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The evolution of religion and morality: a synthesis of ethnographic and experimental evidence from eight societies

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ABSTRACT

Understanding the expansion of human sociality and cooperation beyond kith and kin remains an important evolutionary puzzle. There is likely a complex web of processes including institutions, norms, and practices that contributes to this phenomenon. Considerable evidence suggests that one such process involves certain components of religious systems that may have fostered the expansion of human cooperation in a variety of ways, including both certain forms of rituals and commitment to particular types of gods. Using an experimental economic game, our team specifically tested whether or not individually held mental models of moralistic, punishing, and knowledgeable gods curb biases in favor of the self and the local community, and increase impartiality toward geographically distant anonymous co-religionists. Our sample includes 591 participants from eight diverse societies – iTaukei (indigenous) Fijians who practice both Christianity and ancestor worship, the animist Hadza of Tanzania, Hindu Indo-Fijians, Hindu Mauritians, shamanist-Buddhist Tyvans of southern Siberia, traditional Inland and Christian Coastal Vanuatians from Tanna, and Christian Brazilians from Pesqueiro. In this article, we present cross-cultural evidence that addresses this question and discuss the implications and limitations of our project. This volume also offers detailed, site-specific reports to provide further contextualization at the local level.

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1. Introduction

1.1. Introduction to the volume

This is a time of exciting developments in the scientific study of religion. Religion is a human universal found in every known culture, and permeates virtually all domains of social life. Nevertheless, contemporary scientific studies of religion often remain quite narrow in focus, design, and sampling,

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and research still adheres to traditional disciplinary divides. Additionally, when studies utilize cross-cultural data, either by design or necessity, many do not consider appropriately balancing attention toward human universals with the particulars of social contexts. Our effort – the Evolution of Religion and Morality Project – sought to bring evolutionarily minded anthropologists and psychologists together to weave economic, psychological, and ethnographic methods into a coherent whole in order to address the relationship between religion and sociality by drawing on a diverse sample of participants. We sought to integrate the rigor of systematic data collection, experiments, and statistical analyses from psychology and economics with the full ethnographic richness of anthropological research. By employing analyses at both global (i.e., across sites) and local (site-specific) levels, the present special issue attempts to situate cross-culturally salient experimental effects in local contexts while also enabling cross-cultural comparisons afforded by a global view. This synthetic article details the overall project and builds on the omnibus results reported in Purzycki et al. (2016c). The supporting seven articles provide detailed case studies for each field site and focus on the cultural particulars, experimental extensions, and contextual variables otherwise not captured by a global view.

1.2. Contribution of the Evolution of Religion and Morality Project

In recent years, there has been considerable growth in the evolutionary study of religion (Bulbulia, 2008; Frey, 2010; Volland & Schiefenhövel, 2009; Watts & Turner, 2014). This field has produced important hypotheses and insights that have generated active debate and discussion (e.g., Galen, 2012; Martin & Wiebe, 2014). Key empirical studies show that components of religious systems can effectively curb selfish behavior and harness our sociality in important ways. A variety of proximate mechanisms grounded in human psychology are currently under consideration in this burgeoning field, including but not limited to ritual (Atkinson & Whitehouse, 2011; Soler, 2012; Sosis, 2005; Sosis & Bressler, 2003; Sosis, Kress, & Boster, 2007; Sosis & Ruffle, 2003), behavioral and mental synchrony (Cohen, Mundry, & Kirschner, 2014), fear of supernatural retribution (Johnson, 2005; Johnson & Krüger, 2004; Schloss & Murray, 2011), and supernatural monitoring (Atkinson & Bourrat, 2011; Norenzayan et al., 2016; Piazza, Bering, & Ingram, 2011).

While these recent developments have advanced the field, the current research has a few notable limitations that we sought to address in this project. For one, despite its importance, cross-cultural work (Baumard, Hyafil, Morris, & Boyer, 2015; Botero et al., 2014; Johnson, 2005; Murdock & White, 2006; Stark, 2001; Watts et al., 2015; Watts, Sheehan, Atkinson, Bulbulia, & Gray, 2016) typically relies on extant databases or coded data drawn from qualitative ethnographies and other historical documents that lack: (a) specific concern for evolutionary hypotheses, and (b) the precise measures at the individual level required to test hypotheses regarding psychological mechanisms. Additionally, cross-national databases of contemporary populations typically lack non-state societies, and often rely on survey questions that do not target crucial aspects of variation relevant to evolutionary hypotheses (Norris & Inglehart, 2012; Solt, Habel, & Grant, 2011). Empirical psychological studies using living people as sources of data typically rely on Western, Christian-majority samples, often North American college-aged psychology students (Bering, McLeod, & Shackelford, 2005; Piazza et al., 2011; Shariff & Norenzayan, 2007; Shariff, Willard, Andersen, & Norenzayan, 2016). This, of course, severely limits generalizability (Henrich, Heine, & Norenzayan, 2010; Sears, 1986), and also creates inadvertent biases that extend Christian-centric, and sometimes even more narrowly, Protestant-centric notions to religion as a whole (Cohen, Hall, Koenig, & Meador, 2005; Henrich, Heine, et al., 2010; Norenzayan, 2016; Sears, 1986). Experimental studies that have sampled from diverse populations have faced other limitations. Some notable studies (Cohen et al., 2014; Sosis & Bressler, 2003; Sosis & Ruffle, 2003; Xygalatas, 2013) have focused on non-Western samples, but are limited insofar as their samples are mono-cultural, preventing cross-cultural comparisons and generalizations. In a previous effort that heralded the current project, a set of cross-cultural economic experiments investigated the link between religion and prosocial behavior, while targeting 15 societies ranging from foragers to horticulturalists, farmers, and wage

workers. But this study lacked precise measures for how people conceptualize their deities, and did not consider the religiosity of the recipients' or participants' economic decisions (Henrich, Ensminger, et al., 2010).

In this special issue, we address these limitations by introducing several new features of inquiry. We test key hypotheses about religion's contribution to the expansion of sociality in eight diverse societies from around the world by concentrating on the variation critical to current evolutionary hypotheses about religion's role in the evolution of cooperation, and focusing on sample, group, and individual-level sources of variation. In this introductory piece, we provide an overview of the project and synthesize analyses that address our central theoretical questions. We begin by reviewing the project's theoretical foundations and the hypotheses built upon them, and then detail the methods used systematically across all sites. A brief summary of our sample precedes the omnibus results, before we conclude by summarizing and interpreting our findings, and by highlighting the limitations of this work.

2. Theoretical framework

Evolutionary theory offers an abundance of approaches to explaining religion (Bulbulia, 2008; Shaver, Purzycki, & Sosis, 2016; Volland & Schiefenhövel, 2009; Watts & Turner, 2014). It is beyond the scope of the current article to offer an exhaustive overview, or to discuss ongoing, lively debates. Briefly, we note that some approaches argue that religion is largely a by-product of evolved cognitive adaptations (Atran, 2002; Barrett, 2004; Boyer, 2000; Kirkpatrick, 1999). Others emphasize the functional effects of religious systems (Alcorta & Sosis, 2005; Purzycki, Haque, & Sosis, 2014; Sosis, 2009) as adaptive responses to problems of trust and cooperation (Purzycki & Arakchaa, 2013; Soler, 2012; Sosis, 2005; Sosis & Bressler, 2003; Sosis & Ruffle, 2003; Tan & Vogel, 2008; Xygalatas et al., 2013) and/or coordination of resources (Bliege Bird, Taylor, Coddling, & Bird, 2013; Lansing, 1987, 2007; Lansing & Kremer, 1993; Rappaport, 2000). Another line of evolutionary thinking sees the spread of certain religious elements as a dynamic process coevolving with escalating social complexity over time (e.g., Atran & Henrich, 2010; Norenzayan et al., 2016; Watts et al., 2015).

The present work is grounded in cultural evolution and seeks first to partly explain human social complexity by attending to the effects of socially transmitted and accumulated religious beliefs and behaviors. Cultural evolution may have harnessed and exploited evolved psychological systems and through time assembled specific configurations of explicit supernatural agent concepts and ritual forms that induce greater cooperation, trust, and solidarity within expanding groups (Atran & Henrich, 2010; Bulbulia et al., 2013; Henrich, 2009; Norenzayan, 2013; Norenzayan et al., 2016). Given the tendency of cultural evolution to produce self-reinforcing stable patterns of beliefs and practices, these supernatural agent concepts may also have been favored within groups as by-products of mechanisms related to signaling, reputation, and punishment. Heightened sociality, in turn, may also promote greater success in intergroup competition, which may have contributed to the widespread proliferation of these representational and behavioral strains throughout the world, thus partly explaining the ubiquity of the world religions such as Christianity, Islam, Hinduism, and Buddhism.

In the following section, we situate the project in the literature examining the problem of social complexity and cooperation. Afterwards, we discuss the purported proximate mechanisms responsible for religion's contribution to this problem, and delineate our hypotheses.

2.1. Social complexity and the expansion of cooperation

Humans' tendency to engage in costly behaviors that benefit others who are neither kin nor likely ever to reciprocate (i.e., when there is little to no concern for maintaining a reputation) is widely considered to be an evolutionarily unique problem (Fehr & Gächter, 2002; Henrich & Henrich, 2007; Richerson & Boyd, 1999; Turchin, 2013). Standard models of kin selection (Hamilton, 1964; Williams & Williams, 1957) and

reciprocal altruism (Axelrod, 1984; Axelrod & Hamilton, 1981; Trivers, 1971) largely predict, respectively, that individual organisms will engage in costly behaviors that benefit others when those others are genetically related or when non-kin are likely to reciprocate in prolonged tit-for-tat interactions. Both are central factors in human social behavior (Axelrod, 1984; Cronk & Leech, 2013; Trivers, 1971). However, in large-scale societies, individuals regularly engage in costly behaviors that benefit others who are not likely to reciprocate directly, since the features that typically make adaptive reciprocity possible (e.g., repeated interactions and the ability to sanction misconduct) are absent; large-scale societies afford anonymity, one-shot interactions, and ease of defection. As such, standard models incorporating reputational mechanisms and indirect reciprocity or signaling (Gintis, Smith, & Bowles, 2001; Nowak & Sigmund, 2005) remain limited in their explanatory power in the case of large-scale social organizations without additional mechanisms of enforcement (Nowak, 2006; West, El Mouden, & Gardner, 2011). Various components of religious systems may provide such enforcement. We focus here on supernatural agent beliefs.¹

2.2. Variation in models of the minds of gods

Supernatural agent concepts can harness at least three corresponding psychological systems that promote cooperation: (1) moral cognition; (2) supernatural monitoring; and (3) supernatural punishment. We now turn to each of these proximate mechanisms.

2.2.1. Moralistic deities

While all societies might not have a lexical equivalent to the notion of “morality,” all societies have a sense of what is “good” and “bad,” “right” and “wrong” (Brown, 1991). Here, we refer to “morality” and “virtue” as a domain of explicit information consisting of interpersonal social phenomena that entail or imply a cost or benefit to others (Cronk, 1994; Gray, Young, & Waytz, 2012; Greene, 2013; Haidt, 2008; McKay & Whitehouse, 2014; Purzycki, 2013; Rossano, 2008; Smetana, 2006; Turiel, 1983). Explicitly associating morality and gods’ concerns should make interpersonal social behavior more predictable, consistent, and normative. The more people represent their gods as explicitly moralistic, the more likely they are to extend corresponding moral behaviors – those that provide a benefit to others when actors stand to gain – toward people beyond themselves and their local community (see Purzycki & McNamara, 2016).

