



Religion, parochialism and intuitive cooperation

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Religions promote cooperation, but they can also be divisive. Is religious cooperation intuitively parochial against atheists? Evidence supporting the social heuristics hypothesis (SHH) suggests that cooperation is intuitive, independent of religious group identity. We tested this prediction in a one-shot prisoner's dilemma game, where 1,280 practising Christian believers were paired with either a coreligionist or an atheist and where time limits were used to increase reliance on either intuitive or deliberated decisions. We explored another dual-process account of cooperation, the self-control account (SCA), which suggests that visceral reactions tend to be selfish and that cooperation requires deliberation. We found evidence for religious parochialism but no support for SHH's prediction of intuitive cooperation. Consistent with SCA but requiring confirmation in future studies, exploratory analyses showed that religious parochialism involves decision conflict and concern for strong reciprocity and that deliberation promotes cooperation independent of religious group identity.

Protocol registration

The Stage 1 protocol for this Registered Report was accepted in principle on 28 January 2020. The protocol, as accepted by the journal, can be found at <https://doi.org/10.6084/m9.figshare.12086781.v1>.

Many world religions have scriptures and rituals that regulate prosocial behaviour. It is perhaps not a coincidence that the expansion of large-scale cooperative networks coexisted with the emergence and spread of these religious teachings and practices^{1–4}. Historical records, cross-cultural studies and laboratory results indicate that religious belief promotes cooperation, at least among believers^{3,5–7}. This widespread cultural phenomenon may be an evolutionary adaptation or a by-product⁸. However, it is not yet clear whether the cooperativeness of religious believers is general (that is, inclusive of out-groups) or whether it is parochial (that is, biased against out-groups)^{9–12}. The distinction is crucial to ongoing debates on the role of religion in the public sphere^{13,14}, since parochialism emphasizes the need to protect religious minorities and secular institutions. Furthermore, the form that these protections should take (for example, behavioural interventions or 'nudges') depends on the cognitive underpinnings of the phenomena in question, such as whether religious discrimination is intuitive (for example, relying on spontaneous associations and simple heuristics) and whether it is amenable to change through deliberation.

Cooperation often requires one to make a personal sacrifice for the sake of group benefit. Various psychological and social mechanisms have been put forward to explain how religious belief promotes cooperation. Belief in god can increase cooperation in social dilemmas through motivational mechanisms that counteract incentives to free-ride. Such changes in incentive structures can be achieved through religious teachings of benevolence¹⁵ as well as through fear of a punitive and omnipotent god^{16,17}. Consistent with this motivational view, the psychological salience of religious and punitive concepts has been found to increase altruism towards anonymous others^{18,19}, and regular attendance at religious services has been associated with charitable giving²⁰. Religious belief can also support cooperation through its positive effects on trust and the consequent coordination of behaviour⁹. Given the prosociality of religious behavioural norms and the fear of punishment for their violation, social identity as a religious believer works as a valuable

signal of trustworthiness in reciprocal social interactions. Because most people in social dilemmas are willing to cooperate conditionally (that is, to the extent that they believe others will cooperate)^{21–24}, religious identity further strengthens cooperation^{9,25}, particularly in religious social networks^{26–28}.

In short, religious belief promotes cooperation, especially when religious identity is a reliable signal of trustworthiness and prosociality. However, the personal benefits of signalling religiosity expose religious identity to exploitation by free-riders posing as religious believers. This threat is often countered by costly displays of faith (for example, regular participation in religious public rituals), which help screen out those without a genuine belief in god (or fear of supernatural punishment) for whom the psychological costs of participation are often too high⁶. The consequent increase in the reliability of this socially valuable information may, however, come at the cost of increased distrust and systematic discrimination against atheists and believers of other religions.

The evidence remains mixed regarding the question of whether religious prosociality is general or parochial. Whereas widespread anti-atheist prejudice suggests parochialism^{9,11}, some studies find that religiosity increases altruism and cooperativeness in general¹², even towards atheists¹⁰. Recent cross-cultural evidence for the parochialism of religious belief further suggests that religious prejudice may be intuitive, taking shape through spontaneous associations^{11,29}. These findings motivate us to ask whether intuitive religious biases in judgements extend to behavioural biases in cooperation, namely whether religious cooperation is intuitively parochial, and whether deliberation helps to reduce such discrimination.

The primary goal of this study is to investigate the extent to which the social heuristics hypothesis (SHH) provides answers to these questions. Built on the background of dual-process models of the mind³⁰, SHH posits that social decisions can be driven either by more intuitive and low-effort or by more deliberated and high-effort cognitive processes^{31–33}. According to SHH, intuitive decisions reflect simple heuristics acquired in previous social

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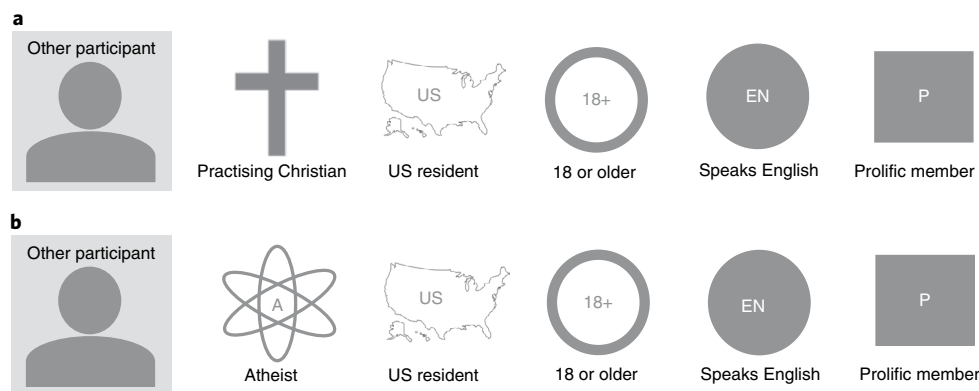


Fig. 1 | Group identity manipulations. a, b, Participants previously self-described as Christians regularly participating in public religious rituals ($n=1,280$) were either not shown the identity information of their PD game partner or assigned to one of two social media profile conditions: **(a)** the In-Group condition, where their partner was described as a practising Christian or **(b)** the Out-Group condition, where their partner was described as an atheist. The additional information on the profiles did not vary across the two conditions. The positions of the five information items were counterbalanced. The data from an equal number of atheists, recruited to avoid deception, are not analysed here in detail. The figure displays a simplified version of the actual images used in the study.

interactions, which tend to be cooperative³². Supporting SHH, cognitive process manipulations that enhance intuitive thinking (such as time pressure, cognitive load or priming) have been shown to increase cooperation in games involving social dilemmas^{31,32,34–36}. Furthermore, previous tests of SHH among natural and minimal groups showed both strong group bias and intuitive cooperation but no interaction between cognitive and group manipulations^{34,37–39}. Consequently, accumulated evidence for SHH supports the hypothesis that cooperation is intuitive in general (that is, independent of group identity).

