Article

Social Psychological and Personality Science
1-8

DOI: 10.1177/1948550616632577
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How Good Is the Samaritan, and Why? An Experimental Investigation of the Extent and Nature of Religious Prosociality Using Economic Games

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Abstract

What is the extent and nature of religious prosociality? If religious prosociality exists, is it parochial and extended selectively to coreligionists or is it generalized regardless of the recipient? Further, is it driven by preferences to help others or by expectations of reciprocity? We examined how much of a US$0.30 bonus Mechanical Turk workers would share with the other player whose religion was prominently displayed during two online resource allocation games. In one game (but not the other), the recipient could choose to reciprocate. Results from both games showed that the more central religion was in participants' lives, the more of the bonus they shared, regardless of whether they were giving to atheists or Christians. Furthermore, this effect was most clearly related to self-reported frequency of "thinking about religious ideas" rather than belief in God or religious practice/experience. Our findings provide evidence of generalized religious prosociality and illuminate its basis.

Keywords

religion, altruism, behavioral economics, decision making, judgment and decision making

Compared to the nonreligious, religious individuals are perceived as being more moral (Hout & Fisher, 2002), likable (Bailey & Young, 1986), and trustworthy (Bailey & Doriot, 1985). In contrast, atheists experience distrust and prejudice—and particularly so from religious people (Gervais, Shariff, & Norenzayan, 2011). But to what extent are such stereotypes of atheists as less cooperative grounded in reality? Are religious people really more prosocial than atheists?

To investigate this, researchers have increasingly utilized behavioral economic games as an effective tool to explore prosocial behavior, wherein participants make real monetary choices that affect earnings for themselves and others. Behavior in these games has been shown to have a reliable relationship with prosocial behavior in everyday life (Peysakhovich, Nowak, & Rand, 2014). For example, giving in a Dictator Game (DG) predicts both giving to charity in a field setting (Benz & Meier, 2008) and the returning of money mistakenly mailed to them in a misdirected letter (Franzen & Pointner, 2013). But using economic games, the relationship between religiosity and prosocial behavior remains unclear (for critical reviews see Galen, 2012; Preston, Ritter, & Hernandez, 2010). Some studies find a positive relationship, whereby religiosity is associated with prosocial behavior as measured by giving in DGs (Eckel & Grossman, 2003; Tan, 2006), Public Goods cooperation (Ahmed, 2009), and trustworthiness in a Trust Game (TG; Tan & Vogel, 2008). Other work, however, has failed to find a relationship between religiosity and prosocial behavior in the DG (Anderson, Mellor, & Milyo, 2010; Eckel & Grossman, 2004; Paciotti et al., 2011), the TG (Anderson et al., 2010; Bellemare & Kröger, 2007), or Public Goods Dilemmas (Ahmed & Salas, 2009; Orbell, Goldman, Mulford, & Dawes, 1992). Still other studies have suggested that such effects are moderated by the (lack of) religious belief of the other player, whereby greater prosocial behavior by religious people in a DG is seen only when the recipient shares the religious identity of the participant (Ben-Ner, McCall, Stephane, & Wang, 2009).

Consideration of the existing literature on religious prosociality, then, reveals mixed effects and three competing hypotheses can be discerned. First, a religious *prosociality equivalence* hypothesis predicts that religious and nonreligious individuals are equally prosocial. Second, a *generalized*

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religious prosociality hypothesis predicts that religious individuals are more prosocial regardless of the recipient. Third, a *religious parochialism* hypothesis predicts that religious people are only more prosocial toward coreligionists. Beyond these hypotheses derived from the body of research on religious prosociality, a fourth hypothesis is that atheists are more prosocial overall and a fifth hypothesis that atheists exhibit parochial prosociality.

Here, we distinguish between these competing hypotheses by examining the relationship between religiosity and prosociality when giving to Christian versus atheist partners in economic games. Previous work has often compared prosociality among religious individuals versus individuals who have not specified a religion (e.g., Kibbutz members vs. anonymous city members, Ruffle & Sosis, 2006). Yet to explore the extent of religious prosociality, atheists are an ideal recipient comparison group because they constitute a clearly distinct group from religious individuals (one cannot easily be a Christian *and* an atheist), and moreover one that is particularly disliked across many demographics (Edgell, Gerteis, & Hartmann, 2006). If religious prosociality is primarily parochial—rather than generalized—we would expect prosocial behavior to be extended selectively toward other religious individuals and not atheists. On the other hand, if religious prosociality is a generalized phenomenon, we would expect religious individuals to be more prosocial even to members of such a generally disliked group.

