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Religious Priming: A Meta-Analysis With a Focus on Prosociality

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Abstract

Priming has emerged as a valuable tool within the psychological study of religion, allowing for tests of religion's causal effect on a number of psychological outcomes, such as prosocial behavior. As the literature has grown, questions about the reliability and boundary conditions of religious priming have arisen. We use a combination of traditional effect-size analyses, *p*-curve analyses, and adjustments for publication bias to evaluate the robustness of four types of religious priming (Analyses 1-3), review the empirical evidence for religion's effect specifically on prosocial behavior (Analyses 4-5), and test whether religious-priming effects generalize to individuals who report little or no religiosity (Analyses 6-7). Results across 93 studies and 11,653 participants show that religious priming has robust effects across a variety of outcome measures—prosocial measures included. Religious priming does not, however, reliably affect non-religious participants—suggesting that priming depends on the cognitive activation of culturally transmitted religious beliefs.

Keywords

helping/prosocial behavior, morality, research methods, religion, priming

Religious beliefs and behaviors are ubiquitous features of human lives, and have received enduring research attention in psychology. Major figures in the field such as James (1902), Hall (1917), Freud (1927/1961), and Allport (1950) each struggled to explain the motivations for and consequences of religious beliefs, rituals, and teaching (see Emmons & Paloutzian, 2003, for a review). These early attempts, however, were limited in their scope and conclusions by the methodological tools available at the time. Recently, innovative and diverse methodologies have fueled a rapid growth in psychological research on religion. One such innovation is the use of priming techniques to address religion's causal role in human behavior and thinking. By testing hypotheses about the impact of religion using true experimental designs with random assignment, the technique has generated new insights into the effects of religious reminders on psychological constructs (e.g., Shariff & Norenzayan, 2007).

As this literature has grown, open questions have emerged about important conceptual and methodological issues: Does religious priming reliably alter psychological phenomena, such as prosocial behavior? What types of priming techniques produce the strongest and most reliable results? Do religious primes affect the attitudes and behavior of non-believers who report little or no religious beliefs or commitments? And, in light of recent failures to replicate seminal priming studies, is the religious-priming literature robust to concerns about publication bias and questionable research practices (QRPs)?

Given these questions, the time is ripe for a systematic review and analysis of the religious-priming literature. This review has three goals:

1. Evaluate the validity of the evidence for religious-priming effects in psychology, and test their robustness against methodological artifacts and QRPs, such as publication bias and *p* hacking.
2. Review the experimental evidence in particular for the hypothesis that religious reminders have prosocial effects (i.e., Religious Prosociality).
3. Probe boundary conditions of religious priming, in particular, whether effects generalize to non-believers who report little or no explicit religious belief or commitment.

We address these goals with a series of meta-analyses but first provide a more detailed discussion of the theory, utility, and methods of religious priming and of recent debates about religious prosociality.

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Religious Priming

The experimental use of priming in social psychology refers to a technique whereby the presentation of one stimulus passively and temporarily affects processing or response, often in a different domain (Bargh, Chen, & Burrows, 1996). Although it is not necessary that priming stimuli are perceived outside of awareness, it is a necessary characteristic of this form of priming that the individual is either unaware of the influence of the priming stimulus on its measured response, or that this influence is unintended. The theoretical origins of priming are rooted in mid-20th century information-processing models (Hebb, 1949) and suggest that the cognitive accessibility of a concept affects how subsequent information is encoded, even if that information derives from an unrelated context. The precise mechanisms underlying priming effects remain a topic of debate (for an extended discussion, see Dijksterhuis & Bargh, 2001; Molden, 2014).

The experimental psychology of religion has turned to priming techniques to address limitations inherent in existing correlational and quasi-experimental designs. By directly manipulating the salience of religious thinking in the moment, and thereby allowing random assignment to different groups in which aspects of religion are or are not made salient, religious priming provides a powerful tool to test the causal effects of religious thinking on theoretically relevant psychological outcomes, and disentangle them from myriad other characteristics that covary with a "religious disposition," such as personality dimensions, demographic background, and political orientation. Religious-priming studies have probed a wide range of core psychological topics such as social desirability and prejudice; neural signals such as error-related negativity; and overt behaviors such as goal pursuit, temptation resistance, generosity, and cooperation.

Researchers have attempted to prime religion using a host of methods. These attempts can be categorized into four broad types—attempts to *explicitly* prime religion, *implicitly* prime religion, *subliminally* prime religion, and *contextually* prime religion. Each form of priming has its strengths and limitations.

Explicit primes, which are actively perceived and consciously processed by the participant, activate more specific and complex constructs than can subliminal and implicit primes, but are more vulnerable to demand characteristics. In an explicit priming situation, participants may be randomly assigned to answer a series of questions about their religious beliefs either before or after the dependent measure (e.g., Ginges, Hansen, & Norenzayan, 2009; Schumann, McGregor, Nash, & Ross, 2014). Other studies have had participants read over Biblical passages (e.g., Carpenter & Marshall, 2009). In these attempts to explicitly prime religious concepts, there are no efforts made to disguise the religious nature of the stimulus. Therefore, participants' explicit knowledge of the study's religious content may introduce suspicions.

Researchers attempt to prime religious concepts implicitly and even subliminally to minimize this possibility. Using a sentence-unscrambling task (Srull & Wyer, 1979) is one strategy for implicitly priming religious concepts (e.g., Shariff & Norenzayan, 2007). In this task, participants are exposed to a number of embedded keywords (e.g., *divine, sacred*) in a set of five-word sentence scrambles. Together, the keywords form a cognitive concept to which, it is argued, the participant has no explicit awareness. That is, although the participant's exposure to the religious stimuli was fully supraliminal, she does not explicitly realize that she has been led to think about religion. In funneled debriefing questions, very few participants report being aware that they were exposed to religion-related stimuli (see, for example, Ahmed & Hammarstedt, 2011; Rounding, Lee, Jacobson, & Ji, 2012).

Researchers aiming to prime religious concepts subliminally do so by presenting the religious stimuli in a manner in which participants are unlikely to consciously recognize being exposed to anything at all. One commonly used task has embedded rapidly flashed religious words in a Lexical Decision Task (e.g., Johnson, Rowatt, & LaBouff, 2010; Saroglou, Corneille, & Van Cappellen, 2009). In this task, participants are instructed to attend to the center of a screen where they are presented with strings of letters and report whether the letter string represents an actual word. In between presentation of the letter strings, religious words are presented for very short ($<$40 ms) durations and bookended by forward and backward masks. Unlike in the attempts to prime religious concepts implicitly, wherein the relevant stimuli are supraliminally perceived but not explicitly recognized to be about religion, in subliminal priming attempts, participants report no awareness of even having seen the stimuli (see, for example, Dijksterhuis, Preston, Wegner, & Aarts, 2008; McKay, Efferson, Whitehouse, & Fehr, 2011; though see Pratte and Rouder, 2009, for an alternative interpretation of subliminal priming effects).

Although attempts at implicit and subliminal priming can provide more rigorous experimental control, they are confined to simpler constructs and limit generalization to the real world. To expand religious priming to more naturalistic situations, several researchers have used contextual settings to evoke religious concepts. Aveyard (2014) and Duhaime (2014) both used the audible Islamic call to prayer—either in the field or subtly reproduced in the lab—as an ecologically valid example of religious priming. Similarly, several researchers have conducted field studies in which experimental participants are tested inside or in view of religious buildings (LaBouff, Rowatt, Johnson, & Finkle, 2012; Rutchick, 2010; Sagioglou & Forstmann, 2013; Xygalatas, 2012). Although these primes involve supraliminal stimuli, they need not be overt. Clever contextual priming designs, although challenging to conduct, can thus be very powerful by closely simulating real-world religious reminders, while still maintaining experimental control, and minimizing demand characteristics. In Analysis 2, we examine the relative effects of religious priming using each of these techniques.

Religious Prosociality

Some of the earliest religious-priming studies tested the effect of priming on *prosocial behavior*—a constellation of constructs dealing with normative actions that benefit others (see Norenzayan & Shariff, 2008 for a brief review). This was an opportune topic for the use of priming techniques for two reasons. First, whether religion causes people to act more prosocially has been a question of key interest to social scientists, philosophers, and the broader public (e.g., Durkheim, 1912/2001; Voltaire, 1825), especially in the evolutionary study of religion (e.g., Bering, 2011; Bulbulia 2007; Norenzayan, 2013, Sosis & Ruffle, 2003). Second, existing correlational techniques were ill-equipped to draw firm conclusions about causation.

Prosocial behavior tends to have multifactorial causes and be especially prone to socially desirable responding. Thus, correlational studies linking religion with prosocial behavior left numerous alternative explanations open. For example, it is conceivable that a prosocial disposition may cause people to become religious, or that third variables, such as compassion or guilt, may increase both religiosity and prosociality (Norenzayan & Shariff, 2008). In addition, socially desirable responding—shown to be higher among the religious (Sedikides & Gebauer, 2010)—could account for any positive relationship between self-reports of religiosity and prosocial behavior. Furthermore, given the widespread stereotype in majority religious societies of religious people being nicer and more cooperative than the non-religious (Gervais, Shariff, & Norenzayan, 2011), even peer reports of the prosociality of religious versus non-religious individuals may be confounded by religion's halo effect (Galen, 2012). Priming techniques address these issues by introducing an experimental manipulation that could establish causation and also mask the hypotheses of the study behind implicit or subliminal primes. In Analyses 4 and 5, we provide meta-analyses of the results of 25 published and unpublished studies that have used religious priming to test different aspects of the religious prosociality hypothesis.

Methodological Concerns

Recently, two large-scale failures to replicate highly cited unconscious priming studies have raised questions about the robustness of priming effects in social psychology (Doyen, Klein, Pichon, & Cleeremans, 2012; Shanks et al., 2013; see, for example, Dijksterhuis, van Knippenberg, & Holland, 2014; Newell & Shanks, 2014; Pashler, Coburn, & Harris, 2012, for debates on this issue). Concurrently, broader concerns have emerged over *p hacking*—sets of QRPs in data collection and analysis that artificially increase the likelihood that effects will reach statistical significance (Simmons, Nelson, & Simonsohn, 2011). Coupled with the perennial issue of publication bias caused by file-drawered null findings, the danger of pervasive *p* hacking is that it allows many more false-positive effects to enter the literature.

