

Does Religious Priming Induce Greater Prejudice? A Meta-Analytic Review

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Personality and Social
Psychology Bulletin
1–13

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DOI: 10.1177/01461672221135956
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Abstract

The current meta-analysis addressed whether theistic religious beliefs are causally related to greater prejudice by analyzing 44 studies ($N_{\text{total}} = 11,330$) that used experimental designs—priming religion and then measuring negativity toward outgroups (e.g., LGBT and Muslim). The overall priming effect was significant but small ($r = .06$), indicating that priming religion increases prejudice. The implications of these results for the relation between religion and prejudice and for future work on religious priming are discussed.

Keywords

religious priming, prejudice, meta-analysis, group processes

Received March 9, 2021; revision accepted October 3, 2022

Both theorizing and empirical work have established links between religion and two seemingly contradictory sets of human conduct: prosocial behavior and prejudice. As noted by Allport (1954), religion “makes prejudice and it unmakes prejudice” (p. 444). History bears witness to this paradox as the charitable work that is often carried out by religious institutions coexists with wars and massacres perpetrated against people of other faiths (e.g., the crusades, the Thirty Years’ War and St. Bartholomew’s Day massacre). Consistent with Allport’s (1954) thesis, research has shown that religion encourages prosocial behavior but is additionally related to prejudice and racism. However, though previous meta-analyses have assessed correlational studies of religion and prejudice (Hall et al., 2010; Whitley, 2009), these meta-analyses cannot establish causality. In addition, each of these meta-analyses examined prejudice directed at one particular group (e.g., Black, LGBT, and immigrants). The current meta-analysis examined the causal relation of theistic religious beliefs with prejudice by synthesizing studies with experimental designs that examined a wide variety of target groups. This contributes to a better understanding of the relation of religion with prejudice across target groups and provides an estimate for the average effect of priming religion on prejudice.

Religion and Prejudice

Priming religion has been meta-analytically linked to greater prosocial behavior (Shariff et al., 2016), possibly because priming reminds religious people that their behavior is monitored by supernatural deities who reward good deeds and punish misconduct. Conversely, researchers have also

established meta-analytic links between greater religiosity and racism (Hall et al., 2010), between greater religiosity and negative attitudes toward lesbians and gay men (Whitley, 2009), and between religious affiliation and negative attitudes toward immigrants and refugees (Deslandes & Anderson, 2019).

A number of theoretical frameworks within social psychology help explain the link between religion and prejudice. Religion provides a strong basis for intergroup bias, which occurs when group members favor their ingroup and derogate outgroups (Johnson et al., 2012). Religion is also a powerful meaning-making framework and thus may make certain social categories seem more threatening compared with others (e.g., value-violating groups like LGBT people or atheists; Rowatt et al., 2013). In addition, conservative religious values are associated with religious fundamentalism (belief in one set of religious teachings as essential and inerrant truth) and right-wing authoritarianism (characterized by conventionalism, submission to authority, and authoritarian use of aggression), which, in turn, are related to prejudice (Hunsberger & Jackson, 2005).

That priming religion is related to prosociality while religiosity and religious affiliation are related to prejudice is not necessarily contradictory. Hall et al. (2010) provides a compelling explanation for the dual relation based on theories of

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intergroup bias and conservative values. Intergroup bias is the tendency to behave more favorably to members of one's own in-group, while derogating members of outgroups (Dovidio & Gaertner, 2010). Believers practice religion with other same-race members and, thus, may view people of other races as members of outgroups. Believers are likely to distinguish between their faith and the faith of others, and to generalize this distinction to other attributes and personal characteristics. Given that the prosocial conduct religions encourage in their members is mostly directed at in-group targets (Haidt, 2012; Norenzayan, 2013), Hall et al.'s (2010) explanation accounts for the relations of religiosity with both racism and prosociality: Racism is meant to derogate the out-group and prosociality is meant to help the in-group. Hall et al. (2010) additionally posited that religious beliefs espouse conservative values such as conformity and respect for tradition. Since conservative values are associated with opposition to social equality and greater concern about threat from outgroups, they may also contribute to the relation between greater religiosity and racism.

However, the relation between religiosity and greater prejudice is nuanced, and the findings are somewhat inconsistent. Within prior meta-analyses, the same measures of religiosity demonstrated different relations with prejudice. Hall et al. (2010) found that extrinsic religiosity was related to racial prejudice, but intrinsic and quest religiosity were related to tolerance. In contrast, Whitley (2009) found that intrinsic religiosity was related to negative attitudes toward lesbians and gay men, but the relationship reversed for quest and was null for extrinsic religiosity. In a review of studies published from 1990 to 2003, Hunsberger and Jackson (2005) found that intrinsic religiosity was positively related to prejudice toward LGBT people but not racial/ethnic groups, while extrinsic religiosity was related to prejudice toward both LGBT people and racial/ethnic groups. Deslandes and Anderson (2019) reported that religious affiliation, but not self-reported religiosity, was related to negative attitudes toward migrants, particularly refugees.

One reason for these inconsistencies is that the intrinsic/extrinsic religiosity measure may not tap religiosity in the way it was intended (Hunsberger & Jackson, 2005). However, a more compelling explanation is that the mechanism by which religion is related to prejudice may differ by target group. As previously stated, religion may provide a meaning-making system that makes certain social groups seem more threatening than others (Rowatt et al., 2013). For example, groups that are viewed as value-violating groups (e.g., LGBT people) might be more threatening than non-value-violating groups (e.g., racial groups). Thus, those who strongly internally identify with religious values might be especially likely to demonstrate prejudice toward value-violators. Traditional religious teachings are directly opposed to homosexuality, but religious teachings also promote humanitarian values that oppose racism. This may explain why intrinsic religiosity was related to negative attitudes toward

lesbians and gay men, but not racism (Hall et al., 2010; Hunsberger & Jackson, 2005). Those who are religious for extrinsic reasons do not integrate religious values as personal values and, therefore, are not consistently opposed to lesbians and gay men. However, extrinsic religiosity is related to social conformity (Hall et al., 2010), which may explain the relation between extrinsic religiosity and racism; racist attitudes are still widely shared in both the United States and international society.

Thus, the inconsistent relationship between various measures of religiosity and prejudice is not surprising given that previous meta-analyses linking religion to prejudice covered different targets. Clearly, it is necessary to synthesize studies covering a broad range of target groups. The present research contributes further to the literature by examining the relation between religious priming and prejudice toward a wide range of groups (LGBT, atheist, Muslim, Jewish, Black, and women targets).