As a secondary question, we investigated the psychological mechanisms underlying religious prosociality—assuming it was observed at all—using the conceptual apparatus of social preferences and expectations (for a review see Everett, Faber, & Crockett, 2015). To what extent is religious prosociality driven by expectations that others are more likely to reciprocate prosocial behavior, versus a more enhanced valuing of others' outcomes? If the religious prosociality is driven primarily by social preferences whereby religious people simply prefer to help others, one would expect to observe religious prosociality even in circumstances in which there is no potential for the partner to reciprocate. Conversely, if religious prosociality is explained primarily by the belief that being prosocial is personally advantageous (cooperating conditionally based on expected reciprocation), this could lead to increased *self-interested* prosociality. To address this question, we chose to examine prosociality in two different settings: first, in an economic game that primarily measures social preferences regarding the outcomes of others (DG, Forsythe, Horowitz, Savin, & Sefton, 1994) and second, in a game that primarily involves expectations of reciprocity (TG, Berg, Dickhaut, & McCabe, 1995). In the DG, one player—the dictator—is given some money and makes a unilateral decision about how much of this money to share with a second (anonymous) player—the recipient—who must accept whatever amount the dictator chooses to give. In our DG, any money the dictator gave to the recipient was doubled by the experimenter. Behavior in the DG can be interpreted as resulting from the dictator's social preferences, not expectations about how the recipient will respond, because the recipient does not take any action—thus there is no potential for reciprocity. In contrast, behavior in the TG is affected by both preferences and expectations. The TG also has two players, an investor and a recipient, and like the DG, the investor is given some money and told that they can send a proportion to the recipient and that the experimenter will double any money sent. Unlike the DG, however, once the recipient receives the money, they can choose to send some portion of what they have received back to the investor. Therefore, while a self-interested dictator would send nothing in the DG, a self-interested investor in the TG might send money depending on their expectations about the recipient's behavior: If the investor trusts that the recipient will return 50% or more of what she receives, the investor pays no cost (or even makes money) while helping the recipient.

Leaving aside disagreements in the literature as to *whether*—and to *whom*—religious people are more prosocial, different accounts have also been posited to explain *why* increased religious prosociality occurs. One possibility is that religious individuals may be more prosocial due to the kinds of beliefs they have about the nature of reality, whereby a sensitivity to punishments and rewards from supernatural agents may enforce norms even in the absence of human social monitoring (Atkinson & Bourrat, 2011; McKay, Efferson, Whitehouse, & Fehr, 2011). Alternatively, religious prosociality may be a by-product of sociological factors, including stronger social networks among religious groups (Lewis, MacGregor, & Putnam, 2013; Putnam & Campbell, 2010). Yet more possibilities are that specific kinds of religious practices inculcate prosocial tendencies, such as religious rituals (Xygalatas, 2013) or religious experiences (James, 1902/1985). To help shed light on this, we measured multiple components of religiosity (including frequency of private prayer, frequency of public worship, and certainty of belief in God) to see, if religious prosociality is observed, what elements of religiosity best predict this prosociality.

Study 1

Method

Participants

Five hundred and eighty two American participants (268 female) were recruited online using Amazon Mechanical Turk (MTurk) and paid US$0.50 for their time, with an option of keeping an additional bonus of US$0.30. Only participants who passed simple comprehension checks concerning the payment structure and rules of the games were included in data analysis. Because we had planned on running a TG in a subsequent study, and the TG instructions are more complex than those of the DG, we were concerned that there would be systematic variation in participants across studies (i.e., participants in the TG would be more sophisticated on average than those in the DG). Therefore, after participants made their DG decision, they were shown the instructions for the TG and completed the TG comprehension checks.

