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COMMENTARY TO HABILITATION THESIS[1]

Why religious people trust each other? A synthesis of experimental research on religious beliefs and behaviors

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Abstract

The main goal of this habilitation thesis is to elucidate whether and how religious people trust each other. To this end, this thesis utilizes an interdisciplinary suite of methods and approaches and provides a multi-level investigation into the role that religious systems play in facilitating interpersonal trust. Across nine studies organized into four thematic clusters, we first investigated low-level mechanisms harnessed by rituals to increase interpersonal trust. The second cluster of studies examined mechanisms by which rituals with specifically religious content promote trustworthy behavior. The third cluster investigated the role that belief in moralizing gods plays in normative conduct. Finally, studies in the fourth cluster examined how trustworthy people can find each other to initiate a trustworthy exchange for mutual benefit. Together, these studies showed that human religious practice harnesses mechanisms such as mirroring and synchrony to induce trust, associative learning to instill normative behavior, perceptual mechanisms to make religious norms objective, belief in moralizing gods to increase trustworthiness through the fear of punishment, and specific appearances and behaviors (or ban thereof) to reliably communicate membership, its associative norm compliance, and overall trustworthiness to other co-religionists.

[1] The commentary must correspond to standard expectations in the field and must include a brief characteristic of the investigated matter, objectives of the work, employed methodologies, obtained results and, in case of co-authored works, a passage characterising the applicant's contribution in terms of both quality and content.

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Contents

1. Introduction

Compared to other animals, humans are an extremely cooperative species. A key component of human psychology facilitating cooperation is interpersonal trust. Every day, we entrust our lives to bus drivers, our health to medical doctors, our children to childcare workers, and our food choices to restaurant chefs. This level of trust is remarkable given that we often do not personally know these trustees, not to mention that they are not part of our family, where we could expect this level of cooperation. The latter point is crucial because trust is defined by the vulnerability of the trustor, who expects positive intentions from the trustee (Rousseau et al., 1998). In other words, trust is risky. Yet, trust is indispensable for any functioning social relationship, a fact long recognized in psychology (Erikson, 1950). Human exceptionality in this regard stems from the ability to commonly bestow trust on people with whom we have no prior experience nor are genetically related.

Potential explanations of this exceptionality stem either from dispositional or situational factors. Dispositional trust is the propensity to trust other people even in the absence of any prior information on their behavior, a concept initially proposed by Rotter (1967). Although multiple factors generate the variation in this disposition (from genetic, over developmental, to experiential; Krueger & Meyer-Lindenberg, 2019), it could be argued that humans have, on average, high dispositional trust, which allows them to enter risky cooperative endeavors easily. On the other hand, the situational accounts point out that humans have a unique psychological make-up that allows them to reliably recognize trustworthy people. Indeed, people are quick to judge trustworthiness from statics faces (Willis & Todorov, 2006) and facial expressions (Krumhuber et al., 2007), and these situational judgments are relatively stable in children after ten years of age (Caulfield et al., 2016). These judgments also have direct consequences in real life. For example, inmates with untrustworthy faces (as rated by independent raters) received more severe punishments for the same class of offense (J. P. Wilson & Rule, 2015).

Although both the dispositional and situational accounts have merit, as is often the case with binary scientific concepts in direct opposition, a more careful examination of these propositions reveal that neither of them is exclusively supported. While there is an intra-individual variability in trusting interactions across different situations, over the long term, individual patterns in trusting decisions can be detected (Thielmann & Hilbig, 2015). In other words, people actively judge whom to trust, and some people have, on average, a higher percentage of positive trust decisions.

However, the story of trusting and trustworthy people falls apart when we consider human mentalizing abilities (Frith & Frith, 2007). Apart from the relatively quick and intuitive judgments of trustworthiness, human psychology has another powerful tool for social interaction: cognitive mechanisms related to understanding others' intentions, imaginative

capacities pertaining to planning and simulating possible action outcomes, and, importantly, mechanisms allowing symbolic communication of intentions to other people (Tomasello et al., 2005). While these mechanisms are crucial for human-typical social interactions, including goal sharing, coordination, and mutual adaptation of expectations, they also have significant side effects in allowing people to pretend a quality or a character (e.g., trustworthiness; Hare, 2017). We all likely have some experience with people who let us down by pretending to be someone else, not meeting our expectations (be it in a cooperative context, romantic relationship, or work-related partnership), or downright cheating on us. Moreover, research benchmarking participants' trustworthiness ratings with the actual behaviors of the rated people (Rule et al., 2013) failed to confirm the predictive power of these ratings across different contexts (e.g., people failed to reliably identify military convicts vs. military heroes from their faces). Although trust bestowed on family members and close friends can be scaffolded by a mutual history of cooperative interactions, interactions with anonymous people lacking shared history cannot rely on trustworthiness cues alone since these cues can be easily manipulated. To explain the extraordinary (in comparison to other animals) human cooperativeness, we need to add another mechanism beyond trust to the mix. This mechanism is culture. Specifically, cultural norms and institutions.

Consider an example from the Middle Ages. Before the arrival of Islam to West Africa in the 8th century, there was limited long-distance trade between different groups, and these trade networks usually facilitated the trading of single items (Ensminger, 1997). Islam began to quickly spread along these trading routes, not by military conquest or forced conversion, but by voluntary conversions of various clan chiefs and elites. It has been argued that Islam was so successful as a cultural institution in this part of Africa because it offered moral rules guaranteed by the belief in a supreme being, and these rules helped long-distance traders trust each other (Horton, 1975). Of special interest here is the *commenda* credit system, where one Muslim entrusted their merchandise or capital to another Muslim—a middleman—often without a written contract. This middleman would travel with merchandise to sell it and receive part of the profit. Should anything happen to this middleman, their family was bound to honor the commitment to the original trustor (Ensminger, 1997). This practice, facilitated by Islam's moral norms, helped intensify trade networks and spread Islam along the trade routes. Analogous historical examples are the success of Ultra-Orthodox diamond merchants (Richman, 2006) or the Russian Skoptsy religious groups (Maltsev, 2022).

Surveys of contemporary societies and experimental and observational data point to a similar association between religious affiliation and trustworthy social conduct. For example, across 70 countries, people who voluntarily express religious affiliation are more likely to engage in charity work, condemn lying for personal gains, and, importantly, are less likely to commit fraud than non-religious participants (Stavrova & Siegers, 2014). Religious people are also more charitable, trusting, and trustworthy in economic games (Everett et al., 2016). And while the subliminal priming method for inducing particular psychological states has been recently widely critiqued (Watanabe & Laurent, 2020), it appears that explicit religious primes have a robust effect on moral behavior (defined as fair conduct toward others) in participants self-identified as religious (Lang et al., 2016; Shariff et al., 2016; Xygalatas, 2013).

These examples illustrate that religious institutions help facilitate interpersonal trust and cooperation. While there are, of course, other secular institutions with similar aims (e.g., police), religions are often the most effective because they affect internal motivations for trustworthy conduct. Indeed, both historically and geographically, religions are considered the primary source of morality (defined as fair interpersonal conduct based on mutual trust). So much so that even atheists express implicit anti-atheist prejudices in the moral domain, judging religious people as less likely to commit immoral atrocities (Gervais et al., 2017). The appeal of many religious traditions to morality and the various ways religions support fair conduct add essential elements to human trust decisions.

A logical consequence of this review is that to understand when and why people trust each other, we need to take into account both dispositional and situational factors, as well as the fact that these factors are embedded in a rich cultural milieu that crucially modifies them. In other words, we need to study how culture affects internal cognitive processes responsible for trust decisions. This the main goal of the present habilitation thesis. By investigating the mechanisms by which religions increase interpersonal trust, this work contributes to capturing the complexity of human trust decisions and explaining how humans became such a cooperative species. The next section discusses my conceptual and methodological approaches to tackling this aim.

2. Conceptual and Methodological Approaches: An Argument for Interdisciplinarity

Using psychological methods to understand religious beliefs and behaviors has a long tradition that can be traced to the inception of the discipline of psychology. Indeed, many of the progenitors of psychology presented their theories on the function or dysfunction of religion. Nevertheless, in contrast to the topic of the current thesis, most of this early theorizing was focused on understanding the individual experience of the supernatural, assuming that religiosity is motivated by internal experiences. For instance, in "The Varieties of Religious Experience", James (1902) investigated mystical experiences, sudden and dramatic religious conversions, and submissions to higher powers. In his understanding, these experiences are the center of religiosity and motivate individual beliefs and behaviors (see also Otto, 1936). Freud, on the other hand, focused on the process of illusionary perception and anthropomorphizing nature as the key factors motivating religious beliefs and behaviors (Freud, 1961 [1927]). In line with his idea of suppressed traumas and sexual desires locked away in unconsciousness, religion stems from the primeval murder of a father by his sons (i.e., the Oedipean complex; Freud, 2004 [1913]). His intellectual progeny, Jung, did not understand unconsciousness as negatively as Freud did and ascribed collective unconsciousness the role of a reservoir of collective ideals and aspirations. Yet again, Jung emphasized the importance of individual spiritual experience as a path to personal growth (Jung, 1958 [1938]).

With the advantage of hindsight, we now know about the various problematic aspects of the work of these early pioneers (more on some of these problems later). However, the purpose of this mini-review was to show that a) religion was of the utmost interest to psychologists from the inception of the discipline and b) that despite the predominant focus on individual experiences, these early pioneers could not but notice that religions have a vital function in regulating human interpersonal conduct. While James' Protestantism played an essential role in his dismissal of ritual behavior as a fruitful topic in the study of religion (Proudfoot, 2004), he noticed the importance of religion in providing a universal moral order. Likewise, although Freud described religion as an illusion or a neurosis, he could not but note its important role in socializing individuals into the norms and customs of particular societies (Freud, 1961 [1927]). The role that religion (and especially ritual) plays in this process was further elaborated by Erikson, who, using the psychoanalytic approach, drew parallels between the ritualization of mother-child interactions during ontogeny and similar ritualized norm enforcement in societies (Erikson, 1966). The tension between the individual inner religious experiences and the role that religions play in social life was later captured in the intrinsic and extrinsic religiosity concepts proposed by Allport and Ross (1967), placing religion's role in social life in the mainstream interest for psychologists.

Nevertheless, despite the promising start, the psychology of religion began to wane with the advancement of secularization theories in the second half of the 20th century, which predicted

that religious devotion would soon be a thing of the past (for an overview, see Stark, 1999). Yet, at the beginning of the 21st century, it is clear that religious beliefs and behaviors still play a fundamental role in the lives of billions of people across the globe as well as in global politics and, correspondingly, the research on religion is spotlighted across top interdisciplinary journals (Isler et al., 2021; Schulz et al., 2019; C. J. M. White et al., 2021). This revived interest is also characteristic of the field of psychology (especially after the 9/11 terrorist attacks; Paloutzian, 2017), which has produced significant insights into the various facets comprising religious devotion such as mystical experiences, religious coping, religious attachment, fundamentalism, spiritual intelligence, and many others (Cherniak et al., 2021; Coleman III et al., 2019; Saroglou et al., 2020; Skrzypińska, 2021; Wilt et al., 2019). Importantly, a considerable body of psychological literature explored the relationship between religious belief and moral conduct using various correlational and experimental methodologies (Xygalatas & Lang, 2017). For instance, reporting religious devotion is associated with charitable donations (Everett et al., 2016), moral personality traits (Saroglou et al., 2005), or lower incidences of illicit behaviors (Shariff & Norenzayan, 2011).

Despite the thematic broadness of the psychology of religion, recent decades of scientific endeavor undeniably revealed that understanding complex cultural phenomena (such as religion) could never be achieved through the lens of a single discipline (Lang & Kundt, 2020). A variety of approaches are gravely needed to capture the essential aspects of cultural phenomena, including their psychological dimensions. Nevertheless, such an endeavor often faces challenges related to the different epistemological standards of the humanities and the sciences, dubbed the “two cultures problem” (Snow, 1961). While humanities are often associated with attention to detail and micro-historical events, focusing on nurture, and holism, sciences are broadly associated with a global, universalistic perspective, focusing on nature, and reductionism. Of course, the situation is much more complex and nuanced. Yet, the straw-man versions of these extreme positions often serve as a fuel for criticism of other approaches: while researchers in the humanities criticize sciences for excessive generalizations, reductionism, and genetic determinism, sciences often point out excessive holism, resignation on any attempts to generalization, and cultural determinism as the shortcoming of the humanistic approaches (Lang & Kundt, 2020). Although there are essential points raised by both sides that should not be ignored, in their extreme versions, they often block productive scientific progress.

This situation became most prominent during the first decade of the 21st century when scientific approaches started to explore potential explanations for religious beliefs and behaviors. Scientists, driven by the allure of modern technology, often oversimplified religion and aimed to reduce all variations in religious beliefs and behaviors into a single gene or brain region (Albright, 2000; Hamer, 2005). This approach, which proposed single-cause explanations like "the God gene" or "the God module" in the brain, not only repeated past mistakes of naïve reductionism but was in direct opposition to the typically holistic approaches to religion in the humanities. Although the scientific community later dismissed these simplistic approaches (Geertz, 2008) and pointed the way to a more humble inference

from neuroscientific studies (Schjoedt & Elk, 2019), the allure of the single-cause explanation of religion often remains.

Similarly, the reductionist approaches to human cultures (and religion, more specifically) manifest in geographically and religiously limited study samples often used to make inferences about general human dispositions and mechanisms regulating religiosity (Henrich et al., 2010). As a matter of fact, the theoretical rationales, concept operationalizations, as well as studied populations in the current literature on the psychology of religion are profoundly Western-based, with the majority of literature equating religions with US Christians (Clobert, 2021). This is problematic on both theoretical and methodological grounds. Religiosity is often conceptualized as a personal belief, easily separated from other value systems such as political orientation. Yet, such an understanding of religiosity heavily depends on the idea of separation of church and state, which is often foreign to non-Western societies. Methodologically, the Western bias may be illustrated by questions such as the importance of God in one's life, which would be surely puzzling in India for both Hindus (which God?) and Buddhists alike (devas from the Ārūpyadhātu or Rūpadhātu realm?).

Despite an immense promise of the potential contribution of the scientific method to understanding religion, these simplifications are often (and rightfully so) a 'red flag' for scholars in the humanities. However, finding the middle ground between the scientific and humanistic approaches is crucial if we aim to arrive at generalizable inferences about human psychology regarding mental processes and behavioral patterns associated with a specific cultural institution, that is, religion. In other words, while traditionally, human psychology has been studied using methods closer to the sciences, religion was studied (not exclusively, of course) mostly by methods closer to the humanities. To answer the fundamental question about the role of religious traditions in facilitating interpersonal trust, we need to bridge these two separate approaches, avoiding the traps of excessive reductionism and generalizations and respecting the cultural nuance of individual religious traditions, including the possible feedback loops from culture to human psychology. I previously argued that the framework of complex adaptive systems might provide a necessary toolkit to this end (Lang, 2019, 2020; Lang & Kundt, 2020).

The idea that some cultural institutions are complex dynamical systems relies on insights from complexity science, an interdisciplinary field that draws on developments in mathematics, such as non-linear dynamical systems and deterministic chaos theory, self-organization principles from thermodynamics, feedback loops from cybernetics, and systems science applied in anthropology, sociology, and economics (Thurner et al., 2018). Biologists and geneticists have also applied the principles of complex systems to living organisms and their adaptive change over time, adding an evolutionary dimension to complex systems (Holland, 1995). Specifically, the term "complex" refers to a system with many interdependent parts that interact nonlinearly (as opposed to a complicated system with additive effects). Systems are structures defined by interconnected parts that work towards a specific goal or purpose. Systems convert energy into specific outputs. The fact that they are adaptive means that the system's composition and interactions between elements can change with the changing

environment to increase the effective conversion of energy into desired outputs (or disintegrates if unsuccessful).

One of the advantages of the systemic approach is that it is scalable. We can identify and study, for instance, the human immune system as a complex adaptive system, or financial markets for that matter, depending on which level of our analysis we will select the interacting components of the system. For the purpose of the work presented in this habilitation thesis, it will be useful to understand religious traditions as complex adaptive systems. While a system's boundaries are often difficult to draw, we may think about a religious system at the level of a religious community where individuals interact with each other. Religious systems are usually composed of eight basic elements: ritual, taboo, authority, myth, sacred, supernatural agent, moral obligation, and meaning (Sosis, 2019). Ritual is the key component here because it allows the energy in the form of caloric expenditure to enter the cultural system, which can be transformed to produce the system's outputs.[2]

Among the key outputs of religious systems are the coordinated and cooperative behaviors of its members, their health, and reproduction. In other words, individuals may benefit from being part of a religious community, which involves participation in religious rituals, keeping taboos, believing in supernatural agents demanding a specific moral order, and subjecting themselves to sacred authority. The extent that one would benefit from the religious community depends on the setup of specific religious traditions in different socio-ecologies, an issue we will return to later in this chapter. For now, the important conclusion is that the chief interest of this habilitation thesis is how religious systems facilitate cooperative output. That is, how the cultural institution of religion affects the cognitive mechanisms of its members during cooperative decision-making (trust the partner or not?).

How can treating religions as complex adaptive systems help overcome the "two cultures problem"? The first key observation is that systems evolve over time (i.e., adapt to their socio-ecologies), and we should expect systems to be path-dependent (Lang, 2019). That is, their past development (be it adaptive or stochastic) will constrain their possible future states. While this path-dependency does not mean that we need to resign on claims about the general pattern of systems' development, historical stochasticity offers rich micro-histories that need to be understood by historians (and are often the subject of the humanities). A second implication following from the system's dynamical nature is that different socio-ecologies will mold systems differently (Purzycki & Sosis, 2009). That is, there is a cross-cultural variability in religious systems. Thus, each system does not have to contain the same components and the exact same feedback loops between those components. Instead, religious systems usually share most of those components, but their weight/interactions will be context specific. Again, this contextual specificity is the usual domain of the humanities and may help

[2] Ritual behavior is a key component keeping religious systems alive. While we can know about the myths, beliefs, and practices of ancient religions, the fact that no one performs their rituals also means that these religious systems are no longer active. Only by reviving these religious traditions through human activity (such as in contemporary neo-pagan movements) would we see the system transforming energy into specific outputs.

us understand the structure of particular religious systems. Finally, since religious systems are complex, they cannot be reduced into a single component or mechanism because these components non-linearly interact, giving the system emergent properties. This is not to say that religious systems cannot be decomposed; quite the contrary. It is to say that we need to be careful when studying the system's part in isolation because we are destroying the phenomena on the higher level of complexity. In order to fully capture that phenomenon, we also need to understand the non-linear interactions between the system's components (what scholars in the humanities usually see as their holistic approach).

Translating these principles into concrete research practice that would help answer the topical question of this habilitation thesis (why religious people trust each other?), we need to identify the main components of a religious system that facilitate trust (including its underlying mechanisms), track their development (during the history of a specific system as well as human evolution: how has been trust traditionally secured in communities) and their variation across contexts (how religious systems facilitate interpersonal trust in different cultural contexts). Together, this holistic approach might help us answer the topical question in more detail and nuance and with higher precision since findings from various disciplines should ideally cross-corroborate each other.[3] Given the recent emphasis on interdisciplinarity, it is no surprise that we may find the equivalents of the three approaches also in psychology: cognitive psychology, evolutionary/historical psychology, and cross-cultural psychology, respectively. Combining the methodological and conceptual toolkit of these three disciplines, the convolute of studies in this habilitation thesis aims to show how these different approaches may cross-fertilize each other.

[3] At this point, many readers would probably recognize the strong influence of E. O. Wilson's consilience ideas (1998) on my approach. Indeed, his vision of the unity of sciences is inspirational, yet naïve and problematic. I tend to see my approach as a more realistic endeavor correcting previous errors, sometimes called a second wave consilience (Slingerland & Collard, 2011).

3. The Convolute

The convolute is a result of more than a decade of my research on the topic of interpersonal trust. The presented research papers illustrate my commitment to a radically interdisciplinary approach, which I argued for above. The research also reflects my academic career that stands betwixt the humanities and the sciences. While my previous training in Religious Studies provided a humanistic perspective on religious phenomena, my work at the Laboratory for the Experimental Research of Religion at Masaryk University, Connecticut Institute for the Brain and Cognitive Sciences, and Harvard's Human Evolutionary Biology department equipped me with rigorous scientific methods and experimental logic. As such, the presented research is defined by a combination of experimental methods utilized both in laboratory and field settings, by cross-cultural comparisons to understand the contextual specificity of detected effects, and by a tight evolutionary logic that motivates research questions and hypotheses.

As introduced at the beginning of this habilitation thesis, the research question unifying the presented convolute of studies is whether and how religions facilitate interpersonal trust. By religion, I mean cultural beliefs, practices, and taboos related to the sacred and supernatural shared by a community. By interpersonal trust, I mean a willingness to rely on other people's good intentions, often making the trustor vulnerable to the trustee. However, given that the convolute comprises independent studies that do not necessarily follow from each other, trust is sometimes addressed directly and sometimes under broader terms such as social bonding, cooperation, or morality. The assumption is that social bonding often indicates rapport between people that is reflected in their mutual trust. Cooperation and, especially, risky cooperation where people need to invest in the relationship to gain larger mutual gains is critically dependent on mutual trust. Likewise, being moral, that is, adhering to a group's moral norms, can usually be equated with being trustworthy. In this commentary, I will use these three terms interchangeably so that I can draw broader conclusions from the presented studies. Illustrating the practical application of the conceptual and methodological approaches sketched above, this convolute proceeds in the following order of four clusters of studies.

In the first cluster, my co-authors and I investigate low-level behavioral mechanisms that may facilitate trust and are often found in religious systems in the form of collective rituals (but these behaviors are not themselves religious). That is, these mechanisms are also found in secular systems, usually fulfilling the same function. In the second cluster of studies, we investigate the behavioral and perceptual mechanisms facilitating interpersonal trust, specifically in religious systems. In other words, how practices with religious content may promote trustworthy exchange. In the third cluster, we investigated the effects of the quintessential aspect of religious systems—belief in supernatural agents—on trustworthy cooperative exchange. Finally, in the fourth cluster, we investigate how religious systems afford the communication of trustworthiness (rather than how the systems create trustworthiness as in previous clusters), allowing trustworthy people to find each other and cooperate for mutual benefits.

3.1. Cluster #1

The first cluster is inspired by the notion that when we decompose a religious system, its low-level building blocks do not need to be religious (e.g., cognitive mechanisms). Thus, even though we are ultimately asking about the function of religious systems (and will proceed to higher levels of complexity in the next clusters), we can start our investigation with a simple observation that religions share many social-bonding mechanisms with other social activities such as sports, military, or music-making (Dunbar et al., 2012; Newson et al., 2018; Pearce et al., 2015). One prominent behavioral mechanism that increases interpersonal trust observed across different contexts is movement mirroring and, in a more organized way, synchronous movements (Hove & Risen, 2009; Müller et al., 2012).

Mirroring is defined here as imitating others' movements, postures, gestures, or verbal expressions. The purported effects of mirroring on trust may be facilitated by perceptual mechanisms that assess the kin-membership based on visual similarity and experience (DeBruine, 2002). For example, researchers who purposefully imitated participants were more likely to receive help compared to researchers who did not imitate (Müller et al., 2012). Participants are also more likely to imitate the behaviors of their ingroup members compared to the behavior of outgroup members (Likowski et al., 2008; Yabar et al., 2006). Synchrony, on the other hand, is defined as the purposeful matching of movements performed at a phase-locked rhythm. Comparing participants who together engaged in this rhythmical movement matching with participants moving in asynchrony, previous research showed that synchronized groups were more cooperative in economic games (Reddish et al., 2013; Wiltermuth & Heath, 2009). Importantly, as mentioned above, these synchronous performances are often part of cultural ceremonies and rituals (e.g., marching army), and religious rituals are no exception. For example, Catholic mass participants regularly stand, kneel, sit together, chant in the same rhythm, or pray together. Other religious systems may involve synchronous music-making (e.g., drumming) or dancing, both behaviors being ethnographically well-documented (Curran, 2010; Deloria, 1929; Jochelson, 1910).

However, if we are to argue that religious systems naturally use mirroring and synchronous movements to increase trust between their members, we first need to show that increased pressure on cooperation increases the uptake of synchronous activities and that, in turn, these activities increase interpersonal trust (for a real-world example with ritual behavior see Henrich et al., 2019). Translating this causal chain into laboratory studies, we can utilize the fact that mirroring is often subconscious (or not consciously initiated) and synchrony a conscious goal-oriented activity. Using mirroring, we can investigate whether external pressures on cooperation naturally increase imitative activities in the laboratory (without conscious decision), which would not be feasible with synchrony that usually needs to be pre-scribed. Yet, by pre-scribing synchrony (or not), we can then observe whether these naturally occurring movements may increase trustworthiness (and through which channels).

The former part of the causal chain was investigated in Study 1 of this convolute (Lang, Xygalatas, et al., 2022). Harnessing human cross-cultural variability as argued above, we

conducted the same experiment across six different countries with each country representing one continent. In each country, we invited participants in groups of four into a laboratory space and asked them to select one person from a list of candidates to represent their country at an international conference related to various threats. The group had 20 minutes to discuss a suitable candidate and write down the reasons for their choice on a whiteboard. To facilitate discussion, we used the Hidden Profiles task, where each participant had a unique piece of information about the candidates (the best group choice could therefore be reached only by information sharing). Crucially, we manipulated the topic of the conference or, more specifically, the nature of the threat on which the conference was focused. We used a between-subjects design with three conditions: an outgroup threat (terrorist attack), a natural disaster threat (earthquake), and a control group (a conference about an unspecified topic). During the discussion period, we assessed the rate of movement mirroring between participants with the help of Sociometric Badges that use accelerometers and Bluetooth detections to index participants' relative positions and movement. Using these devices, we were able to test whether a threat spontaneously increased the amount of movement mirroring, reflecting the need for increased cooperation in threatening situations.

We found that movement mirroring was larger in the condition comprising the natural disaster threat compared to the control condition, while the same effect was not observed for the comparison of the outgroup threat and control conditions. Nevertheless, when we interacted condition with gender, we found that the outgroup threat increased movement mirroring in men but not in women, indicating that natural disaster galvanized both sexes while outgroup threat only men. The latter result agrees with previous findings (Yuki & Yokota, 2009), suggesting that the mirroring mechanism is indeed activated by threat, but this activation is context-specific. Notably, our results also varied across studied countries, a finding that we discuss in more detail in the full article.

The second part of the causal chain can be tested by directly manipulating synchronous movements. Two studies in the convolute represent this testing. In Study 2 (Lang et al., 2017), we invited participants individually into a laboratory and asked them to synchronize with another participant through a video transmission projected on a wall. However, in reality, the second “participant” was a videorecording of our confederate, where we manipulated the amount of synchronization. In the high-sync condition, the confederate performed all the prescribed movements with exact timing such that participants easily matched these movements and were in synchrony. In the low-sync condition, the confederate’s movement timing was sometimes shifted, resulting in periods of low synchrony between participants performing the pre-scribed movements and the confederate. Finally, in the control condition, participants performed the same movements as in the other two conditions, but there was no video transmission, that is, no social aspect of the performance.

After engaging in the motor exercise, participants were asked about the likeability of the other performer and played a trust game with him (in the control condition, participants played the trust game with another anonymous participant). In the trust game, participants are endowed with a sum of money from which they can send a portion to the second player. Whatever they

send is tripled and received by the second player, who can decide whether to return a portion of this tripled sum to the original sender. If players trust each other, they can earn the most when the first player sends the full amount, which gets tripled, and the second player sends half back. However, if they do not trust each other, the first player should send nothing because if they would send everything, the second player might not reciprocate. Our study observed that participants in the high-sync condition sent the largest portion of their endowment, corresponding to the fact that synchronous behavior elicited trust. The confederate was also rated as the most likable in the high-sync condition.

Study 3 focused on whether the effects of synchrony on trustworthy behavior would lead to a social preference for synchronized comrades or a generally elevated trustworthiness toward everyone (Chvaja et al., 2020). We used the same protocol to elicit synchrony as in the previous study (Lang et al., 2017), but rather than asking participants to play the trust game with the confederate, we asked them to judge the immoral behavior of the confederate. Specifically, after the synchronization period, participants watched the confederate leave a more demanding task to other participants, despite being selected for it. In the high-sync condition, participants judged this behavior as less immoral, suggesting that while synchrony binds people together, it does so only in the circle of performers and possibly at the expense of outsiders.

Together, these studies suggest that imitation is recruited as a natural social bonding activity under pressure for group cooperation and that these activities have positive effects on interpersonal trust. This causal chain help us why religious rituals and ceremonies often recruit synchronous movements as a means to bond people together and increase their mutual trust, effectively achieving larger cooperative benefits. However, an important qualifier of this effect is that the effects of synchrony are bound to the community, possibly at the expense of trust toward outsiders. In other words, the effects are parochial and do not necessarily transfer beyond the performing group (c.f., Reddish et al., 2014, 2016).

3.2. Cluster #2

While the first cluster of studies was dedicated to low-level behavioral mechanisms recruited by religious systems but not themselves religious, the second cluster of studies is focused on mechanisms that are often associated with religious rituals. These mechanisms interact with religious norms that regulate interpersonal conduct to internalize these norms, effectively making people trustworthy.

The first study in this cluster—Study 4—examines how religious ritual affects the perception of moral norms as objectively existing (Chvaja et al., 2022). The stability of moral norms is crucial for any society because a quick norm turnaround (e.g., during revolutionary times) leaves people living in uncertainty, not knowing whom to trust. Religious norms are exceptional in this respect because they often evoke the sanctity of norms, shrouding them in a veil of eternal and objective existence. In other words, religious norms are sanctified by gods and independent of human will. When moral norms are thought to exist objectively, they

are less likely to be doubted, and people are more motivated to follow them (Chvaja, forthcoming).

How does religious ritual affect the perception of moral norms as objectively existing? We argued that this perception is affected by the specific aspects of ritual behavior, defined as the rigid and repetitive performance of acts and utterances not encoded by the performers (Rappaport, 1999). The fact that rituals must be performed is crucial here because religious norms are materialized in the acts and utterances of the performers. For example, ritually swearing an oath to each other materializes the existence of the oath to a larger extent than if the oath would be only present in individual minds. Moreover, the performance of the oath makes it clear who took it and who did not, forcing people to clearly express their commitment to a specific moral norm. Likewise, the rigidity and repetitiveness of rituals, that is, the fact that they are performed in the same manner across generations, may additionally increase the perception of the eternality of moral norms.

To test these hypotheses, we conducted six survey studies across three different populations, focusing on the frequency of ritual performance and participants' perceptions of moral norms. We found that the more participants attended collective rituals, the more they perceived moral norms as objectively existing. Furthermore, we also designed a nuanced scale that directly addressed the various ritual aspects and their effects on moral objectivity (e.g., how much rituals materialize norms), finding that higher scores on this scale were associated with moral objectivity. Importantly, we observed an interaction between ritual frequency and ritual aspects, suggesting that moral objectivity was the highest for participants who perceived rituals as rigid, repetitive, and materializing abstract norms and who, at the same time, attended rituals frequently.

In the second study in this cluster—Study 5—we examined whether moral norms regulating interpersonal trust might be evoked by reminders of ritual performance that encoded these norms (Nichols et al., 2020). Across three countries, we identified relevant musical stimuli that are associated with rituals (e.g., the Ave Maria melody in the Czech sample) and selected comparable secular music as control conditions. We invited participants individually into a laboratory and asked them to engage in a "dots task." In this task, participants are presented with a screen divided into two halves and each half contains several dots. Participants' task is to select the side of the screen with more dots (over 100 trials), and participants are paid for their selection. Crucially, one side is always more rewarding than the other (independent of the number of dots), so participants are incentivized to report the more rewarding side.

In a between-subjects design, participants listened to either a religious song, secular song, white noise, or no sound during this task. We found no difference between conditions in the amount of cheating when looking at the full sample. However, interacting condition with religious affiliation, we saw that the religious stimuli decreased the amount of cheating in religious participants. This result is in line with the predicted conditioning effects of religious rituals: only people who associated religious music with specific religious norms decreased

their cheating. Logically, religious stimuli did not affect secular participants, who do not necessarily associate religious music with particular normative behavior.

3.3. Cluster #3

The third cluster of studies is focused on examining the quintessential aspect of religions—belief in supernatural agents—and its effect on trustworthiness. This investigation was motivated by the world-wide recurrence of certain aspects of belief in supernatural agents, namely, their interest in human interpersonal conduct and mandates of its fairness. Across the world's most populous religions, gods are often believed to care about how humans treat each other, mandate certain behaviors and/or adhere to norms that regulate interpersonal conduct, and, importantly, punish transgression of these norms. For example, the Ten Commandments, believed to be set forth by the Christian God, summarize the essential normative appeal of the Christian community with specific instructions on how (not) to treat other people. Importantly, Christian God is believed to be omniscient and omnipotent, meaning that he can observe people's adherence to the Ten Commandments and punish them for misbehavior directly in their lives or in the afterlife. This makes the Christian god a moralizing god, that is, a god who can observe and punish human interpersonal conduct.

Interestingly, previous studies working with ethnographic data suggested that the presence of belief in moralizing gods is associated with a certain level of social complexity, indicating that this belief appears mostly in large-scale societies (Botero et al., 2014; but see Lightner et al., 2022). This observation was in line with a cluster of influential theories suggesting that belief in moralizing gods evolved at the onset of the Holocene as a solution to societal pressures on cooperation with anonymous people (Norenzayan, 2013; Norenzayan et al., 2016). With the arrival of agriculture, permanent settlement, and the growth of human societies, people started to interact with anonymous members of their societies regularly but could not rely on mechanisms facilitating trust in small-scale societies (e.g., reciprocity or reputation). Supernatural agents who were believed to care about human conduct and norm adherence as well as believed to have the ability to punish norm trespassing might have solved this pressure because the potentially high penalty deterred believers from norm trespassing (e.g., eternal punishment in the afterlife). Believing in moralizing gods and knowing that other people also believe in the same gods may have stabilized cooperation among anonymous co-religionists, further promoting societal growth (for a further discussion, see Beheim et al., 2021).

To test this theory, in Study 6 (Lang et al., 2019), we recruited over 2 000 participants from 15 societies who differed in their religious beliefs (from local ancestral beliefs to world religions), subsistence (from hunter-gatherers to industrialized societies), as well as geographical location (from South America, over Africa, to Oceania). In each site, we selected a moralizing god that cared about human behavior and punished misbehavior, and a local god who was not interested in human interpersonal conduct. Participants were individually invited into a secluded area where they played two types of economic games – a Random Allocation Game (RAG) and a Dictator Game (DG).

In RAG, participants are endowed with 30 coins, a binary die (e.g., black and white), and two cups. They are instructed to think of a rule associating one particular die color with a particular cup, then 30 times roll the die and allocate coins to cups based on the die rolls. Importantly, the coins in each cup are intended for different recipients, and participants play this game alone, so they can allocate their money according to their will, potentially ignoring the die roll results. DG is much simpler than that. Participants are endowed with ten coins and can allocate the coins between the two cups however they want.

Importantly, we used different labels on these cups across the iterations of the RAG and DG. Participants played with cups labeled as Self vs. Distant Co-religionists, Local Co-religionist vs. Distant Co-religionist, Self vs. Outgroup, and Distant Co-religionists vs. Outgroup. For the purpose of the game, coins in the Self cup were delivered to the participant, coins in the Local Co-religionist cup to the participant's religious community in their village, coins in the Distant cup to a religious community matching the participant's affiliation but located in a distant anonymous village, and outgroup to a distant anonymous community with a different religious affiliation. Selecting these different cups was motivated by examining whether belief in moralizing gods helps extend cooperation to anonymous co-religionists (distant co-religionist cup) but not to outgroups (outgroup cup), as predicted by the theory on the evolution of moralizing gods (Norenzayan et al., 2016).

Our results mostly supported the theory. Across the 15 societies, the more people believed that a god monitors how people treat each other and can punish maltreatment, the more they contributed to the distant co-religionist cup in both the RAG and DG. In other words, they treated distant co-religionists more fairly rather than preferring themselves or their own local community. Interestingly, the same effects were not observed for the belief that god is loving and rewarding, suggesting that punishment and monitoring are the main mechanisms through which belief in supernatural agents affects trustworthiness. The effects of belief in moralizing gods on allocations to outgroups were quite variable across our sites and did not allow us to discern any systematic pattern, except for a speculative result that Christian sites, on average, contributed more to the outgroup cups. However, compared to defining the relationship to distant co-religionists, which was rather systematic across sites, finding a similar level of relationship to outgroups proved more difficult despite our efforts (e.g., at some sites, outgroups were directly in conflict over resources, at others working together).

The second study in this cluster—Study 7—examined the explanatory potential of identity fusion in human cooperation across eight different field sites (Purzycki & Lang, 2019). Using the same dataset as in the previous study (but abbreviated only to eight sites), we utilized the fact that we measured participants' identity fusion with co-religionists and outgroups to explore whether the concept of identity fusion can capture meaningful variation in participants' behavior (most of the previous evidence was self-reported). Identity fusion has been described as a visceral feeling of oneness with a group where personal and group identities intercept each other (Swann et al., 2010, 2012). Identity fusion was formulated as a competitive concept to social identity theory (Tajfel & Turner, 1979), where personal identity is absorbed by group identity, leaving group members passive. In contrast, fused individuals

are motivated to act personally on behalf of the group, and this activity may even take the form of self-sacrifice (Atran et al., 2014). Using the visual measure of identity fusion where participants see circles that either do not overlap or overlap to a varying degree (Gómez et al., 2011), we assessed whether fusion with co-religionists predicts behavior toward co-religionists and outgroups in the RAG. Although we observed rather small effects, identity fusion positively predicted contributions to the co-religionist cups (vs. to self), providing needed evidence that the concept of identity fusion tracks real-world behavior.

3.4. Cluster #4

Whereas the thirst three clusters of papers examined how religious behaviors and beliefs generate trustworthiness, the final cluster of studies investigate how religious systems facilitate the communication thereof. In other words, how the communication of one's commitment to a religious tradition indicates a commitment to the tradition's norms that others can rely on. For instance, returning to the example of the *commenda* credit system, how can merchants recognize that their agent is a trustworthy Muslim who will obey laws mandated by Allah? The final two studies in this convolute provide an answer by testing how visual markers of religious affiliation affect trust decisions and how these markers are made especially reliably in risky cooperative dilemmas.

Religious traditions often require their members to sport specific attires, adorn specific visual markers, or keep to specific dietary restrictions. For example, Hasidic Jews are recognizable by their traditional garb: long black suit, a hat, and gartel. Muslim women wear various types of head coverings, from scarfs to burqas and niqabs. Christians often wear a cross hanging on a necklace or rosary beads around their wrists. Jews and Muslims are forbidden to consume pork and Hindus do not eat meat altogether or, at least, avoid beef. All these observable cues indicate membership in a specific religious group as well as subjection to the norms of the given tradition. As such, other co-religionists may recognize the people as trustworthy cooperative partners.

To test this hypothesis, we conducted a study in the multi-religious society of Mauritius (Shaver et al., 2018), where people from different religious traditions interact with each other on a daily basis yet remain relatively segregated (e.g., by restricting marriages mostly to co-religionists). Moreover, religious affiliation is tightly connected to ethnicity: whereas people with Indian ancestry are mostly Hindus, people with African ancestry are traditionally Catholics. We harnessed these natural associations between ethnicity and religious affiliation and prepared a set of photographs of men with both African and Indian ancestries, selecting the most neutral faces. Then, we photoshopped religious markers onto these faces (a necklace with a Christian cross and a Hindu tilak—an ash mark on the forehead).

We invited people of both ethnicities individually into an improvised laboratory and presented them with a cooperative dilemma: using the Trust Game rules as explained above, participants were endowed with a sum of money and were shown ten faces (five Indian and five African)

from which one face adorned the cross and one the tilak. Participants were asked to decide whether they wanted to invest their endowment in any of the faces, expecting some portion of the tripled amount to be returned to them. The results showed that participants with Indian ancestry invested the most money in Indian faces with the tilak and participants with African ancestry in African faces with the cross. We also investigated participants' investments into faces where the religious marker was incongruent with the ethnicity (e.g., Indians wearing the cross), finding that these people would usually be mistrusted. Overall, these results support the hypothesis that religious markers help communicate trustworthiness and are understood as such by co-religionists.

While religious markers seem to be effective in everyday exchange with anonymous individuals, they can also be faked: a person might wear a cross to persuade others of their Christian affiliation without committing to the norms mandated by the Christian God. The potential unreliability of these markers may help us explain why we observe that some affiliation markers are more costly than others (e.g., head scarf vs. niqab), and the same applies to ritual performance (e.g., taking a bus to pray at a pilgrimage site vs. walking the same route on knees). To explain the costliness of some religious practices, we proposed that these practices reliably communicate a commitment to the group (Lang & Kundt, in press). In our model, reliable communication of cooperative intent is facilitated by attaching costs to signal production, as suggested by costly signaling theory (Zahavi & Zahavi, 1999). Originally developed to explain non-human animal communication, this theory postulates that if Person A would benefit from reliably advertising their quality (cooperative intent) and Person B would benefit from receiving this information (e.g., Person B is looking for trustworthy partners), Person A would produce a signal that carries significant costs (e.g., walk on knees). Since individuals with low-quality traits cannot afford to produce the signal, paying the cost of the signal and later garnering the associated benefits of trustworthy exchange is profitable only for individuals with high-quality traits.

