Faithless found: Replication and extension of Gervais (2011)

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Abstract

Much research has examined negative stereotyping and prejudice against atheists. Comparatively less research has examined how to reduce prejudice against atheists, nor have anti-atheist prejudice interventions been replicated. Gervais’ (2011) studies were an early entry into the atheist prejudice reduction research and found, counterintuitively, that when atheists were perceived as prevalent, anti-atheist prejudice decreased. No research has been done to successfully replicate this interesting finding. Our three proposed studies conceptual replications of Study 3 in Gervais’ (2011) original studies. Participants in all three studies will be presented with information suggesting that atheists are either common or rare. Study 1 will replicate Gervais’ (2011) original conditions in an undergraduate sample; specifically, participants will read that atheists are either common (50% of students on campus are atheists) or rare (5% of studies are atheist). In Study 2, a nationally representative sample of Americans will read that either 33% (common) or 5% (rare) of Americans are atheist. Finally, Study 3 will have a nationally representative sample of Americans view a graphic showing that Americans overestimate (vs. underestimate) the prevalence of atheists in the general population. Potential moderating factors such as participant religiosity and Christian Nationalism will be assessed across all studies. We will use three criteria to determine whether the replication was successful (frequentist statistics, a “small telescopes” approach, and Bayes Factors).

Keywords: atheists, prevalence, prejudice reduction, replication, Registered Report

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Atheists are one of the most disliked social groups in the United States (Edgell et al., 2016; Franks & Scherr, 2014; Gervais, 2014; Giddings & Dunn, 2016) and are seen as untrustworthy and immoral (Cook et al., 2015; Gervais et al., 2011; Gervais, 2014; Grove et al., 2020). Anti-atheist prejudice has been documented across the world (Gervais et al., 2017), and even atheists intuitively see atheists as immoral (Gervais, 2014; Gervais et al., 2017; but see Mallinas & Conway, 2022). Research indicates that distrust is a major driver of anti-atheist prejudice (Gervais et al., 2011). Trust is crucial in social relationships (Cottrell et al., 2007); therefore, understanding ways to reduce anti-atheist prejudice and atheist distrust is important.

Much research has examined how to reduce prejudice and increase trust in atheists. Subverting stereotypes about atheists being untrustworthy or immoral (Grove et al., 2020) and presenting morality as innate and nonreligious in nature (Mudd et al., 2015) have been shown to *not* reduce anti-atheist prejudice. Other attempts at reducing prejudice against atheists have been more successful. Past research has found that reminding religious believers of secular forms of authority (e.g., the police; Gervais & Norenzayan, 2012), having religious believers read about evolution (Magee & Hardin, 2010), describing atheists as adhering to a moral code and showing warm concern for others (Mallinas & Conway, 2022), presenting atheists as moral by emphasizing their concern for harm reduction, care, and compassion (Simpson & Rios, 2017; Simpson et al., 2019), and imagining intergroup contact with atheists (LaBouff & LeDoux, 2016) can reduce anti-atheist prejudice and increase willingness to interact with and help atheists.

Given the increasing number of religiously unaffiliated individuals in the US (Pew, 2022) and in the West more broadly (Inglehart, 2021), it is important to examine whether attempts to reduce anti-atheist prejudice replicate over time. However, replication studies in the psychology of religion, and in the area of anti-atheist prejudice reduction research, are few and far between (Van Elk, 2019), with one exception being a series of studies that replicated the effect of emphasizing atheists’ capacity for care and compassion on prejudice reduction (Simpson & Rios, 2017; Simpson et al., 2019). Without replicating and extending the findings of previous work, it is difficult to understand the efficacy of anti-atheist prejudice interventions. In the current research, we attempt to replicate the findings of one of the earliest sets of studies on anti-atheist prejudice reduction. In that set of studies (Gervais, 2011), perceptions of greater atheist prevalence reduced distrust of atheists. Gervais’ (2011) work has been cited over 180 times, but the results of this research have not been thoroughly re-investigated (see Koger, 2018, for a partial exception).

