Research Square

Preprints are preliminary reports that have not undergone peer review.
They should not be considered conclusive, used to inform clinical practice,
or referenced by the media as validated information.

Do Prosocial Nudges Shift Reported Religiosity? Evidence from a Randomized Survey Experiment

Evangelos Dioikitopoulos
Athens University of Economics and Business
Rigissa Megalokonomou
rigissa.megalokonomou@monash.edu
Monash University
Tommaso Sartori
Monash University

Article

Keywords: nudges, malleability of identity, religiosity, belief formation, secular vs. religious messages, prosocial behavior

Posted Date: October 1st, 2026

DOI: https://doi.org/10.21203/rs.3.rs-10856926/v1

License: This work is licensed under a Creative Commons Attribution 4.0 International License.
Read Full License

Additional Declarations: No competing interests reported.

Do Prosocial Nudges Shift Reported Religiosity? Evidence from a Randomized Survey Experiment *

Evangelos Dioikitopoulos Rigissa Megalokonomou Tommaso Sartori

September 2, 2026

Abstract

Nudges are widely used to shape choices, yet their capacity to move deeply rooted aspects of identity such as religiosity remains largely unknown. We present evidence from a registered randomized survey experiment in which we test whether brief prosocial messages—framed in either religious or scientific language—can shift religiosity. Participants (N=1,136) were randomly assigned to one of three arms: A religious nudge drawing on biblical teachings, a scientific nudge grounded in psychological research, or a no-message control. Both messages conveyed a parallel appeal to human connection, each paired with an image consistent with its framing. The religious message takes the form of a moral injunction and the scientific message describes a documented relationship, so we interpret the estimates as effects of the two implemented treatment packages. We find that the scientific nudge reduced self-reported religiosity relative to the control group. Baseline religiosity tracks regional patterns in an independent national survey, and is predicted by a name-based proxy for religious family background. These findings suggest that reported religiosity can respond at the margin to brief, low-cost messages.

Keywords: nudges, malleability of identity, religiosity, belief formation, secular vs. religious messages, prosocial behavior

*Evangelos Dioikitopoulos: Athens University of Economics and Business (edioik@aueb.gr), Rigissa Megalokonomou: Department of Economics, Monash Business School, CESifo, and IZA (email: rigissa.megalokonomou@monash.edu), Tommaso Sartori: Department of Economics, Monash Business School (email: tommaso.sartori@monash.edu). This project has been reviewed by the Office of Research Ethics at the Athens University of Economics and Business—and the related clearance number is $EY\Delta$-5/06102023—and Monash University—and the related clearance project number is 49782. Rigissa Megalokonomou acknowledges financial support from a 2025 Early Career Research Grant from Monash University (Fund No: 1750891). Evangelos Dioikitopoulos acknowledges financial support from the Hellenic Foundation for Research and Innovation (Fund No: BBASP). The RCT has also been preregistered in the AEA RCT Registry with RCT ID number AEARCTR-0016648.

Introduction

Religiosity—an individual's degree of religious belief, practice, and identification—constitutes a fundamental dimension of human identity, and shapes moral values, social norms, political attitudes, and economic behaviors worldwide (Iannaccone, 1998; McCleary & Barro, 2006; Putnam & Campbell, 2010; Stark & Finke, 2000). Rooted deeply in culture, tradition, and personal experience, religiosity is often considered stable over time and resistant to transient influences (Akerlof & Kranton, 2000; Bénabou & Tirole, 2011; Voas & Crockett, 2005). Yet religiosity is only one example of a broader class of deeply rooted identities—alongside nationalism, political ideology, and partisanship—that are thought to anchor individuals' worldviews and guide behavior. Understanding whether such identities are malleable—and how they may shift in response to subtle contextual cues—remains a critical question in the social and behavioral sciences.

In economics, religiosity has been linked to a variety of outcomes, including labor supply, consumption patterns, and social capital formation (Arruñada, 2010; Becker & Woessmann, 2009; Della Lena, Sato, & Zenou, 2026; Gruber, 2004; Guiso, Sapienza, & Zingales, 2006). Yet, because most studies rely on observational data, identifying the causal determinants or short-run dynamics of religiosity—a trait often treated as a stable component of identity—remains difficult (Bentzen, 2019; Guiso, Sapienza, & Zingales, 2006; McCleary & Barro, 2006; Norris & Inglehart, 2011; Voas, 2009). This gap is especially important given the growing recognition in behavioral economics that identity plays a central role in shaping preferences and choices (Bursztyn & Jensen, 2015; Chen & Li, 2009; Cinnirella, Della Lena, Manzoni, & Panebianco, 2026; DellaVigna, 2009; Falk & Szech, 2013). If even the most durable aspects of identity can be influenced by external framings or nudges, then their downstream consequences for economic and social behavior may be more context-dependent than previously assumed.

Behavioral economics and policy design increasingly employ nudges—small, low-cost, non-coercive interventions designed to steer individual choices without restricting freedom (Thaler & Sunstein, 2008). Nudges have been widely applied in domains such as health (Chapman, Li, Colby, & Yoon, 2010; Hollands, Shemilt, Marteau, Jebb, Kelly, Nakamura, Suhrcke, & Ogilvie, 2013; Milkman, Beshears, Choi, Laibson, & Madrian, 2011); savings (Beshears, Choi, Laibson, Madrian, & Milkman, 2015; Chetty, Friedman, Leth-Petersen, Nielsen, & Olsen, 2014; Thaler & Benartzi, 2004); environmental behaviors (Allcott, 2011; Ferraro & Price, 2013; Newell & Siikamäki, 2014; Yoeli, Hoffman, Rand, & Nowak, 2013); self-control problems such as procrastination (Ariely & Wertenbroch, 2002; Kaur, Kremer, & Mullainathan, 2015; O'Donoghue & Rabin, 1999), and have recently been tested at scale through randomized trials using crowdsourced or institutional design approaches (DellaVigna & Linos, 2022; Paley & van de Ven, 2023). Yet their potential to shape deeply rooted belief systems remains largely unexplored. Research on religiosity and identity has relied predominantly on correlational evidence, leaving unanswered whether subtle interventions can causally shift self-perceptions of identity and belief. Addressing this question offers insights not only for the economics of religion but also for the broader literature on identity malleability.

Our study bridges these strands of the literature by experimentally testing whether brief prosocial nudges framed through religious or scientific sources (compared with a control that was not exposed to a nudge) can shift self-reported religiosity. Specifically, we investigate whether messages drawing on biblical teachings attributed to Jesus Christ and scientific research on human social connectedness differentially influence religious self-concept. The two messages convey parallel prosocial content but frame the importance of human connection through religious and scientific sources, respectively.

We conduct a large-scale online randomized experiment in which we randomly expose participants to different framings (nudges). This design isolates the causal effect of framing on self-reported religiosity.

We use original data from an online survey experiment conducted in Greece between June 2024 and August 2025. Participants were initially recruited through university-affiliated networks and subsequently through informal sharing by participants with others in their networks. As an incentive, participants who completed the survey entered a lottery to win one of 100 video projectors. After providing informed consent, participants completed baseline survey questions on demographic characteristics and religiosity. They were then randomly assigned by the survey platform to one of three conditions: A brief prosocial message framed in religious language, an analogous message framed in scientific language, or a control condition with no message. Immediately after treatment exposure, participants completed the post-treatment religiosity measures and other survey outcomes.

We find that exposure to the scientific message reduces self-reported religiosity relative to the control group. The estimated effect of the religious message is also negative, but smaller in magnitude and less precisely estimated, and the difference between the two messages is itself imprecisely estimated. This pattern suggests that self-reported religiosity, an element of individual identity often viewed as deeply rooted, can respond to brief prosocial messages. Scientific messages, by highlighting psychological and social mechanisms underlying well-being, may momentarily weaken reliance on religious interpretations. This asymmetric responsiveness is consistent with research in identity economics, which shows that contextual frames can reshape self-conception, but that belief updating is more likely when the new frame credibly challenges—rather than reinforces—prior views (Akerlof & Kranton, 2000).

The implications extend beyond religion: Religiosity is linked to a wide range of economic and social behaviors, including charitable giving, saving, trust, and political preferences. Showing that self-reported religiosity can exhibit sensitivity to the framing of prosocial messages raises the question of whether similar contextual sensitivity may arise in other identity-relevant domains, such as nationalism, ideology, or partisanship. Everyday communication—through public campaigns, education, or digital media—may therefore affect not only the behavior these messages are designed to influence but also how individuals report identity-relevant beliefs. Recognizing this sensitivity to framing may be important for designing prosocial interventions that are both effective and ethically sound.

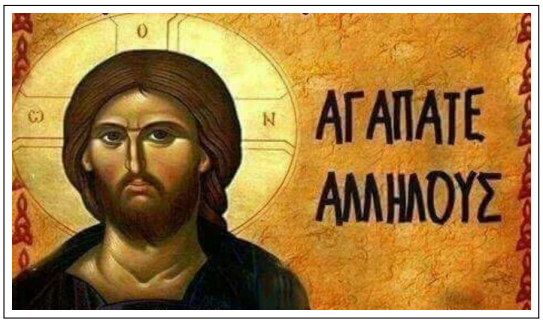
This study contributes to prior research in several ways. First, while much of the existing literature relies on observational associations between religiosity and behavior (Guiso, Sapienza, & Zingales, 2006; Iannaccone, 1998; McCleary & Barro, 2006), we provide experimental evidence that self-reported religiosity can respond to brief external cues. Second, we show that measures associated with religious beliefs—which are typically regarded as stable over time—can exhibit short-run sensitivity to informational context. Third, our design directly compares prosocial messages framed through scientific and religious sources, allowing us to examine whether responses depend on the source through which otherwise parallel prosocial content is presented. In doing so, we extend the study of nudges beyond choices and behavior to examine their effects on immediately reported identity-relevant beliefs (Benjamin, Choi, & Strickland, 2010; DellaVigna, 2009), and contribute to broader theories of identity and belief formation (Bénabou & Tirole, 2011; Falk & Szech, 2013).

Current Study

We conducted a preregistered randomized survey experiment to test the malleability of individuals' self-reported religiosity. Adult participants were recruited online and completed the survey on Qualtrics. Before treatment assignment, participants provided demographic information and baseline measures of religiosity. Participants were then randomly assigned by the survey platform to one of three conditions: (i) a religious nudge summarizing Jesus Christ's teachings on the importance of human connection; (ii) a scientific nudge summarizing peer-reviewed research on the importance of human connection; or (iii) a control condition that received no message. The two treatment messages each conveyed a prosocial appeal centred on the importance of human connection, attributed in one case to religious teaching and in the other to psychological research. The messages were not matched on all dimensions: the religious message takes the form of a moral injunction addressed to the reader, while the scientific message describes a documented relationship between social interaction and well-being. We therefore interpret the estimates as effects of the two implemented treatment packages rather than as isolating the source of an otherwise identical message. Each message was also accompanied by an image consistent with its respective framing. Figure 1 presents the messages and images shown to participants.

Figure 1: Religious and Scientific Randomized Treatments

Religious Message



Jesus Christ taught us to:

- Love one another (John 13:35)
- Love your enemies (Luke 6:27–28)

Scientific Message



"Research from the University of California concludes that people can increase their happiness through simple positive activities, such as expressing gratitude or interacting with other people (Lyubomirsky & Layous, 2013)."

Notes: The figure reproduces the verbatim text of the two treatment messages shown to participants. The accompanying images are licence-cleared equivalents of those presented in the experiment. The original treatment images are available from the authors on request.

Immediately after treatment exposure, participants completed the post-treatment survey. Our main outcomes capture two dimensions of self-reported religiosity: The importance of God, measured on a scale from 0 ("not very important") to 10 ("very important"), and the desire for frequent prayer, measured by a binary indicator equal to one if participants report wanting to pray at least once per day and zero otherwise. The survey also elicited a broader set of post-treatment religious beliefs, attitudes, and behavioral preferences. Participants advanced through the survey one block at a time,

without the ability to revise their previous answers. This setup limited the scope for anchoring and revision bias. Random assignment allows us to identify the short-run causal effect of exposure to each message relative to the no-message control condition.

Participants were recruited in Greece through a voluntary online survey. Recruitment initially targeted university students through university-affiliated networks and subsequently expanded through informal sharing by participants with others in their networks. Although recruitment initially centered on university networks, participants originated from nearly every region of Greece, with the largest share coming from Athens, the country's most densely populated metropolitan area. Figure A1 reports the geographic distribution of respondents by region of origin.

The experiment was registered in the AEA RCT Registry (AEARCTR-0016648). All participants provided informed consent before beginning the survey, and the study received approval from the institutional review boards at Monash University and the Athens University of Economics and Business. Further details on recruitment and sample construction are provided in Recruitment and Sample Construction, while additional details on measurement and the empirical specification are provided in Empirical Specification.