We welcome the need for additional scrutiny that raises the evidentiary bar in psychology, and in priming research in particular. We conducted these meta-analyses to address this challenge. The use of meta-analyses has been widely recommended as one of several important strategies for improving the integrity of psychological research (Asendorpf et al., 2013; Cumming, 2014; Cumming, Fidler, Kalinowski, & Lai, 2012). We have complemented traditional effect-size analyses with two additional tools: an estimation of possible publication bias and *p-curve* analyses. Publication bias is a well-known problem, and estimating and correcting for it can improve our confidence in the scientific literature. *P* curve was created by Simonsohn, Nelson, and Simmons (2014)—who initially raised concerns about *p* hacking—to test whether a set of statistically significant effects likely represent true findings, or false positives teased to statistical significance using QRPs and selective reporting. Further details about all our analyses, including *p* curve, are reported in the next section.

Overview of the Meta-Analyses

Three questions were addressed with seven analyses:

1. *Does religious priming have reliable psychological effects?* We conducted an effect-size meta-analysis on all qualifying religious-priming studies to evaluate the evidentiary value and effect size (Analysis 1), as well as subsidiary analyses to investigate important potential moderators of priming effects such as priming type and experimental setting (Analysis 2). In addition, we conducted a *p*-curve analysis on eligible studies to test whether this literature has evidentiary value or is merely an artifact of *p* hacking (Analysis 3).
2. *Does religious priming cause people to engage in prosocial behavior?* An effect-size meta-analysis (Analysis 4) and *p*-curve analysis (Analysis 5) were conducted to evaluate the impact of religious priming specifically on prosocial behavior.
3. *Do religious-priming effects depend on preexisting dispositional religious belief?* Separate effect-size analyses investigated whether priming effects are found among both believers and non-believers. Analyses were run for all priming studies (Analysis 6), and for the subset testing prosocial outcomes only (Analysis 7).

Question 1: Is the Effect of Religious Priming Robust?

Our first analysis sought to measure the effect sizes of existing work using religious priming, regardless of the dependent measure. Note that, for the purposes of testing religious priming's effectiveness, this is a particularly stringent test. A significant result in these studies depends not just on the priming being effective but also on the accuracy of the researchers' hypothesis about the prime's effect on the outcome measure. That is, the results of these studies indicate both that religious priming successfully activated thoughts about religious concepts *and* that those thoughts successfully affected the hypothesized psychological construct.

Analysis 1: Effect-Size Analysis

Method

Selection of studies and inclusion criteria. An initial overinclusive list of studies published or in press before November, 2014 was compiled by, first, searching *PsycINFO, Google Scholar*, and *Web of Knowledge* using keywords *relig*, god*, and *prim**; second, searching through *Google Scholar* lists of articles that cited the early priming articles but were not caught using the first method; third, posting an email on the Social Psychology Network (socialpsychology.org) listserv on January 29, 2013, requesting information from religious-priming studies, whether published or not, and whether they had positive or null findings; and finally, searching the reference sections of all these articles for any relevant but overlooked articles. This process allowed us to start with as comprehensive a list as possible.

From this list, we retained all studies that had (a) at least one group exposed to religious primes, that is, used subliminal, implicit, explicit, or contextual methods to manipulate religious salience; (b) a clear neutral condition with which primed groups could be compared; and (c) random assignment to these groups. These criteria produced a list of 94 priming studies (8 unpublished; Tables 1 and S1; see Appendix A for a list of excluded studies).

Quality ratings were not conducted. Although some have recommended weighting studies by quality assessment (Rosenthal, 1995), others have criticized its use for being unreliable and invalid (Herbison, Hay-Smith, & Gillespie, 2006; Juni, Witschi, Bloch, & Egger, 1999). Instead, objective indicators on which systematic quality judgments could be made, such as sample size and whether the study was conducted online or in person, are reported in Table S1. Sample reliability was also taken into account when making the trim and fill estimates. In addition, original means and standard deviations were used to correct statistical errors where possible. Finally, the use of random assignment was a necessary requirement for any study to be included in the analyses[1].

Effect sizes. All effects sizes were calculated as the standardized differences in dependent measures between religious prime and neutral-prime groups. Effect sizes were calculated directly from the means, standard deviations, and sample sizes for the experimental and control groups wherever possible. When not possible, effect sizes were computed from *F* ratios, *t*-tests, or zero-order correlation coefficients (Hullett & Levine, 2003). Effect sizes were calculated as Hedges' *g*, a standardized effect size that corrects for a slight positive bias in Cohen's *d* that is present in small samples (Hedges, 1981).

When religion-specific primes were used, religious groups were paired with their own religion's primes (e.g., Hindu participants with Hindu primes, Christians with Christian primes). Non-believers, however, were paired with the culturally dominant religious primes (i.e., Christian). For studies in which effects for multiple religious groups were reported, these different comparisons were weighted by sample size and averaged across all groups, including non-believers. For studies that had multiple religious-priming conditions but a single dependent variable, the effect size was calculated for each condition separately, and then averaged according to sample size (e.g., Ginges et al., 2009). Confidence intervals (CIs) were approximated based on standard errors and default assumptions of normality. No corrections for measurement error or range restriction were used, as reliability estimates were either not relevant or not included for a majority of our studies. Outliers were assessed, but none were found to have sufficient influence to warrant deletion in any analysis.

Two studies were excluded for having experimental cells that contained zero values. In Study 2 of Randolph-Seng and Nielsen (2007), there were zero cheaters among participants receiving a religious prime, and in Study 2 of Duhaime (2014), no participant receiving a religious prime elected to allocate money to themselves, rather than to a charity. Because calculations with zero value cells lead to infinite odds ratios, the studies were dropped from effect-size analyses, leaving Analysis 1 with a total of 92 studies ($n = 11{,}608$). However, in Appendix B, results are reported if these two studies are included with the adjustment of 0.5 being added to each cell before calculating the odds ratio (following Higgins, 2008; Littenberg & Moses, 1993). Including these two studies slightly increases the effect sizes.

Publication bias. Meta-analytic results were complemented by estimates made using the "trim and fill" method, to estimate studies that are missing due to publication bias. Duval and Tweedie (2000) developed "trim and fill," as a way to account for file-drawered studies, a perennial challenge of meta-analyses. Starting with a funnel plot, the method first removes ("trims") outlying studies on one side of the plot that have no corresponding study on the other side. After trimming, the new "true" center of the funnel plot is calculated. Finally, the previously trimmed studies are symmetrically added back in ("filled") on *both* sides of the plot's new center (see Figure 1). The newly filled studies on the opposite side of the plot represent estimates of those that were likely to be missing due to underreporting. At this point, a new effect size can be calculated that includes an estimated correction for publication bias.

Table 1. List of Studies in Meta-Analyses.

Authors	Year	Study	Priming method	Analysis: 1 and 2: Overall effect-size analysis	3: Overall *p* curve	4: Prosocial effect-size analysis	5: Prosocial *p* curve	6: Overall Believers vs. Non-believers	7: Prosocial Believers vs. Non-believers
Baldwin, Carrel, and Lopez	1990		Subliminal	*	*				
Shariff and Norenzayan	2007	Study 1	Implicit	*	*	*	*	*	*
		Study 2	Implicit	*	*	*	*	*	*
Randolph-Seng and Nielsen	2007	Study 1	Implicit	*	*	*	*		
		Study 2	Subliminal		*		*		
Pichon, Boccato, and Saroglou	2007	Study 1	Subliminal	*	*	*	*		
		Study 2	Implicit	*		*			
Dijksterhuis, Preston, Wegner, and Aarts	2008	Study 3	Subliminal	*	*			*	
Ginges, Hansen, and Norenzayan	2009		Explicit	*	*				
Saroglou, Corneille, and Cappellen	2009	Study 1	Subliminal	*					
		Study 2	Subliminal	*	*				
Carpenter and Marshall	2009		Explicit	*	*	*	*	*	*
Pichon and Saroglou	2009		Contextual	*	*	*			
Benjamin, Choi, and Fisher	2010	Study 1a	Implicit	*		*		*	*
		Study 1b	Implicit	*		*		*	*
Inzlicht and Tullet	2010	Study 1	Explicit	*	*			*	
		Study 2	Implicit	*	*				
Toburen and Meier	2010		Implicit	*	*				
Johnson, Rowatt, and LaBouff	2010	Study 1	Subliminal	*	*				
		Study 2	Subliminal	*	*				
Rutchick	2010	Study 4	Contextual	*					
		Study 5	Subliminal	*				*	
Horton, Rand, and Zeckhauser	2011		Explicit	*		*		*	*
Ahmed and Salas	2011	Study 1	Implicit	*	*	*	*	*	*
McKay, Efferson, Whitehouse, and Fehr	2011		Subliminal	*		*			
Ahmed and Hammarstedt	2011		Implicit	*	*	*	*		
Laurin, Kay, and Fitzsimons	2011	Study 1	Implicit	*	*				
		Study 2	Implicit	*	*				
		Study 3	Implicit	*	*				
		Study 4	Explicit	*	*				
		Study 5	Explicit	*	*				
		Study 6	Explicit	*	*				
LaBouff, Rowatt, Johnson, and Finkle	2011		Contextual	*	*				
Cappellen Corneille, Cols, and Saroglou	2011		Subliminal	*					
Tsang, Schulwitz, and Carlisle	2012		Implicit	*					
Gervais and Norenzayan	2012	Study 1	Explicit	*	*			*	
		Study 2	Implicit	*				*	
		Study 3	Implicit	*	*			*	
Rounding, Lee, Jacobson, and Ji	2012	Study 1	Implicit	*	*				
		Study 2	Implicit	*	*				
		Study 3	Implicit	*	*				
		Study 4	Implicit	*	*				
Xygalatas	2012		Contextual	*	*	*	*		
Hadnes and Schmacher	2012		Explicit	*	*	*	*		
Johnson, Rowatt, and LaBouff	2012	Study 2	Subliminal	*	*				

(continued)

Table 1. (continued)