Going back to costly religious behavior, previous research showed that costly religious acts were associated with helping others (Power, 2017), and taking part in an extreme ritual positively predicted subsequent anonymous charitable donations (Xygalatas et al., 2013). Costly rituals are also recognized as commitment signals by the signal receivers. For instance, our study on the perceived trustworthiness of pilgrims to Santiago de Compostela (Chvaja et al., forthcoming) showed that religious pilgrims walking longer distances on foot (i.e., sending costlier signals) are trusted more. However, while supportive of the costly signaling theory, this evidence cannot disentangle whether cooperative intentions lead to costly signaling or whether costly signals cause these intentions, as the effort justification theory would have it (Aronson & Mills, 1959).

To this end, we developed an experimental framework for studying costly signals in a laboratory setting (Lang, Chvaja, et al., 2022). Using a sample of 450 Czech participants, we first scored our participants on their cooperative intentions and then let them choose between membership in different groups. The group task was a Public Goods Game (PGG), where each participant receives an initial endowment and can invest any proportion of this

endowment to a shared common pool with three other participants. Any money in this pool is doubled and equally redistributed among the four players. Thus, while everyone's full cooperation would double participants' earnings, keeping the endowment and reaping the benefits of investments of others is an even more profitable strategy. Thus, every participant faces a dilemma of whether to trust others and engage in a collective action to potentially double their endowment or whether the risk that someone will free-ride on the collective effort is too high.

Our participants could choose between two groups: revealed where they would sacrifice part of their endowment to signal that they will contribute large sums to the common pool and concealed where no such signal would be sent. That is, this framework simulated costly signaling as observed in the real world. Importantly, we randomly assigned participants to high cost (signal cost = 15% of their endowment) and low cost (signal cost = 2.5% of their endowment) conditions, expecting that the high cost condition would be more effective in assorting people with cooperative intentions and facilitating cooperation compared to the low cost condition. The result of this testing revealed two critical findings: people with selfish intentions were less likely to choose the costly signal in the high cost condition compared to the low cost condition, and paying the cost to signal cooperative intentions was associated with larger contributions to the common pool. That is, the study provided the first experimental evidence for the functional role of costly signals in facilitating cooperation and complements previous quasi-experimental studies on religious costly signaling, where the signal cost would be unethical to manipulate.

4. Summary and Future Directions

At the beginning of this commentary, I pondered the easiness with which people bestow trust on others and suggested that an explanation should be sought at the interface of human psychology and culture. More specifically, at the interface of psychology and religion. The habilitation thesis presented four clusters of studies that examined this interface from various angels and at various levels of complexity: from low-level behavioral mechanisms to complex religious beliefs nested within particular cultural milieus. Together, these studies showed that human religious practice harnesses mechanisms such as mirroring and synchrony to induce trust, associative learning to instill normative behavior, perceptual mechanisms to make religious norms objective, belief in moralizing gods to increase trustworthiness through the fear of punishment, and specific appearances and behaviors (or ban thereof) to reliably communicate membership, its associative norm compliance, and overall trustworthiness to other co-religionists. I argued that, altogether, these mechanisms are part of a complex adaptive system that facilitates intra-group cooperation and the proliferation of these beliefs and practices. In other words, that religions evolved because they provided group members benefits by stabilizing risky cooperation.

Throughout the presented studies, I heavily relied on a radically interdisciplinary approach. While psychology serves as the unifying approach, our studies utilized the methods and theories from anthropology, behavioral ecology, cultural evolution, economy, evolutionary biology, and religious studies. Likewise, the studies used a variety of data collection methods, from laboratory experiments, over experiments in the field, to surveys and analysis of existing data sets. Together with my co-authors, we experimentally tested more than 4,000 participants in 19 countries across the studies in this convolute. At this point, it is only proper to acknowledge all the help and sincerely thank all my 42 co-authors on the presented studies (see the Attachment for the specification of my contribution to each study). Realizing research into the various facets of complex phenomena is impossible without interdisciplinary approaches, and these approaches are impossible without interdisciplinary teams.

Reflecting on the methodologies used, I would also like to highlight the role of open science practices in my own research as well as its importance for the field of psychological science (and science in general). By meticulously documenting research procedures and sharing these materials along with data and statistical code, this habilitation thesis forms an ecosystem of data and resources that allows others to take up our work and build further extensions and applications. Likewise, open science principles allow others to scrutinize our work and improve on our potential shortcomings, which are inevitable in any scientific endeavor. On top of the open data and open code policies, this habilitation thesis also comprises one registered report (Lang, Chvaja, et al., 2022), where the introduction, methods, hypotheses, and analysis were written up and peer-reviewed before the start of data collection. Albeit not a panacea, such a careful approach may help overcome the “replication crisis” in psychology associated with practices such as p-hacking, harking, etc. (Munafò et al., 2017).

A similar argument can be made about the importance of so-called nonweird populations in psychological research. The acronym WEIRD is a catch-phrase that stands for Western, Educated, Industrialized, Rich, and Democratic: adjectives characterizing most study samples in psychology (Apicella et al., 2020; Henrich et al., 2010). Most studies historically rely on convenient sampling (usually students), assuming that their findings are applicable globally to the totality of the human population. However, as is more and more evident (Blasi et al., 2022; Henrich, 2016), this assumption does not hold, and exploring human psychological diversity is the next challenge for psychological science. Likewise, claiming the generalizability of results should be backed up by a representative global sample. This is not always feasible, as I can personally testify after managing a large cross-cultural project involving 15 different field sites (Lang et al., 2019; Purzycki et al., 2022); perhaps then, our inferences should be more modest and relate only to specific cultural areas.

Despite our best efforts, the conclusions that can be inferred from the studies in the convolute are limited in several aspects and, as is customary in science, can be resolved only by future research. In the remainder of this commentary, I will present several future directions that would help us to better understand the dynamic relationship between human psychology and religion in creating, sustaining, and communicating trustworthiness.

The first future direction direly needed is longitudinal studies assessing the above-mentioned dynamic relationship. All of the studies in this convolute were cross-sectional, and although we often used experimental manipulation to arrive at the causal relationship between two variables, these manipulations are rather artificial. Of course, manipulating actual beliefs and practices (e.g., by forcing people to participate in a ritual or asking them to stop believing in god) is either highly unethical or straightforwardly impossible. Ideally, our experimental studies would be complemented by longitudinal studies where the causal relationship could be observed naturally. For instance, using the longitudinal approach, future research may investigate how changes in religiosity change people's trustworthiness and trustworthiness as perceived by their peers. Indeed, while religiosity if often treated as a stable personality trait predicted by a wide variety of cognitive dispositions (for an overview, see Yilmaz, 2021), such a static view of religious belief is at odds with the process of becoming religious (conversion) or abandoning faith (apostasy), which are by definition dynamical. There is only a handful of longitudinal studies on the dynamics of religious devotion, but all of them suggest substantial variation across the lifespan as well as different individual pathways (e.g., a considerable decrease of religiosity, volatility, or reversed U-shape pattern; Chan et al., 2015; Major-Smith et al., 2023; McCullough et al., 2005). While providing initial supportive evidence for the notion that religious devotion varies across the lifespan, the consequences of this variation on trustworthiness, cooperation, and obedience to moral norms are yet unknown.

The second challenge for future research pertains to understanding the cognitive computations behind trust decisions and how religious beliefs and practices modify these computations. There is no formal model on how religious belief enters cognitive computations during decision-making that would allow explaining this relationship in a more principled way (Jolly

& Chang, 2019). Despite an avid interest of neuroscientists in religion (for an overview, see Schjoedt & van Elk, 2019) and of scholars of religion in cognitive science (for an overview, see C. White, 2021), these research lines are mostly orthogonal and do not use each other's expertise to their full potential. Recognizing the benefits of computational modeling in social cognitive sciences (as evidenced by recent special issues on the topic in relevant journals: (Cushman & Gershman, 2019; Langdon & Schoenbaum, 2021), the time is ripe for developing a computational cognitive model that will simulate the individual subprocesses of the complex mapping from sensory signals to a behavioral response. Such a model should combine formal specifications of (1) how are incoming situational inputs processed (e.g., cooperative dilemma) and (2) categorized based on previous experiences and semantic knowledge encoded in memory (e.g., religious prescriptions) to produce (3) a set of possible weighted actions to facilitate the most rewarding response.

The third challenge relates to the role of religious systems in facilitating interpersonal trust in secularized societies. Writing this habilitation thesis in the Czech Republic—one of the more secularized countries in the world—readers undoubtedly ponder how the arguments presented in this thesis translate into secular societies. A potential explanation may be that cultural systems facilitating cooperation are themselves highly secularized (e.g., police, legislature, courts) and effective enough that the religious facilitation of trust is no longer needed. For example, proponents of the above-mentioned moralizing gods hypothesis (Norenzayan, 2013; Norenzayan et al., 2016) suggested that in the absence of secular institutions regulating cooperation, this role is fulfilled by the belief in punitive and omniscient moralizing gods. However, if societies comprise well-functioning secular institutions, belief in moralizing gods is unnecessarily expensive. This unnecessary expense relates to the transmission mechanism needed to perpetuate the beliefs to subsequent generations: Credibility Enhancing Displays (CREDs). Such displays are costly (e.g., participation in extreme rituals, forgoing mating opportunities) in order to persuade social learners that the model indeed holds these beliefs, and so should they (Henrich, 2009; Norenzayan & Gervais, 2013). Thus, with evolving secular institutions in the West, religious CREDs are seen as less beneficial, and their lack effectively decreases the transmission of religious beliefs (Lanman, 2012b). Future research should therefore investigate whether religious and secular people in these societies are trustworthy to similar levels and whether secular institutions facilitate this trust.

All these three future directions are currently in various stages of preparation by myself and my colleagues and should provide further insights into the complex relationship between trust and cultural institutions. Together, these insights will primarily inform scholars in psychology and related disciplines but will have an interdisciplinary impact on researchers interested in human evolution and economics. Furthermore, understanding factors affecting interpersonal trust is crucial because interpersonal trust is the key building block of long-lasting democratic regimes (Sullivan & Transue, 1999). Examining factors affecting interpersonal trust is even more important in the current times characterized by the plurality of sources, prevalence of misinformation and disinformation, as well as by shifting human communication into the online world. Finally, while most of the research in this habilitation thesis related to the question of how religious systems build within-group trust and co-operation, some of the

research also touched on between-group relations. As with the importance of trust for democratic regimes, understanding how religious systems may enhance within-group trust at the expense of outgroup hostility is of utmost importance in explaining phenomena such as religiously motivated wars but also handling foreign war refugees and generally living in multi-ethnic and multi-religious societies.

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6. Annexes

6.1. Study 1

Lang, M., Xygalatas, D., Kavanagh, C. M., Boccardi, N., Halberstadt, J., Jackson, C., Martínez, M., Reddish, P., Tong, E. M. W., Vázquez, A., Whitehouse, H., Yamamoto, M. E., Yuki, M., & Gomez, A. (2022). Outgroup threat and the emergence of cohesive groups: A cross-cultural examination. Group Processes and Intergroup Relations, 25(7), 1739–1759. https://doi.org/10.1177/13684302211016961

Abstract

Evolutionary models and empirical evidence suggest that outgroup threat is one of the strongest factors inducing group cohesion; however, little is known about the process of forming such cohesive groups. We investigated how outgroup threat galvanizes individuals to affiliate with others to form engaged units that are willing to act on behalf of their in-group. A total of 864 participants from six countries were randomly assigned to an outgroup threat, environmental threat, or no-threat condition. We measured the process of group formation through physical proximity and movement mirroring along with activity toward threat resolution, and found that outgroup threat induced activity and heightened mirroring in males. We also observed higher mirroring and proximity in participants who perceived the outgroup threat as a real danger, albeit the latter results were imprecisely estimated. Together, these findings help understand how sharing subtle behavioral cues influences collaborative aggregation of people under threat.

Contributions

Conceptualization	40%
Methodology	60%
Data collection	30%
Data curation	90%
Statistical analysis	100%
Supervision	90%
Writing and editing	90%
Project administration	80%

6.2. Study 2

Lang, M., Bahna, V., Shaver, J. H., Reddish, P., & Xygalatas, D. (2017). Sync to link: Endorphin-mediated synchrony effects on cooperation. Biological Psychology, 127, 191–197. https://doi.org/10.1016/j.biopsycho.2017.06.001

Abstract

Behavioural synchronization has been shown to facilitate social bonding and cooperation but the mechanisms through which such effects are attained are poorly understood. In the current study, participants interacted with a pre-recorded confederate who exhibited different rates of synchrony, and we investigated three mechanisms for the effects of synchrony on likeability and trusting behaviour: self-other overlap, perceived cooperation, and opioid system activation measured via pain threshold. We show that engaging in highly synchronous behaviour activates all three mechanisms, and that these mechanisms mediate the effects of synchrony on liking and investment in a Trust Game. Specifically, self-other overlap and perceived cooperation mediated the effects of synchrony on interpersonal liking, while behavioural trust was mediated only by change in pain threshold. These results suggest that there are multiple compatible pathways through which synchrony influences social attitudes, but endogenous opioid system activation, such as endorphin release, might be important in facilitating economic cooperation.

Contributions

Conceptualization	40%
Methodology	60%
Data collection	10%
Data curation	100%
Statistical analysis	100%
Supervision	50%
Writing and editing	90%
Project administration	40%

6.3. Study 3

Chvaja, R., Kundt, R., & Lang, M. (2020). The effects of synchrony on group moral hypocrisy. Frontiers in Psychology, 11, 544589. https://doi.org/10.3389/fpsyg.2020.544589

Abstract
Humans have evolved various social behaviors such as interpersonal motor synchrony (i.e., matching movements in time), play and sport or religious ritual that bolster group cohesion and facilitate cooperation. While important for small communities, the face-to-face nature of such technologies makes them infeasible in large-scale societies where risky cooperation between anonymous individuals must be enforced through moral judgment and, ultimately, altruistic punishment. However, the unbiased applicability of group norms is often jeopardized by moral hypocrisy, i.e., the application of moral norms in favor of closer subgroup members such as key socioeconomic partners and kin. We investigated whether social behaviors that facilitate close ties between people also promote moral hypocrisy that may hamper large-scale group functioning. We recruited 129 student subjects that either interacted with a confederate in the high synchrony or low synchrony conditions or performed movements alone. Subsequently, participants judged a moral transgression committed by the confederate toward another anonymous student. The results showed that highly synchronized participants judged the confederate's transgression less harshly than the participants in the other two conditions and that this effect was mediated by the perception of group unity with the confederate. We argue that for synchrony to amplify group identity in large-scale societies, it needs to be properly integrated with morally compelling group symbols that accentuate the group's overarching identity (such as in religious worship or military parade). Without such contextualization, synchrony may create bonded subgroups that amplify local preferences rather than impartial and wide application of moral norms.

Contributions

Conceptualization	30%
Methodology	30%
Data collection	0%
Data curation	0%
Statistical analysis	30%
Supervision	70%
Writing and editing	40%
Project administration	0%

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The Effects of Synchrony on Group Moral Hypocrisy

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Humans have evolved various social behaviors such as interpersonal motor synchrony (i.e., matching movements in time), play and sport or religious ritual that bolster group cohesion and facilitate cooperation. While important for small communities, the face-to-face nature of such technologies makes them infeasible in large-scale societies where risky cooperation between anonymous individuals must be enforced through moral judgment and, ultimately, altruistic punishment. However, the unbiased applicability of group norms is often jeopardized by moral hypocrisy, i.e., the application of moral norms in favor of closer subgroup members such as key socioeconomic partners and kin. We investigated whether social behaviors that facilitate close ties between people also promote moral hypocrisy that may hamper large-scale group functioning. We recruited 129 student subjects that either interacted with a confederate in the high synchrony or low synchrony conditions or performed movements alone. Subsequently, participants judged a moral transgression committed by the confederate toward another anonymous student. The results showed that highly synchronized participants judged the confederate's transgression less harshly than the participants in the other two conditions and that this effect was mediated by the perception of group unity with the confederate. We argue that for synchrony to amplify group identity in large-scale societies, it needs to be properly integrated with morally compelling group symbols that accentuate the group's overarching identity (such as in religious worship or military parade). Without such contextualization, synchrony may create bonded subgroups that amplify local preferences rather than impartial and wide application of moral norms.

Keywords: group unity, moral judgment, moral hypocrisy, social bonding, synchrony, cooperation

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INTRODUCTION

Morality as a package of psychological and cultural adaptations has evolved to stabilize risky collective action among genetically unrelated individuals (Alexander, 1987; Greene, 2013). Group moral codes are reflected in norms that regulate access to resources, inter-personal conduct, and group defense. Breaching these norms triggers moral judgment which is reflected in a cascade of emotional responses such as anger or disgust with the delinquent and sympathy with victims (Haidt, 2013). This emotional response, in turn, motivates people to act against norm transgressors by imposing punishment for what they deem immoral behavior, thereby effectively stabilizing norm-regulated coordinative and cooperative efforts (Boyd et al., 2003; Henrich et al., 2006). In other words, moral judgment and its associated emotions serve as necessary mechanisms that facilitate group functioning by supporting normative structures that regulate social interactions.

Aside from moral judgments, cooperation in collective action is facilitated by additional social technologies,[1] which help create in-group unity and cement group bonds between unrelated individuals, such as play and sports, dancing and music-making, or religious rituals (Kirschner and Tomasello, 2010; Tarr et al., 2016; Hobson et al., 2018). Ample research focused on disentangling the specific elements of such social technologies that facilitate the bonding effects, pointing to the positive effects of laughter (Dunbar et al., 2012), shared painful experiences (Konvalinka et al., 2011; Bastian et al., 2014), or synchronous movement and vocalization (Reddish et al., 2013b; Lang et al., 2015; Weinstein et al., 2016). Together, these bonding behaviors provide a powerful social glue that can surpass genetic relatedness (Hill et al., 2014; Whitehouse et al., 2014), effectively creating groups of committed individuals.

However, whereas morality and social bonding technologies amplify each other in small and tight-knit communities, in large-scale societies that depend on cooperation between anonymous and unrelated individuals, the relational sub-groupings created by face-to-face bonding technologies may hamper impartial application of social norms (Lang et al., 2019; Schulz et al., 2019). While breaching social norms elicits demand for punishment, the severity of this punishment and associated moral outrage may differ based on whether the transgressor is an anonymous unrelated individual, an individual from a competing subgroup, a close friend, or kin. This phenomenon, labeled as moral hypocrisy, describes the "double moral standards" that people often apply when judging others' and one's own behavior (Valdesolo and DeSteno, 2008; Lammers et al., 2010; Polman and Ruttan, 2012), including the discrepancy between stated prescriptions and an individual's actual behavior (Batson et al., 1999; Batson and Thompson, 2001).

Importantly, Valdesolo and DeSteno (2007) introduced the term "group moral hypocrisy," which describes the application of double moral standards according to group membership of the perpetrator: in their experiment, the participants judged the same moral transgression against the "next subject" less strictly when the transgressor was a member of their in-group compared to an out-group member (cf., Gneezy and Fessler, 2012; also see the discussion section for why humans sometimes punish in-groups more than out-groups). Further research showed that participants punish low offers to a third in-group player in the dictator game less strictly when the "dictator" belongs to their in-group (Bernhard et al., 2006) or that juries are more likely to enact harsher punishments when the defendants are of a different ethnicity (Hymes et al., 1993; Sommers, 2007).

The outstanding question is whether relational structuring facilitated by social technologies would also support group moral hypocrisy? In other words, if social technologies merely create locally bonded groups, these groups may fail to apply moral norms impartially and treat conspecifics preferentially when judging their moral transgressions. While there has been ample research on the positive effects of social-bonding technologies on increased cooperation and coordination on the local level (reviewed below), it is unclear whether this increased bonding also leads to moral hypocrisy, which may erode the functioning of large-scale societies comprised of anonymous individuals that rely on impartially enforced normative structures. To answer this outstanding question, we focus on one of the well-researched social bonding technologies—interpersonal motor synchrony—and investigate whether synchrony promotes moral hypocrisy of the locally bonded group.

Behavioral synchrony, i.e., matching each other's movements in time, was shown to promote a vast array of local prosocial effects ranging from perceptual changes to cooperation. Specifically, performing synchronous movements increases perceptions of mutual similarity (Rabinowitch and Knafo-Noam, 2015), group unity (Lakens, 2010; Paladino et al., 2010; Tarr et al., 2014), social rapport (Miles et al., 2009), and sympathy between the members of the synchronized group (Hove and Risen, 2009). On the behavioral level, synchronizing with other participants elicits trust-based cooperative exchange, which translates into greater cooperation in various economic games (Wiltermuth and Heath, 2009; Fischer et al., 2013; Sullivan et al., 2015; Lang et al., 2017) and even altruistic acts in real-life situations (Valdesolo and DeSteno, 2011; Cirelli et al., 2017). For a more extensive overview of these effects, see two meta-analyses (Rennung and Göritz, 2016; Mogan et al., 2017). In summary, this research suggests that synchrony indeed strengthens social bonds at the local level, that is, between the performers.

However, there is some evidence that synchronization may also promote behaviors that may be harmful to members of other groups. For instance, compared to the asynchronous and control conditions, participants in the synchrony condition were more likely to comply with a request from another synchronized participant (confederate) to administer an unpleasant blast of noise to a member of another team (Wiltermuth, 2012). That is, participants collaborated with their synchronized partners even though the prompted behavior might be considered immoral or aggressive. Nevertheless, it is not clear how synchrony affects intra-group relations, especially in large groups where the impartial application of moral norms is crucial for the stabilization of large-scale cooperation. In other words, while there is ample evidence supporting the notion that synchrony creates local bonds, it is not clear how these local bonds affect impartial application and enforcing of moral norms and whether they support moral hypocrisy.

To this end, we designed a between-subject study where we first manipulated experienced synchrony by asking participants to engage in a movement task either with another coordination partner (high-sync and low-sync conditions) or alone in front of a blank wall. In the former conditions, the participants were asked to synchronize with another participant through a live transmission projected on a wall. This transmission was, in fact, a pre-recorded video with a confederate where the confederate

[1]We are using the term "social technology" in the same way as Fischer and Xygalatas (2014) to refer to behavioral patterns deeply rooted in human evolutionary history that provide solutions to the problem of cooperation. Note that these behaviors operate on a bio-social level. For example, religious rituals are transmitted socially but they activate certain biological and psychological reactions that affect human cooperation (e.g., Charles et al., 2020).

was either in high-synchrony with the participants or in low-synchrony. After the synchrony manipulation, participants filled out a post-manipulation questionnaire that assessed social bonding with the confederate. Then, in an ostensibly separated session, participants were asked to help the researcher to evaluate the effectiveness of an unrelated stimulus for another study. Participants watched through a video-transmission how their synchronization partner (confederate) participates in an unrelated experiment where he commits a moral transgression against another anonymous student, and participants were asked about the fairness of such behavior.

Based on the theoretical foundations laid out above, we predicted that the participants in the high-synchrony condition would judge the moral transgression as less unfair than the participants in the low-synchrony and control conditions. Apart from this basic prediction, we also identified five potential mediators of the purported synchrony effect, namely perceived group unity, similarity, sympathy, and perceived cooperation, and explored whether moral hypocrisy is facilitated specifically by some of these mediators.

These particular mediators were chosen on the basis of their importance in the strengthening of ties between individuals as well as their previously reported association with synchrony. Therefore, these mediators were expected to facilitate the anticipated bias in the application of moral norms in the high synchrony condition. More specifically, perceived group unity should sharpen the group's boundaries in the high synchrony condition more relative to the low synchrony and control conditions, effectively strengthening the parochial bias of norm application (Choi and Bowles, 2007). Second, similarity and sympathy are rooted in human kin psychology, providing individuals with psychological cues of genetic relatedness (Sigmund, 2009) and again strengthening the parochial bias. Finally, perceived cooperation is related to direct reciprocity (Trivers, 1971) and reputation building (Nowak and Sigmund, 2005). The positive effects of cooperation should, therefore, affect nepotistic cooperation also in other contexts, namely during moral judgment. Investigating these mediators formed an exploratory part of the current study that should suggest venues for future research.

MATERIALS AND METHODS

Participants

Using a sample size from similar studies (e.g., Wiltermuth and Heath, 2009; Reddish et al., 2016; Lang et al., 2017), we recruited 129 participants (82 females, M_{age} = 23.1, SD = 5.4) from the student pool at Masaryk University (subjects participated in exchange for course points which they needed to complete the course) and randomly assigned them to one of the three conditions: high synchrony (16 males, 25 females), low synchrony (18 males, 31 females), and control (13 males, 26 females). Four subjects expressed doubts about the authenticity of the video transmission at the end of the experiment. We decided to retain their data in the analyses presented in the main text because removing the data does not qualitatively affect the results as we show in the **Supplementary Material** (SM),[2] Section S1. All subjects were debriefed after the end of the data collection.

Materials

To manipulate synchrony, we utilized the general procedure and specific videos from Lang et al. (2017). Participants engaged in two[3] 5-min rounds of motor activity to induce the differential levels of synchrony. In the high and low synchrony conditions, the participants were asked to perform a motor task together with a second participant who was located in another room through a live video transmission. The participants in both synchrony conditions were instructed to perform easy hand-movement sequences with a gong sound announcing the start of each movement sequence, and to synchronize those movements with the movements of another participant. In reality, the transmission was a pre-recorded video (see the videos in SM, see footnote text 2), which was designed to accurately manipulate the participants' experience of either high or low synchrony.

To increase the feeling that the video was a real-time transmission, we added a loading sequence to the beginning of the video consisting of a loading symbol and text stating "waiting for the connection" and "waiting for the other party." The confederate in the pre-recorded video was a male in his thirties with a gray square covering his face to reduce the influence of attractiveness and sympathy (for more details, see Lang et al., 2017). In the high synchrony condition, the confederate made no mistakes during the task. In the low synchrony condition, the confederate made systematic errors: (1) the speed of the confederate's hand movements varied; (2) the confederate's reaction time was delayed by 0.1, 0.3, and 0.5 s; and (3) the confederate performed different movements 15 times during each round. In the control (baseline) condition, the participants were instructed to perform the same hand-movement sequences alone in front of a blank canvas (this served as a projection canvas in the other conditions). The control condition was included as a baseline measure to assess whether the potential difference between the two synchrony conditions might be caused by the high synchrony making the moral judgment more lenient or by the low synchrony making the moral judgment harsher.

Measurements

Immediately after this moving task we measured several single-item and multi-item variables such as potential mediators of the hypothesized effect of synchrony on moral hypocrisy and manipulation checks (see the Post-Manipulation Questionnaire in Section S2 in SM, see footnote text 2). These latent variables included: (1) perceived synchronization with the confederate

[2]SM is accessible at osf.io/pfu6e. This link contains raw data in the *.xlsx* format, R scripts in the *.html* format, two examples of video stimuli, and SM document in the *.pdf* format. In the *.pdf* SM document, sections S2 and S3 list survey questions used to obtain control, check, mediation and dependent variables while sections S1, S4, and S5 contain supplementary analyses that are not displayed in the main text.

[3]While Lang et al. (2017) used three rounds of the motor task, we opted to use only two rounds. Lang et al. (2017) were interested in measuring pain threshold as a proxy of β-endorphin release after each round of the coordination task and, therefore, needed more rounds.

(five items, Cronbach's α = 0.90) as a manipulation check[4]; (2) perceived cooperation (six items, Cronbach's α = 0.90), sympathy (three items, Cronbach's α = 0.88), similarity (two items, split-half reliability = 0.81), and group unity (three items, Cronbach's α = 0.84) as potential mediators; and (3) mood (six items, Cronbach's α = 0.86) and the physical and psychological difficulty of the synchronization task (single-item variables) as control variables. All questions were answered on nine-point Likert scales (1 = not at all, 9 = yes, definitely). When answering these questions, participants in the control condition were instructed to imagine a random person from the participant pool and relate their answers to that person. In doing so, participants in the control condition were also exposed to some level of cognitive load similarly as the participants in the synchrony conditions. While participants in the synchrony conditions had to pay attention to the movements of the confederate, participants in the control condition had to mentally project a third person on the wall.

To assess moral hypocrisy, we used a modified version of the task utilized by Valdesolo and DeSteno (2007). Participants were informed that a colleague of the researcher running the current experiment needs feedback on their newly developed application for assigning participants into different conditions. The participants' task was to assess the functionality of the application by watching (via "shared screens") the confederate with whom they have previously synchronized. The piloted study's title was "The impact of music on analytical thinking" and the video was again pre-recorded. It started with the same loading sequence as the synchrony task video, and then proceeded with the synchrony partner answering a few questions related to the musical record (e.g., "Did you find the record boring?") to induce the feeling that the application was being tested in real time. The authenticity of the video was also reinforced by adding the moving confederate's cursor to the screen. After the cursor showed confederate's answers to these questions, the application instructed the confederate to use a randomizer that would allocate the following task either to himself or to the next subject in the experiment. The other subject was described simply as a "next participant" without any further specification to standardize any biases participants might have had. Nevertheless, participants knew that only students from the course may participate in the experiments which implicitly formed the wider in-group of students from the same university. There were two types of tasks allocated: an easy green task containing simple mathematical exercises and lasting approximately 10 min, or a difficult red task full of mathematical equations that would take approximately 40 min.[5] In the video, the confederate used the randomizer and was assigned the difficult task but nonetheless selected the easy task on purpose and left the difficult task to the next participant.

After seeing this video (which they were led to believe was a live video transmission), the participants completed a pen-and-paper[6] questionnaire with several distracting questions regarding the quality of the application. Mixed within these questions was our main dependent variable, a question that assessed the fairness of the confederate's behavior: "Did the participant act fairly?" (1 = not at all, 9 = very much). Note that we chose to ask about fairness because directly mentioning morality would not fit the cover story (see Section S3 in SM for the Feedback Questionnaire, see footnote text 2).

Procedure

After reading the information about the study and signing informed consent, participants were asked to perform two rounds of simple motor activity. All the procedural steps were explained via a pre-programmed application (written in HTML and run online via an internet browser) on a computer that was connected to the online post-manipulation questionnaire. The instructions presented in the application first described the whole procedure, stating that there will be two 5-min rounds of easy hand movements. The participants in the synchrony conditions were told that they will perform these movements together with another participant in a different room while the participants in the control condition were told only that there is another participant doing the same task in a different room. Afterward, the instructions provided participants with a precise description of the first-round of movements accompanied by pictures with our colleague showing how to perform all movements step-by-step. Participants were then given free time to practice. The same procedure followed after the first round, although the movements were slightly different so participants would not get bored. After the second round, the instructions redirected participants to a post-manipulation questionnaire that assessed the mediating variables explained above. After filling out the post-manipulation questionnaire, participants were instructed to knock on the door of an adjacent room. Then, a research assistant invited participants to take part in a pilot testing of another experiment and asked them to give feedback on this procedure, which was

[4]Note that Lang et al. (2017) demonstrated the validity of this manipulation by both directly asking the participants about perceived synchrony and by analyzing the temporal synchronization of participants' hand movements with the video using accelerometers positioned on participants' wrists (i.e., measuring arm movement acceleration). This additional measurement allowed Lang et al. (2017) to assess the extent of actual movement synchronization between participants and the confederate in the high and low sync condition, showing that participants in the high sync condition had no problems to match the confederate's movements (made no mistakes) while the movements in the low sync condition were misplaced mostly due to the confederate's mistakes. To assure that our manipulation had similar validity, we included at least a question about perceived synchronization.

[5]During our pilot study, we utilized the original version of the task used by Valdesolo and DeSteno (2007). In this first version, the confederate was only recommended to use the randomizer with the sentence "participants usually use the randomizer." However, this was not a direct instruction. In the first pilot testing (n = 22) of this version of the task, participants were asked to judge the behavior on a dichotomous scale (Yes/No), then on a nine-point scale, and finally to provide a reason for their decision. Pilot participants often reported that the behavior of the confederate was not unfair because using the randomizer was not obligatory. Thus, we changed the instruction in the video as follows: "For fair assignment, use our randomizer that randomly assigns you the color of the condition that you should be part of." After this modification, we again piloted the new task (n = 9). Participants in this second pilot did not report similar concerns as participants in the first pilot; therefore, we used this version for the present experiment.

[6]The reason to use a paper questionnaire instead of an online survey was to harness all potential means to induce the feeling that providing feedback on the application really was separate from the original experiment where participants answered online surveys.

currently under development. Participants then watched the video with moral transgression and subsequently received a pen and paper with feedback questions. In the final step, participants were asked about their suspicion of the goals of the current experiment ("What do you think the whole experiment was about?") and were thanked for their participation. The whole procedure took about 50 min.

Analysis

All analyses were performed in R, version 3.5.3 (R Core Team, 2019). To analyze the differences in moral judgment (and also other variables) that were recorded as on a 9-point Likert scale, we fitted linear regression models. Only linear models comprising the manipulation-check variables of physical and mental difficulty as dependent variables revealed non-normally distributed residuals. Thus, we re-analyzed these variables using cumulative link models from the package *ordinal* (Christensen, 2019) that are suitable for modeling ordinal data. SM Section S4 (see footnote text 2) includes residual diagnostics of the two linear models with poor fit and also the fit diagnostics of the main model with moral judgment. The Supplementary R code includes residual diagnostics for all models in this manuscript. All fitted models were adjusted for sex because women and men were not distributed evenly across the different conditions. Since all of the self-reported variables were measured on nine-point Likert scales, we report simple effect sizes (unstandardized regression coefficients) rather than standardized effect sizes (Baguley, 2009). For cumulative link models, we also report odds ratios in the table. The mediation analysis was performed using the function *sem* from package *lavaan* (Rosseel, 2012). **Table 1** displays the estimated differences between conditions.

RESULTS

Manipulation Check

To assess whether our manipulation was effective in eliciting differential synchrony levels, we first regressed the perceived synchrony construct on our conditions. Adjusting the model for sex, the participants in the low-sync condition indicated lower levels of perceived synchrony than those in the high-sync condition (b = −3.07, 95% CI [−3.65, −2.48]). Note that because participants in the control condition performed movements alone, we did not include this condition in the analysis of the manipulation check here. However, this analysis was recommended by one of the reviewers in Frontiers in Psychology (see SM Section S5, see footnote text 2).

Furthermore, the mood and perceived physical and mental difficulty of the task was not predicted by the condition. Neither the low-sync condition (b = −0.04, 95% CI [−0.64, 0.56]) nor the control condition (b = −0.01, 95% CI [−0.64, 0.63]) were associated with statistically reliable differences in mood compared to the high-sync condition. The same absence of difference was observed for perceived physical difficulty (low-sync condition: b = −0.06, 95% CI [−0.83, 0.72]; control condition: b = 0.63, 95% CI [−0.17, 1.43]) and mental difficulty (low-sync condition: b = 0.25, 95% CI [−0.54, 1.04]; control condition: b = 0.17, 95% CI [−0.66, 1.00]). Interestingly, women reported that the task was less physically demanding than the men.

Main Analysis

Next, we analyzed the main effect of the condition on moral judgment. The average moral judgment located on a 1–9 scale and anchored by "totally unfair" and "totally fair" was 5.70 (SD = 2.00) in the high-sync condition, 4.82 (SD = 2.13) in the low-sync condition, and 4.54 (SD = 1.64) in the control condition. These differences also showed a stable pattern in the linear regression framework: compared to the high-sync condition, the low-sync condition was associated with the lower fairness rating (b = −0.90, 95% CI [−1.71, −0.09]), as was the control condition (b = −1.20, 95% CI [−2.06, −0.34]). See **Figure 1A** for raw differences and **Figure 1B** for estimated differences between conditions.

Exploratory Mediation Analysis

After detecting the effect of the condition on moral judgment, we proceeded with the mediation analysis, where we first assessed whether the potential mediators were affected by the synchrony treatment. Results displayed in **Table 1** show that all potential mediators were affected by our manipulation. Then, we used AIC model selection to decide which potential mediator variable should be modeled as a mediator using structural equation modeling (SEM). Note that the potential mediators were meaningful only in the high and low synchrony conditions. Thus, we used only data from these two conditions for all the following analyses. We built five models which are displayed in **Table 2**. First model is a reference model with sex as a single predictor. The following four models include respective mediators together with sex. The predictor from the model that has AIC value at least two points lower than the reference model was chosen to be modeled as a mediator using SEM. AIC numbers displayed in **Table 2** suggest that the only suitable variable is perceived group unity between participants and the confederate (the difference between AICs of these models is 4.49).

Therefore, we built a structural equation model with group unity as a mediator. The model showed good fit to the data even when stringent cut-off values were used given the sample size of the present study (RMSEA = 0.00, SRMR = 0.02, CFI = 1.00, TLI = 1.03; see Sivo et al., 2006). The specific estimates from this mediation model are reported in **Figure 2**.

DISCUSSION

In this study, we tested whether interpersonal motor synchrony affects participants' judgment of moral transgression committed by their synchrony partner against another anonymous non-synchronized person. Moreover, we also explored whether this effect would be mediated by psychological mechanisms synchrony is known to promote, namely group unity, perceived cooperation, similarity, and sympathy. We found that participants in the high-synchrony condition judged the same moral transgression committed by their synchrony partner

TABLE 1 | Unstandardized regression slopes with standard errors for the effects of condition on manipulation check variables, moral judgment, and potential mediators.

	Manipulation Check Variables				Moral Judgment	Potential Mediators			
	Perceived synchrony	Mood	Mental difficulty	Physical difficulty		Perceived cooperation	Sympathy	Similarity	Group unity
Predictors	*Estimate*	*Estimate*	*Odds ratio*	*Odds ratio*	*Estimate*	*Estimate*	*Estimate*	*Estimate*	*Estimate*
Intercept	6.48*** (0.53)	6.39*** (0.48)			4.95*** (0.65)	6.47*** (0.53)	6.15*** (0.48)	4.91*** (0.63)	5.71*** (0.60)
Low-sync	−3.07*** (0.29)	−0.04 (0.30)	1.28 (0.40)	0.94 (0.39)	−0.90* (0.41)	−2.91*** (0.29)	−0.62* (0.26)	−1.20*** (0.35)	−2.57*** (0.33)
Control		−0.01 (0.32)	1.18 (0.42)	1.87 (0.41)	−1.20** (0.43)				
Sex: female	−0.19 (0.30)	0.09 (0.26)	1.26 (0.34)	0.46* (0.34)	0.47 (0.36)	−0.05 (0.30)	0.08 (0.27)	−0.34 (0.36)	−0.26 (0.34)
N	90	129	129	129	129	90	90	90	90
R^2/R^2 adjusted	0.557/0.547	0.001/−0.023	0.007	0.066	0.073/0.051	0.537/0.526	0.061/0.039	0.129/0.109	0.413/0.399

Each column represents individual dependent variable while rows show the intercepts (set as the high synchrony condition), and estimated differences between the intercept and the low-synchrony and control conditions. That is, the slopes indicate by how much the dependent variable shifts (on a nine-point Likert scale) when going from the intercept to the low-synchrony and control conditions. For sex, males are the reference category and the estimate is the difference between males and females. Mental difficulty and Physical difficulty were analyzed using cumulative link models. R^2 for these two models represents Nagelkerke pseudo R^2. Moreover, function clm that we used for cumulative link models does not compute intercepts; thus, we do not display intercepts for those two models. The perceived synchrony and mediator models include reduced sample size because they exclude the control condition that had no interacting partner.
$^*p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

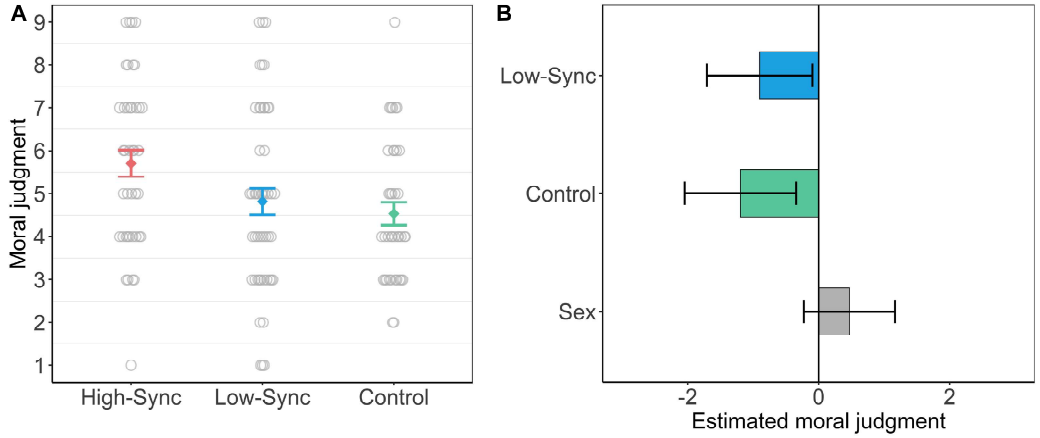

FIGURE 1 | (A) Displays the raw differences in moral judgment between conditions with 95% CI. **(B)** Displays the estimated differences from the intercept (the high-synchrony condition) for the low-synchrony condition and the control condition with 95% CI. Sex shows the difference between males (the intercept) and females.

as less unfair than participants in the low-synchrony and control condition. The results of the mediation analysis suggested that perceived group unity mediated the effect of synchrony on moral judgment, i.e., feeling more united with the synchronized partner led to more lenient judgments.

The results of the present study suggest that a society-wide application of cultural norms may be hampered by social bonding technologies that modify moral judgment based on the perpetrator's sub-group identity. By creating smaller compact groupings within the larger society, bonding technologies such as synchrony may motivate preferential treatment of the bonded partners. We conjecture that this effect is akin to the real-world phenomena such as cronyism and nepotism when a society's resources are preferentially distributed along kith and kin lines (Schulz et al., 2019). While the present study focused specifically on the effects of synchrony on group moral hypocrisy, we expect that similar effects should be observed with other bonding technologies such as extreme rituals (Xygalatas et al., 2013) and political rallies (McNeill, 1995).