We tested the generality of intuitive cooperation by observing the cooperative behaviour of practising religious believers in a one-shot continuous prisoner's dilemma (PD) game^{40,41}. In the PD game, a pair of participants individually and simultaneously decide how much of an initial monetary endowment to keep for themselves and, as our measure of cooperation, how much to give to the other participant, where any money given is doubled before being transferred. PD constitutes a social dilemma by making personal monetary sacrifice necessary for increasing the pair's total earnings. In the PD game, practising Christians were randomly paired with either a coreligionist (In-Group) or an atheist (Out-Group), and PD decisions were elicited either under 10 s time pressure (TP, for inducing decisions that are relatively more intuitive) or under 20 s time delay (TD, for inducing decisions that are relatively more deliberated). Hence, we study group bias in cooperation among practising believers by randomly manipulating the religious identity of their pair in the PD game, while at the same time manipulating the cognitive processes involved in their PD decision.

H_1 : Believers will be intuitively cooperative in general such that those assigned to the intuition condition (TP) will be more cooperative than those assigned to the deliberation condition (TD) independent of the religious identity of their pair. We seek evidence for H_1 by jointly testing for intuitive cooperation (that is, the main effect of time limits in the hypothesized direction) and for its generality (that is, the lack of an interaction effect with a pair's religious identity) (see Methods).

In contrast to the above-mentioned evidence supporting SHH, the generalizability of the phenomenon of intuitive cooperation has been questioned^{42,43}. Since cooperative heuristics thrive in contexts of routine cooperation and wither with routine exposure to selfishness^{44–46}, a likely explanation for the strength of intuitive cooperation is variation in background social experiences and the consequent differences in social heuristics^{32,47}.

A secondary goal of our study is to explore whether an alternative approach, the self-control account (SCA), can provide further insights into the psychology of cooperation. SCA posits that automatic visceral reactions are often selfish and that cooperation requires effortful deliberation and self-control⁴⁸. Regular participation in communal religious practices may result in experiences where prosociality and trust towards coreligionists emerge as a cooperative heuristic, and where atheism may be (even if implicitly) associated with selfishness and distrust. For a believer, the identity of an interaction partner as a practising coreligionist would then cue cooperative heuristics, while the prospect of interacting with an atheist may cue selfishness²⁶. Particularly for this latter case, SCA suggests that deliberation increases cooperation by allowing control over visceral selfish reactions^{48–50} and by encouraging impartial moral judgements of fairness and equality^{51–53}. Nevertheless, with few exceptions (for example, Isler, Gächter, Maule and Starmer, unpublished manuscript), evidence supporting SCA remains correlational and suggestive. Support for our exploratory analysis of SCA would provide a basis for future confirmatory hypothesis tests.

Our study provides a strong test of SHH in the context of naturally occurring (and possibly contrasting) heuristics. It also allows exploration, based on suggestive evidence for SCA, of whether religious cooperative behaviour is intuitively parochial. A more nuanced dual-process account of parochialism in cooperation would also be possible if, for example, SHH were valid for only in-group while SCA were valid for only out-group behaviour. The intuitive cooperation account of SHH, however, predicts intuitive cooperation independent of whether the recipient is in-group, out-group or without group identity. While the In-Group and Out-Group conditions provide a comparison of these contrasting predictions, we also ran a control condition without identity manipulation (No-Group), allowing a test of SHH as in the original studies³¹. We surmised that the comparison of SHH's deliberated selfishness account with SCA's deliberated cooperation account may help us discover whether deliberation can be employed to mitigate intuitive religious parochialism.

Results

We recruited 1,280 practising Christian believers and 1,280 atheists on the online platform Prolific (see Participants). Our analysis does not focus on the atheist participants, who were recruited to avoid deception. The number of religious believers in our sample did not statistically differ across the six experimental conditions

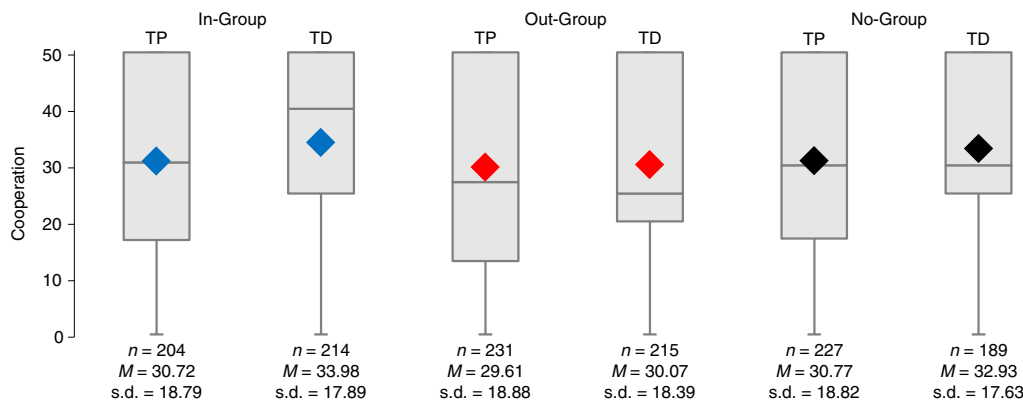


Fig. 2 | Cooperation among believers across experimental conditions. Cooperation (that is, the amount transferred to the pair in the PD game out of an endowment of 50 cents) among 1,280 previously self-reported practising Christians under 10 s time pressure (TP) and 20 s time delay (TD) towards coreligionists (In-Group), atheists (Out-Group) or pairs without identity information (No-Group). Box plots indicate the mean (diamonds), median (centre line), upper and lower quartiles (box limits) and first quartile, including the minimum (whiskers).

($\chi^2(2, n = 1,280) = 2.775, P = 0.250$). These six groups were similar in their main demographic features (Supplementary Table 1). Consistent with previous social dilemma experiments, a Shapiro–Wilk test indicated that cooperation of believers in the PD game was not normally distributed ($W(1,280) = 0.98, P < 0.001$). The distribution of cooperation was trimodal, with 12.3% of religious believers giving none, 19.5% giving half and 39.3% giving all of their endowment to the other participant. We use statistical tests that are standard in and appropriate for the analysis of social dilemma experiments with a large number of observations. All tests are two-tailed, except for ANOVAs, χ^2 tests and equivalence testing, which are based on single-tailed distributions by design. Except for equivalence testing that uses a 90% confidence interval (see Methods), we report 95% confidence intervals and we use parentheses to denote open intervals (e.g., to exclude value zero) or square brackets to denote closed intervals.