More than a decade has passed since the Gervais (2011) research was conducted, and the religious context has changed substantially since that time. This research was originally conducted in Canada, where 77% of the population identified as religious in 2011 (Pew, 2013), a proportion that declined to 68% in 2019 (Cornelissen, 2021). A similar decline can be seen in the United States: 76.5% of the population identified as religious in 2015, a proportion that has dropped to 69% as of 2021 (Smith, 2021). Concurrently, atheist identification in the United States may be underestimated, with nearly one in three atheists not acknowledging their atheism in online anonymous surveys (Gervais & Najle, 2018). The total number of atheists worldwide may be similarly underestimated.

Perceptions of atheist prevalence seem to be rather high. Across four studies, we asked two undergraduate samples and two MTurk samples to estimate the percentage of Americans who identified as nonreligious (defined as “atheist, agnostic, or nothing in particular religiously”; Smith, 2021). See Table 1 for the full descriptive statistics of these estimations. While we did not specify atheist prevalence, research conducted by YouGov (Orth, 2022) has found that Americans estimate that 33% of Americans are atheist. Our estimates are a bit higher, perhaps reflecting the larger nature of the “nonreligious” category.

Past research (Craig & Richeson, 2014; Schleuter & Scheepers, 2010) has shown that when people learn that groups perceived as threatening are projected to increase in size, they subsequently report *more* prejudice against those groups. Therefore, the finding that presenting atheism as common can reduce anti-atheist prejudice (Gervais, 2011) is counterintuitive. There is a possibility that social consensus effects can explain these findings. If people believe that atheists are prevalent (i.e., the consensus is that atheism is “no big deal”), then higher perceptions of atheism should reduce prejudice against this group. This line of thinking squares with “coming out” as atheist (language that mirrors tactics embraced by the LGBTQ community) as a way to engage in atheist activism (Cimino & Smith, 2007). If Gervais’ (2011) findings replicate, the tactic of “coming out'' to increase perceptions of prevalence may be another method of reducing anti-atheist prejudice.

**Case Study: Gervais (2011)**

Gervais’ (2011) first study demonstrated that, across 54 countries, atheist prevalence (operationalized as the percentage of respondents who answered *no* to the item “I believe in God” in each country) was negatively associated with anti-atheist prejudice among religious believers, controlling for individual differences in demographic characteristics (e.g., educational attainment) and international differences in socioeconomic development or individualism/collectivism. Individuals in Study 2 demonstrated the same negative relationship between perceived prevalence of atheists and anti-atheist prejudice, even when controlling for belief in a dangerous world (BDW) and belief in God in an undergraduate sample. Studies 3 and 4 demonstrated a causal relationship between perceived atheist prevalence and anti-atheist prejudice. In Study 3, participants (undergraduate students at the University of British Columbia) in the “atheists common” condition read an article stating that roughly 50% of students at their university were atheists and that atheists are prevalent worldwide (Zuckerman, 2007). Participants in the “atheists rare” condition instead read that only about 5% of studies at their university were atheists and that atheists were rare around the world. All participants then responded to measures of atheist distrust, a feeling thermometer where a score of 0 indicated the coldest possible feelings toward atheists and a score of 100 indicated the warmest possible feelings toward atheists, an open-ended question about how many atheists they knew, and two items tapping into contact with atheists. Results indicated that participants in the “atheists common” condition exhibited significantly less distrust against atheists compared to participants in the “atheists rare” condition. There was no difference in positivity toward atheists between conditions. Importantly, perceived atheist prevalence did not increase perceived contact with atheists. However, contact with atheists predicted more positive feelings toward atheists, consistent with intergroup contact research (Pettigrew & Tropp, 2006). Study 4 replicated and extended these findings. Participants either read and wrote about the demographic rise of nonreligious people, including an atheist prevalence rate of 20%, or were told to think and write about their favorite food. Participants then took a trust-specific version of the IAT. In this task, participants were familiarized with two fictional women, Julie and Vanessa, who were counterbalanced as being either an atheist (someone who does not believe in God) or religious (someone who believes in God). Participants were then tasked with associating each target with words related to either trust (e.g., honest) or distrust (e.g., deceitful). Results indicated that implicit distrust (i.e., speed of associating the atheist target with the distrust-related words) was reduced in the atheist prevalence condition compared to the control condition.