Results

Sample and Balance: Our analytic sample comprises 1,136 adults (54% women; mean age = 28.4). Table 1 reports respondent demographics. Most participants have a high school education (52%), followed by a bachelor's degree (27%), a master's degree (17%), and a PhD (4%). The majority are single (70%), and 23% are married. In terms of religion, 75% identify as Christian (Orthodox) and 20% as non-religious. Also, 29% of participants completed the survey during the Christmas or Easter months, and 78% share a grandparent's first name. The high share of intergenerational transmission of first names in Greece is consistent with evidence that naming conventions serve as a key channel of cultural and familial identity across generations (Alesina, Giuliano, & Nunn, 2013). Randomization produced a near-even split across arms (34% religious nudge, 33% scientific nudge, 33% control). For completeness, Appendix A presents descriptive statistics of the main outcomes in Table A1, and Table A2 reports means and standard errors by treatment.

Tables 2 and A3 assess balance in baseline characteristics across treatment arms. Table 2 reports arm-specific means together with the p-value from an F-test of the joint null that the coefficients on both treatment indicators are zero. Three of the 20 characteristics reject at the 5 percent level—being above the median age, holding a high-school diploma, and being in a relationship—and a fourth, being single, rejects at the 10 percent level. Table A3 reports the corresponding arm-by-arm estimates, which comprise 60 tests in total (20 characteristics $\times$ 3 arms); five reject at the 5 percent level.

These rejections are not distributed evenly across characteristics or across arms. They are concentrated in age and in the education and relationship variables that are correlated with it, and they arise almost entirely from the control group, which is older than either treatment arm (57.4 percent above the median age, against 44.8 and 45.2 percent in the religious and scientific arms). Treatment assignment was implemented individually and continuously by Qualtrics throughout the field period, so we interpret this as a chance draw rather than a failure of the randomization procedure; it nonetheless motivates the covariate-adjusted specifications we report throughout.

We therefore report specifications controlling for the full set of baseline covariates alongside the unadjusted estimates. The estimated treatment effects are similar across specifications, which indicates

that the results are not sensitive to adjustment for the observed baseline differences. We return to the age imbalance when interpreting the heterogeneity analysis, since age is also the dimension along which we find the most robust evidence of heterogeneous effects.

Preregistered Analyses: Our preregistered analyses examine whether the scientific and religious nudges affect a broad set of religiosity, attitudinal, and preference outcomes relative to the no-message control. Our primary focus is on two measures of religiosity: (i) Importance of God, measured on a 0–10 scale (0 = not very important; 10 = very important), and (ii) Desire to pray often, which we report both as a binary indicator equal to 1 if respondents report wanting to pray once or several times per day and as the underlying ordered measure of desired prayer frequency. Exact survey items are reported in Appendix B. The Importance of God item is adapted from the World Values Survey. Desire to pray is constructed from the following question: "Ideally, how often would you like to pray?" The binary indicator equals 1 for respondents selecting "Several times a day" or "Once a day", and 0 for those selecting "Several times a week", "Only when attending religious services", "Only on special religious holidays", "Once a year", "Rarely", or "Never/almost never". Wanting to pray at least once per day represents a natural threshold for frequent religious practice and provides an easily interpretable outcome, distinguishing respondents expressing a desire for daily prayer from those reporting less frequent or no desired prayer. We estimate linear models for continuous outcomes and linear probability models for binary outcomes. For each outcome, we estimate intent-to-treat effects using indicators for assignment to the scientific and religious nudges, with the control group as the omitted category. We additionally report direct comparisons between the scientific and religious nudges where relevant; these comparisons, rather than differences in the statistical significance of the treatment-control coefficients, provide the appropriate test of whether the two treatments produce different effects. Appendix C provides a complete mapping between the registration and the analyses reported in the paper, including outcomes, treatment contrasts, estimators, sample restrictions, deviations, and analyses added subsequently.

Figure 2 provides a graphical summary of the main results. Panel A presents the estimated treatment effects on our main religiosity outcomes, with the corresponding coefficient estimates reported in Table A4. We control for baseline religiosity in all specifications—participants' pre-treatment response to the importance-of-God question—and additionally include demographic covariates in the even-numbered columns. Because treatment assignment is randomized, these covariates are not required for identification and are included to improve precision. Results are similar with and without the additional controls.

Assignment to the scientific nudge reduces the post-treatment importance of God by 0.168 points on the 0–10 scale relative to the control group (95% CI: $[-0.324, -0.011]$, column 2). The corresponding standardized estimate is -0.045 standard deviations (95% CI: $[-0.088, -0.003]$, column 4). The estimated effect is also small in magnitude and indicates a modest immediate shift in reported religiosity following exposure to the scientific message. The estimated effect of the religious nudge is also negative, although somewhat smaller, at -0.105 points relative to the control group (95% CI: $[-0.250, 0.039]$). The two treatments are not statistically distinguishable for this outcome (Table A4). Given the estimated magnitudes, the design is not powered to detect differences between the two messages: distinguishing effects of this size from one another would require a substantially larger sample than the one available here.

The results for desired prayer frequency point in a similar direction. Using an indicator for wanting to pray at least once per day, assignment to the scientific nudge reduces the probability of this outcome by 5.8 percentage points (95% CI: $[-0.111, -0.006]$) in column (6). Table A5 shows that

the results for the binary outcomes of the desire to pray often indicator are robust to estimating a logit model rather than a linear probability model. The estimated effect of the religious nudge is close to zero, at -0.9 percentage points (95% CI: $[-0.059, 0.041]$). Retaining the underlying ordered variation in desired prayer frequency in column (8), the estimated effect of the scientific nudge is -0.216 points (95% CI: $[-0.444, 0.012]$), or approximately -0.09 standard deviations—less precise than the binary estimate but consistent with it in direction. For desired prayer frequency, the direct scientific-versus-religious contrast is negative under both specifications—-5.0 percentage points for the binary indicator (95% CI: $[-0.101, 0.002]$) and -0.195 points on the ordered scale (95% CI: $[-0.420, 0.030]$)—and is distinguishable from zero at the 10% level in each. As a robustness exercise, in Table A6 we re-estimate the main specifications restricting the sample to respondents who report believing in God (Panel A), and to those who identify as Orthodox (Panel B). Among respondents who report believing in God, the estimated effects of the scientific nudge are consistently larger in magnitude than in the full sample, although somewhat less precisely estimated due to the smaller sample. This pattern is observed for both the importance of God and prayer frequency. When restricting the sample to Orthodox respondents, by contrast, the estimates remain broadly similar in magnitude to those obtained in the full sample, with the expected loss of precision associated with the smaller sample. Unlike the importance-of-God outcome, then, the prayer measures provide consistent, if imprecise, evidence that the scientific message moves reported religiosity more than the religious one does. Exploratory analyses reported below indicate that this difference is concentrated among respondents with above-median baseline religiosity.

These results indicate small immediate reductions in reported religiosity following the scientific nudge. Because religiosity is measured immediately after treatment exposure, these estimates capture short-run changes in self-reported religiosity. Accounting for multiple hypothesis testing across the two main religiosity outcomes and both treatment–control comparisons using the Benjamini–Hochberg procedure (Benjamini & Hochberg, 1995) yields adjusted q-values of 0.071 for the effects of the scientific nudge on both the importance of God and the desire to pray often. The estimates therefore remain statistically significant at the 10% level, but not at the 5% level. When we instead apply the correction jointly across the full set of preregistered outcomes and both treatment–control comparisons, none of the estimated treatment effects remains statistically significant after adjustment.

In Panel B of Figure 2, we report placebo tests using two outcomes collected after treatment but determined prior to treatment exposure: Whether respondents had been vaccinated against COVID-19 and political polarization. Because neither respondents' prior vaccination status nor their pre-existing political orientation can plausibly be affected by assignment to the messages, treatment effects on these outcomes would indicate systematic differences across experimental groups rather than responses to the intervention. Reassuringly, we find no evidence of treatment effects on either placebo outcome (the corresponding estimates are reported in Table A7).

Effects on Other Preregistered Outcomes: The registration additionally includes a broader set of religious beliefs, moral and social attitudes, and individual preferences. Tables A8 and A9 report treatment effects on these outcomes across separate panels. In Figure A2—as well as in columns (1) and (2) of Panel A of Table A8—we find that both nudges lead to more favorable views of immigrants, although the estimated effect is statistically different from zero only for the scientific nudge. Across the remaining panels of Tables A8 and A9, the estimated effects are generally small and imprecisely estimated, including those for revenge and punishment preferences, religious beliefs

and openness, civil-rights attitudes, church attendance, social comparison, and long-term thinking. We therefore interpret these results cautiously, particularly given the number of outcomes examined.

Exploratory (not preregistered) Analysis: We next examine exploratory treatment-effect heterogeneity. Figure 3 and Table A10 split respondents according to whether their baseline importance of God is below or above the sample median. Among respondents above the median, the scientific nudge is associated with a 0.174-point reduction in the importance of God ($\hat{\beta} = -0.174$, SE $= 0.121$) and a 12.1-percentage-point reduction in the probability of wanting to pray at least once per day ($\hat{\beta} = -0.121$, SE $= 0.047$). The corresponding estimates are smaller in absolute magnitude among respondents below the median. Within the above-median group, direct comparisons between the scientific and religious nudges yield $p = 0.073$ for the importance of God and $p = 0.004$ for the desire to pray often.

We interpret these patterns cautiously. When we model baseline importance of God continuously rather than splitting the sample at the median, Table A11 provides no statistically significant evidence that the treatment effects vary with baseline religiosity. Thus, while the median-split results suggest more pronounced treatment differences among respondents with higher initial religiosity, the continuous specification provides no evidence of a systematic gradient in treatment effects with baseline religiosity.

For completeness, Tables A12 and A13 and Figures A3 and A4 in Appendix A report exploratory heterogeneity analyses across demographic characteristics. Across these dimensions, we generally find little evidence of systematic treatment-effect heterogeneity. One exception is age. For the importance of God, the negative effects of both the religious and scientific nudges are more pronounced among older respondents. For the desire to pray often, age heterogeneity emerges only for the religious nudge, with effects of opposite sign across age groups—positive among younger respondents and negative among older ones. This difference remains statistically distinguishable from zero at the 10% level after applying the Benjamini–Hochberg correction for multiple hypothesis testing ($q = 0.072$; Table A14), but not at conventional 5% levels. Evidence of heterogeneity along the other dimensions becomes substantially weaker after the multiple-testing adjustment. We therefore interpret the broader heterogeneity results—including the median-split patterns by baseline religiosity—as suggestive, with age providing the most robust evidence of heterogeneous treatment effects.

Potential Interpretation: One possible interpretation of the negative scientific-nudge estimates is that presenting prosocial behavior through a scientific frame temporarily makes a secular source of moral guidance more salient, thereby reducing the immediate salience of religion. At the same time, the religious-nudge estimates do not consistently differ from those of the scientific nudge: We cannot distinguish the two treatments for the importance of God, while the prayer outcomes provide suggestive evidence of a more negative effect of the scientific message. The results therefore do not establish that substitution between scientific and religious moral frames is the operative mechanism; responses may also reflect features shared by both interventions, including exposure to a prosocial message and the reflection it induces. Because the experiment does not directly measure these mechanisms, we interpret them as possible explanations rather than as causal channels identified by the design.

Validation

We conduct a series of validation exercises to assess whether our baseline measures of religiosity capture meaningful cross-sectional variation in religious orientation. These exercises are not intended to validate the causal interpretation of the treatment effect or to distinguish changes in religiosity from alternative mechanisms such as salience or measurement reactivity. Rather, they assess whether the religiosity measures used in our experiment exhibit patterns consistent with independent measures and observable correlates of religiosity.

We first compare the geographic variation in baseline religiosity in our sample with that observed in the World Values Survey (WVS), a widely used independent survey of social attitudes (Haerpfer, Inglehart, Moreno, Welzel, Kizilova, Díez-Medrano, Lagos, Norris, Ponarin, & Puranen, 2022). We use the wave fielded in Greece in 2017 and post-stratify WVS observations to match our sample's age distribution. Both surveys use an identical item—"How important is God in your life?"—on a 0–10 scale (0 = not very important; 10 = very important). We harmonize regional units and compute region-level means by respondents' region of origin (prefectures), which we then compare with the corresponding WVS regional means. Figure A5 compares control-group means from our survey with these WVS benchmarks. Religiosity is positively correlated across regions in the two datasets, which indicates that the geographic variation observed in our sample is consistent with that observed in an independent survey. Figure A6 further presents the spatial variation in the importance of God by respondents' region of origin in our sample.

As a second descriptive validation exercise, we examine whether baseline religiosity is associated with a name-based proxy for religious family background, adapting recent approaches that exploit the cultural transmission of given names within families (Andersen & Bentzen, 2023; Knudsen, 2024). The underlying idea is that religious first names may reflect persistent family religious and cultural traditions. Consistent with the intergenerational transmission of naming practices, 78% of our participants report having the same first name as one of their grandparents (Table 1).

We classify first names as religious or secular (predominantly Ancient Greek). A name is coded as religious if it appears in the official registry of Greek Orthodox saints, allowing for canonical variants.Because many Greek names have an associated "name day" without a canonized saint, name-day status alone is not treated as evidence of sanctity. For names without a saint match, we consult the Lexicon of Greek Personal Names (LGPN)—a comprehensive corpus of Greek personal names from inscriptions, papyri, coins, and literary sources—to determine earliest attestation. Names predominantly attested prior to the 1st century CE are coded as secular (ancient); names whose earliest attestation is in Christian sources or usage are coded as religious; and modern non-Christian names are coded as secular. Names of non-Greek origin are assigned a value of zero. Figure A7 displays a word cloud of respondents' first names, with size proportional to their frequency in the sample.

