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Religion, Parochialism and Intuitive Cooperation

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Abstract

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 test this prediction in a one-shot prisoner's dilemma game, where 1,280 practicing Christian believers are paired either with a coreligionist or an atheist and where time-limits are 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.

Main

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-product[8]. However, it is not yet clear whether the cooperativeness of religious believers is general (i.e., inclusive of out-groups) or whether it is parochial (i.e., 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 (e.g., behavioural interventions or "nudges") depends on the cognitive underpinnings of the phenomena in question, such as whether religious discrimination is intuitive (e.g., 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

62   mechanisms that counteract incentives to freeride. Such changes in incentive structures can be achieved

63   through religious teachings of benevolence[15] as well as through fear of a punitive and omnipotent

64   god[16,17]. Consistent with this motivational view, the psychological salience of religious and punitive

65   concepts have been found to increase altruism towards anonymous others[18,19], and regular attendance

66   at religious services has been associated with charitable giving[20]. Religious belief can also support

67   cooperation through its positive effects on trust and the consequent coordination of behavior[9]. Given

68   the prosociality of religious behavioural norms and the fear of punishment for their violation, social

69   identity as a religious believer works as a valuable signal of trustworthiness in reciprocal social

70   interactions. Because most people in social dilemmas are willing to cooperate conditionally (i.e., to the

71   extent that they believe others will cooperate)[21–24], religious identity further strengthens cooperation[9,25],

72   particularly in religious social networks[26–28].

73   In short, religious belief promotes cooperation, especially when religious identity is a reliable

74   signal of trustworthiness and prosociality. However, personal benefits of signalling religiosity expose

75   religious identity to exploitation by free-riders posing as religious believers. This threat is often

76   countered by costly displays of faith (e.g., regular participation in religious public rituals), which help

77   screen out those without genuine belief in god (or fear of supernatural punishment) for whom the

78   psychological costs of participation are often too high[6]. The consequent increase in the reliability of this

79   socially valuable information may, however, come at the cost of increased distrust and systematic

80   discrimination against atheists and believers of other religions.

81   The evidence remains mixed regarding the question of whether religious prosociality is general

82   or parochial. Whereas widespread anti-atheist prejudice suggests parochialism[9,11], some studies find

83   that religiosity increases altruism and cooperativeness in general[12], even towards atheists[10]. Recent

84   cross-cultural evidence for the parochialism of religious belief further suggests that religious prejudice

85   may be intuitive, taking shape through spontaneous associations[11,29]. These findings motivate us to ask

86   whether intuitive religious biases in judgments extend to behavioural biases in cooperation, namely,

87   whether religious cooperation is intuitively parochial, and whether deliberation helps to reduce such

88   discrimination.

89   The primary goal of our study is to investigate the extent to which the Social Heuristics

90   Hypothesis (SHH) provides answers to these questions. Built on the background of dual-process models

91   of the mind[30], SHH posits that social decisions can be driven either by more intuitive and low-effort or by

92   more deliberated and high-effort cognitive processes[31–33]. According to SHH, intuitive decisions reflect

93   simple heuristics acquired in previous social interactions, which tend to be cooperative[32]. Supporting

94   SHH, cognitive process manipulations that enhance intuitive thinking (such as time-pressure, cognitive-

95   load or priming) have been shown to increase cooperation in games involving social dilemmas[31,32,34–36].

96   Furthermore, previous tests of SHH among natural and minimal groups showed both strong group bias

97   and intuitive cooperation but no interaction between cognitive and group manipulations[34,37–39].

98   Consequently, accumulated evidence for SHH supports the hypothesis that cooperation is intuitive in

99   general (i.e., independent of group identity).

100   We tested the generality of intuitive cooperation by observing cooperation behaviour of

101   practicing religious believers in a one-shot continuous prisoner's dilemma (PD) game[40,41]. In the PD

102   game, a pair of participants individually and simultaneously decides how much of an initial monetary

103   endowment to keep for themselves and, as our measure of cooperation, how much to give to the other

104   participant, where any money given is doubled before being transferred. PD constitutes a social

105   dilemma by making personal monetary sacrifice necessary for increasing the pair's total earnings. In the

106   PD game, practicing Christians were randomly paired with either a coreligionist (In-Group) or an atheist

107   (Out-Group), and PD decisions were elicited either under 10s time-pressure (TP, for inducing decisions

108   that are relatively more intuitive) or under 20s time-delay (TD, for inducing decisions that are relatively

109   more deliberated). Hence, we study group bias in cooperation among practicing believers by randomly

110   manipulating the religious identity of their pair in the PD game, while at the same time manipulating the

111   cognitive processes involved in their PD decision.

112   H_1: Believers will be intuitively cooperative in general such that those assigned to the intuition

113   condition (TP) will be more cooperative than those assigned to the deliberation condition (TD)

114   independent of the religious identity of their pairs. We seek evidence for H_1 by jointly testing for

115   intuitive cooperation (i.e., the main effect of time-limits in the hypothesized direction) and for its

116   generality (i.e., the lack of an interaction effect with a pair's religious identity) (see Methods).