Manipulation checks. Compliance with time limits among religious believers was 81.0% in TP and 81.9% in TD. Response times under TP ($MD = 6.95$ s, $s.d. = 7.30$ s) were faster than under TD ($MD = 26.36$ s, $s.d. = 115.7$ s) (Wilcoxon rank-sum, $z = 26.53, P < 0.001, d = 0.31, 95\% \text{ CI } [0.20, 0.42]$). The composite of two self-report questions on the effects of time limits on cognitive processes (that is, having limited time to think and deciding on the basis of ‘gut reaction’) was higher under TP ($M = 3.12, s.d. = 1.01$) than TD ($M = 2.47, s.d. = 0.82$) ($t(1,278) = 12.75, P < 0.001, d = 0.71 [0.60, 0.83]$). Religious believers in the group identity conditions (Fig. 1) reported higher subjective closeness to their pair in the In-Group condition ($M = 3.46, s.d. = 1.94$) than in the Out-Group condition ($M = 2.72, s.d. = 1.63$) ($t(862) = 6.10, P < 0.001, d = 0.42 [0.28, 0.55]$). Hence, these three preregistered tests indicate that our manipulations worked as intended.

Preregistered analysis. Figure 2 depicts the behaviour of practising Christians in the PD across the experimental conditions. A two-way ANOVA on the group identity conditions indicated higher cooperation towards in-group than out-group pairs (with point estimate of $M_{\text{In-Group}} - M_{\text{Out-Group}} = 3.91 [0.41, 7.72]$), $F(1, 860) = 3.98, P = 0.046, \eta_p^2 = 0.005 (0, 0.018]$). However, we failed to provide evidence for the general intuitive cooperation (H_1) predicted by SHH; there was no main effect of time limits on cooperation ($M_{\text{TD}} - M_{\text{TP}} = 3.26 [-0.29, 6.81]$) ($F(1, 860) = 2.19, P = 0.140, \eta_p^2 = 0.003 [0, 0.014]$). There was also no significant interaction ($F(1, 860) = 1.23, P = 0.267, \eta_p^2 = 0.001 [0, 0.011]$). The No-Group conditions, estimated separately to test SHH as in the original studies, also did not reveal any

evidence for intuitive cooperation ($M_{\text{TD}} - M_{\text{TP}} = 2.16 [-1.38, 5.70]$) ($t(414) = 1.20, P = 0.231, d = 0.12 [-0.08, 0.31]$).

The lack of evidence for intuitive cooperation rendered irrelevant the equivalence test planned to check the generality of intuitive cooperation (see Methods), which we nevertheless report for completeness: the upper bound of the 90% CI for the interaction effect size ($\eta^2 = 0.009$) was less than the smallest effect size of interest (SESOI = 0.012). Bayesian analysis with default priors is consistent with the equivalence test result and provides strong support for the null hypothesis ($\text{BF}_{10} = 0.023$).

Exploratory analysis. Here, we explore the effect of time limits on cooperation decisions from the contrasting perspectives of SHH (predicting intuitive cooperation) and SCA (predicting intuitive selfishness). For this purpose, we use four two-way ANOVA models (M1a–M4a). Unlike the confirmatory analysis and to achieve more powerful tests, these exploratory models include all experimental conditions, reflecting the broader 2 (TP or TD) \times 3 (In-Group, Out-Group or No-Group) experimental design. The first model (M1a) uses the complete sample of 1,280 practising Christians, whereas the next three models are based on subsamples excluding those who reported being experienced with PD experiments (M2a), those who did not comprehend the social dilemma (M3a) or those who did not self-describe as practising Christians during data collection (M4a). Whenever possible, the models include experience with PD experiments and two questions measuring social dilemma comprehension as preregistered control variables (see Control measures). In the overall sample (that is, M1a), cooperation was negatively correlated with understanding of the self-gain maximization strategy ($r = -0.072 [-0.126, -0.017], P = 0.010$) and positively correlated with understanding of the group-gain maximization strategy ($r = 0.212 [0.159, 0.264], P < 0.001$), but it was not significantly correlated with PD experience ($r = -0.027 [-0.082, 0.028], P = 0.332$). While M1a and M4a control for all three variables, due to exclusions, M2a controls only for the understanding measures while M3a controls only for experience. Next, we describe these models in more detail.

Experience with economic games has been shown to weaken intuitive cooperation^{32,47}. In response to a replication attempt that failed to find evidence for SHH among Amazon Mechanical Turk participants,⁴³ evidence for intuitive cooperation emerged when the sample was restricted to those 17.2% who had no experience with economic games.⁴⁷ We recruited practising Christians on Prolific, most of whom reported inexperience with PD experiments (74.1%). M2a restricts the analysis to these 948 inexperienced participants.

Table 1 | Cooperation among believers across four exploratory models

	M1a: complete			M2a: inexperienced			M3a: comprehended			M4a: current		
	<i>M</i>	<i>s.d.</i>	<i>n</i>	<i>M</i>	<i>s.d.</i>	<i>n</i>	<i>M</i>	<i>s.d.</i>	<i>n</i>	<i>M</i>	<i>s.d.</i>	<i>n</i>
Time pressure												
In-Group	30.7	18.8	204	31.7	18.5	148	32.0	19.7	104	32.2	18.2	164
Out-Group	29.6	18.9	231	30.1	18.8	165	27.6	19.9	141	29.9	18.7	188
No-Group	30.8	18.8	227	30.8	18.6	184	30.6	19.0	120	31.2	18.5	188
Time delay												
In-Group	34.0	17.9	214	34.3	17.8	146	34.6	18.4	130	35.3	17.2	179
Out-Group	30.1	18.4	215	30.5	18.5	169	31.3	19.4	121	30.9	18.5	172
No-Group	32.9	17.6	189	32.8	17.8	136	35.0	18.4	97	33.6	17.1	159
	η_p^2	95% CI	<i>P</i>	η_p^2	95% CI	<i>P</i>	η_p^2	95% CI	<i>P</i>	η_p^2	95% CI	<i>P</i>
ANOVA												
Time limit	0.004	(0, 0.013]	0.028	0.003	[0, 0.014]	0.088	0.009	(0, 0.027]	0.014	0.004	(0, 0.015]	0.041
Group identity	0.003	[0, 0.011]	0.151	0.004	[0, 0.014]	0.158	0.009	(0, 0.026]	0.044	0.004	[0, 0.014]	0.111
Interaction	0.002	[0, 0.008]	0.330	0.001	[0, 0.009]	0.501	0.000	[0, 0.005]	0.866	0.001	[0, 0.007]	0.609

Note: Cooperation by practising Christians in the PD game analysed across four exploratory models: the complete experimental sample (M1a), among those inexperienced with the PG game (M2a), among those who comprehended the social dilemma (M3a) and among those who currently identify as practising Christian (M4a). The top two blocks describe cooperation mean (*M*), standard deviation of cooperation (*s.d.*) and number of observations in condition (*n*) by time limits (pressure or delay) and group identity manipulations (In-Group, Out-Group or No-Group). The bottom block describes the effect size (η_p^2), 95% confidence interval (CI) and significance level (*P*) for the main effects of time limit and group identity manipulations and their interaction in the corresponding two-way ANOVA models.