We plan to attempt to conceptually replicate and extend the findings of Study 3 in this suite of studies. The first two studies in the original suite of studies (Gervais, 2011) are correlational in nature, which makes replicating these studies a less informative endeavor. Study 4 utilized the Implicit Association Test (IAT), which has come under fire in recent years for its questionable validity (e.g., Lai & Wilson, 2021). Furthermore, the effect sizes of the explicit distrust and implicit distrust findings in Study 3 and Study 4 (Gervais, 2011) are comparable. Therefore, the effect of atheist prevalence on distrust of atheists should be similar in size, regardless of whether distrust is measured explicitly or implicitly. Anti-atheist prejudice (in terms of voting for atheist candidates) has been shown to be commonplace; that is, people are largely comfortable explicitly disapproving of atheists (Brown-Iannuzzi et al., 2019). Other research examining prevalence and prejudice (Craig & Richeson, 2014; Mackey & Rios, 2022; Ponce de Leon et al., 2022) has measured prejudice through explicit measures. Thus, focusing on the study using explicit anti-atheist prejudice is a viable methodological option.

**Follow-up Research**

Gervais (2011) indicated that “contact-like effects are a possibility” when individuals read about atheists being prevalent (p. 552). Indeed, more recent research has demonstrated that even imagined contact with atheists can decrease anti-atheist prejudice. LaBouff and LeDoux (2016), using an imagined interaction paradigm (see Turner et al., 2007), found that imagining a positive interaction with an atheist (compared to simply thinking about atheists) led to less distrust and more willingness to help atheists. Further, these findings held when controlling for participant religiosity, right-wing authoritarianism, and religious fundamentalism. However, the authors (LaBouff & LeDoux, 2016) caution that their manipulation may have induced a demand characteristic whereby participants may have been motivated to respond in accordance with LaBouff and LeDoux’s (2016) hypothesis. Furthermore, the distinction between perceived prevalence and imagined contact remains uninvestigated.

In an unpublished study, Koger (2018) attempted to replicate Gervais’ (2011) original findings. They conducted a pilot study on undergraduate students to determine how prevalent atheists were on their campus. Participants in this pilot study reported that they believed that 27% (*SD* = 19%) of students were atheist, larger than Gervais’ (2011) pilot data (*M* = 11.45%, *SD* = 9.49%). However, the original manipulation, with the “atheists common” condition referring to 50% of the university population being atheists and the “atheists rare” condition referring to 5%, was used in this research as well (Koger, 2018). Participants were randomly assigned to read one of six potential articles, half of which claimed that atheist prevalence was high (10% worldwide; 50% on campus) and half claiming that atheist prevalence was low (rare worldwide and 5% on campus). Two articles were almost identical to Gervais’ (2011) and only provided information about atheist prevalence at the university and worldwide. Four other articles included atheist prevalence information as well as information about the power and coherence of these groups. In high coherence/power conditions, atheists were described as having formed atheist organizations, electing theists to public office, and reporting interest in changing social policy worldwide and atheist students on campus are active in atheist groups on campus and are involved in student government. In low coherence/power conditions, atheists were described as having few atheist organizations and rarely elected to public office worldwide and atheist students on campus do not have an active atheist group and are unlikely to be involved in student government. Utilizing a 3 (group organization: high, low, no information control) x 2 (prevalence: high, low) design, Koger (2018) examined whether atheists needed to be seen as a powerful, coherent group to elicit threat when they are perceived as prevalent. Overall, Gervais’ (2011) main findings that increased atheist prevalence would reduce anti-atheist prejudice did not replicate, nor were there effects of perceived power or coherence of atheists as a group (Koger, 2018). To date, this is the only attempt to replicate Gervais’ (2011) original findings.

Many unexplored questions remain regarding what the original findings (Gervais, 2011) represent. For example, why does reading about the prevalence of atheists reduce distrust of atheists but not negative feelings toward atheists? What is the relationship between intergroup contact and perceived prevalence of nonreligious outgroup members? The following proposed studies attempt to elucidate the mechanisms underlying the relationship between prevalence of atheists and distrust of atheists.