117   In contrast to the above-mentioned evidence supporting SHH, the generalizability of the

118   phenomenon of intuitive cooperation has been questioned[42,43]. Since cooperative heuristics thrive in

119   contexts of routine cooperation and wither with routine exposure to selfishness[44–46], a likely explanation

120   for the strength of intuitive cooperation is variation in background social experiences and the

121   consequent differences in social heuristics[32,47].

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122   A secondary goal of our study is to explore whether an alternative approach, the Self-Control

123   Account (SCA), can provide further insights into the psychology of cooperation. SCA posits that

124   automatic visceral reactions are often selfish and that cooperation requires effortful deliberation and

125   self-control[48]. Regular participation in communal religious practices may result in experiences where

126   prosociality and trust towards coreligionists emerge as a cooperative heuristic, and where atheism may

127   be (even if implicitly) associated with selfishness and distrust. For a believer, the identity of an

128   interaction partner as a practicing coreligionist would then cue cooperative heuristics, while the

129   prospect of interacting with an atheist may cue selfishness[26]. Particularly for this latter case, SCA

130   suggests that deliberation increases cooperation by allowing control over visceral selfish reactions[48–50]

131   and by encouraging impartial moral judgments of fairness and equality[51–53]. Nevertheless, with few

132   exceptions (e.g., Isler, Gächter, Maule & Starmer, unpublished manuscript), evidence supporting SCA

133   remains correlational and suggestive. Support for our exploratory analysis of SCA would provide a basis

134   for future confirmatory hypothesis tests.

135   Our study provides a strong test of SHH in the context of naturally occurring (and possibly

136   contrasting) heuristics. It also allows exploration, based on suggestive evidence for SCA, of whether

137   religious cooperation behaviour is intuitively parochial. A more nuanced dual-process account of

138   parochialism in cooperation would also be possible if, for example, SHH were valid only for in-group

139   while SCA were valid only for out-group behaviour. The intuitive cooperation account of SHH, however,

140   predicts intuitive cooperation independent of whether the recipient is in-group, out-group or without

141   group identity. While the In-Group and Out-Group conditions provide a comparison of these contrasting

142   predictions, we also ran a control condition without identity manipulation (No-Group) allowing a test of

143   SHH as in the original studies[31]. We surmised that the comparison of SHH's deliberated selfishness

144   account with SCA's deliberated cooperation account may help us discover whether deliberation can be

145   employed to mitigate intuitive religious parochialism.

Results

147   We recruited 1,280 practicing Christian believers and 1,280 atheists on the online platform Prolific (see

148   Participants in Methods). Our analysis does not focus on the atheist participants, who were recruited to

149   avoid deception. The number of religious believers in our sample did not statistically differ across the six

150   experimental conditions, χ^2 (2, n = 1,280) = 2.775, P = .250. These six groups were similar in their main

151   demographic features (see Supplementary Table 1). Consistent with previous social dilemma

experiments, a Shapiro-Wilk test indicated that cooperation by believers in the PD game was not normally distributed, $W(1280) = 0.98$, $P < .001$. 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 large number of observations. All tests are two-tailed, except for ANOVAs, χ^2 tests and equivalence testing that are based on single-tailed distributions by design. We report 95% confidence intervals in brackets, except for equivalence testing (see Methods).

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, $SD = 7.30$) were faster than under TD ($MD = 26.36$, $SD = 115.7$), Wilcoxon rank-sum, $z = 26.53$, $P < .001$, $d = 0.31$, 95% CI [0.20, 0.42]. The composite of two self-report questions on the effects of time-limits on cognitive processes (i.e., having limited time to think and deciding based on 'gut reaction') was higher under TP ($M = 3.12$, $SD = 1.01$) than TD ($M = 2.47$, $SD = 0.82$), $t(1278) = 12.75$, $P < .001$, $d = 0.71$ [0.60, 0.83]. Religious believers in the group identity conditions (see Fig. 1) reported higher subjective closeness to their pairs in the In-Group condition ($M = 3.46$, $SD = 1.94$) than in the Out-Group condition ($M = 2.72$, $SD = 1.63$), $t(862) = 6.10$, $P < .001$, $d = 0.42$ [0.28, 0.55]. Hence, these three preregistered tests indicate that our manipulations worked as intended.

Preregistered analysis. Fig. 2 depicts the behaviour of practicing 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 $M_{In-Group} - M_{Out-Group} = 3.91$ [0.41, 7.72]), $F(1, 860) = 3.98$, $P = .046$, $\eta_p^2 = .005$ (0, .018]. However, we failed to provide evidence for general intuitive cooperation (H_1) predicted by SHH; there was no main effect of time-limits on cooperation ($M_{TD} - M_{TP} = 3.26$ [-0.29, 6.81]), $F(1, 860) = 2.19$, $P = .140$, $\eta_p^2 = .003$ [0, .014]. There was also no significant interaction, $F(1, 860) = 1.23$, $P = .267$, $\eta_p^2 = .001$ [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 ($M_{TD} - M_{TP} = 2.16$ [-1.38, 5.70]), $t(414) = 1.20$, $P = .231$, $d = 0.12$ [-0.08, 0.31].

The lack of evidence for intuitive cooperation rendered irrelevant the equivalence test planned to check generality of intuitive cooperation (see Methods), which we 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 ($BF_{10} = 0.023$).