The Present Research

This work presents a significant contribution to the literature by addressing three issues. First, a meta-analysis is vital to understand whether there is truly a relation between religious priming and prejudice. A number of studies have shown that religious primes produce more prejudicial attitudes toward African Americans (Johnson et al., 2010), LGBT people (LaBouff, 2011), Muslims (LaBouff et al., 2012), and atheists (Clobert et al., 2015). These findings are often cited as evidence that religiosity is causally linked to prejudice (e.g., “. . . priming Christians with God concepts have been shown to increase derogation of a range of ethnic, national, and religious outgroups . . .,” Shariff, 2015, p. 108). But replication attempts have not always met with success (Ramsay et al., 2016), and there is even evidence linking religious primes with more positive attitudes toward outgroups (Shamoa-Nir & Razpurker-Apfeld, 2019b). Thus, there is a need for more definitive answers to two related questions. First, what is the overall effect of religious priming on prejudice? Second, if a reliable effect exists, does it generalize across “. . . a range of ethnic, national, and religious outgroups . . .”? as Shariff et al. (2016) suggested. The present meta-analysis addresses both questions by synthesizing studies across a broad range of target groups. We collected and coded data for all target groups that served as the object of prejudice in each study.

Second, the primary goal of this research was to establish whether there is a causal relation between religion and prejudice. This has not yet been established since the correlational data surveyed in the Deslandes and Anderson (2019), Hall et al. (2010), and Whitley (2009) reviews do not allow inferences of a causal relation between religion and prejudice. It is conceivable, for example, that prejudice moves people to seek and accept a value system (such as religion) that is consistent with their beliefs. The link between religion and prejudice may also be due to a third variable, such as the

conservative values that Hall et al. (2010) proposed, or other variables like personality characteristics. These limitations can be overcome with experiments that manipulate a religious prime and then administer prejudice measures. An identical rationale was used by Shariff et al. (2016) as a justification for their meta-analysis, which started with the general question of whether religious priming produces reliable psychological effects of all kinds. After they obtained evidence in support of the religious priming effect, these authors examined a subset of their data to examine whether religious priming facilitated prosocial behavior. The goal of this work is to examine a different subset of the religious priming literature to address a question almost inverse to what Shariff et al. (2016) examined, Does religious priming increase prejudice?

Third, this meta-analysis may be useful for future research decisions about empirical study designs. For example, a meta-analytic estimate of effect size for this relation will aid researchers in determining power and sample size, resulting in better-designed and adequately powered studies.

Type of Prime

Like Shariff et al. (2016), we divided the primes that researchers used into four categories: explicit, implicit, contextual, and subliminal. *Explicit primes* (e.g., “Think about God”) expose participants to religious content, facilitating conscious processing of the primes. *Implicit primes* also expose participants to religious content, but the purpose of the primes is disguised. Participants do not realize that a goal of the researcher is to increase the salience of religion. For example, a common implicit prime is a task in which participants form sentences from scrambled words (Srull & Wyer, 1979). In a religious priming condition, some of the words are related to religion (e.g., Haggard et al., 2019). Thus, although participants see the prime directly, they are not aware of the relevance of the prime because they believe they are simply performing a word task. *Contextual primes* mean that participants perform experimental tasks (e.g., completing measures of prejudice) in a religious location, such as a church. *Subliminal primes* are below awareness, such as when a religious word or symbol is flashed on a screen for such a short amount of time that the participant is not aware of seeing it (e.g., a Lexical Decision Task [LDT]; see Johnson et al., 2010). Researchers have speculated about the effects of different types of primes. For example, Razpurker-Apfeld and Shamo-Nir (2015) suggested that primes operating below awareness might fail to generate the group identity that serves as the link to intergroup bias. Alternatively, participants may respond to primes that are above awareness by seeking to alter reactions they deem to be undesirable (Fazio & Olson, 2014). It thus remains an open question which primes will have a greater impact on intergroup prejudice.

In summary, past meta-analyses have linked religiosity with prejudice but the causal relation between religion and prejudice is unknown. Although several investigators cite individual studies as evidence that religious priming increases prejudice, there has been no definitive review of the research literature on this topic.

Method

This section describes the literature search, study inclusion criterion, article screening, coding process, and plan of analysis. Figure 1 presents a flowchart depicting this process. We did not preregister the present research.

Literature Search and Data Requests

We first conducted searches using PsycINFO and Google Scholar. For the PsycINFO search, we used “relig* priming” as a stem and combined it with (i.e., “AND”) each of the following: “discrimination,” “conflict,” “racism,” “attitudes,” “implicit bias,” and “outgroup.” To this, we added searches based on “religious priming AND prejudice”; “relig* AND priming,” “relig* prompt AND prejudice,” “religion AND priming AND prosocial,” and “religion AND prejudice AND prosocial.” For the Google Scholar search, we used “religious priming” as a subject heading and “prejudice” as a key term. We additionally used cross-referencing to identify potential articles for inclusion. We also sent a request for unpublished data and published or unpublished papers, using the Society for Personality and Social Psychology and the APA Division 36 Listservs. Finally, we emailed authors who publish in this field to request additional papers or data that they could provide. Together, these search strategies yielded 875 records after removal of duplicates. After screening, we identified 28 records with 44 studies (total $N = 11,330$) that were included in this meta-analysis.

Selection Criteria

Studies were included if they utilized a religious prime versus a control prime and administered at least one measure of prejudice as an outcome variable. Studies were excluded if the religious prime used terms from a religion different from the participants’ religious affiliation (e.g., Christian terms administered to Muslim participants) as the prime would not be self-relevant. Studies were also excluded if the religious prime referred to nontheistic religions (e.g., Buddhism) for two reasons. The number of such studies that fit the other inclusion criteria was limited to two studies and, since nontheistic religions emphasize universalism and tolerance, religious primes in such contexts may not change attitudes toward outgroups. Hence, we focused only on theistic religions to ensure that religious priming activated in-group identity rather than universalistic values.