We measured social dilemma comprehension with two standard questions about (1) the monetary self-gain maximization strategy (63.5% correct) and (2) the monetary group-gain maximization strategy (78.7% correct). In line with previous findings showing that time pressure does not harm understanding^{35,54} the rate of social dilemma comprehension—those correctly answering both questions—did not differ between the time-limit conditions (56.3% in TD and 55.1% in TP) ($\chi^2(1, n=1,280) = 0.179, P=0.672$). On the other hand, restricting analysis to those with comprehension of the game rules has previously supported SCA⁵⁴. Therefore, M3a is restricted to analysis of the 713 participants with PD comprehension.

The information used as sample selection criteria was previously elicited by Prolific, which could have been outdated at the time of the study. The survey elicited as part of our study revealed that, among the 1,280 recruits, 52 no longer self-identified as Christian believers and a further 178 declared they no longer regularly participated in religious public ceremonies. M4a restricts the sample to the 1,050 currently practising Christian believers.

Table 1 describes the cooperation rates of believers and treatment effects across the four models. Contrary to SHH and in support of SCA (Fig. 3), cooperation was higher under TD than under TP for each group identity condition across all four models. On average, cooperation was higher under TD than under TP, by 6.4% in M1a, 5.0% in M2a, 12.6% in M3a and 7.1% in M4a. The main effect of time limits on cooperation was statistically significant for three models, including the complete sample of believers (M1a) ($F(1, 1271) = 4.83, P=0.028$), those with social dilemma comprehension (M3a) ($F(1, 706) = 6.12, P=0.014$) and those who satisfied the screening criteria at the time of the study (M4a) ($F(1, 1041) = 4.17, P=0.041$). Even among believers who were inexperienced with the PD game (M2a), for whom statistical estimates did not provide clear evidence for SHH or SCA ($F(1, 940) = 2.92, P=0.088$), there was no evidence of a decrease in cooperation with deliberation (Fig. 3). The main effect of group identity manipulation was weakened with the inclusion of the No-Group condition into the analysis, and was significant only in M3a ($F(2, 706) = 3.14, P=0.044$). Likewise, evidence for SCA did not seem to depend on religious group identity, as the interaction effect was not significant in any of the models

(*P* values ≥ 0.330), although this may also stem from a lack of statistical power in detecting small interaction effects.

To further evaluate the robustness of these exploratory findings and increase the power of the associated statistical tests, we estimated modified versions of the four models described above that included all participants in our experiment—not only the believers but also the atheists. The modified models (M1b to M4b) have the same configuration as the initial models (M1a to M4a) but additionally include participant type as an independent factor, involving 2 (believer or atheist) \times 2 (TP or TD) \times 3 (In-Group, Out-Group, or No-Group) three-way ANOVAs: As detailed in Table 2, the evidence for SCA was robust to the inclusion of atheists in the analysis, resulting in significant main effect of time limits on cooperation in all four models. Specifically, cooperation was higher under TD than under TP, by 4.2% in the complete sample (M1b) ($F(1, 2545) = 4.96, P=0.026$), by 5.5% among those inexperienced with the PD game (M2b) ($F(1, 1823) = 5.95, P=0.015$), by 6.7% among those with social dilemma comprehension (M3b) ($F(1, 1574) = 4.75, P=0.003$) and by 4.3% among those who currently identify as either practising Christian or atheist (M4b) ($F(1, 2225) = 4.03, P=0.045$). All four models showed a significant main effect of group identity manipulation (*P* values ≤ 0.009), but none of the models indicated a significant main effect of participant type (*P* values ≥ 0.396) nor interactions between any of the factors (*P* values ≥ 0.142).

Finally, using two measures elicited after the PD—decision conflict and expected cooperation—we explore the cognitive drivers of religious parochialism in cooperation. Since these were elicited without time limits, we focus here on the effect of group identity manipulations. Decision conflict measures, on a scale ranging from 0 to 100, the difficulty of choosing how much to keep and how much to share with one's partner in the PD⁵⁵, providing in our context a subjective correlate of religious parochialism. In both conditions, decision conflict experienced by religious believers showed small to moderate negative correlations with cooperation behaviour (In-Group: $r = -0.201$ [$-0.291, -0.107$], $P < 0.001$; Out-Group: $r = -0.152$ [$-0.242, -0.060$], $P = 0.001$). Believers found it easier to cooperate with coreligionists than atheists, as they reported experiencing stronger feelings of decision conflict in the Out-Group

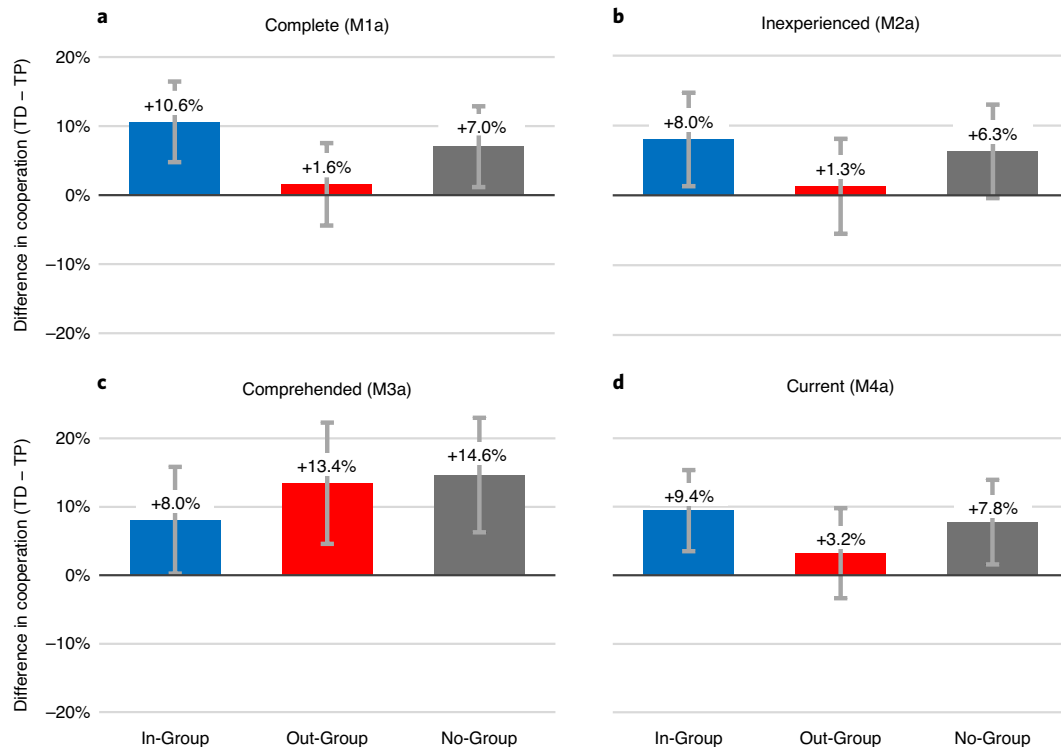


Fig. 3 | Difference in cooperation among believers between time-limit conditions (TD – TP). **a–d**, Difference in mean cooperation by practising Christians in the PD game between time delay (TD) and time pressure (TP) conditions as a percentage of cooperation in TP for **(a)** the complete sample of believers (M1a, $n=1,280$), **(b)** those without experience of PD experiments (M2a, $n=948$), **(c)** those with correct social dilemma comprehension (M3a, $n=713$) and **(d)** current practising Christians (M4a, $n=1,050$). Cooperation indicates monetary allocations in the PD game towards coreligionists (In-Group), atheists (Out-Group) or pairs without identity information (No-Group). Error bars indicate standard errors.

condition ($M=37.85$, $s.d.=32.43$) than in the In-Group condition ($M=33.04$, $s.d.=30.57$) ($t(862)=2.24$, $P=0.025$, $d=0.15$ [0.02, 0.29]). These two findings together suggest that decision conflict is involved in religious parochialism in cooperation.