182   **Exploratory analysis.** Here, we explore the effect of time-limits on cooperation decisions from the

183   contrasting perspectives of SHH (predicting intuitive cooperation) and SCA (predicting intuitive

184   selfishness). For this purpose, we use four two-way ANOVA models (M1a-M4a). Unlike the confirmatory

185   analysis and to achieve more powerful tests, these exploratory models include all experimental

186   conditions, reflecting the broader 2 (TP or TD) by 3 (In-Group, Out-Group or No-Group) experimental

187   design. The first model (M1a) uses the complete sample of 1,280 practicing Christians, whereas the next

188   three models are based on subsamples excluding (M2a) those who reported being experienced with PD

189   experiments, (M3a) those who did not comprehend the social dilemma or (M4a) those who did not self-

190   describe as practicing Christians during data collection. Whenever possible, the models include

191   experience with PD experiments and two questions measuring social dilemma comprehension as

192   preregistered control variables (see Control Measures in Methods). In the overall sample (i.e., M1a),

193   cooperation was negatively correlated with understanding of the self-gain maximization strategy (r = -

194   .072 [-.126, -.017], P = .010) and positively correlated with understanding of the group-gain

195   maximization strategy (r = .212 [.159, .264], P < .001) but it was not significantly correlated with PD

196   experience (r = -.027 [-.082, .028], P = .332). While M1a and M4a control for all three variables, due to

197   exclusions, M2a controls only for the understanding measures, and M3a controls only for experience.

198   Next, we describe these models in more detail.

199   Experience with economic games has been shown to weaken intuitive cooperation[32,47]. In

200   response to a replication attempt that failed to find evidence for SHH among Amazon Mechanical Turk

201   participants,[43] evidence for intuitive cooperation emerged when the sample was restricted to those

202   17.2% who had no experience with economic games.[47] We recruited practicing Christians on Prolific,

203   most of whom reported inexperience with the PD experiments (74.1%). M2a restricts the analysis to

204   these 948 inexperienced participants.

205   We measured social dilemma comprehension with two standard questions about (1) the

206   monetary self-gain maximization strategy (63.5% correct) and (2) the monetary group-gain maximization

207   strategy (78.7% correct). In line with previous findings showing that time-pressure does not harm

208   understanding,[35,54] the rate of social dilemma comprehension—those correctly answering both

209   questions—did not differ between the time-limit conditions (56.3% in TD and 55.1% in TP), χ^2 (1, n =

210   1,280) = 0.179, P = .672. On the other hand, restricting analysis to those with comprehension of the

211   game rules has previously supported SCA[54]. Therefore, M3a is restricted to the analysis of 713

212   participants with PD comprehension.

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

218   Table 1 describes the cooperation rates of believers and treatment effects across the four
219   models. Contrary to SHH and in support of SCA, and as visualised in Fig. 3., cooperation was higher
220   under TD than under TP for each group identity condition across all four models. On average,
221   cooperation was higher under TD than under TP by 6.4% in M1a, 5.0% in M2a, 12.6% in M3a, and 7.1%
222   in M4a. The main effect of time-limits on cooperation was statistically significant for three models
223   including (M1a) the complete sample of believers, $F(1, 1271) = 4.83$, $P = .028$; (M3a) those with social
224   dilemma comprehension, $F(1, 706) = 6.12$, $P = .014$; and (M4a) those who satisfied the screening criteria
225   at the time of the study, $F(1, 1041) = 4.17$, $P = .041$. Even among believers who were inexperienced with
226   the PD game (M2a), where statistical estimates did not provide clear evidence for SHH or SCA, $F(1, 940)$
227     $= 2.92$, $P = .088$, there was no evidence of a decrease in cooperation with deliberation (see Fig. 3). The
228   main effect of group identity manipulation was weakened with the inclusion of the No-Group condition
229   into the analysis, and was significant only in M3a, $F(2, 706) = 3.14$, $P = .044$. Likewise, evidence for SCA
230   did not seem to depend on religious group identity, as the interaction effect was not significant in any of
231   the models, Ps $\geq .330$ (but this may also stem from a lack of statistical power in detecting small
232   interaction effects).

233   To further evaluate the robustness of these exploratory findings and increase the power of the
234   associated statistical tests, we estimated modified versions of the four models described above that
235   included all participants in our experiment—not only the believers but also the atheists. The modified
236   models (M1b to M4b) have the same configuration as initial models (M1a to M4a) but additionally
237   include participant type as an independent factor, involving 2 (believer or atheist) by 2 (TP or TD) by 3
238   (In-Group, Out-Group, or No-Group) three-way ANOVAs: As detailed in Table 2, the evidence for SCA
239   was robust to the inclusion of atheists in the analysis, resulting in significant main effect of time-limits
240   on cooperation in all four models. Specifically, cooperation was higher under TD than under TP (M1b) by
241   4.2% in the complete sample, $F(1, 2545) = 4.96$, $P = .026$; (M2b) by 5.5% among those inexperienced
242   with the PD game, $F(1, 1823) = 5.95$, $P = .015$; (M3b) by 6.7% among those with social dilemma
243   comprehension, $F(1, 1574) = 4.75$, $P = .003$; and (M4b) by 4.3% among those who currently identify as

244   either practicing Christian or atheist, $F(1, 2225) = 4.03$, $P = .045$. All four models showed a significant

245   main effect of group identity manipulation (Ps ≤ .009), but none of the models indicated a significant

246   main effect of participant type (Ps ≥ .396) nor interactions between any of the factors (Ps ≥ .142).