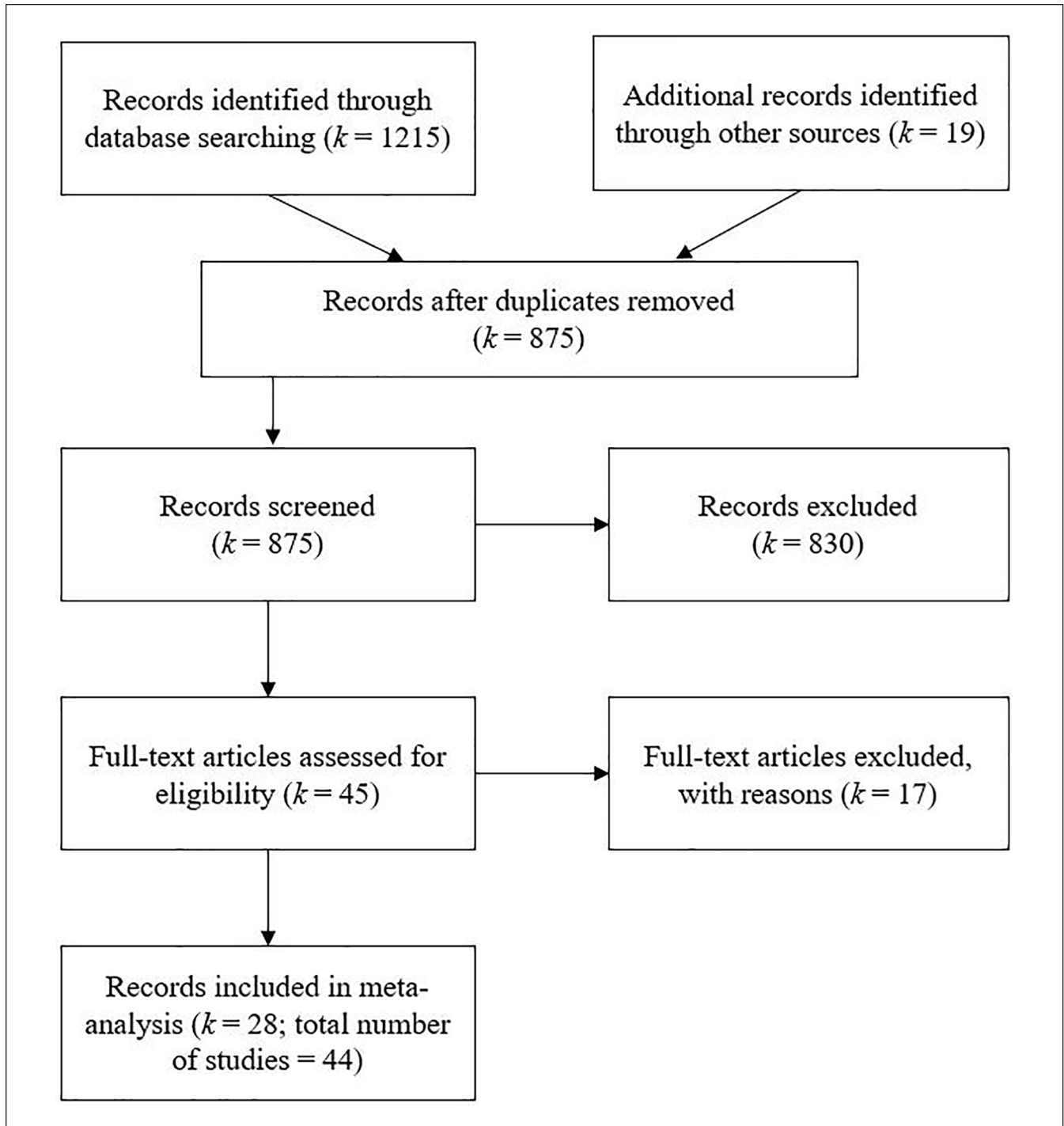


Figure 1. Flow Diagram Depicting the Database Search and Selection Process.

Note. The total number of independent records identified was 28. Of those 28 records, several had multiple studies, yielding 44 total studies included in the meta-analysis.

Coding

We coded the percentage of female participants, publication status (published vs. unpublished), year of study, participants' mean age, racial/ethnic composition of the participants (in percentages), the country in which the study was conducted, and participants' religious affiliations (in percentages). Following Shariff et al. (2016), we coded the type of prime used in the study as subliminal, contextual, implicit, or explicit.

For each study, we calculated a Pearson product-moment correlation to assess the magnitude of the relation between a religious versus control prime and the outcome measure. A positive correlation implies that participants who received a religious prime expressed greater prejudice than participants receiving a control prime. When more than one measure of prejudice was used, separate r s were calculated for each measure and then a weighted average was calculated (weighted by sample size). If only one target group was examined in the study, the effect size associated with that target group served as the overall priming outcome. If a study examined more than one target group, separate r s were calculated for each target, and an average r (weighted by sample size) was computed across all targets as an estimate of the overall priming effect. Targets examined by four or more studies in this meta-analysis were LGBT (sexual and gender minorities), atheist, Muslim, Jewish, Black, and women. Targets examined by single studies were European, foreign, rich, poor, immigrant, Hispanic, Hindu, and members of a rival university.

We also coded effects representing the interaction between the level of participants' religiosity (an individual difference variable) and religious priming. Finally, we coded whether the prejudice measure that served as the dependent variable was behavioral or self-report. Behavioral measures of prejudice include amount of money allocated to a target outgroup compared with an ingroup or active support for public policy that may affect an outgroup (such as support for policies that restrict LGBT rights), while self-report measures include judgments about others, scale ratings of preference for a group, or associating positive or negative words with a group.

All effect sizes were calculated by the first author and, independently, by a research assistant supervised by the second author. Discrepancies triggered new calculations to identify potential mistakes, which were corrected for the effect sizes ultimately used in the analyses. Using this process, we achieved full agreement between coders. Table 1 presents a list of studies, the relevant variables coded for each study, and the effect sizes computed. An expanded version of Table 1 (Table S1), the associated codebook, and other supplementary materials can be accessed on the Open Science Framework (OSF) at https://osf.io/gh7uy/?view_only=11539177a51b4c66a1a0814d79aee4bc.

Data Analysis. We conducted analyses using the statistical software R v. 4.1.1 (R Studio Team, 2020) with the metafor package (Viechtbauer, 2010). We tested random effects models, using restricted maximum likelihood estimation. These

models allow generalization of the results beyond the population of studies included in the meta-analysis (Borenstein et al., 2010). R analysis scripts can be accessed on the OSF link given previously. We also assessed our studies for evidential value using a p -curve analysis using the Web App, Version 4.06 (Simonsohn et al., 2015). Power analyses for random effects models were conducted in R with the meta-power package (Griffin, 2020).