Authors	Year	Study	Priming method	Analysis: 1 and 2: Overall effect-size analysis	3: Overall *p* curve	4: Prosocial effect-size analysis	5: Prosocial *p* curve	6: Overall Believers vs. Non-believers	7: Prosocial Believers vs. Non-believers
Granqvist, Mikulincer, Gewirtz, and Shaver	2012	Study 3	Subliminal	*	*				
		Study 4	Subliminal	*					
McCollough, Carter, DeWall, and Corrales	2012	Study 1	Explicit	*	*				
		Study 3	Implicit	*	*				
Rand et al.	2013	Study 2	Explicit	*		*		*	*
Preston and Ritter	2013	Study 2	Explicit	*	*	*	*		
	2013	Study 3	Subliminal	*	*	*	*		
Sasaki et al.	2013		Implicit	*	*	*	*		
Rodriguez, Neighbors, and Foster	2013		Explicit	*	*				
Sagioglou and Forstmann	2013	Study 1	Implicit	*	*				
		Study 2	Implicit	*					
		Study 3a	Implicit	*	*				
		Study 3b	Implicit	*	*				
		Study 4	Contextual	*	*				
Harrison and McKay	2013		Implicit	*					
Ramsey, Pang, Johnson-Shen, and Rowatt	2013		Subliminal	*	*				
Ahmed and Salas	2013		Contextual	*	*	*	*	*	*
Cohen, Mundry, and Kirschner	2014		Explicit	*		*			
Aveyard	2014	Study 1	Implicit	*		*			
		Study 2	Contextual	*	*	*	*		
Cavrak and Kleider-Offutt	2014	Study 2a	Subliminal	*	*				
		Study 2b	Subliminal	*	*				
Schumann, McGregor, Nash, and Ross	2014	Study 1	Explicit	*	*				
		Study 2	Explicit	*	*				
		Study 3	Explicit	*	*				
		Study 4	Explicit	*	*				
		Study 5	Explicit	*	*				
		Study 7	Explicit	*					
Chan, Tong, and Tan	2014	Study 1	Subliminal	*	*				
		Study 2	Implicit	*	*				
		Study 3	Implicit	*					
Fergus and Rowatt	2015		Implicit	*	*				
Kupor, Laurin, and Levav	in press	Study 1a	Implicit	*	*				
		Study 1b	Implicit	*	*				
		Study 1c	Implicit	*	*				
		Study 1d	Explicit	*	*				
		Study 2	Explicit	*	*				
		Study 3	Explicit	*	*				
		Study 4	Explicit	*	*				
		Study 5	Explicit	*	*				
Yilmaz and Bahcekapili	unpub	Study 2	Implicit	*					
Hurst	unpub		Implicit	*		*		*	*
Duhaime	unpub	Study 1	Contextual	*		*			
	unpub	Study 2	Contextual						
Purzycki, Baimel, McNamara, and Willard	unpub		Explicit	*		*		*	*
Willis	unpub		Implicit	*					
Gervais and Norenzayan	unpub		Implicit	*		*			
Carter	unpub		Implicit	*					
Hone	unpub		Implicit	*					

Note. See also Table S1 in the online supplemental materials for a more detailed overview of the included studies.

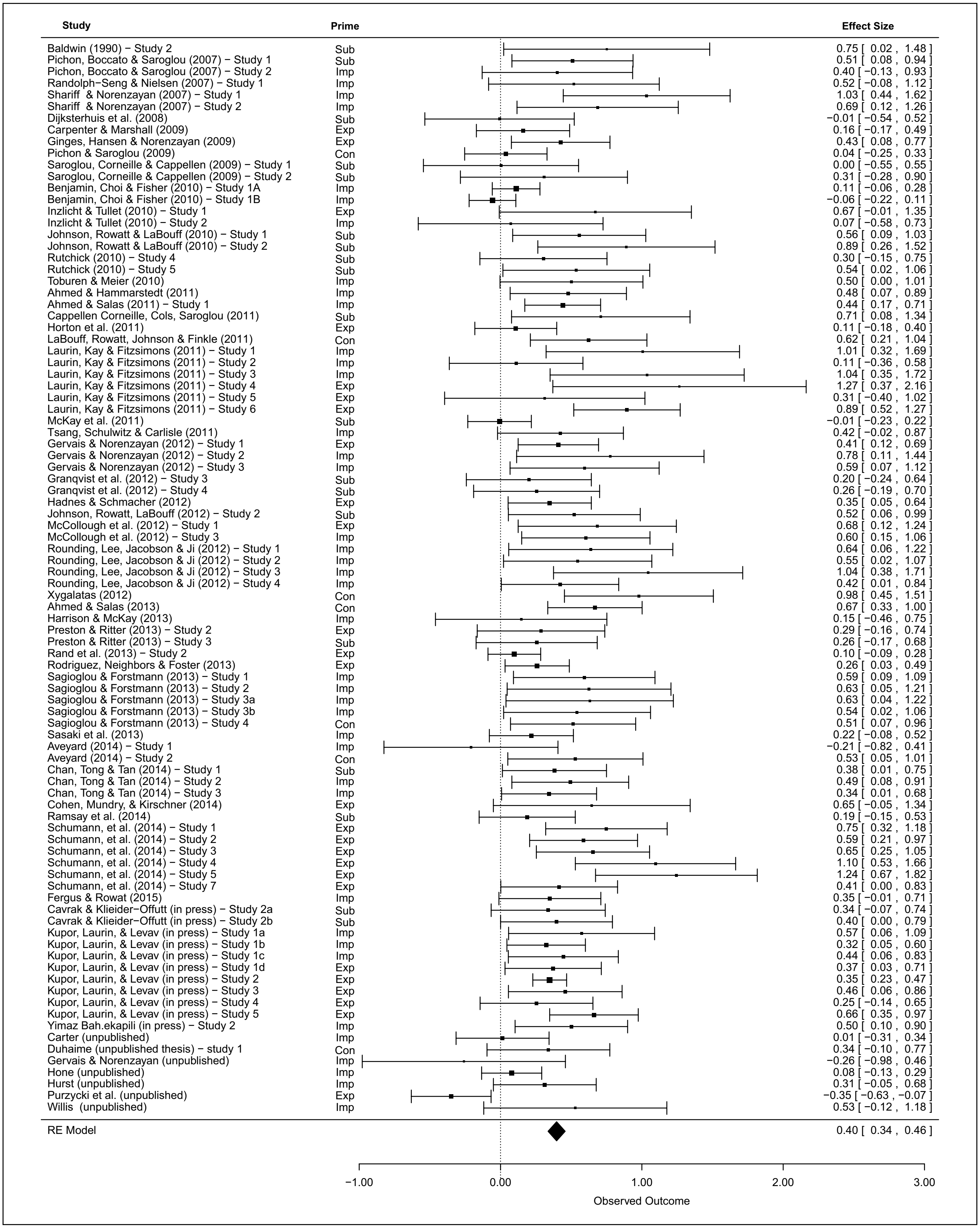

Figure 1. Forest plot of 92 religious-priming studies in Analysis 1, with effect sizes and confidence intervals.
Note. Mean effect size is $g = .40$, $p < .0001$. Imp, Exp, Sub, and Con denote whether implicit, explicit, subliminal, or contextual primes were used, respectively.

Table 2. Distribution and Effect Sizes of Studies Broken Down in Different Categories.

	k	*n*	Effect size	95% CI
Priming technique				
Implicit	40	5,190	0.39***	[0.29, 0.49]
Explicit	26	3,846	0.42***	[0.31, 0.53]
Subliminal	18	1,702	0.33***	[0.20, 0.48]
Contextual	8	870	0.49***	[0.28, 0.71]
Experimental Setting[a]				
Lab	55	5,336	0.40***	[0.32, 0.48]
Online	25	4,709	0.38***	[0.26, 0.49]
Mechanical turk	13	1,907	0.34***	[0.17, 0.52]
Not mechanical turk	12	2,802	0.41***	[0.27, 0.56]
Field	11	1,365	0.44***	[0.25, 0.62]
Dependent measure				
Self-report measures	42	5,152	0.46***	[0.37, 0.55]
Behavioral measures	50	6,456	0.34***	[0.26, 0.42]

Note. CI = confidence interval.
[a]Ginges, Hansen, and Norenzayan (2009) was the only study to be conducted via telephone and was not included in the Experimental Setting breakdown.
***$p < .001$.

Statistical package. R's metafor package (Viechtbauer, 2010) was used to calculate and analyze effect sizes.

Results and discussion. Given the diversity of outcome measures and prime types, some variation in the true effect was expected across these studies (e.g., Table 2). Due to this, a random-effects model was used. A Cochran's test revealed the studies to be significantly heterogeneous, $Q(df = 91) = 195.23$, $p < .0001$, supporting this choice. Random-effects models tend to be less vulnerable to Type 1 error than do fixed-effects models and are thus widely recommended (Cumming, 2014).

Across 92 studies, the average effect size of the difference between religiously primed and control groups was $g = 0.40$, $p < .0001$, 95% CI = [0.34, 0.46] (Figure 2), indicating a small to medium effect of religious priming across all tested DVs (Cohen's, [1992] convention of small [$d = 0.2$], medium [$d = 0.5$], and large [$d = 0.8$] effect sizes using Cohen's d is roughly interchangeable with effect sizes using Hedges' g). For this and all subsequent analyses, removing studies involving the current authors did not notably change the pattern of results (see Appendix C for these analyses).

Accounting for Publication Bias

Because this analysis tests for effects of religious priming on all hypothesized dependent measures, unreported studies are likely. The negative correlation between effect and sample size (a pattern found in 80% of meta-analyses, Levine, Asada, & Carpenter, 2009) further supports that suspicion.

The funnel plot of the current analysis was significantly asymmetric, $z = 5.96$, $p < .0001$. The trim and fill method was used to correct for this asymmetry by estimating missing studies, yielding an estimated effect size of $g = 0.29$, $p < .0001$, 95% CI = [0.22, 0.35] (see Duval & Tweedie, 2000). Although this figure is smaller than the unadjusted number above, suggesting the presence of some underreporting of null findings, a robust significant effect nevertheless emerges across the studies. On the basis of this estimate, then, the results should not be discounted as artifacts of publication bias.

Analysis 2: Subsidiary Analyses

In addition to the overall effect-size analysis, researchers may be curious about moderation effects or boundary conditions in the religious-priming literature. In Analysis 2, we have examined whether there are systematic differences in effects sizes between different priming types (e.g., subliminal, implicit, explicit, contextual), between different experimental settings (e.g., lab-based experiments, those run using Amazon's Mechanical Turk [MTurk]), and between broad categories of dependent measures (i.e., self-report and behavioral measures). Although there are not sufficient numbers of studies to determine definitive conclusions on these questions, the data may provide important preliminary information about the conditions under which religious priming works and does not.