Expected cooperation, on the other hand, measures participants' beliefs regarding the cooperation decisions of their pair in the PD game^{23,56}. This measure allows exploration of whether strong reciprocity—the motivation to cooperate at personal cost conditional on the belief that others will do so as well⁵⁷—drives religious parochialism in cooperation. Actual and expected cooperation were highly correlated for religious believers interacting with both coreligionists ($r=0.745$ [0.699, 0.785], $P<0.001$) and atheists ($r=0.684$ [0.632, 0.731], $P<0.001$). Furthermore, these participants expected their in-group coreligionist PD pairs to be more cooperative towards them ($M=30.00$, $s.d.=16.51$) than their out-group atheist pairs ($M=26.56$, $s.d.=17.40$) ($t(862)=2.97$, $P=0.003$, $d=0.20$ [0.07, 0.34]). These results suggest that strong reciprocity drives religious parochialism in cooperation identified in the confirmatory analysis.

Discussion

We studied Christian believers who regularly participated in public religious rituals, since regular social interactions among coreligionists can be expected to result in cooperative heuristics towards in-group members. Contributing to the debates about the role of religion in the public sphere reviewed earlier^{13,14}, we found evidence for parochialism based on religious identity, with Christians cooperating more with coreligionists than with atheists. However, we failed to find support for generalized intuitive cooperation (H_1). This hypothesis, derived from SHH^{31–33} and implied by recent findings³⁴, predicts that Christian believers assigned to the intuition condition (TP) would be more cooperative than those assigned to

the deliberation condition (TD) independent of the religious identity of their pair. Neither was there any support for SHH in conditions where no group identity was revealed, which were run to provide comparability with the original studies. At least at first sight, our results are consistent with the interpretation emerging from the accumulated evidence that intuitive cooperation is either non-existent overall⁵⁸ or small in effect size when time-pressure manipulations are used⁵⁹.

Our exploratory analyses, on the other hand, provided evidence for intuitive selfishness as predicted by SCA. Across three of the four models tested among believers, including a model with the complete sample of participants and a model restricted to Christian believers actively practising at the time of the study, cooperation was found to increase with deliberation independent of group identity. These models used all experimental conditions to increase statistical power (including those without group identity information), and where applicable, they controlled for the preregistered covariates of experience with and comprehension of the PD game. The model that provided strongest evidence for SCA restricted the analysis to those who comprehended the social dilemma underlying the PD. Even in the model that failed to provide conclusive evidence (M2a), where those with experience in the PD game were excluded from the analysis, average cooperation was higher when participants were encouraged to deliberate. Furthermore, the main effect of time limits was significant in the direction of SCA when four additional models were estimated using all participants—both believers and atheists. These exploratory findings highlight the need for future confirmatory tests of SCA. One should also be cautious when interpreting estimates based on restricted subsamples, since these exclusions are open to annulment of random assignment and to sample selection bias⁶⁰. Nevertheless, while we found no confirmatory

Table 2 | Cooperation among all participants across four exploratory models

	M1b: complete			M2b: inexperienced			M3b: comprehended			M4b: current		
	<i>M</i>	<i>s.d.</i>	<i>n</i>	<i>M</i>	<i>s.d.</i>	<i>n</i>	<i>M</i>	<i>s.d.</i>	<i>n</i>	<i>M</i>	<i>s.d.</i>	<i>n</i>
Time pressure												
In-Group	32.4	18.7	404	32.4	18.6	288	32.8	19.4	254	33.3	18.3	364
Out-Group	29.4	18.8	443	29.7	18.7	311	28.0	19.6	285	29.5	18.8	400
No-Group	30.3	18.5	445	30.9	18.0	333	30.3	18.7	266	30.5	18.3	406
Time delay												
In-Group	33.7	18.1	427	34.6	17.6	292	33.3	18.9	260	34.3	17.8	392
Out-Group	29.7	19.2	423	30.7	19.1	322	30.6	20.2	270	30.0	19.3	380
No-Group	32.4	18.1	418	32.9	17.6	291	33.1	19.1	252	32.7	17.9	388
	η_p^2	95% CI	<i>P</i>	η_p^2	95% CI	<i>P</i>	η_p^2	95% CI	<i>P</i>	η_p^2	95% CI	<i>P</i>
ANOVA												
Participant type	0.000	[0, 0.001]	0.999	0.000	[0, 0.003]	0.686	0.000	[0, 0.005]	0.396	0.000	[0, 0.004]	0.398
Time limit	0.002	(0, 0.007]	0.026	0.003	(0, 0.010]	0.015	0.003	(0, 0.011]	0.029	0.002	(0, 0.007]	0.045
Group identity	0.006	(0, 0.013]	0.001	0.005	(0, 0.013]	0.009	0.007	(0, 0.016]	0.005	0.007	(0, 0.015]	0.001
Interaction	0.001	[0, 0.004]	0.380	0.000	[0, 0.003]	0.675	0.000	[0, 0.001]	0.923	0.000	[0, 0.003]	0.693

Note: Cooperation by practising Christians and atheists in the PD game analysed across four exploratory models: the complete experimental sample (M1b), among those inexperienced with the PG game (M2b), among those who comprehended the social dilemma (M3b) and among those who currently identify as practising Christian or atheist (M4b). The top two blocks describe cooperation mean (*M*), standard deviation of cooperation (*s.d.*) and number of observations in condition (*n*) by time limits (pressure or delay) and group identity manipulations (In-Group, Out-Group or No-Group). The bottom block describes effect size (η_p^2), 95% confidence interval (CI) and significance level (*P*) for the main effects of participant type (believer or atheist), time limits and group identity manipulations and the three-way interaction in the corresponding three-way ANOVA models. None of the two-way interactions were significant ($P \geq 0.142$).

evidence for SHH in any of our models, our study provides support for SCA when considering the complete sample of participants.