Social bonding technologies have been instrumental in the functioning of smaller communities as they facilitate tribal morality (Fischer and Xygalatas, 2014), which is crucial in competition between different groups (Bowles, 2006; Choi and Bowles, 2007). However, the same bonding technologies may be detrimental to the functioning of large-scale societies, unless these technologies are properly integrated within mechanisms that support society-wide norms such as moralizing gods

TABLE 2 | The association between potential mediators and moral judgment with 95% confidence intervals.

	Reference Model	(1)	(2)	(3)	(4)
Predictors	*Estimate*	*Estimate*	*Estimate*	*Estimate*	*Estimate*
Intercept	4.34*** (2.80-5.87)	3.13*** (1.36-4.89)	3.37*** (1.50-5.24)	4.87*** (2.99-6.75)	3.40** (0.85-5.95)
Sex: female	0.55 (−0.36-1.45)	0.63 (−0.25-1.52)	0.57 (−0.33-1.47)	0.50 (−0.41-1.41)	0.54 (−0.37-1.44)
Group unity		0.27* (0.06-0.49)			
Cooperation			0.19 (−0.03-0.41)		
Sympathy				−0.12 (−0.38-0.13)	
Similarity					0.16 (−0.19-0.51)
Observations	90	90	90	90	90
R^2/R^2 adjusted	0.016/0.005	0.084/0.063	0.050/0.028	0.027/0.004	0.025/0.003
AIC	392.741	388.252	391.621	393.768	393.876

Note that the AICs of models 2–4 are not substantially lower than the reference model which means that these models are not substantially more informative than the reference model.
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.*

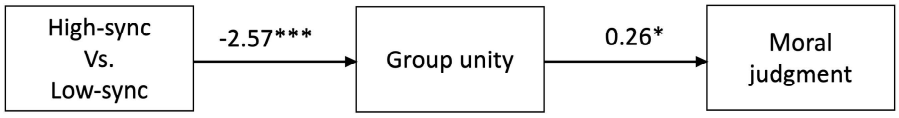

FIGURE 2 | The mediation model. Note that the estimated slopes are different from those displayed in **Table 2** because we did not included sex as covariate in the mediation analysis. Note: *$p < 0.05$, ***$p < 0.001$.

(Purzycki et al., 2016; Lang et al., 2019), state ideologies (McNeill, 1995), and social institutions (Gaechter and Schulz, 2016). While social bonding technologies such as dance or collective rituals likely evolved to facilitate group cohesion within smaller communities of nomadic hunter-gatherers (to promote tribal morality Lang, 2019), these technologies could be scaled up to support larger societies with hierarchical leadership structure by emotionally charging universally shared symbols, norms and institutions (Alcorta and Sosis, 2005; Fischer and Kruekaew, 2019). That is, with appropriate group hierarchy and shared symbols, the locally confined bonding effects of synchrony might be scaled to the society level (e.g., local parishes facilitating adherence to the Roman Catholic Church), effectively supporting an overarching group identity and impartial norm application (Lang et al., 2019); but without such established hierarchy, synchrony may promote biased application of moral norms.

If the conjecture that synchrony supports moral tribalism is correct, then it should be expected that the results of the current study would change according to the perpetrator's and victim's identities. In the current study, participants were informed that their synchrony partner (transgressor) and the third participant (victim) are subjects from the same pool, implying that they are both part of participants' extended in-group; i.e., all of them are students in the same class (but anonymous to each other). Despite this weakly shared general identity, synchrony led to the hypocritical judgment that downplayed the maltreatment of an anonymous student from an extended in-group. Since Wiltermuth (2012) showed that synchrony is also conducive to harming out-group members, we expect that synchrony should facilitate more lenient (and perhaps even approving) judgment of norm transgressions against out-groups. It is safe to assume that if the victim would be of more distant identity such as a student from a different university, we should expect even stronger effects of synchrony on hypocrisy. Indeed, previous cross-cultural research showed that morality is highly parochial and often does not extend beyond group borders (Fessler et al., 2015), unless promoted as a conversion strategy to attract more people into the group (Lang et al., 2019).

Furthermore, if all three actors in the current study would assume the same tightly shared identity such as membership of a soccer hooligans fan club or of a gang, the synchrony effects on moral hypocrisy should disappear or even reverse. This prediction is supported by the black sheep hypothesis, which argues that individuals apply harsher judgments to inappropriate behavior of in-groups because the harmful effects of norm-transgression trickle down, by extension, to all group members (Marques et al., 1988). The reverse effects of synchrony should pan out especially during inter-group conflict when adherence to widely shared norms might be the key factor influencing success in intergroup competition (Richerson et al., 2016). In support of this prediction, Gneezy and Fessler (2012) documented increased altruistic punishment of in-group norm

transgressors during the 2006 Israel–Hezbollah war, and inter-group conflict was shown to bolster equality and equality-promoting norms long after the ceasefire (Bauer et al., 2014; Henrich et al., 2019).

An alternative to the proposed tribalistic effects of synchrony is the generalized prosociality hypothesis, formulated around the empirical findings of Reddish and colleagues (Reddish et al., 2013a, 2016). In their experimental study of the relationship between synchrony and prosociality, Reddish et al. (2013a) found that the synchronized participants were more willing to help another student who did not synchronize with them and sent a larger portion of their monetary endowment to members of an out-group (compared to participants who did a puzzle task together). In another study, Reddish et al. (2016) found that the synchronized (vs. asynchronized) participants were more willing to help an anonymous out-group student whereas there was no difference in willingness to help a member of an extended in-group. These results are in contrast with the present study where we found that, at least in the moral domain, synchrony effects are selective rather than generalized (see also Wiltermuth, 2012). If synchrony would produce generalized prosociality, we should expect greater prosociality toward the victim and harsher judgment of the transgressor in the synchrony condition; or no effect of synchrony at all, depending on the strength of the assumed prosociality. What may account for the differences between the current study and the studies by Reddish et al. (2013a, 2016)?

One possible explanation may be the different manipulations employed across these studies. For instance, while Reddish et al. (2016) let groups of three or four people synchronize (or move in asynchrony) together in the laboratory, our participants were alone in the room and all interactions with the confederate was mediated through a video-transmission. Although a previous study using video-transmission found sizable effects of synchrony on cooperation (Lang et al., 2017), we do not know whether the lack of other people in the room may inhibit the effects of synchronization on generalized prosociality. Furthermore, the generalized prosociality effects of synchrony may be specific to the dependent variables assessed in those studies. While synchrony positively affected monetary contributions to out-groups in Reddish et al. (2013a), the same study failed to find any effects of synchrony on mitigating self-reported preferential biases toward in-groups rather than out-groups. Yet another explanation of these disparate findings could be that synchrony positively affects moral hypocrisy independently of the transgressor's identity (akin to the generalized effects on prosociality). That is, synchronization may reduce moral vigilance such that synchronized participants would judge any norm transgressions as less severe, no matter who committed them. Although this hypothesis requires proper experimental testing, the results of our mediation analysis suggest that this is likely not to be the case.

The mediation analysis revealed that the only mediator was perceived group unity. In the current study, rather than having generalized effects, synchrony affected moral judgment by strengthening the unity between the synchronized partners. As a consequence, the perceived group boundaries between the participant and the victim were sharpened, even though both were members of an extended in-group (students from the same university). Indirect support for this speculation could be drawn from research on the effects of entitativity on prejudices toward out-groups (Gaertner and Schopler, 1998): perceiving one own's group as an entity both correlates with and fosters out-group prejudice (Effron and Knowles, 2015). Nevertheless, this conclusion does not yet explicate why sympathy and similarity were not mediators of the synchrony effect on moral judgment, despite their direct connection to the process of group formation. That is, why did similarity and sympathy not mediate the effects of synchrony on group moral hypocrisy?

The perceptions of similarity and sympathy to others are tightly interwoven and provide subtle cues on genetic kinship (Sigmund, 2009), with objective or even perceived similarity having positive effects on cooperation in economic games and in leader-follower interactions (Krupp et al., 2008; Cornelis et al., 2011). Perhaps, the increased feelings of similarity and sympathy induced by synchrony may have spilled over to the third participant (the victim), and participants also felt closer to the victim, effectively creating a group that included both the synchronized partner and the victim. (Note that participants were first asked about sympathy and similarity to the sync partner and only afterward observed his immoral behavior; hence, they were forced to explicitly think about similarity and sympathy before the transgression task took place). In fact, **Table 2** suggests that similarity was negatively associated with moral judgment, although the effect was not precisely estimated. Returning to the example of inter-group conflict, perceiving that all group members are committed to the same cause (unity) directed against another group, should be sufficient motivation for taking part in the clash, even without necessarily being close (similarity, sympathy) to the other members of the hooligan fan club. Moreover, by emphasizing group boundaries, harming members of the other group might appear as permitted and even desired (cf., Newson et al., 2018).

The final variable with mediating potential that we evaluated, perceived cooperation during the synchronization task, positively predicted moral hypocrisy, although this effect was less precisely estimated than the effect of group unity. Moreover, adding perceived cooperation to the model did not increase the explanatory power of the model compared to the reference model with sex as a single predictor. Whereas perceived cooperation was previously implied as an important mediator of the synchrony effects on prosociality (Reddish et al., 2013b but see Lang et al., 2017), it may have weaker effects on moral judgment because perceived cooperation does not directly influence perceived group boundaries. Nevertheless, the small predictive power of perceived cooperation that we observed might be attributed to the principle of reciprocity (Trivers, 1971). Similar to the effects on prosocial behavior, where perceived cooperation increased the probability that synchronized partners will also cooperate in the following interactions, the participants who scored higher on perceived cooperation may have felt obliged to help their partner (the transgressor) as a form of reciprocal exchange for preceding successful coordination. As

the mediation analysis was only exploratory, future research should test the causal effects of the group unity on group moral hypocrisy. Such an experiment may directly manipulate the perceived group unity and then use the task we used to measure moral hypocrisy.

Apart from the direct test of mediators, we propose that further important insights into the suggested social bonding effects of synchrony might be gained by examining the neuro-hormonal underpinnings of synchrony. For example, while a lot of attention has been paid to the understanding of the β-endorphin's role in mediating prosocial effects of synchrony (Cohen et al., 2010; Sullivan et al., 2014; Tarr et al., 2015, 2017; Launay et al., 2016; Weinstein et al., 2016; Lang et al., 2017), substantially less attention has been devoted to another important social hormone, namely oxytocin. In fact, we are aware of only one study that showed increased levels of oxytocin in reaction to a group singing lesson across professionals and amateurs (Grape et al., 2003) and only one study (Gebauer et al., 2016) that investigated whether oxytocin promotes synchronization. Since oxytocin is often considered to be a parochial hormone that promotes positive behavior toward family members (Galbally et al., 2011) but negative attitudes (De Dreu et al., 2011) and behavior toward out-group members (Zhang et al., 2019), studying whether synchrony increases oxytocin levels is the next logical step in the future synchrony research.

Likewise, to achieve better generalizability and higher validity, the examination of synchrony effects on moral group hypocrisy should move to real environments such as collective religious rituals (e.g., Xygalatas et al., 2013) or football stadiums (e.g., Newson et al., 2018). The real-life conditions might either foster or diminish the laboratory-induced effects of synchrony, depending on other socio-cultural factors such as religious identities (Purzycki et al., 2016; Lang et al., 2019) or the presence or absence of intergroup conflict (Bauer et al., 2016). Moreover, if synchrony evolved as a social technology that helped cement bonds within small communities of nomadic hunter-gatherers during inter-group competition (Choi and Bowles, 2007), we should expect the frequency of group synchronous practices to increase during inter-group conflict, similar to other mechanisms promoting norm adherence (Sosis et al., 2007; Schaub, 2017; Francois et al., 2018; Henrich et al., 2019). This prediction could be tested against data from ethnographic and historical databases, large-scale surveys, or in the laboratory (Miles et al., 2011).

Finally, another extension improving the limitations of the current study would be to measure the ultimate behavioral outcome of moral judgment, namely altruistic punishment (Boyd et al., 2003; Fehr and Fischbacher, 2003) or, alternatively, an action to stop the moral transgression. While we aimed to increase the validity of the current study by adopting a real-world scenario of norm transgression instead of just using vignettes with hypothetical moral transgressions, such a scenario only allowed us to measure moral judgment, which is usually cheap to produce (cf., Saltzstein, 1994). Moral judgment might motivate others to adhere to norms due to reputational sensitivity, however, it remains to be tested whether synchrony also affects more lenient punishments. Therefore, we suggest that future studies should adopt different and more nuanced behavioral measures such as altruistic punishment, helping the victim of the transgressions, or the tendency to copy the immoral behavior.

DATA AVAILABILITY STATEMENT

The dataset generated in this study can be found in online repository at: osf.io/pfu6e.

ETHICS STATEMENT

The study was approved by the Research Ethics Committee at Masaryk University. Participants provided their written consent.

AUTHOR CONTRIBUTIONS

RC and RK developed the research idea. RC designed the study with contributions provided by RK and ML. RC managed the data collection. RC and ML analyzed the data and drafted the manuscript with revision provided by RK. All authors approved the final manuscript.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.544589/full#supplementary-material

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Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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6.4. Study 4

Chvaja, R., Horský, J., Lang, M., & Kundt, R. (2022). Positive association between ritual performance and perceived objectivity of moral norms. The International Journal for the Psychology of Religion, 1–21. https://doi.org/10.1080/10508619.2022.2121454

Abstract

We examined the relationship between religious rituals and how people perceive moral norms. Prominent anthropological theories propose that rituals charge associated moral norms with objectivity such that moral norms are perceived as absolute and independent of time and space. We used two cross-sectional datasets to test this hypothesis and conducted five correlational studies with three culturally distinct populations. The results, supported by meta-analysis of our effect sizes, show a positive association between attending collective religious rituals and perceiving moral norms as objective. Moreover, increased saliency of the characteristic aspects of ritual form, namely the perceived invariance, and digitalizing and materializing potentials, was associated with increased reporting of moral norms as objective. Overall, this manuscript provides initial support for theories suggesting that ritual behavior helps ground moral norms by affecting perceptual mechanisms related to norm processing.

Contributions

Conceptualization	30%
Methodology	30%
Data collection	0%
Data curation	0%
Statistical analysis	20%
Supervision	40%
Writing and editing	30%
Project administration	0%

6.5. Study 5

Nichols, A. D., Lang, M., Kavanagh, C., Kundt, R., Yamada, J., Ariely, D., & Mitkidis, P. (2020). Replicating and extending the effects of auditory religious cues on dishonest behavior. PLoS ONE, 15(8), e0237007. https://doi.org/10.1371/journal.pone.0237007

Abstract

Although scientists agree that replications are critical to the debate on the validity of religious priming research, religious priming replications are scarce. This paper attempts to replicate and extend previously observed effects of religious priming on ethical behavior. We test the effect of religious instrumental music on individuals' ethical behavior with university participants (N =408) in the Czech Republic, Japan, and the US. Participants were randomly assigned to listen tone of three musical tracks (religious, secular, or white noise) or to no music (control) for the duration of decision-making game. Participants were asked to indicate which side of a vertically-bisect ed computer screen contained more dots and, in every trial, indicating that the right side of the screen had more dots earned participants the most money (irrespective of the number of dots). Therefore, participants were able to report dishonestly to earn more money. In agreement with previous research, we did not observe any main effects of condition. However, we were unable to replicate a moderating effect of self-reported religiosity on the effects of religious music on ethical behavior. Nevertheless, further analyses revealed moderating effects for ritual participation and declared religious affiliation congruent with the musical prime. That is, participants affiliated with areligious organization and taking part in rituals cheated significantly less than their peers when listening to religious music. We also observed significant differences in cheating behavior across samples. On average, US participants cheated the most and Czech participants cheated the least. We conclude that normative conduct is, in part, learned through active membership in religious communities and our findings provide further support for religious music as a subtle, moral cue.

Contributions

Conceptualization	40%
Methodology	40%
Data collection	10%
Data curation	50%
Statistical analysis	80%
Supervision	90%
Writing and editing	40%
Project administration	0%

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RESEARCH ARTICLE

Replicating and extending the effects of auditory religious cues on dishonest behavior

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Abstract

Although scientists agree that replications are critical to the debate on the validity of religious priming research, religious priming replications are scarce. This paper attempts to replicate and extend previously observed effects of religious priming on ethical behavior. We test the effect of religious instrumental music on individuals' ethical behavior with university participants (N = 408) in the Czech Republic, Japan, and the US. Participants were randomly assigned to listen to one of three musical tracks (religious, secular, or white noise) or to no music (control) for the duration of a decision-making game. Participants were asked to indicate which side of a vertically-bisected computer screen contained more dots and, in every trial, indicating that the right side of the screen had more dots earned participants the most money (irrespective of the number of dots). Therefore, participants were able to report dishonestly to earn more money. In agreement with previous research, we did not observe any main effects of condition. However, we were unable to replicate a moderating effect of self-reported religiosity on the effects of religious music on ethical behavior. Nevertheless, further analyses revealed moderating effects for ritual participation and declared religious affiliation congruent with the musical prime. That is, participants affiliated with a religious organization and taking part in rituals cheated significantly less than their peers when listening to religious music. We also observed significant differences in cheating behavior across samples. On average, US participants cheated the most and Czech participants cheated the least. We conclude that normative conduct is, in part, learned through active membership in religious communities and our findings provide further support for religious music as a subtle, moral cue.

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Introduction

Religious systems use various emotionally charged symbols to induce individual normative behavior. For example, in Judaism, the sound of the *shofar*(a ram's horn) is a spiritual alarm, indicating it is time for people to repent and ask God for forgiveness. In the Muslim tradition, the call to prayer, *athan*, is a regular reminder to pray and re-establish the norms of communal life that Allah stipulated through his prophet Muhammad. Importantly, previous studies have suggested that such subliminal religious reminders induce prosocial and normative behavior. However, the research documenting the effects of religious priming on prosocial behavior has recently faced substantial criticism [1–3]. To contribute additional empirical data to this ongoing debate, we investigate the effects of religious auditory cues on individual honesty and extend the research literature on religious priming by adapting the methodology employed by Lang and colleagues [4]. Using a more generalizable sample and a less biased cheating task, we replicate important conceptual results observed by Lang and colleagues [4] yet fail to find evidence for the anticipated moderating effect of self-reported religiosity. Our results add important nuance to previous results by investigating how the efficacy of culturally specific religious primes are impacted not only by self-reported religiosity but also by religious socialization, identity, and ritual participation.

Perceptual cues associated with religion have been observed to affect decision-making across many behavioral domains [5–13] and their unconscious effects on ethical behavior have been studied extensively [1,14,15]. One particular behavioral domain studied in relation to religious cues is cheating. As an example, Aveyard [16] conducted two experiments investigating the effects of religious cues on ethical behavior in Middle Eastern participants. In the first experiment, participants unscrambled Arabic sentences with embedded religious or non-religious themes prior to taking a math test. The math test was incentivized, but unsupervised- so crucially, participants were able to dishonestly report their performance to earn more money. Aveyard [16]observed that participants' honesty was unaffected by their exposure to religious (or non-religious) anagrams. However, in a second experiment, listening to the Islamic call to prayer, *athan*, was observed to impact reporting in the unsupervised math test. Specifically, participants that listened to the Islamic call to prayer were more honest than the control participants who did not listen to the call to prayer. These results indicate that auditory religious primes can affect individual behavior. Further, Aveyard's findings suggest that individuals must have deep and natural associations with the sacred cue in order for it to change behavior.

Recently, however, religious priming has been the subject of a heated debate concerning the replicability of individual effects and the broader validity of this technique [1–3]. In a meta-analysis of 93 published and unpublished religious priming studies, religious priming was observed to have a consistent, small effect on the behavior of religious individuals [1]. These findings were contested as van Elk et al. [2] raised concerns that questionable research practices (QRPs) in social psychology confound meta-analysis results. They observed that religious priming effects are significant when Bayesian meta-analytic methods are used, but are non-significant when a Precision-Effect Testing–Precision-Effect Estimate with Standard Error (PET-PEESE) is used [2]. As these conflicting findings suggest, meta-analyses are not a panacea [17]; they are dependent upon the quality of the original data included and are subject to researchers' degrees of freedom involved with conducting analyses and interpreting the results [18]. PET-PEESE methodology, for instance, is sometimes unreliable when the true effect size is small and when the meta-analysis includes twenty or fewer studies, studies that have small sample sizes, or studies that exhibit a large degree of heterogeneity of effect sizes [17,19]. Taken together, while meta-analyses are useful and needed tools for research, they are often inconclusive and should be supplemented with replication efforts, including both direct and conceptual replications [2].

To determine the validity and generalizability of religious priming, improved experiment procedures and more diverse samples must be explored. Oftentimes, religious priming research has utilized dictator games to measure prosociality [6,7,20–24]. However, behavior in dictator games has been observed to lack global generalizability, as people from different cultures demonstrate unique norms for altruism when playing dictator games [25,26]. The method of religious priming can also influence results as recent work indicates that explicit religious primes (i.e., writing tasks) produce only small effects on prosociality, while implicit religious primes (i.e., anagrams) do not appear to influence responses to prosocial measures [22]. Furthermore, religious priming effects have primarily been studied using largely homogeneous, WEIRD (Western, Educated, Industrialized, Rich, and Democratic) samples, raising additional concerns about the generalizability of observed effects across other populations [1,27,28]. Indeed, a cross-cultural study of 15 small-scale societies found that religious priming inconsistently impacts people across cultural and religious contexts [29; see also 4,27]. Consequently, replication attempts should test novel, yet conceptually-relevant prosociality measures that seek to avoid the limitations of existing measures and should investigate if an observed effect replicates in the same sample (typically Western university students), but also across various cultural and religious settings, where an effect is claimed to have cross-cultural validity.

To that end, the current study attempts to replicate and extend the findings from Lang et al. [4], examining whether priming with instrumental religious music would decrease the rate of participants' dishonest behavior. Notably, this replication effort is distinct from what is sometimes referred to as a direct replication [30]. In this work, we adapt aspects of the methodology employed by Lang et al. [4] to investigate the conceptual consistency of their results. We designed this replication to provide results that would either increase or decrease the confidence in the interpretation of Lang et al. [4]. Therefore, this study utilizes the Open Science Framework (OSF) method of replication that, "reduces emphasis on operational characteristics of the study and increases emphasis on the interpretation of possible outcomes" [31]. (For a comparison of replication methodologies across disciplines, see Goméz and colleagues [30].) Below, we provide a succinct overview of Lang et al. [4]. Thereafter, we outline the unique aspects of this research and clarify the strategic motivations for modifying the procedure of Lang et al. [4]. Finally, in the Discussion, we detail how our adapted methodology may have limited our ability to replicate the findings observed in Lang et al [4].

Lang and colleagues [4]conducted a cross-cultural study on the effects of auditory cues on normative behavior using treatment with three musical stimuli (religious, secular, or white noise) prior to completing an arithmetic task designed to measure dishonest reporting for monetary reward (the Matrix Task, adapted from Mazar and colleagues [8]). Whereas they did not observe a main effect of musical treatment on dishonest reporting, the results revealed a Treatment*Religiosity interaction, such that participants self-reporting higher religiosity claimed less money upon hearing a religious musical track. Drawing from their results and previous research [16], Lang and colleagues [4] hypothesized that, ". . .instrumental music can serve as a reminder of normative behavior, but only for participants who previously formed an association between religion and specific music." Here, we re-examine this learned association hypothesis and test the impact of exposure to religious symbolism (music) on cheating behavior. As opposed to studying various forms of voluntary sharing and altruism that vary across cultures, we examine cheating as an anti-social behavior that is frequently and explicitly targeted by religious norms [32–34]. Critically, this study builds on previous efforts in four distinct ways.

First, to strengthen the link between musical stimuli and their effect on dishonest reporting, the musical tracks were played on-loop, throughout the entirety of the experimental task (rather than 2 minutes before the task as in Lang and colleagues [4]). Second, we use a

decision-making task that allows us to benchmark participants' self-reported earnings against factually correct answers over 200 total trials (the Dots Game, adapted from Gino and colleagues [35]). Compared to the previously used Matrix Task, the Dots Game provides a less biased measurement of cheating and allows the participant more opportunities to report dishonestly, strengthening the signal without increasing time requirements. In the Matrix Task employed by Lang et al. [4], the researchers were unable to determine participant accuracy in a specific trial. Rather, Lang et al. [4] measured an aggregate of self-reported 'correctly solved matrices' to make calculated assumptions about cheating behavior. In comparison, the Dots Game records accuracy in each trial and, therefore, enables the direct measurement of beneficial mistakes (cheating), which decreases the likelihood that genuine, erroneous participant judgments will impact our model. Third, we add a no-music condition to compare the impact of not listening to any sounds at all. Finally, we broaden the generalizability of this research by diversifying the sample, adding an East Asian (Japan) site to our sample in which the religious traditions represented are non-exclusive and focus more on practice (orthopraxy) than belief (orthodoxy) [36,37]. Given the population size of Japan (126.2 million: [38]) and the prevalence of orthopraxic religion in East Asia [39], adding a Japanese sample enables further examination of the generalizability of the hypothesized religious priming relationships. For a detailed summary of the differences between the design employed in this paper and the design used in Lang et al. [4], see S1 Table H in S1 File.

Data were collected from three culturally distinct samples that differ in intriguing ways in their general religiosity levels: the Czech Republic, Japan, and the USA. While the majority of people in Japan [40] and in the Czech Republic [41] indicate that religion is not important in their lives, the majority of people in the USA think religion is important in their lives [42]. Further, only a minority of people in the USA are not affiliated to any religion (16%), while the majority of people in the Czech Republic (76%) and in Japan (57%) report no affiliation [43,44]. Despite their high level of non-affiliation, however, over 40% of Japanese people indicated that they believe in god and a majority (62.7%) indicated they go to religious places at least once a year [45]. Japanese religious systems are also broadly non-exclusive and there is substantial overlap in the ritual and festival practices performed in both Shinto shrines and Buddhist temples [37,46–49]. Hence, differences between these sites should enable useful comparisons of the hypothesized effect, as they introduce variation in levels of ritual participation, religious belief, and the significance of religious affiliations. Critically, the inclusion of a Japanese sample enabled us to test for the effect of using musical performances drawn from a distinctive non-Western musical heritage, echoing Lang and colleagues' [4] choice to include Mauritian music.

Importantly, to date, there is only one published study that has investigated religious priming effects using a Japanese sample [50]. Miyatake and Higuchi [50] attempted a direct replication of Shariff and Norenzayan [6], utilizing identical methodology, and found that visual religious priming did not affect individuals' pro-sociality. As Miyatake and Higuchi [50]suggest, these results may have been due to their decision to use primes that relied on the Western, Christian references traditionally employed in religious priming techniques. Their suggestion that "if local religions and culture had been reflected in the religious primes, the results might have been different" [50] supported our decision to select culturally relevant auditory cues for each sample (see Materials).

To isolate the effects of religious music, we designed four conditions: religious, secular, white noise, and control (no music). Following Lang et al.'s [4] learned-association supposition and their results, we predicted there would be no main effect of condition on dishonest behavior, but we expected an interaction between religiosity and condition. Specifically, we expected that participants higher in self-reported religiosity would behave more honestly than their less-

religious peers but only when religious participants were listening to religious music. We also examined two supplementary hypotheses assuming: a) the moderating effects of affiliation to a religious organization that is congruent with the religious stimuli(religious affiliation) and b) the moderating effects of ritual participation frequency on the relationship between religious music and dishonest behavior.

The motivation for adding these supplementary hypotheses was to provide further nuance to previous findings by exploring the specific mechanisms that may facilitate the tentative effects of religious cues on normative behavior. The fact that Lang et al. [4] did not observe any main effect of condition on dishonest behavior and, consequently, that we do not expect such an effect in the present study supports the broader thesis that priming materials inherently connected to a specific cultural context will not affect people's behavior indiscriminately. That is, to the extent that priming effects rely on symbolic communication, they should be detected only in people who are able to access the symbol's conventional meaning and its connection to behavioral norms. Of course, some symbols include anthropomorphic characteristics that may exert additional effects on normative conduct. For instance, symbols with eyes may induce normative behavior because the presence of eyes, in general, indicates that one is being watched [13,51,52]. However, for most culturally specific cues(i.e., instrumental religious music), symbolic meaning and its association to moral norms must be learned and reinforced. In Lang et al. [4], the authors approximate individual reinforcement of religious music and its associated normative conduct by utilizing a concept of religiosity that subsumes dimensions such as religious belief, practice, experience, and commitment [53]. While this broad measure of religiosity is a useful and easy to use approximation, it does not afford for the precise estimation of an individual's commitment to and understanding of culturally specific religious symbols.

Indeed, being religious or spiritual does not guarantee that one will understand the meaning of a symbol and its connection to the normative structure of a religious system. In multi-religious contexts (USA and Japan in our sample) or in contexts where people declare to be religious/spiritual but unaffiliated (Japan and the Czech Republic in our sample), people may believe in various supernatural agents and take part in many religious activities that are not directly connected to the religious system from which we sampled our priming material. In other words, participants who do not self-affiliate with religious organizations that practice the specific tradition of our religious stimuli may not have learned the conventional meaning and associations of the specific cue used in the present study (despite self-reporting high religiosity). Compared to religiosity, affiliation may serve as a more fine-grained predictor of the learned association between a religious cue and normative behaviors. To examine this idea in greater detail, we measured affiliation and tested its interaction with our treatment, anticipating that religious affiliation would enhance the effects of religious music on ethical behavior.

Furthermore, in many Western religious systems (i.e., Protestantism), religiosity is often associated with personal belief [54–56]. However, in other more orthopraxic oriented religious systems, the dominant dimension of religiosity may be ritual behavior. Together, collapsing orthopraxic and orthodoxic perspectives under the measure of religiosity obfuscates clear distinctions between the effects of practice and belief on behavior [54,57]. Critically, Lang et al. [4] suggested that it is through communal rituals, rather than belief itself, that the conventional association between a sacred symbol and norms are established and perpetuated. Relatedly, previous findings have observed that increased frequency of ritual behavior facilitates favorable treatment of other co-religionists in real-life and in laboratory contexts, and across various economic games [4,12,58–60]. Music is central to many religious rituals [61,62], serving several functions like coordination and synchronization [10,63,64], but also creating lasting associations between ritual context and normative conduct. Importantly, Lang and colleagues [4]

observed a relationship between frequency of ritual participation and the effectiveness of an auditory religious prime on moral behavior. Thus, to provide a more nuanced investigation of this issue, we also included a measure of ritual participation frequency and interacted this measure with our manipulation, expecting that frequent ritual participation would strengthen the effects of religious music on honest behavior.

Materials and methods

Participants

Data were collected at universities across three sites: the USA, the Czech Republic, and Japan. A total of 460 (228 females) adults were randomly assigned to one of four conditions: religious, secular, white noise, or control (no sound). Due to self-reported suspicion on the goals of the experiment and previous experience with studies using the Dots Game, a total of 52 participants were excluded from the analysis. In support of this decision, supplementary analyses of the incorrectly claimed higher-paying sides in the Dots game showed that the excluded participants had 5.5 higher odds of incorrectly claiming all money from the higher-paying side (100% claimed) compared to participants in the included sample (see S1 Table G in S1 File for the analysis of the full sample). Five additional participants had missing data on crucial variables such as sex and age. Data included in the analyses therefore comprise: 123 American participants (M_{age} = 25.5, SD = 9.8), 128Czech participants (M_{age} = 24.4, SD = 3.4), and 157 Japanese participants (M_{age} = 19.8, SD = 0.9). Within the four experimental conditions, there were:100 participants in the control condition, 103 participants in the white-noise condition, 103 participants in the secular condition, and 102 participants in the religious condition. Based on the increase of 0.023 in R^2 for the interaction between condition and religiosity found in Lang et al. [4], this sample size should give us 0.85 power to detect the same interaction at alpha = 0.05 (calculated in G*Power).

Experiments were conducted in laboratory settings containing tables, chairs, and computers with headphones. Participants were seated in cubicles such that only the content of their own screen was visible. Experimental materials, informed consent forms, and scripts were translated into local languages (Czech and Japanese) from English. The institutional review boards of Duke University, Hokkaido University, and Masaryk University approved this research. All participants provided an informed consent before taking part in the experiment.

Materials

The Dots Game is designed to measure participants' willingness to cheat for real monetary rewards (adapted from Gino et al. [35]). The Dots Game is a digital task consisting of 200 trials in which dots quickly appear and disappear on a vertically bisected screen. In order to ensure comprehension, all participants completed 10 practice trials before starting the 200 trials that earned compensation. After each trial, participants were asked to indicate the side of the screen (left or right) that had contained more dots. There was no time limit for participants to make their decisions.

The total number of dots presented in each trial summed to 22 and a minimum of eight dots was randomly presented on the left or right side in every trial. Of the 200 payment trials, 120 displayed more dots on the left side while only 80 trials displayed more dots on the higher paying, right side. During the task, earnings accumulated and were displayed at the top of each participant's screen as selections were made. Although participants were instructed to be as accurate as possible, accuracy does not affect payment in the Dots Game.

It was explained to participants that detecting more dots on the left side of the screen is easier and that the payment for right-side selections would therefore be worth more than left-side

selections. Specifically, indicating that the left side of the screen contained more dots always earned the participant $0.005 USD while indicating that the right side of the screen contained more dots always earned the participant $0.05 USD. To measure cheating, we do not look to the total number of higher paying selections made [35]; rather, we limit response bias by observing the percentage of inaccurately claimed higher-paying sides [65]. In Japan and the Czech Republic, compensation was paid in local currency and approximately equaled USD amounts ($0.05 and $0.005). Therefore, a maximal cheater could earn $10 and a completely honest and accurate player would earn $4.60. Musical tracks (religious, secular, white noise) played on-loop were added to the Dots Game for the purposes of this experiment.

Musical tracks were pre-tested using participant pools specific to each site (online with Lancers in Japan and Amazon's Mechanical Turk in the USA, and with a student population in the Czech Republic). At each site, we compared eight 2-minute musical tracks across their musical characteristics, including tempo, affect, and impact. See S1 Table I in S1 File for the musical tracks tested in each population. The songs selected for the USA and CzR samples were identical to the ones used in Lang and colleagues [4]. For the USA sample, Johan Sebastian Bach's *Jesu joy of man's desiring* was chosen as the religious track, while Bach's *Sleepers awake* was chosen as the secular track. In the Czech Republic, Bach's *Ave Maria* (Gounod's interpretation) was used as the religious music, while Tchaikovsky's *Romance for piano in F Minor*, *Op. 5* was selected for the secular music. A *Gagaku*(雅楽) musical track was selected as the religious stimuli for the Japan sample. *Gagaku* music is a form of traditional classical music performed by an orchestra and usually features the distinctive sound of a traditional mouth organ, referred to as *Shō*(笙). *Gagaku* has been described as the world's oldest orchestral music and is associated with Shinto ritual performances and ceremonies conducted at the imperial court [66,67]. For the Japanese secular music condition, music performed on a *koto* (箏), a traditional Japanese stringed instrument, was selected. Furthermore, in order to avoid any associations with traditional ritual or religious settings, a *koto* performance of a more contemporary arrangement was selected [68]. Across all sites, the selected religious and secular musical tracks were instrumental only and included no vocal elements. They differed in perceived sacredness but were similar in tempo and affect (See 'Manipulation Check'). Finally, the white noise comprised a loop of white noise played through headphones at all sites, while the control condition comprised just silence.

Surveys were administered after completion of the Dots Game to assess religiosity (0 –Not religious at all, 4 –Very religious/spiritual person), ritual attendance frequency (0 –Never, 6 –More than once per week), religious organization affiliation (i.e., church), and religious tradition participants were affiliated with. Participants in the music conditions (religious, secular, and white noise) rated how secular/religious and profane/sacred the sound was on a 7-point Likert scale (1 = Secular, 7 = Religious; 1 = Profane, 7 = Sacred). Additionally, music condition participants used a 5-point Likert scale (1 = Not at all, 5 = Extremely) to rate the extent to which the song they heard was: sad, fast, boring, pleasant, happy, irritating, slow, exciting, deep, interesting, distressing, powerful, relaxing, and distracting. Recognition of the musical track (Yes, No) was also assessed for music conditions' participants. All participants were asked about the perceived difficulty of the task (1 –Very easy, 5 –Very difficult), as well as their age and gender. Given the Dots Game was developed and studied locally, US participants also indicated their involvement in previous research using the Dots Game (Yes or No).

Procedure

Participants were first randomly assigned to one of four conditions: religious music, secular music, white noise, or control (no music). Participants were informed that the research was studying decision-making and at each site, local research assistants facilitated the experiment.

Upon arrival to the lab, participants were seated in front of a tablet or a computer and could see their screen only. First, participants read instructions for the Dots Game where it was explained that the game consisted of 200 trials in which dots would temporarily flash onto computer screens. For the duration of the game, the computer screens were divided into two vertical halves (left and right) and, after each trial, participants were asked to accurately determine which side of their screen contained the majority of the dots that had appeared by pressing either the 'M' key (to indicate right) or the 'Z' key (to indicate left) on a computer keyboard. Dots remained visible on the computer screen for only one second before participants were prompted to make their selection. As participants made their selections, earnings accumulated and were displayed at the top of each participant's screen. On average, it took participants six minutes and four seconds (SD = 1.15 minutes) to finish the Dots Game.

All participants were instructed to wear the headphones at their computer for the entire duration of the game. Control participants played the Dots Game without music; all other participants played the Dots Game while listening to their site-specific and randomly assigned musical track on-loop. The research assistant was available in an adjacent room to provide any necessary assistance. After the game was over, participants completed a post-study questionnaire (see Materials for overview, Supporting Information for detailed review) and received the reward amount they had earned in the Dots Game. Completing the Dots Game and survey took participants no more than 30 minutes.

Results

All data were analyzed in R (version 3.4.3, R Core Team 2017). We first constructed an Ordinary Least-Square Regression (OLS) model with treatment as a factor variable, investigating the main effects of musical condition on the percentage of dishonestly claimed earnings. We set the religious condition as a reference category to compare its effects with various controls (secular music, white noise, control), while holding the effects of age, gender, and site constant as simple fixed effects. Note that the USA was set as a reference category for the site factor variable; however, this selection was arbitrary and did not affect any of the main estimates of interest. Next, three interaction OLS models were constructed, looking at the moderating effects of religiosity, ritual participation, and religious affiliation on the relationship between the treatment and dishonest behavior. Religious affiliation was determined using religious organization affiliation and religious identity responses. Specifically, a participant was considered affiliated if they belonged to a religious organization and self-identified with the religion associated with the religious stimulus at each site (i.e., Christianity in the Czech Republic and US, Shinto in Japan). Note that while Lang and colleagues [4]used beta regression to model the percentage data in their previous analyses [69], the current results from OLS regressions are qualitatively similar to the results of beta regression; hence we opted for simpler models. The results from beta regression models are reported in the, S1 Table D in S1 File. Likewise, since our participants were nested within sites, it would be more appropriate to use linear mixed models to investigate our main hypotheses. However, given that there are only three categories in our nesting variable, estimating individual site intercepts from the partially pooled data did not yield qualitatively different results compared to using sites as simple fixed effects (see S1 Table E in S1 File). Additionally, we adjusted our models to account for: the perceived difficulty of the dots task, the difference between the average trial completion time, and the completion times for trials where participants cheated. In the final robustness check, we hold constant the ratings of musical stimuli to ensure that the observed effects were not caused by differences between the stimuli's perceived affect, tempo, or impact (see S1 Table F in S1 File).

Manipulation check

Perceived sacredness was observed to be significantly different between musical conditions [F(2,299) = 53.8]. Across all sites, the religious track received significantly higher ratings of sacredness than did the secular(β = -0.85; 95% CI = [-1.21, -0.49]) or white-noise tracks (β = -1.91; 95% CI = [-2.27, -1.54] see Table 1 for descriptive statistics). Similar results were obtained with the secular/religious measure [F(2,300) = 40.34], showing higher religiosity ratings of the religious song compared to the secular (β = -1.32; 95% CI = [-1.69, -0.95]) or white-noise tracks (β = -1.60; 95% CI = [-1.97, -1.23]).

Dishonest behavior

Dishonesty in the Dots Game was observed by measuring the proportion of inaccurate higher paying (right side) selections made. This dishonesty metric was calculated by dividing the number of times a participant inaccurately indicated that the higher paying side contained more dots by the number of trials (120) in which the lower paying (left side) truly contained more dots. Participants across all sites earned an average of $5.86 (SD = $1.52), indicating right side incorrectly on average in 27.39% of trials (SD = 29.21%). Interestingly, the average rates of dishonest reporting differed between our sites: while in the Czech Republic, participants claimed on average 11.69% (SD = 13.72%) incorrectly, in Japan and the USA the rates were as high as 29.88% (SD = 28.43%) and 40.56% (SD = 34.28%), respectively (see Fig 1B).

Looking at the distribution of dishonest reporting across our musical treatment, we observed that the control condition had the lowest amount of cheating, followed by the religious, white-noise, and secular conditions (see Table 1 and Fig 1A). However, these raw results ignore the hierarchical structure of our data where participants are nested within sites. Hence, to examine the effects of our treatment on dishonest reporting more rigorously, we regressed the incorrectly claimed right sides on our musical treatment, holding the site-specific mean levels of dishonest reporting constant. This regression model revealed that there were no substantial differences between the religious and the secular (β = 3.45; 95% CI = [-4.02, 10.91]), white-noise (β = 3.21; 95% CI = [-4.21, 10.62]), or control conditions (β = -4.91; 95% CI = [-12.37, 2.55]; see Table 2). The inability to find differences between conditions replicates Lang and colleagues' [4] previous finding.

Following the absence of treatment main-effect, we tested three moderator models, investigating the role of self-declared religiosity, ritual frequency, and religious affiliation. First, we did not observe an interaction between condition and self-reported religiosity. While an increase in self-declared religiosity predicted decrease in the proportion of incorrectly reported

Table 1. Descriptive statistics of aggregate unethical behavior and post-experiment ratings of musical stimuli.