It has long been observed that social complexity is associated with what researchers have traditionally called “moralistic High Gods” (Stark, 2001; Swanson, 1960; Wallace, 1966; Watts et al., 2015), and some analyses suggest that resource scarcity (Snarey, 1996), animal husbandry (Peoples & Marlowe, 2012), and other ecological factors such as harsh environments (Botero et al., 2014) also play an important role in the emergence of such gods. While these studies have substantially contributed to our understanding of the socioecological factors involved in the evolution of moralistic gods, they do not tease apart the theoretically important variables of what makes a god “High” (Schloss & Murray, 2011) or “moralistic” (McKay & Whitehouse, 2014; Purzycki, 2016).²

This is partly due both to the intellectual history of the concept and to the continued reliance upon cross-cultural databases for testing predictions. For instance, the Standard Cross-Cultural Sample (Murdock & White, 2006) defines a “High God” as “a spiritual being who is believed to have created all reality and/or to be its ultimate governor, even though his/her sole act was to create other spirits who, in turn, created or control the natural world.” High Gods that are specifically “moralistic” are determined by coding qualitative ethnographic data on a five-point categorical scale: such gods are “Present, active, and specifically supportive of human morality.” However, it is unclear what constitutes being “supportive of human morality,” how such ethnographic data were coded, how ethnographers, travelers, and the coders of their subsequent writings determined what constitutes “human morality” and its “support,” and what kinds of things gods might care about that do not constitute “human morality.” Nonetheless, there have been some developments.

For instance, coding data from 43 ethnographies, Boehm (2008) assessed the moral and punitive qualities of the gods and spirits of 18 foraging societies. Making the distinction between “immoral behaviors” (“predatory [behaviors] on fellow band members”) and “nonmoral taboos,” he found that foragers’ gods are less concerned with morality than they are with nonmoral taboos. Similarly, using free-list data collected in the Tyva Republic, Purzycki (2011, 2016) analyzed representational models of local gods’ concerns, finding that they were concerned primarily with resource conservation practices and ritual rather than “morality” as defined by both participants and *etic* constructs.³ Here, we distinguish gods concerned primarily with morality and virtue from those primarily concerned with ritual and ecology (Purzycki & McNamara, 2016; Purzycki & Sosis, 2011). In addition to moral concern, the notion that gods are watching also plays a critical role in altering cooperation.

2.2.2. Watchful deities

It is now widely thought in the cognitive science of religion (e.g., Barrett & Keil, 1996; Pyysiäinen, 2009) that conceptions and cognition of supernatural agents are built upon some form of anthropomorphism (Guthrie, 1980, 1995). This idea has deep intellectual roots (Darwin, 1871; Durkheim, 2001; Hume, 1889; Xenophanes & Leshner, 2001). In turn, anthropomorphism depends upon “theory of mind” systems – i.e., the ability to detect and ascribe beliefs, desires, and perceptions of other beings (Baron-Cohen, 1995; Baron-Cohen & Wheelwright, 2004; Dennett, 1987; Premack & Woodruff, 1978). Indeed, some evidence suggests that mentalizing constrains thinking about personified supernatural agents (Norenzayan, Gervais, & Trzesniewski, 2012; cf. Reddish, Tok, & Kundt, 2015). Moreover, as a variety of experimental treatments have shown, when people are exposed to indices of agency (e.g., two eyes on a computer screen or a picture of another person), they are more generous and less prone to engage in self-serving behavior (Bateson, Nettle, & Roberts, 2006; Fehr & Schneider, 2010; Haley & Fessler, 2005; Nettle et al., 2013). This suggests that under certain conditions, social regulatory systems engage upon the detection of other minds (Krátký, McGraw, Xygalatas, Mitkidis, & Reddish, 2016; Raihani & Bshary, 2012; Vogt, Efferson, Berger, & Fehr, 2015).

Because people often explain events and their circumstances in terms of agency, the gods may be a collectively shared response to the unknown, but appeals to gods’ minds – their knowledge and concerns – are also useful in manipulating others (Boyer, 2001; Purzycki & McNamara, 2016). As supernatural monitors, then, gods may contribute to cooperation insofar as they can induce the sense of being watched. The more knowledge people attribute to the gods, the more effectively one may invoke the gods as knowing others’ behavior. Various traditions appear to have exploited supernatural monitoring by putting, for example, watchful images of Buddha on the tops of nearby hills, the watchful eye of Horus, and the images or indices of local spirits on territorial borders (Jordan, 2003; Norenzayan, 2013; Purzycki, 2010), and/or explicitly attributing the gods with omniscience. Several experiments (Bering et al., 2005; Piazza et al., 2011) and correlational studies using extant databases (Atkinson & Bourrat, 2011) have shown the prosocial effects of supernatural monitoring. However, such studies do not attend to the explicitly held models of what gods know. There is considerable cultural and historical variability as to how much gods know and monitor; not all gods are explicitly conceptualized as omniscient, but some evidence suggests that there is a positive relationship between the breadth of knowledge attributed to gods and their concern with moral matters (Purzycki, 2013). Beyond affecting social behavior as a result of creating the sensation of being watched by morally concerned agents, the perception of gods’ punishment also appears to play a crucial role in human sociality.

2.2.3. Punitive deities

The experimental economics literature shows that cross-culturally, people are less inclined to be selfish when the threat of costly punishment looms (Fehr & Fischbacher, 2004; Henrich et al., 2006; Ohtsubo, Masuda, Watanabe, & Masuchi, 2010). Similarly, the Supernatural Punishment Hypothesis predicts that fear and/or perception of supernatural retribution for various selfish behaviors curbs those behaviors, thus minimizing the need for costly punishment by other people (Johnson, 2005,

2011, 2014; Johnson & Krüger, 2004; Schloss & Murray, 2011). Supernatural agents, however, have the added feature of having access to social information when other people are not around to regulate behavior (see above).

Schloss and Murray (2011) differentiate between two related but distinct approaches to the Supernatural Punishment Hypothesis. One focuses on “cooperation enhancement” and posits that punitive gods boost prosocial behavior (Norenzayan & Shariff, 2008). Another focuses on “punishment avoidance” – i.e., by reducing the costs incurred by getting punished for misconduct, punitive gods can enhance cooperation by reducing threats to sociality (Johnson, 2014; Johnson & Krüger, 2004). The present study is situated within the former approach. We add a particular twist to this literature, however, because immediate costly punishments are less likely from people beyond one’s community. We test the hypothesis (Norenzayan, 2013; Norenzayan et al., 2016) that such gods not only contribute to human prosociality, but also expand prosociality beyond the standard investments to and from kith and kin. As discussed above, it is precisely this type of investment that we see within highly complex societies (Richerson & Boyd, 1999; Turchin, 2013). Knowing and caring about human morality is likely not enough to curb partial behavior, however. Rather, the threat of punishment – either real or perceived – is likely a critical if not more critical element in altering human behavior and should therefore have a greater impact than merely feeling watched by a morally concerned agent (Johnson, 2015).

2.3. Hypotheses

Together, the more people represent their supernatural agents as moralistic, knowledgeable, and punitive, the more likely they should correspondingly show prosocial behavior toward others (Johnson, 2005; Norenzayan & Shariff, 2008; Schloss & Murray, 2011). More precisely, the prosocial effects of moralistic, punishing, and knowing gods may be large enough to widen the breadth of human sociality beyond one’s local community to include distant co-religionists with whom individuals would not necessarily regularly interact (Norenzayan, 2013; Norenzayan et al., 2016). We test the prediction that for religion to contribute to the development of escalating social complexity, the psychologically optimal forms of gods that may contribute to this expansion are those that are more moralistic, punitive, and knowledgeable relative to other god concepts. Moreover, recognizing that a supernatural agent will punish for misconduct should be more effective in promoting the expansion of impartial fairness than its monitoring capacity or concerns; the anticipation of an agent’s behavior should curb biased sociality beyond its perceived knowledge and concern. Together, our central hypotheses are as follows. Given that gods are thought of as moralistic:

H1. The more knowledgeable people claim their gods are, the less they will favor themselves and local co-religionists over distant co-religionists.

H2. The more punitive people claim their gods are, the less they will favor themselves and local co-religionists over distant co-religionists.

Considering the aforementioned importance that social and material security has on the expansion of prosociality (Hruschka & Henrich, 2013), we therefore add a third general prediction:

H3. The more socially secure people are, the less they will favor themselves and their local community.

Using a mix of experimental and ethnographic methods, we tested these predictions in eight diverse field sites from around the world.

3. Methods

Figure 1 illustrates the four core modules of our research protocols: (1) the *Religious Landscape and Market Integration* interviews; (2) the *Random Allocation Game* (section 3.1); (3) the *Demographic and Material Security* interview; and (4) the *Religiosity* interview.⁴ We administered Module 1 before

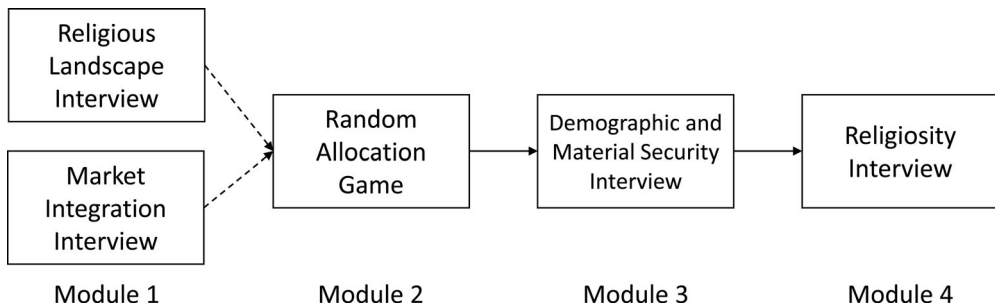


Figure 1. Roadmap of project modules.

Note: When possible, Module 1 was conducted with separate samples from Modules 2–4. However, in all cases, we carried out Module 1 in separate sessions from Modules 2–4.

executing the main study. We did this in order to elicit reliable data that would influence the design of Modules 2–4, which all experiment participants completed.

Importantly, in the *Religious Landscape* interview, we asked participants to list a maximum of five deities and asked them to rank them in terms of their importance in participants' lives, along with corollary questions about whether or not these gods are knowledgeable, punitive, and rewarding. We then selected those that approximated to the most knowledgeable, punitive, and rewarding as target moralistic gods, and selected locally salient gods with relatively less knowledge and punitive powers as our local spirits (see section 5.1).

Then, with a separate sample (when available), we conducted Modules 2–4. Participants completed the Random Allocation Game (section 3.1), then did the post-game interviews (section 3.2) in order to minimize the potential priming effect of religious questions. Protocols are available on our project's website (<http://www.hecc.ubc.ca/the-cultural-evolution-of-prosocial-religions/>). Data sets (Purzycki et al., 2016a, 2016b) are also publicly available. The following sections detail the Random Allocation Game (section 3.1) and post-game interviews (section 3.2).

3.1. The Random Allocation Game

3.1.1. Rules

Drawing from the economics literature devoted to understanding dishonest behavior (Fischbacher & Föllmi-Heusi, 2013; Gigerenzer & Hug, 1992; Jiang, 2013), the Random Allocation Game measures behavioral deviations from rules in an economic game (Cohn, Fehr, & Maréchal, 2014; Hruschka et al., 2014; McNamara, Norenzayan, & Henrich, 2016; Purzycki et al., 2016c). The game materials consist of two cups, a stack of coins, and a fair, six-sided die. The rules of the game as told to participants are as follows:

Step 1: Choose one of these two cups in your mind.

Step 2: Roll the die once.

Step 3: The die has six sides, each of which is colored by one of two colors. If the die lands with a black side facing up, you will put one of the coins in the cup you chose in your mind in Step 1. If the die lands with a white side facing up, you will put one of the coins into the *opposite* cup from what you chose.

The two cups represent actual people to whom the coins will be given and researchers ensure that all coins are distributed appropriately (no deception).