We then examine whether this predetermined proxy for religious family background is associated with participants' baseline religiosity. Figure A8 reports differences between participants with secular and religious first names in the importance of God in their lives (Panel A) and in the share reporting a desire to pray often (Panel B). Participants with religious first names report higher baseline religiosity on both measures ($p < 0.011$ in Panel A and $p < 0.030$ in Panel B). These associations provide descriptive evidence that our classification of first names captures variation related to respondents' pre-existing religious orientation. Importantly, because first names are predetermined, this exercise speaks only to the cross-sectional correlates of religiosity in our sample; it does not establish that the outcome measure necessarily captures experimentally induced changes in religious identity.

Discussion

We provide experimental evidence that self-reports of religiosity respond to the framing of prosocial messages. In our randomized experiment, exposure to a scientific nudge produces small immediate reductions in two measures of religiosity, with suggestive evidence of a more negative effect of the scientific relative to the religious nudge. Our exploratory analyses further suggest that these responses differ across individuals and may be more pronounced among highly religious respondents. Our validation exercises provide complementary evidence on the measurement of religiosity in our sample. Geographic variation in baseline religiosity is positively associated with corresponding variation in the World Values Survey, while predetermined naming patterns associated with religious family background are correlated with respondents' baseline religiosity. These exercises indicate that our measures capture meaningful cross-sectional variation in religious orientation.

Why might exposure to a scientific prosocial message reduce immediately reported religiosity? One possibility is that scientific framing temporarily increases the salience of a secular source of moral guidance, thereby reducing the immediate salience of religion when respondents subsequently assess their own religiosity. However, the religious-nudge estimates sometimes move in the same direction, and the two treatments are not consistently distinguishable from one another. Responses may therefore also reflect features shared by both interventions, such as exposure to a prosocial message or the reflection it induces.

Several limitations qualify these results. The sample is a convenience sample recruited through university networks and informal sharing, and is younger and more educated than the Greek adult population; the estimates should not be read as nationally representative. Religiosity is measured immediately after treatment exposure, so we cannot distinguish a shift in reported religiosity from a temporary change in the salience of religious considerations, nor establish persistence. The control condition received no message and no image, so the treatment-control contrasts capture the combined effect of receiving a prosocial message, viewing an accompanying image, and the additional time and attention this required, rather than the framing alone; only the scientific-versus-religious contrast isolates the source of the message, and the design is not powered to detect differences of the magnitude we estimate. Our main outcomes are single survey items, which limits precision relative to multi-item scales.

Because self-reported religiosity is generally viewed as relatively stable, its sensitivity to the framing of otherwise similar prosocial messages may be relevant in settings where scientific and religious narratives coexist as alternative ways of motivating prosocial behavior, including education, public communication, and digital media. Whether these responses represent temporary shifts in salience or more persistent changes in religious self-conception remains an open question.

Methods

Recruitment and Sample Construction

Participants were recruited through a voluntary online survey disseminated in Greece between June 4, 2024 and August 21, 2025. Recruitment initially targeted university students through university-affiliated networks and subsequently expanded through informal sharing by participants with others in their networks. The survey link could therefore be forwarded beyond the individuals initially approached, and the resulting sample should be interpreted as a convenience sample rather than as a probability-based or nationally representative sample.

A total of 1,228 respondents entered the initial sample. We excluded 50 respondents who did not complete the survey up to the point of randomization, 12 respondents who were younger than 18 and therefore did not satisfy the study's eligibility criterion, and 30 respondents whose survey completion time exceeded 60 minutes. These exclusions yield a final analytic sample of 1,136 respondents. Importantly, the 50 incomplete responses occurred before treatment assignment and therefore do not constitute post-randomization attrition. All respondents reaching randomization and satisfying the eligibility and completion-time criteria completed the primary outcome measures.
Treatment assignment was implemented individually through Qualtrics, which randomly allocated respondents with equal probability to the religious-message, scientific-message, or no-message control condition. The same randomization procedure operated throughout the field period and was not blocked by recruitment source, institution, or calendar period.

Ethics Declarations

This study was approved by the Office of Research Ethics at the Athens University of Economics and Business (clearance number EYΔ-5/06102023) and by the Monash University Human Research Ethics Committee (project number 49782). All methods were performed in accordance with the relevant guidelines and regulations, including the Declaration of Helsinki. Informed consent was obtained from all participants through an electronic consent process administered at the beginning of the survey; participation was voluntary and consent was documented electronically. Participation was restricted to adults aged 18 years or older. The experiment was pre-registered in the AEA RCT Registry (AEARCTR-0016648).

Empirical Specification

Our identification strategy relies on a preregistered randomized controlled trial (RCT) designed to causally estimate how brief prosocial nudges—framed in either religious or scientific language—affect individual religiosity. A total of 1,136 participants were randomly assigned to one of three experimental conditions: (i) a *religious framing* group, (ii) a *scientific framing* group, or (iii) a *control* group that received no messages. Both treatment groups viewed a short prosocial message paired with an image thematically consistent with the respective framing. Participant recruitment and data collection took place between 4 June 2024 and 21 August 2025. All participants provided informed consent prior to participation through an electronic consent process administered at the beginning of the survey. Participation was voluntary and consent was documented electronically.
Random assignment ensures that, in expectation, both observed and unobserved characteristics are balanced across groups. This design allows us to attribute post-treatment differences in religiosity to the framing of the message rather than to preexisting traits conditional on baseline religiosity.
Religiosity was measured immediately after exposure to the prosocial nudges assessing belief strength and self-reported religiosity. Our main estimating equation is:

$$Y_i = \alpha + \beta_1 \cdot \textit{Religious}_i + \beta_2 \cdot \textit{Scientific}_i + \mathbf{X}_i'\gamma + \varepsilon_i \tag{1}$$

where Y_i denotes the religiosity score for individual i; Religious_i and Scientific_i are binary indicators for treatment assignment; and $\mathbf{X}_i$ is a vector of predetermined characteristics (age, gender, education, marital status, baseline importance of God, whether a participant's family income is above the median), and indicators for religious affiliation (not religious/Orthodox/other religion) included to

improve precision. Heteroskedasticity-robust standard errors are used throughout.
The coefficients β_1 and β_2 identify the average treatment effects (ATEs) of the religious and scientific nudges relative to the control group. This specification provides a transparent and internally valid framework to test whether exposure to differently sourced prosocial messages can shift a deeply rooted dimension of identity—religiosity—in a controlled environment.

References

Akerlof, G. A., & Kranton, R. E. (2000). Economics and Identity. *Quarterly Journal of Economics*, *115*(3), 715–753.

Alesina, A., Giuliano, P., & Nunn, N. (2013). On the Origins of Gender Roles: Women and the Plough. *Quarterly Journal of Economics*, *128*(2), 469–530.

Allcott, H. (2011). Social Norms and Energy Conservation. *Journal of Public Economics*, *95*(9–10), 1082–1095.

Andersen, L. H., & Bentzen, J. S. (2023). *In the Name of God! Religiosity and the Emergence of Modern Science and Growth* [Unpublished manuscript]. https://web.econ.ku.dk/bentzen/Andersen_Bentzen_InTheNameOfGod.pdf

Ariely, D., & Wertenbroch, K. (2002). Procrastination, Deadlines, and Performance: Self-Control by Precommitment. *Psychological Science*, *13*(3), 219–224.

Arruñada, B. (2010). Protestants and Catholics: Similar Work Ethic, Different Social Ethic. *Economic Journal*, *120*(547), 890–918.

Becker, S. O., & Woessmann, L. (2009). Was Weber Wrong? A Human Capital Theory of Protestant Economic History. *Quarterly Journal of Economics*, *124*(2), 531–596.

Bénabou, R., & Tirole, J. (2011). Identity, Morality, and Taboos: Beliefs as Assets. *Quarterly Journal of Economics*, *126*(2), 805–855.

Benjamin, D. J., Choi, J. J., & Strickland, A. J. (2010). Social Identity and Preferences. *American Economic Review*, *100*(4), 1913–28. https://doi.org/10.1257/aer.100.4.1913

Benjamini, Y., & Hochberg, Y. (1995). Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing. *Journal of the Royal Statistical Society: Series B (Methodological)*, *57*(1), 289–300.

Bentzen, J. (2019). Acts of God? Religiosity and Natural Disasters Across Subnational World Districts. *Economic Journal*, *129*(622), 2295–2321.

Beshears, J., Choi, J. J., Laibson, D., Madrian, B. C., & Milkman, K. L. (2015). The Effect of Providing Peer Information on Retirement Savings Decisions. *The Journal of Finance*, *70*(3), 1161–1201. https://doi.org/10.1111/jofi.12258

Bursztyn, L., & Jensen, R. (2015). How Does Peer Pressure Affect Educational Investments? *Quarterly Journal of Economics*, *130*(3), 1329–1367.

Chapman, G. B., Li, M., Colby, H., & Yoon, H. (2010). Opting in vs Opting out of Influenza Vaccination. *Journal of the American Medical Association (JAMA)*, *304*(1), 43. https://doi.org/10.1001/jama.2010.892

Chen, Y., & Li, S. X. (2009). Group Identity and Social Preferences. *American Economic Review*, *99*(1), 431–457.

Chetty, R., Friedman, J. N., Leth-Petersen, S., Nielsen, T. H., & Olsen, T. (2014). Active vs. Passive Decisions and Crowd-Out in Retirement Savings Accounts: Evidence from Denmark. *Quarterly Journal of Economics*, *129*(3), 1141–1219.

Cinnirella, F., Della Lena, S., Manzoni, E., & Panebianco, F. (2026). *God, Guilt, and Giving: Public Good Contribution among Catholics and Protestants* (No. 12414) [Unpublished manuscript], CESifo Working Paper. https://ssrn.com/abstract=6106114

Della Lena, S., Sato, Y., & Zenou, Y. (2026). *Believing and Practicing: How Religion Shapes Human Capital and Growth* (SSRN Working Paper No. No. 6801278) [Unpublished manuscript], Social Science Research Network. https://doi.org/10.2139/ssrn.6801278

DellaVigna, S. (2009). Psychology and Economics: Evidence from the Field. *Journal of Economic Literature*, *47*(2), 315–372.

DellaVigna, S., & Linos, E. (2022). RCTs to Scale: Comprehensive Evidence from Two Nudge Units. *Econometrica*, *90*(1), 81–116.

Falk, A., & Szech, N. (2013). Morals and Markets. *Science*, *340*(6133), 707–711.

Ferraro, P. J., & Price, M. K. (2013). Using Nonpecuniary Strategies to Influence Behavior: Evidence from a Large-Scale Field Experiment. *Review of Economics and Statistics*, *95*(1), 64–73.

Gruber, J. (2004). Pay or Pray? The Impact of Charitable Subsidies on Religious Attendance. *Journal of Public Economics*, *88*(12), 2635–2655. https://doi.org/10.1016/j.jpubeco.2004.03.006

Guiso, L., Sapienza, P., & Zingales, L. (2006). Does Culture Affect Economic Outcomes? *Journal of Economic Perspectives*, *20*(2), 23–48.

Haerpfer, C., Inglehart, R., Moreno, A., Welzel, C., Kizilova, K., Díez-Medrano, J., Lagos, M., Norris, P., Ponarin, E., & Puranen, B. (2022). *World Values Survey: Round Seven—Country-Pooled Datafile* (dataset; Version 5.0.0). dataset. JD Systems Institute & WVSA Secretariat. https://doi.org/10.14281/18241.24

Hollands, G. J., Shemilt, I., Marteau, T. M., Jebb, S. A., Kelly, M. P., Nakamura, R., Suhrcke, M., & Ogilvie, D. (2013). Altering Micro-environments to Change Population Health Behaviour: Towards an Evidence Base for Choice Architecture Interventions. *BMC public health*, *13*(1), 1218.

Iannaccone, L. R. (1998). Introduction to the Economics of Religion. *Journal of Economic Literature*, *36*(3), 1465–1496.

Kaur, S., Kremer, M., & Mullainathan, S. (2015). Self-Control at Work. *Journal of Political Economy*, *123*(6), 1227–1277.

Knudsen, A. S. B. (2024). Those Who Stayed: Selection and Cultural Change in the Age of Mass Migration. *Working Paper*.

Lyubomirsky, S., & Layous, K. (2013). How Do Simple Positive Activities Increase Well-Being? *Current Directions in Psychological Science*, *22*(1), 57–62.

McCleary, R. M., & Barro, R. J. (2006). Religion and Economy. *Journal of Economic Perspectives*, *20*(2), 49–72.

Milkman, K. L., Beshears, J., Choi, J. J., Laibson, D., & Madrian, B. C. (2011). Using Implementation Intentions Prompts to Enhance Influenza Vaccination Rates. *Proceedings of the National Academy of Sciences of the United States of America*, *108*(26), 10415–10420.

Newell, R. G., & Siikamäki, J. (2014). Nudging Energy Efficiency Behavior: The Role of Information Labels. *Journal of the Association of Environmental and Resource Economists*, *1*(4), 555–598.

Norris, P., & Inglehart, R. (2011). *Sacred and Secular: Religion and Politics Worldwide* (2nd). Cambridge University Press.