	Religious (*n* = 102)				Secular (*n* = 103)				White Noise (*n* = 103)				Control (*n* = 100)			
	M	SD	CI	*d*	M	SD	CI	*d*	M	SD	CI	*d*	M	S	CI	*d*
% Claimed	27.33	29.92	[21.52,33.14]	-	30.32	30.98	[24.34,36.30]	0.10	29.40	31.09	[23.40,35.40]	0.07	22.38	23.89	[17.69,27.06]	0.18
Sacredness	5.17	1.39	[4.90, 5.44]	-	4.32	1.35	[4.06, 4.58]	0.62	3.26	1.19	[3.04, 3.49]	1.48	-	-	-	-
Negativity	1.82	0.65	[1.70, 1.95]	-	1.64	0.51	[1.55, 1.74]	0.31	2.58	0.96	[2.39, 2.76]	0.93	-	-	-	-
Positivity	2.54	0.85	[2.37, 2.70]	-	2.72	0.78	[2.56, 2.87]	0.22	1.38	0.62	[1.26, 1.50]	1.56	-	-	-	-
Tempo	2.60	0.81	[2.45, 2.76]	-	2.78	0.82	[2.62, 2.94]	0.22	3.99	0.72	[3.85, 4.13]	1.80	-	-	-	-
Impact	2.90	1.10	[2.70, 3.10]	-	2.73	1.13	[2.51, 2.95]	0.16	1.76	0.93	[1.58, 1.94]	1.13	-	-	-	-

M = Mean; SD = Standard Deviation; CI = 95% Confidence intervals. Cohen's d represents the effect size of comparisons between musical conditions.

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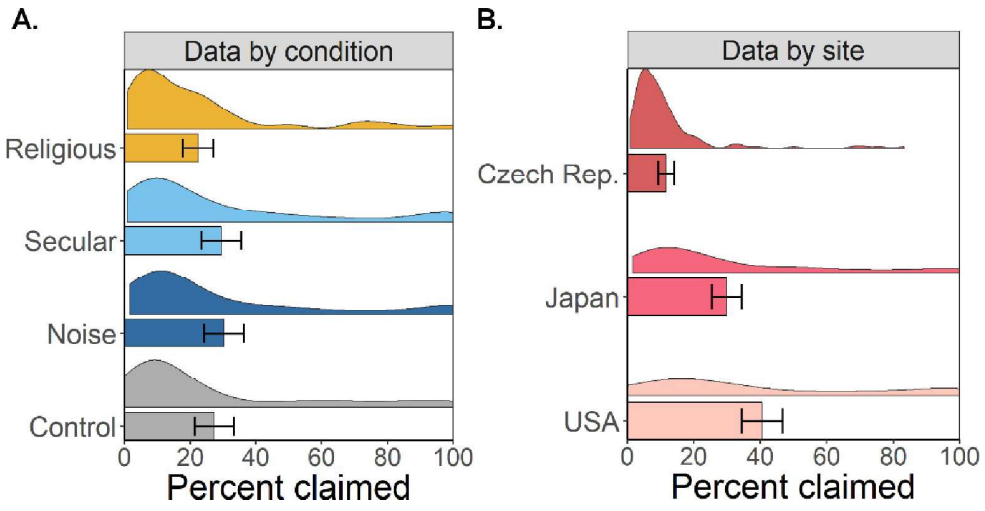

Fig 1. Mean values for dishonestly claimed earnings with 95% CIs divided by condition (A) and site (B). Each level displays a bar with 95% CIs and a density plot.

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right sides in the religious music condition (β = -2.12), this decrease was imprecisely estimated and the 95% CI crossed zero (-6.77, 2.53). Furthermore, this religiosity coefficient was not substantially different from coefficients in the secular ($\beta_{difference}$ = 2.97; 95% CI = [-3.40, 9.35]), white-noise ($\beta_{difference}$ = -1.40; 95% CI = [-7.94, 5.14]), or control conditions ($\beta_{difference}$ = 0.82; 95% CI = [-5.49, 7.13]; see Fig 2A). For site-specific results, see S1 Table A in S1 File.

Following the effects of ritual participation found in Lang et al.[4], we built a second moderator model measuring the effect of the ritual attendance frequency and its interaction with treatment. An increase of one on the ritual frequency scale (0 –Never, 6 –More than once per week) predicted a decrease of roughly 4 percentage points in dishonestly claimed compensation in the religious condition (β = -4.19; 95% CI = [-7.34, -1.03]). Importantly, the slope of the ritual attendance differed between conditions, showing that ritual attendance decreased the ratio of incorrectly claimed right sides only to a small extent in the secular condition($\beta_{difference}$ = 3.46, 95% CI = [-1.01,7.94]), and had no effect in the noise($\beta_{difference}$ = 4.49, 95% CI = [-0.10, 9.08]) and control conditions($\beta_{difference}$ = 3.89; 95% CI = [-0.48,8.26]). See also Table 2 and Fig 2B and S1 Table B in S1 File for site-specific results.

Finally, in the third moderator model, we analyzed the effects of self-reported affiliation matching the specific religious stimulus at each site(binary yes/no variable) and its interactions with the musical treatment. Religious affiliation had a strong negative relationship with dishonest reporting in the religious condition, predicting roughly 26 percentage points lower number of unfairly claimed compensations (β = -26.09; 95% CI = [-40.12, -12.06]). Importantly, we observed a significant Condition*Affiliation interaction: the effect of religious affiliation was much weaker in all three remaining conditions (Secular: $\beta_{difference}$ = 23.78, 95% CI = [5.64, 42.90]; Noise: $\beta_{difference}$ = 21.84, 95% CI = [2.76, 40.92]; Control: $\beta_{difference}$ = 24.94, 95% CI = [5.97, 43.91]; see Table 2 and Fig 2C). These findings indicate that participants who affiliated with a religious tradition matching our stimuli were less dishonest when listening to the religious song, but affiliation had no effect in the remaining conditions (see S1 Table C in S1 File for site-specific results). See also, S1 Table D in S1 File for a robustness check of these results using the Beta regression and S1 Table E in S1 File for using linear mixed models (these robustness checks support our findings obtained with simpler models reported here).

Table 2. Estimates with 95% CIs from Ordinal Least Squares regressions for the percentage of higher-paying side (right) claimed as having more dots.

	M1: Baseline	M2: Religiosity	M3: Ritual frq.	M4: Affiliation
Intercept	38.06***	42.08***	46.61***	44.61***
	(31.09, 45.02)	(31.37, 52.80)	(36.86, 56.35)	(36.75, 52.47)
Secular	3.45	-1.34	-2.70	-0.78
	(-4.02, 10.91)	(-14.26, 11.57)	(-13.84, 8.44)	(-9.31, 7.76)
Noise	3.21	5.81	-5.82	-0.76
	(-4.21, 10.62)	(-7.31, 18.93)	(-16.85, 5.22)	(-9.04, 7.52)
Control	-4.91	-6.12	-11.41*	-9.52*
	(-12.37, 2.55)	(-18.88, 6.63)	(-22.13, -0.68)	(-17.88, -1.17)
Sex	2.18	1.93	2.51	2.65
	(-3.24, 7.60)	(-3.62, 7.48)	(-3.04, 8.07)	(-2.82, 8.13)
Age	0.49*	0.47†	0.52*	0.48*
	(0.03, 0.96)	(-0.004, 0.94)	(0.06, 0.99)	(0.02, 0.94)
Site: Czech Rep.	-28.56***	-28.90***	-29.74***	-30.86***
	(-35.27, -21.85)	(-35.84, -21.97)	(-36.74, -22.73)	(-37.84, -23.88)
Site: Japan	-8.14*	-9.23*	-10.26*	-11.32**
	(-15.23, -1.06)	(-16.67, -1.79)	(-18.03, -2.49)	(-18.72, -3.92)
Moderator	-	-2.12	-4.19**	-26.09***
	-	(-6.77, 2.53)	(-7.34, -1.03)	(-40.12, -12.06)
Secular*Moderator	-	2.97	3.46	23.83*
	-	(-3.40, 9.35)	(-1.01, 7.94)	(5.64, 42.01)
Noise*Moderator	-	-1.40	4.49†	21.84*
	-	(-7.94, 5.14)	(-0.10, 9.08)	(2.76, 40.92)
Control*Moderator	-	0.82	3.89†	24.94*
	-	(-5.49, 7.13)	(-0.48, 8.26)	(5.97, 43.91)
Observations	403	395	384	397

Moderator is either religiosity, ritual frequency, or religious affiliation, see model names. The condition*moderator interactions represent the estimated differences between the slope of the moderator in the religious condition and moderator slopes in the other conditions.

†p< 0.1

*p < .05

**p < .01

***p < .001.

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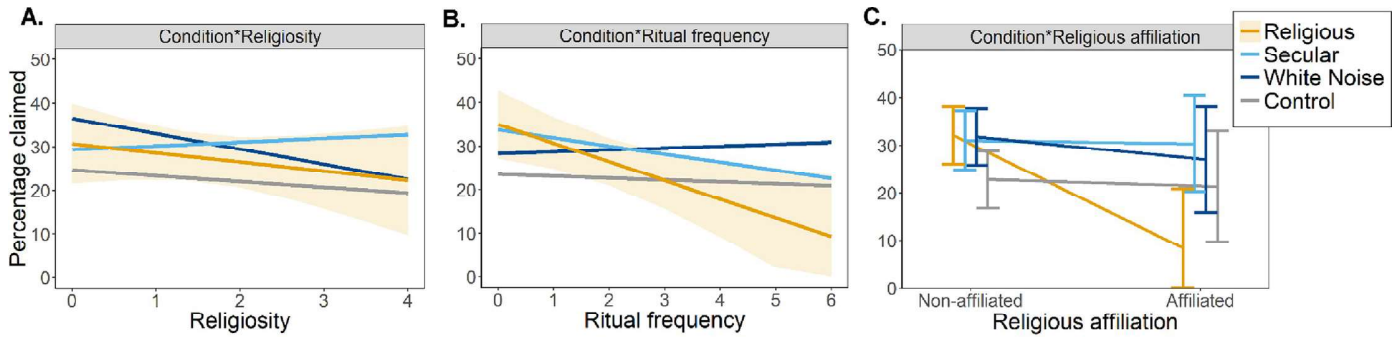

Fig 2. Interaction plots with predicted values for dishonestly claimed earnings and 95% CIs. A. The regression slope of religiosity in the religious condition did not differ from the other conditions. **B.** Ritual frequency predicted decreased cheating, and the regression slope differed from other condition (albeit the 95% CI crossed zero for the difference between the religious and secular conditions). **C.** Self-declared affiliation to religious organization congruent with our stimuli predicted decreased cheating in the religious condition but not in the other conditions. We display 95% CIs only for the religious condition for easier reading. All CIs are displayed in Table 2.

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As a final robustness check, we also adjusted our models for the mean completion time of the dots task, perceived difficulty of the task, and musical characteristics of our stimuli (see S1 Table F in S1 File). Across all models, we observed that the rates of dishonest behavior were predicted by faster completion times on the dishonestly reported trials. That is, the less time participants dedicated to decision making, the more likely they were to report dishonestly because correct answers required deliberately counting the dots and making sure one selected the correct answer. This finding is congruent with perceived difficulty of the task, which negatively predicts the number of incorrectly reported dots. Notably, these findings can be explained by dishonest participants' willingness to pre-determine the higher compensation choice (hold down the 'M' key), which would naturally decrease participation time and task difficulty. After adjusting our models for these variables, the moderating effects of ritual participation frequency and religious affiliation remained stable. Furthermore, we assessed whether the reported results hold even after the models accounted for the musical characteristics of our stimuli: tempo, influence, positivity, and negativity (see S1 Table F in S1 File). While perceived negativity of the played track predicted dishonesty, we still observed the effect of ritual and religious affiliation as well as the interactions of Condition*Affiliation and Condition*Ritual, albeit we could not assess the effects of musical characteristics in the control condition due to the fact this condition had no musical stimulus. These findings indicate that the religious condition tracks did not affect cheating in the Dots Game due to their musical characteristic ratings; rather, participants who affiliated with a religious organization and participated in rituals were uniquely affected by the sacred music primes and, in turn, played less dishonestly than unaffiliated participants.

Discussion

In the present study, we conducted a cross-cultural replication and extension of religious priming research by Lang et al. [4]. Specifically, we tested the hypothesis that auditory religious cues decrease unethical behavior in cheating games compared to other auditory cues (secular, white noise, and control). We collected data on university populations from three countries with distinct religious and cultural norms: the Czech Republic, Japan, and the United States of America. All participants played a cheating game, the Dots Game, during which they listened to a musical track and had an opportunity to maximize their earnings by playing dishonestly. There was no main effect of musical treatment on cheating behavior. Further, we were unable to replicate the interaction effect of Condition*Religiosity observed by Lang et al. [4]. However, we did observe interactions between condition and religious affiliation and condition and ritual participation. Auditory religious cues were found to decrease dishonest reporting for participants frequently attending religious services and for those affiliated with a religious organization matching our religious stimuli.

Taken together, our results generally provide support for Lang and colleagues' [4] observation that exposure to religious music is not enough to prime honest behavior in all contexts. Notably, our findings provide additional evidence for a mechanism they proposed; specifically, an entrenched association between music and the religious values it reinforces is required for the activation of normative behavior. In their paper, Lang & colleagues [4] observed that situational religious factors, such as reported religiosity and ritual participation, played a role in the activation of religious cues and facilitation of their effects on ethical behavior. Although we did not observe the Condition*Religiosity interaction in the present research, we did observe similar interactions indicating that individual religious characteristics play a role in how people react to religious auditory cues.

In order to understand why we were unable to replicate the Religiosity*Treatment interaction, it is important to consider two points. First, compared to the Mauritian sample in Lang et al. [4], our Japanese sample included greater religious diversity. All Mauritian participants were Hindu in Lang et al. [4], hence the selected religious music was generally familiar and associated with their affiliated tradition. In our Japanese sample, however, participants from a variety of religions were represented (Christianity, Buddhism, Shinto, Judaism, atheism, unknown, and other). While *Gagaku* music is sometimes performed as part of Buddhist religious ceremonies, it is most strongly associated with Shintoism. Therefore, it is possible that for some of our participants this music did not cue normative behavior typical for the targeted religious affiliation. Second, religion in Japan, as elsewhere in Asia, is typically regarded as non-exclusive and has been described as having a 'practical' orientation wherein adherence to religious beliefs is seen as of secondary importance to ritual practices [37,39,70], potentially confounding the measure of religiosity. For instance, Kavanagh and Jong [39], recently demonstrated that while only 10% of 1,000 Japanese respondents self-identified as religious, 34% in the same sample identified as Buddhist, 5% as Shinto, and 33% endorsed the existence of God.

The increased religious diversity of our sample implies that religiosity in the current paper has a distinct meaning as it is understood within the context of an individual's religious background. Indeed, the site-specific analyses of the Condition*Religiosity interaction (see S1 Table A in S1 File) revealed that in Japan, religiosity had the smallest effect on dishonest behavior in the religious condition (albeit these coefficients were imprecisely estimated). This finding is in line with similar research showing that in multi-religious societies, cross-religious symbols may reduce trust and cooperation [71, but see also 72].

Secondly, the relationship between affiliation, ritual participation, and religiosity is complex. It is entirely possible for someone to be religious/spiritual and, at the same time, be unaffiliated with a specific religion or religious organization. In the USA, for instance, where religiosity is typically understood as affiliation to a specific church [73], more than a quarter (27%) of adult respondents indicated they were spiritual, but not religious [42]. However, if a person is spiritual without being affiliated to a specific religious institution, they may lack the necessary associations with the sacred auditory cues we selected and therefore could be less, or entirely, unaffected by our stimulus. To make the relationship clearer, we coded affiliation to specific religious organizations and demonstrated that membership in the matching tradition had an effect. We conjecture that this is due to people affiliated with a religious organization having more exposure to sacred music in meaningful contexts than unaffiliated peers and consequently, affiliated persons are likely to have stronger associations to the moral implications of religious auditory cues than the unaffiliated. In support of this, research on exposure and learning has demonstrated that a person's affective response to a stimulus can change over time [74]. In addition to repeated exposure, the meaningfulness of a stimulus can affect a person's response to it [75]. With repeated exposure to meaningful stimuli, people are able to create associations and have the ability to strengthen their judgments about the stimuli and its propositions [76].

It is also important to consider the fact that unaffiliated religious people often have differing beliefs about the connection between specific religions, belief in God(s), and morality [77]. The majority of religiously affiliated people in the USA (55%) agree that the belief in God is necessary to be moral; conversely, however, of all adults—affiliated and unaffiliated—the majority in the Czech Republic (78%), Japan (55%), and in the USA (56%), do not think it is necessary to believe in god to be moral [44,78]. Based on previous literature and on the findings of the present research, we suggest that religious auditory stimuli cue ethical behavior only for participants who believe that their affiliated religion is inherently linked to morality.

This interpretation is congruent with the results from the regression model of the interaction between condition and the frequency of ritual attendance, where ritual attendance has the

largest negative effect on dishonest reporting in the religious condition. Through repetitive reminders of sacred cues during religious ceremonies, the association between a specific sacred cue and the moral doctrine of a specific religion is strengthened and the cue is emotionally charged with special significance [61]. This in turn may result in a larger influence over participants' behavior being exerted upon perception of the cue [79,80]. Importantly, collective rituals are also a public venue for communicating commitment to supernatural agents and the norms they impose on believers [80–82]. However, the commitment is not signaled only to other believers but also to oneself as a form of auto-signaling reassuring participants about their beliefs in supernatural agents [58,83,84]. Hearing religious music may be a subconscious reminder of participation in rituals, strengthening the auto-communicated commitment signals. While the effect of ritual participation was weaker compared to religious affiliation (similar to the findings of Lang et al. [4]), we still detected a signal supporting this interpretation.

It is important to note that our analyses do not take into account what type of rituals our participants attend as well as the frequency with which our religious stimuli are played during specific religious ceremonies; hence, the signal is noisy. A larger, high-powered sample allowing for testing a three-way interaction between treatment, religiosity, and affiliation to specific religious organization may solve this issue in future studies. Furthermore, our sample consisted predominantly of university students. While the sample was culturally and religiously diverse, it generally lacked diversity in age or employment status. Moreover, due to specific lab guidelines, participants in the Czech sample received an additional reward of points that were redeemable for a course credit, which may have partially decreased their motivation to report dishonestly for monetary compensation. Indeed, this motivation may have acted as a boundary condition for religious priming across samples. As previously noted by other researchers using priming techniques [23,29], there may have been little room to observe religious priming effects if the motivation to be dishonest was relatively low in the Czech Republic. To avoid these problems, cross-cultural researchers should be careful to align laboratory incentive structures across samples. Moreover, future studies on priming with religious music should consider sampling from more populations with cultural and demographic diversity that would allow for the assessment of between-site differences in the hypothesized effects (see S1 Tables A-C in S1 File). Likewise, since stimulus selection can influence Type I error rates if stimuli are not representative of their theoretical construct and are treated as fixed-factors [85], the selection of locally salient religious stimuli should be made more robust by including at least three different stimuli at each site. Future researchers should also explicitly control for stimulus variation by employing mixed models that treat both participants and stimuli as random factors [86].

Additionally, it is useful to consider the limitations of single-item scales when interpreting our results [87]. The reliability of the religiosity measure we employed was not clearly established in this research. This measure contained only a single-item and was not pre-tested between sites prior to experimentation [87]. Therefore, it may have been the case that religiosity was understood and expressed idiosyncratically within each of our cross-cultural samples. Furthermore, the religiosity scale was asymmetrical as the word "spiritual" was not included at both ends of the scale (see Questionnaire materials in S2 File). This scale asymmetry was consistent across all sites and was due to an error in implementing the materials of Lang et al [4]. Together, the lack of pre-tested validity of our single-item measure as well as the scale's asymmetry, may have limited our ability to replicate the primary Condition*Religiosity interaction observed in Lang et al [4]. To explicitly address such concerns, future researchers should investigate the reliability of different religiosity scales across cultures, peoples, and religions.

Finally, it is worthwhile to discuss a limitation of the Dots Game procedure. The Dots Game has been utilized to measure ethical decision-making preferences for nearly a decade

[35] (See Materials for a review of the procedure). In this research, we use the Dots Game to observe cheating behavior as it provides a less biased- and more granular- measurement than the Matrix task employed by Lang and colleagues [4]). Furthermore, we investigated religious priming effects on honesty because normative regulations of altruism and sharing may vary across cultures substantially more compared to norms regulating cheating [27,29]. Despite the benefits of using the Dots Game (see in-depth discussion in Introduction), our results may be influenced by signal detection biases (for a review of signal detection research, see [88–90]). Due to the compensation scheme, participants may have preferred the higher-paying (right) side and therefore, may have had an unconscious bias to detect more dots on the higher-paying side.

Indeed, research has shown that Dots Game participants attend more to the higher-paying side in incentivized Dots Games [65]. Unconscious attentional bias may increase ambiguity, making it less clear to participants when their choices are inaccurate. Thus, cheating behavior may have been facilitated by an interaction of signal detection bias with various factors(e.g., noise, perceived difficulty or distraction). Greater familiarity with a musical track, for example, could have increased participants' bias for detecting higher-paying side dots, which in turn may have increased participants' likelihood to report inaccurate higher-paying selections. However, given the potential for third variable problems, we adjusted our models for perceived difficulty and musical ratings (See Supporting Information for an in-depth review). Furthermore, it is unlikely that unconscious signal detection biases can fully explain the cheating behavior we observed in this paper, as other findings indicate that Dots Game participants are at least partially conscious of their unethical behavior [65].

We recommend that future research extends the religious priming literature by exploring methods that will provide less biased cheating data. Specifically, future studies should test the religious priming effects on a broader spectrum of samples, including horticulturalists and pastoralists from small-scale societies [29]. Moreover, we encourage future researchers to independently investigate if the differences observed between our samples replicate. For instance, cross-cultural researchers could attempt to replicate and extend the understanding of the uniquely low cheating rates observed in our Czech sample, as well as the depressed Condition*Religiosity interaction coefficient observed in the Japan model. Additional, experiments could test religious priming effects on cheating behavior across a variety of cheating tasks. Finally, we encourage healthy science practices and invite researchers to improve the generalizability of our findings by iterating on our replication with pre-registered designs. In support of such future endeavors, we have made our study materials and data available for access on the Open Science Framework (https://osf.io/k4dt8).

Conclusion

In summary, we conceptually replicated findings in the religious priming literature that indicate sacred cues affect individual ethical behavior and support a learned association hypothesis [4]. Although we did not find evidence for the expected interaction effect between religiosity and musical primes, we did observe an interaction effect between ritual participation and musical prime and between religious affiliation and musical prime. More specifically, sacred auditory cues were found to affect ethical behavior for individuals who attend religious rituals or were affiliated with a religious organization that practices the tradition associated with the relevant musical cues. These indirect priming effects were congruent with Lang and colleagues [4], even though the current paper extended the research design by utilizing a decision-making task better able to detect cheating (the Dots Game) and by including a non-Western site with greater religious diversity and an orthopraxic religious orientation (Japan).

It is our hope that the current research will inspire others to conduct replications and further examinations of religious priming effects, especially with understudied populations. Indeed, replications have been identified as a solution to the reproducibility crisis in the social sciences and may one day end debates within religious priming research [13,15,16].

Supporting information

S1 File.
(DOCX)

S2 File. Post-study questionnaire materials, translated from English into Czech and Japanese for the Czech Republic and Japan sites, respectively.
(DOCX)

S1 Data.
(ZIP)

S2 Data.
(ZIP)

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Replicating and extending the effects of auditory religious cues on dishonest behavior

Supplementary Material

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S0 Music Rating

The nine musical characteristic ratings were collapsed into two emotional valence dummy-variables: positivity and negativity. Positivity represented the average ratings of "happy, pleasant, exciting, interesting and relaxing," while negativity represented the average ratings of "sad, boring, irritating, and distressing." Regressing our measure of positivity on experimental conditions revealed that participants gave significantly different ratings of positivity by musical condition [$F(2,291) = 90.19$]. Using the Tukey's HSD post-hoc correction, the religious and secular music tracks received significantly higher ratings of positivity ($ps < 0.001$) than did the white noise track. Similarly, negativity ratings were significantly different by musical condition [$F(2, 295) = 46.26$]. The white noise tracks were given greater negativity ratings than were the religious and secular tracks ($ps < 0.001$; Tukey's HSD). There were no differences observed between the secular and religious tracks for positivity ($p = 0.224$), nor were there differences in ratings of negativity between the secular and religious tracks ($p = 0.198$).

Further analyses yielded similar results for musical ratings of tempo ("fast, slow") and influence ("deep, powerful"). Between conditions, participants gave significantly different ratings for tempo [$F(2,297) = 91.25$] and influence [$F(2,296) = 33.92$]. Participants rated white noise track significantly higher in tempo than the religious and secular music tracks ($ps < 0.001$), while there were no differences in tempo between the religious and secular music condition ($p = .249$). Likewise, participants rated the white noise track significantly lower in influence than the religious and secular music tracks ($ps < 0.001$), but there were no differences between the religious and secular conditions ($p = 0.451$).

S1 Tables

S1. Table A. Site-specific estimates with 95% CI for the percentage of higher-paying side (right) claimed as having more dots.

	USA	Czech Republic	Japan
Intercept	53.92***	9.98†	32.10**
	(30.37, 77.47)	(-0.14, 20.10)	(10.19, 54.01)
Secular	-19.26	-7.72	7.32
	(-52.81, 14.29)	(-22.57, 7.13)	(-12.09, 26.74)
Noise	-4.00	5.86	12.50
	(-38.26, 30.27)	(-7.11, 18.84)	(-9.51, 34.50)
Control	-24.96	6.25	0.63
	(-56.63, 6.70)	(-7.79, 20.28)	(-19.06, 20.33)
Sex	5.41	-0.83	2.67
	(-7.82, 18.64)	(-6.13, 4.47)	(-6.82, 12.16)
Age	0.52	-0.25	1.73
	(-0.13, 1.16)	(-1.07, 0.56)	(-3.52, 6.98)
Religiosity	-6.30	-1.09	0.52
	(-15.81, 3.21)	(-6.05, 3.87)	(-8.98, 10.03)
Secular*Religiosity	7.88	7.30*	-0.79
	(-5.76, 21.51)	(0.41, 14.19)	(-14.58, 13.01)
Noise*Religiosity	1.50	-0.63	-4.55
	(-13.05, 16.05)	(-6.88, 5.62)	(-18.40, 9.30)
Control*Religiosity	6.71	-0.44	-6.03
	(-6.27, 19.69)	(-7.10, 6.23)	(-18.88, 6.82)
Observations	122	118	155

Note. This model describes Condition*Religiosity interaction effects for each of the three sites: USA, Czech Republic, and Japan. The religious condition was set as a reference category in each site.
†p < 0.1; *p < .05; **p < .01; ***p < .001

S1. Table B. Site-specific estimates with 95% CI for the percentage of higher-paying side (right) claimed as having more dots.

	USA	Czech Republic	Japan
Intercept	59.75***	8.92*	44.42***
	(39.10, 80.40)	(1.26, 16.58)	(20.24, 68.59)
Secular	-17.38	-3.69	2.94
	(-48.57, 13.81)	(-14.03, 6.65)	(-15.01, 20.89)
Noise	-22.25	2.70	-3.69
	(-54.63, 10.14)	(-7.49, 12.88)	(-21.52, 14.13)
Control	-32.52*	4.45	-10.45
	(-60.79, -4.25)	(-6.29, 15.19)	(-27.35, 6.45)
Sex	7.28	1.13	2.59
	(-5.71, 20.28)	(-4.01, 6.27)	(-7.24, 12.42)
Age	0.56ŧ	-0.32	3.31
	(-0.08, 1.20)	(-1.13, 0.48)	(-2.90, 9.51)
Ritual frq.	-7.30*	-0.84	-5.65
	(-13.35, -1.26)	(-4.15, 2.48)	(-12.94, 1.63)
Secular*Ritual frq.	5.39	6.48**	3.38
	(-3.95, 14.74)	(1.74, 11.21)	(-7.21, 13.96)
Noise*Ritual frq.	8.11	0.95	7.03
	(-2.06, 18.29)	(-3.54, 5.43)	(-4.67, 18.74)
Control*Ritual frq.	8.02ŧ	0.42	3.17
	(-0.50, 16.54)	(-4.29, 5.12)	(-8.09, 14.43)
Observations	122	121	141

Note. This model describes Condition*Ritual interaction effects for each of the three sites: USA, Czech Republic, and Japan. The religious condition was set as a reference category in each site.
ŧp < 0.1; *p < .05; **p < .01; ***p < .001

S1. Table C. Site-specific estimates with 95% CI for the percentage of higher-paying side (right) claimed as having more dots.

	USA	Czech Republic	Japan
Intercept	60.13***	8.52**	34.32***
	(44.63, 75.63)	(2.73, 14.32)	(14.73, 53.91)
Secular	-19.76†	1.09	7.64
	(-41.59, 2.06)	(-6.39, 8.57)	(-5.95, 21.23)
Noise	-22.05*	4.45	6.93
	(-43.07, -1.03)	(-2.94, 11.83)	(-6.25, 20.12)
Control	-32.62**	5.55	-7.42
	(-53.76, -11.47)	(-2.00, 13.10)	(-20.48, 5.64)
Sex	8.07	-0.32	1.90
	(-4.31, 20.44)	(-5.31, 4.66)	(-7.62, 11.42)
Age	0.53†	-0.38	1.85
	(-0.08, 1.14)	(-1.14, 0.38)	(-3.22, 6.92)
Affiliation	-48.78***	-4.04	-13.37
	(-71.92, -25.63)	(-20.25, 12.17)	(-54.03, 27.29)
Secular*Affiliation	41.15*	30.93**	-1.40
	(8.44, 73.87)	(10.91, 50.96)	(-50.23, 47.44)
Noise*Affiliation	52.75**	2.12	-6.61
	(18.22, 87.29)	(-17.60, 21.84)	(-59.47, 46.25)
Control*Affiliation	52.08**	-1.65	11.36
	(19.36, 84.79)	(-23.12, 19.82)	(-41.38, 64.10)
Observations	122	120	155

Note. This model describes Condition*Affiliation interaction effects for each of the three sites: USA, Czech Republic, and Japan. The religious condition was set as a reference category in each site.
†p < 0.1; *p < .05; **p < .01; ***p < .001

S1. Table D. Estimates with 95% CIs from beta regressions for the percentage of higher-paying side (right) claimed as having more dots.

	M1: Religiosity	M2: Ritual frq.	M3: Affiliation
Intercept	0.48	0.53	0.51
	(0.37, 0.60)	(0.43, 0.64)	(0.43, 0.60)
Secular	0.02	0.02	0.03
	(-0.12, 0.15)	(-0.10, 0.13)	(-0.06, 0.12)
Noise	0.07	-0.05	0.002
	(-0.07, 0.20)	(-0.16, 0.07)	(-0.08, 0.09)
Control	-0.06	-0.10†	-0.08*
	(-0.19, 0.07)	(-0.2, 0.01)	(-0.17, 0.01)
Sex	0.02	0.03	0.03
	(-0.04, 0.08)	(-0.03, 0.09)	(-0.03, 0.09)
Age	0.01**	0.01***	0.01**
	(0.003, 0.01)	(0.003, 0.01)	(0.003, 0.01)
Site: Czech Rep.	-0.25***	-0.26***	-0.28***
	(-0.3, -0.19)	(-0.31, -0.20)	(-0.33, -0.22)
Site: Japan	-0.08†	-0.10*	-0.11**
	(-0.15, 0.002)	(-0.17, -0.01)	(-0.18, -0.03)
Moderator	-0.03	-0.05**	-0.27***
	(-0.08, 0.02)	(-0.08, -0.01)	(-0.36, -0.14)
Secular*Moderator	0.03	0.03	0.20*
	(-0.04, 0.10)	(-0.02, 0.08)	(0.01, 0.34)
Noise*Moderator	-0.004	0.05*	0.28**
	(-0.07, 0.06)	(0.003, 0.10)	(0.10, 0.39)
Control*Moderator	0.02	0.04	0.24**
	(-0.05, 0.09)	(-0.01, 0.09)	(0.05, 0.37)
Observations	395	384	397

Note: Beta-regression coefficients were back-transformed from logit link; however, we kept the coefficients on the [0,1] interval. Moderator is either religiosity, ritual frequency, or religious affiliation, see model names.

†p < 0.1; *p < .05; **p < .01; ***p < .001

S1. Table E. Estimates with 95% CIs from linear mixed models for the percentage of higher-paying side (right) claimed as having more dots.

	M1: Religiosity	M2: Ritual frq.	M3: Affiliation
Intercept	29.38**	33.23***	30.55**
	(10.26, 48.50)	(14.34, 52.12)	(11.88, 49.22)
Secular	-1.31	-2.73	-0.80
	(-14.22, 11.61)	(-13.87, 8.41)	(-9.33, 7.74)
Noise	5.79	-5.85	-0.77
	(-7.33, 18.91)	(-16.89, 5.19)	(-9.05, 7.51)
Control	-6.10	-11.38*	-9.53*
	(-18.85, 6.66)	(-22.11, -0.66)	(-17.88, -1.18)
Sex	1.91	2.50	2.62
	(-3.64, 7.45)	(-3.06, 8.05)	(-2.85, 8.10)
Age	0.47†	0.52*	0.48*
	(-0.003, 0.94)	(0.06, 0.99)	(0.02, 0.94)
Moderator	-2.09	-4.14*	-25.86***
	(-6.74, 2.55)	(-7.30, -0.99)	(-39.89, -11.84)
Secular*Moderator	2.93	3.47	23.73*
	(-3.45, 9.30)	(-1.01, 7.94)	(5.55, 41.92)
Noise*Moderator	-1.41	4.49†	21.69*
	(-7.96, 5.13)	(-0.10, 9.08)	(2.61, 40.77)
Control*Moderator	0.79	3.88†	24.90*
	(-5.51, 7.10)	(-0.49, 8.25)	(5.93, 43.87)
Observations	395	384	397

Note: Moderator is either religiosity, ritual frequency, or religious affiliation, see model names. Site is not displayed because it is set up as a random effect in the model.

†p < 0.1; *p < .05; **p < .01; ***p < .001

S1. Table F. Estimates with 95% CIs from linear mixed models for the percentage of higher-paying side (right) claimed as having more dots.

	M1.1: Religiosity	M1.2: Religiosity	M2.1: Ritual frq.	M2.2: Ritual frq.	M3.1: Affiliation	M3.2: Affiliation
Intercept	61.30***	40.20**	67.23***	47.10***	61.65***	30.64**
	(49.09, 73.51)	(17.16, 63.23)	(55.67, 78.79)	(23.88, 70.33)	(51.88, 71.42)	(9.35, 51.94)
Secular	-1.06	1.93	-3.53	-1.65	-0.24	0.63
	(-13.37, 11.26)	(-10.99, 14.85)	(-14.11, 7.04)	(-12.69, 9.38)	(-8.43, 7.94)	(-7.99, 9.25)
Noise	3.39	3.40	-5.56	-5.40	-0.05	-0.98
	(-9.14, 15.92)	(-12.57, 19.37)	(-16.08, 4.96)	(-19.59, 8.79)	(-8.01, 7.91)	(-12.70, 10.74)
Control	-5.98	-	-11.75*	-	-8.22*	-
	(-18.17, 6.20)	-	(-21.93, -1.57)	-	(-16.32, -0.13)	-
Sex	3.55	4.46	3.97	4.96	3.99	5.63†
	(-1.82, 8.92)	(-2.11, 11.03)	(-1.36, 9.30)	(-1.52, 11.44)	(-1.32, 9.31)	(-0.83, 12.09)
Age	0.52*	0.62*	0.57*	0.65*	0.53*	0.61*
	(0.07, 0.98)	(0.09, 1.15)	(0.13, 1.01)	(0.14, 1.16)	(0.09, 0.97)	(0.10, 1.12)
Site: Czech Rep.	-23.53***	-24.24***	-24.43***	-25.67***	-25.15***	-26.12***
	(-30.46, -16.61)	(-34.39, -14.09)	(-31.35, -17.52)	(-35.68, -15.67)	(-32.10, -18.21)	(-36.06, -16.19)
Site: Japan	-5.28	-5.37	-6.60†	-7.34	-6.72†	-6.70
	(-12.66, 2.10)	(-15.12, 4.39)	(-14.16, 0.97)	(-17.30, 2.61)	(-14.06, 0.62)	(-16.29, 2.89)
Moderator	-2.54	-2.74	-4.53**	-4.57**	-22.35**	-27.15***
	(-7.01, 1.93)	(-7.45, 1.98)	(-7.56, -1.49)	(-7.72, -1.42)	(-36.37, -8.33)	(-41.74, -12.56)
Secular*Moderator	2.43	1.54	3.49	3.17	19.20*	22.01*
	(-3.66, 8.53)	(-4.78, 7.86)	(-0.78, 7.76)	(-1.21, 7.56)	(1.32, 37.07)	(3.59, 40.44)
Noise*Moderator	-0.23	0.88	3.99†	4.79*	17.09†	29.13**
	(-6.51, 6.04)	(-5.83, 7.60)	(-0.41, 8.38)	(0.12, 9.47)	(-1.66, 35.84)	(8.96, 49.29)
Control*Moderator	0.56	-	3.71†	-	18.46†	-
	(-5.55, 6.66)	-	(-0.48, 7.89)	-	(-0.18, 37.10)	-
Task difficulty	-7.71***	-6.16***	-8.16***	-6.69***	-7.58***	-5.76***
	(-10.45, -4.98)	(-9.58, -2.73)	(-10.89, -5.43)	(-10.11, -3.26)	(-10.28, -4.89)	(-9.11, -2.40)
Completion time	5.39**	22.39***	5.36**	21.71***	5.54***	23.24***
	(2.08, 8.71)	(13.62, 31.17)	(2.10, 8.61)	(13.12, 30.30)	(2.27, 8.82)	(14.70, 31.78)
Negativity	-	5.26*	-	4.97*	-	-0.25
	-	(0.69, 9.83)	-	(0.45, 9.48)	-	(-4.19, 3.70)
Positivity	-	2.99	-	2.68	-	0.90
	-	(-2.49, 8.47)	-	(-2.68, 8.03)	-	(-3.18, 4.98)
Tempo	-	0.47	-	0.54	-	4.36
	-	(-3.70, 4.64)	-	(-3.59, 4.67)	-	(-0.99, 9.70)
Impact	-	0.57	-	0.73	-	6.09**
	-	(-3.45, 4.59)	-	(-3.20, 4.66)	-	(1.58, 10.61)
Observations	389	280	379	273	391	282

Note: Moderator is either religiosity, ritual frequency, or religious affiliation, see model names. Completion time is the average completion time of trials that participants dishonestly reported subtracted from average completion time.

S1. Table F. Estimates with 95% CIs from linear mixed models for the percentage of higher-paying side (right) claimed as having more dots.

†p < 0.1; *p < .05; **p < .01; ***p < .001

S1. Table G. Estimates with 95% CIs from Ordinal Least Squares regressions for the percentage of higher-paying side (right) claimed as having more dots. Analysis of full sample.

	M1: Baseline	M2: Religiosity	M3: Ritual frq.	M4: Affiliation
Intercept	40.24***	42.85***	46.35***	45.66***
	(33.69, 46.80)	(32.72, 52.98)	(36.86, 55.84)	(38.16, 53.17)
Secular	3.04	-1.79	-2.89	-0.34
	(-4.06, 10.13)	(-14.09, 10.51)	(-13.49, 7.71)	(-8.47, 7.80)
Noise	1.26	3.16	-6.28	-1.90
	(-5.78, 8.31)	(-9.17, 15.49)	(-17.03, 4.47)	(-9.76, 5.96)
Control	-5.80	-5.81	-9.72†	-9.08*
	(-12.81, 1.20)	(-17.68, 6.05)	(-19.86, 0.42)	(-16.93, -1.24)
Sex	4.31	3.91	4.65†	4.63†
	(-0.82, 9.44)	(-1.35, 9.17)	(-0.63, 9.92)	(-0.59, 9.85)
Age	0.55*	0.54*	0.57*	0.55*
	(0.11, 0.99)	(0.09, 0.98)	(0.13, 1.02)	(0.10, 0.99)
Site: Czech Rep.	-29.75***	-30.05***	-30.52***	-31.99***
	(-35.93, -23.57)	(-36.46, -23.63)	(-37.03, -24.01)	(-38.52, -25.47)
Site: Japan	-10.64**	-11.11**	-12.07**	-13.37***
	(-17.45, -3.83)	(-18.26, -3.96)	(-19.54, -4.60)	(-20.50, -6.23)
Moderator	-	-1.31	-2.86†	-19.98**
	-	(-5.67, 3.04)	(-5.85, 0.14)	(-33.23, -6.73)
Secular*Moderator	-	2.90	3.15	18.13*
	-	(-3.16, 8.96)	(-1.07, 7.37)	(0.78, 35.49)
Noise*Moderator	-	-1.09	3.49	16.10†
	-	(-7.28, 5.10)	(-0.90, 7.88)	(-2.42, 34.62)
Control*Moderator	-	0.03	2.15	16.88†
	-	(-5.84, 5.90)	(-1.95, 6.26)	(-1.29, 35.04)
Observations	455	447	436	449

Note: Moderator is either religiosity, ritual frequency, or religious affiliation, see model names. The condition*moderator interactions represent the estimated differences between the slope of the moderator in the religious condition and moderator slopes in the other conditions.
†p < 0.1; *p < .05; **p < .01; ***p < .001

S1. Table H. Review of experiment designs for Lang & colleagues [1] and Nichols & colleagues [2]

	Lang and colleagues [1]	Nichols and colleagues [2]
Participants	University students in the Czech Republic and USA; general population in Maritius	University pool (majority students)
Location	USA, Czech Republic, Maritius	USA, Czech Republic, Japan
Experiment Task	The Matrix Task [3]	The Dots Game [4]
Dependent Variable	Percentage of claimed correctly solved matrices (Self-reported on paper)	Percentage of higher paying sides inaccurately claimed (Recorded digitally by the Dots Game)
Independent Variables	Instrumental music tracks: Religious, Secular, White Noise (Control)	Instrumental music tracks: No Music (Control), Religious, Secular, White Noise
Administration	Participants listened to musical track for two minutes, then solved the Matrix task and then completed questionnaire.	Participants played the Dots Game and then completed a questionnaire. For non-Control participants, music was played on loop for the duration of the Dots Game.
Moderators	Religiosity, ritual attendance, music recognition, musical characteristics of the stimuli, age, gender, religion, suspicion	Religiosity, ritual attendance, music recognition, musical characteristics of the stimuli, age, gender, religion and religious organization affiliation (religious affiliation), suspicion, perceived difficulty of task, level of distraction, previous experience with the Dots Game
Control variables		
Compensation	$0.50 per reported correctly solved matrix, up to $10 in total.	$0.05 (or $0.005) for selecting the right (left) side having more dots, up to $10.00 in total and $4.60 with complete accuracy.
Musical Tracks	See S1 Table I for a review of the musical tracks tested in this research.	