Participants play anonymously. As such, there is no record of players' decisions. They can therefore cheat or conveniently forget which cup they chose, and favor one cup or the other. For example,

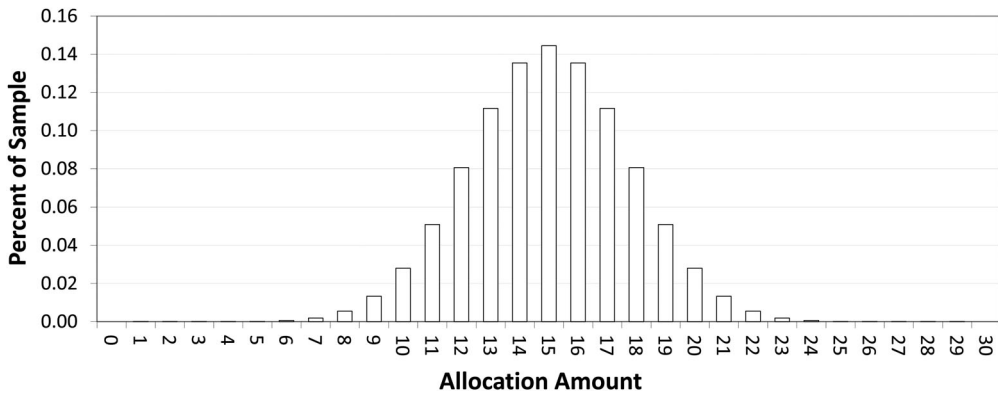


Figure 2. Theoretical binomial distribution of coin allocations to any single cup for a hypothetical sample playing by the rules.

if one cup is designated for participants and the other cup was for an anonymous stranger from another town, participants may ignore the rules and allocate more coins to themselves. However, if all participants follow the aforementioned rules, then all allocations should follow a binomial distribution (see [Figure 2](#)).

We used the Random Allocation Game to measure impartiality toward others, our measure of prosociality. In our experiments, each game consisted of 30 die rolls for 30 coins. So, if our sample were to play impartially, modal allocations (~14% of the sample) should be 15 for any given cup. However, if there are systematic, biased allocations to any given cup, the actual distribution will deviate from this theoretical baseline of impartial play.

3.1.2. The religious group Random Allocation Game

All participants played two counterbalanced core games: the *Self Game* and the *Local Co-Religionist Game*. In the *Self Game*, one cup was for participants and the other cup was for an anonymous co-religionist living in a specific, distant area within participants' home countries (the DISTANT cup). In the *Local Co-Religionist Game*, one cup was for an anonymous co-religionist in the players' community (the LOCAL cup), and the other cup was for *another* anonymous co-religionist in the same specific, distant area as that identified in the *Self Game*. Per site information of DISTANT cups is provided in in Supplementary Table S2. The cups were labeled with images and relevant text for each site ([Figure 3](#)). We ensured that all players played anonymously and that – other than participants – no one could interfere or touch coins or cups until the games were finished.

Cups were covered with slotted lids to minimize participant manipulation or the ability to see how many coins were placed into them. These cups were placed onto trays that allowed researchers and their assistants to easily turn to the next game without touching the cups. We used dice that had three

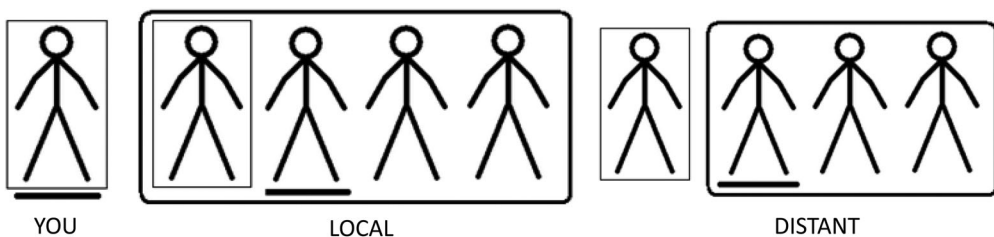


Figure 3. Generic cup labels for games.

Note: Illustrations adapted from Hruschka et al. (2014); McNamara et al. (2016).

sides of one color and three sides of another color. Prior to running the experiments, each die was rolled 50–100 times and assessed for fairness in the event the nail polish or other coloration techniques used to color the dice altered their weight distribution. All dice were fair.

We paid all participants a show-up fee (approximately 25% of a local average day's wage) and we set combined stakes for games at roughly one day's wage in the local community. Stakes per game were therefore roughly the average daily wage divided by the number of games played divided by 30 (for the 30 rolls per game). As the Hadza were our least market-integrated sub-sample, and cash has inconsistent significance and utility across Hadza groups, they played with tokens worth 8 oz. of maize per each dice roll. All other participants played with cash in local currencies.

3.1.3. Religious primes

To examine the social effects of religion, we used religious primes when feasible (in five sites). In previous work, researchers have used a variety of experimental religious primes to harness the predicted effects of religion. For example, Xygalatas (2013) found that in a sample of Mauritian Hindus, people were more prosocial toward others in a religious setting compared to a similar setting without religious significance. Religious or religious-like primes have been found to reduce cheating and increase generosity (Bering et al., 2005; Piazza et al., 2011; Shariff & Norenzayan, 2011; cf. Gomes & McCullough, 2015) and increase costly punishments toward others (McKay, Efferson, Whitehouse, & Fehr, 2011). A recent comprehensive meta-analysis of religious priming studies found that there was heterogeneity of effect sizes including null effects. Nevertheless, a reliable effect was found on a wide range of behaviors, including prosocial behaviors among believers in mostly (but not exclusively) Western samples (Shariff, Willard, Andersen, & Norenzayan, 2015). Importantly, the average effect was not different from zero for nonbelievers. Also, p-curve analyses of the published literature revealed results consistent with evidentiary value. As such, we predicted that primes associated with moralistic, punitive, and socially knowledgeable gods will induce prosocial behaviors toward co-religionists. In five of our sites – Pesqueiro, Yasawa, Tyva, Lovu, and Mauritius – roughly half of each sample played games with a prime associated with each site's moralistic god. In all cases these were objects, with the exception of Mauritians who played in a religious location (a temple). Tyvans (see Purzycki and Kulundary's article in this issue) played next to a Buddhist luck charm, Indo-Fijians (see Willard's article) played near an abstract statuette representing Shiva, Yasawans (see McNamara and Henrich's article) participated with games on top of cloths with printed text and image primes, and the Brazilian prime condition was a Bible and cross pendant (see Cohen, Baimel, and Purzycki's article). All primes were selected so that they included no immediate indicators of agency (e.g., eyes or faces). See the individual articles in this volume for details about each site's prime and site-specific analyses and discussion. Images of some of the primes are included in Purzycki et al. (2016a, 2016b).

3.2. Post-game interviews

3.2.1. Demographics and material security interview

We asked standard demographic questions including age, sex, years of formal education, and number of children. Additionally, we asked about participants' sense of material security and confidence. In previous iterations of the Random Allocation Game (Hruschka et al., 2014; McNamara et al., 2016), the more players reported food insecurity, the more coins went to their own cups. Consistent with this finding, the same pattern should hold for our sample. We asked:

- *Material Insecurity*: Do you worry that in the next _____ your household will have a time when it is not able to buy or produce enough food to eat?
- *Material Confidence*: How certain are you that you will be able to buy or produce enough food to eat in the next _____?

We asked these questions in four different timeframes: (1) one month, (2) six months, (3) one year, and (4) five years. Participants answered them on five-point Likert scales from -2 to 2 : very uncertain (-2), a little uncertain (-1), I don't know (0), a little certain (1), and very certain (2). We also asked for years of formal education, number of children, and evaluation of the police (see next section) as proxies for existential security insofar as more schooling, fewer children, and more positive evaluations of the police should positively associate with wellbeing and therefore impartiality toward DISTANT co-religionists.

In order to ensure that we had the proper controls for variation in intergroup relations, we asked participants about their perceived religious similarity between players' groups and the DISTANT co-religionist, and perceived effectiveness of secular justice institutions, and how emotionally close they felt to various groups. We asked the following questions in order to control for any site-specific variation contributing to allocation amounts:

- (a) *Police Evaluation*: Most members of the police are: very bad, bad, neither good nor bad, good, very good
- (b) *DISTANT Emotional Closeness*: Using these pictures, how emotionally close do you feel to a DISTANT person?
- (c) *LOCAL Emotional Closeness*: Using these pictures, how emotionally close do you feel toward a LOCAL person?
- (d) *OUTGROUP Emotional Closeness*: Using these pictures, how emotionally close do you feel toward an OUTGROUP person?
- (e) *DISTANT Religious Similarity*: How similar are DISTANT people's traditions/religious beliefs and practices with LOCAL people?

For items (b)–(d), we used a visual fusion scale (Swann, Gómez, Conor, Francisco, & Huici, 2009) where participants could point to the image that best represented their emotional proximity. Religious OUTGROUPs in (d) are individuals presumably devoted to other deities.⁵ The OUTGROUPs in this question for each site were: (1) Tyva Republic: ethnic Russians from Ak Dovurak; (2) Yasawa: Indo-Fijians; (3) Coastal and Inland Tanna: people from Noumea; (4) Lovu: a Muslim from Vanua Levu; (5) Hadza: the Datoga; (6) Pesqueiro: if Catholic, an Evangelical from São Paulo, if Evangelical, then a Catholic from Sao Paolo; (7) Mauritius: Muslims from Mauritius. Note that these groups vary considerably across sites. In preliminary discussions, our "OUTGROUPs" were difficult to select commensurably; a variety of factors ranging from regularity in contact, to ethnic variation, to unfamiliarity varied across sites.

3.2.2. Religiosity interview

To ensure that the target gods of our experiments were locally recognized as more moralistic, punitive, and knowledgeable, we measured gods' moralism in a variety of ways.⁶ After our preliminary interviews (Module 1; see above), we selected two salient gods in each site—one "moralistic god" and one salient "local god." We asked experiment participants questions about these two gods' knowledge, punishment, and moral concern. To account for variation in gods' punishment and knowledge, we asked the following questions (gods' names are inserted in the blanks):

- (1) Does _____ ever punish people for their behavior?
- (2) Can _____ influence what happens to people after they die?
- (3) Can _____ see into people's hearts or know their thoughts and feelings?
- (4) Can _____ see what people are doing if they are far away in _____ [a distant town or city familiar to locals]?

These dichotomous questions were suitable across all field sites as members of traditional societies often struggle with continuous scales. We did, however, also ask about the frequency of gods' rewards on a continuous scale (variable names in bold):

- (5) **Reward:** How often does _____ assist people in their lives or reward them for proper behavior?⁷

We used three methods to determine how moralistic gods were. First, we asked direct questions about the frequency with which gods punish for theft, deceit, and murder (see note 3). Second, we asked about how important it is for gods to punish for these behaviors (our **Moral Index**). And third, we conducted two free-list tasks asking participants to list the kinds of things that gods (a) care about or like and (b) dislike.

3.2.3. Game and site controls

We asked participants what they thought the game was about. If people mentioned it was about honesty, deception, or cheating, we subsequently dummy coded this to control for correctly assessing the games (this variable is included in the analyses below and showed no effects on outcome) (the variable **Honesty Mentioned**).⁸ We also controlled for **Treatment** (0 = control; 1 = prime condition) and the order in which people played the game (**Game Order**).

4. Participants, sampling, and procedures

4.1. Participants

We executed this study among eight different populations detailed in Table 1.⁹ Our sample represents a diverse array of societies engaged in various modes of subsistence that practice various religious traditions (see specific articles for more details about field sites). Supplementary Table S2 details our target variables by site and Table S1 is a correlation matrix of our key variables.

4.2. Sampling

We recruited participants using various sampling strategies including random sampling from censuses, from entire camps, chain sampling in towns, and door-to-door recruitment (see Supplementary Table S2). In order to control for collusion and contamination, in some cases participants who had already participated were not allowed to recruit others, though they were allowed to recruit others before participation. In such cases, those recruits had to arrive at the same time to participate. In some cases, the only way to ensure that participants could not collude in the game was to administer the experiments all at once and then proceed with follow-up interviews over the following week; as some communities were so small, allowing participants to converse about the games over an

Table 1. Site information and sample summary.