O'Donoghue, T., & Rabin, M. (1999). Doing It Now or Later. *American Economic Review*, *89*(1), 103–124.

Paley, A., & van de Ven, N. (2023). Crowdsourcing as a Tool for Creating Effective Nudges: An Example for Financial Oversubscription. *Proceedings of the National Academy of Sciences of the United States of America*, *120*(44), e2308129120. https://doi.org/10.1073/pnas.2308129120

Putnam, R. D., & Campbell, D. E. (2010). *American Grace: How Religion Divides and Unites Us*. Simon & Schuster.

Stark, R., & Finke, R. (2000). *Acts of Faith: Explaining the Human Side of Religion* [Chapters 1–2 articulate religion as a fundamental dimension of human identity shaping moral and social life.]. University of California Press.

Thaler, R. H., & Benartzi, S. (2004). Save More Tomorrow: Using Behavioral Economics to Increase Employee Saving. *Journal of Political Economy*, *112*(S1), S164–S187. https://doi.org/10.1086/380085

Thaler, R. H., & Sunstein, C. R. (2008). *Nudge: Improving Decisions About Health, Wealth, and Happiness*. Yale University Press.

Voas, D. (2009). The Rise and Fall of Fuzzy Fidelity in Europe. *European Sociological Review*, *25*(2), 155–168.

Voas, D., & Crockett, A. (2005). Religion in Britain: Neither Believing nor Belonging. *Sociology*, *39*(1), 11–28.

Yoeli, E., Hoffman, M., Rand, D. G., & Nowak, M. A. (2013). Powering Up with Indirect Reciprocity in a Large-Scale Field Experiment on Energy Conservation. *Proceedings of the National Academy of Sciences of the United States of America*, *110*(Suppl 2), 10424–10429.

Acknowledgements

R.M. acknowledges financial support from a 2025 Early Career Research Grant from Monash University (Fund No. 1750891). E.D. acknowledges financial support from the Hellenic Foundation for Research and Innovation (Fund No. BBASP). The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the manuscript.

Author Contributions

E.D., and R.M., conceived and designed the study and developed the survey instrument. E.D. and R.M. supervised data collection. T.S. constructed the analysis dataset and conducted the statistical analysis. R.M. and T.S. prepared the figures and tables. R.M. and T.S. wrote the initial draft of the manuscript. All authors interpreted the results, reviewed and edited the manuscript, and approved the final version.

Competing Interests

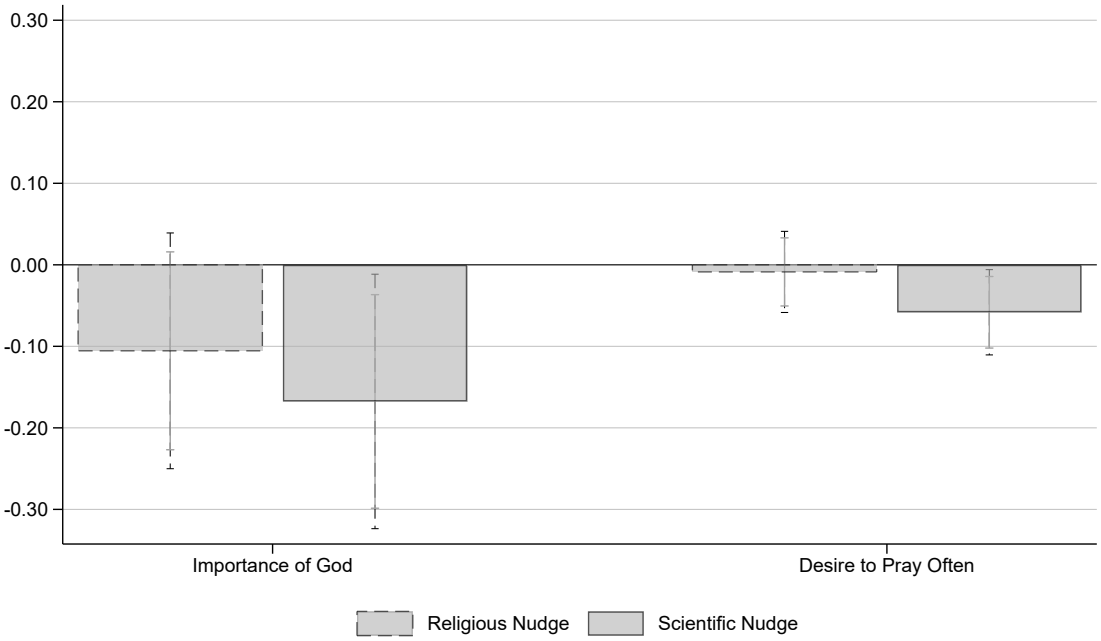
The authors declare no competing interests.

Data Availability Statement

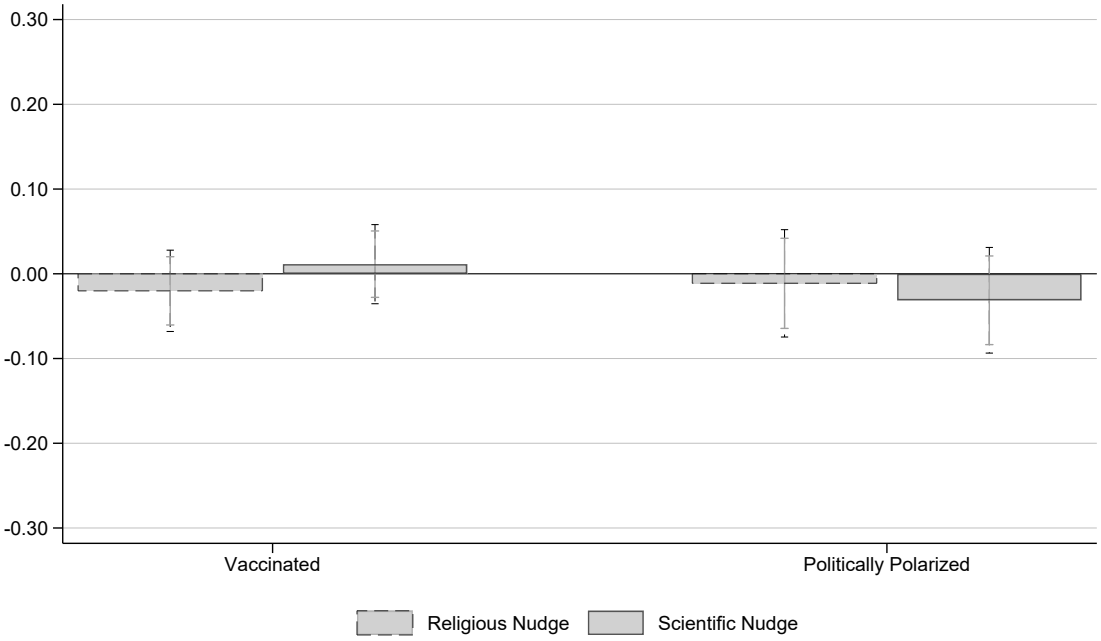
The de-identified participant-level dataset, survey materials, and Stata code required to reproduce the results, tables, and Stata-generated figures reported in this article are available in the following Dropbox folder. The replication materials include the cleaned analysis dataset, the do-files used for data cleaning and analysis, and a README file describing the folder structure and documenting the original survey variables in detail. The pre-registration is available in the AEA RCT Registry (AEARCTR-0016648). The World Values Survey data used in the validation exercise are publicly available from the World Values Survey Association (https://doi.org/10.14281/18241.24). The Lexicon of Greek Personal Names is publicly accessible at http://www.lgpn.ox.ac.uk/, and the registry of Greek Orthodox saints at https://ecclesiagreece.gr/. Participants' first names and prefecture of origin are withheld because they are potentially identifying; the derived religious/secular name classification and non-identifying analysis variables used in the analysis are included in the deposited data.

Figure 2: Treatment and Placebo Effects for Importance of God and Desire to Pray Often

Panel A: Treatment Effects



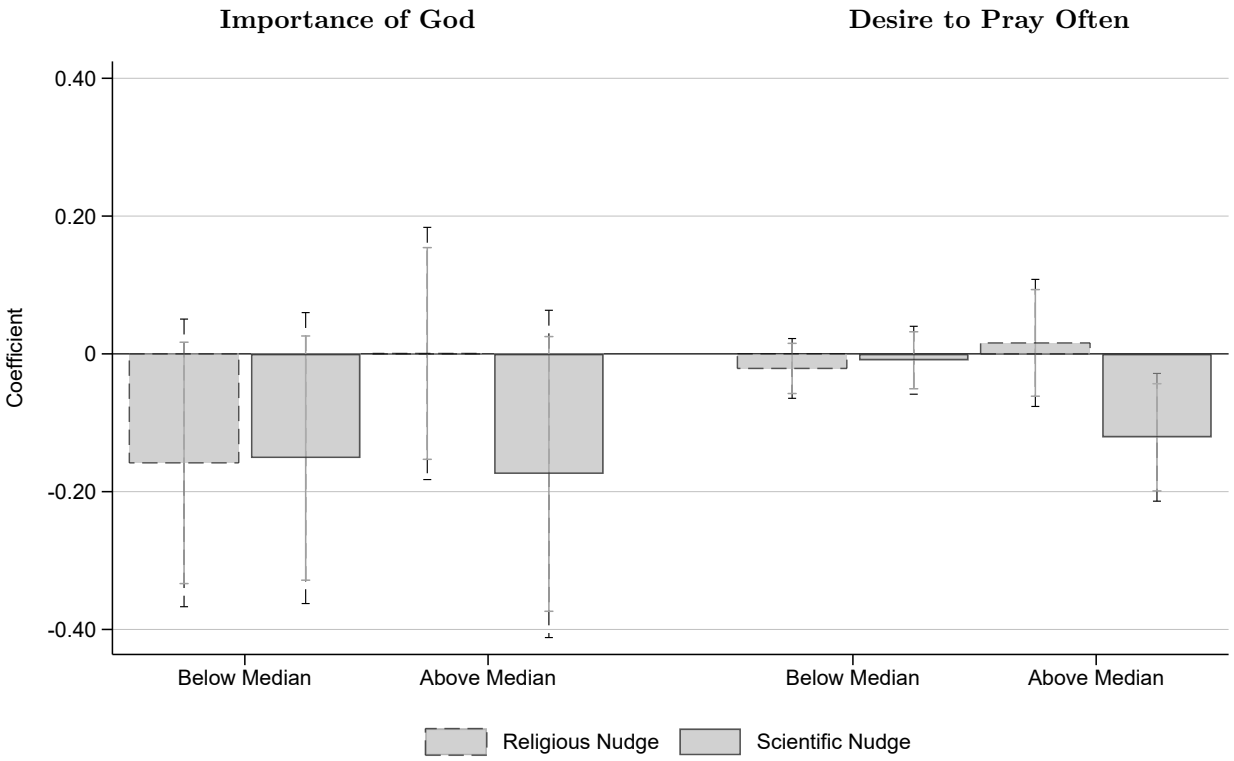
Panel B: Placebo Effects



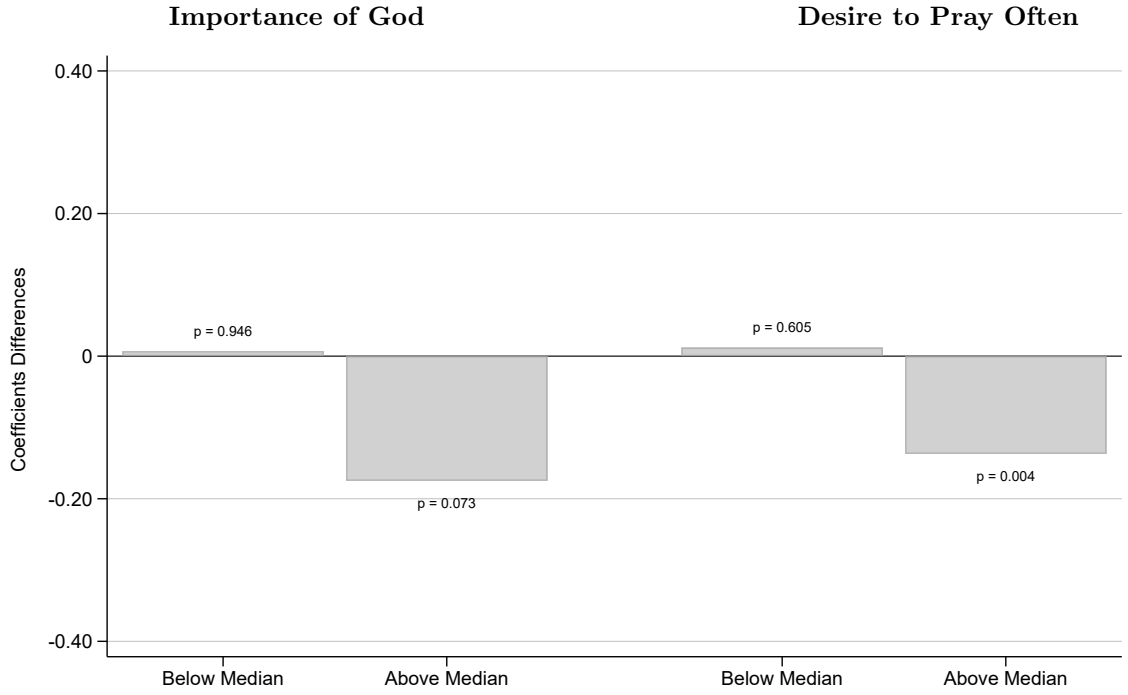
Notes: Panel A reports estimated treatment effects of religious and scientific nudges on two measures of religiosity: The self-reported importance of God and the desire to pray often (an indicator equal to 1 if the respondent reports wanting to pray once or several times per day, and 0 otherwise). Bars plot coefficients from OLS regressions with robust standard errors, controlling for baseline religiosity and demographic covariates. These include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). The omitted category is the control group. Panel B presents the estimated effects from placebo regressions. Placebo outcomes are COVID-19 vaccination status and political polarization. They are unrelated to religiosity and we test whether treatment effects spill over to different domains. Error bars denote 90% (solid gray line) and 95% (dashed black line) confidence intervals with robust standard errors. All estimates are based on the full analytic sample of 1,136 participants.