**Predictions**

Based on previous research, we make three competing predictions. First, there may be a successful replication of the original findings, whereby individuals who are led to believe that atheists are prevalent will exhibit less anti-atheist prejudice. Previous work (Gervais & Norenzayan, 2012; Labouff & LeDoux, 2016; Magee & Hardin, 2010; Mallinas & Conway, 2022) has demonstrated that atheist-prejudice can be ameliorated through experimental means. There is a precedent for anti-atheist prejudice interventions to replicate as well (Simpson & Rios, 2017; Simpson et al., 2019). Additionally, even imagined contact with atheists can reduce anti-atheist prejudice (LaBouff & LeDoux, 2016). There is also the possibility that atheist prevalence brings on anticipation for future contact with atheists (Gervais, 2011). This retroactive contact mechanism was purported to be a potential reason for the original findings found by Gervais (2011), and a similar effect may still emerge if the current research successfully replicates the original findings.

Second, research shows that increases in the prevalence of certain groups can *increase* prejudice (Craig & Richeson, 2014). For example, individuals who perceive large immigrant population sizes also report greater anti-immigrant prejudice and perceive more threats to their group (Schleuter & Scheepers, 2010). More recent research has shown that groups that elicit symbolic threat are perceived to be more pervasive compared to groups that do not elicit as much symbolic threat (Ponce de Leon et al., 2022). Thus, symbolic threat increases perceptions that a group is pervasive in a given area as well as growing in number. When groups associated with symbolic threat (e.g., LGBTQ Americans; Rios et al., 2018) are projected to increase in number, they elicit even more symbolic threat, which in turn predicts more prejudice against them (Mackey & Rios, 2022). Atheists are typically associated with symbolic threat (Rios et al., 2018); thus, increasing perceptions of prevalence and/or pervasiveness of atheists within a given area (e.g., the presence of atheists on a university campus) may increase feelings of symbolic threat, which in turn might predict more (rather than less) prejudice against atheists. For example, when atheists are perceived to be prevalent in science, religious believers distrust science more (Simpson & Rios, 2019). This tendency to hold prejudice against atheists may be especially likely for individuals high in Christian Nationalism, an ideology that advocates for Christianity to hold a major role in American civic life, as has been discovered in recent work (Al-Kire et al., 2021).

Finally, it is possible that there will be null effects of the prevalence manipulation on anti-atheist prejudice. The previously discussed research by Koger (2018) did not replicate the original Gervais (2011) effect, whereby perceived prevalence of atheists reduced anti-atheist prejudice. Furthermore, the religious demographic makeup of the United States has dramatically shifted since Gervais’ work was published. Twenty-nine percent of Millennials (i.e., adults born between the early 1980s and the mid 1990s) and 34% of Generation Z (i.e., adults born after the mid 1990s) identify as religiously unaffiliated (Cox, 2022). Furthermore, across the American population, traditional markers of religiosity such as church attendance and believing that religion is an important aspect of one’s life have declined as well (Pew, 2019; Smith, 2021). It is possible that younger people, who are less hostile to nonreligion than older people, may simply not find atheists threatening. Perhaps perceptions of atheist prevalence in the overall American population are already high, given the increasing number of nonreligious Americans and their continued predicted growth (Pew, 2022). Indeed, previous estimates (Orth, 2022) indicate that Americans perceive many minority groups as larger than they actually are. Thus, an atheist prevalence manipulation may have a negligible effect of reducing anti-atheist prejudice if atheists are already seen as prevalent.

**Overview of Research**

The current manuscript will involve directly and conceptually replicating previous work (Gervais, 2011, Study 3). Study 1 will involve undergraduate students. Study 2 will replicate Study 1 with a nationally-representative sample through an online crowdsourcing platform (Prolific Academic). Study 3 will be a replication of Study 2, with updated atheist prevalence information presented in graphical format. All studies will be reviewed by the Institutional Review Board at the first author’s home institution. After the first review process, we will pre-register all our hypotheses (discussed further in the manuscript) and supply a link to the materials for the studies on the Open Science of Religion homepage in the Open Science Framework site (https://osf.io/d3r8q/).