How can we reconcile the evidence supporting SCA in our exploratory analyses and elsewhere in the literature^{48,50,54,55} with previous support for SHH^{31,34–36}? Pointing towards a resolution, we note that the two phenomena—intuitive cooperation predicted by SHH and intuitive selfishness predicted by SCA—have different premises regarding the underlying social and cognitive processes. While SHH relies on mental shortcuts developed during past social interactions, SCA points towards a primordial—visceral and instinctive—response for self-protection⁶¹. In principle, the two effects can therefore coexist with varying magnitudes across decision-making contexts such that, overall, one may dominate the other. As they may also cancel each other out, these two independent mechanisms can also explain the overall weak or null effect of tests of intuitive cooperative behaviour in social dilemmas^{42,58,59}. Therefore, procedures for disentangling the two phenomena are needed for conducting independent tests of SCA and SHH. For example, to allow relatively isolated tests of SHH, social heuristics can be developed in the laboratory by repeated exposure to cooperative social dilemma environments^{44,46}. Similarly, cultural comparisons can help identify social contexts where cooperative heuristics are prevalent^{45,62}, and framing manipulations can help identify the contextual cues that trigger them⁶³.

Novel procedures that independently test SCA are also needed. A potential candidate relates to the ongoing debate about whether miscomprehension of the social dilemma confounds tests of intuitive cooperation^{54,64–66}. Other things being equal, systematic misperception of the experimental task is methodologically undesirable, since participants with misperceptions may be playing a different game than intended by the researchers. However, SHH predicts intuitive cooperation in part because of such a misperception. Accordingly, people develop prosocial heuristics since regular cooperation among affiliates tends to be self-serving, but deliberation will reveal cooperation to be a mistake in the particular case of anonymous one-shot games. In this sense, the misperception that the one-shot PD game does not involve a social dilemma is arguably

a necessary condition for observing support for SHH. Hence, providing extensive instructions about the dilemma and screening participants on the basis of comprehension (for example, using control questions⁶⁷) can provide independent tests of SCA by minimizing intuitive cooperation due to social heuristics. Consistent with this argument as well as with previous findings in the literature⁵⁴, our model that excluded participants with social dilemma miscomprehension provided no evidence for SHH and showed even stronger exploratory evidence for SCA.

We initially asked whether cooperation depends on religious group identity and whether religious parochialism in cooperation has an intuitive basis. Although religious believers in our sample did not exhibit intuitive cooperation, they were parochial, giving more to coreligionists than to atheists in the PD game. Exploratory tests provided suggestive evidence that strong reciprocity, and to some extent decision conflict, drive religious parochialism in cooperation. In other words, believers tend to cooperate more with coreligionists than with atheists because they expect coreligionists to be more cooperative, and because they feel less conflicted when making such a decision. While this goes against recent findings of generalized religious prosociality¹⁰, it is consistent with strong meta-analytic evidence for in-group favouritism in cooperation across various domains⁶⁸.

Our experimental protocol, used to manipulate group identity, is likely to have influenced our finding on religious parochialism. We used a quasi-naturalistic setting, where an online profile was used to reveal multiple group identity attributes simultaneously, thereby mimicking the social media profiles that people regularly use to learn about others (Fig. 1). In our case, the religious group identity of ones' partner in the PD game was varied to induce in-group and out-group manipulations, while country of residence, age group, language and recruitment platform membership were kept constant across the group identity conditions. The use of a profile has the advantage of increased ecological validity, and it can limit socially desirable responding by obscuring the manipulation. However, this comes at the cost of weakening the experimental manipulation (that is, religious affiliation). Although we did find evidence for in-group

favouritism, the effect size was smaller than that found in the literature, indicating that it may have been dampened by the presence of other in-group attributes. In particular, country of residence as an in-group attribute may have evoked strong binding reactions by cueing nationality. Future research on parochialism should vary multiple attributes to estimate the importance of religious identity relative to others.

In conclusion, our study provides exploratory support for SCA, but this does not necessarily refute SHH because the two accounts refer to different social and cognitive processes. Future research is needed to improve our understanding of the economic and psychological factors that determine which of the two phenomena—intuitive cooperation or intuitive selfishness—is likely to be dominant in a given decision context. Without this understanding, the question as to when public policies should appeal to intuition and when they should appeal to deliberation remains open. We initially sought in this project to investigate whether parochialism can be weakened by policies that promote deliberation. While we found no evidence for an intuitive basis for religious discrimination, our results suggest that nudging deliberation can promote cooperation independent of group identity.

Methods

Overview. Our research complies with all relevant ethical regulations. Ethics approval was obtained from the University of Leeds Research Ethics Committee, and informed consent was received from participants at the outset of the study. An incentivized prisoner's dilemma (PD) game was used to study cooperation behaviour. Participants were recruited from previously self-declared practising Christians and atheists, who were randomly assigned to one of six conditions while playing the PD game. Data on atheists are not analysed here in detail since this study focuses on the decisions of Christian participants. The experiment involved a 3 (religious group identity of one's pair in the game: practising Christian, atheist or no identity) \times 2 (time limit: 10 s time pressure or 20 s time delay) between-subjects design. Each participant was randomly assigned to one of six experimental conditions. Participants and the researchers were blind to the conditions of the experiment during data collection. All manipulation checks and applicable control measures showed that the manipulations worked as intended.

Power analysis. We estimated our sample size on the basis of the hypothesized main effect, and let this sample size determine the smallest effect size that can be detected for the hypothesized lack of an interaction effect. To do so, we used the most relevant effect size for the main effect of time-limit manipulations found in the literature³⁵—a test of SHH on a sample recruited from Prolific using a similar protocol ($f = 0.11$). Because the one-shot PD game does not involve interaction or feedback, each individual decision in the game constitutes an independent observation. To detect a main effect of time limit of this size in a two-way ANOVA model with $\alpha = 0.05$ and $1 - \beta = 0.95$, we estimated using G*Power 3.1.9.2 that our sample should consist of at least 1,280 believers³⁹. Sensitivity analysis indicated that the minimum interaction effect size that can be detected in a two-way ANOVA model with $n = 1,280$, $\alpha = 0.05$ and $1 - \beta = 0.95$ is $\eta^2 = 0.012$, which we took to be our smallest effect size of interest (SESOI)^{70,71}. Although we focus on the behaviour of believers, we avoided deception by also recruiting 1,280 atheists, who were paired either with each other or with believers in the PD game.