Different priming types. For each study, two of the authors independently classified which of the four priming methods—explicit, implicit, subliminal, and contextual—the researchers were attempting to use to prime religion.[2] Interrater agreement was 95%. In the five cases of disagreement, a third author served as tiebreaker. The four types of primes vary considerably in frequency of use (Table 2). These small numbers limit the conclusions that can be drawn about the techniques' respective effectiveness. Nevertheless, a

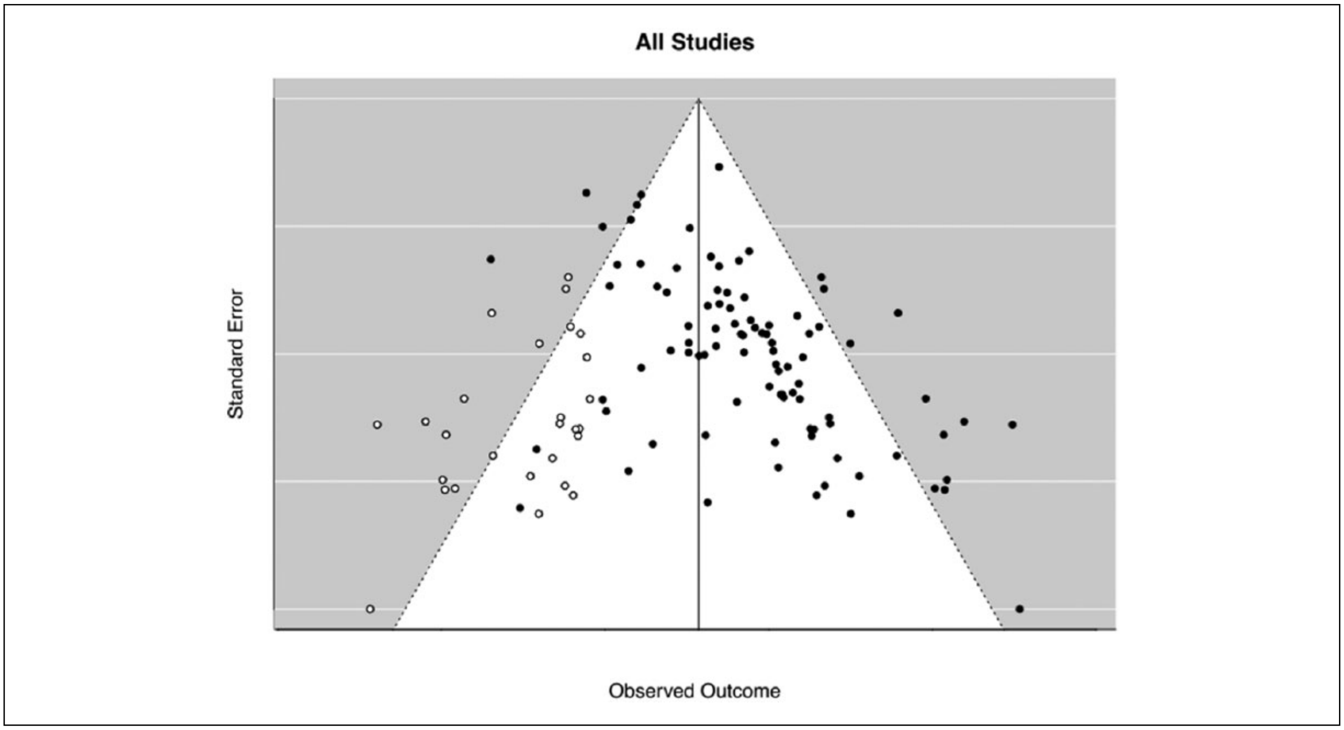

Figure 2. Trimmed and filled funnel plot for Analysis 1.
Note. 27 estimated missing studies added on the left side of the plot. Adjusted Hedges' $g = 0.29$, $p < .0001$.

mixed-effects model analysis found each type to be effective (Table 2). The strongest effects were found for studies using contextual primes—although with only eight studies, the result should be interpreted with caution.

Different experimental settings. We divided the studies based on where they had been conducted—whether in the researchers' laboratories, online through MTurk crowdsourcing marketplace, online through other means, or out in the field (e.g., in or in front of religious buildings). To investigate whether any setting was markedly better than any other, and, perhaps more importantly, to detect settings in which priming was *not* effective, we conducted a mixed-effect analysis on the studies in these different categories. As with the analysis of different priming types, each experimental setting was found to show a significant effect size, and there were no significant differences between them. It should be noted that the field study category in this comparison overlapped considerably with the contextual category of priming type.

MTurk has become a popular tool for running studies online. Supporting previous assessments (Horton, Rand, & Zeckhauser, 2011), these results suggest that it is a sound venue for conducting religious-priming experiments. Kupor, Laurin, and Levav (in press) whose studies account for 7 of the 13 MTurk studies in this analysis, used several strategies to obtain high quality and uncontaminated samples from the site. For example, they used Java programming to ensure no participant was involved in more than one of their studies to avoid cross-contamination, collected each study's full sample in a single day to minimize chatter on discussion boards hosted by MTurk workers, and removed studies at 5 p.m. Eastern Standard Time to minimize workers suffering from end-of-workday depletion.

Self-report versus behavioral dependent measures. Religious priming has been used to investigate the effect of religious concepts on myriad phenomena. Aside from the cluster of prosociality measures, which are tested in 25 studies and are analyzed on their own in Analyses 4, 5, and 7, there is no other dependent construct that has been tested across a sufficient number of studies to warrant its own analysis. Nonetheless, we did separately test those studies that used behavioral measures (e.g., economic games or task persistence) and those that used self-report measures (e.g., scales measuring submissiveness or thermometer ratings of outgroups). Both types of dependent measures saw robust effect sizes; however, effects were significantly smaller for behavioral measures ($g = 0.34$, $p < .0001$, 95% CI = [0.26, 0.42]) than for self-report measures ($g = 0.46$, $p < .0001$, 95% CI = [0.37, 0.55]), Cochran's Q-test for moderators (QM) ($df = 2$) = 169.06, $p < .0001$.

Analysis 3: P-Curve Analysis

As p curve is a new tool, we will briefly explain the logic behind the analysis. We refer interested readers to the original paper (Simonsohn et al., 2014) for more details. *P* curve analyzes the distribution of *p* values among published articles to distinguish whether the findings provide evidence for a true phenomenon, or whether they likely reflect an artifact of publication bias and *p* hacking. This reasoning is based on evidence that studies demonstrating true effects (where the null is false) will be more likely to produce particularly low *p* values ($ps < .025$) than will *p* values in the higher range of significance ($.025 < ps < .05$; Lehmann, 1986; Wallis, 1942). The distribution of *p* values (the "*p* curve") for a true effect should thus be right-skewed. Studies that are investigating null effects will produce an equal distribution of *p* values, resulting in a uniform *p* curve. This type of "flat" *p* curve suggests that the body of literature lacks evidentiary value. The use of QRPs to pull findings below the threshold of statistical significance—when there is no real effect—is more likely to produce *p* values in the upper range of significance (e.g., $.04 < ps < .05$). A set of studies overwhelmingly composed of *p*-hacked effects but that actually lacks evidentiary value will thus likely produce a left-skewed *p* curve. *P*-curve analysis thus tests the skew of the distribution of *p* values for a given set of studies and can detect evidential value even with a small number of underpowered studies. As recommended in Simonsohn et al. (2014), a detailed disclosure table reports the hypotheses, results, and *p* values of all included studies selected, as well as details on how any ambiguous decisions were resolved (Table S2 in online supplemental materials).

Selection of articles. *P* curve aims to test the distribution of *p* values in the full record of published research. As a result, the inclusion criteria for *p*-curve analyses exclude all unpublished studies and all studies that resulted in effects that did not reach $p < .05$. Because studies that result in *p* values more than .05 are only selectively reported, with some published and most file-drawered, they are not representative of a full distribution. However, we can be confident that studies with *p* values less than .05 will find their way into journals; thus, the publication record largely represents the full distribution. Eliminating unpublished and $p > .05$ studies[3] from our initial list in Analysis 1 left 66 studies. Adding Study 2 from Randolph-Seng and Nielsen (2007), which was excluded from Analysis 1 but is viable for *p* curve, brings this total to 67 studies ($N = 6,949$).

Selection of analyses. In addition to choosing studies, *p* curve also requires the selection of the specific *p* values that will be entered into the meta-analysis. Although the *p*-curve architects provide a set of selection criteria for which *p* values should be included (Simonsohn et al., 2014), they recognize that, as with any meta-analysis, ambiguous situations will require subjective judgments. Transparency for these judgments is achieved through two mechanisms—disclosure and secondary "Robustness Test" analyses. In terms of disclosure, a column in Table S2 in the online supplemental materials reports the decisions that were made in cases where the *p*-value selection was not obvious. In cases where there is a different value that, although not the primary choice, could have been alternatively included, this value is reported in the "Robustness Test" column. The Robustness Test is a second *p* curve, calculated alongside the main *p* curve, but with all the alternate *p* values replacing the primary ones (for those studies for which there are no alternate analyses, the primary analyses are retained). This second test provides confidence that the true *p* curve is robust to subjective decision rules.

To give an example of this selection process, several articles tested the interaction between religious priming and religious affiliation, predicting that priming would produce an effect for religious participants, but not for those identifying as non-religious (e.g., Carpenter & Marshall, 2009; Dijksterhuis et al., 2008). In these cases, the *p* value for the two-way interaction was included in the primary analysis, whereas the *p* value from the simple effect of priming on just religious participants was included in the Robustness Test. However, when authors predicted a main effect of priming, but made no specific prediction of the primes' effect on non-religious participants (e.g., Toburen & Meier, 2010), the *p* value from the main effect was included in the primary analysis.

When a single priming study had multiple DVs, including all these as separate *p* values would have overweighted the study in the overall *p*-curve analysis. As a result, following Simonsohn et al.'s (2014) recommendations, the *first* reported analysis was always included in the primary analysis. This occurred in five studies (Johnson, Rowatt, & LaBouff, 2012; LaBouff et al., 2012; Pichon & Saroglou, 2009; Ramsay, Pang, Shen, & Rowatt, 2014; Rodriguez, Neighbors, & Foster, 2013). In each case, an overall *p* value was calculated by combining all dependent measures to provide a "representative" analysis of the overall effect. This value was entered into the Robustness Test.