**Methods**

**Participants**

Participants for our studies will be recruited from a large public Midwestern university (Study 1) and on Prolific Academic (Studies 2-3). Study 1 will involve recruiting undergraduate students and compensating them with course credit for participating in the study, and Studies 2-3 will involve recruiting nationally-representative samples and compensating them with $1.00 for participation in the study. We will accomplish this using Prolific’s “nationally representative” distribution feature. An *a priori* power analysis in G\*Power 3.1 (Faul et al., 2009) to detect a small-to-medium effect size of *f* = .15 (Cohen’s *d* = .30) at 90% power using a 2x2 ANOVA design indicated that a sample size of 469 would be needed. Anticipating exclusions, we will recruit 500 participants for each study. This sample size was chosen in light of Gervais’ (2011) original effect size (*d* = .41). Building on this original effect size, we selected a sample size that would be able to detect our proposed moderators at higher power levels (Brandt et al., 2014). We chose a 90% power level to combat the possibility that the original study was underpowered (Anderson et al., 2017).

The original study (Gervais, 2011, Study 3) did not use exclusionary criteria based on demographics; thus, across all studies, we will not exclude participants based on demographic criteria. However, we will exclude participants based on statistical outliers on our dependent variables. We will exclude participants with Cook’s *D* and/or standardized residuals that are more than 3 *SD* from the mean. We will also exclude participants on Prolific Academic with duplicate IP addresses and participants who express suspicion of our hypotheses (see the Suspicion Probe below for details) or who are not American citizens. Finally, participants who do not complete every variable for any reason (including technical errors) will be excluded from analysis.

**Materials and Procedures**

**Independent variable: Atheist prevalence.** In Study 1, participants will be randomly assigned to read one of two articles. In the “atheists common” condition, participants will read that atheists are common worldwide and that roughly 33% of students at their university are atheist. In the “atheists rare” condition, participants will read that atheists are rare worldwide and only about 3% of students at their university are atheist. These percentages are based on previous estimates and the actual percentage of atheists as reported in a YouGov report (Orth, 2022). In Study 2, the same conditions will be presented, but instead of specifying one’s university, participants will read that either 3% of Americans (rare) or 33% of Americans (common) are atheist (as these are adults recruited online). In Study 3, participants will be presented with a brief writeup showing how Americans either overestimate the number of atheists in the general population (including atheists, who they rate 33% of the population but are actually 3% of the population) in the “atheists rare” condition or that Americans underestimate the number of atheists in general (where Americans believe 3% of the population are atheists when in reality 33% of the population are atheists). They will additionally be presented with a graph based on a YouGov report who conducted similar research (Orth, 2022), thus providing more ecological and face validity than the previous two studies. See Figures 1-2 and the full materials for prototype graphs used in this manipulation. We use 3% in the “rare” condition because this number is consistent with previous work (Gervais, 2011; Orth, 2022) estimating atheist prevalence and 33% in the “common” condition because our pilot data indicates that individuals see that percentage as relatively common.

**Manipulation check.** As a manipulation check, participants across all studies will rate their agreement with the statement “Atheists are very common” on a 7-point scale (*strongly disagree* to *strongly agree*).

**Suspicion probe.** We will also include a suspicion probe at the end of the survey. We will ask participants “What do you think was the aim of this study?” to probe the extent to which participants are aware that the prevalence manipulation was intended to influence their attitudes toward atheists. Participants will additionally be asked “Do you think you have been deceived in any way?” as a yes-no question. If participants indicated that they were deceived, they will see the following prompt: “How do you think you were deceived?” This will allow a secondary check of suspicion of our hypotheses. If participants express suspicion of our hypotheses (i.e., if participants believe that the vignette they read was meant to influence their attitudes toward atheists), they will be excluded from analysis.

**Believability check**. Participants will be asked at the end of each study, “When reading the material for this study, did you believe that atheists made up the percentage of the population you were given?” on a 7-point scale (1 = *I didn’t believe the statistic was true at all*, 7 = *I totally believed the statistic to be true*).