In the main article, results are reported only for participants who passed comprehension checks for *both* games, took the

survey only once, and who identified as either Christian or atheist ($n = 296$); results including all religious participants can be seen in the supplementary information (SI). Of the Christian participants ($n = 165$), nearly all were at least moderately certain that God exists (98%); and, with a frequency of at least "occasionally," 83% reported thinking about religious issues, 80% reported praying, 76% reported experiencing God intervening in their life, and 55% took part in religious services (see SI for further details). Participants were politically moderate on average (on a 1–7 scale; $M = 4.48$, $SD = 1.67$). An a priori power analysis using G*Power (Faul, Erdfelder, Buchner, & Lang, 2009) revealed that for a 2 × 2 design and taking Cohen's f effect size standards of 0.14 as small and 0.39 as medium, 256 participants were required to detect a small- to medium-sized interaction effect ($f = 0.20$) with an α of .05 and power of .80. Therefore, this study was sufficiently powered.

Design

Participants were informed at the start of the study that they would be playing a simple game with another participant. To manipulate the religious identity of the recipient (Person B; the second player), participants were given four pieces of information about the other player. All participants were explicitly reminded that the other player would not know any information about themselves or their (lack of) religious affiliation. The other player was described as living in the United States, speaking English as their primary language, and being over the age of 18. The fourth piece of information constituted the experimental manipulation, where participants were told that the player identified as Christian or as an atheist. This information was presented in both text form and with a symbol presented on both the predecision page and the decision page, and it was made salient to participants that there was no deception in this task and that the decisions they made were real. Participants answered questions on their own religiosity after making their decisions in the economic game to avoid potential religious priming effects.

Participants were given the following instructions:

> In this interaction you are matched with a real other person. One of you will be Person A, one of you will be Person B. Person A starts with a 30 cent bonus, and Person B starts a 0 cent bonus. Person A makes a choice. Person A can choose how much of their 30 cents to transfer to Person B. Whatever person A transfers is doubled and given to person B.

Participants indicated how many cents they wanted to transfer in 5-cent increments. We doubled the money transferred in our DG in order to make the game equivalent to the TG, described subsequently in Study 2, with the important exception that in the DG the recipient is powerless and takes no action.

Measures

Centrality of religion was measured using Huber and Huber's (2012) short item Centrality of Religiosity Scale (CRS). The CRS is a measure of the centrality, importance, and salience of religiousness in a person and consisted of 5 items each tapping one of the theoretically defined core dimensions of religiosity: public practice, private practice, religious experience, ideology, and intellectual. Participants were asked to rate on a 5-item scale "How often do you think about religious issues?" (Intellect: 1 = *never,* 5 = *very often*); "To what extent do you believe that God or something divine exists?" (Ideology: 1 = *not at all*, 5 = *very much so*); "How often do you take part in religious services?" (Public practice: 1 = *never*, 5 = *very often*); "How often do you pray?" (Private practice: 1 = *never*, 5 = *very often*); and "How often do you experience situations in which you have the feeling that God or something divine intervenes in your life?" (Experience: 1 = *never*, 5 = *very often*). Scores were combined into a single reliable measure ($\alpha = .91$) of the centrality of religiousness for each participant.

Negative Attitudes Toward Atheists (NATA) were measured using 7 items adapted from Gervais (2011). Participants rated their agreement with a number of statements on a 1–7 scale (1 = *not at all,* 7 = *very much*), including "I would prefer to spend time with people who are religious believers" ($\alpha = .92$).

Results

We first looked at whether there were main effects of participant identification as Christian or atheist on the amount of money transferred (indicating religious prosociality) and whether there was an interaction between religiousness and whether the recipient was atheist or Christian (indicating that such prosociality was parochial vs. generalized). Christian participants transferred more in the DG, $F(1, 292) = 12.39$, $p < .001$, and while there was a significant interaction, $F(1, 292) = 6.28$, $p = .01$, simple effects showed that only atheist participants, $t(128.41) = 2.19$, $p = .03$,[1] but not Christian participants $t(163) = -1.58$, $p = .12$, transferred significantly different amounts based on the recipient's religiosity (see Figure 1a). Given the sample size of 165, the current analysis had an 80% statistical power to detect a small- to medium-sized effect ($f = 0.20$) at $\alpha = .05$, two tailed. Thus, if there had been a large effect of recipient religion on Christian participants' behavior in the DG, we would likely have been able to reject the null hypothesis.