Figure 3: Heterogeneity by Baseline Religiosity

Panel A: Treatment Effects



Panel B: Net Differences Between Scientific and Religious Nudge



Notes: Panel A reports estimated heterogeneous treatment effects of religious and scientific nudges on two measures of religiosity: The self-reported importance of God and the desire to pray often (an indicator equal to 1 if the respondent reports wanting to pray once or several times per day, and 0 otherwise). We present estimates separately for participants declaring the importance of God measured before the treatment below or above the median. Bars plot coefficients from OLS regressions with robust standard errors, controlling for baseline religiosity and demographic covariates. These include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). The omitted category is the control group. Panel B presents the difference between coefficients for scientific and religious nudges—based on estimates in Panel A. P-values refer to the p-value of the t-test of the difference between the two coefficients. Error bars denote 90% (solid gray line) and 95% (dashed black line) confidence intervals with robust standard errors. All estimates are based on the full analytic sample of 1,136 participants.

Table 1: Participant Demographic Characteristics

	Mean	SD	Min	Max	N
Age (years)	28.44	12.14	18	78	1,136
Survey Completion (min)	8.93	5.29	2	56	1,136
	Percentage		Frequency		N
Gender					1,136
Female	54		609		
Male	45		517		
Undisclosed/Other	1		10		
Education					1,136
High School Diploma/Other	52		591		
BSc	27		310		
MSc	17		189		
PhD	4		46		
Family Income					1,136
Above Median	37		415		
Marital Status					1,136
Single	70		794		
Married/Long-Term Relationship	23		265		
Divorced/Separated/Widow/No Answer	7		77		
Religion					1,136
No Religion	20		226		
Orthodox	75		854		
Other	5		56		
Has a Religious First Name					1,122
Yes	71		793		
Has a Grandparent Name					1,136
Yes	78		889		
Response during a Religious Holiday					1,136
Yes	29		335		
Treatment					1,136
Religious Nudge	34		382		
Scientific Nudge	33		376		
Control	33		378		

Notes: The table presents the demographic characteristics of participants in the sample. Means, standard deviations, and minimum and maximum values are reported for continuous variables; percentages and frequencies are reported for categorical and binary variables. "Has a Grandparent's Name" equals 1 if the participant shares the same first name as a grandparent and 0 otherwise. "Response during a Religious Holiday" indicates whether the survey was completed during Christmas or Easter months and 0 otherwise. "Family Income" indicates whether the family income of the respondent is above the median (20,000 euros).

Table 2: Balance Checks of Covariates

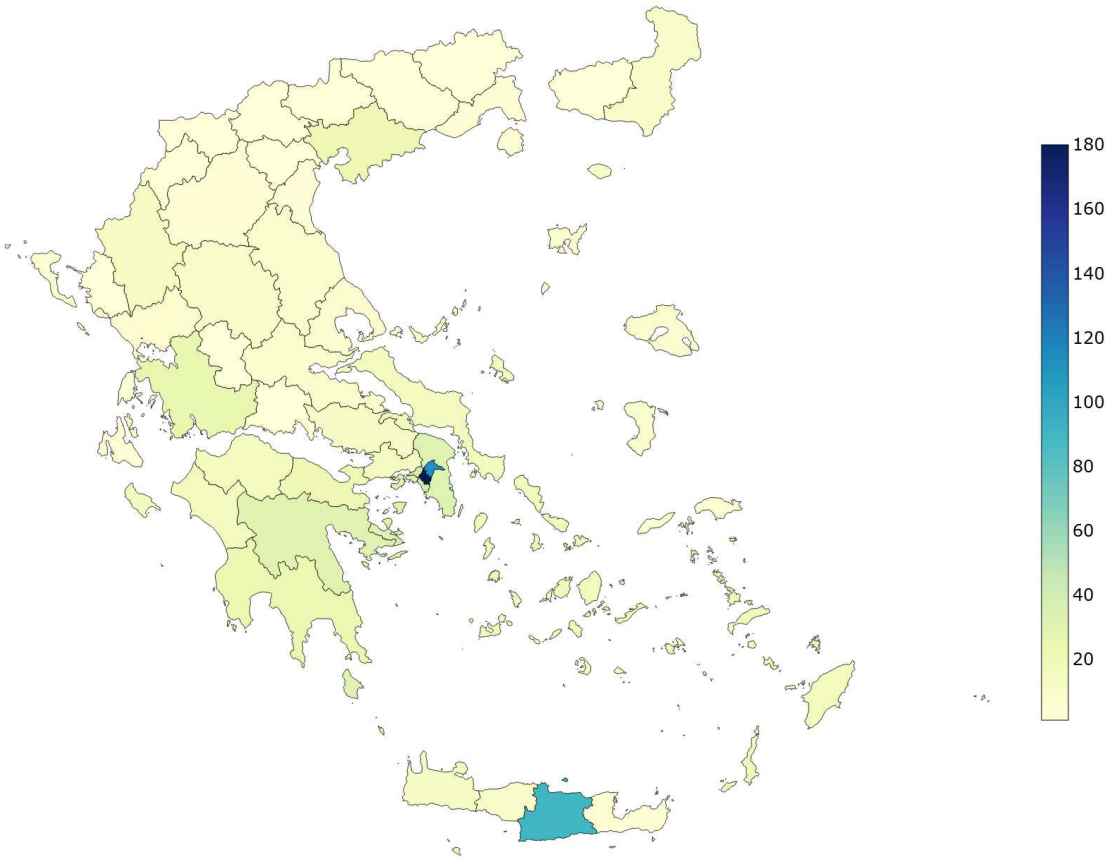
	(1) Religious Nudge Mean	(2) Scientific Nudge Mean	(3) Control Group Mean	(4) Joint p-value	(5) N
Survey Completion (min)	9.23	8.91	8.64	0.341	1,136
Importance of God (baseline)	5.15	5.55	5.17	0.224	1,136
Above Median Age (years)	44.8	45.2	57.4	0.000	1,136
Sex: Female	51.3	54.3	55.3	0.522	1,136
Education: Diploma	53.9	55.6	46.6	0.030	1,136
Education: BSc	27.0	24.5	30.4	0.185	1,136
Education: MSc	15.7	15.7	18.5	0.501	1,136
Education: PhD	3.4	4.3	4.5	0.708	1,136
Marital Status: Single	73.6	70.5	65.6	0.057	1,136
Marital Status: In Relationship	21.2	20.7	28.0	0.036	1,136
Marital Status: Divorced/Separated	5.2	8.8	6.3	0.160	1,136
Religion: None	19.6	17.6	22.5	0.238	1,136
Religion: Orthodox	76.2	77.1	72.2	0.266	1,136
Religion: Other	4.2	5.3	5.3	0.696	1,136
Family Income	35.9	35.1	38.6	0.576	1,136
Political Beliefs: Left/Center-Left	35.1	33.8	37.8	0.497	1,136
Political Beliefs: Center	14.9	13.6	14.8	0.837	1,136
Political Beliefs: Right/Center-Right	22.5	25.8	23.0	0.530	1,136
Political Beliefs: Not Interested	27.0	26.3	24.1	0.629	1,136
Response during Religious Holiday	30.4	26.6	31.5	0.296	1,136
Sample Size	382	376	378		

Notes: The table reports balance in baseline characteristics across the three treatment arms. Columns (1)–(3) report arm-specific means. Survey completion time is measured in minutes and the baseline importance of God on a 0–10 scale; all remaining characteristics are binary indicators, and their means are reported as percentages of the respective arm. Column (4) reports the p-value from an F-test of the joint null that the coefficients on both treatment indicators equal zero, from a regression of the listed characteristic on indicators for the religious and scientific nudges with heteroskedasticity-robust standard errors; this tests equality of means across all three arms. Column (5) reports the number of observations. P-values are not adjusted for the simultaneous testing of 20 characteristics.

Supplementary Appendix

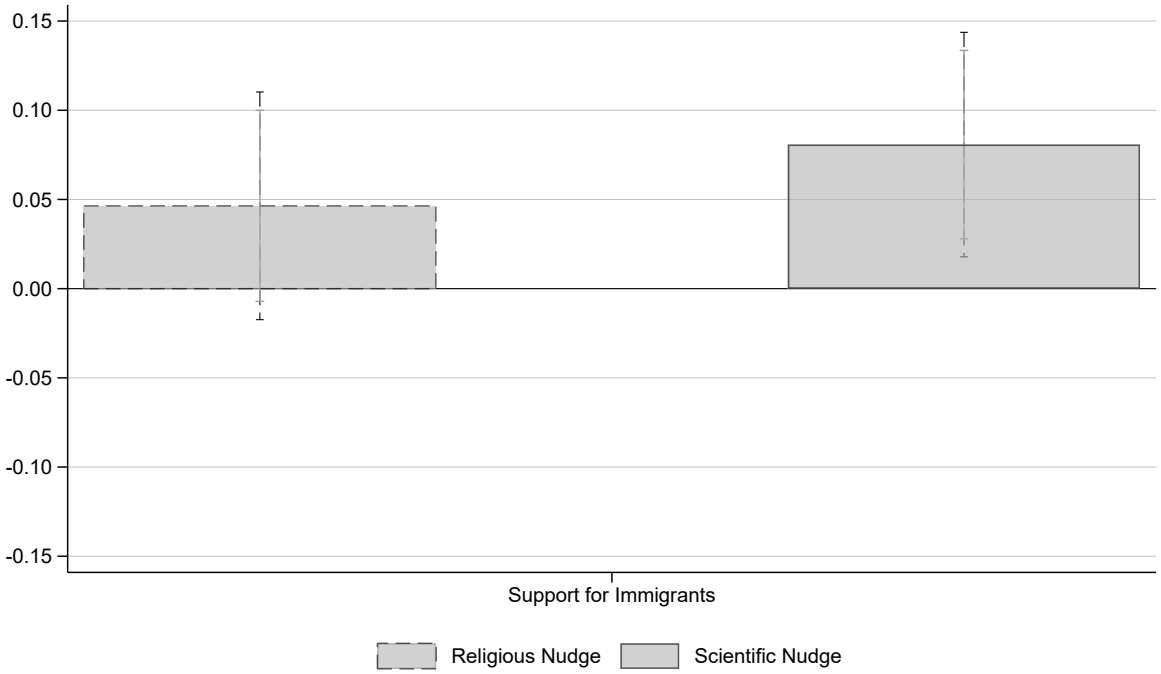
A Additional Figures and Tables

Figure A1: Respondent Spatial Density



Notes: The map shows the density of survey respondents across regions.