Site	Sample	Economy	Moralistic God/Local God	Language of Study	Prime Condition?	<i>N</i>
Coastal Tanna	Tannese	Horticulture	God/Garden Spirit	Bislama	N	44
Hadzaland	Hadza	Hunting	<i>Haine/Ishoko</i>	Hadzane/Swahili	N	68
Inland Tanna	Tannese	Horticulture	<i>Kalpapan</i> /Garden Spirit	Navhal	N	76
Lovu	Indo-Fijians	Market	<i>Bhagwan</i> /None	Fiji-Hindi/English	Y	76
Mauritius	Mauritians	Market/Farming	<i>Shiva/Nam</i>	Mauritian Creole	Y	94
Pesqueiro	Brazilians	Market	God/St. Mary	Portuguese	Y	77
Tyva Republic	Tyvans	Market/Herding	Buddha/Spirit-Masters	Tyvan	Y	81
Yasawa	Native Fijians	Fishing/Farming	God/Ancessor Spirits	Bauan	Y	75

Note: More summary statistics are found in Supplementary Table S2.

extended period of time would increase the chances of strategizing. In other settings, such as cities and towns, tracking down participants later for interviews was difficult or impossible, so we made sure to conduct all follow-up interviews immediately after gameplay. No unscheduled participants were allowed to play in case they were invited by someone who had already played. Note that in our main analyses below we used sites as fixed effects, which should remove any differences inadvertently introduced by these different approaches.

4.3. Procedure

Participants were brought into an experimental area either as a group or as individuals and introduced to the protocol. Researchers or assistants told participants about the show-up fees. We emphasized that show-up fees were strictly for participation, and not part of the games. While in some cases, participants were paid show-up fees up front, in other cases, show-up fees and game payouts were paid upon completion of the study to minimize collusion. Upon agreement to participate, we took candidates to a game area, and introduced them to the game. The entire procedure took a total of roughly 90 minutes for each individual.

We read the instructions to all participants. We then asked a series of five test questions to participants before beginning the experimental tasks in order to ensure that they understood the game and its rules. If they repeatedly did not pass, they were allowed their show-up fee, but were removed from the sample; participants in the present sample all successfully answered our test questions. After the test questions, researchers and assistants left the experimental area, allowing participants to play alone. Upon finishing, players notified the researcher and/or assistants. The researcher and/or assistant would return to the game area, and turn a tray to the next game without touching the cups. We then repeated the instructions and asked another round of test questions. Participants were once again left alone to play. After finishing, participants were either dismissed until follow-up interviews or taken to an interview area (see [Figure 1](#)). We encouraged participants not to discuss the game with anyone else. Additionally, monitors ensured that participants who played were not colluding with others who had already played (see above).

5. Results

In this section, we first briefly highlight participants' views on the minds of their gods (section 5.1). We then examine the effects of the experimental prime conditions across sites (section 5.2). Section 5.3 presents the overall results of the experiments and examines how, among the aforementioned variables, these indices help explain gameplay. Using a series of binomial logistic regressions, we assess the effects of our target variables – moralistic gods' knowledge and punishment – on the odds that coins go to distant co-religionists. We detail these results by game using sites as fixed effects, and then broaden our view to assess group-level patterns in allocation and existential security.

5.1. The minds of gods

For the purposes of comparison and the design of the experiments, recall that in Module 1 (section 3, [Figure 1](#)) we designed our protocols to select the more moralistic, monitoring, and punishing god as well as a god rated lower on these qualities. To check our manipulation, we used a variety of methods. First, to establish whether or not experimental participants viewed deities as more “moralistic,” we compared the Moral Index items using factor analysis (Supplementary Section S4.1). Second, we ran site-by-site analyses of the knowledge and punishment scales and found that, by and large, our subsamples viewed “local gods” as less punitive and knowledgeable than the “moralistic gods” (Supplementary Section S4.2). Third, we collected naturalistic free-list data regarding what local samples thought these two gods liked and disliked. These data were subsequently coded using 12 different

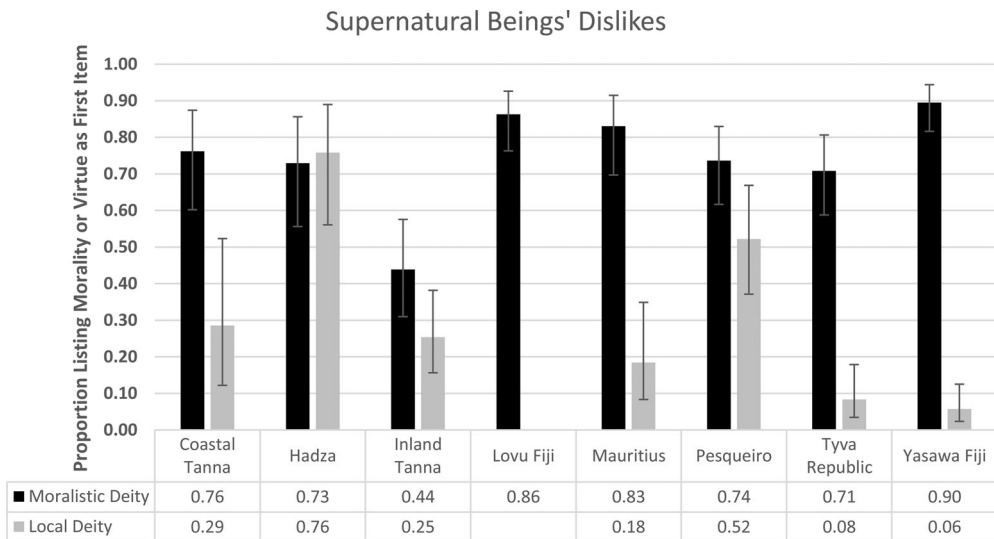


Figure 4. Proportion of per site samples listing breaches of morality and virtue first among the things that anger deities.

Notes: Error bars are continuity-corrected 95% confidence intervals of proportion. Data include individuals who did experimental extensions and/or other tasks. Not included are those who listed nothing or answered with “I don’t know.”

categories detailed by Purzycki and McNamara (2016). These ranged from morality and virtue to ritual and ecological practices (see Supplementary Section S4.3). Figure 4 illustrates the proportion of individuals by site who listed “immoral” items (i.e., breaches of morality and virtue) first among the things that anger the gods. Assuming that earlier items listed are more salient (Romney & D’Andrade, 1964), moralistic gods are indeed proportionately more concerned with morality and virtue than local gods. Note this is a different version of a similar graph in Purzycki et al. (2016c) which counted participants who responded “I don’t know” and contained some minor coding errors for the Lovu data. See Supplementary Section S4.3 for further analyses and discussion.

5.2. Overall allocations

As each individual played with 30 coins, the total number of observations in the *Local Co-Religionist Game* was 17,670, and 17,730 for the *Self Game* (see note 3) for a total of 35,400 observations. As illustrated in Figure 5, an overall view of raw coin amounts in distant co-religionist cups for both

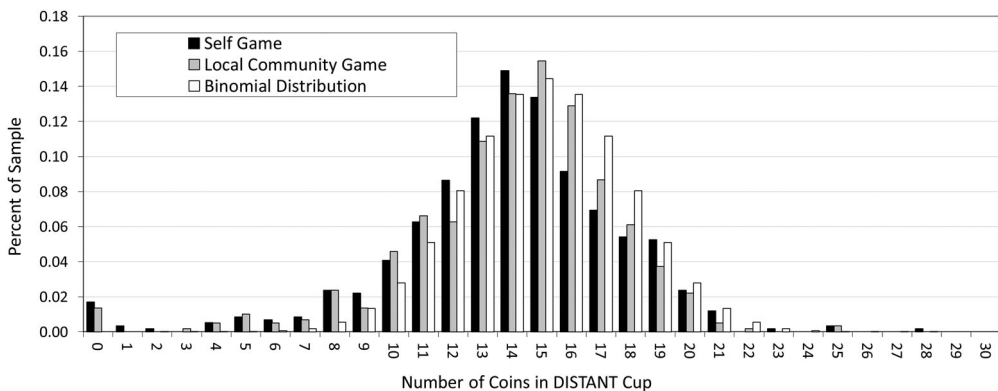


Figure 5. Allocation distribution for entire sample.

games indicates that there were systematic deviations in allocations to the left of the binomial distribution; players favor themselves and their local communities. While no individual games resulted in 30 coins in the DISTANT cups, eight players put all 30 coins in the LOCAL cup and 10 put all 30 coins in the SELF cup ($P(30) = 9.31 \times 10^{-10}$).

5.3. Regression models

To test for the effects of participants' views of their gods on game behavior, we regressed coin allocations to the distant co-religionists using binomial logistic regressions.¹⁰ In the focal models discussed below, we used sites as fixed effects as a conservative means of holding inter-site variation constant. Since Tyvans had the highest mean and lowest standard deviation for allocations to the DISTANT co-religionist in the *Self Game* ($M = 15.30$, $SD = 2.95$), we used them as the reference group for all analyses using sites as fixed effects.

Tables 2 and 3 present the focal models regressing coins in the DISTANT cups for both games and our target variables.¹¹ For the sake of illustration, we include models with all of our target variables (denoted "FULL"). In doing this, the Hadza and Lovu samples drop out as the Hadza did not answer scale items in the same manner, and the Lovu sample did not answer questions about local gods. The subsequent "No Local Gods" models remove local god variables in order to include the Lovu sample. Models denoted "All Groups Included" remove all variables that exclude the Hadza and Lovu. The "Reduced" models are backward-selected models that retain all variables with odds ratios that – at most – surpass 1 by .02 (+.02 for upper bound of negative effects and -.02 for lower bound of positive effects, see below) from the "All Groups Included" models. These also included condition and game order as controls. "Interaction" models are these models with an additional interaction term of the treatment and punishment index. "Group Only" models include only sites as fixed effects.¹²

Note that throughout this report, our annotation procedures for logistic regressions are not based on p-values. Rather, we emphasize confidence intervals whether or not the effects cross 1. Positive effects are indicated by odds ratios >1.00 and negative effects are indicated by odds ratios that are <1.00 . When the lower bound of a positive effect's confidence interval is >1.00 and when the upper bound of a negative effect is <1.00 , a variable gets the denotation of three asterisks (***) . Effects get a single asterisk (*) when the lower bound of CI = 1.00 for a positive effect and when the upper bound of a negative effect is 1.00.

5.3.1. Local Co-Religionist Game

For the *Local Co-Religionist Game*, we expected participants to allocate more coins to co-religionists in their communities than those in distant regions. In this game, 9388 coins went to the LOCAL ($M = 15.94$, $SD = 3.56$) whereas 8282 coins went to the DISTANT co-religionist ($M = 14.06$, $SD = 3.56$) for a nearly two-coin difference on average. The modal allocation amount to the DISTANT co-religionists was 15 ($n = 91$; $P(15) = .14$). At $\alpha = .95$, the critical value to reject that our sample played impartially is when allocations to the DISTANT cup are ≤ 8944 . Even if we relax alpha to .75, the critical value is 8880, still 601 coins above what was actually allocated. We can therefore conclude that, overall, our participants deviated from the rules and favored LOCAL co-religionists at the expense of DISTANT co-religionists.

Our central prediction was that the more gods are conceptualized to know and punish moral violations, the more likely they will allocate coins to the distant co-religionists. The models for the *Local Co-Religionist Game* are detailed in Table 2. In the Reduced model ($n = 513$) that retains the most participants ($n = 511$), moralistic gods' punishment (OR = 1.14, 95% CI [1.03, 1.27], $p = .01$) and knowledge (OR = 1.19, 95% CI [1.02, 1.38], $p = .03$)¹³ both had a significant impact on increasing the odds that a coin went into the DISTANT cup. Relative to the Tyvan reference group, the Hadza and Yasawans consistently favored co-religionists from their local community beyond what we would expect by chance.

Table 2. Local Co-Religionist Game with sites as fixed effects.