Finally, when multiple conditions (e.g., religious, control, and "secular"; McCullough, Carter, DeWall, & Corrales, 2012) were included, extraneous conditions were dropped and the *p* value for the differences of means between the religious condition and most neutral control condition was included in the *p*-curve analysis.

Again, Table S2 in the online supplemental materials contains notes on the specific decision rules used for each study with ambiguity.

Method. Test results for each relevant analysis (see Table S2 in the online supplemental materials) were entered into the *p-curve* web application (http://www.p-curve.com/app/). Exact *p* values were recalculated to five decimal places from the original test values (e.g., *t*-test, chi-square) or, where available, the reported means and standard deviations. When calculating new test values, and when specific sample sizes for individual conditions were not reported, equal sizes were assumed. Once all values were entered, the web app calculated the *p*-curve skew. *P* values were winsorized at .01 and

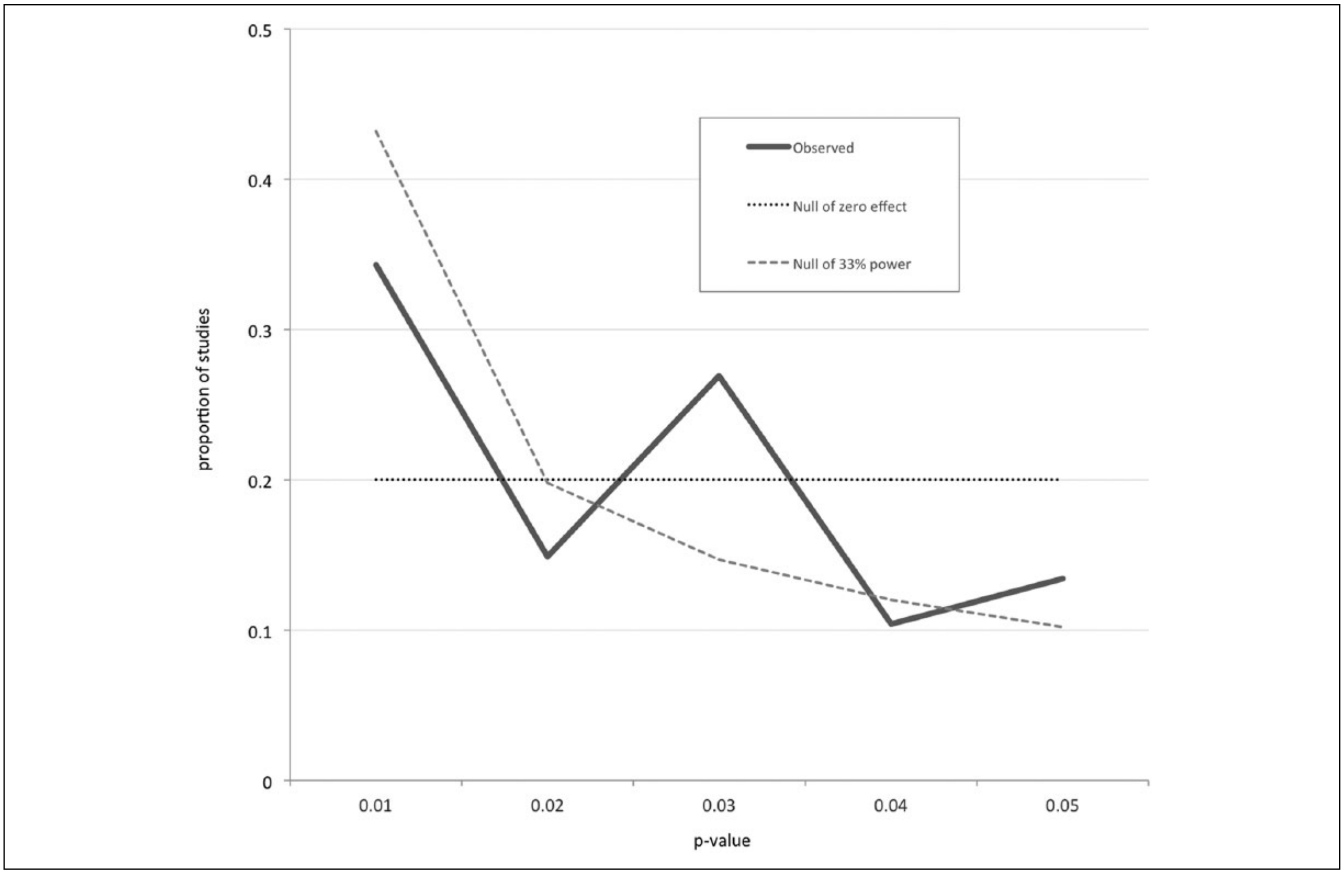

Figure 3. *P* curve for 67 religious-priming effects.
Note. Significant right skew (p = .0001) suggests that the body of results reflects evidentiary value for religious priming, and little evidence for extensive publication bias and p hacking.

.99 to insulate the analysis from outliers (not winsorizing does not notably change results for either Analysis 3 or 5).

Results and discussion. The resulting p curve was significantly right-skewed, $\chi^2(134) = 201.98, p = .0001$ (Figure 3), with 45 of 67 p values lower than $p = .025$. The Robustness Test's p curve was similarly right-skewed, $\chi^2(136) = 230.05, p < .0001$, with 43 of 68 p values lower than $p = .025$. These results suggest that the body of studies reflects a true effect of religious priming, and not an artifact of publication bias and p hacking.

Question 2: Does Religious Priming Cause Prosocial Behavior?

To evaluate the specific effect of religious priming on prosocial behavior, an effect-size meta-analysis and p-curve analysis were conducted on only those studies with prosocial outcomes. Prosociality was defined broadly to encompass measures related to ethical, cooperative, or generous behavior or attitudes. These included sharing resources in the dictator game, contributing to a common good in the public goods game, cooperating in the prisoner's dilemma, willingness to volunteer time and effort, and refraining from lying and cheating. Theoretically, these behaviors are unified insofar as they all defer the opportunity for immediate self-interest in lieu of actions that are beneficial to others—hypothesized to be one of the key effects of adherence to world religions (Norenzayan & Shariff, 2008).

Analysis 4: Effect-Size Analysis

Selection of articles. Starting with the list created in Analysis 1, the first and second authors independently selected those that had a prosocial outcome measure as their main dependent variable (agreement was 100%). This left 25 studies (n = 4,825).

Method. The analysis followed the same method and statistical procedure as Analysis 1. As in Analysis 1, a random-effects model was used due to theoretical considerations and significant heterogeneity, $Q(df = 24) = 74.42, p < .001$.

Results and discussion. Across 25 studies, the average effect size of the difference between religiously primed and control groups was $g = 0.27$, $p < .001$, 95% CI = [0.15, 0.40]

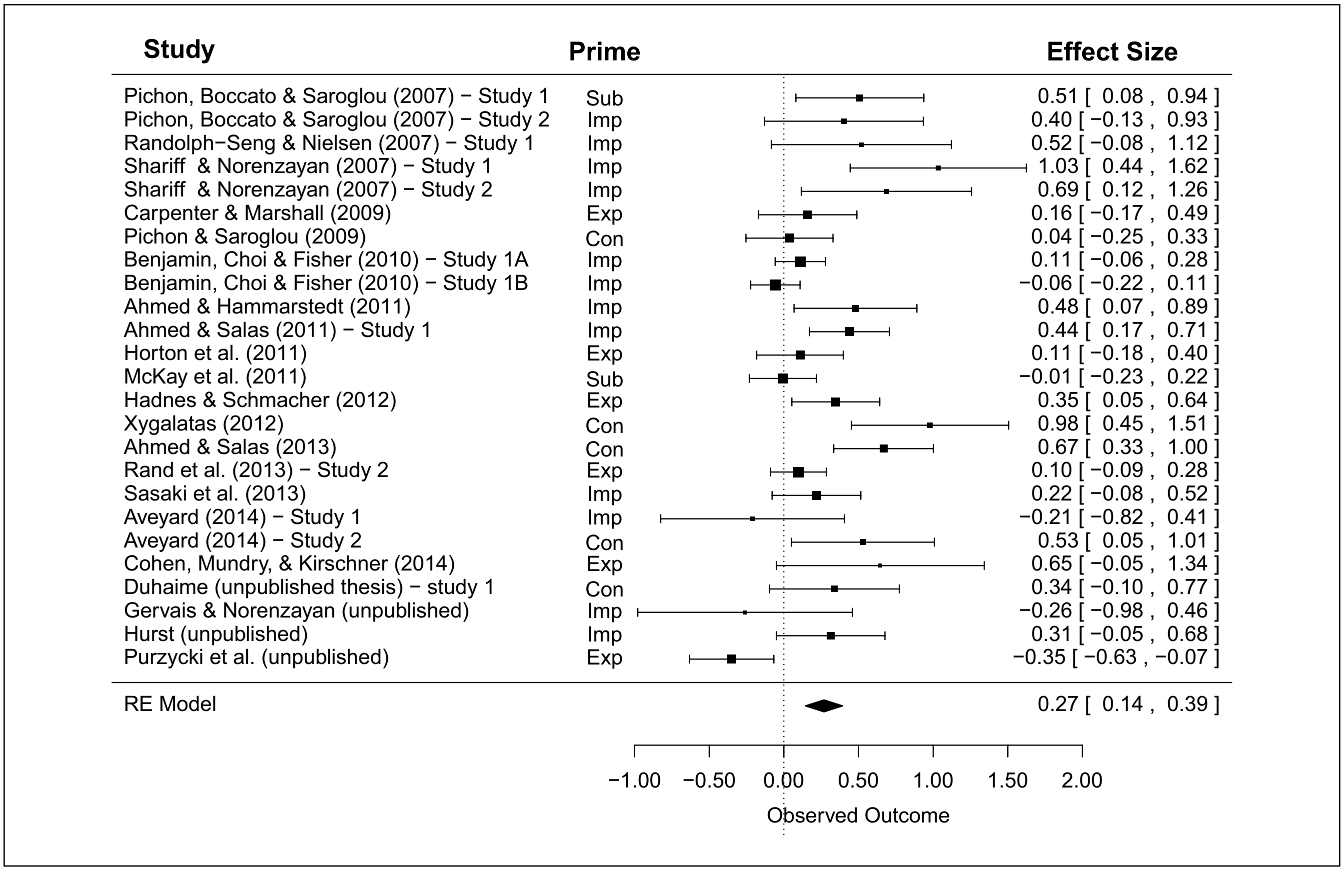

Figure 4. Forest plot of 25 religious prosociality studies, with effect sizes and confidence intervals.
Note. Mean effect size is $g = .27$, $p < .001$.