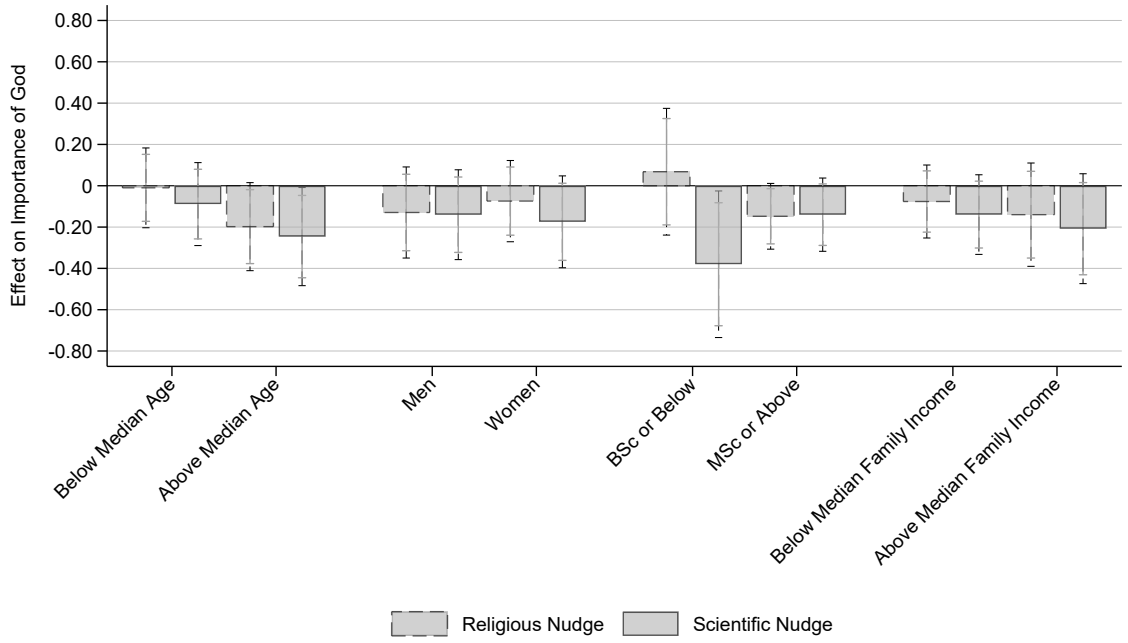
Figure A2: Treatment Effect on Support for Immigrants



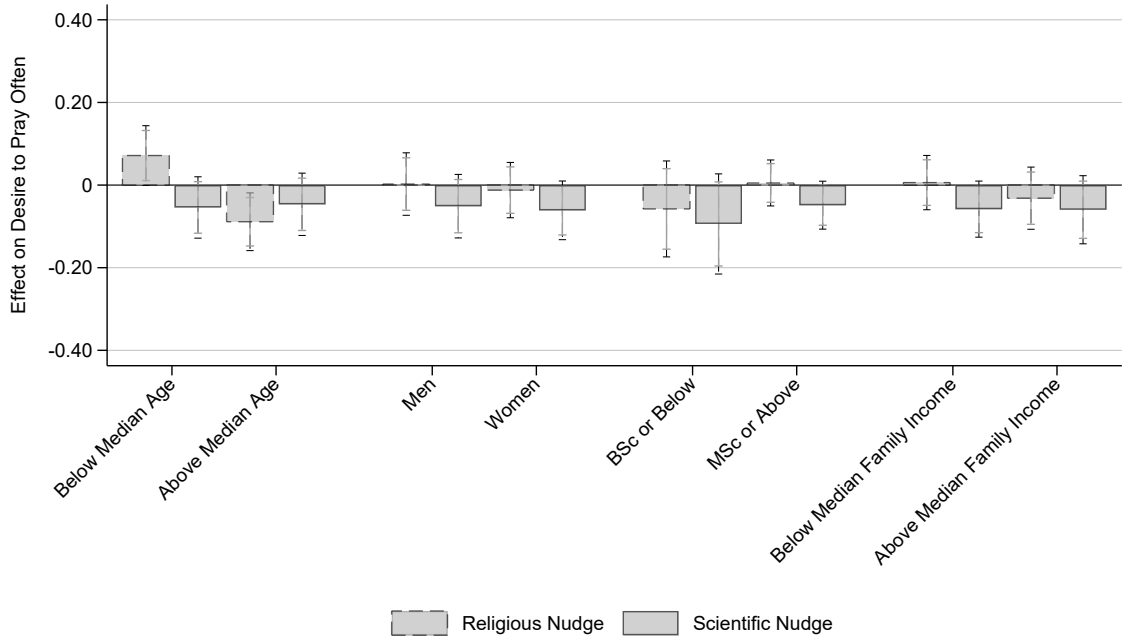
Notes: We report the estimated treatment effects of religious and scientific nudges on support for immigrants (binary). Bars plot coefficients from OLS regressions with robust standard errors, controlling for baseline religiosity and demographic covariates. These include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). The omitted category is the control group. Error bars denote 90% (solid gray line) and 95% (dashed black line) confidence intervals with robust standard errors. All estimates are based on the full analytic sample of 1,136 participants.

Figure A3: Heterogeneity by Demographic Characteristics

Panel A: Effect on the Importance of God



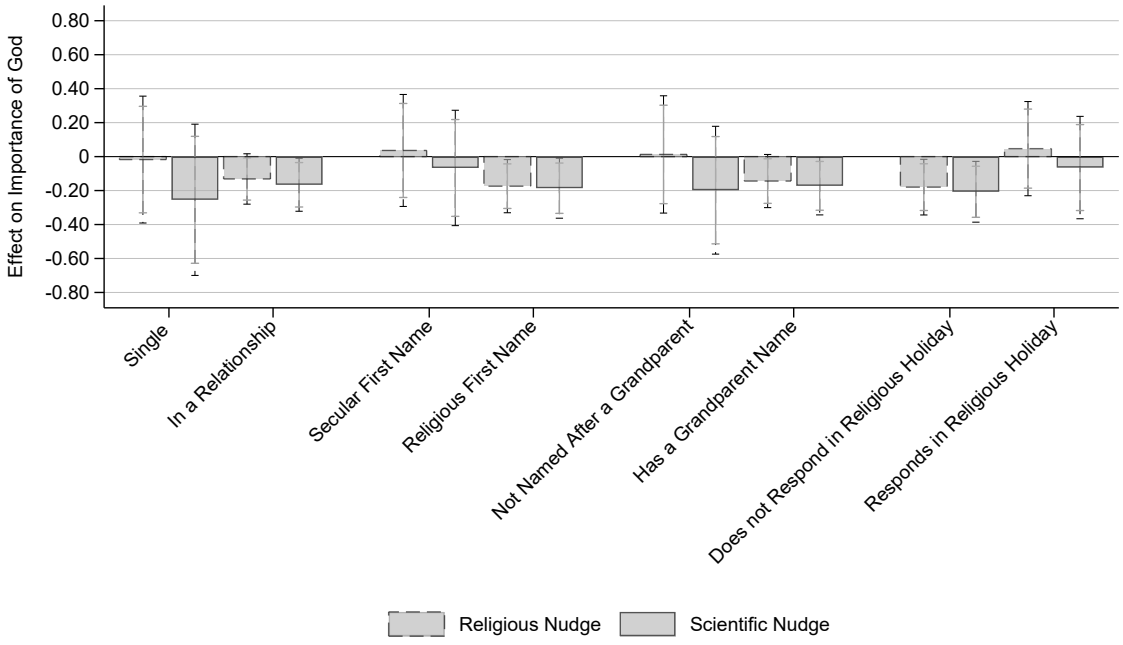
Panel B: Effect on the Desire to Pray Often



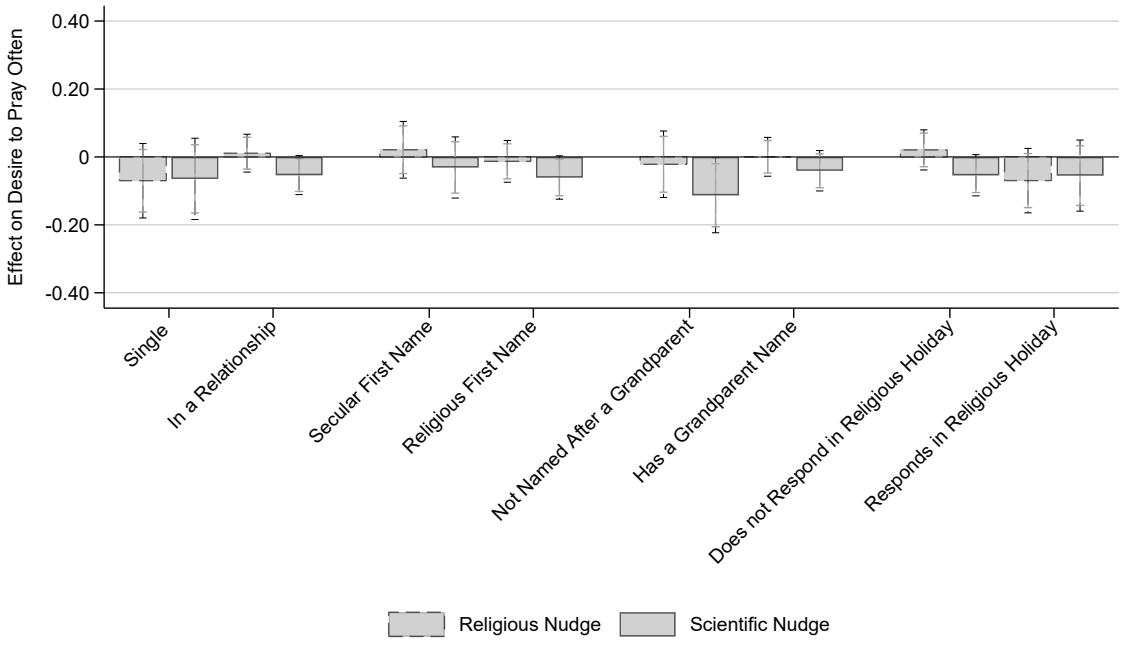
Notes: Panels A and B report estimated heterogeneous treatment effects of religious and scientific nudges on the self-reported importance of God (Panel A) and the desire to pray often (Panel B), respectively. Heterogeneity variables are age (below/above median), gender (male/female), education (below/above BSc), and family income (below/above median). Bars plot coefficients from OLS regressions with robust standard errors, controlling for baseline religiosity and demographic covariates. These include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). The omitted category is the control group. Error bars denote 90% (solid gray line) and 95% (dashed black line) confidence intervals with robust standard errors. All estimates are based on the full analytic sample of 1,136 participants.

Figure A4: Heterogeneity by Demographic Characteristics (continued)