Hypothesis tests. In a two-way ANOVA model of the PD decisions of religious believers on religious identity and time-limit factors, H_1 would be supported by evidence (1) for intuitive cooperation in a null-hypothesis significance test (that is, a significant main effect of time limits on cooperation such that cooperation is higher under time pressure than under time delay) and (2) for the generality of intuitive cooperation in a one-tailed equivalence test showing the lack of a significant interaction effect. While step (1) is operationalized as indicating evidence if $P < 0.05$, evidence in step (2) would be indicated by the upper bound of the 90% confidence interval of the interaction effect size (η^2) being less than 0.012 (that is, excluding the SESOI). In step (2), we also calculate a Bayes factor (BF) for the interaction effect, as confirmation such that $BF \leq 1/3$ is interpreted as substantial evidence for the null result.⁷²

Participants. We recruited participants from Prolific (<https://prolific.co/>) and conducted our experiment online. Data generated online, including Prolific, have been shown to replicate various well-established laboratory results^{73,74}, including incentivized games measuring cooperation⁷⁵. We used Prolific because it allows prescreening on the basis of a previously completed comprehensive demographic questionnaire, including religious affiliation and practices. Participants were adult US residents fluent in English. We report data on 1,280 practising Christians,

recruited among those who in the Prolific questionnaire answered 'Christianity' for the question 'What is your religious affiliation?' and chose either 'Yes. Both public and private.' or 'Yes. Public only.' for the question 'Do you participate in regular religious activities?' The sample of believers had a balanced gender distribution (54% female) and an age distribution ranging from 18 to 77 yr ($M = 35.60$ yr, $s.d. = 12.98$ yr). The majority (74.1%) of these participants reported that they have not previously participated in an experiment involving PD games. An equal number of atheists, recruited to avoid deception, were selected among those who answered 'Non-religious' to the religious affiliation question and who then qualified their answer as 'Atheist' in the follow-up question 'Which of the following do you most identify as?'. Participants with complete submissions earned a participation fee (\$0.25 USD), in addition to their earnings from the PD game.

Materials and procedures. *Materials.* A copy of the experimental materials is available at the OSF study preregistration page (<https://osf.io/kzwgn/>).

Procedure. We conducted the experiment using the Qualtrics software (<https://www.qualtrics.com/>). After eliciting informed consent, participants received training on the slider tool to increase their familiarity with the interface for eliciting PD decisions³⁵. They next read a general description of the study, explaining that there were three parts and that, after the study was over, one part was to be selected at random for determining participant's additional payments from the study. Participants were not informed about the tasks involved in upcoming parts beforehand. The first part included the main task, namely the one-shot PD game, whereas the other two parts included exploratory measures of social dilemma comprehension and social expectations (see below). The procedure for randomly selecting one of the three parts for determining additional payments is an effective cost-saving method, well established in experimental economics⁷⁶, with theoretical support for its incentive compatibility⁷⁷ and significant evidence that participants consider each part independently^{78,79}.

The main task was a one-shot PD game and included the experimental manipulations. Compliance with time limits was incentivized to strengthen cognitive manipulations³⁵. After reading the instructions for the PD game at their own pace, a transitory screen explained the time limits and the monetary incentives for compliance. This screen was displayed for at most 30 s, or less if participants chose to proceed earlier, allowing time for reading while preventing deliberation about the upcoming task. Next came the PD decision screen, which first revealed—for participants in the identity manipulation conditions—an 'online profile' of each participant's pair in the game and, after 2 s, displayed the slider tool and a timer. The PD decision was elicited under one of two time-limit conditions (that is, 10 s time pressure or 20 s time delay). Afterwards, manipulation checks and exploratory measures were elicited, followed by a brief questionnaire including basic demographic information.

Prisoner's dilemma (PD). We used a one-shot continuous prisoner's dilemma (PD) game, relying on instructions used in the previous literature³⁹. In the PD, a pair of participants individually decided, without observing each other's actions, how much of \$0.50 (USD) to keep and how much of it to allocate (in 1 cent increments) to their pair. The amount allocated to the pair (a whole number ranging from 0 to 50 cents) is our measure of cooperation. If PD was selected for payment, participants earned double the amount allocated to them by their pair in addition to any money they kept for themselves. From each participant's perspective, the game involved a strict trade-off between personal earnings and total earnings by the two participants, rendering it a social dilemma. In a previous social dilemma experiment on Prolific ($N = 3,653$), using a four-person public-good game with marginal per capita return of 0.5, we found that 63.6% of endowments was given to the public good ($s.d. = 29.6$), that 6.4% of participants gave nothing and that 25.1% gave everything (Isler, Gächter, Maule and Starmer, unpublished manuscript). With substantially lower time and effort required for its completion (median completion time 5 min), our study provides a ratio between endowment size and opportunity cost that is comparable to laboratory studies. Furthermore, a large-scale meta-analysis found no overall effect of stakes on giving in dictator games⁸⁰, and similar findings are reported elsewhere^{81–85}. Finally, a recent study found evidence of religious prosociality in low-stake (\$1) games using explicit primes⁸⁶.

Group assignment. Practising Christians played the PD game in equal probability either with another practising Christian (In-Group), with an atheist (Out-Group) or with someone without identity information (No-Group). Participants did not know that they had been recruited on the basis of their religious identity because the prescreening questions were elicited beforehand by Prolific. Participants in the identity manipulation conditions (but not those in the control condition) were informed on the PD instruction screen that the decision screen would show an 'online profile' describing their pair in the game. Specifically, modifying a previously established method¹⁰, the decision screen revealed (in balanced Latin square order) the other participant's religious identity ('Christian' or 'Atheist') together with four constant, in-group identity information categories (country of residence, age group, language and experimental platform). This approach was intended to minimize demand characteristics (since deciding on the basis of

multiple identity categories makes religious belief less focal) and to increase the realism of the experimental setting (since acquiring information from social media profiles with these kinds of group identity categories is a familiar experience). Identity information was paired with symbols to speed comprehension (for example, the Christian cross, the atheism symbol, a map of the United States etc.).

Time-limit manipulations. The PD decision was elicited either under 10 s time pressure (TP) with prompts to 'be quick' or under 20 s time delay (TD) with prompts to 'carefully consider' the decision. On the basis of previously developed methods, we incentivized compliance with time limits³⁵, and we informed participants that additional earnings from the PD were highly likely to be invalidated by noncompliance. The uncertainty prevents the annulment of incentivization that could otherwise occur in cases of noncompliance. We in fact randomly chose 90% of noncompliant decisions to be invalid. We did not inform participants of the probability of invalidation for noncompliance ($P=0.9$) so as not to induce a calculative mindset.

Control measures. We planned various controls to check whether: (1) our manipulations affected decision processes as intended, (2) the information used for sample selection is accurate, (3) our sample is representative in that it replicates well-established behavioural biases and our results are (4) robust when controlling for experience and comprehension in the PD game and (5) specific to religious believers or generalizable to other natural groups. Since we did not find evidence for intuitive cooperation, we followed our preregistered procedure and did not conduct the last control measure (5) (see Result generalisability check).

