGROUP allocations. Our supplemental
21 analyses indeed suggest opposite trends in allocation to OUTGROUPS at Christian versus non-Christian
22 sites; however, caution should be exercised in interpreting these results as we would need larger
23 samples and other religions with universalistic appeal for precise estimates of this effect (see SM, section
24 S3.3 for further discussion).

25 While deploying our experiments across cultures, we also encountered limitations to our multi-
26 site experimental approach. Our main measure (punishment-monitoring abilities) exhibited low
27 variation at some sites where the maximum value possible was also the modal score. This may have
28 been an artifact of our pre-selection of gods that were specifically concerned with human interpersonal
29 normative conduct, but more nuanced measurements of participants' beliefs should improve future
30 estimates. Furthermore, while we attempted to define DISTANT co-religionists and OUTGROUPS solely
31 along religious lines, some sites— due to facts on the ground— had to merge ethnicity and religion
32 when defining recipient groups (see SM for more details and additional analyses). We attempted to
33 control for different OUTGROUP relationships by our measure of relationship to the OUTGROUP, but
34 future research should obtain detailed estimates of interaction frequency, cooperative exchange, and
35 conflict history. This applies to other types of culturally evolved groupings such as markets or political
36 institutions (for details see [49]). Finally, adapting priming techniques to fit the specific context at each
37 site yielded substantial variability in those techniques, possibly hindering the general estimates of the
38 priming effects. These effects might have also been confounded by the fact that our design necessitated
39 the use of religious reminders on cup labels, possibly subtly priming all participants (see SM 3.2.4 for
40 discussion).

41 Despite these limitations, the current work used a larger and more culturally diverse sample to
42 support our previous findings regarding the role of moralizing gods in expanding the social circle [8,22],

1 and replicated these findings in a new experiment (the Dictator Game) while also revealing tentative
2 support for our predictions using the priming technique in the field. Extending our experimental
3 paradigm to investigate religious parochialism, we observed little or no support for our outgroup
4 predictions; however, exploratory analyses suggested new lines of theoretical and empirical work. Taken
5 together, this study investigated a particular culturally evolved mechanism that may have contributed to
6 the expansion of human societies, and illustrates one interdisciplinary approach for moving beyond
7 narrow sampling strategies and harnessing the planet's rich human diversity to shed light on key
8 questions of cultural evolution.

9

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18 **Author Contributions**

19 J.H., A.N. and B.G.P. conceived the study, prepared protocols, and managed data collection. C.L.A., Q.D.A.,
20 A.B., E.C., E.K.K., C.H., C.L, S.M., R.A.M., C.M., C.P., B.G.P, M.S., T.V., J.L.W., A.K.W. and D.X. collected data.
21 M.L. and B.G.P. managed the data set and team communication, and M.L. conducted all analyses, and
22 made all graphs and tables. M.L., J.H., B.G.P. and A.N. drafted the paper. All authors provided input on
23 methods and the manuscript.

24 **Competing interests**

25 The authors declare no competing interests.

26 **Data Accessibility**

27 The data set used for the current analyses together with protocols, hypotheses, and R code can be found
28 at the Open Science Framework (<https://osf.io/epkbw/>).

29

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Figure Captions

Fig 1. Density Plots of DISTANT Co-religionist and OUTGROUP Allocations in the Random Allocation and Dictator Games. **A.** Participants allocated 30 coins between two cups in each RAG. Deviations from the predicted binomial distribution point to biased money allocation. **B.** The distribution of participants' allocations in the DGs revealed more between-site variability compared to the RAGs. The dashed vertical lines indicate an equitable split between cups (15 in the RAGs and 5 in the DGs). In. Tanna = Inland Tanna; Co. Tanna = Coastal Tanna. Note that these raw data do not control for the fact that some participants were primed during experiments.

Fig 2. Positive Effects of Moralizing Gods on DISTANT Allocations and Varying Effects on OUTGROUP Allocations. Displayed are raw means with 95% CI and density plots; dashed vertical lines indicate the moralizing gods effect. These effects are illustrative only because we collapsed the punishment-monitoring score into three (instead of five categories for easier reading: 0.0-0.3, 0.3-0.7, 0.7-1.0). These plots also do not take into account between site differences and distributional assumptions (see Tab. 2 for specific estimates). Note that the number of participants in each level of the MG Pun-Mon variable substantially differs (with MG Pun-Mon = 1 having the most and MG Pun-Mon = 0 the least participants); see Fig. S2 for histograms.

Fig 3. Moralizing Gods' Punishment-Monitoring Regression Coefficients with 95% CI Showing Different Effects on DISTANT and OUTGROUP Allocations. Rating moralizing gods as monitoring and punishing predicted larger allocations to the DISTANT co-religionist cups when playing with both SELF and LOCAL co-religionist cups in both the RAGs and DGs. The same rating did not predict allocations to the OUTGROUP cups in the SELF vs. OUTGROUP RAG; however, we observed a positive effect in the SELF vs. OUTGROUP DG. The coefficients for the DISTANT vs. OUTGROUP RAG and DG were in predicted directions but exhibited between-site variability. X-axis for the RAG is on the logistic scale.