	Full	No Local Gods	All Groups Included	Reduced	Interaction	Groups Only
Punishment (MG)	1.36 [1.17, 1.59]***	1.13 [1.00, 1.28]*	1.15 [1.03, 1.27]***	1.14 [1.03, 1.27]***	1.14 [1.00, 1.29]*	–
Knowledge (MG)	1.08 [.87, 1.34]	1.19 [.98, 1.45]	1.17 [1.00, 1.36]*	1.19 [1.02, 1.38]***	1.19 [1.02, 1.38]***	–
Reward (MG)	1.03 [.85, 1.24]	1.01 [.87, 1.18]	–	–	–	–
Moral Index (MG)	.98 [.93, 1.03]	1.02 [.98, 1.06]	–	–	–	–
Punishment (LG)	.96 [.82, 1.12]	–	–	–	–	–
Knowledge (LG)	.94 [.81, 1.08]	–	–	–	–	–
Reward (LG)	.91 [.78, 1.07]	–	–	–	–	–
Moral Index (LG)	1.03 [.98, 1.08]	–	–	–	–	–
Age (Centered)	1.01 [1.00, 1.01]*	1.00 [1.00, 1.01]	1.00 [1.00, 1.01]	–	–	–
Sex (0 = female)	1.10 [1.00, 1.20]*	1.10 [1.02, 1.19]***	1.06 [.99, 1.13]	1.06 [.99, 1.13]	1.06 [.99, 1.13]	–
Years of Formal Education	1.00 [.98, 1.01]	1.00 [.99, 1.01]	.99 [.98, 1.01]	–	–	–
Material Insecurity	1.09 [.96, 1.24]	1.04 [.95, 1.16]	1.02 [.92, 1.12]	–	–	–
# of Children	.96 [.93, .99]***	.98 [.96, 1.00]*	.98 [.96, 1.01]	1.00 [.98, 1.01]	1.00 [.98, 1.01]	–
DISTANT Religious Similarity	1.02 [.97, 1.06]	1.00 [.97, 1.04]	–	–	–	–
LOCAL Emo. Closeness	.99 [.94, 1.03]	.99 [.96, 1.03]	–	–	–	–
DISTANT Emo. Closeness	.99 [.95, 1.02]	1.00 [.97, 1.03]	–	–	–	–
OUTGROUP Emo. Closeness	.96 [.93, .99]***	.96 [.94, .99]***	–	–	–	–
Police Evaluation	1.02 [.97, 1.08]	1.01 [.97, 1.06]	–	–	–	–
Honesty Mentioned	.91 [.75, 1.10]	.92 [.78, 1.08]	–	–	–	–
Treatment	1.05 [.96, 1.15]	.99 [.92, 1.08]	1.00 [.92, 1.08]	.98 [.91, 1.06]	.97 [.82, 1.15]	–
Order (Loc. Com. First = 1)	1.13 [1.04, 1.23]***	1.08 [1.00, 1.16]*	1.06 [1.00, 1.13]*	1.05 [.99, 1.12]	1.05 [.99, 1.12]	–
Treatment*Punishment (MG)	–	–	–	–	1.02 [.83, 1.25]	–
Coastal Tanna	1.08 [.87, 1.35]	1.00 [.84, 1.20]	.98 [.83, 1.15]	1.00 [.86, 1.15]	1.00 [.86, 1.15]	1.04 [.91, 1.20]
Hadza	–	–	.62 [.50, .77]***	.66 [.56, .77]***	.66 [.56, .77]***	.66 [.59, .74]***
Inland Tanna	1.06 [.80, 1.42]	.99 [.77, 1.27]	.94 [.76, 1.16]	.99 [.87, 1.13]	.99 [.86, 1.13]	1.01 [.90, 1.13]
Lovu	–	.98 [.81, 1.18]	.94 [.81, 1.09]	1.03 [.91, 1.15]	1.02 [.91, 1.15]	1.06 [.94, 1.19]
Mauritius	.93 [.77, 1.13]	.95 [.81, 1.12]	.90 [.78, 1.04]	.95 [.85, 1.07]	.95 [.85, 1.07]	.97 [.87, 1.09]
Pesqueiro	.92 [.74, 1.14]	.97 [.81, 1.16]	.91 [.78, 1.07]	.96 [.85, 1.08]	.97 [.85, 1.08]	.99 [.88, 1.11]
Yasawa	.82 [.61, 1.11]	.77 [.62, .96]***	.80 [.69, .92]***	.83 [.73, .95]***	.83 [.73, .95]***	.85 [.75, .95]***
Constant	.87 [.60, 1.27]	.73 [.53, .99]***	.80 [.62, 1.03]	.71 [.62, .84]***	.71 [.60, .84]***	.94 [.87, 1.02]
Mean VIF	5.72	4.92	2.32	1.25	2.66	–
AIC	1740	2229.5	2716.9	2761.3	2763.3	3267.9
Pseudo-R ²	.22	.15	.18	.17	.17	.16
N	317 ^a	412 ^b	503	513	513	589

Note: Odds ratios [and 95% CIs] for variables predicting allocations to the DISTANT co-religionist.

^aHadza, and Lovu not included, Hadza not included. All models are binomial logistic regressions. Models include field sites as fixed effects. Moralistic god variables are denoted as “MG,” and local gods are denoted as “LG.” Pseudo-R² are Nagelkerke’s R².

***If positive effect, lower bound of CI > 1.00, if negative effect, upper bound of CI < 1.00; *If positive effect, lower bound of CI = 1.00, if negative effect, upper bound of CI = 1.00.

5.3.2. Self Game

We expected participants to allocate more coins to themselves than to anonymous, geographically distant co-religionists in the *Self Game*. The modal allocation was 14 ($n = 88$; $P(14) = .14$). There were a total of 8195 coins in the DISTANT cups ($M = 13.86$, $SD = 3.86$) whereas 9535 coins were in the SELF cups ($M = 16.13$, $SD = 3.86$) for a nearly three-coin difference on average. At $\alpha = .95$, the critical value to reject impartial play in this case is an allocation ≤ 8975 to the DISTANT cup. At $\alpha = .75$, that critical value is 8910. As such, we can safely conclude that, overall, participants favored themselves above and beyond what we would expect by chance (see following section for site-specific analyses).

Again, we predicted that the more people conceptualize their moralistic gods to know and punish moral violations, the less people will favor themselves in the *Self Game*. The models for this game are detailed in Table 3. As in the *Local Co-Religionist Game*, the Reduced model ($n = 514$) shows that the odds of allocating coins to a DISTANT co-religionist increased as a function of moralistic gods' punishment (OR = 1.11, 95% CI [1.00, 1.23], $p = .05$) and knowledge (OR = 1.24, 95% CI [1.06, 1.44], $p = .01$).

5.3.3. Comparison of games by site

To illustrate the differences between the two games by site, Figure 6 portrays the odds ratios and 95% confidence intervals from the Groups Only models from both games (Tables 2 and 3) using sites as fixed effects (Tyva Republic as a reference group) without any other variables. In the *Local Community Game*, Yasawans and the Hadza are the only two groups with confidence intervals that lie beyond the scope of what we would expect from playing by the rules (i.e., the odds ratios and confidence intervals do not cross 1). In the *Self Game*, however, all groups other than the Coastal Tannese and Indo-Fijians deviated from what we would expect by chance allocations. Comparing the magnitude of biases across games, participants were clearly more inclined to favor themselves than their local communities.

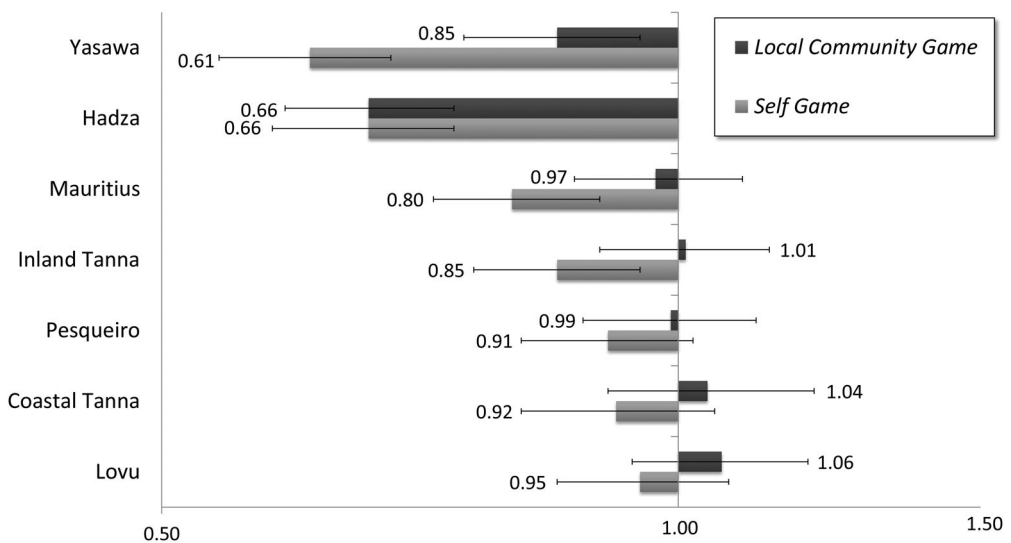


Figure 6. Site-specific odds of coins going to a distant co-religionist (Tyva Republic as reference group).

Note: Plots are odds ratios with 95% confidence intervals. No other variables are included. Values to the left of 1.00 indicate lower odds of a coin going to the DISTANT cup and values to the right indicate an increase in odds of a coin going to the DISTANT cup. X-axis is on a logarithmic scale.

Table 3. *Self Game* with sites as fixed effects.

	Full	No Local Gods	All Groups Included	Reduced	Interaction	Groups Only
Punishment (MG)	1.32 [1.13, 1.54]***	1.15 [1.02, 1.31]***	1.11 [1.00, 1.23]*	1.11 [1.00, 1.23]*	1.04 [0.92, 1.17]	–
Knowledge (MG)	1.18 [.95, 1.48]	1.24 [1.02, 1.51]***	1.22 [1.05, 1.42]***	1.24 [1.06, 1.44]***	1.25 [1.07, 1.45]***	–
Reward (MG)	1.11 [.92, 1.34]	1.09 [.93, 1.27]	–	–	–	–
Moral Index (MG)	.99 [.94, 1.04]	1.00 [.96, 1.03]	–	–	–	–
Punishment (LG)	1.05 [.89, 1.23]	–	–	–	–	–
Knowledge (LG)	.97 [.84, 1.13]	–	–	–	–	–
Reward (LG)	.91 [.78, 1.07]	–	–	–	–	–
Moral Index (LG)	.98 [.93, 1.03]	–	–	–	–	–
Age (Centered)	1.01 [1.00, 1.01]*	1.00 [1.00, 1.01]	1.00 [1.00, 1.01]	–	–	–
Sex (0 = female)	1.06 [.97, 1.17]	1.02 [.95, 1.11]	1.01 [.95, 1.09]	–	–	–
Years of Formal Education	1.01 [.99, 1.03]	1.00 [.99, 1.02]	1.00 [.99, 1.01]	–	–	–
Material Insecurity	1.07 [.94, 1.21]	1.00 [.90, 1.12]	.98 [.89, 1.08]	–	–	–
# of Children	.96 [.94, .99]***	.99 [.96, 1.01]	.98 [.96, 1.00]*	1.00 [.98, 1.01]	1.00 [.98, 1.01]	–
DISTANT Religious Similarity	1.02 [.97, 1.07]	1.02 [.98, 1.06]	–	–	–	–
LOCAL Emo. Closeness	1.01 [.96, 1.06]	1.01 [.97, 1.04]	–	–	–	–
DISTANT Emo. Closeness	.99 [.95, 1.03]	.99 [.96, 1.02]	–	–	–	–
OUTGROUP Emo. Closeness	1.02 [.99, 1.05]	1.03 [1.00, 1.06]*	–	–	–	–
Police Evaluation	1.02 [.97, 1.08]	1.01 [.97, 1.06]	–	–	–	–
Honesty Mentioned	1.16 [.96, 1.41]	1.14 [.97, 1.34]	–	–	–	–
Treatment	.94 [.86, 1.03]	.96 [.88, 1.04]	.97 [.90, 1.04]	.97 [.90, 1.04]	.84 [.71, .99]***	–
Order (Loc. Com. First = 1)	1.11 [1.02, 1.20]***	1.07 [.99, 1.15]	1.04 [.97, 1.11]	1.03 [.97, 1.10]	1.03 [.97, 1.10]	–
Treatment*Punishment (MG)	–	–	–	–	1.22 [.99, 1.50]	–
Coastal Tanna	.92 [.73, 1.14]	.91 [.76, 1.09]	.87 [.74, 1.03]	.88 [.77, 1.02]	.89 [.77, 1.02]	.92 [.81, 1.05]
Hadza	–	–	.70 [.56, .86]***	.70 [.61, .82]***	.70 [.60, .82]***	.66 [.58, .74]***
Inland Tanna	.93 [.70, 1.25]	.79 [.62, 1.02]	.80 [.65, .99]***	.82 [.72, .94]***	.82 [.72, .93]***	.85 [.76, .95]***
Lovu	–	.87 [.72, 1.05]	.91 [.79, 1.05]	.95 [.84, 1.07]	.95 [.84, 1.07]	.95 [.85, 1.07]
Mauritius	.82 [.67, .99]***	.81 [.69, .95]***	.78 [.68, .90]***	.82 [.73, .91]***	.81 [.73, .91]***	.80 [.72, .90]***
Pesqueiro	.91 [.73, 1.12]	.90 [.75, 1.08]	.90 [.77, 1.04]	.91 [.81, 1.02]	.90 [.80, 1.02]	.91 [.81, 1.02]
Yasawa	.69 [.51, .93]***	.71 [.57, .88]***	.62 [.54, .72]***	.64 [.56, .72]***	.64 [.56, .72]***	.61 [.54, .68]***
Constant	.60 [.41, .87]***	.67 [.49, .91]***	.88 [.69, 1.14]	.80 [.68, .94]***	.83 [.71, .98]***	1.04 [.96, 1.13]
Mean VIF	5.69	4.84	2.31	1.24	2.85	–
AIC	1882.9	2381.8	2876.3	2920.2	2918.7	3443.9
Pseudo- R^2	.32	.25	.21	.20	.20	.21
N	317 ^a	413 ^b	504	514	514	591