(Figure 4), indicating a small to moderate effect of religious priming on prosocial behaviors.

The resulting funnel plot was asymmetric ($z = 3.06$, $p < .001$; Figure 5). Correcting for the asymmetry using trim and fill produced a small but significant effect size of 0.18, $p = .001$, 95% CI = [0.04, 0.32].

Larger studies (Benjamin, Choi, & Fisher, 2010; Rand et al., 2013) had smaller effects, for at least two possible reasons. First, larger sample sizes may provide a more accurate indication of the actual effect, which may be overestimated by the smaller studies. Another explanation is that the smaller effects in these larger samples may result from their disproportionately large number of religious non-believers—a group who may be less affected by religious priming, if at all. Analyses 6 and 7 investigate this possibility, and find that when these three studies are broken down by believers versus non/low-believers, two show strong effects for the believers only. Indeed, when only participants denoted as "religious" or "of high religiosity" are included in the meta-analysis across all prosocial studies, the effect size is markedly increased (see Analysis 7).

Analysis 5: P-Curve Analysis

Selection of articles. In accordance with *p*-curve requirements, all unpublished studies and studies with *p* values exceeding .05 were removed from Analysis 5's list, leaving 16 studies ($n = 1,943$).

Method. The method for calculating the *p* curve was identical to that used in Analysis 3.

Results and discussion. The resulting *p* curve was significantly right-skewed, $\chi^2(32) = 79.94$, $p < .0001$ (see Figure 6), with the *p* values from 15 of 16 studies lower than $p = .025$. These results are inconsistent with the pattern expected with *p*-hacked literatures and suggest that the current set of results has evidentiary value.

Question 3: Does Religious Priming Depend on Preexisting Religious Belief?

Analyses 6 and 7—Effect Sizes for Believers and Non-Believers

Our next question of interest was to examine one potential and theoretically important boundary condition for religious-priming effects—whether they emerge regardless of participants' stated level of religious devotion, or whether the effects are confined to those who identify as religious believers. We tested this question for all studies with

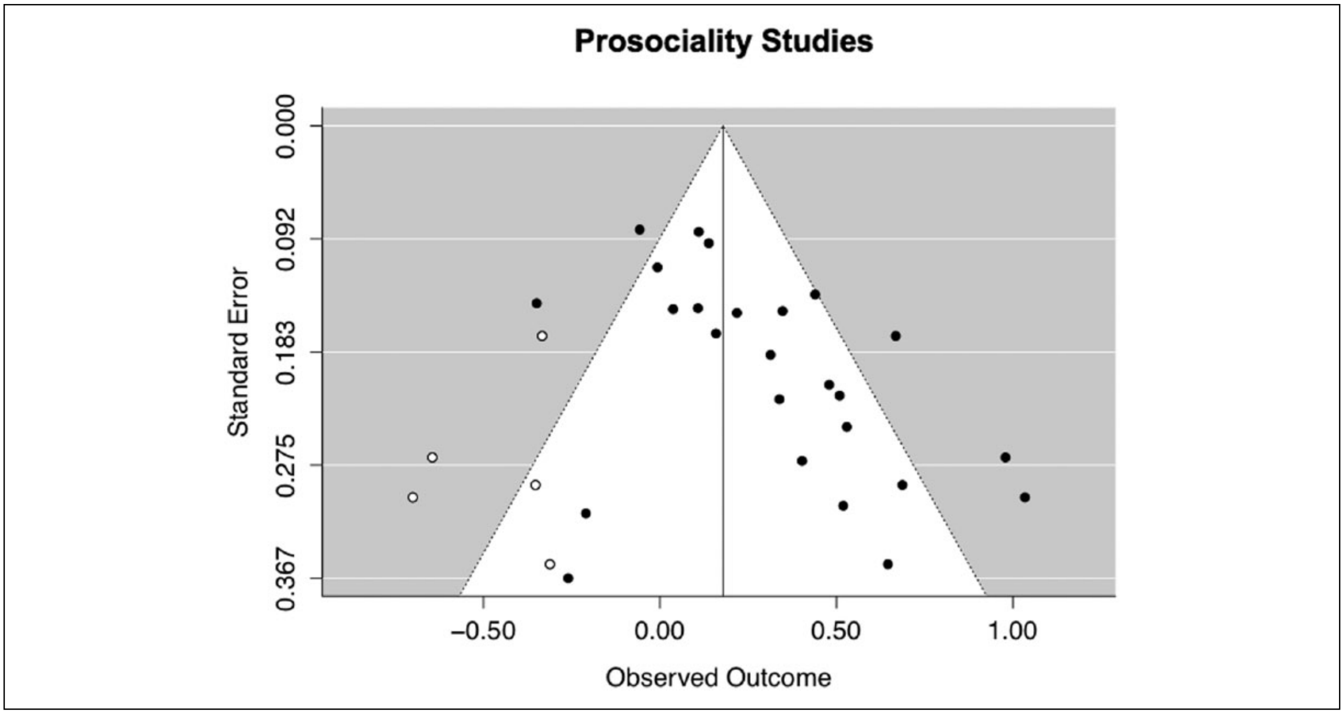

Figure 5. Trimmed and filled funnel plot for Analysis 4.
Note. 5 estimated missing studies added on the left side of the plot. Adjusted Hedges' $g = 0.18$, $p < .001$.

usable religiosity breakdowns (Analysis 6), and for the subset of studies with prosocial outcomes within these (Analysis 7).

Selection of articles. Starting with the list created in Analysis 1, we retained all studies ($k = 17$, $n = 4{,}038$) in which means were separately reported for "religious" or "high religiosity" participants, and for "non-religious" or "low-religious" participants. We used religion as a dichotomous, rather than continuous, measure because religiosity was variably and inconsistently measured across studies, making fine-grained analyses and interpretation difficult. Instead, we relied on the criteria that the studies' authors themselves used to make the divisions between the two groups. As a result, although we have used the broad terms *believers* and *non-believers*, the actual operational definition used by the various authors will differ. This necessary limitation adds noise to the comparisons between the two groups and thus likely underestimates actual differences. Alternatively, these results may be somewhat inflated by a reporting bias, a possibility that is discussed further below. For Analysis 7, we tested the subset of these studies that probed prosocial behaviors ($k = 11$, $n = 3{,}533$).

Method. Separate effect sizes were calculated for those identified as "religious" or "high religiosity" participants, and those "non-religious" or "low-religious" participants. Effect sizes were calculated using the same method as in Analysis 1. Because the studies showed high heterogeneity for all groups (Qs > 40, ps < .001), random-effects models were used.

Results

Analysis 6. First, the mean religious-priming effect size for the 17 studies included in this analysis, when including both the "religious/high religiosity" and "non-religious/low religiosity" together, was $g = 0.28$, $p = .001$, 95% CI = [0.12, 0.44]. Correcting for funnel plot asymmetry using trim and fill yields a mean effect size of $g = 0.21$, $p = .015$, 95% CI = [0.04, 0.37].

When only the "religious/high religiosity" participants are included, this effect increases to $g = 0.44$ ($p < .0001$, 95% CI = [0.24, 0.65]), indicating a medium effect size. After trim and fill estimates, the figure is $g = 0.27$, $p = .011$, 95% CI = [0.06, 0.47]. In contrast, the mean effect size for "non-religious/low religiosity" participants was 0.04 ($p = .71$, 95% CI = [−0.17, 0.24]). The effect sizes are significantly different, $z(17) = 3.07$, $p = .002$, 95% CI = [0.17, 0.75].

Analysis 7. The mean religious-priming effect size for the 11 prosocial studies included in this analysis, when including both the "religious/high religiosity" and "non-religious/low religiosity" together, was $g = 0.24$, $p = .02$, 95% CI = [0.04, 0.44]. The trim and fill procedure does not estimate any studies missing due to publication bias, and thus no correction is needed.

When analyzing just the "religious/high religiosity" participants, the effect size is 0.38 ($p = .002$, 95% CI = [0.14,