S1. Table I. Experiment stimuli and pre-tested music tracks by site

	USA	Czech Republic	Japan
Religious Stimulus	J. S. Bach - BWV 147 Jesu joy of man's desiring*	J. S. Bach - Ave Maria (Gounod's interpretation)*	Anonymous - Enteraku (Clip #2, 60-120 seconds)
Secular Stimulus	J. S. Bach - BWV 140 Sleepers Awake*	Tchaikovsky - Romance for piano in F Minor, Op. 5*	Yatsuhashi Kengyo - Rokudan-no-sirabe
White Noise		Brownian noise	
Pre-Tested Religious Stimuli	J. S. Bach - Ave Maria (Gounod's interpretation) Jan Zwart - Toccata Psalm 146 J. S. Bach - BWV 147 Jesu joy of man's desiring J.S. Bach - BWV 29 We thank thee, God		Anonymous - Enteraku (Clip #1, 0-60 seconds) Anonymous - Enteraku (Clip #2, 60-120 seconds) Anonymous - Gagaku #3 Anonymous - Gagaku #4
Pre-Tested Secular Stimuli	Max Richter - H In New England P. I. Tchaikovsky - Romance for piano in F Minor, Op. 5 Yann Tiersen - Comptine d'Un Autre Été J. S. Bach - BWV 140 Sleepers Awake		Kengyo Yoshizawa II - Chidori-no-kyoku Michio Miyagi - Concerto No. 3 Tegoto Yatsuhashi Kengyo - Rokudan-no-sirabe Anonymous – Koto #4

Note: All musical tracks did not include vocals (instrumental only). Musical tracks were pre-tested on Amazon's Mechanical Turk in the USA, a student population in the Czech Republic, and Lancer in Japan. Two clips of the religious track Enteraku were tested; Clip #1 contained the first minute of track, whereas Clip #2 contained the second minute (60-120 seconds) of the track. All stimuli are available upon request.

* - Indicates this exact track was used for the same sites in Lang et al., [1].

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6.6. Study 6

Lang, M., Purzycki, B. G., Apicella, C. L., Atkinson, Q. D., Bolyanatz, A., Cohen, E., Handley, C., Klocová, E. K., Lesorogol, C., Mathew, S., McNamara, R. A., Moya, C., Placek, C. D., Soler, M., Vardy, T., Weigel, J. L., Willard, A. K., Xygalatas, D., Norenzayan, A., & Henrich, J. (2019). Moralizing gods, impartiality, and religious parochialism across 15 societies. Proceedings of the Royal Society B, 286(1898), 1–10. https://doi.org/doi.org/10.1098/rspb.2019.0202

Abstract
The emergence of large-scale cooperation during the Holocene remains a central problem in the evolutionary literature. One hypothesis points to culturally evolved beliefs in punishing, interventionist gods that facilitate the extension of cooperative behaviour toward geographically distant co-religionists. Furthermore, another hypothesis points to such mechanisms being constrained to the religious ingroup, possibly at the expense of religious outgroups. To test these hypotheses, we administered two behavioural experiments and a set of interviews to a sample of 2228 participants from 15 diverse populations. These populations included foragers, pastoralists, horticulturalists, and wage labourers, practicing Buddhism, Christianity, and Hinduism, but also forms of animism and ancestor worship. Using the Random Allocation Game (RAG) and the Dictator Game (DG) in which individuals allocated money between themselves, local and geographically distant co-religionists, and religious outgroups, we found that higher ratings of gods as monitoring and punishing predicted decreased local favouritism (RAGs) and increased resource-sharing with distant co-religionists (DGs). The effects of punishing and monitoring gods on outgroup allocations revealed between-site variability, suggesting that in the absence of intergroup hostility, moralizing gods may be implicated in cooperative behaviour toward outgroups. These results provide support for the hypothesis that beliefs in monitoring and punitive gods help expand the circle of sustainable social interaction, and open questions about the treatment of religious outgroups.

Contributions

Conceptualization	10%
Methodology	10%
Data collection	10%
Data curation	90%
Statistical analysis	90%
Supervision	70%
Writing and editing	90%
Project administration	70%

6.7. Study 7

Purzycki, B. G., & Lang, M. (2019). Identity fusion, outgroup relations, and sacrifice: A cross-cultural test. Cognition, 186, 1–6.

Abstract
Identity fusion theory has become a popular psychological explanation of costly self-sacrifice. It posits that while maintaining one's own individual identity, a deep affinity with one's group can contribute to sacrifice for that group. We test this and related hypotheses using a behavioral economic experiment designed to detect biased, self-interested favoritism among eight different populations ranging from foragers and horticulturalists to the fully market-integrated. We find that while individuals favor themselves on average, those with higher ingroup fusion sacrifice more money to other members of their ingroup who are unable to reciprocate. We also find that positive outgroup relations has a similar effect. Additionally, we assess a recently-posited interaction between ingroup and outgroup relations and show no consistent effect at the individual or sub-sample levels.

Contributions

Conceptualization	40%
Methodology	40%
Data collection	0%
Data curation	0%
Statistical analysis	40%
Supervision	50%
Writing and editing	50%
Project administration	50%

6.8. Study 8

Shaver, J. H., Lang, M., Krátký, J., Klocová, E. K., Kundt, R., & Xygalatas, D. (2018). The boundaries of trust: Cross-religious and cross-ethnic field experiments in Mauritius. Evolutionary Psychology, 16(4), 1–15. https://doi.org/10.1177/1474704918817644

Abstract
Several prominent evolutionary theories contend that religion was critical to the emergence of large-scale societies and encourages cooperation in contemporary complex groups. These theories argue that religious systems provide a reliable mechanism for finding trustworthy anonymous individuals under conditions of risk. In support, studies find that people displaying cues of religious identity are more likely to be trusted by anonymous coreligionists. However, recent research has found that displays of religious commitment can increase trust across religious divides. These findings are puzzling from the perspective that religion emerges to regulate coalitions. To date, these issues have not been investigated outside of American undergraduate samples nor have studies considered how religious identities interact with other essential group-membership signals, such as ancestry, to affect intergroup trust. Here, we address these issues and compare religious identity, ancestry, and trust among and between Christians and Hindus living in Mauritius. Ninety-seven participants rated the trustworthiness of faces, and in a modified trust game distributed money among these faces, which varied according to religious and ethnic identity. In contrast to previous research, we find that markers of religious identity increase monetary investments only among in-group members and not across religious divides. Moreover, out-group religious markers on faces of in-group ancestry decrease reported trustworthiness. These findings run counter to recent studies collected in the United States and suggest that local socioecologies influence the relationships between religion and trust. We conclude with suggestions for future research and a discussion of the challenges of conducting field experiments with remote populations.

Contributions

Conceptualization	30%
Methodology	30%
Data collection	10%
Data curation	50%
Statistical analysis	100%
Supervision	40%
Writing and editing	40%
Project administration	10%

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The Boundaries of Trust: Cross-Religious and Cross-Ethnic Field Experiments in Mauritius

John H. Shaver[1], Martin Lang[2,3], Jan Krátký[3], Eva Kundtová Klocová[3], Radek Kundt[3], and Dimitris Xygalatas[4]

Abstract

Several prominent evolutionary theories contend that religion was critical to the emergence of large-scale societies and encourages cooperation in contemporary complex groups. These theories argue that religious systems provide a reliable mechanism for finding trustworthy anonymous individuals under conditions of risk. In support, studies find that people displaying cues of religious identity are more likely to be trusted by anonymous coreligionists. However, recent research has found that displays of religious commitment can increase trust across religious divides. These findings are puzzling from the perspective that religion emerges to regulate coalitions. To date, these issues have not been investigated outside of American undergraduate samples nor have studies considered how religious identities interact with other essential group-membership signals, such as ancestry, to affect intergroup trust. Here, we address these issues and compare religious identity, ancestry, and trust among and between Christians and Hindus living in Mauritius. Ninety-seven participants rated the trustworthiness of faces, and in a modified trust game distributed money among these faces, which varied according to religious and ethnic identity. In contrast to previous research, we find that markers of religious identity increase monetary investments only among in-group members and not across religious divides. Moreover, out-group religious markers on faces of in-group ancestry decrease reported trustworthiness. These findings run counter to recent studies collected in the United States and suggest that local socioecologies influence the relationships between religion and trust. We conclude with suggestions for future research and a discussion of the challenges of conducting field experiments with remote populations.

Keywords

cooperation, ancestry, Mauritius, religion, trust

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Cooperation is an essential component of human life, and identifying the cultural features that support or inhibit cooperation is one of the most fundamental questions facing social science (Hill, Barton, & Hurtado, 2009). Building upon a long history of general social theory, evolutionary researchers have recently looked to religion as a critical contributor to the high levels of cooperation that are characteristic of human societies (Purzycki, Kiper, Shaver, Finkel, & Sosis, 2015; Shaver, Purzycki, Sosis, 2016).

The majority of contemporary evolutionary theories of religion assume that religious systems promote within-group trust and cooperation, both in small-scale societies and in complex social settings where interacting coreligionists are often anonymous (Norenzayan & Shariff, 2008; Purzycki et al., 2016). Moreover, religion's positive influence on sociality is thought to have contributed to the emergence of large-scale societies over the course of the past 12,000 years and to be critical to the

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stability of complex social organization in most contemporary societies (Norenzayan et al., 2016).

While a considerable body of work has documented the positive association between religious belief and within-group trust and cooperation among anonymous coreligionists (e.g., Lang et al. 2016; Power, 2017; Purzycki & Arakchaa, 2013; Tan & Vogel, 2008; Xygalatas et al., 2013), research has yet to critically examine the dynamics of trust and religious beliefs, behaviors, and institutions across various religious systems and ethnicities representative of the intense social fragmentation that are characteristic of all large-scale societies (Hall, Cohen, Meyer, Varley, & Brewer, 2015; McCullough, Swartwout, Shaver, Carter, & Sosis, 2016; Turchin, 2013). Understanding the ways in which religious systems exacerbate or mitigate social divisions is critical to a more accurate understanding of religion's role in the emergence and stability of large-scale cooperation that breaks parochial boundaries. The dynamics of religion, trust, and cooperation are also important to understanding the cultural traits of the peaceful multiethnic and multireligious societies of today.

Here, we begin by describing the obstacles to large-scale cooperation, particularly one-shot interactions between anonymous agents who are unlikely to have opportunities to reciprocate in the future and therefore cannot be based on the presumption of a future ongoing relationship. Subsequently, we summarize theories which argue that religion provides solutions to these barriers. We describe the results of studies that test hypotheses derived from these theories, noting current gaps in our understanding—namely, whether or not, and under what conditions, religious identity can be expected to motivate trust across social boundaries. We then fill in these gaps with experimental data drawn from Mauritius which examine trust within and between Christians, Hindus, Afro-Mauritians, and Indo-Mauritians.

The Cooperative Affordances of Religions

In small-scale societies, human cooperative interactions usually take place in the context of delineated social groupings (Hill et al., 2009; Nowak & Highfield, 2011). Group resources, such as those acquired through cooperative hunting or territorial defense, provide net benefits that often exceed those acquired individually (Pulliam & Caraco, 1984; Ridley, 1996; Tiger, 1969). Individual members of groups are often better off when everyone contributes to collective resources. However, there are several obstacles to achieving successful cooperation (Cronk & Leech, 2012). Critically, because individuals face incentives to cheat the group or to extract resources without commensurate investment, successful cooperation is difficult to achieve and can deteriorate rapidly (Frank, 1988, 2001; Schelling, 1980, 2001)

In the face-to-face social environments that characterize most of human history, unrelated individuals can avoid exploitation (a) by biasing cooperation toward individuals with whom cooperation has been successful in the past (Trivers, 1971), (b) by favoring cooperation with those who hold reputations as reliable cooperators (Alexander, 1987; Nowak & Sigmund, 1998, 2005; Panchanathan & Boyd, 2003, 2004), and/or (c) if threats of punishment against defectors are credible (Fehr & Fischbacher, 2003). However, large-scale social settings exacerbate problems of exploitation, where social histories, reputation, and/or punishment mechanisms are unavailable and/or unreliable (Boyd & Richerson, 1988). Cooperation in large and anonymous contexts, where reliable information is difficult to acquire, and the risks of exploitation are high, therefore requires additional mechanisms to encourage trust between potential cooperators (Frank, 1988; Johnson, 2005; Schelling, 1980).

There is an emerging consensus among evolutionary scholars that religion increases cooperative affordances between anonymous coreligionists and therefore may have been critical to the emergence of large-scale cooperation (e.g., Irons, 2001; Johnson, 2016; Norenzayan, 2013; Watts et al., 2015). While this conjecture is generally agreed upon (Shaver, Purzycki, Sosis, 2016), at least two outstanding questions persist. First, the psychological mechanisms that motivate trust and cooperation between anonymous coreligionists remain obscure; and second, it is unknown whether these mechanisms also encourage cooperation between anonymous individuals with different religious and/or ethnic identities. We here describe and investigate one possible mechanism—a *coalitional recognition hypothesis*—which stresses the importance of markers of religious identity for providing religious individuals with a reliable mechanism to select anonymous coreligionists from out-group members (Bulbulia, 2004; Irons, 2001; Purzycki et al., 2016; Sosis, 2006). That is, by signaling commitment to monitoring and punishing supernatural agents that foster cooperation, religious individuals can reliably assort and enter into trust-based interactions even in otherwise anonymous settings.

Specifically, the coalitional recognition hypothesis contends that across most settings, the reliable communication of group affiliation is expected to increase trust and cooperative affordances among otherwise anonymous community members (Irons, 2001; Sosis, 2005). The communication of religious group membership can occur through several modalities, but particularly important to most religious traditions are displays of ritual behavior, the public observance of religious norms and taboos, and/or the adornment of religious badges (Sosis, 2006). Religious badges are those observable physical manifestations of religious group membership that clearly advertise a person's affiliation to a specific religious group. Religious badges range widely across religious traditions and vary in their permanence (e.g., from clothing to scarring), but some familiar examples include the hijab worn by some Muslim women, the yarmulke worn by Jewish men, crosses worn by Christians, or tilak (a white ash mark on the forehead; see below) worn by Hindus.

Speculation based on anecdotal evidence suggests that under certain conditions, trust might also be extended to any individual who signals commitment to a moralizing deity, even those deities who are associated with religious out-groups (Norenzayan, 2013, p. 65; Sosis, 2005). That is, an *inferred supernatural monitoring hypothesis* posits that indications of

belief in *any* omniscient supernatural entity who punishes uncooperative behaviors can be used as a cue that an anonymous person can be trusted. In other words, since religious people believe that they will be punished if they fail to cooperate, one can reliably assume that they are trustworthy.

The coalitional recognition hypothesis and the inferred supernatural monitoring hypothesis overlap to the extent that both are based on the premises that (a) the communication of shared supernatural belief leads to cooperative affordances, (b) the members of religious groups trust anonymous coreligionists, and (c) those more committed to the group are more likely to be more trusting of anonymous coreligionists. Indeed, studies consistently find that religious people are more cooperative with one another than secular individuals, even under anonymous conditions (e.g., Sosis & Ruffle, 2003), and recent cross-cultural research finds that belief in omniscient and punishing gods is associated with increased cooperative tendencies toward anonymous and distant coreligionists (Bulbulia & Mahoney, 2008; Purzycki et al., 2016). There also appears to be a positive relationship between a person's overall commitment to their religion (i.e., a person's religiosity) and the amount of trust allocated to anonymous coreligionists (Paciotti et al., 2011). The coalitional recognition hypothesis and the inferred supernatural monitoring hypothesis also overlap in that both see signals indicating shared belief as inducing the highest levels of trust.

The coalitional recognition hypothesis and the inferred supernatural monitoring hypothesis make competing predictions, however, with respect to trust allocated to the anonymous members of religious *out*-groups. The coalitional recognition hypothesis argues that indications of religious identity increase trust and motivate cooperation primarily among in-group members. By contrast, the inferred supernatural monitoring hypothesis posits that both in- *and* out-group members find as trustworthy those individuals who believe they are being watched by supernatural entities who punish uncooperative behavior.

In support of the inferred supernatural monitoring hypothesis, several studies find that individuals perceived as religious are trusted more than secular individuals, regardless of whether or not the rater is a coreligionist (Bailey & Doriot, 1985; Bailey & Garrou, 1983; Bailey & Young, 1986; Galen, Smith, Knapp, & Wyngarden, 2011; Gervais, Shariff, & Norenzayan, 2011; Orbell, Goldman, Mulford, & Dawes, 1992; Paciotti et al., 2011; Purzycki & Arakchaa, 2013; Tan & Vogel, 2008). Moreover, recent work suggests that the communication of religious commitment can even increase trust across religious group boundaries. For example, individuals adorning Christian religious badges (Christian crosses or Catholic Ash Wednesday ashes) were rated as more trustworthy than individuals not adorning these badges by both Christian and non-Christian American undergraduates (McCullough et al., 2016). Similarly, Hall and colleagues (2015) found that among Christian Americans, ratings of trust in both Christians and Muslims increased the more the targets of each religion were depicted as committed to those religions. The authors of both of these studies speculated that these effects may not be universal and implied the need for research with more diverse populations.

Here, we test the coalitional recognition hypothesis and then examine these results against the inferred supernatural monitoring hypothesis in the non-Western and diverse population of Mauritius. Moreover, we posit that there are at least three reasons why people may not universally grant higher levels of trust to those communicating any supernatural beliefs. Instead, it is likely that the relationships between religious identity and trust will vary due to (a) the rigidity of social boundaries which are to some degree orthogonal to religion (such as ancestry), (b) socioecological conditions, and (c) the way that trust is assessed.

Cross-Cultural Variation in Religion and Trust and Previous Measurement Issues

All large-scale societies, and major contemporary religions, are internally heterogeneous in several respects, and these heterogeneities are often impediments to cooperation. For example, experimental studies consistently find that people trust those of different ancestries less than members of their own ancestry (e.g., Buchan & Croson, 2004; Fershtman & Gneezy, 2001). While some studies have found that religious badges increase trust regardless of shared religious membership, it is unknown whether the positive effects of religion on trust can offset the divisiveness of ethnic differences.

Indeed, there are reasons to suspect that out-group religious markers on faces of in-group ancestry will not be found as more trustworthy, as the inferred supernatural monitoring hypothesis predicts. The *black sheep effect* refers to findings that in-group members who deviate from group norms (e.g., clothing, habits, behaviors) are judged more harshly than out-group members (with likable in-group members judged more positively; Marques &Yzerbyt, 1988; Marques, Yzerbyt, & Levins, 1988). In the context of the communication of religious identity, a black sheep effect may be the result of an inference that people who indicate shared religious membership, but who violate other normative expectations (such as might be the case of an atypical ancestry for that religion), are potential free riders and are more likely to threaten group cooperative resources.

Moreover, findings that religions increase trust across parochial boundaries run counter to cultural evolutionary theories which posit that religions play a crucial role in intergroup competition (Norenzayan et al., 2016) and encourage in-group cooperation during intergroup conflict over resources and/or values (Atran & Ginges, 2012; Choi & Bowles, 2007). Indeed, previous studies indicate that religious affiliation predicts derogation of religious out-groups (Blogowska & Saroglou, 2011; Bushman, Ridge, Das, Key, & Busath, 2007) and participation in collective religious activities predict support for suicide attacks during religious conflicts (Ginges, Hansen, & Norenzayan, 2009). How can we align this evidence with studies reporting cooperative behaviors toward religious outgroups?

A possible reason for these contradictory findings may stem from the fact that the majority of previous research supporting the inferred supernatural monitoring hypothesis was conducted among Western populations that exhibit unusually high rates of baseline trust (Johnson & Mislin, 2011) and are otherwise psychologically peculiar (Henrich, Heine, & Norenzayan, 2010; Sears, 1986). In other words, previous findings that religious people are judged as more trustworthy regardless of shared group membership may not be generalizable beyond Western populations, as these populations are unique in the allocation of trust. However, if in non-Western settings indications of supernatural belief are found to transcend parochial boundaries, then such findings would lend support to the inferred supernatural monitoring hypothesis and suggest these effects may be universal.

There are reasons to expect that inferred supernatural monitoring hypothesis may not universally hold. Specifically, we expect regional path dependencies of conflict and peace to influence judgments of the trustworthiness between religious groups (Shaver, Troughton, Sibley, & Bulbulia, 2016); that is, we expect socioecological variance in the degree of trust granted to out-group members. In areas of high levels of historical conflict over resources, for example, commitment to one religion can be expected to reduce trust toward the members of other religions (e.g., Ginges, Hansen, & Norenzayan, 2009). Conversely, in environments relatively free of conflict, such as the Western societies where the majority of previous research has been conducted, religious badges can be expected to increase generalized trust both within and across divides (Hall et al., 2015; McCullough et al., 2016; but see Ginges, Sheikh, Atran, & Argo, 2016). Moreover, recent research found that while atheists are universally distrusted, such biases are lower in Western societies (Gervais et al., 2017). However, if religious identity (relative to no indication of religiosity) is ineffective at motivating trust across group lines in non-Western settings, then such a finding would suggest that there is something unique to the local ecology that is influencing the way that people respond to signals of religious membership.

Finally, there is a difference between a general perception of the trustworthiness of anonymous others, as assessed by survey measures, and the high levels of trust required for successful economic cooperation in anonymous settings (Bonnefon, Hopfensitz, & De Neys, 2017). Indeed, there is evidence that the neuropsychological mechanisms involved in attitudinal ratings of interpersonal relationships (as assessed in surveys) may be distinct from those motivating economic exchange between individuals (Lang, Bahna, Shaver, Reddish, & Xygalatas, 2017), and some studies have found differences between attitudinal ratings and behavioral measures of trust (e.g., Johansson-Stenman, Mahmud, & Martinsson, 2009). Specifically, trust in economic settings involves a risk that initial trust of cooperation is returned (Theilmann & Hilbig, 2017), while attitudinal measures of trust are devoid of such risk.

Although previous studies have used both attitudinal and economic-based measures of trust in the same design (e.g., McCullough et al., 2016), they are typically both conceptualized as assessing the same underlying construct of trust. We see these measures as assessing different kinds or at least degrees of trust and expect that they might not always yield the same results across all contexts.

Here, we test for the cooperative affordances and impediments of religious markers by investigating attitudinal trust and risky economic investment within and across religious and ethnic lines in Mauritius, a country of considerable diversity, high levels of religious pluralism, and a complicated history of ethno-religious relations.

Ethnographic Setting: Mauritius

Mauritius is a small island nation located in the Indian Ocean, 450 miles east of Madagascar. Its 1.3 million inhabitants comprise an ethnic and religious diversity that provides an ideal setting to investigate how religion and ancestry affect between- and within-group trust (Xygalatas et al., 2016). Mauritius has no indigenous population, as all of its current inhabitants came though immigration, whether voluntary or forced, from the 18th century onward. People of African and Malagasy descent, whose ancestors were brought to Mauritius to work as slaves on sugarcane plantations, and members of the Indian Diaspora, whose ancestors arrived in Mauritius as indentured laborers in the 19th century, make up the two largest ethnic populations (Eriksen, 2002). In addition to Indo- and Afro-Mauritians, there are smaller populations of Chinese and European (mostly French) ancestry.

Mauritius is a religiously plural society with freedom of religion granted under the constitution. Mauritius accommodates a variety of belief systems and has national holidays set aside every year for each of the Christian, Hindu, Islamic, and Chinese traditions. While data on ancestry are not collected by the national census, the government does track religious group identification. The majority of Mauritians are Hindus (49%), with Christians (32%) and Muslims (17%) making up the second and third largest religious groups, respectively (Statistics Mauritius, 2012). Religion and ancestry are heavily overlapping and often conflated in Mauritius. Afro- and Franco-Mauritians are overwhelmingly Christian while those of Indian descent are overwhelmingly Hindu. As a result, people of Indian descent are often referred to as Hindus, and those of African descent are known as Creoles but are most frequently referred to as Chrétiens (Christians).

Mauritius is often considered as an example of a successful multicultural country (Eriksen, 2002); yet, the country is not free of social inequality. In general, Creoles are relatively marginalized and have lesser economic and political standing, while members of the Hindu population enjoy much higher socioeconomic status (Eriksen, 2004). Hindus are more likely to be agriculturalists or work in public service, while Creoles are more likely to work in manual labor. These disparities may be attributed to a variety of historical and sociopolitical factors. Since most Mauritian Creoles are descendants of slaves, they do not have access to accumulated inherited wealth, which is

known to define socioeconomic status as well as occupation for many generations (Barone & Mocetti, 2016). Additionally, Afro-Mauritians are a minority, and as Mauritians traditionally vote along ethno-religious lines, they often do not have enough voting power to promote their interests. Since independence in 1968, the position of prime minister has always been held by a member of the Hindu majority (the only exception was a Franco-Mauritian who served for 2 years without being elected after his Hindu ally resigned).

Moreover, although ethnic violence is rare (though not absent) in Mauritius, racial stereotypes and distrust are abundant across groups (Eriksen, 1998). Indeed, prior to the third stage of data collection described below, many Hindus refused to sit in chairs where Creoles had been seated. On the official, but superficial ethnic and religious harmony of Mauritius, the anthropologist Thomas Hylland Eriksen (2004) stated,

> The official multiculturalism of the country is a foil concealing systematic discrimination against particular ethnic groups [Creoles]: they are granted equal symbolic significance, cultural rights, and formal equality, but are discriminated against in informal, nearly invisible, but no less efficient ways. (pp. 92, 93)

We utilize this context of both multiculturalism and social inequality/ethnic distrust to examine the dynamics of religious signaling within and across ethnic, social, and religious boundaries. We here generate and evaluate two basic hypotheses. First, religious badges encourage risky economic investment in anonymous coreligionists from the same ethnic group (Hypothesis 1). That is, in comparison with shared ethnic group members without badges, those with religious badges will be trusted more. This is consistent with the coalitional recognition hypothesis, which predicts that in-group members expressing their commitment to moralizing gods are found trustworthy. However, the coalitional recognition hypothesis also predicts that (Hypothesis 2a) people from different ethnic groups who adorn in-group religious badges are trusted less than individuals of the same ethnic group with in-group religious badges (owing to a black sheep effect) and that (Hypothesis 2b) people from different ethnic groups adorning out-group religious badges are trusted less than individuals who share a rater's ancestry. Note that the inferred supernatural monitoring hypothesis makes opposing predictions for Hypotheses 2a and 2b and suggests that people adorning religious badges will be trusted more regardless of shared religion or ancestry. To examine these hypotheses, we assess attitudinal and behavioral trust and vary a Christian religious badge (a cross) and a Hindu religious badge (a tilak) across individuals of both Afro-Mauritian and Indo-Mauritian descent. To allay uncertainty from previous studies regarding whether attitudinal ratings of trust are mirrored by risky economic decisions, we used both measures in our study, hypothesizing that religious badges would encourage higher trustworthiness ratings regardless of share ethnic/religious identity, but that only shared religious/ethnic identity would encourage risky economic investments in anonymous coreligionists.

Data and Methods

Photo Collection

Data collection was carried out in three stages, with each stage at a different location in Mauritius. We first took photographs of 24 individuals in La Gaulette, a village located in the Southern portion of the island, with a population of 2,315 at the most recent census in 2011 (Mauritius, 2012). We recruited 12 Afro-Mauritian and 12 Indo-Mauritian males between the ages of 25 and 35. We asked each man to wear the same white shirt and stand against a white wall while we took two photographs. Each man was photographed with and without a necklace band and gave permission to use their images. We used only male targets to control for differences in the trustworthiness ratings of faces attributable to sex (Scharlemann, Eckel, Kacelnik, & Wilson, 2001).

Photographs were digitally modified in Adobe Photoshop to standardize appearance. First, noticeable light reflections (from sunlight) were masked so that skin color was equalized on all parts of the face. Additionally, each face was standardized so that the head-to-canvas ratio was similar across all images.

Photo Selection

In order to select images for experimentation, we went to a new location, where people were not able to identify these individuals. Specifically, we asked both Afro- and Indo-Mauritian informants from the West-Central town of Quatre Bornes (population = 77,505) to rate these faces for their trustworthiness, attractiveness, and dominance/submission. Based on these ratings, we eliminated faces with high or low values on attractiveness and submissiveness/dominance, as both of these dimensions are known to influence perceptions of trustworthiness (e.g., Stewart et al., 2012; Willis & Todorov, 2006).

Faces (without the necklaces) were presented to participants on laptop computers with software we designed using the Adobe Flash Version 8 software bundle. We administered all instructions in the local Creole language. Fifteen participants rated only the Afro-Mauritian faces and 15 other participants rated only the Indo-Mauritian faces. Participants were shown each face, one at a time, in randomized order, and asked to rate each face on attractiveness, submissiveness/dominance, and trust. To assess participants' perceptions of each face's trustworthiness, we modified 12 items from the Propensity to Trust Scale (Glaeser, Laibson, Scheinkman, & Soutter, 2000; McCullough et al., 2016). Rather than self-ratings (as in the Propensity to Trust Scale), participants rated the trustworthiness of each target (e.g., "[I] listen to my conscience" was changed to "This person listens to his conscience"). Participants evaluated each face by choosing a point along an Unnumbered Sliding Scale from "strongly disagree" to "strongly agree" for each of the 12 items. The software recorded participant responses to a continuous 100-point Visual-Analog

Scale, where 0 equaled *strongly disagree* and 100 equaled *strongly agree*. These 12 trustworthiness questions evinced high reliability ($\alpha = .92$). This scale has previously been found to correlate with trusting behavior as measured in the trust game described below (McCullough et al., 2016).

Similarly, participants rated each face on an Unnumbered Continuous Scale by clicking their mouse anywhere along a line that ranged from *very unattractive* (0), to *neither attractive nor unattractive* (50), to *very attractive* (100), and from *very submissive* (0), to *neither submissive nor dominant* (50), to *very dominant* (100). From this screening exercise we selected 10 faces, 5 Afro-Mauritian and 5 Indo-Mauritian, each with a mean submissiveness/dominance, attractiveness, and trust ratings between 50 and 60.

Table 1. Participant Counts and Descriptive Statistics.

Religious Affiliation (*n*)		Ancestry (*n*)		Sex (*n*)	
Christian	47	Afro-Mauritian	41	Males	51
Hindu	50	Indo-Mauritian	34	Females	46
		Other	22		
Sum	97	Sum	97	Sum	97
Age (Six Categories by 10 Years)		Religiosity (1–5)		Ritual Frequency (0–5)	
Mode	18–24	Mean	2.85	Mean	2.29
SD		*SD*	0.95	*SD*	1.46

Note. Analyses in the main text are based on participants' religious affiliation. In the Supplemental Material, we show analyses only for participants with self-declared Afro- and Indo-Mauritian ancestry, excluding other and mixed ancestry.

Participants

To assess the effects of religious badges on perceptions of trustworthiness among in-group and out-group members, we moved to a third location, thus again ensuring that participants would be naive to the identities of the men in the photographs. Specifically, we conducted an experiment in Pointe aux Piments, a village on the North-West coast of the island with a population of 9,079 (Statistics Mauritius, 2012). We collected data from 53 males and 47 females, modal age 18 and 24 (60% of our sample). We excluded two participants whose religious affiliation did not match religions used in our study (one Muslim and one not affiliated) and one participant who did not operate the computer program correctly. We grouped participants based on their self-reported religious affiliation only (i.e., dichotomizing the sample into Christians and Hindus). Warrant for dichotomizing the sample according to religion comes from the recognition that although ancestry and religion are not always coupled in Mauritius (there was one Afro-Mauritian Muslim and one Indo-Mauritian who did not affiliate to any religion in our sample), the majority of individuals of a specific religious tradition are of the same ancestry. If a person is Christian, for example, most of their fellow religious in-group members will be of African descent. Moreover, since ancestry is sometimes mixed in Mauritius, we presume that religious affiliation is a more salient coalition signal for unmatched participants (i.e., those individuals who are not Indo-Mauritian Hindus or Afro-Mauritian Christians). Our final sample comprised 97 participants, of which 47 indicated that they were Christian and 50 indicated that they were Hindu.

However, as noted above, not all participants reported ancestry that traditionally matches the two religious affiliations used in this study. While we recruited participants equally from Indo- and Afro-Mauritian communities, of the 97 participants in our sample, 15 people reported mixed ancestry, 3 Sino-Mauritian ancestry, 3 Franco-Mauritian, and 1 participant reported "other" ancestry. In the main text, we treat religious affiliation as more salient and use the full sample of 97 participants (possibly neglecting some nuanced effects of mixed ethnicities); however, we present an alternative analysis in the supplement, removing data from those who did not self-identify as belonging to the African-Mauritian or Indo-Mauritian ethnic groups, and the religion with which they are most typically associated (i.e., Afro-Mauritian Christians and Indo-Mauritian Hindus). There were no practically important differences between the results from this reduced sample and the results presented in the main text. See Table 1 for an overview of participant counts and descriptive statistics.

Experiment

We ran experiments in a large square tent set up in a public park for the purposes of data collection. To test multiple participants at the same time, the tent was separated into quadrants, each with its own doorway. We placed a table, chair, and laptop in each quadrant. Local assistants randomly sampled from Indo-Mauritian and Afro-Mauritian ethnic communities, told them that they were taking part in a study on economic decision-making, and obtained informed consent. The experiment was conducted in the local language and run by local assistants who were Indo-Mauritian, but from a different location on the island and unknown to participants. Waiting participants were corralled outside of the experimental tent to prevent collusion from participants who had already completed the experiment. To limit disclosure of study design, we collected all data within 3 days.

The experiment employed an elaborated version of the software used in the screening task. Participants were first screened for their ability to use a computer and asked to operate a computer mouse to mark the current time on a scale in front of a research assistant. If a participant passed this test, the she or he was shown 10 faces: 5 Afro-Mauritian and 5 Indo-Mauritian faces selected from the screening procedure described above. To control for contrast effects, we randomly assigned participants to one of the four conditions. In all four conditions, 2 of the 10 faces wore a religious badge. For the Christian badge, we added a simple black wooden cross to the necklace band. For the Hindu religious badge, we used a white tilak taken from a photograph of a man attending a Hindu temple in Quatres Bournes. Tilak vary, and differences in color and the orientation of markings indicate sectarian membership. However, white tilak do not demarcate the wearer as

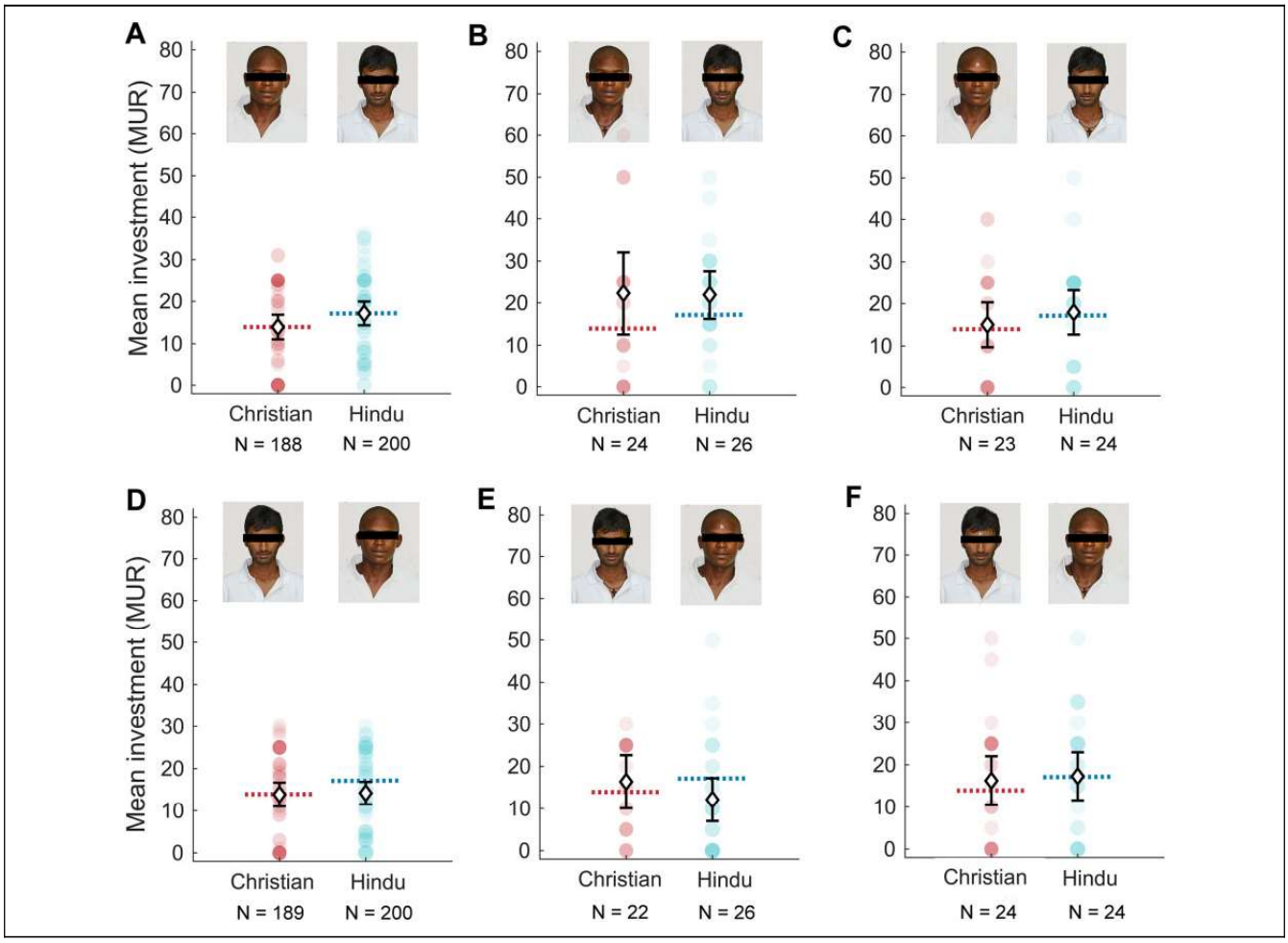

Figure 1. Mean investments with 95% confidence interval and face illustrations. Each plot illustrates a type of face into which Christian and Hindu participants were, respectively, investing. The numbers below *x*-axis illustrate how many times each type of face was viewed. Horizontal lines are mean investments for in-group faces with no religious badge (this served as a reference category in our regression models; red is Christian participants, blue is Hindus). (A) Investments in in-group faces with no religious badge. (B) Investments in in-group faces with an in-group religious badge. (C) Investments in in-group faces with an out-group religious badge. (D) Investments in out-group faces with no religious badge. (E) Investments in out-group faces with an in-group religious badge. (F) Investments in out-group faces with an out-group religious badge. Note that the manipulated religious badges are a white ash mark (tilak) on a forehead or a wooden cross on a necklace. Participants' eyes are covered to protect their anonymity.

belonging to a sect and can be worn by any Hindu. In general, tilak communicate a person's religious commitment and involvement in the Hindu community. Thus, both badges we used indicated general religious affiliation (Christian or Hindu) without any indication of sectarian membership (e.g., Catholic or Marathi). We digitally added either a tilak or a cross with Adobe Photoshop (see Figure 1 for illustration).

Each participant viewed faces, two of which were modified to include a religious badge. Crosses were added to the images in which men were wearing the necklace in the initial photographs. One third of participants viewed only stimuli with tilak, one third viewed stimuli with only a cross, and one third viewed a face with a tilak and a face with a cross. Each face was judged 100 times: 80 times without any treatment, 10 times with a cross, and 10 times with a tilak.

Our design of 10 faces with two receiving the religious badge treatment is based on the design of previous research (McCullough et al., 2016). Here, we split the 10 faces by ancestry to achieve balance (i.e., 5 Afro-Mauritians and 5 Indo-Mauritians) and varied religious badges equally in their three possible combinations (i.e., cross/cross, cross/tilak, tilak/tilak). All participants viewed two faces with religious badges, but these badges and the faces to which they were connected varied between subjects. As in the original study, two faces received the treatment to make the manipulation less obvious and less subject to experimenter demand effects.

After completing the comprehension task, informants were shown each of the 10 faces in random order, one at a time, and were asked to answer the six trust questions that evinced the highest reliability in the screening task. Ratings were assessed

on an analog scale ranging from 0 to 100. Questions used to assess targets' trustworthiness were: "This person finishes what he starts," "This person respects others," "This person returns extra money when the cashier makes a mistake," "This person is always completely fair to others," "This person follows the rules," and "This person listens to his conscience." Data collected for the experiment reveal that these questions exhibited adequate reliability ($\alpha = .79$).