Results

Overall Effect Size and Publication Bias

A preliminary test for outliers identified four effect sizes with a Cook's distance value bigger than .10, the cut-off criterion suggested by Cohen et al. (2003). As Hunter and Schmidt (2004) noted, it is difficult to distinguish between large sampling errors and outliers based on false data. Since the removal of the cases did not change the significance of the results, we retained all cases to avoid loss of information. Figure 2 presents a forest plot of all effect sizes and their CIs.

Across all studies, the overall effect of religious priming was significant but small, $r = .06$, [.03, .08], $p < .001$. The amount of heterogeneity in the distribution of the effect sizes was significant, $\tau^2 = .002$, $Q(43) = 68.24$, $p = .009$, and there was a moderate percentage of heterogeneity across studies, $I^2 = 31.16\%$. Heterogeneity can indicate the presence of moderators, which may include the wide variety of priming methods, prejudice measures, and targets employed in studies. A funnel plot, which graphs the effect sizes of all included studies based on the size and error of each effect (Sterne et al., 2011), is shown in Figure 3. Egger's regression was not significant, indicating that the effect sizes were evenly distributed around the average effect size, $Z = 1.90$, $p = .058$.

We also calculated the overall effect of religious priming on prejudice for each target group. However, power analyses revealed that only analyses of the LGBT and atheist target groups were adequately powered (LGBT group power = .95, atheist group power = .72) so we report only the results for those two groups, summarized in Table 2.¹ Both effects were significant: $r = .12$, [.05, .19], $p < .01$, for LGBT people and $r = .09$, [.01, .17], $p < .05$, for atheists.

It was important to test whether the classification of studies as published ($k = 26$) or unpublished ($k = 18$) influenced the overall priming effect. This moderation was significant, $b = .07$, [.04, .11], $p < .001$, and power to detect this moderating effect was $> .95$. A simple effects analysis shows that the religious priming effect was significant for published studies, $b = .09$ (.06, .12), $p < .001$, but not for unpublished studies, $b = .02$, (-.01, .04), $p = .208$. This raises a question about whether religious priming produces an actual effect. One concern is that evidence of publication bias might indicate the occurrence of data mining in the literature.

To address this, a p -curve analysis was conducted. A p -curve analysis examines the distribution of p -values in a

Table 1. Summary of Studies in Meta-Analysis.

Authors	Year	Study	Age	Country	Gender	<i>n</i>	Prime type	Prime task	Prejudice measure	Target group	<i>r</i>
Johnson et al.	2010	1	20	United States	0.78	73	Subliminal	LDT	Self-report	African Americans	0.133
Johnson et al.	2010	2	19	United States	0.62	43	Subliminal	LDT	Self-report	African Americans	0.329
LaBouff et al.	2012	1	32		0.51	98	Contextual	Context	Self-report	Multiple	0.287
Johnson et al.	2012	2	20	United States	0.57	73	Subliminal	LDT	Self-report	Muslim, Atheist, and LGBT	0.294
Clobert et al.	2015	2	20	Belgium	0.88	78	Subliminal	LDT	Other	African, Muslim	0.043
Clobert et al.	2015	3	21	China and Taiwan	0.59	82	Subliminal	LDT	Other	African, Muslim	0.125
Howard & Sommers	2019	1	19	United States	0.62	108	Subliminal	Image	Self-report	African Americans	0.047
Howard & Sommers	2019	2	20	United States	0.64	187	Subliminal	Image	Self-report + other	African Americans	0.12
Ramsay et al.	2016	1	21	Singapore	0.66	232	Implicit	Image	Self-report	Rival university	0.011
Ramsay et al.	2016	2	21	Singapore	0.55	104	Implicit	Image	Self-report	Immigrants	0.034
Haggard et al.	2018	1	27	Belgium	0.57	271	Implicit	Sentence unscramble	Self-report	Women	0.104
Haggard et al.	2018	2	23	Belgium	0.77	106	Subliminal	LDT	Self-report	Women	0.212
Haggard et al.	2018	3	37	United States	0.59	143	Implicit	Sentence unscramble	Self-report	Women	-0.082
Haggard et al.	2018	4	19	United States	0.78	133	Subliminal	LDT	Self-report	Women	0.132
LaBouff, J. P.	Unpub	1	20	United States	0.85	134	Contextual	Church vs. secular building	Self-report	LGBT, Muslim, African American	0.008
LaBouff, J. P.	Unpub	2		United States	0.85	88	Contextual	Church vs. secular building	Self-report + other	African, Atheist, and LGBT	0.127
Shamoa-Nir & Razpurker-Apfeld	2019b	1	28	Israel	0.87	47	Implicit	Word search task	Self-report	Muslim	0.03
Shamoa-Nir & Razpurker-Apfeld	2019b	2	24	Israel	0.83	42	Subliminal	LDT	Self-report	Muslim	-0.224
Shamoa-Nir & Razpurker-Apfeld	2019b	3	25	Israel	0.75	81	Subliminal + Explicit	LDT	Self-report	Muslim	0.039
Johnson, M. K.	Unpub	1	19	United States	0.76	182	Subliminal	LDT	Behavioral	Muslim and Atheist	0.034
Johnson, M. K.	Unpub	2	19	United States	0.71	156	Subliminal	LDT	Behavioral + Self-report	Muslim and Atheist	-0.013
Pope, A. R. D.	Unpub	1	38	United States	0.57	110	Explicit	Listening to sermon	Other	LGBT	0.197
Pope, A. R. D.	Unpub	2		United States	0.82	98	Subliminal	LDT	Behavioral	LGBT	0.058
Rutchick, A. M.	2010	2		United States		1457	Contextual	Church vs. secular building	Behavioral	LGBT	0.066
Bassett & Van Tongeren	Unpub	1		United States		651	Explicit	Reading task	Other	Muslim vs. Christian	0.006
Erum & Tsang	Unpub	1		United States	0.78	98	Implicit	Sentence unscramble	Behavioral + self-report	Muslim	-0.072
Al-Kire, R. L.	Unpub	1	19	United States	0.87	82	Explicit	Read and wrote about the prime	Self-report	Muslim vs. Christian	0.268
Ratchford & Al-Kire	2019	1	44	United States	0.66	447	Implicit	Sentence unscramble	Self-report	Multiple	-0.016
Shamoa-Nir & Razpurker-Apfeld	2019a	3	22	Israel	0.68	254	Implicit	Word search task	Self-report	Jewish	0.11
Jennings, J.	Unpub	2		United States		382	Implicit	Sentence unscramble	Self-report	Black	-0.01
Dolinska et al.	2019	2	26	Poland	0.50	280	Implicit + Subliminal	Word search task + LDT	Self-report	Muslim vs. Polish	0.176
Razpurker-Apfeld & Shamoa-Nir	2015	1	25	Israel	0.65	101	Implicit	Word search task	Self-report	Muslim + Jewish	0.136
Ginges et al.	Unpub	1				782	Explicit	"Think about God"	Behavioral	Atheist	0.047
Ginges et al.	Unpub	2				828	Explicit	"Think about God"	Behavioral	Muslim	0.009
Ginges et al.	Unpub	3				1,849	Explicit	"Think about God"	Behavioral	Atheist + Muslim	0.008
Yilmaz et al.	2016	2	20	Turkey	0.63	83	Implicit	Sentence unscramble	Self-report	Atheist, composite outgroup	0.302
Bloom et al.	2015	1				469	Explicit	Questions about religion	Self-report	Immigrants	0.045
Deel, D	Unpub	1	20	United States	0.57	123	Implicit	God images	Self-report	LGBT	-0.13
Gilad & Stepanova	2015	1	20	United States	0.57	145	Explicit	Read Bible verses	Self-report	LGBT	0.023