Panel A: Effect on the Importance of God

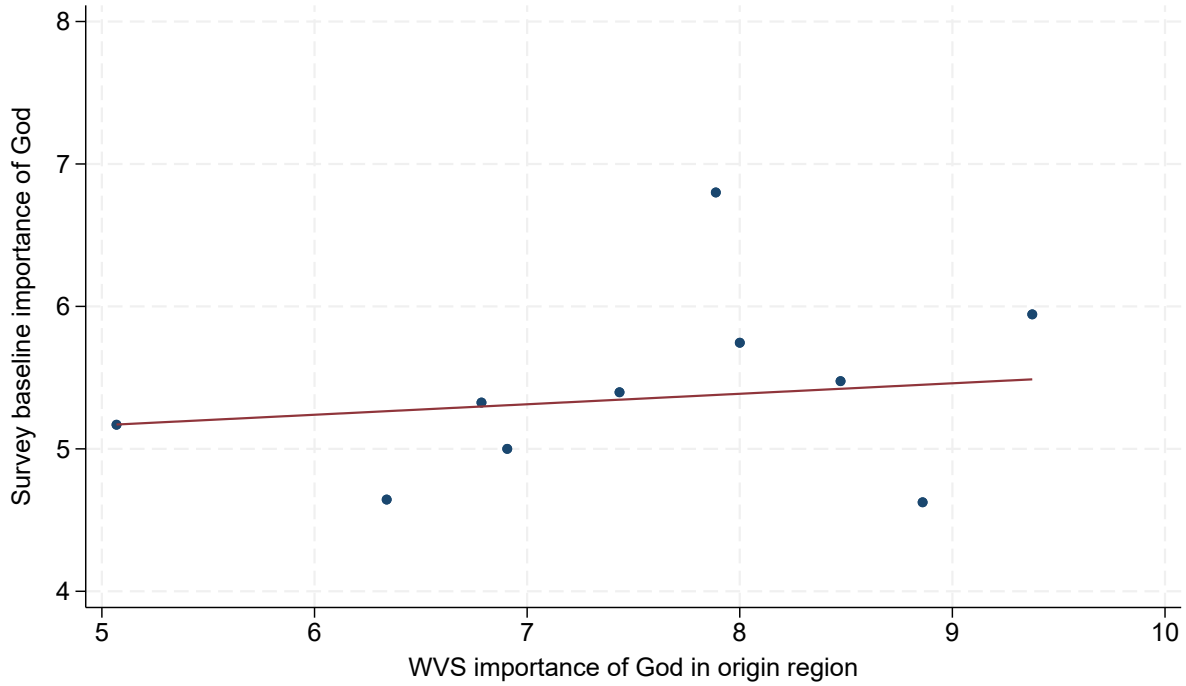


Panel B: Effect on the Desire to Pray Often



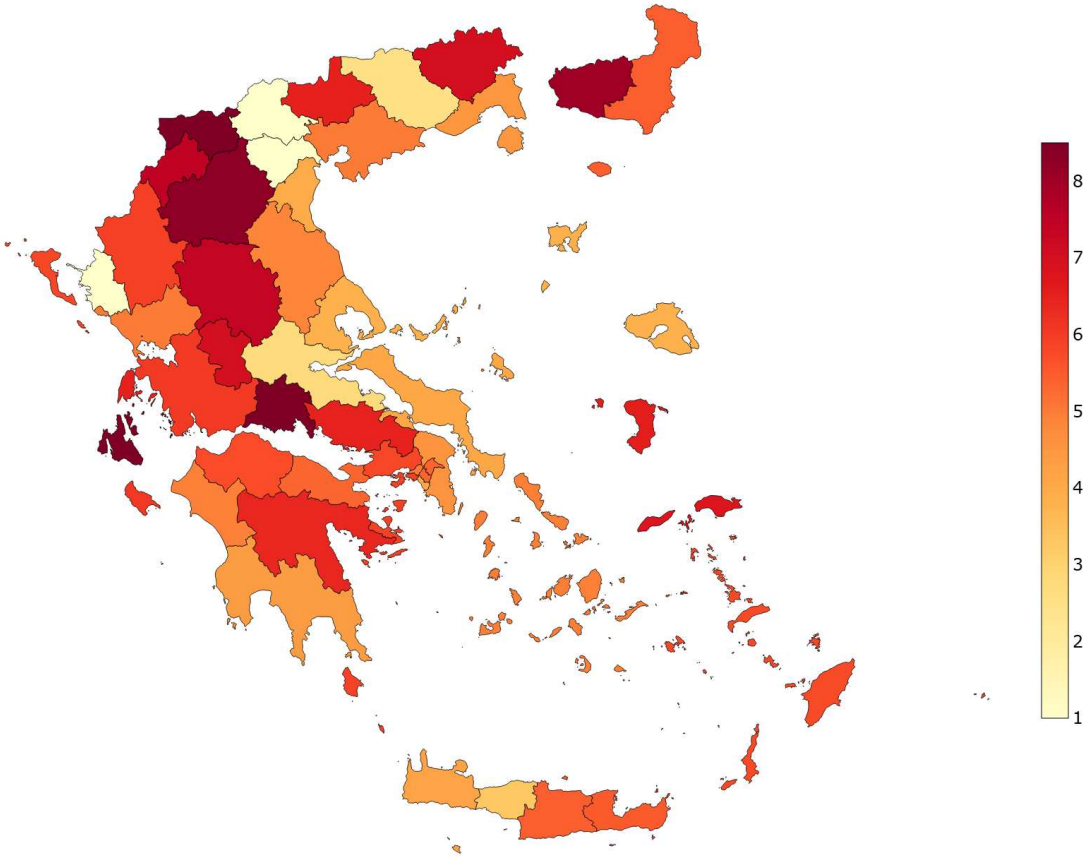
Notes: Panels A and B report estimated heterogeneous treatment effects of religious and scientific nudges on the self-reported importance of God and the desire to pray often, respectively. Heterogeneity variables are marital status (single/in a relationship), whether the participant has a religious first name (no/yes), whether the participant has their grandparent's name (no/yes), and whether the participant responded during the month of a religious holiday (no/yes). Bars plot coefficients from OLS regressions with robust standard errors, controlling for baseline religiosity and demographic covariates. These include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). The omitted category is the control group. Error bars denote 90% (solid gray line) and 95% (dashed black line) confidence intervals with robust standard errors. All estimates are based on the full analytic sample of 1,136 participants.

Figure A5: Correlation between the Religiosity Measure in our Survey and the World Values Survey



Notes: The figure shows the correlation between participants' responses to "How important is God in your life?" in our survey and in the World Values Survey (WVS). Both items are identically worded and use a 0–10 scale (0 = not very important; 10 = very important). Higher values indicate greater perceived importance of God.

Figure A6: Spatial Variation of the Importance of God Based on our Survey



Notes: The map shows the average spatial values of the Importance of God in our survey across regions. Survey items use a 0–10 scale (0 = not very important; 10 = very important).

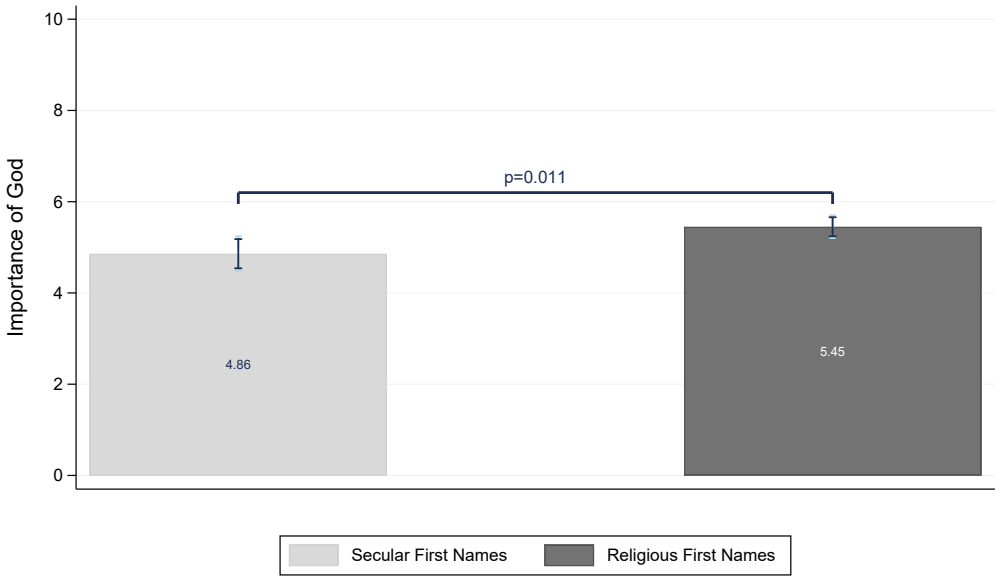
Figure A7: Word Cloud with First Names from our Survey



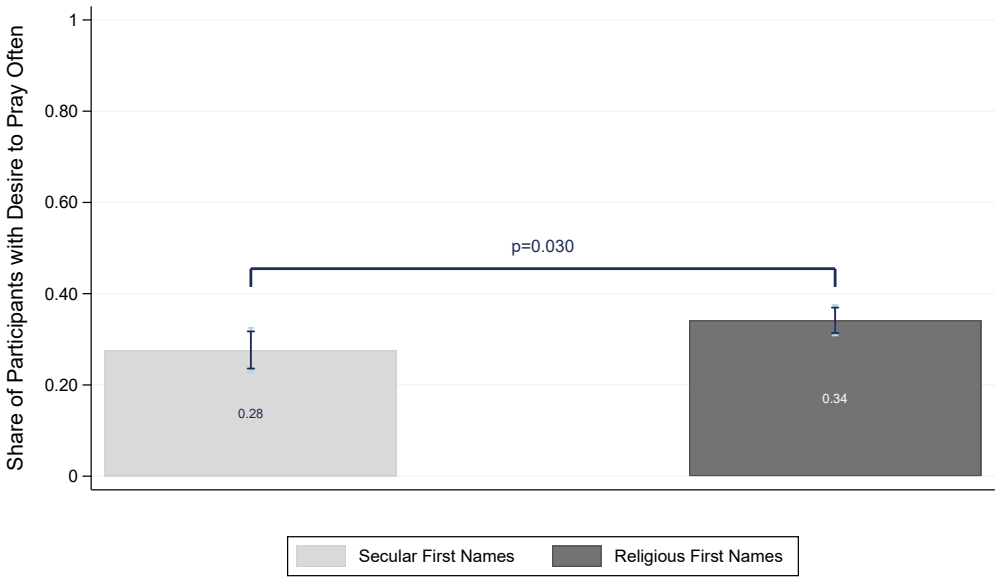
Notes: The figure displays a word cloud of first names from the survey. Name size is proportional to its frequency among respondents.

Figure A8: Survey Self-reported Religiosity and First-name–based Religiosity Based on Epigraphic Studies (LGPN)

Panel A: Importance of God in Life by First-name–based Religiosity



Panel B: Desire to Pray Often by First-name–based Religiosity



Notes: The figure shows mean differences in the importance of God in life measured in the survey (Panel A) and mean differences in the desire to pray often (Panel B) for participants who have a secular first name and those with a religious first name. First names are classified as *religious* (canonically associated with an Orthodox saint; ecclesiagreece.gr) or *secular* (no saint match; identified as Ancient Greek or modern non-Christian via the LGPN epigraphic corpus). Y-axis values are raw scores of the importance of God in life measure in the survey with a range of [0-10] (0 = not very important; 10 = very important). P-values denote significance levels from two sample mean comparison tests.

Table A1: Participant Religiosity, Placebo Outcomes, and Other Attitudinal Measures

	Mean	SD	Min	Max	N
Main Outcomes and Placebo Variables					
Importance of God	5.06	3.68	0	10	1,136
Desire to Pray Often	0.32	0.47	0	1	1,136
Politically Polarized	0.27	0.45	0	1	1,136
Vaccinated for COVID-19	0.86	0.35	0	1	1,136
Willingness to					
Take Revenge	4.47	2.76	0	10	1,136
Punish if YOU Affected	4.76	2.81	0	10	1,136
Punish if OTHER Affected	5.13	2.67	0	10	1,136
Religious Beliefs					
God	0.69	0.46	0	1	1,136
Afterlife	0.48	0.50	0	1	1,136
Hell	0.30	0.46	0	1	1,136
Heaven	0.41	0.49	0	1	1,136
Attitudes towards					
Immigrants	2.70	0.95	1	5	1,136
Homosexuals	2.32	1.35	1	5	1,136
Acceptance of					
Abortion	7.18	3.07	0	10	1,129
Homosexuality	7.74	3.25	0	10	1,117
Premarital Sex	9.19	2.11	0	10	1,131
Religion vs Science	1.79	0.72	1	4	1,126
Religious Openness	1.68	0.76	1	4	1,124

Notes: The table presents descriptive statistics for measures of religiosity, moral attitudes, and prosocial preferences. Variables are measured on 0–10 or 1–5 Likert scales unless otherwise noted. "Religion vs. Science" and "Religious Openness" capture attitudes toward religious versus scientific worldviews and openness to other religions. "Religious Beliefs" include binary indicators for belief in God, afterlife, hell, and heaven. Being politically polarized is defined by someone stating that they are represented by political ideologies of the "left" or of the "right".

Table A2: Descriptives of Religiosity Measures by Treatment Status

Outcome	Treatment Type		
	Religious Nudge	Scientific Nudge	Control
Main Outcomes and Placebo Variables			
Importance of God	4.92	5.24	5.02
	(3.65)	(3.63)	(3.77)
Desire to Pray Often	0.32	0.31	0.34
	(0.47)	(0.46)	(0.47)
Politically Polarized	0.27	0.25	0.30
	(0.45)	(0.43)	(0.46)
Vaccinated for COVID-19	0.84	0.86	0.87
	(0.37)	(0.34)	(0.34)
Willingness to			
Take Revenge	4.49	4.53	4.39
	(2.77)	(2.78)	(2.73)
Punish if YOU Affected	4.76	4.68	4.85
	(2.82)	(2.77)	(2.85)
Punish if OTHER Affected	5.02	5.08	5.28
	(2.70)	(2.60)	(2.72)
Religious Beliefs			
God	0.67	0.71	0.69
	(0.47)	(0.45)	(0.46)
Afterlife	0.50	0.48	0.47
	(0.50)	(0.50)	(0.50)
Hell	0.31	0.30	0.29
	(0.46)	(0.46)	(0.46)
Heaven	0.42	0.42	0.40
	(0.49)	(0.49)	(0.49)
Acceptance of			
Abortion	7.02	7.15	7.38
	(3.15)	(3.00)	(3.06)
Homosexuality	7.91	7.54	7.76
	(3.24)	(3.25)	(3.27)
Premarital Sex	9.14	9.22	9.20
	(2.16)	(2.09)	(2.09)
Attitudes towards			
Immigrants	2.73	2.66	2.72
	(0.95)	(0.89)	(1.00)
Homosexuals	2.26	2.36	2.32
	(1.34)	(1.34)	(1.39)
Religion vs Science	1.75	1.82	1.79
	(0.69)	(0.71)	(0.75)
Religious Openness	1.62	1.73	1.70
	(0.72)	(0.77)	(0.80)
Individuals	382	376	378

Notes: The table reports mean values with standard deviations in parentheses for each outcome, separately by treatment arm (religious message, scientific message, control). All variables are measured post-treatment. Importance of God is measured on a 0–10 scale (0 = not very important; 10 = very important). Desire to pray often, political polarization, COVID-19 vaccination, and the religious-belief items are binary indicators. Willingness to take revenge and to punish, and acceptance of abortion, homosexuality, and premarital sex are measured on 0–10 scales; attitudes towards immigrants and homosexuals on 1–5 scales; religion vs. science and religious openness on 1–4 scales. The final row reports the number of respondents assigned to each arm. Acceptance of abortion, homosexuality, and premarital sex, religion vs. science, and religious openness have a small number of missing responses, so the means for these items are computed on marginally fewer observations; Table A1 reports the sample size for each measure.