After rating each face, participants played a modified form of the trust investment game, designed to measure trust and trustworthiness (Berg, Dickhaut, & McCabe, 1995). In the standard trust game, participants are anonymously paired and randomly assigned to the role of either trustor (Player A) or trustee (Player B). Both participants start with equal endowments; however, Player B's endowment never enters game play. In the initial decision-making task, Player A sends any amount of her endowment to Player B. If Player A sends none of her endowment, the game ends. If, however, Player A sends some or all of her endowment, this amount is tripled and then sent to Player B. In the second stage of the experiment, Player B decides how much, if any, of her received amount to send back to Player A. The amount Player A sends to Player B assesses trust since this amount represents a risk that the trustee will return less money than was sent (Camerer, 2003).

All of our participants were assigned to the role of Player A (trustor). They were told that they controlled 250 Mauritian rupees (about 1 day's wage for unskilled labor; US$7.58) and that they could allocate none, some, or all of their money across 10 men located in La Gaulette (the southern town where the photographs were taken). Participants were told that their decisions would be transmitted electronically and anonymously over the computer to the 10 men in La Gaulette who would then respond with how much, if any, of the "transferred" money to return to the participant.

In order to make sure that participants understood the trust game, after completing the attitudinal ratings of the faces, the software stopped and asked participants to alert the local assistant who had left the "room." The assistant then reentered the room and explained the trust game to the participants. The participants were not allowed to proceed to the game until they had passed a comprehension task and the assistant had left the room. All participants completed the comprehension task.

In the economic decision-making task, faces were presented in a two-row × five-column array, in the same order as they were presented in the initial rating task, positioned from the top-left corner, with a "bank" on the right of the array that indicated the 250 rupee initial endowment. Participants could allocate money in 5-rupee increments to each player by clicking on the arrows beneath their face. When a participant did so, it decreased her starting endowment as shown in her bank (see Figure S1 in Supplemental Material). In these experiments, and unlike the traditional trust game, there was no Player B. We decided to determine payouts based only on participant's decisions, adding the amount kept to half of the amount sent after tripling. For example, if a person trusted all of her endowment (250), she would have received a payout of 375 rupees ([250 × 3]/2). After making decisions in the economic game, participants completed questionnaires that asked about age, sex, ethnicity, marital status, education, religious affiliation, degree of religiosity, and frequency of ritual behavior. At the end of the study, each participant received the payout based on the above specified algorithm.

This study design is based upon McCullough, Swartwout, Shaver, Carter, and Sosis (2016), but adapted to the local context, and adjusted to vary the stimuli according to ancestry. In the original McCullough study, the researchers included a control condition of a necklace band to examine whether any additional stimuli added to a face increased trust (it did not). Based on these results, we did not include a control condition due to the small population size of our study area, and the lack of an appropriate control for prayer ashes (there are no locally relevant secular forehead markings).

All portions of this study were approved by the ethical committee of the Czech Association for the Study of Religions, Masaryk University, and written informed consent was obtained from all subjects.

Data Analysis

All data were analyzed in R (R Core Team, 2016). Participants were split based on their reported religious affiliation (i.e., Christian or Hindu) in order to assign correct labels to each target face (e.g., an Afro-Mauritian face with a cross would be a face of in-group ancestry with an in-group religious badge for Christian participants, but an out-group face with an out-group badge for Hindu participants; see Figure S1 in Supplemental Material). The basic model, for both trust ratings and decisions in the trust game, included only the effects of a religious badge (Hindu tilak or a Christian cross) with faces of in-group ancestry without a badge as a reference category (the intercept). In the next step, we added religious affiliation (a binary variable Christian/Hindu), age centered on its mean, and sex, and in the third step, we held constant self-reported religiosity and frequency of ritual behavior. This approach allowed us to examine general trends in the effects of religious badges, regardless of participants' religious affiliation (e.g., the effect of an in-group face with an in-group badge compared to an in-group face without a badge). While it would be fruitful to explore a Badge × Religious Affiliation interaction, comparing differential badge effects on investment between Christian and Hindu participants, our sample size is not large enough for such a comparison. Our basic model has 83% power to detect a medium size effect (Cohen's $f^2 = .15$) at $\alpha = .05$. However, to explore a Badge × Religious Affiliation, we would have needed to double the sample size (which is difficult in remote field settings, see Discussion section). We display the raw means for each religious affiliation and ancestry/badge type in Figure 1 together with the number that each type of ancestry–badge combination was viewed. Note that each participant saw 8 of the 10 faces without a badge, hence the inflated numbers for no-badge faces.

As a general approach, we used either linear mixed models (on trust ratings) or generalized linear mixed models (on trust

Table 2. Estimates and Odds Ratio With 95% Confidence Interval for Self-Reported Trustworthiness and Investment Decision in the Trust Game.

	Model		
Variables	Trustworthiness (β Estimates)	Binomial Invest (Odds Ratios)	Positive Invest (β Estimates)
Outface no badge	−1.05 [−3.26, 1.15]	0.58* [0.36, 0.92]	−0.58 [−2.00, 0.84]
Outface out-badge	2.01 [−2.79, 6.82]	1.21 [0.45, 3.23]	−0.86 [−3.77, 2.05]
Inface in-badge	3.84 [−0.88, 8.55]	3.96* [1.29, 12.17]	2.88* [0.07, 5.68]
Inface out-badge	−4.92* [−9.77, −0.06]	1.54 [0.56, 4.24]	0.15 [−2.84, 3.14]
Outface in-badge	−3.57 [−8.38, 1.23]	1.16 [0.41, 3.28]	−0.56 [−3.71, 2.59]
Affiliation	−0.76 [−6.36, 4.84]	3.15 [0.63, 15.73]	−2.66 [−7.79, 2.47]
Age	0.3 [−2.58, 3.19]	0.89 [0.40, 1.96]	−0.54 [−3.31, 2.23]
Female	−1.08 [−6.77, 4.61]	3.23 [0.62, 16.74]	−4.87** [−9.89, 0.15]
Religiosity	−0.17 [−3.20, 2.86]	0.92 [0.40, 2.13]	0.55 [−2.68, 3.77]
Ritual	0.97 [−1.02, 2.96]	0.84 [0.48, 1.50]	1.09 [−0.90, 3.07]
Intercept	58.49*** [44.90, 72.09]	2.56 [0.06, 114.12]	25.55*** [21.22, 29.89]
Observations	970	970	970

Note. We present β estimates with 95% confidence intervals and odds ratios (exponentiated logistic coefficients). Intercept is in-group face with no badge. Affiliation = Catholic versus Hindu; female = male versus female.
*$p < .05$. **$p < .01$. ***$p < .001$.

game behavior) with participant ID as a varying intercept to account for the fact that each participant made 10 decisions (i.e., these 10 decisions were nested within each participant). This step accounts for the fact that the 10 investment decisions were not independent of each other. Trust ratings were modeled using the lme function in the nlme package (Pinheiro, Bates, DebRoy, Sarkar, & Team, 2014). Because the investment data were counts bounded at 0, we built generalized linear mixed models to model the effects of religious badges on multiple investments in the trust game. Specifically, we employed a hurdle model that fit a binomial distribution on the binary decision to invest or not, and a truncated negative binomial distribution to the positive (nonzero) part of the investment data (Martin et al. 2005; Mullahy, 1986). The hurdle model revealed the best fit to the data when compared with zero-inflated poisson and negative binomial distributions (as assessed by Akaike's information criteria). Hurdle models were run with the *glmmadmb* command (glmmADMB package; Fournier et al., 2012; Skaug, Fournier, Nielsen, Magnusson, & Bolker, 2013) by specifying the "binomial" and "truncnbinom1" distributions, respectively. Binomial coefficients were exponentiated and are reported as odds ratios. All plots were created using MATLAB (MathWorks Inc., 2013) or ggplot2 (Wickham, 2009).

Results

Are Individuals Adorning Religious Badges Rated as More Trustworthy on Survey Measures Than Individuals Not Adorning Religious Badges, Regardless of the Shared Group Membership of the Raters?

On a scale from 0 to 100, faces of in-group ancestry with an in-group religious badge were rated as slightly more trustworthy compared to in-group faces with no badge (estimated difference = 3.84); however, the 95% confidence interval (CI) showed that this effect was not precisely estimated [−0.88, 8.55]. Faces of in-group ancestry with an out-group religious badge were *distrusted* compared to in-group faces with no badge (β = −4.92; 95% CI [−9.77, −0.06]). In other words, on a face of shared ancestry, an in-group religious badge had variable effects (with most of the probability mass on the positive side), while an out-group badge decreased trust. The out-group ancestry trustworthiness ratings did not show any reliable patterns. Faces of out-group ancestry without a badge were rated, on average, as slightly less trustworthy compared to faces of in-group ancestry without a badge, but there was substantial uncertainty around this effect (β = −1.05; 95% CI [−3.26, 1.15]). Similarly, faces of out-group ancestry with an out-group religious badge were rated slightly more positively, but again, with substantial uncertainty (β = 1.99; 95% CI [−2.81, 6.79]). Faces of out-group ancestry with an in-group religious badge were rated more negatively (β = −3.55; 95% CI [−8.36, 1.25]) compared to in-group faces with no badge. For this effect, while the 95% CI include both positive and negative values, the effect size and variability is comparable to the in-group ancestry/in-group badge ratings. In general, findings indicate that religious badges varied in the effects on attitudinal measures of trust. Although most of the probability mass was above 0 for the in-group ancestry/in-group badge effect, this effect was small and variable. Moreover, out-group religious badges increased *distrust* when adorned by people of in-group ancestry, and we also observed a small and variable negative effect of the trustworthiness of individuals of out-group ancestry with an in-group badge (see Table 2 and Figure 2), both indicative of a *black sheep effect*. Detailed modeling steps are described in Table S1 in Supplemental Material.

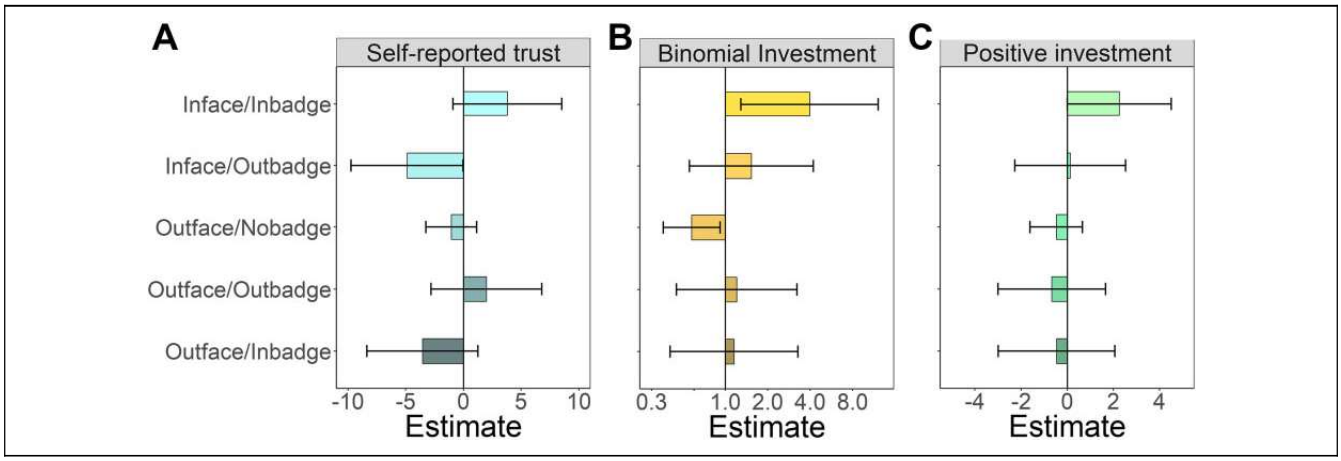

Figure 2. Estimates of badge effects with 95% confidence intervals. (A) Model of trustworthiness ratings. (B) Model of binary decisions to invest in a face. Note the coefficients are odds ratios and the *x*-axis was log transformed. (C) Model of nonzero investments in faces.

Do Religious Badges (Relative to No Badge) Encourage Risky Economic Investment Regardless of the Shared Group Membership of the Raters?

Are Individuals Adorning Religious Badges Rated as More Trustworthy on Survey Measures Than Individuals Not Adorning Religious Badges, Regardless of the Shared Group Membership of the Raters?

The raw data displayed in Figure 1 suggest that compared to in-group faces with no badge, in-group faces with an in-group badge were trusted with more money across the entire sample. To investigate this matter more rigorously, we regressed the investment data on our ancestry/badge variable, collapsing results across participants' religious affiliations to gain more statistical power.

First, using a logistic regression model with varying intercepts by participant's ID, we found that the odds of investing in an in-group face with an in-group badge were 3.96 higher (95% CI [1.29, 12.17]) compared to an in-group face with no badge; that is, photos of in-group ancestry adorning an in-group religious badge had a nearly 4 times greater chance of receiving an investment (compared to in-group faces with no badge), supporting the *coalitional recognition hypothesis*. The estimates for all badge types are displayed in Table 2 and Figure 2. Second, we modeled the positive amount sent using the truncated negative binomial regression with a participant's ID as a varying intercept. This analysis showed that having an in-group badge on an in-group face was associated with getting roughly 3 more Mauritian rupee of the 250 (95% CI [0.07, 5.68]). In other words, when participants decided to invest, they invested, on average, higher amounts to in-group faces with an in-group badge. The somewhat small absolute increase is a result of the fact that participants in general chose to invest in multiple faces evenly, spreading risk ($M_{\text{investment}} = 15.34$, $SD = 14.17$, maximum allocation recorded = 100; see Figure 1); hence, an increase of three rupees for in-group faces with in-group badge indicates a detectable deviation from the baseline tendency in roughly equal allocations across multiple faces. However, we did not observe similar effects for in-group faces adoring out-group badges—while the coefficients were positive, these effects were small and variable (contrary to the *inferred monitoring hypothesis*; see Table 2).

Regarding investments in faces of out-group ancestry, we observed only a negative effect of an out-group face without a badge. Compared to an in-group face without a badge, the out-group face without a badge had a 37% lower probability of receiving money. This finding points to a baseline preference to invest in faces of shared ancestry (independent of religious badges). Interestingly, though not significantly different from the in-group face with no badge, all out-group faces with a religious badge (i.e., both in-group and out-group) had a positive probability of receiving an investment, suggesting that even an out-group badge might, to some extent, mitigate the baseline distrust among faces of out-group ancestry. However, we would need a larger sample to evaluate this conjecture (see Table 2, Figure 2, and Discussion section). See Supplemental Material for detailed analytical steps of the models.

Discussion

The majority of contemporary evolutionary theories of religion hold that various features of religious systems support—at the very least—in-group cooperation. Often, these theories suggest that religious communities also encourage parochial altruism because religious beliefs communicate the rejection, at least implicit, of the beliefs and rituals of competing groups (religious or otherwise; Sosis, 2003). Recent research on the role of religious belief in intergroup relationships has found mixed evidence. For example, some studies found that displaying religious identity increases trust across group boundaries (e.g., Hall et al., 2015; McCullough et al., 2016) and even increases willingness to sacrifice oneself for an out-group member (Ginges et al., 2016). However, other studies suggest parochial altruism might better capture religions'

effects on out-group relationships (Blogowska & Saroglou, 2011; Bushman et al., 2007). Nevertheless, the former results are surprising, given the intense group boundary generating features of religion and the tendency for humans to respond parochially to group classifications, even those which are fleeting and superficial.

Here, we investigated two hypotheses related to the effect of religious signaling on cooperation among otherwise anonymous individuals. The *coalitional recognition hypothesis* assumes that religious individuals use markers of shared religious group membership to assess religious commitment among coreligionists and to distinguish between in-group members and others. The *inferred supernatural monitoring hypothesis*, on the other hand, contends that because religious people believe in omniscient supernatural entities who punish individuals who violate cooperative norms, raters perceive as trustworthy individuals signaling commitment to any punitive supernatural deity. Unlike previous studies conducted in the United States, which found that religious people are trusted regardless of shared group membership, we find the effect of markers of religious identity on trust are contingent upon shared religious group membership and expected ancestry. We suspect that the differences in findings between ours and previous research are because intergroup trust varies systematically across cultures and religious traditions (Shaver, Troughton, 2016). For example, the features of some religious systems may better ameliorate the divisive nature of ethnicity, and cross-religious trust is less likely in areas with histories of religious tensions.

Specifically, our findings indicate that as assessed by attitudinal and economic measures, Mauritians use religious badges to allocate trust in the service of coalition regulation—Mauritians prefer to trust fellow religious in-group members and to allocate less trust to people who indicate an out-group religious identity. Put differently, we find no reliable evidence that markers of religious identity increase trust across religious lines in Mauritius, compared to baseline trust behavior toward in-group members without a religious badge. We do find that faces of out-group ancestry with no badges receive fewer economic investments than out-group faces with a religious badge, but these increases did not differ from baseline levels of trust allocated to in-group faces. These results offer a preliminary conclusion that religious badges might mitigate some of the ethnic-based out-group hostility. However, out-group badges do not increase trust beyond baseline in terms of economic investments. In general, our findings provide greater support for the *coalition recognition hypothesis*.

Moreover, we found that, in terms of attitudinal ratings, individuals who were of in-group ancestry but who wore an out-group religious badge, and individuals who were of out-group ancestry who wore an in-group religious badge were *distrusted*. These findings suggest that religious markers have divergent effects on trust depending on the ancestry of the target and the rater. These findings are similar to the *black sheep effect* and suggest that people are suspicious of those individuals who appear to indicate membership in a group but violate other expectations typically associated with group members. Individuals in our sample did not infer that these individuals were trustworthy, even though they were depicted as believing in supernatural entities.

Taken together, our findings suggest that research which has found religion to increase trust across religious group boundaries may be partially attributable to the ambient Christian cultural history of the Euro-American societies where these studies were conducted, and/or to the Christian identification of both raters and targets, or the unusual nature of American undergraduates (Sears, 1986). Christianity has a cultural history of proselytization across ancestries, unlike Hinduism. Rather than being indicative of religion, in other words, previous findings may only be generalizable to majority Western Christian populations. Alternatively, the differences between previous studies and the results reported here may be due to cross-cultural differences in the number of secular individuals in each country. In contexts where rates of unbelief are high, such as most Western societies, religious markers could perhaps function as markers of belonging to a "religious" in-group vis-à-vis a secular "out-group" (Hall et al., 2015); however, recent research finds little evidence of a religious in-group in New Zealand, a society with high rates of unbelief (Shaver, Troughton, et al., 2016), and the religious congruence fallacy (that religious people are more prosocial) appears inversely related to rates of atheism (Gervais et al., 2017).

Above, we suggested that researchers ought to distinguish between the measurement of general attitudes regarding the trustworthiness of the target (i.e., participants feel that the target will return their money) and the level of trust toward the target (i.e., willingness to extend trust in the form of such money). We suspect that when asked about the trustworthiness of a target—for instance, when asked if a person listens to her conscience—people infer that indications of religious involvement signal general trustworthiness as the *inferred supernatural monitoring* hypothesis predicts. Numerous studies suggest that people perceive religious individuals as more trustworthy regardless of shared group membership (Bailey & Doriot, 1985; Bailey & Garrou, 1983; Bailey & Young, 1986; Galen et al. 2011; Gervais et al., 2011; Orbell et al., 1992; Paciotti et al., 2011; Tan & Vogel, 2008). The trustworthiness as assessed by survey questions may be somewhat different, or perhaps be insufficient, to motivate risky economic cooperation. Indeed, the latter has more relevance for understanding the conditions under which religion leads to the promotion of large-scale cooperation.

We here assessed both attitudinal trust as based on survey ratings and risky trust as assessed by the trust game. In general, we find that the two measures do not differ. We found a positive effect of both attitudinal and economic investment in faces of in-group ancestry with in-group religious markers, but all other effects were variable. Although some of the coefficients trended in different directions for the two trust measures across the different conditions, they cross 0, and thus we hesitate to speculate further without additional data collection. Nonetheless, when selecting methodologies, we encourage that future

researchers consider the possibility that trust is multidimensional and/or that different measures may capture different levels of trust.

Conclusion and Future Directions

The historical move from small-scale, face-to-face social living to large and anonymous societies is one of the major transitions in human evolutionary history (Richerson & Boyd, 1998). There is an emerging consensus among evolutionary scholars that religion facilitated this transition, in part by providing coreligionists with reliable mechanisms for the communication of cooperative intentions. Across the world, during what is known as the Axial Age (c.a. 800 to 200 BCE), religion helped large groups of unrelated and ethnolinguistically diverse individuals to find one another and to cooperate (Turchin, 2013).

Yet, while religion can create bridges, it typically does so by building walls. Understanding how religion unites and divides is not only critical for understanding the history of human ultrasociality but is also important to understanding the conditions that encourage or inhibit social integration in the highly diverse and largely anonymous societies of today. While some previous work has suggested that the communication of religious commitments can increase trust across social boundaries, we here found that religious markers are primarily used in the service of in-group cooperation and the regulation of social coalitions. We suspect that these divergent findings are the result of cross-cultural variation in conflict, differences in the proselytizing tendencies of religious traditions, and/or minority/majority intergroup dynamics.

We note that our conclusions are limited by several factors. First, while our sample size was sufficient to detect the differences between an in-group face with no badge and in-group face with a badge, it did not allow us to explore how these effects might differ between the two religious traditions sampled. Furthermore, while our manipulation (Hindu tilaks and Christian crosses) can be understood as signaling commitment to specific religious groups, a Hindu tilak is more directly associated with recent ritual participation (the ash mark is only temporary) and thus may be a more effective symbol when compared to a Christian cross. Indeed, it may be challenging to find badges across religious groups that have similar signaling functions, yet such badges would constitute a more rigorous test of the present hypotheses. Likewise, adding a different religious group with the same ancestry (e.g., Indo-Mauritian Muslims) could provide further insights into the complex relationships between religious affiliation and ancestry.

Another important limitation concerns the effect sizes detected in the current study. We chose to ask our participants to invest in 10 faces; however, as noted in the text, participants generally spread their allocations evenly across multiple faces, making any differences difficult to detect. Decreasing the number of faces (e.g., to four) could significantly increase the observed effect sizes. Alternatively, a within-subjects design (with every ethnic/religious combination) would increase effect sizes, but at the sacrifice of ecological validity (individuals would be variously depicted as belonging to multiple religions). It might instead be fruitful to vary signal strength by including badges signaling participation in more extreme and demanding rituals, such as the Thaipoosam Kavadi in Mauritius (Xygalatas et al., 2018). While many have rightfully suggested that more data ought to be collected among non-WEIRD (Barone & Mocetti) samples, data collection procedures from these samples come at high material and temporal costs. Such data are both expensive to collect and are often limited to single studies and these communities are often small; indeed, we would have had difficulty finding additional participants from our small study community.

Finally, to the extent possible, future work ought to systematically examine cross-religious trust in settings that vary in conflicts over resources, religious traditions, and the relative size and social dominance of religious groups. Such work has the potential to discover the social features that stabilize diverse, yet peaceful, societies.

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Supplemental Material

Supplemental material is available for this article online.

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The Boundaries of Trust: Cross-religious and Cross-ethnic FieldExperiments in Mauritius

Supplementary Material

Shaver et al., 2018. *Evolutionary Psychology*.

1. Supplementary Figures

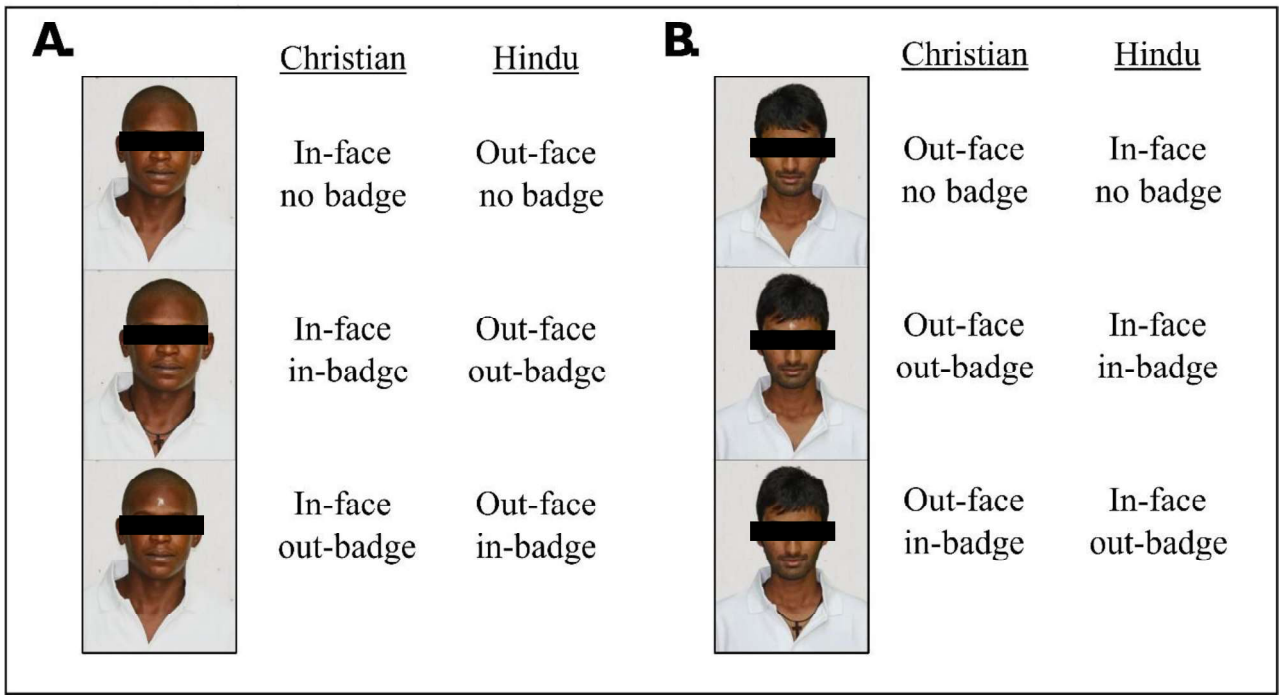

Fig. S1. Illustration of Religious Affiliation*Badge Interaction. A. An Afro-Mauritian face was in-group for Christian participants and out-group for Hindu participants. B. An Indo-Mauritian face was in-group for Hindu participants and out-group for Christian participants. For both A. and B., a cross was in-group badge for Christian participants and out-group badge for Hindu participants, while a tilak was an in-group badge for Hindus and out-group badge for Christians. Participants' eyes are covered to protect their anonymity.

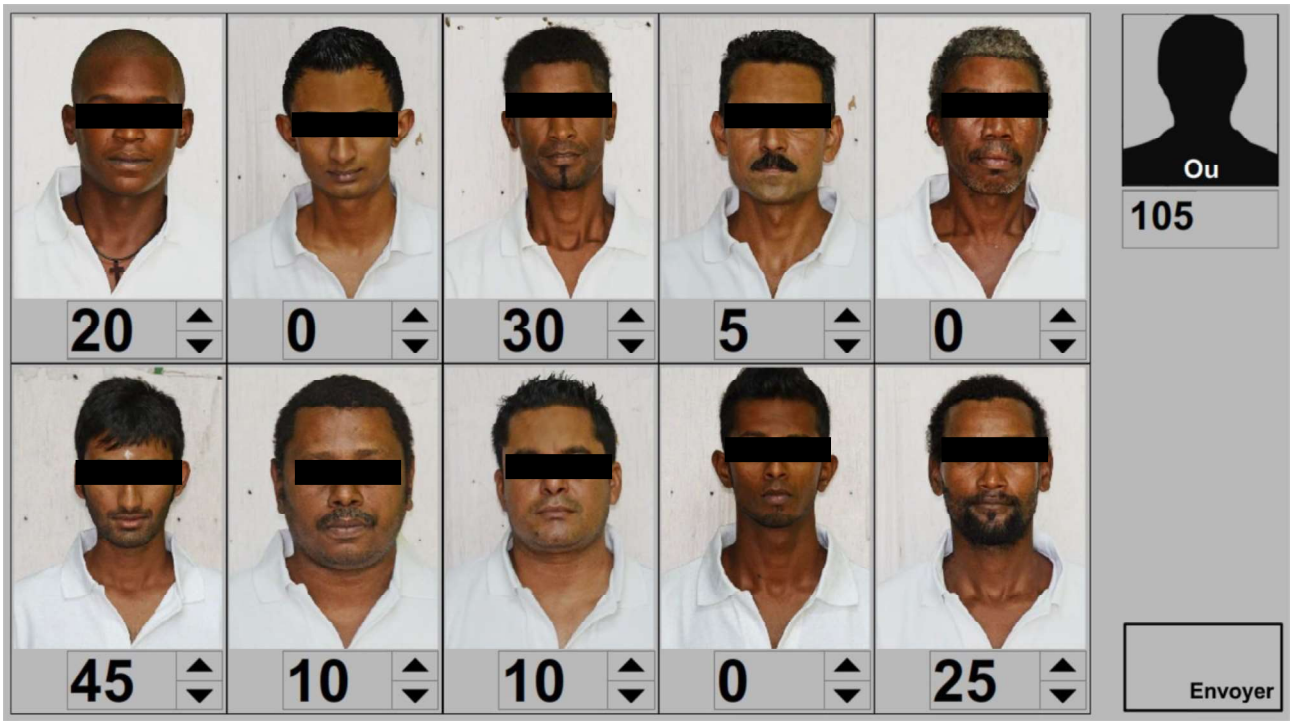

Fig. S2. A screenshot of the computer software designed for the trust game. Participants started with an endowment of 250 Mauritian rupees and could invest in faces in 5-rupee increments. Two faces received the religious badge treatment for each participant – here the left most column contains one Indo-Mauritian with a prayer tilak and one Afro-Mauritian with a cross. *Ou* in the upper right corner is Mauritian Creole for you, denoting the participant's bank, or total endowment. *Envoyer*, in the bottom right corner, indicates that the button "sends" decisions over the computer. Participants' eyes are covered to protect their anonymity.

2. Supplementary Tables

Table S1. Estimates with 95% CI for Self-Reported Trustworthiness

Variable	Modeling step		
	(1)	(2)	(3)
In-face in-badge	3.8	3.81	3.84
	(-0.91, 8.52)	(-0.90, 8.53)	(-0.88, 8.55)
In-face out-badge	-4.88*	-4.89*	-4.92*
	(-9.73, -0.03)	(-9.74, -0.04)	(-9.77, -0.06)
Out-face no badge	-1.05	-1.05	-1.05
	(-3.26, 1.15)	(-3.26, 1.15)	(-3.26, 1.15)
Out-face out-badge	1.99	1.99	2.01
	(-2.81, 6.80)	(-2.81, 6.79)	(-2.79, 6.82)
Out-face in-badge	-3.55	-3.55	-3.57
	(-8.36, 1.25)	(-8.36, 1.25)	(-8.38, 1.23)
Affiliation	--	-0.91	-0.76
	--	(-6.45, 4.62)	(-6.36, 4.84)
Age	--	0.62	0.3
	--	(-2.18, 3.41)	(-2.58, 3.19)
Female	--	-0.89	-1.08
	--	(-6.49, 4.71)	(-6.77, 4.61)
Religiosity	--	--	-0.17
	--	--	(-3.20, 2.86)
Ritual	--	--	0.97
	--	--	(-1.02, 2.96)
Intercept	58.25***	60.03***	58.49***
	(55.27, 61.23)	(50.94, 69.13)	(44.90, 72.09)
Observations	970	970	970

Note. For each model, we present β-estimates with 95% confidence intervals; Intercept is in-group face with no badge; Affiliation = Catholic vs. Hindu; Female = Male vs. Female;
$^{\dagger}p < 0.1$; *p < .05; **p < .01; ***p < .001

Table S2. Odds Ratios with 95% CI for Binary Decision to Invest

Variable	Modeling step		
	(1)	(2)	(3)
In-face in-badge	3.96*	3.97*	3.96*
	(1.29, 12.16)	(1.29, 12.19)	(1.29, 12.17)
In-face out-badge	1.53	1.53	1.54
	(0.56, 4.22)	(0.56, 4.24)	(0.56, 4.24)
Out-face no badge	0.58*	0.58*	0.58*
	(0.36, 0.92)	(0.36, 0.92)	(0.36, 0.92)
Out-face out-badge	1.21	1.21	1.21
	(0.45, 3.23)	(0.45, 3.24)	(0.45, 3.23)
Out-face in-badge	1.15	1.15	1.16
	(0.41, 3.27)	(0.41, 3.27)	(0.41, 3.28)
Affiliation	--	3.31	3.15
	--	(0.66, 16.53)	(0.63, 15.73)
Age	--	0.84	0.89
	--	(0.39, 1.84)	(0.40, 1.96)
Female	--	3.22	3.23
	--	(0.62, 16.61)	(0.62, 16.74)
Religiosity	--	--	0.92
	--	--	(0.40, 2.13)
Ritual	--	--	0.84
	--	--	(0.48, 1.50)
Intercept	14.15***	1.34	2.56
	(5.05, 39.67)	(0.10, 18.44)	(0.06, 114.12)
Observations	970	970	970

Note. For each model, we present Odds Ratios (exponentiated log-odds) with 95% confidence intervals; Intercept is in-group face with no badge; Affiliation = Catholic vs. Hindu; Female = Male vs. Female;
$^{\dagger}p < 0.1$; *p < .05; **p < .01; ***p < .001

Table S3. Estimates with 95% CI for Positive Investments

Variable	Modeling step		
	(1)	(2)	(3)
In-face in-badge	2.26*	3.34*	2.88*
	(0.005, 4.51)	(0.06, 6.62)	(0.07, 5.68)
In-face out-badge	0.13	0.2	0.15
	(-2.27, 2.53)	(-3.30, 3.69)	(-2.84, 3.14)
Out-face no badge	-0.48	-0.68	-0.58
	(-1.62, 0.66)	(-2.34, 0.98)	(-2.00, 0.84)
Out-face out-badge	-0.67	-1.01	-0.86
	(-3.00, 1.67)	(-4.40, 2.39)	(-3.77, 2.05)
Out-face in-badge	-0.46	-0.64	-0.56
	(-2.99, 2.07)	(-4.32, 3.05)	(-3.71, 2.59)
Affiliation	--	-3.38	-2.66
	--	(-9.36, 2.60)	(-7.79, 2.47)
Age	--	-0.32	-0.54
	--	(-3.53, 2.89)	(-3.31, 2.23)
Female	--	-5.61†	-4.87†
	--	(-11.39, 0.18)	(-9.89, 0.15)
Religiosity	--	--	0.55
	--	--	(-2.68, 3.77)
Ritual	--	--	1.09
	--	--	(-0.90, 3.07)
Intercept	20.52***	29.86***	25.55***
	(19.75, 21.28)	(26.76, 32.95)	(21.22, 29.89)
Observations	970	970	970

Note. For each model, we present estimates with 95% confidence intervals; Intercept is in-group face with no badge; Affiliation = Catholic vs. Hindu; Female = Male vs. Female;

†p < 0.1; *p < .05; **p < .01; ***p < .001

Table S4. Estimates and OR with 95% CI for Self-Reported Trustworthiness and Investment Decision in the Trust Game – Reduced Models.

Variable	Model		
	Trustworthiness (β-estimates)	Binomial invest (Odds ratios)	Positive invest (β-estimates)
Out-face no badge	-0.43	0.58*	-0.58
	(-2.70, 1.83)	(0.36, 0.92)	(-2.00, 0.84)
Out-face out-badge	2.9	1.21	-0.86
	(-1.84, 7.64)	(0.45, 3.23)	(-3.77, 2.05)
In-face in-badge	5.20*	3.96*	2.88*
	(0.46, 9.94)	(1.29, 12.17)	(0.07, 5.68)
In-face out-badge	-5.17*	1.54	0.15
	(-10.28, -0.06)	(0.56, 4.24)	(-2.84, 3.14)
Out-face in-badge	-2.11	1.16	-0.56
	(-7.28, 3.06)	(0.41, 3.28)	(-3.71, 2.59)
Affiliation	-0.86	3.15	-2.66
	(-6.70, 4.98)	(0.63, 15.73)	(-7.79, 2.47)
Age	1.24	0.89	-0.54
	(-1.74, 4.22)	(0.40, 1.96)	(-3.31, 2.23)
Female	-0.51	3.23	-4.87x
	(-6.47, 5.45)	(0.62, 16.74)	(-9.89, 0.15)
Religiosity	0.21	0.92	0.55
	(-2.91, 3.33)	(0.40, 2.13)	(-2.68, 3.77)
Ritual	1.39	0.84	1.09
	(-0.66, 3.43)	(0.48, 1.50)	(-0.90, 3.07)
Intercept	55.10***	2.56	25.55***
	(40.53, 69.67)	(0.06, 114.12)	(21.22, 29.89)
Observations	870	870	870

Note. We present β-estimates with 95% confidence intervals and Odds ratios (exponentiated logistic coefficients); Intercept is in-group face with no badge; Affiliation = Catholic vs. Hindu; Female = Male vs. Female;
*p < .05; **p < .01; ***p < .001

3. Supplementary Analyses

As noted in the main text, our sample contained individuals who did not self-identify as Hindu or Christian, and the ancestry with which each is most typically associated (i.e., Afro-Mauritian Christians and Indo-Mauritian Hindus). Our sample included 2 Afro-Mauritian Hindus, 1 Indo-Mauritian Christian, 1 participant with 'other' ancestry, 3 Sino-Mauritians, and 3 Franco-Mauritians. Excluding these 10 participants with unmatched ancestries and religious affiliations reduced the full sample size by 10%. Despite the removal of 10% of the sample, this method of reducing the sample did not yield any many practically important differences from the results presented in the main text (see Tab/ S4). With one exception, all of the differences between the full and reduced samples were those in which significant relationships became marginal or nonsignificant. This is unsurprising given the loss of data in the reduced models. The only substantive difference between the full and reduced models is that in the reduced dataset there is a significant increase in attitudinal trust towards a person of in-group ancestry and in-group religion, likely reflecting the lower levels of diversity in the reduced dataset. See also Fig. S3 for direct comparison of the effects between the full and reduced models.

Table S4. Estimates with 95% CI for Self-Reported Trustworthiness and Investment Decision in the Trust Game – Ancestry-Only Models.

Variable	Model		
	Trustworthiness (β-estimates)	Binomial invest (Odds ratios)	Positive invest (β-estimates)
In-face in-badge	5.20*	5.30*	2.71†
	(0.46, 9.94)	(1.50, 18.69)	(-0.18, 5.59)
In-face out-badge	-5.17*	1.3	0.37
	(-10.28, -0.06)	(0.44, 3.86)	(-2.82, 3.56)
Out-face no badge	-0.43	0.49**	-0.63
	(-2.70, 1.83)	(0.30, 0.80)	(-2.12, 0.87)
Out-face out-badge	2.9	1.1	-0.84
	(-1.84, 7.64)	(0.41, 3.00)	(-3.81, 2.13)
Out-face in-badge	-2.11	1.09	0.61
	(-7.28, 3.06)	(0.33, 3.62)	(-2.80, 4.02)
Affiliation	-0.86	2.75	-3.09
	(-6.70, 4.98)	(0.49, 15.41)	(-8.00, 1.82)
Age	1.24	0.82	0.11
	(-1.74, 4.22)	(0.35, 1.91)	(-2.61, 2.82)
Female	-0.51	2.59	-4.75†
	(-6.47, 5.45)	(0.44, 15.14)	(-9.67, 0.17)
Religiosity	0.21	0.76	1.39
	(-2.91, 3.33)	(0.31, 1.83)	(-1.72, 4.51)
Ritual	1.39	0.68	0.34
	(-0.66, 3.43)	(0.37, 1.26)	(-1.53, 2.20)
Intercept	55.10***	15.17	25.71***
	(40.53, 69.67)	(0.22, 1,036.77)	(21.52, 29.90)
Observations	870	870	870

Note. For each model, we present β-estimates with 95% confidence intervals; Intercept is in-group face with no badge; Affiliation = Catholic vs. Hindu; Female = Male vs. Female;
†p < 0.1; *p < .05; **p < .01; ***p < .001

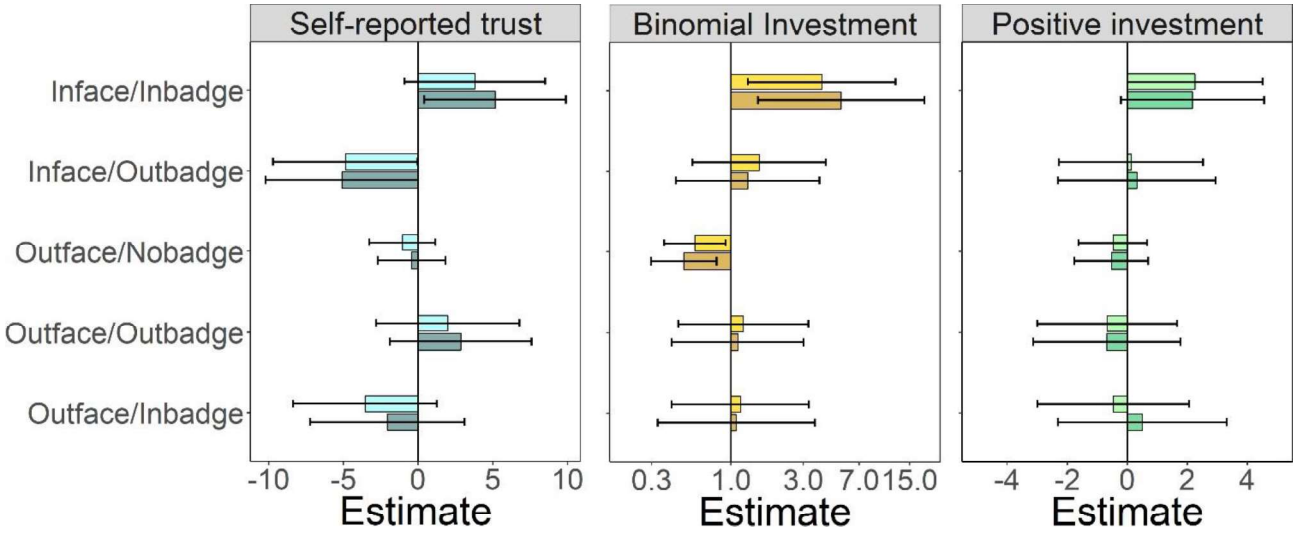

Figure S3. Estimates of Badge Effects with 95% CI Comparing Full and Ancestry-Only Models. Lighter colors display full models as in the main text, while darker colors correspond to the reduced models from SM. A. Model of trustworthiness ratings. B. Model of binary decisions to invest in a face. Note the coefficients are Odds Ratios and the x-axis was log-transformed. C. Model of non-zero investments in faces.