247   Finally, using two measures elicited after the PD—decision conflict and expected cooperation—

248   we explore the cognitive drivers of religious parochialism in cooperation. Since these were elicited

249   without time-limits, we focus here on the effect of group identity manipulations. Decision conflict

250   measures, on a scale ranging from 0 to 100, the difficulty of choosing how much to keep and how much

251   to share with one's partner in the PD[55], providing in our context a subjective correlate of religious

252   parochialism. In both conditions, decision conflict experienced by religious believers showed small-to-

253   moderate negative correlation with cooperation behaviour (In-Group: $r = -.201$ [-.291, -.107], $P < .001$;

254   Out-Group: $r = -.152$ [-.242, -.060], $P = .001$). Believers found it easier to cooperate with coreligionists

255   than atheists, as they reported experiencing stronger feelings of decision conflict in the Out-Group

256   condition ($M = 37.85$, $SD = 32.43$) than in the In-Group condition ($M = 33.04$, $SD = 30.57$), $t(862) = 2.24$, P

257    $= .025$, $d = 0.15$ [0.02, 0.29]. These two findings together suggest that cognitive processes of decision

258   conflict are involved in religious parochialism in cooperation.

259   Expected cooperation, on the other hand, measures participants' beliefs regarding the

260   cooperation decisions of their pairs in the PD game[23,56]. This measure allows exploration of whether

261   strong reciprocity—the motivation to cooperate at personal cost conditional on the belief that others

262   will do so as well[57]—drives religious parochialism in cooperation. Actual and expected cooperation were

263   highly correlated for religious believers interacting with both coreligionists ($r = .745$ [.699, .785], $P <$

264    $.001$) and atheists ($r = .684$ [.632, .731], $P < .001$). Furthermore, these participants expected their in-

265   group coreligionist PD pairs to be more cooperative towards them ($M = 30.00$, $SD = 16.51$) than their

266   out-group atheist pairs ($M = 26.56$, $SD = 17.40$), $t(862) = 2.97$, $P = .003$, $d = 0.20$ [0.07, 0.34]. These

267   results suggest that strong reciprocity is a primary driver of religious parochialism in cooperation

268   identified in the confirmatory analysis.

Discussion

270   We studied Christian believers who regularly participated in public religious rituals, since regular social

271   interactions among coreligionists can be expected to result in cooperative heuristics towards in-group

272   members. Contributing to the debates about the role of religion in the public sphere reviewed

273   earlier[13,14], we found evidence for parochialism based on religious identity, with Christians cooperating

274   more with coreligionists than with atheists. However, we failed to find support for generalized intuitive

275   cooperation (H$_1$). This hypothesis, derived from SHH[31–33] and implied by recent findings[34], predicts that

276   Christian believers assigned to the intuition condition (TP) would be more cooperative than those

277   assigned to the deliberation condition (TD) independent of the religious identity of their pairs. Neither

278   was there any support for SHH in conditions where no group identity was revealed, which were run to

279   provide comparability with the original studies. At least at first sight, our results are consistent with the

280   interpretation emerging from the accumulated evidence that intuitive cooperation is either non-existent

281   overall[58] or small in effect size when time-pressure manipulations are used[59].

282   Our exploratory analyses, on the other hand, provided evidence for intuitive selfishness as

283   predicted by SCA. Across three of the four models tested among believers, including a model with the

284   complete sample of participants and a model restricted to Christian believers actively practicing at the

285   time of the study, cooperation was found to increase with deliberation independent of group identity.

286   These models used all experimental conditions to increase statistical power (including those without

287   group identity information), and where applicable, they controlled for the preregistered covariates of

288   experience with and comprehension of the PD game. The model that provided strongest evidence for

289   SCA restricted the analysis to those who comprehended the social dilemma underlying the PD. Even in

290   the model that failed to provide conclusive evidence (M2a), where those with experience in the PD

291   game were excluded from analysis, average cooperation was higher when participants were encouraged

292   to deliberate. Furthermore, the main effect of time-limits was significant in the direction of SCA when

293   four additional models were estimated using all participants—both believers and atheists. These

294   exploratory findings highlight the need for future confirmatory tests of SCA. One should also be cautious

295   interpreting estimates based on restricted subsamples since these exclusions are open to annulment of

296   random assignment and to sample selection bias[60]. Nevertheless, while we found no confirmatory

297   evidence for SHH in any of our models, our study provides support for SCA when considering the

298   complete sample of participants.