We complemented these analyses with a regression-based approach using a continuous measure of participant religiosity (CRS from 1 to 5, centered) as well as recipient religiosity (categorical manipulated variable; atheist [−1] or Christian [+1]) and the interaction between the two (see SI for further details). There was a significant main effect of participant religiosity, $B = 1.73$, $t(293) = 4.94$, $p < .001$, whereby religiosity was associated with increased transfer amounts (see Figure 1b) and no significant main effect of recipient's religiosity, $B = 0.02$, $t(293) = 0.05$, $p = .96$. The interaction between participant religiosity and recipient religiosity did not reach statistical significance, $B = 0.67$, $t(292) = 1.89$, $p = .06$, and simple effects showed no significant effect of recipient religiosity for

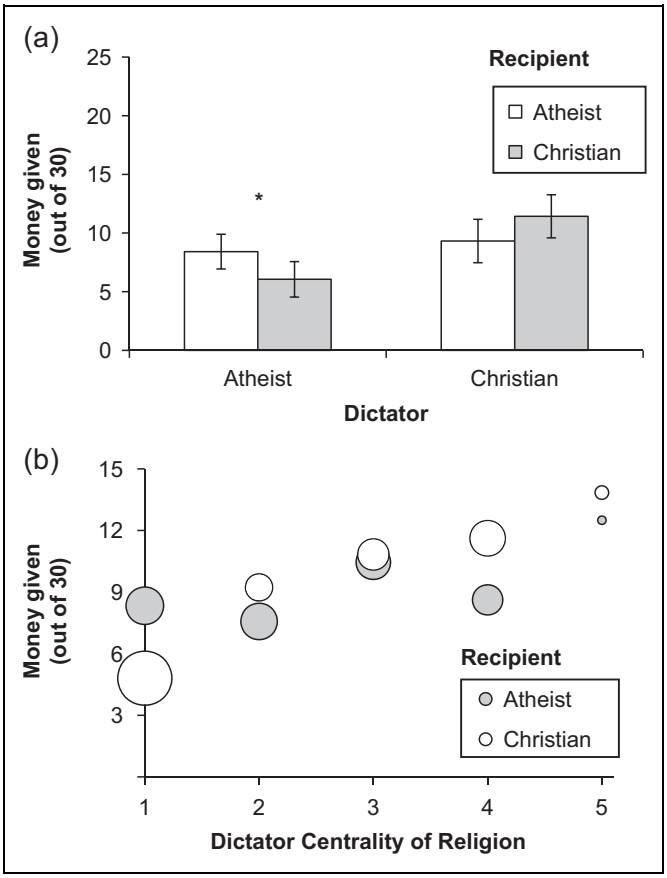

Figure 1. (a) Self-identifying Christian participants transferred more than atheist participants toward both Christian and atheist recipients in a Dictator Game. Error bars represent 95% confidence intervals. (b) Transfers in cents in the Dictator Game in Study 1 as a function of recipient and participant religiosity (binned to the nearest integer with the mean displayed across all observations in that bin). The size of the dots is proportional to the number of observations.

either more or less religious participants. These effects were robust to controlling for gender, ethnicity, education, and political ideology; looking only at differences within Christians and not atheists; and held when using participants from all religious affiliations (see SI for statistics)—in all three cases, the marginally significant interaction between participant religiosity and recipient religiosity became fully nonsignificant. Thus, we found no convincing evidence of any religious-specific parochialism, though the data suggest that there might be some weak form of in-group-favoring parochialism on the part of both atheist and religious participants. Individuals with stronger religious beliefs were significantly more generally prosocial; our evidence does not support the religious parochialism hypothesis.

We next conducted these analyses using each individual component of the CRS to test whether these effects were explained by one particular component of religiosity. While there were significant main effects of all five components of the CRS when analyzed separately, follow-up mediation analyses indicated that how often participants thought about religious issues fully mediated the relationship between DG transfers and all four other components of the CRS (extent of belief in God and frequency of religious practices or experiences; see S. I. for statistics). Furthermore, a multiple regression analysis including each CRS component together showed that the frequency with which participants reported thinking about religious issues was the only significant predictor, $B = 1.78$, $t(289) = 3.69$, $p < .001$. Thus, the extent to which participants believed that God exists, and the frequency of religious experiences, prayer, and attending religious services, did not appear to influence their level of prosociality, except in so much as they acted as proxies for frequency of thinking about religious issues. Furthermore, we note that when replicating our main analysis using frequency of thinking about religious issues rather than the full CRS measure, there is even less evidence of an interaction between participant religiosity and recipient religiosity, $B = 0.47$, $t(293) = 1.25$, $p = .21$.