Note: Odds ratios [and 95% CIs] for variables predicting allocations to the DISTANT co-religionist. Hadza and Lovu not included.

^bHadza not included. All models are binomial logistic regressions. Models include field sites as fixed effects. Moralistic god variables are denoted as “MG,” and local gods are denoted as “LG.” Pseudo- R^2 are Nagelkerke’s R^2 .

***If positive effect, lower bound of CI > 1.00, if negative effect, upper bound of CI < 1.00.

*If positive effect, lower bound of CI = 1.00, if negative effect, upper bound of CI = 1.00.

5.4. Prime condition

5.4.1. The effects of primes

If primes had sufficient causal force in altering allocations, across sites, the chances that a coin is allocated to distant co-religionists should be significantly greater in the prime condition than in the control condition across sites. That is, if religious symbolism harnesses the social effects of religion, and those social effects increase impartiality toward greater numbers of people, then participants in the control condition ought to give fewer coins to distant co-religionists. [Figure 7](#) illustrates the difference between the control and prime conditions for both games (see Supplementary Table S5 for values). While some sites show signs of prime conditions working in the predicted direction (Tyva Republic and Pesqueiro for the *Local Co-Religionist Game*; Pesqueiro for the *Self Game*), other sites appear to have had the opposite effect (Lovu and Mauritius for the *Local Co-Religionist Game*; Mauritius and Tyva Republic for the *Self Game*), or no obvious effect at all. Other findings reveal a more nuanced picture, pointing to interactions between religious priming and other variables of interest. All field-specific findings related to religious priming should be treated with caution given the limited sample size in each field setting.

According to these raw differences, Pesqueiro was the only site that was unambiguously supportive of our hypotheses about the religious prime of moralistic gods promoting greater allocations beyond local communities in both games (see Cohen et al.'s article in this issue). Considering that this region has been Christian for longer than the other sites with world religions, this may offer one possible explanation for the close association. Note, however, that as these are mean allocations, treatment effects vary once the additional controls are included and local context is taken into account (see the articles by Cohen et al., McNamara and Henrich, Purzycki and Kulundary, Willard, and Xygalatas et al. for site-specific effects). Overall, the primes did, however, interact with one of our key variables in the *Self Game* in the expected fashion (next section below).

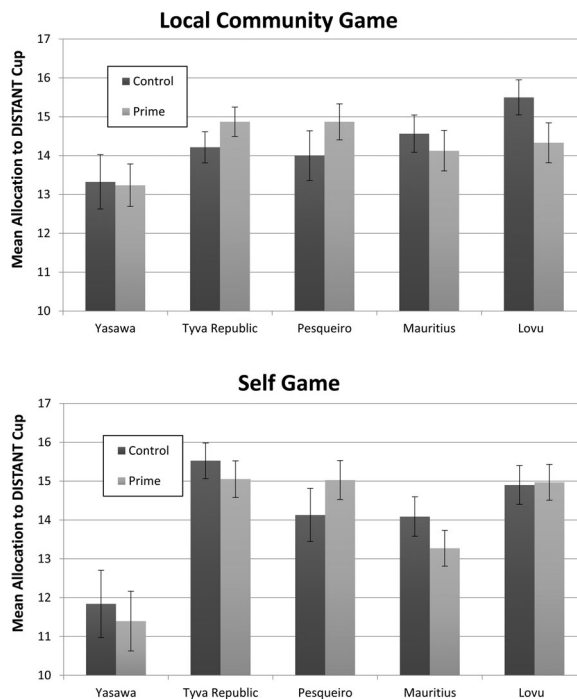


Figure 7. Effect of prime on raw allocations by site (error bars are standard errors of the mean) for *Local Co-Religionist Game* (top) and *Self Game* (bottom).

In Yasawa, Fiji (see McNamara and Henrich's article), there were three conditions: a neutral control condition, a traditional religious prime, and a Christian religious prime. Compared to the neutral condition, the traditional prime produced significantly higher allocations to the village, but not to participants. Additionally, how uncertain players were about resources and individual beliefs in both Christian and local spirit punishment moderated the effects of all primes. In the Tyva Republic (see Purzycki and Kulundary's article), while there were no obvious effects of the prime condition, how participants framed the experiment did affect gameplay. If the game reminded participants of charity or the local Buddhist temple, they were more likely to put coins into the DISTANT cup in the *Self Game*. In our Brazilian sample (see Cohen et al.'s article), participants in the prime condition allocated more coins to the distant co-religionist in both games. In this case, condition interacted with key variables of interest in predicting allocations across the two games. When primed with religious symbols, one's trait level of belief in God's knowledge and punishment became less relevant in participants' decisions to allocate coins to either cup (i.e., the prime may have acted as a reminder of God's knowledge and punishment). In Mauritius (see Xygalatas et al.'s article), there was no observed main effect for the prime for either of the games and no interaction effects found with sex among Hindus. There was, however, an interaction effect with condition and Shiva's punishment. In Lovu, Fiji (see Willard's article), condition did have a significant interaction with sex among Hindu Indo-Fijians; women exhibited fewer biased allocations in the *Local Co-Religionist Game* when they were religiously primed.

5.4.2. Interaction effects

For a deeper examination of the overall influence of the prime condition, we crossed our punishment index with treatment while holding the rest of the critical variables constant (see Interaction Models in Tables 2 and 3). If primes are more effective in inducing impartiality at higher levels of supernatural punishment, there should be a positive interaction effect. In the *Local Co-Religionist Game*, there were no obvious interaction effects (OR = 1.02, CI = [.83, 1.25], $p = .87$; see Table 2). In the *Self Game*, however, the interaction between the moralistic gods' punishment scores and treatment did have a marginal effect on increasing the odds of allocating a coin to the distant co-religionist (Table 3; OR = 1.22, CI = [.99, 1.50], $p = .06$). In other words, the prime increased the odds of allocating a coin to distant co-religionists as supernatural punishment scores increased. Running the same model with only sites that used primes yields a virtually identical effect (OR = 1.24, CI = [.99, 1.58], $p = .07$; $n = 375$). However, as illustrated in Figure 8, if we plot the regression lines for chances of a coin going to the DISTANT cup by punishment scores for both the control and the treatments, it is clear that the effect of the treatment lies primarily with those who claim moralistic gods do not punish. In other words, the treatment *reduces* the chances that people give to DISTANT co-religionists when they claim such deities do not punish.

If we replace moralistic gods' punishment with the knowledge index, the interaction with condition had wider confidence intervals (OR = 1.26, CI = [.89, 1.79], $p = .19$). Pooling the punishment and knowledge indices together yields no important interaction with condition (OR = 1.25, 95% CI = [.92, 1.69], $p = .15$; see Purzycki et al., 2016c for analyses including this combined punishment and knowledge index). For more details about the strength and direction of primes' effects by site, see the contributions by Cohen, Baimel, and Purzycki (Pesqueiro); McNamara and Henrich (Yasawa); Purzycki and Kulundary (Tyva Republic); Willard (Lovu); and Xygalatas et al. (Mauritius) in the present volume.

In order to ensure that treatments were not driving punishment scores, we also regressed the moralistic gods' punishment on the same variables in the All Groups Included Model (with Tyva as a reference group, as their mean punishment score was the closest to the overall mean), and found that the treatment condition had little to no effect on punishment scores (OR = 1.03, 95% CI = [.96, 1.09], $p = .43$). Together, these findings suggest that the primes contributed to gameplay through people's beliefs about supernatural punishment.

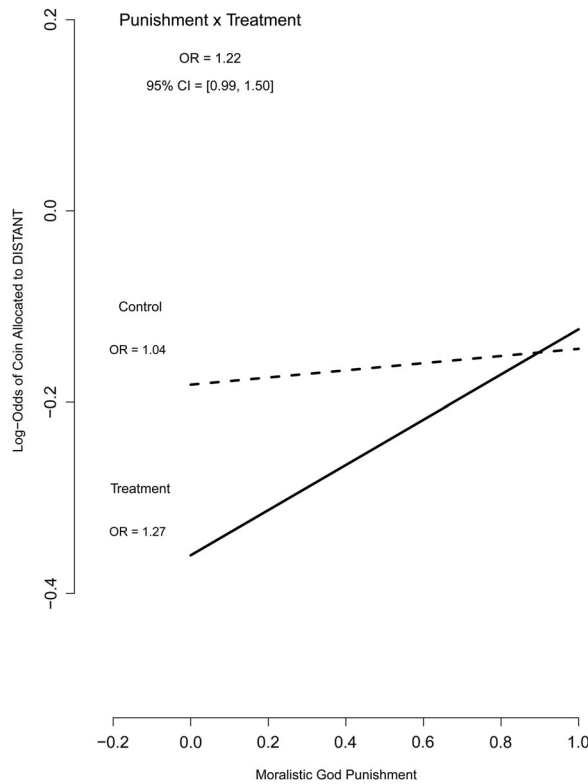


Figure 8. Interaction plot of mean allocations to DISTANT cup in *Self Game* between condition and levels of moralistic god punishment evaluation.

5.5. Group- and national-level material security

We expected that in addition to supernatural punishment and monitoring, indices of existential security should have played a role in promoting impartiality toward distant co-religionists. In the *Local Co-Religionist Game*, the effects for material insecurity, education, and number of children on allocations were negligible. Additionally, our intergroup relations variables and participants' evaluations of the police showed no consistent, significant effects in gameplay overall for either game. However, for the *Self Game*, while we did not retain material insecurity or years of formal education as significant factors in impartiality in our "Reduced" models, the more children participants had, the more likely coins went to their own cups (OR = .98, 95% CI = [.96, 1.00], $p \leq .05$). This is to be expected; people may likely bias allocations to favor themselves and their dependents.