**Outcome measures (Gervais, 2011).**

***Distrust***. Atheist distrust will be measured on a 7-point scale (*strongly disagree* to *strongly agree*) with two items: “Atheists are dishonest” and “Atheists are trustworthy.” The latter item will be reverse-scored. These items will be averaged together (Gervais, 2011).

***General attitudes toward atheists***. General attitudes toward atheists will be assessed with a 101-point feeling thermometer (0 = *coolest feelings*, 100 = *warmest feelings*).

***Perceived contact***. Participants will provide an open-ended estimate of how many atheists they know. Then, participants will rate their agreement with two items: “I often come into contact with atheists” and “I rarely, if ever, come into contact with atheists.” The latter item will be reverse-scored and these items will be averaged together to form a composite measure of contact with atheists.

**Additional outcome variables.**

***Negative Attitudes Toward Atheists***. The Negative Attitudes Toward Atheists (NATA) scale (Gervais, 2011) will be used as a multi-item measure of anti-atheist prejudice, as the original study (Study 3) only used single-item measures of anti-atheist prejudice. This seven-item measure will be on a 7-point scale (*strongly disagree* to *strongly agree*). An example item is “Religion facilitates moral behavior in a way that nothing else can.” This measure correlates positively with other anti-atheist measures (e.g., feeling thermometers; Simpson et al., 2017).

***Magee and Hardin (2010) anti-atheist prejudice measure.*** We will add the 5-item anti-atheist prejudice measure created by Magee and Hardin (2010). This measure will be on a 7-point scale (*strongly disagree* to *strongly agree*) and an example item is “I feel uncomfortable around atheists.”

***Semantic differential scale***. We will add a semantic differential scale from Franks and Scherr (2014) measuring on a 7-point scale the extent to which atheists are seen as untrustworthy (vs. trustworthy), threatening (vs. comforting), and disgusting (vs. appealing).

**Additional measures for hypothesis testing**.

***Religious affiliation***. Gervais (2011) did not indicate the religious affiliation of the participants in their study. However, more recent research (e.g., Van Cappellen & LaBouff, 2022) has demonstrated that Christians are more likely to express prejudice against atheists than against other religious and nonreligious groups. In our demographics section, we will ask participants “What is your religious affiliation?” with the options Christian-Catholic, Christian-Protestant, Christian-Other, Jewish, Muslim, Hindu, Buddhist, Atheist, Agnostic, Spiritual but not religious, Other religious affiliation (with the ability to specify this affiliation), and “I do not consider myself affiliated with any religion” (i.e., religious nones). Thus, in each study, we will compare results between religious individuals (Christians, Jews, Muslims, Hindus, Buddhists) and nonreligious individuals (spiritual but not religious, Atheist, Agnostic, and “religious none”).

***Religiosity***. We plan to examine religiosity as an exploratory moderator using the Supernatural Beliefs Scale (Jong et al., 2013), which measures participants’ tendencies toward religious belief with ten items on a 9-point scale (1 = *strongly disagree*, 9 = *strongly agree*). An example item is “there exists an all-powerful, all-knowing, all-loving God.”

***Christian Nationalism.*** The Christian Nationalism measure (Whitehead & Perry, 2020) will be used to assess Christian Nationalism as a moderator of the relationship between perceptions of atheist prevalence and distrust and general attitudes toward atheists. This measure includes six items on a 7-point scale (*strongly disagree* to *strongly agree*). An example item is “The success of the United States is part of God’s plan.” Recent work (Al-Kire et al., 2021) has demonstrated that presenting Christians with information about Christian demographic decline and the subsequent increase in religiously unaffiliated individuals (including atheists) evoked increased religious threat, which in turn predicted higher levels of Christian Nationalism. The proposed manipulation of atheist prevalence may invoke a similar threat reaction; therefore, we propose Christian Nationalism will moderate the relationship between condition (atheists common vs. atheists rare) and distrust/general attitudes toward atheists. In particular, participants with higher levels of Christian Nationalism will report more distrust and more negative attitudes toward atheists than participants with lower levels of Christian Nationalism.