(continued)

Table I. (continued)

Authors	Year	Study	Age	Country	Gender	n	Prime type	Prime task	Prejudice measure	Target group	r
Howard, S.	Unpub	1	19	United States	0.55	118	Subliminal	LDT	Self-report	Black	0.096
Howard, S.	Unpub	2b	40		0.51	120	Subliminal	LDT	Self-report	Black	-0.076
Howard, S.	Unpub	4	25	United States	0.59	60	Subliminal	LDT	Self-report + other	White	-0.024
Van Tongeren et al.	2013	1	21	United States	0.68	45	Explicit	Religious vs. neutral passage	Self-report	Immigrants	-0.035
Wilkins et al.	2021	2	40	United States	0.61	287	Explicit	Wrote about religion	Self-report	LGBT	0.082

LDT: Lexical Decision Task.

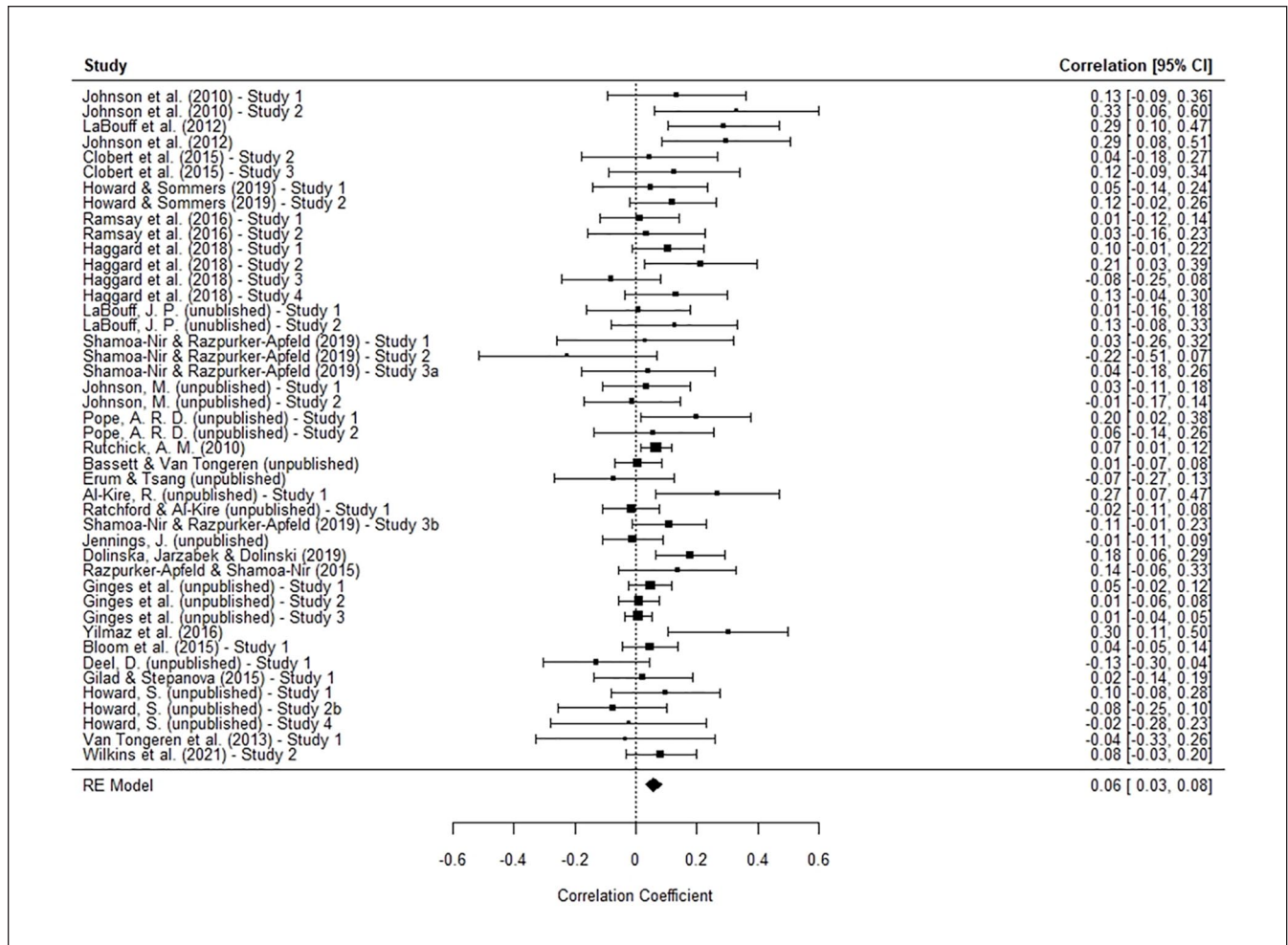


Figure 2. Forest Plot of Effect Sizes and Confidence Intervals.