299   How can we reconcile the evidence supporting SCA in our exploratory analyses and elsewhere in

300   the literature[48,54,55,61] with previous support for SHH[31,34–36]? Pointing towards a resolution, we note that

301   the two phenomena—intuitive cooperation predicted by SHH and intuitive selfishness predicted by

302   SCA—have different premises regarding the underlying social and cognitive processes. While SHH relies

303   on mental shortcuts developed during past social interactions, SCA points towards a primordial—visceral

304   and instinctive—response for self-protection[62]. In principle, the two effects can therefore coexist in

305   varying magnitudes across decision-making contexts such that, overall, one may dominate the other. As

306   they may also cancel each other out, these two independent mechanisms can also explain the overall

307   weak or null effect of tests of intuitive cooperation behaviour in social dilemmas[42,58,59]. Therefore,

308   procedures for disentangling the two phenomena are needed for conducting independent tests of SCA

309   and SHH. For example, to allow relatively isolated tests of SHH, social heuristics can be developed in the

310   laboratory by repeated exposure to cooperative social dilemma environments[44,46]. Similarly, cultural

311   comparisons can help identify social contexts where cooperative heuristics are prevalent[45,63], and

312   framing manipulations can help identify the contextual cues that trigger them[64].

313   Novel procedures that independently test SCA are also needed. A potential candidate relates to

314   the ongoing debate about whether miscomprehension of the social dilemma confounds tests of intuitive

315   cooperation[54,65–67]. Other things being equal, systematic misperception of the experimental task is

316   methodologically undesirable, since participants with misperceptions may be playing a different game

317   than intended by the researchers. However, SHH predicts intuitive cooperation in part because of such a

318   misperception. Accordingly, people develop prosocial heuristics since regular cooperation among

319   affiliates tends to be self-serving, but deliberation will reveal cooperation to be a mistake in the

320   particular case of anonymous one-shot games. In this sense, the misperception that the one-shot PD

321   game does not involve a social dilemma is arguably a necessary condition for observing support for SHH.

322   Hence, providing extensive instructions about the dilemma and screening participants based on

323   comprehension (e.g., using control questions[68]) can provide independent tests of SCA by minimizing

324   intuitive cooperation due to social heuristics. Consistent with this argument as well as with previous

325   findings in the literature[54], our model that excluded participants with social dilemma miscomprehension

326   provided no evidence for SHH and showed even stronger exploratory evidence for SCA.

327   We initially asked whether cooperation depends on religious group identity and whether

328   religious parochialism in cooperation has an intuitive basis. Although religious believers in our sample

329   did not exhibit intuitive cooperation, they were parochial, giving more to coreligionists than to atheists

330   in the PD game. Exploratory tests provided suggestive evidence that strong reciprocity, and to some

331   extent decision conflict, drive religious parochialism in cooperation. In other words, believers tend to

332   cooperate more with coreligionists than atheists because they expect coreligionists to be more

333   cooperative, and because they feel less conflicted when making such a decision. While this goes against

334   recent findings of generalized religious prosociality[10], it is consistent with strong meta-analytic evidence

335   for in-group favouritism in cooperation across various domains[69].

336   Our experimental protocol, used to manipulate group identity, is likely to have influenced our
337   finding on religious parochialism. We used a quasi-naturalistic setting, where an online profile was used
338   to reveal multiple group identity attributes simultaneously, thereby mimicking the social media profiles
339   people regularly use to learn about others (see Fig. 1). In our case, the religious group identity of ones'
340   partner in the PD game was varied to induce in-group and out-group manipulations, while country of
341   residence, age group, language, and recruitment platform membership were kept constant across the
342   group identity conditions. The use of a profile has the advantage of increased ecological validity and it
343   can limit socially desirable responding by obscuring the manipulation. However, this comes at the cost
344   of weakening the experimental manipulation (i.e., religious affiliation). Although we did find evidence
345   for in-group favouritism, the effect size was smaller than that found in the literature, indicating that it
346   may have been dampened by the presence of other in-group attributes. In particular, the country of
347   residence as an in-group attribute may have evoked strong binding reactions by cuing nationality. Future
348   research on parochialism should vary multiple attributes to estimate the importance of religious identity
349   relative to others.

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

Methods
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Overview
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361   Our research complies with all relevant ethical regulations. Ethics approval was obtained from the
362   University of Leeds Research Ethics Committee, and informed consent was received from participants at
363   the outset of the study. An incentivized prisoner's dilemma (PD) game was used to study cooperation
364   behaviour. Participants were recruited from previously self-declared practicing Christians and atheists,
365   who were randomly assigned to one of six conditions while playing the PD game. Data on atheists are

366   not analysed here in detail since this study focuses on the decisions of Christian participants. The

367   experiment involved a 3 (religious group identity of one's pair in the game: practicing Christian, atheist

368   or no identity) by 2 (time-limit: 10s time-pressure or 20s time-delay) between-subjects design. Each

369   participant was randomly assigned to one of six experimental conditions. Participants and the

370   researchers were blind to the conditions of the experiment during data collection. All manipulation

371   checks and applicable control measures showed that the manipulations worked as intended.

372   **Power Analysis**

373   We estimated our sample size based on the hypothesized main effect, and let this sample size

374   determine the smallest effect size that can be detected for the hypothesized lack of an interaction

375   effect. To do so, we used the most relevant effect size for the main effect of time-limit manipulations

376   found in the literature[35]—a test of SHH on a sample recruited from Prolific using a similar protocol (f =

377   0.11). Because the one-shot PD game does not involve interaction or feedback, each individual decision

378   in the game constitutes an independent observation. To detect a main effect of time-limit of this size in

379   a two-way ANOVA model with α = 0.05 and $1 - \beta$ = 0.95, we estimated using G*Power 3.1.9.2 that our

380   sample should consist of at least 1280 believers[70]. Sensitivity analysis indicated that the minimum

381   interaction effect size that can be detected in a two-way ANOVA model with n = 1280, α = 0.05 and $1 - \beta$

382   = 0.95 is η^2 = 0.012, which we took to be our smallest effect size of interest (SESOI)[71,72]. Although we

383   focus on the behaviour of believers, we avoided deception by also recruiting 1280 atheists, who were

384   paired either with each other or with believers in the PD game.