As discussed in section 2.1, we predicted that individual material insecurity and years of formal education would indicate greater allocations to the DISTANT co-religionists. However, there was no robust relationship at the individual level. If we examine group- and national-level indices of material security, however, the story becomes a little more complicated. As our groups only number eight, our ability to conduct reliable hierarchical and group-level analyses is severely limited (Angrist & Pischke, 2009; Cameron, Gelbach, & Miller, 2008; Cameron & Miller, n.d.; Cameron & Trivedi, 2005). Nevertheless, when we examine the relationship between average allocation to the DISTANT cups per site and the per capita GDP of sites' countries in 2013,¹⁴ we see a positive relationship that is consistent with previous research (Hruschka et al., 2014). Table 4 tabulates national-level data (GDP, logGDP, and government effectiveness, and the Human Development Index) alongside mean allocations, material insecurity, and material confidence measures for each field site (see Supplementary Table S7 for a correlation matrix of these variables, and section S6 for R code).

Table 4. National-level data by field site and allocations to DISTANT co-religionists.

Site	Country	GDP per capita		Standardized Government		DISTANT Allocation in	DISTANT Allocation in	Material Insecurity	Material Confidence
		2013 in \$US	log GDP	Effectiveness	Human Dev. Index	<i>Local Co-Religionist Game</i>	<i>Self Game</i>		
						M (SD)	M (SD)	M (SD)	M (SD)
Coastal Tanna	Vanuatu	3277	8.09	-.21	.62	14.86 (2.88)	14.68 (2.51)	.22 (.36)	.66 (1.07)
Hadza	Tanzania	694.8	6.54	-.67	.49	11.49 (4.51)	12.18 (4.31)	.82 (.36)	–
Inland Tanna	Vanuatu	3277	8.09	-.21	.62	14.59 (3.43)	14.07 (3.80)	.26 (.38)	.53 (.80)
Lovu	Fiji	4375	8.38	-.96	.72	14.95 (2.98)	14.93 (2.96)	.83 (.33)	-.75 (1.15)
Mauritius	Mauritius	9203	9.13	.88	.77	14.34 (3.43)	13.67 (3.33)	.39 (.35)	.61 (.90)
Pesqueiro	Brazil	11,208	9.32	-.08	.74	14.43 (3.47)	14.57 (3.74)	.86 (.24)	.18 (.84)
Tyva Republic	Russia	14,612	9.59	-.36	.78	14.53 (2.49)	15.30 (2.95)	.47 (.28)	1.41 (.46)
Yasawa	Fiji	4375	8.38	-.96	.72	13.28 (3.80)	11.61 (4.98)	.50 (.40)	.32 (1.03)

Table 5. Linear regressions for log per capita GDP predicting mean allocation of site.

	<i>B</i>	95% CI	β	<i>SE (B)</i>	<i>p</i>
log(GDP)	.86	[−.00, 1.71]	–	.35	.05
Constant (<i>Local Co-Religionist Game</i>)	6.86	[−.40, 14.12]	.71	2.97	.06
log(GDP)	.79	[−.36, 1.93]	–	.47	.14
Constant (<i>Self Game</i>)	7.23	[−2.46, 16.93]	.57	3.96	.12
log(GDP)	.82	[−.10, 1.74]	.66	.38	.07
Constant (<i>Both Games</i>)	7.05	[−.77, 14.87]	–	3.20	.07

Additionally, the moralization index for our moralistic gods shows strong negative relationships with the national-level data. In other words, higher moralization rates correspond to lower respective countries' GDP, Human Development Index, and government effectiveness (cf. Baumard et al., 2015). This supports the prediction that the roles of supernatural punishment and moralistic deities may be offset by secular institutions performing a similar function. More suspect, however, is the correlation between moralizing gods and allocation. This appears to be negative, but this is driven by Yasawans, who rated God as extremely moralistic, but also showed systematic favoritism in both games (see above).

As inclusion of the other national-level variables contributed to multicollinearity, and it had the clearest relationship, we regressed log-transformed GDP with game allocations with the aforementioned caveats in mind. As illustrated in Table 5 and Figure 9, log-transformed per capita GDP of countries predicted greater mean allocation to the DISTANT co-religionist in the *Local Co-Religionist Game* ($F(1, 6) = 5.95, p = .05, \text{adj. } R^2 = .41$). A similar trend appeared in the *Self Game* ($F(1, 6) = 2.84, p = .14, \text{adj. } R^2 = .21$) and in the mean allocation for both games combined ($F(1, 6) = 4.74, p = .07, \text{adj. } R^2 = .35$). These results are consistent with the aforementioned research that found a positive relationship between national wealth and allocations.¹⁵ Note, however, that the Hadza (Tanzania) are a clear outlier and the relationship is not nearly as strong without them. Moreover, since we only have eight data points, this finding should be interpreted with extreme caution.

6. Discussion

6.1. Summary

There are a few important summary points of our study worth highlighting. First, allocations deviated from impartiality toward favoring participants and their local co-religionists when playing against geographically distant co-religionists. Second, the higher people rated their moralistic gods as punitive and knowledgeable, the less they favored themselves and their local co-religious community in ways that benefitted geographically distant co-religionists. This is consistent with the prediction that moralistic, punishing, and knowledgeable gods can contribute to expanding cooperative behavior beyond family and local communities. Third, supernatural punishment clearly had a more consistent, robust, and significant effect than monitoring. Fourth, while years of formal education and our self-report measures of material insecurity showed no systematic effect on allocation bias, logged per capita GDP predicted mean allocations by site (although this latter result is subject to more uncertainty given the small sample size). After controlling for site effects, number of children had an effect on players allocating more coins to themselves. As such, this indicates that existential security may play an important role.

Whereas beliefs about supernatural punishment and knowing more about humans' thoughts and behaviors predicted more impartial play with distant co-religionists, it is important to note that religious priming alone did not have a clear, consistent effect on game behavior across sites. This could be because the effects of religious primes do not generalize in the same direction beyond non-Western contexts, or it could be because of the methodological challenges in obtaining priming effects across diverse cultural contexts, where simply playing the game creates large cognitive loads. Or,

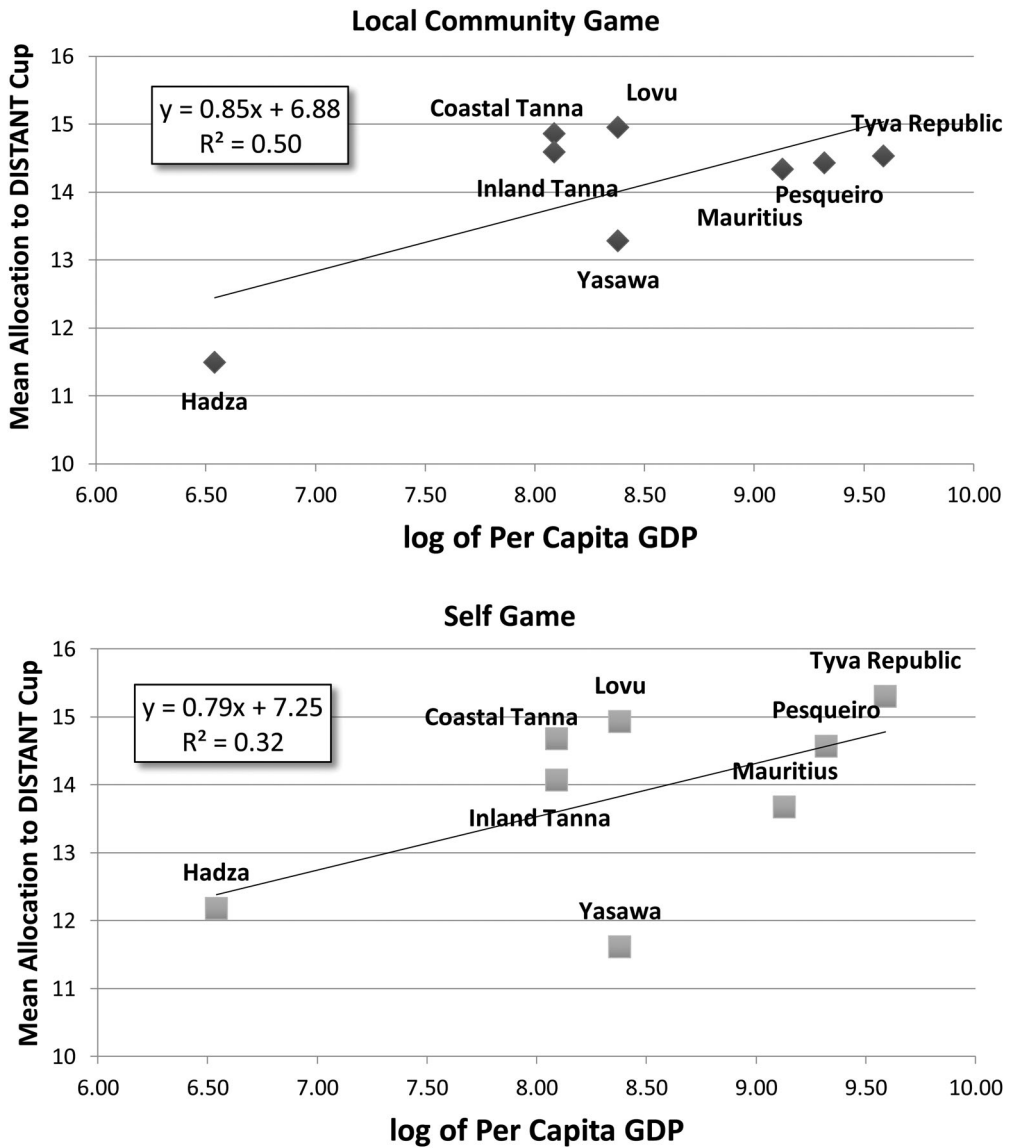


Figure 9. Mean allocation to DISTANT cup increases as a function of 2013 per capita GDP of home country for *Local Co-Religionist Game* (top) and *Self Game* (bottom).

this null result could be due to limitations of social priming as a reliable technique to elicit corresponding concepts (Gomes & McCullough, 2015; Shariff et al., 2015; Van Elk et al., 2015; Willard, Shariff, & Norenzayan, 2016). These are important questions that should be pursued in future research. Nevertheless, our results did suggest that when individual gains are at stake as in the *Self Game*, there was a marginal trend such that religious symbols interacted with individuals' models of gods' punishment, lending more nuanced support to the treatment condition's effects on gameplay.

Our results speak to the ongoing debate over religion's contribution to prosociality in some crucial ways (Galen, 2012; Martin & Wiebe, 2014; Oviedo, forthcoming). First and most parsimoniously, we show that conceiving of gods as moralistic, punitive, and knowledgeable of human intentions and behaviors corresponds to reduced rule-breaking behaviors that benefit distant

individuals who share similar beliefs and/or expectations. Second, in these games, playing by the rules benefits people whom decision-makers are likely never to meet. Both of these findings suggest that religious prosociality is likely to correspond to the kind of religion people affiliate with and/or endorse. What our effects suggest is that certain kinds of religious beliefs open pathways of *potential* cooperation, the kind of cooperation that facilitates reliable, consistent, and persistent “impartial play” among anonymous co-religionists. In turn, this is precisely the kind of cooperation that makes large-scale social organization possible (Norenzayan, 2013; Norenzayan et al., 2016).

6.2. Strengths, problems, and limitations

This study had several unique features. First, we relied on individual-level measures of religious beliefs and practices, rather than on qualitative ethnographic impressions. Second, this is one of the first individual-level studies directly measuring how individual mental models of gods correspond to behavioral outcomes in a culturally diverse sample that went beyond Western populations, using a mixed-methods regime from anthropology, economics, and psychology.

Additionally, while we measured participants’ views of moralistic versus local gods, and while local gods’ punishment and knowledge had no robust overall effect, this does not indicate that local gods have no effect on cooperation. Had our cups, for instance, been associated with co-religionists of local gods rather than moralistic gods, we may have seen an effect. As non-punishing, nonmoralistic spirits with locally bound knowledge may still prime moral cognition (Purzycki, 2011, 2013), we may see similar effects with local god conditions as we do in McNamara and Henrich’s piece in this issue. However, as many local gods serve local functions, some may increase ingroup favoritism, and the idea of a “geographically distant co-religionist” might be meaningless for many traditions. We strongly encourage more research on conceptions of local gods and their effects on sociality.