Finally, to ensure that these results were not driven by our sample being unusually unprejudiced toward atheists, we explored whether self-reported NATA translated into actual discrimination. We found a significant positive main effect of NATA on giving, $B = 0.92$, $t(293) = 3.41$, $p < .001$, but no significant main effect of recipient religiosity, $B = 0.04$, $t(293) = 0.09$, $p = .93$, and no interaction between the two, $B = 0.32$, $t(292) = 1.17$, $p = .24$, whereby self-reported anti-atheist prejudice actually led to *increased* giving to atheists and did so just as much as to religious recipients. This relationship between NATA and transfer amounts was fully mediated by CRS, with the 95% bias-corrected bootstrap confidence interval based on a 10,000 bootstrap sample ($B = 1.33$) not including zero (0.50–2.17).

Study 2

Method

Six hundred participants (282 female) were recruited again through MTurk in the same between-subjects design as in Study 1. The instructions and response format for the TG were designed to be as similar to the DG as possible, where participants chose how much of an initial US$0.30 endowment they would like to transfer to a second participant who was either atheist or religious. The crucial difference was that in the TG participants were told "Person B can then choose how much of the money they want to transfer back to Person A." Participants indicated how much they would like to transfer in 5-cent increments and after the transfer were asked to indicate on the next page the percentage of the money they expected the second player to transfer back to them (0%, 25%, 50%, 75%, and 100%). Predicted returns were measured to complement the main measure of transfer amounts in the TG as a more explicit measure of trust in the recipient.

In order to eliminate deception and to determine the bonuses paid to the participants, we looked at the decisions of two individuals who played as the second mover in a TG in an earlier unrelated study, identified either as a Christian or atheist and

matched the demographic information told to participants. These two second movers were then matched with all participants from the current study and paid accordingly for each matching. As the instructions did not indicate that second movers played only one game, this multiple matching procedure is not inconsistent with the instructions and therefore does not constitute deception.

As in Study 1, results are reported only for participants who passed comprehension checks for *both* games, took the survey only once, and who identified as either Christian or atheist ($n = 272$), but results using all religious participants can be seen in the SI of the Christian participants ($n = 140$), and nearly all were at least moderately certain that God exists (97%); and with a frequency of at least "occasionally," 83% reported thinking about religious issues, 79% reported praying, 79% reported experiencing God intervening in their life, and 62% took part in religious services. As in Study 1, Christian participants were more religious than those from other religions (see SI). Participants were politically moderate on average ($M = 4.67$, $SD = 1.72$). Using the same power analysis as in Study 1, this study was sufficiently powered.

Results

Transfer Amounts

Using the binary identification measure, and contrary to the results in the DG, Christian participants did not transfer more in the TG, $F(1, 268) = 2.01$, $p = .16$; there was no effect of recipient's religiosity, $F(1, 268) = 0.25$, $p = .62$; and there was no interaction with recipient religious belief, $F(1, 268) = 0.35$, $p = .56$ (see Figure 2a). However, when using a regression-based approach with the continuous measure of religiosity, there *was* a significant main effect of participant religiosity, $B = 1.46$, $t(269) = 2.62$, $p = .009$, whereby religiosity was associated with increased transfer amounts (see Figure 2b) and as with the binary measure, no significant main effect of recipient's religiosity, $B = -0.43$, $t(269) = -0.61$, $p = .55$ or interaction $B = 0.39$, $t(268) = 0.70$, $p = .48$. This effect using the continuous measure of religiosity was robust to controlling for gender, ethnicity, education, and political ideology; looking only at differences within Christians and not atheists; and held when using participants from all religious affiliations (see SI for statistics). It remains to be seen why identification as a Christian was not a significant predictor of transfer amounts, but crucially, the continuous measure was—including when looking at religious differences only within self-identifying Christians.

We next conducted these analyses using each individual component of the CRS to test whether these effects were explained by one particular component of religiosity. As in Study 1, there were again significant main effects of all five components when analyzed separately, but frequency of thinking about religious issues fully mediated the effect of all other CRS components and was the only significant term. $B = 1.91$, $t(265) = 2.45$, $p = .01$ in a multiple regression including each of the five components.