Finally, the sample also included 15 participants who reported 'mixed' ancestry. While our sampling method targeted specifically communities of Afro- and Indo-Mauritians and the rates of intermarriage between different ancestries in Mauritius are relatively low (Ericksen, 1997), we retained these participants in our previous analyses, assuming that dominant ancestry for these participants is associated with their religious affiliation (i.e. for Christian participants with mixed ancestry, the Afro-Mauritian ancestry should be dominant, given this is the majorty ancestry of their religious community). This assumption allowed us to retain most of our sample and statistical power. Here, we present an additional analyses excluding these 15 participants (together with the 10 participants excluded in the analyses reported above). However, as in the case of the ancestry-only models, the reduced models' results do not qualitatively differ from the effects reported in the main text (see Tab. S5 and Fig. S4). These supplementary results provide additional confidence in our main results, mitigating potential issues with ancestry reporting in our sample.

Table S5. Estimates with 95% CI for Self-Reported Trustworthiness and Investment Decision in the Trust Game – Reduced Models.

Variable	Model		
	Trustworthiness (β-estimates)	Binomial invest (Odds ratios)	Positive invest (β-estimates)
In-face in-badge	6.11*	5.49*	4.05*
	(1.01, 11.20)	(1.50, 20.06)	(0.92, 7.19)
In-face out-badge	-5.33†	1.00	-0.14
	(-10.98, 0.33)	(0.27, 3.76)	(-3.54, 3.25)
Out-face no badge	-1.45	0.51*	-0.88
	(-3.92, 1.01)	(0.29, 0.89)	(-2.47, 0.72)
Out-face out-badge	2.07	1.07	-1.92
	(-2.91, 7.05)	(0.37, 3.11)	(-5.02, 1.18)
Out-face in-badge	-1.48	3.38	0.68
	(-7.41, 4.44)	(0.65, 17.61)	(-3.01, 4.38)
Affiliation	0.08	3.93	-3.29
	(-6.42, 6.57)	(0.53, 29.28)	(-9.06, 2.47)
Age	1.81	0.72	-0.40
	(-1.48, 5.10)	(0.27, 1.90)	(-3.55, 2.74)
Female	-1.81	1.56	-4.16
	(-8.27, 4.64)	(0.22, 11.14)	(-9.94, 1.61)
Religiosity	0.53	0.74	0.80
	(-2.77, 3.84)	(0.28, 1.97)	(-2.68, 4.27)
Ritual	0.90	0.59	0.55
	(-1.27, 3.06)	(0.30, 1.16)	(-1.57, 2.67)
Intercept	57.00***	42.59	26.01***
	(41.67, 72.32)	(0.38, 4,708.27)	(21.32, 30.69)
Observations	720	720	720

Note. For each model, we present β-estimates with 95% confidence intervals; Intercept is in-group face with no badge; Affiliation = Catholic vs. Hindu; Female = Male vs. Female;
†p < 0.1; *p < .05; **p < .01; ***p < .001

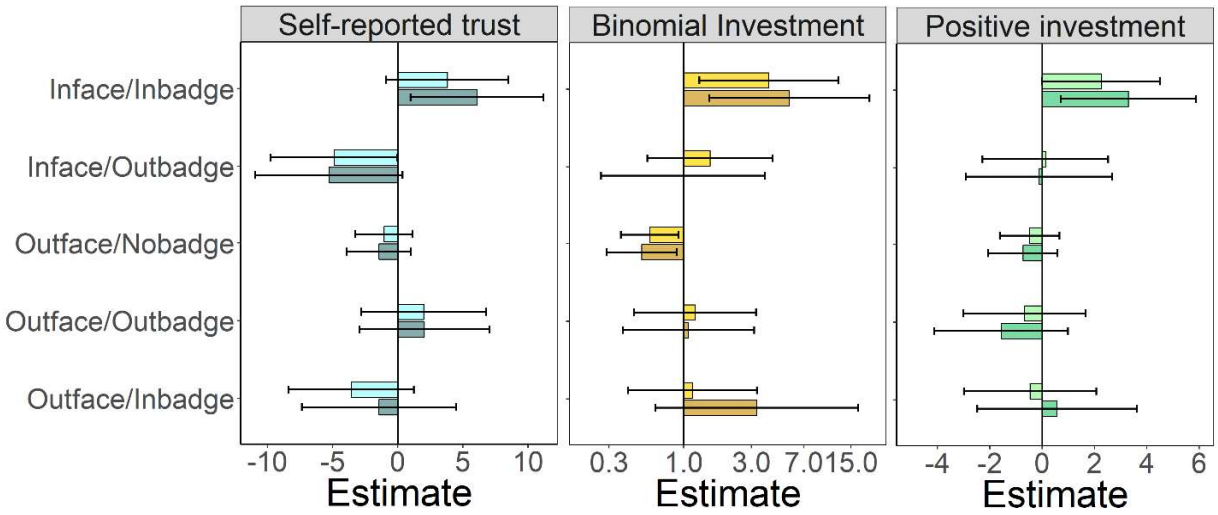

Figure S4. Estimates of Badge Effects with 95% CI Comparing Full and Reduced Models. Lighter colors display full models as reported in the main text, while darker colors correspond to the reduced models reported in the SM. A. Model of trustworthiness ratings. B. Model of binary decisions to invest in a face. Note the coefficients are Odds Ratios and the x-axis was log-transformed. C. Model of non-zero investments in faces.

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6.9. Study 9

Lang, M., Chvaja, R., Purzycki, B. G., Václavík, D., & Staněk, R. (2022). Advertising cooperative phenotype through costly signals facilitates collective action. Royal Society Open Science.

Abstract
Around the world, people engage in practices that involve self-inflicted pain and apparently wasted resources. Researchers theorized that these practices help stabilize within-group cooperation by assorting individuals committed to collective action. While this proposition was previously studied using existing religious practices, we provide a controlled framework for an experimental investigation of various predictions derived from this theory. We recruited 372 university students in the Czech Republic who were randomly assigned into either a high-cost or low-cost condition and then chose to play a public goods game (PGG) either in a group that wastes money to signal commitment to high contributions in the game or to play in the group without such signals. We predicted that cooperators would assort in the high-cost revealed group and that, despite these costs, they would contribute more to the common pool and earn larger individual rewards over five iterations of PGG compared with the concealed group and participants in the low-cost condition. The results showed that the assortment of cooperators was more effective in the high-cost condition and translated into larger contributions of the remaining endowment to the common pool, but participants in the low-cost revealed group earned the most. We conclude that costly signals can serve as an imperfect assorting mechanism, but the size of the costs needs to be carefully balanced with potential benefits to be profitable.

Contributions

Conceptualization	80%
Methodology	80%
Data collection	80%
Data curation	90%
Statistical analysis	100%
Supervision	100%
Writing and editing	90%
Project administration	90%

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Advertising cooperative phenotype through costly signals facilitates collective action

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Around the world, people engage in practices that involve self-inflicted pain and apparently wasted resources. Researchers theorized that these practices help stabilize within-group cooperation by assorting individuals committed to collective action. While this proposition was previously studied using existing religious practices, we provide a controlled framework for an experimental investigation of various predictions derived from this theory. We recruited 372 university students in the Czech Republic who were randomly assigned into either a high-cost or low-cost condition and then chose to play a public goods game (PGG) either in a group that wastes money to signal commitment to high contributions in the game or to play in the group without such signals. We predicted that cooperators would assort in the high-cost revealed group and that, despite these costs, they would contribute more to the common pool and earn larger individual rewards over five iterations of PGG compared with the concealed group and participants in the low-cost condition. The results showed that the assortment of cooperators was more effective in the high-cost condition and translated into larger contributions of the remaining endowment to the common pool, but participants in the low-cost revealed group earned the most. We conclude that costly signals can serve as an imperfect assorting mechanism, but the size of the costs needs to be carefully balanced with potential benefits to be profitable.

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1. Introduction

Finding reliable cooperative partners willing to commit to joint action is a crucial building block of human societies. While many societies ease cooperative problems by instituting norms that guide collective action and establish punitive mechanisms for norm transgression, people still vary in their willingness to obey these norms, especially when the short-term benefits of free-riding are temptingly high [1]. Given that willingness to cooperate in joint tasks is a hidden trait, it cannot be observed directly and might be faked through relatively cheap verbal proclamations. The potential for deception, therefore, presents a problem for the assortment of cooperative partners. Deceitful proclamations may effectively break down collective action if uncommitted individuals verbally fake their commitment and free-ride on the collective efforts. How can committed cooperators reliably recognize each other?

One answer to this conundrum is offered by the strategic choice model [2–4] designed to explain non-human animals' exaggerated phenotypes such as stotting in Thomson's gazelles [5] or the elongated upper tail coverts of the peacock's train [6]. The model asserts that the exaggeration of certain traits reliably signals hidden genetic quality that is otherwise unobservable. Based on the conflict of interest between the sender (who wishes to exaggerate the signal) and the receiver (who wishes to reliably assess the quality of the sender; [7]), the strategic choice model predicts that high-quality signallers benefit from honestly signalling their hidden quality through phenotypes that are exaggerated at a specific cost ('handicap' or 'strategic cost'; [8]), which is not affordable to low-quality signallers. For example, while the elongated tail feathers of male barn swallows handicap their flying ability, they were shown to correlate with underlying genetic quality, increased mating opportunities and reproductive success [9]. Whereas exaggerated phenotypes are not the only means to stabilize reliable communication [10,11], the mathematical formalization of the strategic choice model suggests that the differential fitness pay-offs of signalling through making phenotypes unnecessarily costly is evolutionarily stable and separates signallers based on the quality of their hidden phenotype [4,12]. Using these models, researchers identified added strategic costs in various human behavioural patterns such as meat sharing [13,14], blood donations [15], or subsistence activity, which were assessed using costly signalling theory (CST; [16–18]). Under the auspices of CST, behaviours that ostensibly decrease individual fitness in the short term (e.g. meat sharing) reliably signal hidden qualities (e.g. hunting skills), and advertisement of these qualities provides long-term fitness benefits in the form of increased mating and cooperative opportunities [16,19].

Interestingly, the application of CST to the problem of communicating a commitment to cooperative collective action—'a hidden cooperative phenotype' [1]—has been almost exclusively restricted to the context of ritual behaviour [20–22]. Indeed, it has been long recognized that ritual practices such as penile subincision of Aboriginal Australians [23] or Chukchi sacrifice of herd animals [24], together with the cross-cultural omnipresence of regular ritual gatherings where people spend time and energy, test the reliability of group members [25]. The CST of ritual (henceforth CSTR) suggests that ritual practices may be understood as signals of commitment to cooperative norms that guide collective action. The intensity of the signal may range from extreme signals such as self-mutilation to subtle signals such as attending weekly ritual gatherings [26,27]. According to CSTR, these signals, alongside other evolved mechanisms such as supernatural punishment [28–30], help mitigate problems of cooperation, such as whom to trust, accountability and collective-action maintenance. Ritual practices serve as a communication platform that offers individuals committed to collective action to truthfully express their hidden cooperative phenotype (often through expressing commitment to a supernatural deity or similar group symbols representing the group's cooperative norms), effectively separating truly committed individuals from potential free-riders [20]. Collective rituals and similar religious practices provide both a public arena and a shared code for communication of hidden cooperative phenotype [30]. By binding specific material and energetic costs to ritual performance, the hidden quality of commitment to cooperative norms materializes into physical signals that the receivers may rely upon [31,32].

Several converging lines of empirical evidence support the basic premises of the CSTR model and indicate that (i) sending costly signals (that is, performing costly rituals) correlates with hidden qualities (i.e. a cooperative phenotype) and that (ii) signal receivers understand the signal and act upon the received information. First, Sosis & Ruffle [33,34] found that self-reported participation in a public ritual of religious kibbutz members in Israel predicted contributions to the common pool in the public goods game (PGG), and Soler [35] observed a similar relationship in her work with the members of Brazilian Candomblé groups. By analysing social support networks of two villages in

South India, Power [36] showed that participants who regularly worship in a local temple and carry out public religious acts were more likely to be asked for help by other community members and more likely to provide help. Moreover, Xygalatas *et al*. [37] found that charitable contributions to the local temple after performing an extreme ritual of Thaipoosam Kavadi in Mauritius were predicted by the intensity of participation as well as by self-perceived pain suffered during the ritual. Furthermore, in another study in South India, Power [38] found that regular worship increased the chances of being nominated as generous and devout and that performing costly religious activities was associated with nominations for devoutness and for giving good advice. In her study of Brazilian Candomblé, Soler [35] also found that self-reported intensity of costly signalling predicted the reported number of cooperative offers. Finally, using fictional characters varying on the performance of costly acts showed that costly religious signallers are trusted more [39], even across religious traditions [40,41].

However, while these studies provide valuable support for CSTR by harnessing existing religious practices in various cultures, they usually cannot separate costly signals of commitment from other tangled factors and motivations underlying participation in these rituals such as personal vows to superhuman agents, anxiety management [42–44], or health improvement [27,45]. Nor can these studies disentangle the complex causal chains of religious systems that may affect cooperation [46,47]. Thus, it is not clear whether participation in religious practices is the primary driver of cooperative behaviour or whether it is the signalled cooperative phenotype driving the cooperative outputs. We aim to study the latter. In comparison with field studies, two laboratory studies using PGG suggest that simulated charity contribution and voluntary tax-paying may serve as a signal of prosocial intentions [48,49], providing preliminary evidence for the existence of cooperative-intention signalling. Nevertheless, these studies did not investigate how voluntarily undergoing self-harming (in terms of resources) rather than prosocial acts may serve this function. Furthermore, there are important caveats when applying the strategic choice model on cooperative signalling that previous studies failed to consider.

Since performing a painful ritual or sacrificing livestock is not directly and immutably linked to underlying genetic quality, the lack of commitment to collective action does not preclude ritual performance. Even free-riders may sacrifice their resources if the benefit of subsequent interaction with other ritual practitioners would offset these costs. To overcome this impasse, we suggest two possible solutions. First, based on the model introduced by Roberts [50], we propose that costly signalling of a cooperative phenotype would be stable only in iterative cooperative interactions such that the cost could not be recovered after the first interaction. If a free-rider defects during the first cooperative interaction, they would not compensate the cost of the signal but would be prohibited from other interactions (or collective action of the whole group would fail). Costly signalling may, therefore, be understood as signalling long-term cooperative intentions [50]. However, while Roberts [50] models costly signalling on the example of unspecific helping, we argue that ritual behaviour provides signals directed at cooperative norms, which crucially regulate collective action (as opposed to simple helping). The second model accounting for the discrepancy between the non-human animal and human signalling was proposed by Sosis [20], who argues that committed and uncommitted individuals differ in the *perception of cost*s associated with ritual practices. While committed members discount the costs such that the cost/benefit ratio of participation in ritual activities appears positive, the reverse is true for free-riders who differentially weigh alternative behavioural choices [51] (e.g. individually pursuing monetary gain in an unconstrained group). The differential perception of costs arises through socialization processes that, in interaction with genetically inherited traits, give rise to the cooperative phenotype. According to Sosis' model [20], the differential perception of costs would stabilize costly signals even for one-shot interactions (i.e. free-riders would perceive those as too costly).

In the current study, we test the relationship between the presence of a cooperative phenotype, willingness to send costly signals to assort with other cooperators, and the resulting level of within-group cooperation. Hailing from the strategic choice model [4] and Roberts' [50] and Sosis' [20] extensions of this model, we test four primary hypotheses. First, we test whether participants high on the cooperative phenotype would elect to reveal their hidden cooperative phenotype by sacrificing a substantial part of their resources to reliably signal their commitment to the group (H1). Second, we assess whether participants low on the cooperative phenotype would perceive this signal as too costly and refuse to send the signal (H2). Third, we investigate whether revealing the hidden cooperative phenotype positively correlates with the quality of the phenotype. That is, we examine whether participants who chose to send the costly signal would adhere to cooperative norms and contribute more to the common pool in PGG compared with participants who did not send the signal (H3). Finally, we assess whether the heightened adherence to cooperative norms and mutual assurance

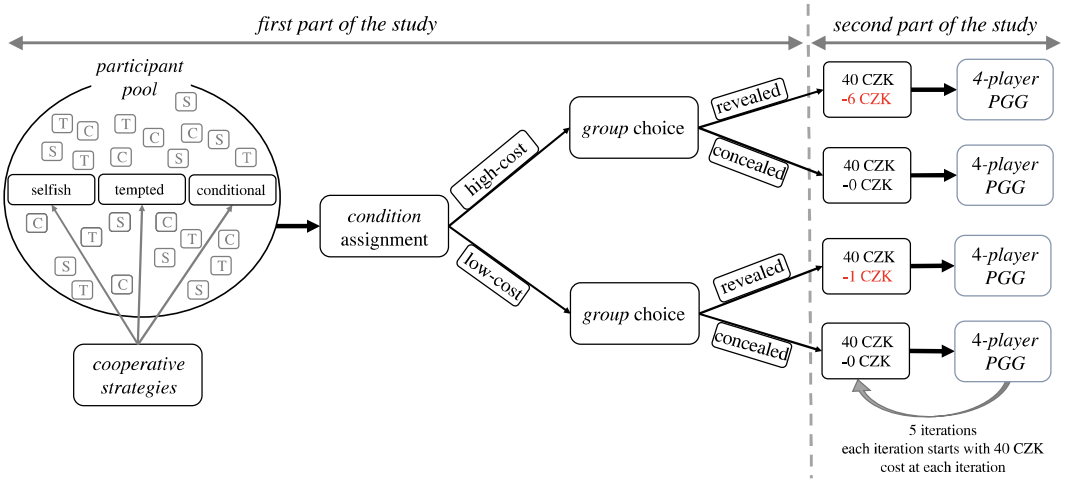

Figure 1. Overview of the design. In the first part of the study, participants filled out a survey on demographic variables, trained on PGG and selected their conditional choices in the pre-experiment PGG to assess their *cooperative strategy*. Next, they were randomly assigned to either the *low- or high-cost conditions* and subsequently selected whether they wanted to play PGG in the *revealed* or *concealed group*. In the second part of the study (approx. a week later), four participants in a given group were endowed with 40 CZK and played five PGG iterations.

through costly ritual signals would stabilize group cooperation such that individuals in groups with costly signals would earn larger monetary rewards compared with groups without signals (H4). (See §2.5 for two additional assumption checks regarding the presence of a conflict of interest and forcing uncommitted individualists to pay the signalling cost.)

To test these hypotheses, we designed a between-subjects study where we first obtained behavioural data on the expression of the cooperative phenotype using an economic game designed by Fischbacher, Gächter and Fehr (henceforth FGF) [52]. This procedure allowed us to categorize participants into three types related to their cooperative strategy (selfish, tempted and cooperative). Next, participants were randomly assigned to either a high-cost or low-cost condition. In both conditions, participants chose whether to sacrifice part of their monetary endowment to play an iterated PGG in a group with other costly signallers (the 'revealed' group) or whether to keep the total endowment and play an iterated PGG with other players who decided to 'conceal' their phenotype. The description of both groups stipulated that members of the group are expected to contribute as much as possible to the common pool (a group norm). Choosing the revealed group in the high-cost condition was associated with sacrificing 15% of the monetary endowment before each PGG iteration to signal a commitment to this norm, while only 2.5% of the endowment was sacrificed in the low-cost condition (figure 1 for a graphical overview of the design). We expected that our four main hypotheses would be supported only in the high-cost scenario, pointing to the causal role of *costly* signals.

Before data collection, we surmised that if our results would not support H1–H4 (i.e. no difference between the revealed and concealed groups in the high-cost condition), it would suggest that the effects of costly signals may be observed only in the context of group competition, which heightens the need for within-group cooperation [53,54]. Alternatively, detecting a difference between the revealed and concealed groups in both the high- and low-cost conditions would mean that even low costs are sufficient to stabilize cooperation and that the CST needs to further investigate human-specific psychology [55]. We further assumed that if H1 would not be supported, but H3–H4 would be supported, then we would conclude that the assortment of cooperators does not depend on the cooperative phenotype, and costly action is a method to induce cooperation in others by making one's choices visible [56]. Likewise, not supporting H2 but supporting H3–H4 would indicate that the cooperative phenotype does not affect the perception of costs (per Sosis' model [20]). Conversely, if H1–H2 would be supported, but H3–H4 would not be supported, we presumed that the cooperative phenotype might successfully separate signallers from non-signallers, but signals are not strong enough to stabilize cooperation above the baseline levels. For more details, see table 1.

A pilot study testing the feasibility of this approach with hypothetical PGG scenarios and high costs revealed that higher scores on a cooperative phenotype scale positively predicted the probability of sending the costly signal (H1). In comparison, lower scores on this scale predicted the probability of

Table 1. Overview of planned analysis and interpretation.

(H1)	*hypothesis*	The positive difference between selfish individuals and cooperators in the probability of choosing the revealed group would be larger in the high-cost relative to the low-cost condition. We remain agnostic about comparisons of selfish individuals with tempted individuals.
	analytical model	$y_i \sim \text{Binomial}(n_i, p_i)$ $\text{logit}(p_i) = \alpha + \beta_{1[LC\ versus\ HC]} + \beta_{2[CC\ versus\ TC]} + \beta_{3[CC\ versus\ SF]} + \beta_{4[HC * TC]} + \beta_{5[HC * SF]}$ y_i is individuals' (i) choice of the revealed group ($n_i = 1$ since there is only one choice). p_i is the probability of choosing the revealed group. α is a fixed intercept, and β_1 is the parameter for the effect of comparison between the low-cost and high-cost conditions for conditional cooperators. β_2 and β_3 are the parameters for the effect of comparison between the conditional cooperators and tempted individuals and between the conditional cooperators and individuals with selfish strategy, respectively, in the low-cost condition. β_4 and β_5 are the interaction terms comparing the effects of the three cooperative strategies between conditions. R model: $glm(group \sim coop\_type * condition, family = 'binomial')$
	interpretation	Pilot data suggest a semi-separating equilibrium whereby some cooperators may choose to hide their phenotype. If no substantial effect would be detected, we would investigate whether the separation process was convoluted by cooperators choosing the concealed group or by selfish individuals choosing the revealed group. In combination with support for H3–H4, we could conclude that the assortment of cooperators does not depend on the cooperative phenotype, and costly action is a method to induce cooperation in others by making one's choices visible.
(H2)	*hypothesis*	The negative difference between individuals playing selfish and cooperative strategies in the probability of stating that the costs in the revealed group were too high/unreasonable would be larger in the high-cost relative to the low-cost condition. We remain agnostic about comparisons of selfish strategies with tempted strategies.
	analytical model	$y_i \sim \text{Binomial}(n_i, p_i)$ $\text{logit}(p_i) = \alpha + \beta_{1[LC\ versus\ HC]} + \beta_{2[CC\ versus\ TC]} + \beta_{3[CC\ versus\ SF]} + \beta_{4[HC * TC]} + \beta_{5[HC * SF]}$ y_i is whether (= 1) or not (= 0) individuals (i) mention that costs are too high/unreasonable in the revealed group ($n_i = 1$ since there is only one choice). p_i is the probability of mentioning high costs. α is a fixed intercept, and β_1 is the parameter for the effect of comparison between the low-cost and high-cost conditions for conditional cooperators. β_2 and β_3 are the parameters for the effect of comparison between the conditional cooperators and tempted individuals and between the conditional cooperators and individuals with selfish strategy, respectively, in the low-cost condition. β_4 and β_5 are the interaction terms comparing the effects of the three cooperative strategies between conditions. R model: glm(high_cost $\sim$ contribution $\times$ condition, family = 'binomial')
	interpretation	If cooperators would separate based on their hidden phenotype, but individuals playing selfish strategy would not be deterred by cost in the high-cost condition, we would interpret this finding as possibly unconscious motivation for joining either of the two groups and re-analyse the free-list data regarding the reasons for choosing the group. In combination with support for H3–H4, we would conclude that the cooperative phenotype does not affect the perception of costs.

(H3)	*hypothesis*	The positive difference between participants in the concealed group and participants in the revealed group in the portion of their endowment contributed to the common pool would be smaller in the low-cost compared with the high-cost condition.
	analytical model	$y_{ri} \sim \beta(\mu_i, \phi)$
		$\text{logit}(\mu_i) = \alpha + \omega_{[m(i)]} + \beta_{1[LC\ versus\ LC]} + \beta_{2[CN\ versus\ RV]} + \beta_{3[HC * RV]}$
		y_{ri} is the proportion of endowment contributed in PGG iteration r by an individual i. α is a fixed intercept, $\omega_{[m(i)]}$ is a varying intercept for individual participants across multiple measures (m). β_1 is the parameter for the effect of comparison between the low-cost and high-cost conditions for the concealed group. β_2 is the parameter for the effect of comparison between the concealed and revealed groups in the low-cost condition. β_3 is the interaction term between condition and group. The parameters of the assumed beta distribution comprise p_i representing location (i.e. proportion of send to kept endowment), and ϕ denotes dispersion.
		R model:
		glmmTMB(proportion_contributed ~ group × condition + (1|ID), family = 'beta_family') If the data would contain >1/3 of zeros and ones, we would also fit a zero-or-one inflated beta (ZOIB) model.
	interpretation	If the 95% CIs for the interaction term would include zero and the assumed separation process functional, we would investigate in a follow-up study whether this is due to the signal being non-functional (no difference between the revealed and concealed groups) or the functional assortment of cooperators in the low-cost condition. If the former would be true, we would continue our investigation by designing an experimental procedure where two groups would compete against each other (following the suggestion that costly signalling intensifies during between-group conflict).
(H4)	*hypothesis*	Participants in the revealed group would earn more than participants in the concealed group in the high-cost condition. This difference would not apply to the low-cost condition.
	analytical model	$y_i \sim \text{Poisson}(\lambda_i)$
		$\log(\lambda_i) = \alpha + \beta_{1[LC\ versus\ HC]} + \beta_{2[CN\ versus\ RV]} + \beta_{3[HC * RV]}$
		λ_i is the expected earned amount after five iterations by an individual i. α is a fixed intercept. β_1 is the parameter for the effect of comparison between the low-cost and high-cost conditions for the concealed group. β_2 is the parameter for the effect of comparison between the concealed and revealed groups in the low-cost condition. β_3 is the interaction term between condition and group.
		R model:
		glmmTMB (earned ~ group × condition, family = 'poisson') If the Poisson model would reveal overdispersion, we would fit a negative binomial model instead. If the residuals would be distributed normally, we would also fit an OLS model.
	interpretation	If the 95% CIs for the interaction term would include zero and the assumed separation process functional, we would investigate the trends in earnings over individual iterations to estimate how many iterations would be needed to support H4. If no trends would be detected, we would proceed in the same steps as in the case of not supported H3.

mentioning that the cost of the signal is too high (H2). Notably, participants who chose to send the costly signal reported that they would send a higher portion of their remaining endowment to the common pool in a hypothetical one-shot PGG (H3). However, comparing the hypothetical earnings between the revealed and concealed groups indicated no substantial difference in their earnings (H4 not supported). See §2.5. While generally supportive of the proposed framework, these results need to be

bolstered by an actual behavioural study on a high-powered sample, by an iterated PGG that better reflects the dynamics of real-world collective action where cooperative interactions between specific individuals are often repeated [57] and by manipulation of the costliness of the signal.

2. Methods

2.1. Ethics information

This study was approved by the Ethics Committee for Research at Masaryk University. Participants provided informed consent and received at minimum 50 CZK (1.7 GBP) as a show-up fee plus any amount they earned in PGG.

2.2. Design

The study used a between-subjects and double-blinded design. Research assistants and participants were blind to our hypotheses. However, note that we did not use deception, and participants knew that they self-selected into one of the two groups. We offered participation in this study to students from the subject pool at Masaryk University, Czech Republic.

2.2.1. First part of the study (individual)

If interested, participants were first asked to fill out an online survey including basic demographic information (age, sex, economic status; note that we did not plan to use these variables in our pre-registered analyses due to the homogeneity of our population, but they may be used for further exploration). Next, participants read instructions on playing a one-shot PGG with the following parameters: an initial endowment of 100 CZK, anonymously invested in an interval of 5 CZK simultaneously with the other three participants). The sum in the common pool was doubled, and the earnings equally distributed among the players. Participants were provided with three examples of how the game could evolve to affect players' earnings. We asked participants to fill out a fourth hypothetical scenario to test their understanding of PGG rules. If they failed to pass the understanding check, the rules of the game were explained again, and participants were offered to answer once more. If they failed the second understanding check, they were not invited to participate in the second portion of the study (but paid the show-up fee).

After passing the understanding check, participants were asked to make unconditional and conditional decisions in a one-shot PGG per the FGF [52] procedure. Specifically, participants made one decision on contributing to PGG out of 100 CZK endowment (in an interval of 5 CZK) without knowing how much the other three players contributed (unconditional). Then, participants made 21 conditional decisions based on the average contribution of the other three players (rounded to the nearest multiple of five; i.e. for an average contribution of 0, 5, 10 … 100). After the end of the first part of the study, participants were randomly paired with three other players, and one participant from each group was randomly selected as the relevant person for the conditional decision. PGG pay-offs were calculated based on the unconditional contributions of the remaining three players and the respective conditional contribution of the selected player. As such, participants were motivated to make genuine decisions because these decisions impacted their earnings. The earnings were known to participants and paid only after the second part of the study. For more details, see FGF [52]. Using the conditional choices, we categorized participants into three different cooperative strategies that we assumed reflect their hidden cooperative phenotype (see below). For the complete survey, see materials at the Open Science Framework (OSF) repository.

After making their choices in the FGF procedure, participants were introduced to the second part of the study conducted approximately one week later. Specifically, participants were told that they will be endowed with 40 CZK and play PGG simultaneously with three other individuals. Participants were informed that they will decide how much of their endowment to send into the common pool while not knowing how much the others will send. After making individual contributions, money in the common pool will be multiplied by two and evenly distributed among the four group members, irrespective of their contributions. Next, they were told that they will again receive 40 CZK and play the second round of PGG with the same group members and likewise in the third, fourth and fifth round of PGG.

Crucially, participants were presented with a choice of two groups for the actual PGG play in the second portion of the study (participants played all five iterations in the same group of their choice). We call these groups a 'revealed' group and a 'concealed' group; however, participants decided between Groups X and Y.

The instructions for the X group (the 'concealed' group) were as follows:

> In Group X, money invested into the common pool is multiplied by two and equally distributed among group members. It is expected that every member should contribute as much as possible to the common pool to increase the welfare of all group members; however, all contributions are anonymous. Choosing this group is not associated with a cost, and it is not necessary to demonstrate intentions regarding the size of the contribution to the common pool.

The instructions for the Y group in the high-cost condition (the 'revealed' group) were as follows:

> In Group Y, money invested into the common pool is multiplied by two and equally distributed among group members. It is expected that every member should contribute as much as possible to the common pool to increase the welfare of all group members; however, all contributions are anonymous. Choosing this group is associated with a monetary cost that no one will benefit from. Specifically, members of this group will sacrifice 15% [or 2.5% in the low-cost condition] of their endowment before each PGG round (6 CZK [1 CZK in low-cost]) to demonstrate their intentions regarding the size of the contribution to the common pool.

The order of the group presentation (X or Y first) was randomized. Upon choosing either the Y or X group, participants were prompted to provide a short rationale for choosing their group and invited to participate in the second part of the study one week later. Participants were invited to the second part of the study in groups of four to interact with members of the same group (revealed or concealed). We invited participants in such a way as to approximately balance the number of women and men for each condition.

2.2.2. Second part of the study (group interaction)

For the second part, we invited at least one extra participant for each session to ensure that each session had four participants. If not needed, these extra participants were paid a show-up fee of 100 CZK plus any earning from the FGF played online before the experiment. If we had fewer than four participants in one session, we did not run the session. Upon joining the online testing session, participants were welcomed by a research assistant through a virtual chat platform, individually read an informed consent and reminded of the rules of PGG specific for their group.

Next, participants were introduced to a software (oTree) programmed to facilitate the multi-player iterated PGG. On a computer screen, they saw their actual earnings, and for each PGG iteration, they inputted the percentage of their endowment they wanted to contribute to the common pool. After each PGG iteration, the software summed all players' contributions, multiplied them by two, and equally distributed this product between the group members, updating their earnings. Specifically, in the high-cost condition, participants in both groups started each PGG round with an initial endowment of 40 CZK (approx. 1.3 GBP), and participants in the revealed group immediately lost 15% of their endowment, i.e. 6 CZK. Using their remaining endowment, participants in both groups made first iteration PGG decisions and learned about others players' contributions and their current earnings. Before the second PGG round, each participant in the revealed group again lost 6 CZK from their 40 CZK endowment for the second round, and the same procedure was repeated for the third, fourth and fifth PGG iteration (after each iteration the participant would see other players' anonymous contributions). Upon finishing the gameplay, participants were paid out their earned sum after five iterations plus a show-up fee and earnings from FGF. The maximum earning for full cooperation in the revealed group was set at 408 CZK, while for the concealed group at 480 CZK in the high-cost condition. The maximum earning for playing the selfish strategy while the other three players would unconditionally cooperate was set at 425 CZK in the revealed group and 500 CZK in the concealed group.

In the low-cost condition, this procedure was identical except that participants chose between a concealed group without any signal and a revealed group that would sacrifice 2.5% of their endowment, that is, 1 CZK. The maximum earning for full cooperation in the revealed group was set at 468 CZK, while for the concealed group at 480 CZK. The maximum earning for playing the selfish strategy while the other three players would unconditionally cooperate was set at 488 CZK in the revealed group and 500 CZK in the concealed group (see OSF materials for these calculations).

2.3. Sampling

Participants were recruited from a student participant pool at Masaryk University, Czech Republic. Expectedly, sampling from this pool in our previous studies [46,47] revealed a young (mean age = 24) and secular sample where the modal answer on religiosity was 'not religious', and the modal answer on ritual participation was 'never/not often'. Hence, testing our hypotheses on a population largely unengaged with costly religious signals should present a strong test of our hypothesis.

For both conditions, we invited participants separately for the revealed- and concealed-group sessions to balance the ratio between the revealed and concealed groups and the ratio of women and men in each group. We planned to recruit 160 participants for each condition: approximately 80 per group and four per session (for a grand total of 320 participants). Participants who filled out the first portion of the study but did not take part in the second part of the study were paid a show-up fee and FGF earnings. The planned sample size was based on the cost/benefit ratio of power analyses for our four hypotheses.

To assess the expected power of the main planned statistical tests for H1–H4, we specified three estimated effect sizes for the interaction between cooperative strategy and condition (H1–H2) and for the interaction between chosen group and condition (H3–H4). Specifically, for each hypothesis, we expected no effects of the main predictors (strategy/group) in the low-cost condition and varied the effect sizes for the high-cost condition based on pilot data and theoretical expectations (see electronic supplementary material, figure S1 for expected effects). Next, we used the command *powerSim* from the *simr* package [58] in R to simulate the planned statistical models for various sample sizes. *simr* uses Monte Carlo simulation with pre-specified effect size and variance explained by varying intercepts (and other relevant parameters for other distributions) to re-fit the planned statistical model a specified number of times, assessing the binomial ratio of models with significant/non-significant results (at significance level $\alpha = 0.05$). We used 1000 Monte Carlo simulations to simulate the expected differences in slopes between conditions for each hypothesis for sample sizes ranging from 40 per condition to 240 per condition (in the steps of 20). The results of these simulations (with 95% CIs) are plotted in electronic supplementary material, figure S1, suggesting that 160 participants per condition should allow us to detect moderate effect sizes of the specific interactions with greater than 80% power for all four hypotheses.

Since most of the questionnaire data were collected online, we did not expect missing data. Since our primary outcome and predictor variables are bounded, we did not expect to detect any outliers, and we used appropriate statistical techniques to account for participants scoring on the boundaries of possible data distribution (see Analysis). Finally, we planned to exclude participants who did not pass an understanding check (specified above) but this was not the case for any participant in the second round. Likewise, we planned to screen participants' reasons for choosing either of the groups and exclude those whose responses indicating a misunderstanding of the group definitions. We did not exclude any participant on this basis. There were no additional exclusion criteria.

2.4. Analysis

Analyses were conducted in R [59] (R version for the presented analyses: 3.6.3). First, we categorized participants into three cooperative strategies based on their play of the FGF version of PGG. Namely, participants playing a cooperative strategy (corresponding to the cooperative phenotype), tempted individuals (cooperate if the temptation to free-ride is low but free-ride if benefits are high), and individuals playing a selfish strategy (always free-ride). To this end, we fitted a finite mixture model to our FGF data using the function *flexmix* from the flexmix package [60]. This function estimates distributional parameters for each of the three cooperative strategies and then classifies participants into one of those strategies (for an example, see Chen & Fischbacher [61]). We also coded participants' responses to the open question on the reasons for choosing their particular group, searching for words such as 'waste', 'loss' or 'unnecessary' concerning the signal cost, which would indicate that the signal was perceived as too costly (see electronic supplementary material, section S2.2 for examples from the pilot data). Two independent coders blind to our hypotheses coded participants' answers with 87% agreement. The first author of this study arbitrated the 13% of responses on which the two coders did not agree.

To analyse the mean contribution to the common pool in each PGG, we first calculated the percentage contributed to the common pool from the remaining endowment, accounting for various costs between groups and conditions and the fact that there was a limit on the minimum and maximum contribution.

The overall earning in PGG for each participant was calculated as a sum of individual earnings in every PGG iteration.

Next, we tested our hypotheses using a generalized linear mixed model (GLMM) framework, accounting for the specific data-generation process and hierarchical structure tailored to each hypothesis. The first hypothesis was assessed using logistic regression with the probability of choosing the revealed group as the outcome variable, and cooperative strategy (selfish versus tempted versus cooperators) interacted with condition (low-cost versus high-cost) as the predictor variables. The second hypothesis was assessed using a binomial regression where the outcome variable was the probability of mentioning that costs were too high in the revealed group. The predictors comprised cooperative strategies (selfish versus tempted versus cooperators) interacted with condition (high-cost versus low-cost). The third hypothesis was planned to be assessed using a beta regression to account for the typical structure of percentage data [62,63], where the proportion of the endowment contributed to the common pool across the five PGG iterations would comprise the outcome variable and group (concealed versus revealed) interacted with condition (low-cost versus high-cost) the main predictor variable. However, we also planned that if more than 1/3 of the PGG contributions would contain 0 or 1, we would fit a zero-or-one-inflated beta model. Since this was the case (see Results), we fitted the zero-or-one inflated beta (ZOIB) model using the *gamlss* package [64]. We adjusted the model estimates for the fact that individuals were nested within the five PGG iterations. Finally, H4 was assessed by a structurally similar model as H3; only the dependent variable was the sum individual earnings in CZK after all five PGG iterations. We planned to use Poisson regression to account for the fact that our data were bounded by minimum and maximum earnings. However, we also planned that if the Poisson model would display overdispersion (as suggested by pilot data), we would opt for a negative binomial model instead and that if the data would be approximately normally distributed, we would consider using an ordinary least-square regression (OLS) for a more straightforward result presentation. As we detected overdispersion, we fitted a negative binomial model. A detailed overview of the statistical tests assessing each hypothesis can be found in table 1 and the electronic supplementary material, R code.

2.5. Pilot data

To assess the feasibility of the planned procedure, we conducted two online pilot studies (henceforth Pilot 1 and Pilot 2) with the Czech student population. Participants for the pilot studies were recruited through advertisement at various student groups on Facebook and asked for help testing a new study. No compensation was offered for participation. For Pilot 1, we recruited 89 participants (63 women; $M_{age} = 23.9$) and for Pilot 2, we recruited 91 participants (68 women; $M_{age} = 24.5$).

2.5.1. Pilot design

Since this is was an online study, we assessed the cooperative phenotype using a cooperative values scale adapted from Peysakhovich *et al*. [1] rather than the cooperative strategy planned for the actual experiment (see electronic supplementary material, section S2.1 for the specific items and reliability analysis). Note that we did not plan to use this scale as a predictor in the actual experiment. Next, we explained the rules of PGG and tested participants' understanding of the PGG rules (see §2.2). Participants who failed the second understanding check were excluded from the analysis (three participants in Pilot 1 and five participants in Pilot 2). We also excluded participants who did not finish the survey (three participants in Pilot 2), and one participant who reported being 96 years old.