**Analyses**

In accordance with the original research (Gervais, 2011, Study 3), we will determine whether participants in the “atheists common” condition are more likely to agree that “Atheists are very common” with a manipulation check. A successful manipulation check would demonstrate that participants in the “atheists common” condition would see atheists as very common compared to participants in the “atheists rare” condition.

Following the guidelines of previous replication research (Gervais et al., 2020), we will define replication success according to three criteria: a) producing statistically significant results in the same direction as the original findings, b) a “small telescopes” approach for assessing the replicability of effects (Simonsohn, 2015), and c) Bayes Factors comparing a null hypothesis with an alternative hypothesis informed by the original effect size (*d* = .41; Gronau et al., 2020). For Study 1, we will test Hypothesis 1, that atheist prevalence is associated with reduced prejudice (as assessed by the atheist distrust measure), using two independent-samples *t*-tests using statistical significance purposes and will compare the null hypothesis effect size of *d* = .0 with the original effect size *d* = .41. Hypothesis 2, that atheist prevalence is associated with warmer feelings toward atheists (as measured by the feeling thermometer) and more positive attitudes toward atheists (as measured by reverse-scoring the Negative Attitudes Toward Atheists scale), will also be assessed through an independent-samples *t*-tests for statistical significance purposes; we will use JASP’s suggested default prior to analyze Bayes Factors (JASP Team, 2022). Hypothesis 3–that participants will not know more atheists between conditions–and Hypothesis 4–that participants will not perceive more contact with atheists between conditions–will also be tested using independent-samples *t*-tests and JASP’s suggested default priors. Hypothesis 5–that more contact with atheists will be positively correlated with warmer feelings toward atheists on the feeling thermometer–will be tested with a correlational analysis. These hypotheses mirror the findings of the original study (Gervais, 2011, Study 3). Our additional Hypothesis 6–that religious affiliation, religiosity, and/or Christian Nationalism will moderate the relationship between condition and distrust as well as condition as attitudes toward atheists–will be tested via Model 1 in PROCESS (Hayes, 2017).

For Studies 2-3, we will conduct multilevel models to control for state-level religiosity, as there is a “paradox” whereby religious engagement and other measures of religiosity are correlated negatively with well-being at the state-level and correlated positively at the individual level (Myers & Deiner, 2018). Since state-level religiosity was not considered in the original study, we will conduct this as an exploratory analysis. We will use the *lme4* package (Bates et al., 2015) in R (R Core Team, 2023) to conduct this analysis. We examine religiosity by averaging participants’ answers to the SBS (Jong et al., 2013) within each state. This process mirrors previous work examining country-level religiosity in multilevel analyses (Gebauer et al., 2017).

To examine dimensionality, reliability and model fit of the measures used in each study, we will conduct split-sample factor analyses. Using R (R Core Team, 2022), the acquired data will be split randomly into two separate samples. One split will be used to conduct an exploratory factor analysis (EFA) to assess dimensionality and initial assessments of reliability. Ideally, this will suggest a single factor solution when assessing various factors such as variance explained and cross loadings. The remaining sample will be used to conduct a confirmatory factor analysis (CFA) to examine the degree of model fit of the measures found in the EFA. The results of these analyses will be placed in Supplementary Materials as these analyses (as well as validity assessments) are beyond the scope of the current manuscript.

**Timeline**

If the Stage 1 manuscript is accepted, the timeline for this project will be as follows: data collection for Study 2 and Study 3 (i.e., the studies recruiting from Prolific Academic) will commence as soon as funding can be obtained after preregistering our analyses. Recruitment for Study 1 (the undergraduate sample) will start in the nearest upcoming semester (currently Fall 2023) and end at the completion of the semester or until the *a priori* sample size is obtained. Data cleaning and analyses for Study 2 and Study 3 will begin when data collection ends, approximately two weeks after first placing these studies on Prolific Academic. Data cleaning and analysis for Study 1 will commence at the end of the semester of data collection (either January 2024 for fall semester or May 2024 for spring semester). Writing up the results of these studies will occur up to five months after this date (May 2024-October 2024).

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