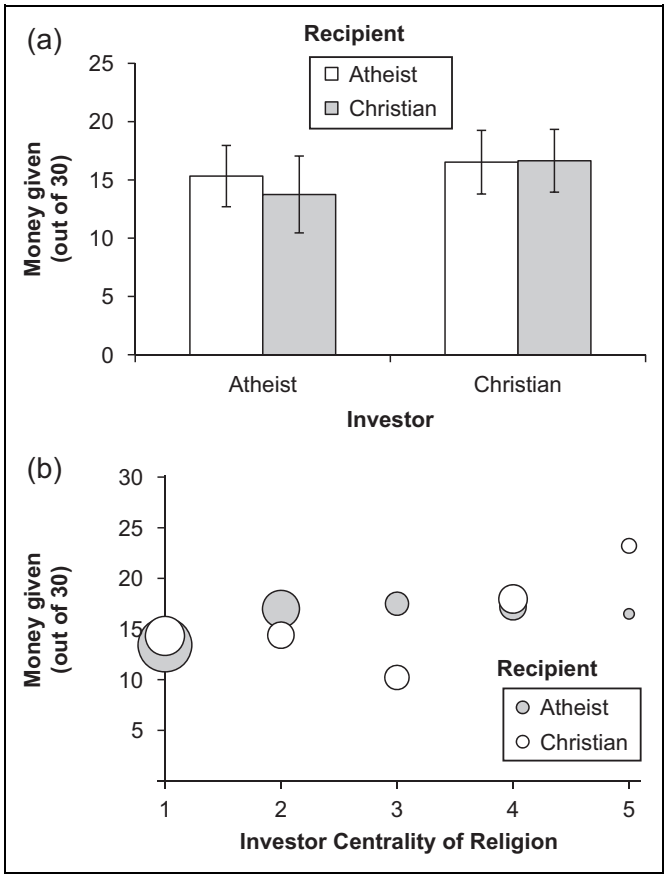

Figure 2. (a). Self-identifying Christian participants transferred more than atheist participants toward both Christian and atheist recipients in a Trust Game. Error bars represent 95% confidence intervals. (b). Transfers in cents in the Trust Game in Study 2 as a function of recipient and participant religiosity (binned to the nearest integer with the mean displayed across all observations in that bin). The size of the dots is proportional to the number of observations.

In contrast to Study 1, we found no main effect of NATA on giving, $B = 0.72$, $t(269) = 1.62$, $p = .11$, no significant main effect of recipient religiosity, $B = -0.45$, $t(269) = -0.63$, $p = .53$, and no interaction between the two, $B = 0.32$, $t(268) = 0.70$, $p = .48$.

Predicted Return

We next used the same analyses to explore effects of participant and recipient religiosity on participants' expectations regarding how much their partner would *return* to them in the TG. Christian participants predicted greater returns, $F(1, 268) = 4.65$, $p = .03$, and there was no interaction effect, $F(1, 268) = 1.43$, $p = .23$. In other words, Christian participants did not selectively trust other Christians by predicting them to return more than atheists. Given the sample size of 139, the current analysis had an 80% statistical power to detect a small- to medium-sized effect ($f = 0.24$) at an $\alpha = .05$, two tailed. Thus, if there had been a large effect of recipient religion on Christian participants' behavior on predicted returns, we

would likely have been able to reject the null hypothesis. Recall that the primary purpose of including the measure of predicted returns was to complement the dependent measure of transfer amounts to explore whether, if religious parochialism was observed in the TG, this could be attributed to greater trust of Christians over atheists. Because Christians were more prosocial in both the DG and the TG and showed no parochialism, we do not report further analyses on predicted returns though these can be seen in the SI

General Discussion

What is the extent and nature of religious prosociality? While some experimental research has suggested that religious individuals are more prosocial in general with regard to resource distribution (e.g., Ahmed, 2009), other work has suggested that religious people are only selectively prosocial toward coreligionists (e.g., Ruffle & Sosis, 2006), or even that they are not more prosocial at all (e.g., Eckel & Grossman, 2004). And if religious people are more prosocial, is this driven only by religious individuals' greater expectations of cooperation from others?