Note. CI = confidence intervals.

data set to determine whether *p*-hacking is likely to have occurred (Simonsohn et al., 2014). When *p*-hacking is present, the distribution of *p*-values is left-skewed, with many *p*-values occurring around .04 and .05. The results of the present *p*-curve analysis suggested that our studies showed good evidential value. As seen in Figure 4, the distribution of *p*-values in this meta-analysis is skewed to the right, indicating that these studies detected a true effect ($Z = -2.46, p = .007$). In addition, the distribution of *p*-values was not flatter

than would be expected with 33% power ($Z = .02, p = .507$). This analysis demonstrates it is unlikely that selective reporting or specification searching occurred in this literature (Simonsohn et al., 2014), but it is still possible that only articles producing significant results were submitted or accepted for publication.

The threat is that there are enough undetected nonsignificant effects that will nullify the results (the file drawer problem; Rosenthal & Rosnow, 2008). Although the question that

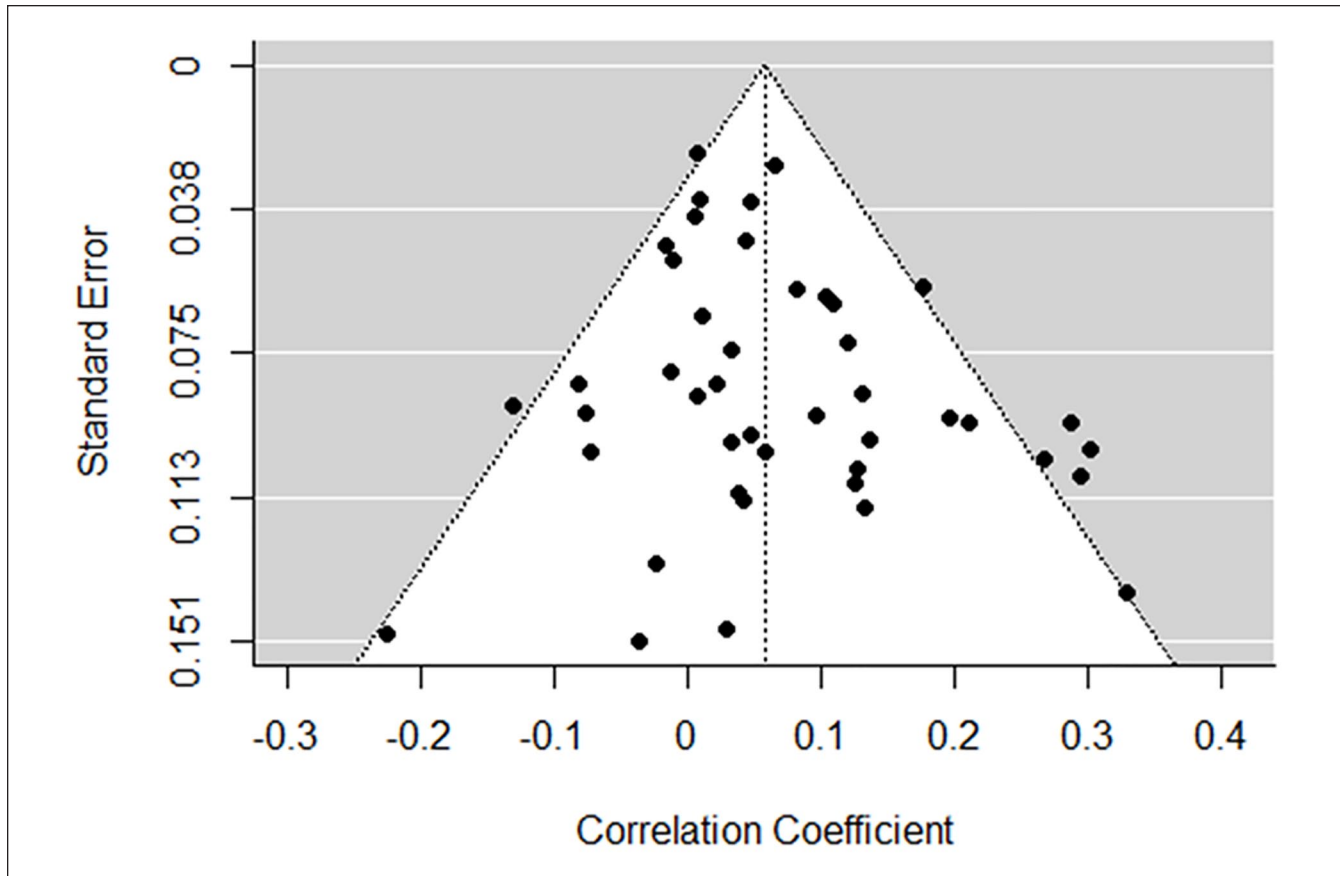


Figure 3. Funnel Plot for 44 Religious Priming Studies.

Table 2. Summary of Effects for Target Groups.

Target group	K	N	Estimates			
			<i>r</i>	95% CI	<i>Q</i>	<i>I</i> ²
LGBT	12	3,113	.12**	[.05, .19]	26.79*	67.53
Atheist	7	2,611	.09*	[.01, .17]	17.87	68.84

Note. CI = confidence interval.

* $p < .05$. ** $p < .01$.

the file drawer problem raises cannot be answered definitively, the threat it raises can be quantified. Using the procedure suggested by Rosenthal and Rosnow (2008), we found there must be 558 studies put away in file drawers that, if added to the sample of studies we analyzed, will produce an overall nonsignificant priming effect. We will comment on this result and the p -curve analysis further in the “Discussion” section.