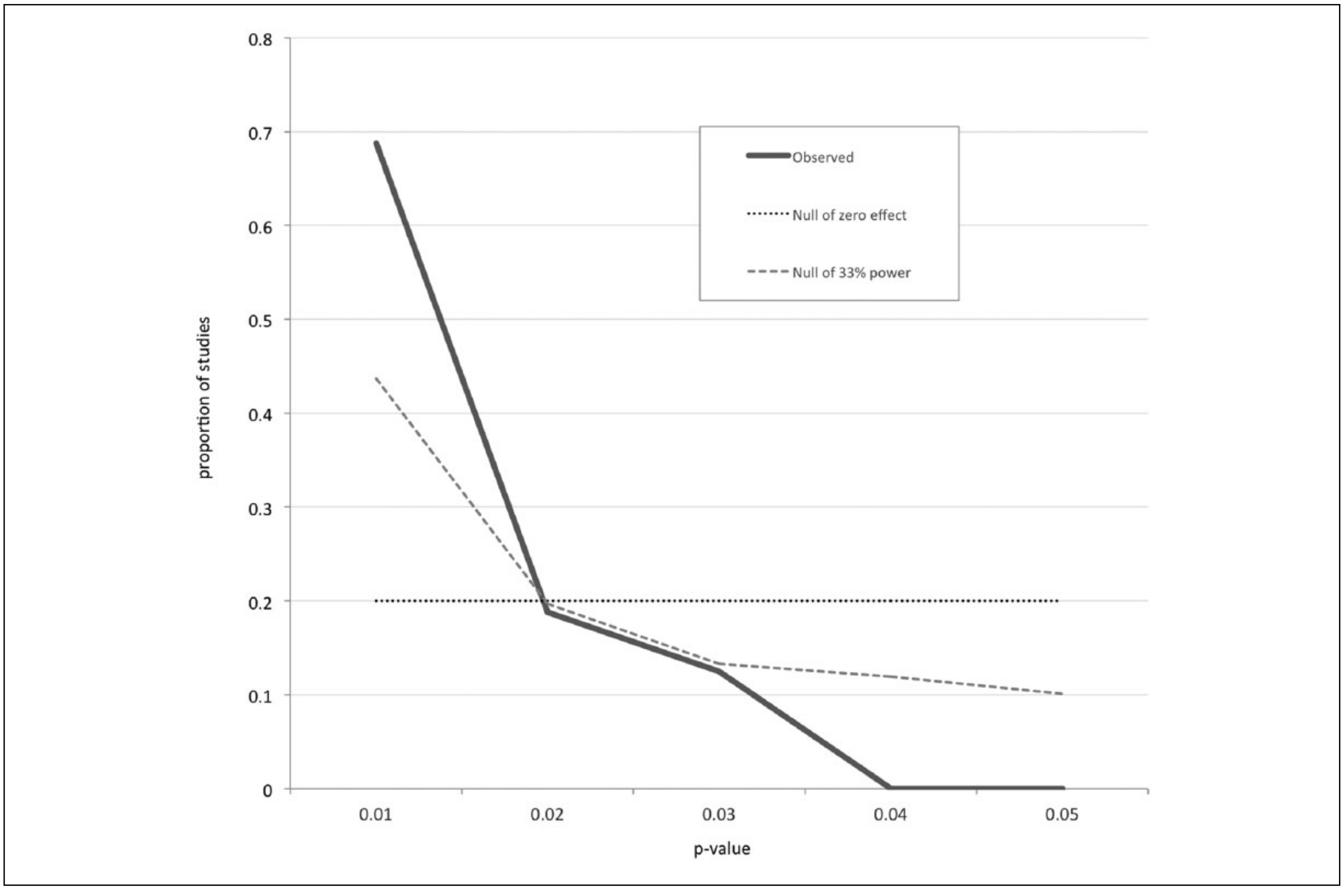

Figure 6. *P* curve for 16 religious-priming effects on prosocial behavior.
Note. Significant right skew ($p < .0001$) suggests that the body of results reflects evidentiary value for religious priming, and little evidence for extensive publication bias and *p* hacking.

0.62]), with a trim and fill estimate of 0.28 ($p = .023$, 95% CI = [0.04, 0.53]). For "non-religious/low religiosity" of 0.12 ($p = .31$, 95% CI = [−0.11, 0.35]), with a trim and fill estimate of 0.07 ($p = .64$, 95% CI = [−0.16, 0.32]). These effect sizes are marginally significantly different, $z(11) = 1.68$, $p = .09$, 95% CI = [−0.04, 0.58].

It should be noted that, although studies examining non-religious and low-religious participants were combined in these analyses, these types of people are not equivalent. Galen (2012) importantly remarks that there are meaningful differences between those who categorize themselves as "not religious" or "atheist," those who categorize themselves as "none" or a "never-attender," and those who are categorized in studies as "low religiosity" (usually by scoring below some distribution threshold). Comparing across the heterogeneous ways that the researchers categorized low- and non-religious participants is bound to be coarse. However, if we do further divide these studies into those that categorize "no religion" participants and those that categorize "low religion" participants, neither group shows an effect, (no religion: $k = 11$, $g = 0.12$, $p = .301$, 95% CI = [−0.11, 0.36]; low religion: $k = 6$, $g = −0.17$, $p = .363$, 95% CI = [−0.53, 0.19]). Comparing the effect sizes of these two sets of studies yields no significant difference, $QM(2) = 1.90$, $p = .387$, although the small number of studies in that comparison limits firm conclusions.

A second caveat to these results is that researchers who found no difference between believers and non-believers may have been less likely to report independent values for these subsamples, making null differences underrepresented in our analysis. While we acknowledge this possibility, it should again be noted that Analyses 6 and 7's results do not simply indicate a difference between effects between the two groups, but they show no reliable effect whatsoever for non-religious/low-religious participants. Thus, it is unlikely that the non-responsiveness of non-believers to religious priming is an artifact of a reporting bias on the part of researchers.

General Discussion

This article had three broad goals: The first goal was to synthesize the religious-priming literature and evaluate the reliability of religious priming in general as well as compare effects for different priming techniques (Analyses 1-3). The second goal was to analyze specifically whether religious priming encourages prosocial behavior (Analyses 4-5), and the third goal was to investigate whether religious primes occur at every level of prior religious belief (Analyses 6-7). In each analysis, we tested the robustness of religious-priming effects against key methodological artifacts and QRPs, such as publication bias and *p* hacking.

Religious Priming

Across 92 religious-priming studies (n = 11,608), testing various hypotheses, and across all levels of prior religious belief, effect-size analyses revealed that religious primes produced a reliable, moderately sized effect (g = 0.40). A subsequent *p*-curve analysis, using 67 qualifying studies (n = 6,949), suggests the literature is robust to extensive use of QRPs that artificially reduce *p* values. The literature was also found to be robust to selective reporting of significant findings (i.e., the file-drawer effect).

Among participants that authors classified as religious or highly religious (for those 17 studies in which such a distinction was made), the mean effect size was g = 0.44. This figure is nearly identical to d = 0.43 (r = .21), the figure that Richard, Bond, and Stokes-Zoota (2003) calculated as the mean effect size of 25,000 social-psychological studies conducted over 100 years. Contrary to previous speculation, however, religious priming produced no consistent effect on the non-religious. That only religious participants appear consistently responsive to religious priming is of theoretical importance. It suggests that responsiveness to religious cues depends to a significant extent on culturally transmitted beliefs and is not merely the result of low-level associations (Norenzayan, Henrich, & Slingerland, 2013). This finding is also consistent with the idea that primes are most effective when they are self-relevant (e.g., Wheeler, DeMarree, & Petty, 2007). The religious primes appear to capitalize on the situational activation of existing beliefs, rather than on society-wide stereotypes about religion that are presumably shared by believers and non-believers alike, particularly in societies with religious majorities such as the United States (Gervais & Norenzayan, 2013). In addition, although an average non-believer did not respond to religious priming, there was variability in the effect sizes across studies, suggestive of the possibility that at least some non-believers may in fact be influenced by some religious primes. Whether this variability can be explained by systematic patterns is open for future research.

Although too few studies have yet been conducted with each of the four priming techniques to reach strong conclusions, the respective effect sizes suggest that each method is effective at roughly comparable levels. Studies using subliminal primes had somewhat smaller effects (g = 0.33), whereas the few studies using contextual primes had the strongest (g = 0.49). Those for implicit (g = 0.39) and explicit (g = 0.42) primes lay in between. It will not be surprising if this pattern remains in future studies; contextual primes, such as testing participants in a church, or while listening to background prayer calls, closely emulate individuals' experience with religion in the outside world. This ecological relevance makes the currently underutilized technique of contextual priming attractive for future research.

That subliminal primes had the smallest effects introduces the important possibility of demand characteristics in some of these studies using alternative priming techniques. By attempting to bypass conscious perception, subliminal priming minimizes the possibility of demand in comparison with the explicit and contextual primes. Implicit priming studies can also minimize demand, but in some cases, may not eliminate the issue completely. Most (but not all) of the implicit studies used awareness probes, such as funneled debriefing, dropping from analysis the rare participants, if any, who recognized the nature of the prime or displayed suspicions about the experimental hypothesis. However, these awareness probes can vary in their sensitivity and sophistication. Although demand characteristics fail as an explanation for the entirety of religious-priming effects, we cannot completely rule out the possibility that a portion of the larger effect sizes in the contextual, explicit, and implicit priming studies could represent the presence of such demand—an issue that future research can address.

Religious Prosociality

Twenty-five studies testing the impact of religious priming on various measures of prosocial behavior were analyzed. The effect size (0.18) remained robust even after correcting for publication bias. This figure increased to 0.27 when the bias-corrected effect size was calculated for believers only. This finding is a crucial piece of evidence for scientific debates about whether religious beliefs and practices spread by having prosocial effects (Norenzayan, 2013; Norenzayan & Shariff, 2008; Norenzayan et al., in press).

Importantly, we of course do not claim that religion solely encourages prosociality; priming research has also shown religious concepts to encourage uncharitable attitudes, such as racism (Johnson et al., 2010; LaBouff et al., 2012). Nor is this to say that the prosociality that religion inspires is indiscriminant and universal; religious prosociality is likely preferentially directed toward ingroup members (Norenzayan et al., in press), a hypothesis that can be tested more methodically in future research. Nor are we arguing that religion is the only, most effective, or most desirable path to prosociality. For example, priming secular institutions of justice can increase dictator game offers to the same degree as priming religion (Shariff & Norenzayan, 2007). None of these caveats, however, refute the evidence that aspects of religious beliefs and rituals motivate people to sacrifice self-interest for others (Norenzayan & Shariff, 2008; Sosis & Ruffle, 2003; Xygalatas et al., 2013).

We found clear support for religious priming in samples drawn from populations culturally shaped by the highly influential prosocial religious traditions of the Abrahamic faiths, comprising a majority of the world's population, where we would expect religious concepts to influence prosociality. Going beyond current studies, religious priming can be an important tool to address theoretical debates about whether religious prosociality is a universal feature of all religious traditions, or whether the religious encouragement of prosocial behavior emerges culturally in large-scale societies but not in smaller-scale groups where religious ideas and prosociality are largely disconnected and where the gods appear to have limited concern about how people treat each other (Norenzayan, 2013; Norenzayan et al., in press; Shariff, 2011). Therefore, a crucial, though methodologically arduous, future direction is to extend religious-priming research across a wider range of cultures to test divergent hypotheses about the origins of religious prosociality. To do so, researchers must move beyond their disproportionate reliance on samples drawn from WEIRD (Western, Educated, Industrialized, Rich, and Democratic) populations, an issue that affects virtually all areas of experimental psychology (Henrich, Heine, & Norenzayan, 2010).