After explaining the rules of PGG, participants were asked to imagine three hypothetical PGG scenarios played with three other players:

2.5.1.1. First scenario

In the first scenario, participants were asked to imagine receiving an endowment of 200 CZK and playing one-shot PGG as the last player, that is, after knowing how much other hypothetical players contributed to the common pool. This scenario aimed to test an assumption of the signalling theory that people vary in cooperative affordances (conditional cooperators versus selfish individuals). That is, we tested Assumption Check 1 (AC1), stating that selfish individuals often defect collective action for personal benefits while conditional cooperators mainly contribute to collective action for their mutual benefit. We varied the contributions of other hypothetical players such that the remaining three players

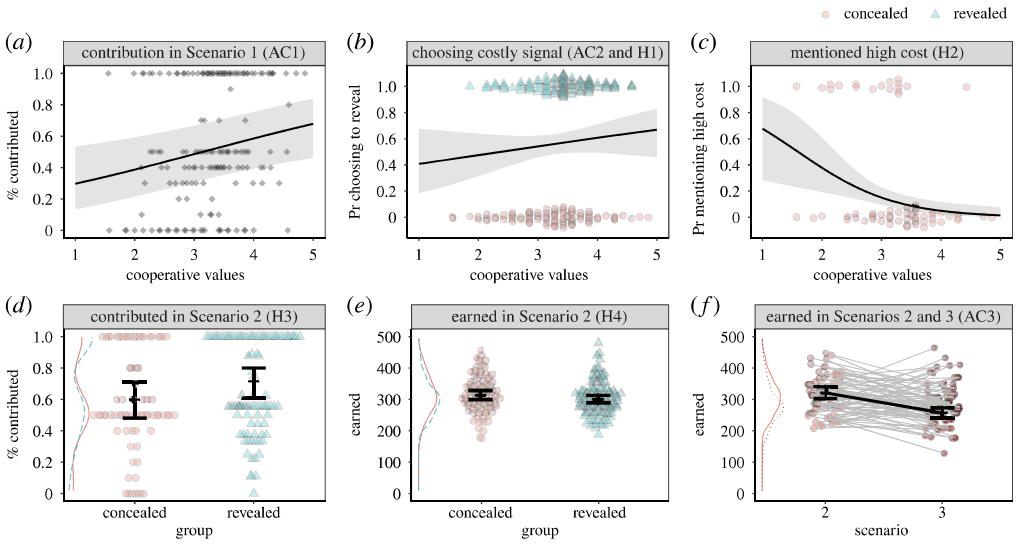

Figure 2. Overview of pilot data. Reporting higher cooperative values increased reported contributions to the common pool in PGG Scenario 1 (*a*) as well as increased the probability of choosing the revealed group (*b*). Lower reported cooperative values positively predicted mentioning costly entrance fee in the revealed group as too high (*c*). Participants in the revealed group reported that they would send a higher percentage of their endowment to the common pool in PGG Scenario 2 (*d*), but this would not lead to higher earnings (*e*). Finally, forcing participants in the concealed group to make the costly signal would decrease their earnings (*f*). Black lines are regression estimates with 95% CIs. Figures *d*–*f* also contain density plots for the respective comparisons.

contributed either their entire endowment (i.e. 200 CZK), part of their endowment (80 CZK), or variable sums (200, 0 and 20 CZK).

2.5.1.2. Second scenario

In the second scenario, participants were presented with two hypothetical groups they could join for yet another PGG in which they would again receive 200 CZK endowment and contribute simultaneously with the other three players to the common pool. These hypothetical groups afforded either to reveal participants' commitment to contribute to the common pool at a cost (20% of their endowment in Pilot 1 and 10% of their endowment in Pilot 2) or to save the money and play in a group that conceals intentions (see §2.2 for definitions of the two groups). Given the assumed variation of the cooperative phenotype in the population [1,65], we expected that participants would self-select roughly 50/50 in the concealed and revealed groups (Assumption Check 2; AC2). After choosing either of the two groups, we asked participants how much they would contribute to the common pool and to give a reason for choosing the specific group, testing H1–H4.

2.5.1.3. Third scenario

Finally, to test another assumption of the signalling model in these hypothetical scenarios, we included a third scenario in Pilot 1 where we asked participants who played in the revealed group to imagine playing in the concealed group and vice versa. The purpose of this manipulation was to examine whether the costly signal would indeed be too costly for the uncommitted individualists, hypothetically present in the concealed group. Thus, the third Assumption Check (AC3) stated that participants who chose the concealed group in the second PGG scenario should earn less when forced to signal norm commitment in the revealed group in the third PGG scenario. The results of the pilot test are described in verbatim below and plotted in figure 2. Note that we used GLMM in the pilot-data analysis analogically to models in table 1 (see also electronic supplementary material, R code).

2.5.2. Pilot results

The results of the first PGG scenario collapsed across both pilots, and three contribution schemas suggest that participants scoring higher on the cooperative values scale reported that they would contribute a larger portion of their endowment to the common pool ($\beta = 0.40$, 95% CI = [0.11, 0.69]; supporting AC1). Scoring 'one' on the cooperative values scale was associated with a reported contribution of

30% of the endowment, while scoring 'five' was predicted to yield contributions at 68% of the endowment. Furthermore, signalling strategies were roughly equally represented (n *revealed* = 94, n *concealed* = 74; supporting AC2), and higher scores on the cooperative values scales positively predicted the probability of choosing the revealed group (supporting H1), albeit this effect was not precise and 95% CIs contained zero ($\beta = 0.27$, 95% CI = [−0.20, 0.74]). The lowest score on the cooperative values scale predicted a 41% probability of choosing the revealed group, while the maximum score was associated with 67% probability. This imperfect separation is further explored in the electronic supplementary material, section S2.3. Supporting H2, higher scores on the cooperative values scale negatively predicted the probability of mentioning that the cost of the revealed group was too high ($\beta = -1.24$, 95% CI = [−2.04, −0.44]) when prompted to explain why they chose to play in the concealed group (estimated 68% probability for cooperative values score of 'one').

Participants in the concealed group reported that they would contribute a smaller proportion of their remaining endowment compared with participants in the revealed group (60% versus 72%), supporting H3 ($\beta = 0.52$, 95% CI = [0.14, 0.90]). However, when assessing how much players in each group would earn after summing contributions in hypothetical sessions with other players, overall estimated earnings were higher in the concealed (315) compared with the revealed (302) group ($\beta = -0.04$, 95% CI = [−0.10, 0.02]). While this result does not support H4, the difference between groups was not precisely estimated, and we expected that using real monetary incentives in iterated PGG would support H4 (there would be a steady decline in mean contributions in the concealed group as more members choose to free-ride in subsequent iterations, as shown by other PGG experiments [66]).

Finally, we compared the potential earnings of participants who chose the concealed group in the second scenario with their hypothetical earnings in the third scenario, where they were forced to play in the other group. Participants in the concealed group would, on average, earn less in the third scenario (321 versus 257), supporting AC3 ($\beta = -0.22$, 95% CI = [−0.28, −0.16]). Further details on the pilot procedures and additional analyses are reported in the electronic supplementary material.

3. Results

3.1. Classification of cooperative strategies

To account for dropouts between the two stages of the study and have sufficient substitutes, we initially recruited 458 participants for the first phase of the study who were randomly assigned to the high-cost and low-cost conditions. From this pool of participants, 372 (197 women; 1 non-binary; $M_{age} = 23.6$, $s.d._{age} = 3.1$) finished the first phase and were interested in taking part in the second phase of the study (we aimed for a final sample of 320 participants, 80 per each combination of group and condition). Of the 372 participants, 90 participants chose the revealed group and 99 participants chose the concealed group in the high-cost condition. In the low-cost condition, 122 participants chose the revealed group and 61 participants chose the concealed group.

We classified participants into three different cooperative types based on the strategies they played in the FGF version of PGG: cooperators, tempted cooperators and individuals playing a selfish strategy (figure 3). We assumed that these strategies should approximate the underlying cooperative phenotype. The model classified 171 participants as cooperators, 107 as tempted cooperators and 94 as playing selfishly. The ratio of prior and posterior probabilities for all three categories was greater than 0.994, suggesting a non-overlapping classification of participants [60].

3.2. Choosing the revealed group (H1)

We used this classification to predict the selection of concealed and revealed groups in the high- and low-cost conditions, hypothesizing that individuals playing selfishly will be less likely to choose the revealed group in the high-cost condition. The results of our binomial regression model lent support to this hypothesis, showing that compared with cooperative behaviour, selfish behaviour in the conditional PGG was associated with a lower probability of choosing the revealed group in the high-cost condition ($\beta = -1.05$, 95% CI = [−1.78, −0.32]). As predicted, this difference was smaller in the low-cost condition, although the 95% confidence intervals of this interaction included zero ($\beta_{interaction} = 0.80$, 95% CI = [−0.25, 1.85]). Since most of the probability mass was positive, we interpret this difference as a preliminary support for H1. Looking at the high-cost condition, cooperators had a 57% chance of choosing the revealed group while this probability dropped to 46% for tempted cooperators and only to 32% for

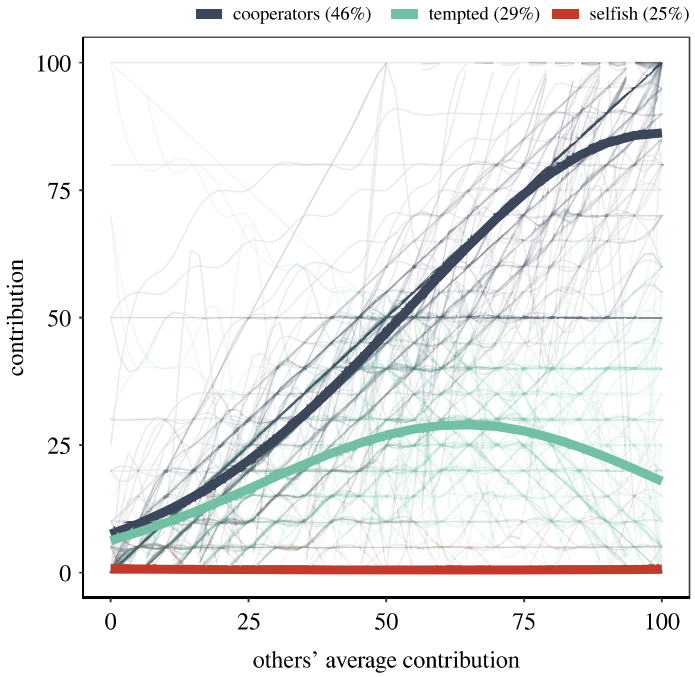

Figure 3. Classification of participants into three cooperative strategies. The thick lines plot the predicted values for each of the cooperative strategies, while the thin lines represent raw data colour-coded based on the specific strategies. We used a cubic spline interpolation on the raw data for easier visual reading.

Table 2. Beta-estimates from logistic regressions with 95% CI from testing hypothesis 1 (probability of selecting the revealed group) and hypothesis 2 (probability of mentioning high costs). The reference category is 'cooperators' for the strategy factor and 'high cost' for the condition factor. The estimates are logged odds.

	hypothesis 1	hypothesis 2
intercept	0.29	−1.89
	(−0.13, 0.71)	(−2.53, −1.26)
strategy: tempted	−0.45	−0.05
	(−1.15, 0.24)	(−1.12, 1.01)
strategy: selfish	−1.05	1.07
	(−1.78, −0.32)	(0.17, 1.96)
condition: low-cost	0.42	0.23
	(−0.21, 1.04)	(−0.65, 1.12)
low-cost × tempted	0.60	0.08
	(−0.41, 1.61)	(−1.37, 1.52)
low-cost × selfish	0.80	−1.44
	(−0.25, 1.85)	(−2.87, −0.01)
N participants	372	345

individuals with selfish strategy. By contrast, these probabilities were estimated at 67%, 70% and 61% in the low-cost condition. See table 2 for all estimates, figure 4*a* for illustration, and electronic supplementary material, R code for re-analysis of this data using raw conditional contributions for each type as predictors.

3.3. Differential perception of costs (H2)

We further used the classification into cooperative strategies to predict whether participants mentioned wasted resources when verbally explaining their choice of the group for the second phase. From 345

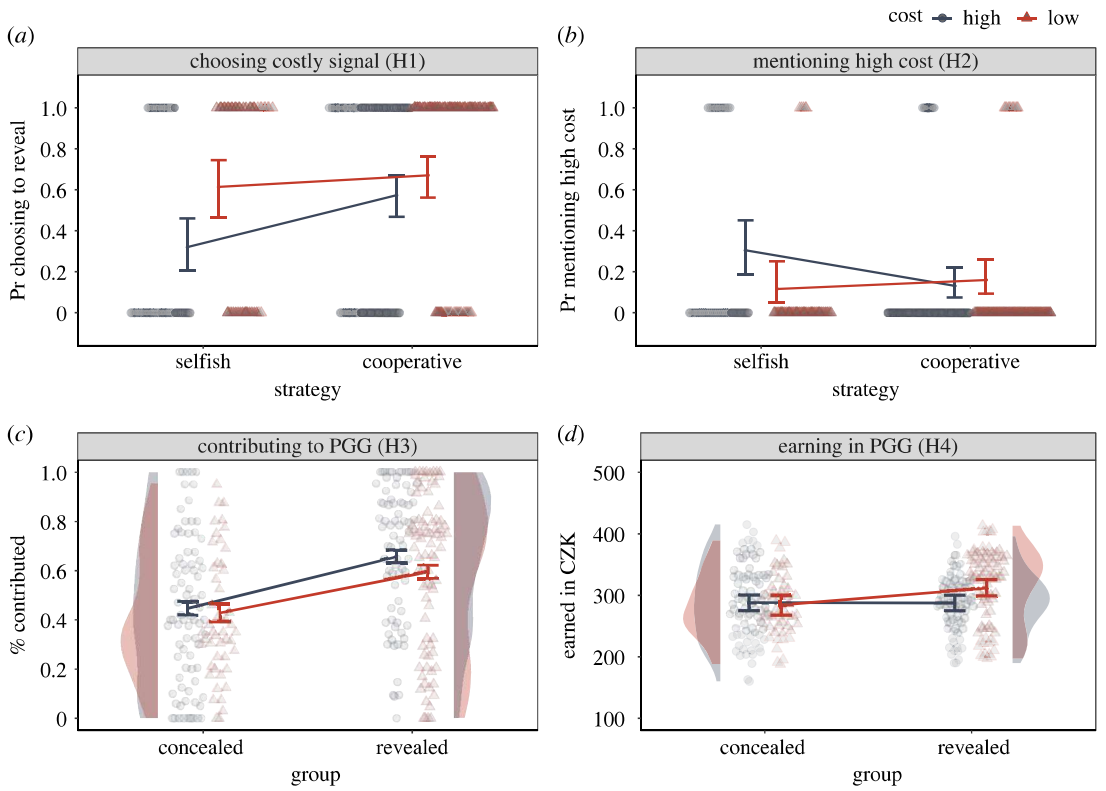

Figure 4. Estimated means with 95% CI plotted over raw data for the four core hypotheses. The density plots represent the distribution of raw data for each group/condition combination. Note that the estimated lines in plot) C are from a beta regression rather than ZOIB regression to include the full spectrum of the data modelled by the mixture of separate regressions in the main text (table 3). Specifically, in this graph, the 0 and 1 contributions were converted using the formula $(y' = (y(n-1)+0.5)/n)$ where y is the transformed variable and n is the sample size, such that the data could be analysed with the beta regression (this correction has negligible effects on inference). See electronic supplementary material, R code for precise estimates from this model.

participants who answered this question, 56 participants mentioned that the signal is a waste of resources. Using a binomial regression model, we found a difference in mentioning waste between individuals playing cooperative and selfish strategies in the high-cost condition ($\beta = 1.07$, 95% CI = [0.17, 1.96]) and this difference was smaller in the low-cost condition ($\beta_{\text{interaction}} = -1.44$, 95% CI = [−2.87, −0.01]). The estimated probabilities of seeing the costly signal as inefficient were 14% for cooperators, 13% for tempted and 30% for individuals playing selfishly in the high-cost condition and 16%, 16% and 12% in the low-cost condition. See table 2 for all estimates and figure 4*b* for illustration.

3.4. Contributions to the common pool (H3)

From 372 participants who proceeded to the second phase of the study, 284 actually participated (146 women; 1 non-binary; $M_{age} = 23.5$, $s.d._{age} = 3.0$). The remaining participants were either substitutes on a given experimental session or did not show up for a session. Note that we succeeded to collect data from 80 participants in the high-cost concealed group and low-cost revealed group as planned, but we missed data from four participants in the high-cost revealed group because not enough participants showed up for an experimental session. Moreover, only 61 participants chose the concealed group in the low-cost condition, and when accounting for participants who were selected as substitutes, this group comprised 48 participants instead of 80. Nevertheless, the *a priori* analysis plotted in electronic supplementary material, figure S1 suggests that 284 participants should be sufficient to detect the expected effects with 80% power for H3 and 75% power for H4.

Looking at the raw contributions, participants allocated 54% of their remaining endowment on average. The average allocations were highest in the first round (62%) and lowest in the last round (36%). Figure 5 provides an illustration of raw data. Since 45% of the allocations were either 0% or 100% of the endowment, we used the zero-or-one inflated beta regression (ZOIB) that allows to infer the probability of contributing zero or one as well as the size of the mean contribution (for technical details see [67]).

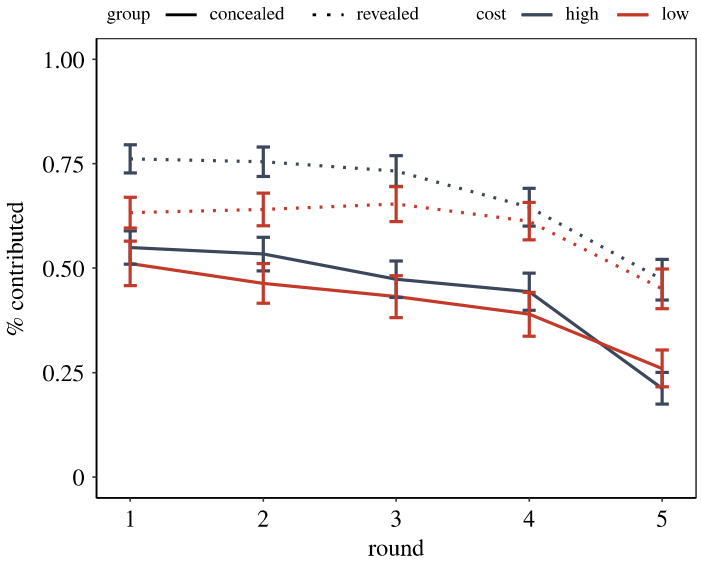

Figure 5. Means with SE of the proportion of remaining endowment contributed to the common pool.

Table 3. Beta-estimates from GLMM with 95% CI from testing hypotheses 3 (highest contributions in the high-cost revealed group) and 4 (highest earnings in the high-cost revealed group). *Note.* The reference category is 'concealed' for the group factor and 'high cost' for the condition factor. The estimates are untransformed.

	hypothesis 3 % sent	hypothesis 3 pr. sending 0	hypothesis 3 pr. sending 1	hypothesis 4
intercept	−0.23	−0.94	−1.57	5.66
	(−0.33, −0.13)	(−1.23, −0.64)	(−1.92, −1.23)	(5.62, 5.67)
group: revealed	0.55	−1.44	1.02	−0.003
	(0.40, 0.69)	(−1.96, −0.93)	(0.57, 1.46)	(−0.06, 0.06)
condition: low-cost	−0.02	−0.63	−0.71	−0.01
	(−0.18, 0.13)	(−1.11, −0.15)	(−1.29, −0.13)	(−0.08, 0.06)
low cost × revealed	−0.21	0.93	0.31	0.10
	(−0.42, 0.004)	(0.19, 1.68)	(−0.39, 1.02)	(0.004, 0.19)
N participants	284	284	284	284

For average contributions excluding zeros and ones across the five rounds of PGG, the model showed that participants in the high-cost revealed group contributed larger portions of their endowment compared with participants in the high-cost concealed group ($\beta = 0.55$, 95% CI = [0.40, 0.69]). This difference was smaller in the low-cost condition ($\beta_{\text{interaction}} = -0.21$, 95% CI = [−0.42, 0.004]). Furthermore, the parameter modelling the probability of contributing zero was smaller in the high-cost revealed group compared with the high-cost concealed group ($\beta = -1.44$, 95% CI = [−1.96, −0.93]) and this difference was again smaller in the low-cost condition ($\beta_{\text{interaction}} = 0.93$, 95% CI = [0.19, 1.68]). The estimated probabilities of contributing zero were 6% for the high-cost revealed group, 25% for the high-cost concealed group, 8% for the low-cost revealed group, and 16% for the low-cost concealed group. Finally, the parameter modelling the probability of contributing the full remaining endowment (conditioned on the probability of contributing zero) was higher in the high-cost revealed group compared with the high-cost concealed group ($\beta = 1.02$, 95% CI = [0.57, 1.46]). However, this difference was larger, albeit not reliable, in the low-cost condition ($\beta_{\text{interaction}} = 0.31$, 95% CI = [−0.39, 1.02]). While the probability of sending everything was the highest in the high-cost revealed group (34% compared with 26% in the low-cost revealed group), it was low in the low-cost concealed group (8% compared with 13% in the high-cost concealed group). Refer to table 3 for all estimates and figure 4*c* for illustration.

3.5. PGG earnings (H4)

For our final hypothesis test, we compared participants' earnings across conditions. Participants, on average, earned 294 CZK in PGG. Using a negative binomial regression to model count data (Poisson distribution was disqualified due to overdispersion), we found that contra our predictions there was no difference in earnings between the high-cost revealed group and the high-cost concealed group ($\beta = -0.003$, 95% CI = [−0.06, 0.06]). However, a larger difference was detected in the low-cost condition ($\beta_{\text{interaction}} = 0.10$, 95% CI = [0.004, 0.19]). Refer to table 3 for all estimates and figure 4*d* for illustration.

4. Discussion

In this registered report, we investigated whether participants with 'a cooperative phenotype' [1] (as indexed by a cooperative strategy in conditional PGG) would choose to reveal the quality of their phenotype using a costly signal and whether this signal would be associated with contributions to a group cooperative effort. Furthermore, we examined whether this relationship would hold only in the case of a highly costly signal. We found that the groups with costly signals mostly deterred participants with selfish strategies and that costlier signals were more effective (H1). However, the contrast in signal cost was not reliably estimated and needs further testing. We also found that participants with selfish intentions were more likely to mention unreasonable costs as a reason for not choosing the revealed group, and this probability increased with increasing signal cost (H2). Furthermore, groups with costly signals contributed larger portions of their remaining endowments to the common pool compared with the concealed groups, and these differences increased with increasing signal costs (H3). However, these larger contributions in the high-cost revealed group did not translate into larger earnings (H4). In summary, the results provide general support for the positive effects of costly signals on assorting cooperators and subsequent cooperation in a joint cooperative task but suggest that frequent high costs would not be evolutionarily stable. There are several important caveats to this conclusion that warrant discussion.

Whereas the separation mechanism worked rather well in the high-cost condition by driving most of the individuals playing selfishly into the concealed group and the majority of the cooperators into the revealed group, the mechanism still allowed for a semi-separating equilibrium where participants playing a selfish strategy chose the revealed group. Even more importantly, high cost deterred many cooperators from choosing the revealed group, suggesting that while increasing cost may better filter out selfish strategies, it also filters out many cooperators to the potential detriment of the joint action (as indicated by the wide 95% CIs of interaction between the group and cost variables in the statistical model testing H1). This result suggests that the mapping of cooperative phenotype on the costly signal is not a simple proportional process as envisioned by the strategic choice model [4], at least not for humans. Humans exhibit substantial communicative flexibility that can be situationally detached from the underlying phenotype. Hence, applying the strategic choice model to humans necessitates an amendment of additional parameters covering this flexibility.

One of these parameters should relate to the cost/benefit perception as suggested by Sosis [20]. We found support for his assertion that the differential perception of the cost size deters individuals playing selfishly from choosing the revealed group (H2). According to Sosis, this perception is facilitated by the socialization process whereby individuals regularly partake in costly actions (e.g. collective rituals) associated with their group's normative system, effectively discounting the perceived costs of future participation. While our data cannot attest to the role of the socialization process, we speculate that at least in our study, cooperative phenotype affected not only the perception of costs but also benefits. That is, rather than differently perceiving costs due to engaging in costly behaviours during the socialization process, it is the differential perception of potential benefits that affected the assessment of cost size as appropriate or wasteful. Looking at the pilot data—where we asked participants about their expected earnings—indicated that participants in the revealed group (compared with the concealed group) had a higher probability of saying they expect to receive the maximum amount from full cooperation ($\beta = 1.55$, 95% CI = [0.52, 1.03]). However, this finding may not apply in real life, where ritual participation does not typically precede any specific cooperative dilemma but rather a host of cooperative opportunities. In such situations, differential cost perception appears as a more likely driver of decisions to partake in costly signalling.

Notwithstanding this evidence, a stronger test of Sosis' proposition [20] would be a modification of our current design into a one-shot PGG. Although free-riders would probably invade the high-cost revealed group more often in the one-round set-up, showing that the assorting mechanism related to cost perception is functional, albeit limited, even in such a set-up would provide robust support for Sosis' proposition [20] (contra to [50]).

Regarding the effects of the separation mechanism on subsequent behaviour in PGG, we observed that participants in the high-cost revealed group invested the largest proportion of their remaining endowment (H3). Compared with participants in the low-cost revealed group, the high-cost revealed group also had lower probability of contributing nothing. Although the high-cost revealed group had the highest probability of contributing everything to the common pool, the predicted interaction effect between group and condition was not reliably estimated due to a relatively high probability of contributing everything in the high-cost concealed group, probably resulting from the large number of participants who played the cooperative strategy but refused joining the revealed group. While we did not statistically assess the difference between the revealed and concealed groups in the low-cost condition, raw data suggests that participants in the low-cost revealed group also invested larger proportions of their remaining endowment compared with participants in the concealed group. One possible explanation for this result is that despite a higher probability that a particular session in the low-cost revealed group would contain individuals playing selfishly, the low-cost signal still allowed many groups to establish a cooperative exchange. Crucially, due to a lower signal cost, the relatively successful assortment of cooperators translated into the highest earnings for this group (contra H4). This result has a plethora of interesting implications for real-life signalling contexts such as human ritual behaviour.

Since high-cost rituals often involve pain, physical effort and the expenditure of material resources, they are usually performed only on special occasions during one's lifetime (such as various rites of passage) or only occasionally during the liturgical year. In our study, the high-cost signal was sent in each PGG round, which turned out to be counterproductive in terms of the overall earnings. Having the high cost only during the first PGG round (such as an initiation ritual) or appearing only in some cyclical intervals would perhaps better simulate the real-world signalling behaviours. Indeed, for everyday mundane cooperative exchanges, low-cost regular signalling may be sufficient to stabilize a profitable level of trustworthy interactions. This conclusion is in accord with a signalling study by Chvaja *et al*. [68] that contrasted the trustworthiness of foot pilgrims to Santiago de Compostela (religious pilgrimage) with the trustworthiness resulting from participation in a Christian mass and in a secular activity. While pilgrims were rated as most trustworthy, the difference between pilgrimage and mass participation was smaller than the difference between mass participation and secular activity. Rather than a linear effect of cost, the difference between no signal and a low-cost signal is probably more important than the difference between low-cost and high-cost signal. Furthermore, in the study of two Indian villages, Power [36,38] showed that regular low-cost signals are more predictive of reputation for being trustworthy because high-cost signals may sometimes be seen as means to individual aggrandizement. While this would not be the case in our study because participants in the low-cost condition did not know about the high-cost condition, a direct comparison of high- versus low-cost choice could shed light on the perception of high-cost signals.

A preliminary inference from the current results could be that cultural evolutionary processes would pressure signal costs to be in equilibrium with expected benefits. For example, regular high-cost signals may be stable only in high-stake contexts such as combats or risky hunts where assorting cooperators without free-riders would be a crucial factor determining a group's success. In support of this conjecture, a survey of ethnographies describing ritual practices in 60 small-scale societies [54] revealed that ritual cost is positively predicted by the frequency of warfare the society experiences. Whereas the imperfect sorting mechanism in the low-cost condition afforded a profitable level of cooperative exchange in our study, the presence of free-riders would presumably disintegrate the group's cooperative effort in high-stake contexts. Our design might be easily modified to test this prediction by pitting various signalling groups against each other and comparing how the competitive context affects the workings of the sorting mechanism and subsequent cooperation.

Other important modifications to the design of the current study could alter the currency of signals and benefits. Signal costs and cooperative benefits are often disassociated in real-life settings such as costly rituals where signallers may, for example, use suffering and pain as the currency of the signal while getting helped in the future as the currency of the benefit. To provide a stronger test of CST, we decided to keep the currency of costs and benefits identical, but it could be speculated that if the cost would be, for instance, time spent on a boring task, participants in the high-cost revealed group might earn the most (due to the possibility to turn their endowment into larger investments rather

than waste them as signals). Furthermore, while having the currency of signals and benefits identical allowed us to assess the role of cooperative phenotype in signalling within the specific context of PGG, it could be speculated that cooperative phenotype would manifest differently in different cooperative contexts. To indicate this uncertainty, we talk about different cooperative strategies specific to PGG throughout the paper rather than hard-coded cooperative types [cf., [1].

In summary, this registered report provides an experimental framework that can be easily amended to examine particular extensions of CST. In our OSF repository, we provide all materials used in the current study, which can be used to replicate this study in different populations or to extend the protocol in order to further empirically develop the strategic choice model. Since cooperative communication is the cornerstone of human group living, understanding factors affecting the reliability of such communication may help us better appreciate the cooperative peculiarity of humankind.

Ethics. The research was approved by the Research Ethics Committee at Masaryk University.

Data accessibility. Data, materials and analytical code are publicly available at OSF: https://osf.io/vsjcp/. The Stage 1 manuscript associated with this Registered Report was granted in-principle acceptance on 11 June 2021 prior to data collection and analysis. The accepted Stage 1 manuscript, unchanged from the point of in-principle acceptance, may be viewed at https://osf.io/c63xk.

The data are provided in electronic supplementary material [69].

Authors' contributions. M.L.: conceptualization, data curation, formal analysis, funding acquisition, investigation, methodology, project administration, supervision, visualization, writing—original draft, writing—review and editing; R.C.: conceptualization, data curation, investigation, methodology, project administration; B.G.P.: conceptualization, methodology, supervision; D.V.: conceptualization, methodology, supervision; R.S.: conceptualization, investigation, methodology, resources, supervision.

All authors gave final approval for publication and agreed to be held accountable for the work performed therein.

Conflict of interest declaration. The authors declare no competing interests.

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Advertising cooperative phenotype through costly signals facilitates collective action

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Supplementary information

1. Supplementary figures

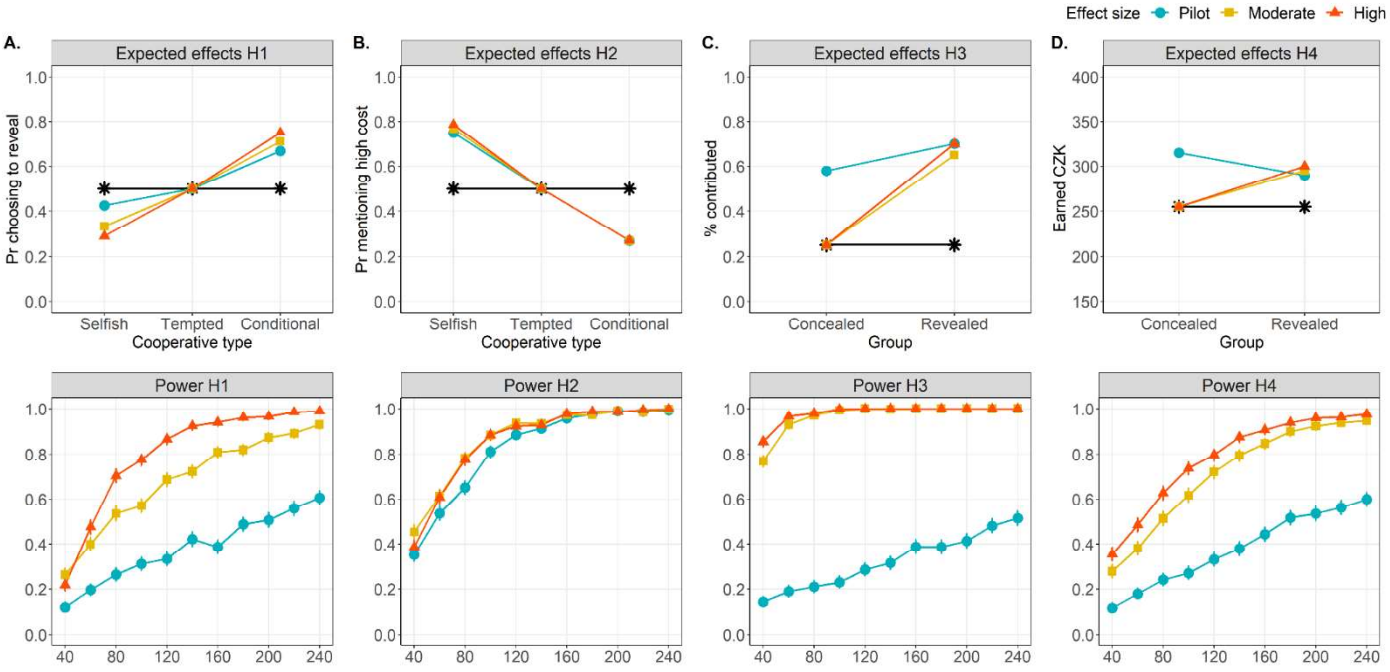

Figure S1 | Power analysis for our main hypotheses. The upper row represents expected effects for each hypothesis. The black lines with asterisk represent the low-cost condition where we expect no effects. Coloured lines represent the effects in the high-cost condition. Specifically, the blue lines with dots apply pilot results to our planned design. Since the pilot data were only hypothetical and not based on iterated PGG, we expected to detect stronger effects in the actual study (especially in H3 and H4; see Pilot data in the main text), denoted by yellow squared and red triangled lines. The lower row represents estimated power for each effects size (that is difference in slopes between the low- and high-cost conditions). Points are estimated power, and error bars are 95% CIs. The X-axis represents the number of participants per group. Power was estimated by simulating 1000 data sets for each combination of sample size per condition (high-cost vs. low-cost) and effect size. The chosen sample size was set at 160 per condition (320 in total). Note that the effect sizes for H4 (D.) are based on expected mean contribution in H3 (C.). Also note that while we plan to analyse H3 using the beta regression, power was estimated using sampling from a normal distribution because the package *simr* (Green & Macleod, 2016) used for data simulation does not allow to model beta distribution (but pilot data showed that both distributional assumptions lead to practically identical results). Likewise, the first model to be tested for H4 assumes the Poisson distribution, but since pilot data showed overdispersion, we foresaw the need to use the negative binomial distribution. As with H3, this distribution is not implemented in *simr* and we used the normal distribution for power analysis.

2. Additional pilot details and analyses

2.1. Cooperative values and trustworthiness scales

To assess the hidden cooperative phenotype, we used a scale adapted from a study by Peysakhovich et al. (Peysakhovich et al., 2014) that correlated self-reported measures of cooperative values with cooperative decisions in three economic games. Using a sample of 567 US individuals recruited on MTurk, Peysakhovich et al. showed that the cooperative values score positively predicts cooperative decisions in the Dictator Game, Trust Game, and Public Goods Game. The cooperative values scale included the following items (on a Likert scale 1-5 from "Strongly disagree" to "Strongly agree"):

1. I would support an increase in taxes if it were used to help the less well-off in society.
2. I would support an increase in taxes if it were used to prevent environmental pollution.
3. I would support an increase in taxes if it were used to help poorer nations.
4. It is very important to obey all laws and regulations.
5. People should be willing to help others who are less fortunate.
6. It is important to allow people you do not know well to borrow items of some value, such as dishes or tools.
7. It is not very important to do small favors for others. For example, looking after a person's plants, mail, or pets while they are away. (R)
8. It is ok to steal small items from your workplace. (R)
9. These days people need to look after themselves and not worry about others. (R)

We conducted three independent translations of these items into the Czech language and reached a consensus on the best translation. Next, we piloted the scale on a sample of 157 Czech university students. This pilot revealed that items 4 and 8 poorly loaded on the one-factor solution in the principal component analysis (loadings scores of 0.26 and 0.12, respectively); hence, we decided to drop those items from the pilot study. The resulting cooperative values scale had seven items that were sufficiently inter-correlated (Bartlett's test of sphericity: $\chi 2$ (21) = 378.98, $p < .001$) and sampled adequately (Kaiser-Meyer-Olkin test - KMO: MSA = 0.76) in factor analysis with oblique rotation ("oblimin"). Cronbach's alpha for the scale was 0.78, suggesting good consistency. Dropping any of the seven items would not increase the scale's consistency. To create the latent score of cooperative values, we averaged the seven items comprising the scale.

We also collected data on participants' trustworthiness using a scale from McAuliffe et al. (McAuliffe et al., 2019) and assessed how trusting participants were using a question on generalised trust "Do you think that, on the whole, people can be trusted or that you cannot be too careful in dealing with people?" from the Czech version of the World Values Survey (Inglehart et al., 2014). Furthermore, we assessed education w by asking participants about the highest level of obtained education (from primary school = 1 to Ph.D. = 7; the intermittent education levels were specific for the Czech educational system) and economic status by asking participants about satisfaction with their available finances on a scale 0-10. However, these data are not used in the present analysis.

2.2. Perceived costs

We coded free responses to questions about the reasons for selecting either the revealed or concealed group for the perception of the signal cost as too high. Specifically, we coded responses that used words such as 'waste,' 'loss,' or 'unnecessary' in relation to the signal cost. We also included phrases that referred positively to the absence of cost. Together, we coded 28% of responses as perceiving the cost of the revealed group as too high. Below we list several examples translated from Czech to English, and the complete list of Czech responses can be found in the Supplementary data set published at OSF:

"The extra fee is dissuading."

"Membership in this group [concealed] does not require any entry cost. I saved 20 CZK."

"I see the entry fee as a loss."

"I do not want to pay fees if I do not have to."

"Unnecessarily wasted money."

2.3. Conditional co-operators

As discussed in the main text, it is recognised that contributions to the common pool in PGG are, at least for some players, dependent on their beliefs about other players' game strategies (Rabin, 1993). While the Nash equilibrium in an anonymous one-shot PGG for all is to defect (when the multiplication factor is < n), having information about cooperative preferences of other people (or other mechanisms allowing co-operators to assort) may stabilise cooperation for "conditional co-operators" (Fischbacher et al., 2001). Conditional co-operators are players who are willing to contribute a large portion of their endowment to the common pool if they can be sure that other players will contribute as well. A study by Fischbacher et al. (Fischbacher et al., 2001) that we refer to by the acronym FGF) showed that when participants selected how much they would contribute to the common pool based on the other players' contributions, about half of the participants chose to match the mean group contribution closely. That is, the size of their contribution to the common pool was conditioned on the contributions of other players. In contrast, "selfish individuals" always chose zero or minimal contributions, irrespective of the group mean contribution. A third strategy that emerged, "tempted individuals" (described also as humpback cooperation or triangle cooperation), would match others' contributions up to the half of their endowment and then invest less with each additional step in the group mean (i.e., the increase in earnings would be more and more tempting).

In our pilot data, we examined whether the assumed cooperative phenotype of our participants (assessed using the cooperative values scale) corresponds rather to a general altruistic tendency or to conditional co-operation (that we aim to use in the main study). We asked participants to imagine a hypothetical scenario where they would know other players' contributions. We used three versions of group contributions: all other players contribute their full endowment; all other players contribute 40% of their endowment; and one player contributes full endowment, one player

'0' and one player 10%. (Note that in contrast to FGF, our design was between-subject rather than within-subject). The results of this manipulation suggest that the concept of cooperative phenotype corresponds to conditional cooperation: in the hypothetical scenarios where other players contributed either their full endowment or an amount fixed at 40% of their endowment, cooperative values positively predicted the hypothetical contribution to the common pool roughly matching the rest of the team contributions using GLMM with the beta family (logit $\beta_{100\%}$ = 0.37, 95% CI = [-0.05 – 0.78]; logit $\beta_{40\%}$ = 0.83, 95% CI = [0.26 – 1.40]). This effect was weaker and more variable if the group comprised a mix of free-riders and co-operators (logit β_{mix} = 0.29, 95% CI = [-0.23– 0.82]). These initial explorations suggest a match between cooperative phenotype and conditional cooperation assumed for the main study in this manuscript.

2.4. Semi-separating equilibrium

While we expected that participants with the cooperative phenotype would separate into the revealed group, the pilot data analysis, including the reasons for choosing either of the groups, suggested otherwise. Interestingly, while researchers usually propose a semi-separating equilibrium to account for selfish individuals who chose to signal and then free ride (Aimone et al., 2013), our data suggest that the imperfect separation concerns committed co-operators. As displayed in Figure 1E, more than half of the participants who chose the concealed group scored > 3 on the cooperative values scale. This distribution may explain why we did not observe a substantial difference in the hypothetical earnings between the revealed and concealed groups. Indeed, the concealed group comprised committed co-operators who were willing to risk higher contribution to the common pool, using the money that would be wasted in the revealed group as additional resources contributed to the common pool.

Further exploration of the reasons for choosing the concealed group revealed that these participants often distrusted the signal (i.e., they would express concerns that costly signals would not prevent free-riding). Below are several examples:

"I do not think that the fee guarantees that people would contribute any amount."

"I do not think that the irreversible deposit in group Y would necessarily guarantee loyalty toward the whole group. On the contrary, some individuals could use the fact that they will look trustworthy due to the deposit and then contribute nothing."

"It is meaningless to give up the money; it does not ensure that others would contribute more."

Coding these responses as '1' and the rest of responses as '0' in the concealed group (in the revealed group, no one expressed their mistrust of the signal as should be expected) and using the cooperative values variable as a predictor revealed that these values positively predicted the probability of mentioning distrust of the signal as a reason for choosing the concealed group (logit β = 0.97, 95% CI = [0.01– 1.94]). In the main study, we assume that this semi-separating equilibrium will largely disappear in the high-cost condition due to the iterated nature of PGG.

3. References

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