Moderator Analyses

To test whether type of prime moderated the effect of religious priming, we entered a set of dummy codes to represent

the four types of primes: subliminal, contextual, implicit, and explicit. Studies using explicit primes were set as the reference group. Two studies were excluded from this analysis because they employed multiple priming methods. Table 3 presents the effect sizes (r estimate) and CIs associated with each priming method. None of the differences among the methods was significant: subliminal versus explicit, $b = .04$, $[-.02, .10]$, $p = .169$; contextual versus explicit, $b = .05$, $[-.02, .12]$, $p = .175$; implicit versus explicit, $b = -.01$, $[-.07, .05]$, $p = .745$. A power analysis indicated that power to detect a significant moderator effect was .71, which is slightly lower than the convention of .80. Thus, it is possible

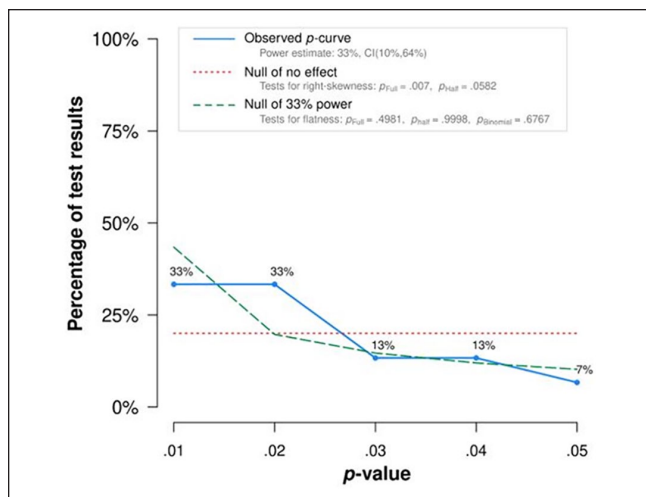


Figure 4. P-Curve Analysis.

Note: Significant right skew ($p = .007$) indicates that findings reflect the evidentiary value of the effect of religious priming on prejudice and are likely not a product of p -hacking. CI = confidence intervals.

that the lack of significant differences could be due in part to the small number of studies available for the various comparisons.²

We also tested the moderating effect of study location, comparing studies conducted in the US to studies conducted outside the US. This effect was small but significant, $b = .07$, 95% CI [.01, .13], $p = .026$, and power to detect this effect was .76. The relationship between religious priming and prejudice was significant for both U.S.-based studies, $r = .05$, (.02, .08), $p = .003$, and studies outside the United States, $r = .12$, (.06, .17), $p < .001$, but the effect was smaller for the former. More research is needed to assess whether this effect persists in a variety of cultural and religious contexts.

Since moderators of meta-analyses are not independent, we examined their interrelations. Inspection of the correlation matrix of study variables (see Table 4) showed that published studies were more likely to use self-reports, $r = .49$, (.20, .69), $p = .002$, less likely to use atheist or LGBT groups as targets, $r = -.33$, (-.57, -.04), $p = .028$, and more likely to be conducted outside the United States, $r = .56$, (.30, .74), $p < .001$. Therefore, we retested the effect of publication status, controlling for the three variables with which it was related. In this analysis, the difference between published and unpublished studies was no longer significant, $b = .04$ [-.03, .12], $p = .28$.

Discussion

The overall effect of religious priming was significant but small in magnitude ($r = .06$, $p < .001$). Nevertheless, as Funder and Ozer (2019) note, even small effects can have a big impact when widespread or over long periods of time.

Thus, an effect size of .06 is consequential despite being small, due to the widespread harm that prejudice exerts in society at large.

However, this result appears dubious as the effect for unpublished studies was not significant. Since it is conceivable that we failed to obtain all unpublished research with null results, this aspect of the findings raises doubts about the existence of an overall priming effect. Three different procedures served to disperse some of these doubts.

First, the results of a p -curve analysis suggest that the set of studies in the meta-analysis have evidential value. This addresses the concern that the effect emerged because of data mining (Simonsohn et al., 2014). Second, we found there must be an additional 558 unpublished studies with null results that, if added to the sample of studies we analyzed, will produce an overall nonsignificant priming effect. We believe it is not likely that so many studies exist. Third, the moderator effect of publication status could be due to the association between this moderator and other characteristics of the studies. Consistent with this notion, we found that the moderator effect of publication status was no longer significant when controlling for prejudice measure, target group, and location of study. However, because this analysis is correlational in nature, other alternative explanations may account for the result. Chief among them is a “third variable” interpretation that includes two elements: (a) other aspects of design or procedure that we did not code led to null results and, therefore, to the study’s unpublished status, and (b) these other variables were also related to the three covariates we used in the moderator analysis. Both this alternative and the original explanation assume that the causal sequence is from some characteristic of design or procedure to null results and from null results to the study’s unpublished status (either because of the authors’ decision not to submit the paper or because of editorial rejection). This assumption and the reasoning that builds upon it do not fully rule out other possible explanations. Nevertheless, the results of the moderator analysis do offer candidate variables that might determine when priming works and when it does not. This opens the door to follow-up research and theoretical expansion, which we think is a key value of these results. In summary, we believe that the three findings reviewed above minimize the likelihood that the failure of unpublished studies to yield a significant effect is evidence religious priming does not impact prejudice.

Both LGBT and atheist target groups elicited significant religious priming effects. This is consistent with previous research that has shown that LGBT people and atheists may be particularly vulnerable targets of prejudice. Hone et al. (2020) found that unconventional sexuality draws particular condemnation from religious people. These authors suggested that religious people put a sex premium on moral judgments of behaviors that might undermine their profamily communities. Blogowska et al. (2013) found that religious participants directed behavioral

Table 3. Summary of Effects for Priming Methods.

Priming method	Estimates					
	<i>k</i>	<i>N</i>	<i>r</i>	95% CI	<i>Q</i>	<i>I</i> ²
Subliminal	16	1,659	.08**	[.03, .13]	20.08	12.90
Contextual	4	1,777	.11*	[.00, .21]	6.11	53.00
Implicit	12	2,285	.03	[-.03, .09]	19.36	41.61
Explicit	10	5,248	.03*	[.00, .06]	11.56	0.01

Note. CI = confidence interval.

p* < .05. *p* < .01.

Table 4. Summary of Correlations Between Study Variables.