What Meta-Analyses Add to the Priming Literature (and What They Do Not)

The controversies over priming in social psychology are serious, and extensive effort is required to separate wheat from chaff. Obviously, neither effect-size analyses nor *p*-curve analyses should be considered a panacea. Nevertheless, they are valuable contributions to the evidentiary base of psychology. Effect-size meta-analyses are diagnostic tools, allowing us to aggregate direct and conceptual replications and evaluate the reliability of effects that emerge in individual studies. Meta-analyses are thus likely—especially when using techniques to minimize the impact of publication bias, as we have done in these analyses—to yield more accurate judgments about the presence or absence of an effect than are single studies (including single failures to replicate). As the American Psychological Association Task Force on Statistical Inference stated, "The results in a single study are important primarily as one contribution to a mosaic of study effects" (Wilkinson, 1999). The analysis in the current article uses such a mosaic to demonstrate—with more confidence than can individual studies—the robustness of religious priming for believers, both in general, and specifically in promoting prosocial behavior.

P-curve analyses cover areas where traditional effect-size analyses do not. As part of the impetus for developing *p* curve, Simmons et al. (2011) found that the use of undisclosed *p* hacking could dramatically raise the rate of published false positive findings. Even with techniques to account for the selective reporting of significant studies, traditional meta-analytic tools cannot differentiate real effects from non-existent ones that were *p*-hacked to significance. However, a right-skewed *p* curve, such as those seen in Analyses 3 and 5, shows that a body of studies reflects a true effect, rather than findings that, although statistically significant, are merely the product of *p* hacking. As *p*-curve analyses can only provide information regarding the evidentiary value of a literature, rather than the aggregate effect size or important moderators, the technique is fruitfully used in concert with traditional effect-size analyses.

Meta-analyses are, however, limited by the quality of the studies included. Whereas *p* curve aims to sniff out deliberate *p* hacking of a study's data, the analysis does not address other methodological issues, such as the presence of subtle experimenter cues that may have biased participants toward a predicted finding or demand characteristics that may have emerged from insufficiently inconspicuous manipulations. Many of these potential problems cannot be detected from simply reading the manuscripts. Good experimental design on behalf of the researchers, such as keeping experimenters blind to condition, mitigates these concerns, as does vigilance on behalf of reviewers, and a robust literature of conceptual replications (which show that effects generalize across different designs) and direct replications (which show that the effects are reliable across attempts, especially if conducted by different labs). As the current analyses indicate, the religious-priming literature has many conceptual replications, but fewer more direct replications (examples are Ahmed & Salas's [2011] and Hurst's [2014] replications of Shariff & Norenzayan [2007], both of which were conducted in different countries, and LaBouff et al.'s [2012] replication of Johnson et al. [2010], also conducted in different countries). Religious priming, like all priming research, can benefit from more direct replication attempts—preferably published and/or preregistered.

In sum, all psychological findings remain open questions. Nevertheless, the present analyses reveal support for the effect of religious priming on prosocial behavior for religious participants. Moreover, the article shows that religious priming is, in general, an effective addition to the toolbox available for psychology's attempt to understand the impact of religious concepts on human behavior. Finally, in showing the effectiveness and reliability of the subset of priming research studying religion, these analyses speak supportively about the use of priming in social psychology more generally. As discussed above, meta-analyses allow for a broader and generally more accurate view of the scientific literature than do individual studies with either positive or null results. Although navigating through the trees of individual studies—including failed replication attempts—is indispensable, given the size of the literature being produced in social-psychological priming, we encourage more effort to be directed toward conducting and evaluating analyses that allow us to see the forest.

Appendix A

Excluded Studies

Year	Authors	Title	Study	Reason for exclusion
2004	Wenger	The Automatic Activation of Religious Concepts: Implications for Religious Orientations		No neutral prime (religious prime compared against student prime)
2010	Rutchick	Deus Ex Machina: The Influence of Polling Place on Voting Behavior	Study 1	No random assignment to Church polling locations
2010	Rutchick	Deus Ex Machina: The Influence of Polling Place on Voting Behavior	Study 2	No random assignment to Church polling locations
2010	Rutchick	Deus Ex Machina: The Influence of Polling Place on Voting Behavior	Study 3	No random assignment to Church polling locations
2012	McCollough, Carter, DeWall, and Corrales	Religious Cognition Down-Regulates Sexually Selected, Characteristically Male Behaviors in Men, but not in Women	Study 2	Prime was about the afterlife, not specifically about religion.
2013	Rand et al.	Religious Motivations for Cooperation: An Experimental Investigation Using Explicit Primes		No neutral condition (all primes done in a church)
2013	Preston and Ritter	Different Effects of Religion and God on Prosociality With the Ingroup and Outgroup	Study 1	No neutral prime (religion prime compared with God prime)
in press	van Elk, Rutjens, van Harreveld, and van der Plig	Priming of supernatural agent concepts and agency detection		No neutral prime (human, animal, and God primes only)

Appendix B

Analysis 1 results when including adjusted versions of Randolph-Seng and Nielsen (2007)'s Study 2 and Duhaime's (2014) Study 2, adding 0.5 to all cells:

Analysis 1: $k = 92$, $g = 0.42$, $p < .0001$, 95% CI = [0.35, 0.48].

Analysis 2:

	k	*n*	Effect size	95% CI
Priming technique				
Implicit	40	5,190	0.39***	[0.29, 0.49]
Explicit	26	3,846	0.42***	[0.31, 0.53]
Subliminal	19	1,747	0.33***	[0.20, 0.48]
Contextual	9	933	0.49***	[0.28, 0.71]
Experimental setting[a]				
Lab	56	5,381	0.40***	[0.32, 0.48]
Online	25	4,709	0.38***	[0.26, 0.49]
Mechanical Turk	13	1,907	0.34***	[0.17, 0.52]
Not Mechanical Turk	12	2,802	0.41***	[0.27, 0.56]
Field	12	1,428	0.44***	[0.25, 0.62]
Dependent measure				
Self-report measures	42	5,152	0.46***	[0.37, 0.55]
Behavioral measures	52	6,564	0.34***	[0.26, 0.42]

Note. CI = confidence interval.
[a]Ginges, Hansen, and Norenzayan (2009) was the only study to be conducted via telephone and was not included in the Experimental Setting breakdown.

Appendix C

Meta-analyses results when excluding studies from the current authors' labs:
Analysis 1: $k = 84$, Hedges' $g = 0.42$, $p < .0001$, 95% CI = [0.35, 0.48].
Analysis 2:

	k	n	Effect size	95% CI
Priming technique				
Implicit	35	4,939	0.39***	[0.29, 0.49]
Explicit	23	3,172	0.42***	[0.31, 0.53]
Subliminal	18	1,702	0.33***	[0.20, 0.48]
Contextual	8	870	0.49***	[0.28, 0.71]
Experimental setting				
Lab	48	4,609	0.40***	[0.32, 0.48]
Online	25	4,709	0.38***	[0.26, 0.49]
Mechanical Turk	13	1,907	0.34***	[0.17, 0.52]
Not mechanical Turk	12	2,802	0.41***	[0.27, 0.56]
Field	11	1,365	0.44***	[0.25, 0.62]
Dependent measure				
Self-report measures	38	3,848	0.46***	[0.37, 0.55]
Behavioral measures	46	6,835	0.34***	[0.26, 0.42]

Note. CI = confidence interval.

Analysis 3: $k = 62$, $\chi^2(124) = 185.32$, $p = .0003$.
Analysis 4: $k = 21$, Hedges' $g = 0.27$, $p < .001$ 95% CI = [0.16, 0.37].
Analysis 5: $k = 14$, $\chi^2(28) = 68.86$, $p < .0001$.

Analysis 6:

- All studies with religiosity moderation: $k = 11$, Hedges' $g = 0.20$, $p = .004$, 95% CI = [0.06, 0.34];
- Just "religious/high religiosity": $k = 11$, Hedges' $g = 0.40$, $p < .001$, 95% CI = [0.18, 0.62];
- Just "non-religious/low religiosity": $k = 11$, Hedges' $g = 0.04$, $p = .72$, 95% CI = [−0.19, 0.27].

Analysis 7:

- All prosocial studies with religiosity moderation: $k = 7$, Hedges' $g = 0.20$, $p = .011$, 95% CI = [0.05, 0.36];
- Just "religious/high religiosity": $k = 7$, Hedges' $g = 0.38$, $p = .006$, 95% CI = [0.11, 0.65];
- Just "non-religious/low-religiosity": $k = 7$, Hedges' $g = 0.38$, $p = .006$, 95% CI = [0.11, 0.65]; non-believers: $k = 7$, Hedges' $g = 0.14$, $p = .27$, 95% CI = [−0.10, 0.39].

Authors' Note

Late in the publication process, we were alerted to the results of a large-scale replication attempt of Shariff and Norenzayan's (2007) study of religious priming on the dictator game (McCullough & Gomes, 2015). Though this was past our stopping point for study selection, because the study found a null effect, we wanted to test whether adding the study to Analyses 1 and 4 meaningfully changed the results. Including this study resulted in only minimal changes in Analysis 1 ($k = 93$, Hedges' $g = 0.39$, $p < .0001$, 95% CI = [0.33, 0.45]) and Analysis 4, $k = 93$, Hedges' $g = 0.39$, $p < .0001$, 95% CI = [0.33, 0.45].

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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.

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Notes

1. Labouff, Rowatt, Johnson, and Finkle (2012) were included in our analysis despite not using true random assignment. The samples were randomly selected from the populations of

pedestrians in front of religious and secular buildings (e.g., in front of the U.K. House of Parliament and Westminster Abbey). Because the authors made a substantial effort to ensure the samples were unbiased, and our judgment that this type of contextual study adds value to our understanding of the effects of religious priming, we decided to include it. The exclusion of this study does not change our overall findings.

2. Note that the great majority of the implicit (75%) and subliminal (79%) priming studies used awareness probes of some sort, which increase confidence that the primes indeed bypass awareness. However, we cannot rule out the possibility that even in these studies, certain participants had conscious awareness of the primed concept or stimuli—an issue that is being debated regarding all unconscious priming research (see Newell & Shanks, 2014, and relevant commentaries, for a discussion). When we refer to studies as having used implicit or subliminal priming, we are thus referring to the researchers' declared interpretations of how participants were primed.
3. In addition to studies that reported $p > .05$ values for their main hypotheses, Study 2 of Pichon, Boccato, and Saroglou (2007) was also excluded, as recalculation of the analysis for their main analysis resulted in a nonsignificant p value of .12.

Supplemental Material

The online supplemental material is available at http://pspr.sagepub.com/supplemental.

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