385   **Hypothesis Tests**

386   In a two-way ANOVA model of the PD decisions of religious believers on religious identity and time-limit

387   factors, H_1 would be supported by evidence (1) for intuitive cooperation in a null-hypothesis significance

388   test (i.e., significant main effect of time-limits on cooperation such that cooperation is higher under

389   time-pressure than under time-delay) and (2) for the generality of intuitive cooperation in a one-tailed

390   equivalence test showing lack of a significant interaction effect. While step (1) is operationalized as

391   indicating evidence if p < 0.05, evidence in step (2) would be indicated by the upper bound of the 90%

392   confidence interval of the interaction effect size (η^2) being less than 0.012 (i.e., excluding the SESOI). In

393   step (2), we also calculate a Bayes Factor (BF) for the interaction effect as confirmation such that BF $\leq$

394   1/3 is interpreted as substantial evidence for the null result.[73]

395   **Participants**

396   We recruited participants from Prolific (https://prolific.co/) and conducted our experiment online. Data

397   generated online, including Prolific, has been shown to replicate various well-established laboratory

398   results[74,75], including incentivized games measuring cooperation[76]. We used Prolific because it allows

399   prescreening based on a previously completed comprehensive demographic questionnaire, including

400   religious affiliation and practices. Participants were adult US residents fluent in English. We report data

401   on 1,280 practicing Christians, recruited among those who in the Prolific questionnaire answered

402   "Christianity" for the question "What is your religious affiliation?" and chose either "Yes. Both public

403   and private." or "Yes. Public only." for the question "Do you participate in regular religious activities?"

404   The sample of believers had a balanced gender distribution (54% female) and an age distribution ranging

405   from 18 to 77 (*M* = 35.60, *SD* = 12.98). The majority of these participants (74.1%) reported that they

406   have not previously participated in an experiment involving PD games. An equal number of atheists,

407   recruited to avoid deception, were selected among those who answered "Non-religious" to the religious

408   affiliation question and who then qualified their answer as "Atheist" in the follow-up question "Which of

409   the following do you most identify as?". Participants with complete submissions earned a participation

410   fee ($0.25), in addition to their earnings from the PD game.

411   **Materials and Procedure**

412   *Materials.* A copy of the experimental materials is available at the OSF study preregistration page

413   (https://osf.io/kzwgn/).

414   *Procedure.* We conducted the experiment using the Qualtrics software (https://www.qualtrics.com/).

415   After eliciting informed consent, participants received training on the slider tool to increase their

416   familiarity with the interface for eliciting PD decisions[35]. They next read a general description of the

417   study, explaining that there were three parts and that after the study was over one part was to be

418   selected at random for determining participant's additional payments from the study. Participants were

419   not informed about the tasks involved in upcoming parts beforehand. The first part included the main

420   task, the one-shot PD game, whereas the other two parts included exploratory measures of social

421   dilemma comprehension and social expectations (see below). The procedure for randomly selecting one

422   of the three parts for determining additional payments is an effective cost-saving method, well-

423   established in experimental economics[77], with theoretical support for its incentive-compatibility[78] and

424   significant evidence that participants consider each part independently[79,80].

425   The main task was a one-shot PD game and included the experimental manipulations.
426   Compliance with time-limits was incentivized to strengthen cognitive manipulations[35]. After reading the
427   instructions for the PD game at their own pace, a transitory screen explained the time-limits and the
428   monetary incentives for compliance. This screen was displayed for at most 30s or less if participants
429   choose to proceed earlier, allowing time for reading while preventing deliberation about the upcoming
430   task. Next came the PD decision screen, which first revealed—for participants in the identity
431   manipulation conditions—an "online profile" of each participant's pair in the game and, after two
432   seconds, displayed the slider tool and a timer. The PD decision was elicited under one of two time-limit
433   conditions (i.e., 10s time-pressure or 20s time-delay). Afterward, manipulation checks and exploratory
434   measures were elicited, followed by a brief questionnaire including basic demographic information.

435   *Prisoner's Dilemma (PD)*. We used a one-shot continuous prisoner's dilemma (PD) game, relying on
436   instructions used in the previous literature[39]. In the PD, a pair of participants individually decided,
437   without observing each other's actions, how much of $0.50 to keep and how much of it to allocate (in 1
438   cent increments) to their pair. Amount allocated to the pair (whole number ranging from 0 to 50 cents)
439   is our measure of cooperation. If PD was selected for payment, participants earned double the amount
440   allocated to them by their pair in addition to any money they kept for themselves. From each
441   participant's perspective, the game involved a strict trade-off between personal earnings and total
442   earnings by the two participants, rendering it a social dilemma. In a previous social dilemma experiment
443   on Prolific (N = 3,653), using a four-person public good game with marginal per capita return of 0.5, we
444   found that 63.6% of endowments was given to the public good (SD = 29.6), that 6.4% of participants
445   gave nothing and that 25.1% gave everything (Isler, Gächter, Maule & Starmer, unpublished
446   manuscript). With substantially lower time and effort required for its completion (the median
447   completion time was 5 minutes), our study provides a ratio between endowment size and opportunity
448   cost that is comparable to laboratory studies. Furthermore, a large-scale meta-analysis found no overall
449   effect of stakes on giving in dictator games[81] and similar findings are reported elsewhere[82–86]. Finally, a
450   recent study found evidence of religious prosociality in low-stake ($1) games using explicit primes[87].