Manipulation checks. We committed to three tests to check that our manipulations worked as intended. First, as a behavioural test of time-limit manipulations, we checked whether the median response time under time pressure was faster than the median response time under time delay using a Wilcoxon rank-sum test. In addition, immediately following the PD game, three questions were elicited in two randomly presented screens to check that time limit and religious group identity conditions manipulated cognitive processes as intended. On the time manipulation check screen, participants rated in random order their agreement with two statements on a five-point scale: (1) 'I did not have time to think through my decisions' (indicating limited opportunities for deliberation), and (2) 'I decided based on my "gut reaction"' (indicating increased spontaneity of decisions). As an indication of successful manipulation of cognitive processes by time limits, an independent-samples *t* test of significant differences in average scores for the two questions between the two time limit conditions was estimated. On the group identity manipulation check screen, participants completed the online version of the Inclusion of the Other in the Self (IOS) scale, a simple and reliable measure of subjective closeness of social relationships⁸⁷. The seven-point IOS question asked active participants to select one of seven pairs of circles with increasing areas of intersection that best described the relationship between the active participant ('You') and the passive participant ('Other'). Successful group manipulation would be indicated by a significant difference in an independent-samples *t* test between the In-Group and Out-Group conditions.

Screening information check. Information on religious affiliations and practices was previously elicited by Prolific. We used two of these questions as screening criteria during data collection (see Participants section). The survey section of our study also elicited answers to these same questions, to check the accuracy of the information used in the selection of practising Christians. Before data collection, we committed to reporting the hypothesis test results on the basis of the identity information elicited in our survey if the match rate on the religious affiliation question was less than 90%. In fact, this match rate was 95.9%. However, because the match rate was 82.0% when considering questions about both religious affiliation and participation in public rituals, we report the hypothesis test results for this restricted sample as part of the exploratory analysis.

Sample behaviour check. The design allows a test of whether our sample of believers is representative in showing commonly observed biases. A significant main effect of religious group identity in the two-way ANOVA, such that believers cooperate more with other believers than with atheists, would replicate the commonly observed group bias.

Experience and comprehension check. The PD game was described in a survey question to elicit participants' experience with the game from past participation in experiments. In addition, we measured comprehension of the social dilemma by eliciting via sliders what participants thought were the self-gain maximizing strategy (that is, keeping all endowment for self) and the group-gain maximizing strategy (that is, giving all endowment to the recipient) in the PD game. Participants had the opportunity to earn \$0.25 for each correct answer. Those who incorrectly answered either question can be considered as having miscomprehended the social dilemma. As standard³⁶, we did not exclude those with miscomprehension or experience from the confirmatory analysis. In exploratory models, we either controlled for them as covariates (M1a,b and M4a,b) or excluded them from analysis (M2a,b and M3a,b).

Result generalisability check. As compared with atheists, practising believers are more likely to have experienced cooperative interactions (and adopted cooperative intuitions) on the basis of religious identity. Conditional on finding evidence for the hypothesis of intuitive cooperation among believers, we planned to test for intuitive cooperation among atheists to check whether intuitive cooperation extends to other natural groups. Given that no evidence was found for intuitive cooperation, we will report atheist behaviour elsewhere.

Additional measures. Expected cooperation. Participants predicted the allocation made by their pair. To incentivize truthful reporting of expectations, participants had the opportunity to earn \$0.50 for predictions that were accurate within 5 cents. Expectations provide a measure of trust towards one's pair⁸⁸. We explore whether differences in expected cooperation are consistent with differences in actual cooperation behaviour (for example, group bias).

Decision conflict. We elicited self-reported subjective conflict experienced during the PD decision. The measure, based on previous literature⁵⁵, uses a scale ranging from 0 (not at all) to 100 (very much) as response to the question 'Some participants find it difficult to make a decision regarding how much money to keep personally and how much to share with others because they find the two goals equally important. To what extent did you experience such a conflict when making your decision?' We explore whether experimental manipulations affected the experience of decision conflict.

Data exclusions. As preregistered, incomplete ($n=77$) and duplicate ($n=19$) submissions were excluded from the analyses. We considered a submission to be complete if it had a valid Prolific ID, which anonymously referred to a unique participant, and if all parts, including the survey, had been completed. On the basis of Prolific ID, we excluded duplicate submissions except for the initial submission, if this initial submission was complete and if it did not coincide in time with another submission by the same participant.

Reporting summary. Further information on research design is available in the Nature Research Reporting Summary linked to this article.

Data availability

The data are available at the OSF study preregistration page (<https://osf.io/kzwgn/>).

Code availability

The analysis code is available at the OSF study preregistration page (<https://osf.io/kzwgn/>).

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Author contributions

O.I. and O.Y. conceived the initial idea and design, which was improved with contributions by A.J.M. O.I. wrote the manuscript, which was revised by all three authors. O.I. collected and analysed the data. All authors had access to the data and approved the final version.

Competing interests

The authors declare no competing interests.

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Study description	Quantitative experimental
Research sample	We recruited fluent English-speaking US residents who were 18 years or older. We analyse data on 1,280 practicing Christians. The sample was non-representative. An equal number of atheists, to be analysed elsewhere, were also recruited to avoid deception.
Sampling strategy	We estimated our sample size based on the hypothesized main effect, and let this sample size determine the smallest effect size that can be detected for the hypothesized lack of an interaction effect. To do so, we used the most relevant main effect size for time-limit manipulations found in the literature—test of SHH on a sample recruited from Prolific using a similar protocol ($f = 0.11$). To detect a main effect of time-limit of this size in a two-way ANOVA model with $\alpha = 0.05$ and $1 - \beta = 0.95$, we estimated using G*Power 3.1.9.2 that our sample should consist of at least 1280 believers. Sensitivity analysis indicated that the minimum interaction effect size that can be detected in a two-way ANOVA model with $n = 1280$, $\alpha = 0.05$ and $1 - \beta = 0.95$ is $\eta^2 = 0.012$, which we took to be our smallest effect size of interest (SESOI). Although we focus on the behaviour of believers, we avoided deception by also recruiting 1280 atheists, who were paired either with each other or with believers in the PD game. Convenience sampling was used.
Data collection	Data were collected online. The researchers did not interact with the participants and they did not know which treatments the participants received during data collection. Participants were blind to the experimental conditions.
Timing	February 16, 2020 - February 19, 2020
Data exclusions	Incomplete ($n = 77$) and duplicate ($n = 19$) submissions were excluded from the analyses as planned.
Non-participation	77 participants started but did not complete the study without reason given for incompleteness.
Randomization	Participants were randomly allocated to experimental conditions.

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|----------------------------|---|
| Population characteristics | The sample of believers had a balanced gender distribution (54% female) and an age distribution ranging from 18 to 77 ($M = 35.60$, $SD = 12.98$). The majority of these participants (74.1%) reported that they have not previously participated in an experiment involving PD games. |
|----------------------------|---|

Recruitment

Participants were recruited via Prolific (<https://www.prolific.co/>). Our results are unlikely to be affected by sample self-selection because our sample was highly unfamiliar with the experimental tasks (see above).

Ethics oversight

Ethics approval was obtained from the University of Leeds Research Ethics Committee.

Note that full information on the approval of the study protocol must also be provided in the manuscript.