There are several limitations to our study, which we hope future research will address. These cross-cultural findings do not allow us to conclusively tease apart correlation from causation, and do not allow us to make strong claims that belief in gods’ knowing and punishing capacities causes impartial behavior toward distant co-religionists. However, it is difficult to postulate what an alternative proximate causal explanation would be as it would presumably have to be correlated with our focal deity variables. The priming conditions showed no consistent effect on allocations across subsamples, limiting inferences about causality. However, the primes did interact with individual beliefs differentially in the *Self Game*. Regarding local-level effects, we refer readers to the articles by McNamara and Henrich (Yasawa, Fiji), Purzycki and Kulundary (Tyva Republic, Russia), Willard (Lovu, Fiji), and Xygalatas et al. (Mauritius) in this issue for further discussion.

Another very important limitation, ripe for future research, is that we do not know whether the effects we found would apply to religious outgroup members (see Willard, this volume, for inclusion of outgroups in these experiments). One possibility is that religiously motivated impartial behavior toward distant co-religionists may spill over and extend even to religious outgroups at least in some circumstances. But we suspect the expanded cooperation enabled by religious elements may still be bounded, and in some cases, as when real or imagined conflict is already present, may turn toxic toward religious outgroups, fueling intolerance and violence, a pressing contemporary issue that currently we know little about (Atran & Ginges, 2012; Norenzayan et al., 2016). While we included in our models measures of intergroup relations and emotional proximity to various groups (section 3.4), these were admittedly crude measures. Relatedly, our LOCAL and DISTANT cups were marked with religious affiliation or similarity to players rather than anonymous others marked by, for example, village or town. While there was enough variation in allocations across sites to rule out the possibility that religious group identification drove allocations to deviate from chance in any specific way (and it was consistent across sites), we lacked measures to determine variation in salience of religiosity as an identifying marker of others. In other words, the Hadza, for instance, are not likely to think of “religion” or “religious affiliation” as a crucial marker of status, whereas in

Mauritius, participants might allocate fewer coins if they didn't know to whom they were going (i.e., they might be given to a Muslim or Christian).

As mentioned in section 4.5, any group-level analyses are constrained by our small group size. By dramatically increasing the group samples, we would be able to more confidently build generalizations applicable to global trends and assess cross-cultural variation rather than control for it as a fixed effect. For instance, we have little in the way of what the relevant norms of “cheating” and “fairness” are in these societies and how those might have played a role in the outcome. While using sites as fixed effects should reduce concerns of the influences of pooled, group-level variation, it remains curious, for instance, that the Hadza are regularly characterized as a highly egalitarian group but consistently favored themselves (Marlowe, 2004), while Tyvans, who suffer from the highest homicide rate per capita than any other region in Russia (Treyger, 2011), largely played by the rules in these experiments (see Purzycki and Kulundary's article). These are the extreme cases, however. As is often the case, experiments such as these often appear to suffer from an acute deficit in ecological validity.

We are currently expanding our set to include more diverse and unique ethnographic settings in order to assess intergroup variation more reliably. Even after controlling for groups, our results remain moderately consistent with preceding studies detailing the effects that market integration, existential security, and religion have on cooperation (Henrich, Ensminger, et al., 2010; Hruschka et al., 2014; McNamara et al., 2016). Further society-level factors are nevertheless crucial to consider in order to further tease apart the causal links between religion and cooperation. In doing so, we may also have more confidence in our results speaking to historical cases and processes as well.

A case in point is a recent study by Watts et al. (2015). Using coded ethnographies (see section 1.2 for discussion), they found that throughout Austronesia, broad beliefs in supernatural punishment, but not creator “High Gods” like those of Christianity and Islam, predated the emergence of social complexity. They also found evidence suggesting that norms for supernatural punishment may have coevolved with social complexity in a mutually reinforcing dynamic. These historical analyses are important and complement the current findings. However, it is difficult to directly compare such results with ours. While Watts et al. relied on group-level data coded from qualitative ethnographies and Bayesian imputation to make inferences about the past, we measured conceptions of gods' minds at the individual level in relation to impartial behavior toward distant co-religionists. Both approaches have their virtues and limitations that future research may explore in greater depth.

6.3. Conclusion

We assessed core hypotheses about the potential role of religious elements in the expansion of human sociality. By including culturally diverse samples, we aimed at achieving some degree of generalizability beyond Western samples. By attending to ethnographic detail using living participants, our cross-cultural study aims to avoid the pitfalls of oversimplification and decontextualization. That said, we now turn our attention to the case studies in this volume that provide a close, on-the-ground look at the context and diversity of our sample.

- (1) Hadzaland, Tanzania: Apicella's article on the Hadza reports results from this unique population. The Hadza are the only sample of foragers in the present volume, and also represent a sample hailed as having virtually no or minimal religious traditions that appears to deify the sun and moon, if only inconsistently.
- (2) Tanna, Vanuatu: Atkinson's comparative study of the Inland and Coastal Tannese horticultural populations provides a glimpse of allocations from two closely related populations who are differently affected by Christianity. Gods' rewards and prayer frequency positively predicted greater allocations to distant co-religionists.
- (3) Pesqueiro, Brazil: Cohen, Baimel, and Purzycki's article reports the prosocial effects of the supernatural prime toward distant co-religionists, finds that God's rewards predict impartial

allocations, and highlights the local significance of St. Mary in Pesqueiro, Brazil, a case where the two focal gods of the study are closely intertwined.

- (4) Yasawa, Fiji: McNamara and Henrich elaborate upon the significance of local mores and how local social and ecological constraints modulate prosocial behavior. By comparing the effects of psychological primes between traditional ancestor spirits, the Christian God, and a non-religious control, reminders of traditional values increased favoritism toward the village.
- (5) Kyzyl, Tyva Republic: Purzycki and Kulundary's article on the impartial play of Buddhist Tyvans from southern Siberia highlights the influence of emerging class, identity, and social change in gameplay. They also report an effect of self-generated priming effects; when the experiment reminded participants of charity or the temple, they gave more to distant co-religionists than they did when they thought of games or gambling.
- (6) Lovu, Fiji: Willard highlights differential treatment of gameplay between Hindu, Christian, and Muslim Indo-Fijians, finding gender-specific effects for the supernatural prime condition in the predicted direction. Moreover, she also found that religious similarity predicts more impartial behavior than ethnic similarity.
- (7) Pointe aux Piments, Mauritius: Xygalatas et al. present results testing the difference in gameplay when Hindus play in a secular versus religious setting. Participation in extreme rituals increases the odds that participants give to distant co-religionists.

Notes

1. Note that religious and secular sources of trust and cooperation may be to some extent interchangeable. As indicated by the literature on secularization (Norenzayan & Gervais, 2015; Norris & Inglehart, 2012; Solt et al., 2011), when the effectiveness of secular institutions increases in some societies, and when wealth and decision-making power are distributed more equally across sectors of society, religiosity dwindles. Ultimately, the selection pressures that favor prosocial religions are minimized when effective secular social institutions take over the role of religious communities and services. Indeed, previous research (Hruschka et al., 2014; McNamara et al., 2016) shows that the more people feel the police are effective, the less they will cheat others to favor themselves and their local communities. Additionally, playing by the rules of experimental games increases as a function of both GDP per capita and government effectiveness (Hruschka et al., 2014). Resource insecurity at local and national levels as well as confidence in secular forms of social regulation such as the police should therefore positively contribute to the expansion of cooperation as well. But in the absence of effective secular monitoring, certain religious concepts may also alter the course of social interaction and may significantly contribute to this expansion. We return to this point below.
2. We note that a god's status as being "High" or as a creator deity has no psychological potency in the expansion of the cooperative sphere in our theoretical model. Note, too, that the studies assessing the emergence of moralistic gods using the Standard Cross-Cultural Sample necessarily includes this constraint in their analyses. In other words, these studies assess the emergence of creator deities that may or may not be "specifically supportive of human morality" rather than concerned with morality regardless of their capacity as creators. See next paragraph.
3. Note that nonmoralistic gods or spirits may nevertheless prime latent moral cognition, perhaps by virtue of their agency and/or concern for human behavior (Piazza et al., 2011; Purzycki, 2013). Explicitly associating moral domains with gods' concerns may expedite behavioral corollaries, especially if "good" behavior is a widely shared expectation of conduct endorsed by powerful agents.
4. This section heavily draws from the supplementary materials of Purzycki et al. (2016c).
5. Note that we removed the Hadza data for these questions and their difficulty with scales suggest the data are suspect. We nevertheless include the data in our public data set for further examination.
6. As the Hadza have difficulties with continuous scales, these questions and response options were altered to accommodate them (e.g., "yes," "no," and "I don't know"). These alterations include our basic religiosity questions, questions about the gods, material security questions, and perceptions of their religious ingroup, distant co-religionists, and religious outgroups. As such, where noted, all models considering such variables exclude the Hadza. Additionally, local god questions were not asked to Lovu participants because of ceiling effects and a lack of variation found in the *Religious Landscape* interview.
7. Because of practical problems with our original frequency scale, we altered the protocol during our field season. As such, two sites (Pesqueiro, Tyva Republic) answered this question on a five-point Likert scale: (4) Every day or multiple times per day; (3) A few times per week; (2) A few times per month; (1) A few times per year; and (0)

Very rarely/never. Among the Tannese, Indo-Fijians, Mauritians, and Yasawans, we used a four-point scale: (0) Never; (1) Sometimes; (2) Frequently; and (3) All the time. Note, too, that at the Coastal Tanna site there was a clerical error in translation on the last two points of this scale (2 and 3) which rendered them virtually indistinguishable (see Atkinson, this volume). We therefore normalized individuals' responses by dividing them by the maximum possible value on the scales used. For all regressions including this variable, we remove the Coastal Tannese from the analyses. Note, however, that in Purzycki et al. (2016c), there were no major differences across models with or without the Coastal Tannese. As the questions regarding gods' frequency of punishment of immoral behavior were the source of the confusion and not as central to our concerns as the amount of importance people claim their gods place on the punishment of immoral behavior, we focus here on the latter.

8. Note that this question was not asked of most of the Hadza due to their difficulty with more open-ended questions.
9. Because of coin visibility, we removed a single individual from the *Local Co-Religionist Game* from both the Coastal and Inland Tanna sites. As such, the sample size in this game is 589.
10. All focal models presented in the main text were run in *R* (R Core Team, 2012). For statistical coding scripts for *R* and *STATA*, see supplements for present article as well as those in Purzycki et al. (2016c) and http://hecc.arts.sites.olt.ubc.ca/files/2016/01/Purzycki_et-al_Nature_Script.R.zip. Purzycki et al. (2016c) report similar models, but without interactions and the OUTGROUP variable.
11. As material insecurity and material confidence were negatively correlated ($r = -.41, p < .001$; see Supplementary Table S1), and the Hadza answered different material confidence questions using a dichotomous scale, we focus here on material insecurity. See online supplements for correlation table for all target variables.
12. Elsewhere (Purzycki et al., 2016c), we report a variety of other analyses including sites as clustered robust standard errors clustering by site, and combined game models using individuals and clusters and mixed effects. Here, we report the most conservative models using sites as fixed effects and also emphasize intergroup variation in allocations.
13. Note that while the breadth of the confidence intervals for moralistic gods' knowledge shifts across models, its coefficients do not seriously fluctuate. This suggests that its effect is stable, but it is not well estimated.
14. These data are drawn from the 2013 World Bank data at: http://data.worldbank.org/indicator/NY.GDP.PCAP.CD?order=wbapi_data_value_2013+wbapi_data_value+wbapi_data_value-last&sort=asc (accessed 10 April 2015).
15. Note, however, that if we regress allocations to DISTANT by standardized measures of government effectiveness, there are no obvious relationships in either the *Local Co-Religionist Game* ($F(1, 6) = .54, p = .49$, standard $R^2 = .08$), or the *Self Game* ($F(1, 6) = .38, p = .56$, standard $R^2 = .06$).

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