451   *Group assignment.* Practicing Christians played the PD game in equal probability either with another
452   practicing Christian (In-Group), with an atheist (Out-Group) or with someone without identity
453   information (No-Group). Participants did not know that they had been recruited based on their religious
454   identity because the prescreening questions were elicited beforehand by Prolific. Participants in the
455   identity manipulation conditions (but not those in the control condition) were informed on the PD

456 instruction screen that the decision screen would show an "online profile" describing their pair in the

457 game. Specifically, modifying a previously established method[10], the decision screen revealed (in

458 balanced Latin Square order) other participant's religious identity ("Christian" or "Atheist") together

459 with four constant, in-group identity information categories (country of residence, age group, language,

460 and experimental platform). This approach was intended to minimize demand characteristics (since

461 deciding based on multiple identity categories makes religious belief less focal) and to increase the

462 realism of the experimental setting (since acquiring information from social media profiles with these

463 kinds of group identity categories is a familiar experience). Identity information was paired with symbols

464 to speed comprehension (e.g., the Christian cross, the atheism symbol, a map of the US, etc.).

465 *Time-limit manipulations.* The PD decision was elicited either under 10s time-pressure (TP) with prompts

466 to "be quick" or under 20s time-delay (TD) with prompts to "carefully consider" the decision. Based on

467 previously developed methods, we incentivized compliance with time-limits[35], and we informed

468 participants that additional earnings from the PD were highly likely to be invalidated by noncompliance.

469 The uncertainty prevents the annulment of incentivization that could otherwise occur in cases of non-

470 compliance. We in fact randomly chose 90% of noncompliant decisions to be invalid. We did not inform

471 participants of the probability of invalidation for noncompliance ($p = 0.9$) so as not to induce a

472 calculative mindset.

473 **Control Measures**

474 We planned various controls to check whether: (1) our manipulations affected decision processes as

475 intended, (2) the information used for sample selection is accurate, (3) our sample is representative in

476 that it replicates well-established behavioural biases, and our results are (4) robust when controlling for

477 experience and comprehension in the PD game and (5) specific to religious believers or generalizable to

478 other natural groups. Since we did not find evidence for intuitive cooperation, we followed our

479 preregistered procedure and did not conduct the last control measure (5) (see Results generalisability

480 check).

481 *Manipulation checks.* We committed to three tests to check that our manipulations worked as intended.

482 First, as a behavioural test of time-limit manipulations, we checked whether the median response time

483 under time-pressure was faster than the median response time under time-delay using a Wilcoxon rank-

484 sum test. In addition, immediately following the PD game, three questions were elicited in two randomly

485 presented screens to check that time-limit and religious group identity conditions manipulated cognitive

486   processes as intended. On the time manipulation check screen, participants rated in random order their

487   agreement with two statements on a 5-point scale: 1) "I did not have time to think through my

488   decisions" (indicating limited opportunities for deliberation), 2) "I decided based on my 'gut reaction'"

489   (indicating increased spontaneity of decisions). As an indication of successful manipulation of cognitive

490   processes by time-limits, an independent samples *t*-test of significant differences in average scores for

491   the two questions between the two time-limit conditions was estimated. On the group identity

492   manipulation check screen, participants completed the online version of the Inclusion of the Other in

493   the Self (IOS) Scale, a simple and reliable measure of subjective closeness of social relationships[88]. The 7-

494   point IOS question asked active participants to select one of seven pairs of circles with increasing areas

495   of intersection that best described the relationship between the active participant ("You") and the

496   passive participant ("Other"). Successful group manipulation would be indicated by a significant

497   difference in an independent samples *t*-test between the In-Group and Out-Group conditions.

498   *Screening information check.* Information on religious affiliations and practices was previously elicited by

499   Prolific. We used two of these questions as screening criteria during data collection (see the Participants

500   section above). The survey section of our study also elicited answers to these same questions, to check

501   the accuracy of the information used in the selection of practicing Christians. Prior to data collection, we

502   committed to reporting the hypothesis test results based on the identity information elicited in our

503   survey if the match rate on the religious affiliation question was less than 90%. In fact, this match rate

504   was 95.9%. However, because the match rate was 82.0% when considering questions about both

505   religious affiliation and participation in public rituals, we report the hypothesis test results for this

506   restricted sample as part of the exploratory analysis.

507   *Sample behaviour check.* The design allows a test of whether our sample of believers is representative in

508   showing commonly observed biases. A significant main effect of religious group identity in the two-way

509   ANOVA, such that believers cooperate more with other believers than with atheists, would replicate the

510   commonly observed group bias.