We found evidence consistent with the claim that religious prosociality is a generalized phenomenon based on prosocial preferences: Christian participants transferred more money to both Christian and atheist recipients in both the DG and the TG. Therefore, we found evidence for generalized religious prosociality and against religious parochialism: Christian religious participants were more prosocial than atheists, toward *both* religious and atheist recipients. This pattern was, however, explained entirely by how often people thought about religious issues. Such results provide clear behavioral evidence supporting conclusions from correlational and self-reported sociological data on the positive relationship between religiosity and both formal and informal prosocial behaviors (e.g., Monsma, 2007; Putnam & Campbell, 2010) while also suggesting that religious prosociality effects cannot be explained entirely as a result of social networks (e.g., Lewis et al., 2013; Putnam & Campbell, 2010), accountability arising from belief in an all-knowing God who punishes bad actions and rewards good actions (e.g., Atkinson & Bourrat, 2011; McKay et al., 2011) or the specific nature of religious rituals or experiences (e.g., James, 1902/1985; Xygalatas, 2013). But why might the frequency of thinking about religious issues be so predictive of prosociality, rather than the extent of belief in God, or frequency of religious practices or experiences? ''Thinking about religious issues'' is a broad category and likely includes representing many aspects of religious life and traditions. One possibility is that increased frequency in thinking about religious issues may represent a more general existential orientation to one's life as a whole and its nature, purpose, and meaning, while another (not mutually exclusive) possibility could be that thinking about religious issues represents reflection on moral norms within religious traditions that decree how to live. Further investigation is necessary to better characterize what this category entails, and the extent to which it could also be a part of nonreligious institutions that inculcate strong moral contemplation (e.g., de Botton, 2012), and help to build habits of virtue (e.g., Peysakhovich & Rand, 2015) or whether its content is specific to religious traditions that incorporate beliefs, practices, and experiences oriented around supernatural and transcendent ideas.

Because Christians treated the TG and DG identically even though each game represents a different incentive structure, this evidence suggests that religious prosociality is driven by social preferences about moral behavior rather than by strategic expectations about how others will respond. This ties together with prior evidence that religious individuals rely more heavily on their intuitions (Shenhav, Rand, & Greene, 2012), and that intuitive processing tends to support cooperation in economics games (Bear & Rand, 2016; Rand, Greene, & Nowak, 2012; Rand et al., 2014), even when interacting with out-group members (Rand, Newman, & Wurzbacher, 2015). It would be fruitful for future research to explore further the connection between religiosity and intuitive cooperation.

A potentially interesting and unexpected finding was that in the DG, Christians who explicitly reported more NATA actually still acted more prosocially toward atheists. Although mediation analysis indicated that this effect was explained by religiosity, rather than negative attitudes against atheists per se, it remains an interesting example of an intergroup context where explicit negative attitudes do not correspond with behavioral discrimination, suggesting that future work on antiatheist and other forms of prejudice should take care to include behavioral manifestations of prejudice rather than reports of attitudes alone.

Certain limitations of the present work bear mention. First, it was salient in our design that the recipient was either a self-identifying atheist or a Christian. As with most economic game experiments, there is always a concern about demand characteristics. Specifically, the saliency of the recipient's belief may have introduced demand characteristics which could have affected results. Nonetheless, even if participants did guess that the study was about religion, and only Christians—but not atheists—were motivated to present their group in a positive light, this would be interesting in itself because atheist participants are the ones for whom acting prosocially would help improve their group's image and so may be especially motivated to respond to the demand characteristics in question. A second consideration is that while the Christian participants in our sample were moderately religious, it remains to be seen whether these results would be observed among participants who are very highly religious and enmeshed in their religious communities. It would be useful, therefore, for future work to explore the extent of religious prosociality in less artificial contexts and with more participants who are extremely high on religiosity.

We end by noting that we welcome future replications of these studies. We do not know under what conditions the present findings are highly likely to replicate but believe that the present findings will replicate—at a minimum—when using (a) online studies with mainly, (b) Christian religious

participants, (c) relatively low payoff stakes, and where (d) the religious identity of the recipient is highly salient.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: Financial support from the John Templeton Foundation is gratefully acknowledged.

Note

1. Assuming equal variance resulted in the same p value of $p = .03$.

Supplemental Material

The online data supplements are available at http://spps.sagepub.com/supplemental.

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Handling Editor: Yuichi Shoda