Variable	1	2	3	4	5	6	7
1. Published	1						
2. Subliminal	.09 (42)	1					
3. Contextual	-.05 (42)	-.26* (42)	1				
4. Implicit	.12 (42)	-.50** (42)	-.21 (42)	1			
5. Explicit	-.19 (42)	-.44** (42)	-.18 (42)	-.35* (42)	1		
6. Self-report	.49** (40)	-.01 (38)	-.03 (38)	.21 (38)	-.21 (38)	1	
7. LGBT/atheist	-.33* (44)	-.21 (44)	.41** (42)	-.17 (42)	.14 (42)	-.39* (41)	1
8. International	.56** (39)	-.10 (39)	-.06 (37)	.38* (37)	-.31 (37)	.30 (35)	-.34* (39)

Note. The number of studies for each correlation is provided in parentheses following the correlation coefficient. Coding for subliminal: 1 = subliminal, 0 = not subliminal; for contextual, 1 = contextual, 0 = not contextual; for implicit, 1 = implicit, 0 = not implicit; for explicit, 1 = explicit, 0 = not explicit; for published, 1 = published, 0 = not published; for prejudice measure: 1 = self-report, 0 = behavioral; LGBT/atheist target: 1 = study included LGBT/atheist target, 0 = non-LGBT or atheist target.

p* < .05. *p* < .01.

aggression at a gay target but not at a neutral target. Gervais et al. (2017) found global evidence of moral prejudice directed at atheists. Consistent with the present results, it appears that religion allows or even encourages treating LGBT people and atheists as outsiders. However, due to the low number of studies with certain groups, we were unable to establish whether LGBT and atheist groups are significantly more likely to be the targets of prejudice after religious priming compared with other groups. Likewise, low power also prevented adequate testing of religious priming effects on individual targets beyond LGBT and atheist groups. Clearly, we need additional research that will include a large range of targets, allowing adequately powered tests of moderation by target group.

Due to low power, the question of whether religiosity moderates religious priming effects was not tested. The literature suggests, however, that reminders of religion may be impactful even for those who do not hold religious views. As Norris and Inglehart (2011) note, the legacy of religious traditions continues to shape worldviews even in highly secular societies (p. 17). Nonreligious people are well-versed in the meaning and implication of religious cues regarding outgroups. Increasing the salience of these cues may thus increase the salience of their implications for religious and nonreligious participants alike. Consistent with this

observation, Gervais et al. (2017) found that atheists partake in the global moral condemnation of atheists. In several different studies, Laurin et al. (2012) reported that religious priming effects were found for both religious and nonreligious participants. This is also consistent with the finding that exposure to stereotypes influences behavior even among those who do not endorse those stereotypes (Bargh et al., 1996). Mere knowledge of the meaning or connotations of cultural representations might be sufficient to influence a person's attitude when those representations are made salient. Future research should assess whether nonreligious or people low in religiosity are also influenced by religious cues.

The results of this meta-analysis have implications for research design in this field. For example, the overall effect of priming on prejudice is small, which indicates that many studies on religious priming may be underpowered. In addition, priming as a methodology has been quite controversial, with some researchers voicing concerns about the reliability of the results of priming studies in general (e.g., Doyen et al., 2012). In their critique of religious priming, Watanabe and Laurent (2020) note that religious priming studies have demonstrated difficulties in producing reliable priming effects. The present meta-analysis demonstrates that priming as a methodology can be effective but is likely to produce only small effects and thus, studies that wish to use priming must

recruit large samples. We suspect that this conclusion might apply to the effects of priming in other domains.

We did not find that the type of prime significantly moderated the relationship between religious priming and prejudice. It should be noted that Shariff et al. (2016) found effect sizes for each type of prime that were roughly comparable (although they did not formally test type of prime as a moderator variable). It is possible then that each priming method may be effective to the same degree. However, the power for this test was somewhat low (.71) and, thus, there is a chance that we failed to find an effect that does exist.

Limitations

We focused our analysis on studies of monotheistic religions that primed participants with their own religion. Priming religion in other religious contexts might have a different overall effect. Buddhist primes have been found to decrease prejudice toward ethnic and religious targets, although this effect is less consistent for atheist and sexual minority targets (Clobert et al., 2015; Ramsay et al., 2014). Clobert et al. (2015) suggests that it is possible that some of the potential drivers of prejudice in Western religions (e.g., dogmatism and rigidity) are attenuated within Eastern religions, such as Buddhism. Since we identified only two usable studies that used Eastern religion primes, we decided not to include them in the current analysis.

We also identified research that utilized cross-religious priming (priming participants with religious concepts or ideas from a religion that is different from the participant's own religion). Although studies that examine cross-religious priming were not appropriate for inclusion in the present work, they can provide additional insight. For example, as Shamo-Nir and Razpurker-Apfeld (2019a) hypothesized, religious primes of one's own religion may have a stronger impact than other-religious primes, as same-religion primes are more self-relevant.

As noted earlier, we were unable to assess whether religiosity or religious identification influences the relationship between religious priming and prejudice as too few studies reported this relationship. Future work should seek to assess the effects of participants' quest orientation, intrinsic religiosity, and extrinsic religiosity as possible moderators of priming effects.

Furthermore, it is important to note that although we examined the causal relation of religion with prejudice, we did not examine bidirectionality. Our finding that religious priming increases prejudice does not preclude the possibility of the reverse relation. For example, prejudice might move people to seek and accept a value system (such as religion) that is consistent with their beliefs. Thus, future research could benefit from experimental studies to understand whether the reverse relation also exists.

Conclusion

Integrating across experimental designs, the current meta-analysis identified a causal relation between religious salience and prejudice. Although the priming effect was not significant for unpublished studies, we found publication status was no longer a significant moderator when controlling for type of prejudice measure, target group, and location of study. This, along with our *p*-curve analysis and the determination that a rather large number of unpublished studies is required to nullify the overall effect, suggests that religious priming has a real effect on prejudice. Critically, the effect is small and applies to two target groups, LGBT and atheists. Religious leaders and socializers should actively combat prejudice toward these groups by challenging negative attitudes toward LGBT people and atheists.

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) received no financial support for the research, authorship, and/or publication of this article.

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Supplemental Material

Supplemental material is available online with this article.

Notes

1. The results of power analyses showed less than .50 power for women, Black, Muslim, and Jewish target groups. Low power can make it difficult to draw conclusions about the meaning of nonsignificant effects, as effects may be nonsignificant either because of a true lack of effect or due to low power. Thus, following the recommendations of reviewers, we decided to provide these results in supplementary materials at the OSF link given previously.
2. We intended to also test the moderating effects of type of prejudice measure and target group. However, power analyses indicated that the power to detect a significant effect for these moderation analyses was low (less than .40). We also planned to test the moderating effect of religiosity, yet only a small number of studies provided information sufficient for calculating the effect size for this interaction. These analyses are provided in Supplementary Materials at the OSF link given previously. We additionally tested demographic moderators (mean age, gender, ethnic composition of the participants, religious affiliation and date of study) but were unable to test the power of these analyses. None of these moderators produced significant results ($p > .09$).

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