511   *Experience and comprehension check.* The PD game was described in a survey question to elicit

512   participants' experience with the game from past participation in experiments. In addition, we

513   measured comprehension of the social dilemma by eliciting via sliders what participants thought were

514   the self-gain maximizing strategy (i.e., keeping all endowment for self) and the group-gain maximizing

515   strategy (i.e., giving all endowment to the recipient) in the PD game. Participants had the opportunity to

516   earn $0.25 for each correct answer. Those who incorrectly answered either question can be considered

517 as having miscomprehended the social dilemma. As standard[36], we did not exclude those with
518 miscomprehension or experience from the confirmatory analysis. In exploratory models, we either
519 controlled for them as covariates (M1a-b and M4a-b) or excluded them from analysis (M2a-b and M3a-
520 b).

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

527 **Additional Measures**

528 *Expected cooperation.* Participants predicted the allocation made by their pair. To incentivize truthful
529 reporting of expectations, participants had the opportunity to earn $0.50 for predictions that were
530 accurate within 5 cents. Expectations provide a measure of trust towards one's pair[89]. We explore if
531 differences in expected cooperation are consistent with differences in actual cooperation behaviour
532 (e.g., group bias).

533 *Decision Conflict*. We elicited self-reported subjective conflict experienced during the PD decision. The
534 measure, based on previous literature[55], uses a scale ranging from 0 (not at all) to 100 (very much) as
535 response to the question "Some participants find it difficult to make a decision regarding how much
536 money to keep personally and how much to share with others because they find the two goals equally
537 important. To what extent did you experience such a conflict when making your decision?" We explore
538 whether experimental manipulations affected the experience of decision conflict.

539 **Data exclusions**

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

545 # Protocol registration

546 The Stage 1 protocol for this Registered Report was accepted in principle on 28 January 2020. The

547 protocol, as accepted by the journal, can be found at https://doi.org/10.6084/m9.figshare.12086781.v1.

Data availability

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

Code availability

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

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733

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Author contributions

738   O.I. and O.Y. conceived the initial idea and design, which was improved with contributions by A.J.M.. O.I.

739   wrote the manuscript which was revised by all three authors. O.I. collected and analysed the data. All

740   authors had access to the data and approved the final version.

Competing interests

742   The authors declare no competing interests.

Figure legends

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

752

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

759

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

767 # Tables

768 **Table 1. Cooperation among believers across four exploratory models**

		M1a: Complete			M2a: Inexperienced			M3a: Comprehended			M4a: Current		
		M	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*	*n*
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
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	*P*	η_p^2	95% CI	*P*	η_p^2	95% CI	*P*	η_p^2	95% CI	*P*
ANOVA	Time-Limit	.004	(0, .013]	.028	.003	[0, .014]	.088	.009	(0, .027]	.014	.004	(0, .015]	.041
	Group Identity	.003	[0, .011]	.151	.004	[0, .014]	.158	.009	(0, .026]	.044	.004	[0, .014]	.111
	Interaction	.002	[0, .008]	.330	.001	[0, .009]	.501	.000	[0, .005]	.866	.001	[0, .007]	.609

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

777

778 **Table 2. Cooperation among all participants across four exploratory models**

		M1b: Complete			M2b: Inexperienced			M3b: Comprehended			M4b: Current		
		M	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*	*n*
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
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	*P*	η_p^2	95% CI	*P*	η_p^2	95% CI	*P*	η_p^2	95% CI	*P*
ANOVA	Participant Type	.000	[0, .001]	.999	.000	[0, .003]	.686	.000	[0, .005]	.396	.000	[0, .004]	.398
	Time-Limit	.002	(0, .007]	.026	.003	(0, .010]	.015	.003	(0, .011]	.029	.002	(0, .007]	.045
	Group Identity	.006	(0, .013]	.001	.005	(0, .013]	.009	.007	(0, .016]	.005	.007	(0, .015]	.001
	Interaction	.001	[0, .004]	.380	.000	[0, .003]	.675	.000	[0, .001]	.923	.000	[0, .003]	.693

779 **Note.** Cooperation by practicing Christians and atheists in the PD game analysed across four exploratory
780 models: (M1b) the complete experimental sample, (M2b) among those inexperienced with the PG
781 game, (M3b) among those who comprehended the social dilemma, and (M4b) among those who
782 currently identify as practicing Christian or atheist. The top two blocks describe cooperation mean (*M*),

783 standard deviation of cooperation (*SD*) and number of observations in condition (*n*) by time-limits
784 (Pressure or Delay) and group identity manipulations (In-Group, Out-Group or No-Group). The bottom
785 block describes effect size (η_p^2), 95% confidence interval (CI) and significance level (*P*) for the main
786 effects of participant type (believer or atheist), time-limits and group identity manipulations and the
787 three-way interaction in the corresponding three-way ANOVA models. None of the two-way interactions
788 were significant (*P*s ≥ .142).

a.

Other participant	Practicing Christian	US resident	18 or older	Speaks English	Prolific member

b.

Other participant	Atheist	US resident	18 or older	Speaks English	Prolific member

a Complete (M1) **b** Inexperienced (M2)

c Comprehended (M3) **d** Current (M4)