Table A3: Balance Checks of Covariates

	(1) Religious Nudge	(2) Scientific Nudge	(3) Control Group	N
Survey Completion (min)	0.004	0.000	-0.003	1,136
	(0.003)	(0.003)	(0.003)	
Importance of God (baseline)	-0.004	0.007*	-0.003	1,136
	(0.004)	(0.004)	(0.004)	
Above Median Age (years)	-0.059**	-0.052*	0.110***	1,136
	(0.028)	(0.028)	(0.028)	
Sex: Female	-0.031	0.009	0.023	1,136
	(0.028)	(0.028)	(0.028)	
Education: Diploma	0.026	0.047*	-0.073***	1,136
	(0.028)	(0.028)	(0.028)	
Education: BSc	-0.006	-0.047	0.053*	1,136
	(0.031)	(0.031)	(0.032)	
Education: MSc	-0.023	-0.023	0.045	1,136
	(0.037)	(0.037)	(0.038)	
Education: PhD	-0.056	0.018	0.038	1,136
	(0.068)	(0.072)	(0.073)	
Marital Status: Single	0.059*	0.009	-0.068**	1,136
	(0.030)	(0.030)	(0.031)	
Marital Status: In Relationship	-0.040	-0.048	0.088***	1,136
	(0.033)	(0.032)	(0.034)	
Marital Status: Divorced/Separated	-0.082	0.105*	-0.023	1,136
	(0.052)	(0.058)	(0.055)	
Religion: None	-0.006	-0.049	0.054	1,136
	(0.035)	(0.034)	(0.036)	
Religion: Orthodox	0.018	0.035	-0.053	1,136
	(0.032)	(0.032)	(0.033)	
Religion: Other	-0.053	0.028	0.026	1,136
	(0.062)	(0.066)	(0.066)	
Family Income	-0.010	-0.020	0.030	1,136
	(0.029)	(0.029)	(0.029)	
Political Beliefs: Left/Center-Left	-0.007	-0.026	0.033	1,136
	(0.029)	(0.029)	(0.029)	
Political Beliefs: Center	0.013	-0.023	0.010	1,136
	(0.040)	(0.039)	(0.040)	
Political Beliefs: Right/Center-Right	-0.023	0.037	-0.014	1,136
	(0.033)	(0.033)	(0.033)	
Political Beliefs: Not Interested	0.021	0.009	-0.030	1,136
	(0.032)	(0.032)	(0.032)	
Response during Religious Holiday	0.014	-0.046	0.032	1,136
	(0.031)	(0.030)	(0.031)	

Notes: Each cell reports the coefficient from a separate regression of an indicator for assignment to the arm shown in the column on the single characteristic shown in the row, with no additional controls, giving 60 estimates in total (20 characteristics $\times$ 3 arms). Because assignment is the dependent variable, each coefficient is the change in the probability of being assigned to that arm associated with a one-unit increase in the characteristic; for binary characteristics it is the difference in assignment probability between respondents with and without the characteristic. There is no omitted category: each column is a separate dependent variable rather than a contrast against a reference arm, and the three coefficients in a row sum to zero by construction. Survey completion time is measured in minutes and the baseline importance of God on a 0–10 scale; all remaining characteristics are binary. Heteroskedasticity-robust standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. P-values are not adjusted for the simultaneous testing of 60 hypotheses.

Table A4: Treatment Effects on Religiosity Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Importance of God				Desire to Pray Often		Desire to Pray	
	Raw		Standardized		Binary		Continuous	
Religious Nudge	-0.082	-0.105	-0.022	-0.029	-0.020	-0.009	-0.035	-0.021
	(0.073)	(0.074)	(0.020)	(0.020)	(0.026)	(0.025)	(0.114)	(0.114)
Scientific Nudge	-0.157**	-0.168**	-0.043**	-0.045**	-0.064**	-0.058**	-0.216*	-0.216*
	(0.080)	(0.080)	(0.022)	(0.022)	(0.027)	(0.027)	(0.116)	(0.116)
Control Mean	5.02	5.02	-0.01	-0.01	0.34	0.34	3.56	3.56
Control SD	3.77	3.77	1.02	1.02	0.47	0.47	2.49	2.49
Individuals	1,136	1,136	1,136	1,136	1,136	1,136	1,136	1,136
Baseline Religiosity	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓

Notes: The table presents OLS estimates of religious and scientific nudges on the importance of God and the desire to pray. Dependent variables are post-treatment measures of the importance of God (raw: 0–10 scale and standardized: mean = 0 and SD = 1) and desire to pray (desire to pray often: binary indicator, = 1 if participants pray once or more than once a day, and 0 otherwise; raw: original Likert-scale measure of prayer frequency). The omitted category is the control group, which received no nudge. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table A5: Effect of the Treatment on Praying Frequency, Logit Model

	Desire to Pray Often	
	(1)	(2)
Religious Nudge	-0.007	-0.006
	(0.025)	(0.024)
Scientific Nudge	-0.053**	-0.057**
	(0.025)	(0.026)
Mean	0.34	0.34
SD	0.47	0.47
Individuals	1,136	1,136
Baseline Religiosity	✓	✓
Controls		✓

Notes: The table presents average marginal effects from logit models of the religious and scientific nudges on the desire to pray often. The omitted category is the control group. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table A6: Robustness: Effect of the Treatment on the Importance of God and Praying Frequency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Importance of God				Desire to Pray Often		Desire to Pray	
	Raw		Standardized		Binary		Continuous	
	Panel A: Only Respondents who Believe in God							
Religious Nudge	-0.087	-0.111	-0.024	-0.030	-0.014	-0.006	-0.006	0.011
	(0.095)	(0.096)	(0.026)	(0.026)	(0.036)	(0.036)	(0.157)	(0.157)
Scientific Nudge	-0.224**	-0.251**	-0.061**	-0.068**	-0.077**	-0.070*	-0.233	-0.224
	(0.101)	(0.104)	(0.028)	(0.028)	(0.036)	(0.036)	(0.156)	(0.155)
Control Mean	6.98	6.98	0.52	0.52	0.48	0.48	4.56	4.56
Control SD	2.73	2.73	0.74	0.74	0.50	0.50	2.30	2.30
Individuals	786	786	786	786	786	786	786	786
	Panel B: Only Respondents who Are Orthodox							
Religious Nudge	-0.096	-0.122	-0.026	-0.033	-0.010	-0.003	-0.012	0.004
	(0.091)	(0.090)	(0.025)	(0.024)	(0.033)	(0.033)	(0.143)	(0.144)
Scientific Nudge	-0.171*	-0.195*	-0.046*	-0.053*	-0.069**	-0.065*	-0.223	-0.222
	(0.101)	(0.101)	(0.027)	(0.027)	(0.034)	(0.034)	(0.145)	(0.145)
Control Mean	6.38	6.38	0.36	0.36	0.44	0.44	4.30	4.30
Control SD	3.17	3.17	0.86	0.86	0.50	0.50	2.39	2.39
Individuals	854	854	854	854	854	854	854	854
Baseline Religiosity	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓

Notes: The table presents OLS estimates of treatment effects on religiosity under alternative sample restrictions. Panel A restricts the sample to respondents who state that they believe in God, while Panel B restricts the sample to respondents who identify as Orthodox. Dependent variables are post-treatment measures of the importance of God (raw: 0–10 scale; standardized: mean = 0 and SD = 1) and desire to pray (desire to pray often: binary indicator equal to 1 if respondents report praying once or more than once a day, and 0 otherwise; raw: original ordinal measure of prayer frequency). The omitted category is the control group. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/Orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table A7: Placebo Check: Effect of Treatments on Probability of Being Vaccinated and Political Ideology

	(1)	(2)	(3)	(4)
	Vaccination		Politically Polarized	
Religious Nudge	-0.036	-0.020	-0.024	-0.011
	(0.025)	(0.024)	(0.032)	(0.032)
Scientific Nudge	0.001	0.011	-0.041	-0.031
	(0.024)	(0.024)	(0.032)	(0.032)
Mean	0.88	0.88	0.29	0.29
SD	0.33	0.33	0.45	0.45
Individuals	1,136	1,136	1,136	1,136
Baseline Religiosity	✓	✓	✓	✓
Controls		✓		✓

Notes: The table presents OLS estimates of the religious and scientific nudges on vaccination status and a binary indicator for being politically polarized. Dependent variables are post-treatment binary variables for whether the respondent is vaccinated against COVID-19 (takes the value of 1 if the participant is vaccinated and 0 otherwise) and whether the respondent is politically polarized (takes the value of 1 if the participant is politically polarized and 0 otherwise). The omitted category is the control group. "Baseline Religiosity" is the importance of God measured before the treatment was administered and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table A8: Effect of the Treatment on Support for Immigrants, Willingness to Take Revenge, and Religious Beliefs

Panel A: Effect on Support for Immigrants and Willingness to Take Revenge								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Support for		Willingness to					
	Immigrants		Take Revenge		Punish (Self)		Punish (Others)	
Religious Nudge	0.029	0.046	0.100	-0.045	-0.093	-0.213	-0.261	-0.274
	(0.033)	(0.033)	(0.199)	(0.196)	(0.206)	(0.205)	(0.197)	(0.198)
Scientific Nudge	0.070**	0.081**	0.155	0.073	-0.154	-0.242	-0.180	-0.206
	(0.033)	(0.032)	(0.201)	(0.199)	(0.205)	(0.203)	(0.193)	(0.195)
Mean	0.35	0.35	4.39	4.39	4.85	4.85	5.28	5.28
SD	0.48	0.48	2.73	2.73	2.85	2.85	2.72	2.72
Individuals	1,136	1,136	1,136	1,136	1,136	1,136	1,136	1,136
Panel B: Effect on Supernatural Beliefs								
	Belief in the Existence of							
	God		Afterlife		Hell		Heaven	
Religious Nudge	-0.021	-0.038*	0.033	0.022	0.019	0.010	0.026	0.011
	(0.021)	(0.020)	(0.032)	(0.032)	(0.028)	(0.028)	(0.027)	(0.027)
Scientific Nudge	-0.018	-0.029	-0.008	-0.009	-0.023	-0.025	-0.009	-0.016
	(0.022)	(0.020)	(0.033)	(0.033)	(0.029)	(0.029)	(0.028)	(0.028)
Mean	0.69	0.69	0.47	0.47	0.29	0.29	0.40	0.40
SD	0.46	0.46	0.50	0.50	0.46	0.46	0.49	0.49
Individuals	1,136	1,136	1,136	1,136	1,136	1,136	1,136	1,136
Baseline Religiosity	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓

Notes: This table presents OLS estimates of treatment effects on attitudes. Dependent variables are post-treatment measures of support for immigrants (binary, takes value one if attitudes towards immigrants are positive or very positive), willingness to take revenge (0-10), willingness to punish someone as a consequence of damage to yourself (0-10), willingness to punish someone as a consequence of damage to someone else (0-10), and whether the respondent believes in God (binary), the afterlife (binary), hell (binary), and Heaven (binary). The omitted category is the control group. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table A9: Treatment Effect on Attitudes Toward Civil Rights and Religious Openness

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect on Support for Homosexual Marriage, Abortion, and Premarital Sex						
	Acceptance of					
	Homosexuality		Abortion		Premarital Sex	
Religious Nudge	0.166	0.132	-0.361*	-0.331*	-0.058	-0.097
	(0.213)	(0.199)	(0.206)	(0.200)	(0.146)	(0.145)
Scientific Nudge	-0.063	-0.083	-0.079	-0.029	0.092	0.065
	(0.214)	(0.198)	(0.201)	(0.193)	(0.147)	(0.148)
Mean	7.76	7.76	7.38	7.38	9.20	9.20
SD	3.27	3.27	3.06	3.06	2.09	2.09
Individuals	1,117	1,117	1,129	1,129	1,131	1,131
Panel B: Effect on Support for Homosexuals, Religion over Science, and Religious Openness						
	Support for		Religious Openness			
	Same Sex Marriage		vs Science		vs Other Religions	
Religious Nudge	0.006	0.010	-0.029	-0.020	-0.070	-0.037
	(0.032)	(0.030)	(0.047)	(0.048)	(0.052)	(0.050)
Scientific Nudge	0.001	0.006	0.003	0.005	0.004	0.020
	(0.032)	(0.031)	(0.049)	(0.049)	(0.054)	(0.051)
Mean	0.61	0.61	1.79	1.79	1.70	1.70
SD	0.49	0.49	0.75	0.75	0.80	0.80
Individuals	1,136	1,136	1,126	1,126	1,124	1,124
Panel C: Effect on Church Attendance, Social Comparison Tendencies, and Long-term Thinking						
	Church Attendance		Social Comparison		Long-term Thinking	
Religious Nudge	0.035	0.005	0.383	0.221	0.392	0.378
	(0.094)	(0.092)	(0.314)	(0.313)	(0.259)	(0.251)
Scientific Nudge	0.119	0.101	0.574*	0.324	-0.007	-0.053
	(0.095)	(0.094)	(0.331)	(0.321)	(0.253)	(0.249)
Control Mean	2.89	2.89	4.99	4.99	6.80	6.80
Control SD	1.63	1.63	2.70	2.70	2.16	2.16
Individuals	1,136	1,136	1,136	1,136	1,136	1,136
Baseline Religiosity	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓

Notes: This table presents OLS estimates of treatment effects on attitudes and related outcomes. Panel A reports post-treatment measures of how justified respondents consider homosexuality (0–10), abortion (0–10), and premarital sex (0–10), as well as support for homosexuals (binary, taking value 1 if attitudes toward homosexuals are positive or very positive). Panel B reports agreement with the statements "When Religion and Science are in conflict, I trust religion" and "I think my religion is the only acceptable religion" (1–4, from strongly disagree to strongly agree). Panel C reports church attendance apart from weddings and funerals (ranging from never/almost never to more than once a week), the extent to which respondents compare themselves to others when making choices (0–10), and willingness to sacrifice today to benefit in the future relative to others (0–10). The omitted category is the control group. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/Orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table A10: Heterogeneity by Baseline Religiosity

	(1)	(2)	(3)	(4)
	Importance of God		Desire to Pray Often	
	Below Median	Above Median	Below Median	Above Median
Religious Nudge	-0.158	0.001	-0.021	0.016
	(0.106)	(0.093)	(0.022)	(0.047)
Scientific Nudge	-0.151	-0.174	-0.009	-0.121**
	(0.108)	(0.121)	(0.025)	(0.047)
Control Mean	2.18	8.54	0.07	0.67
Control SD	2.38	1.55	0.26	0.47
Individuals	629	507	629	507
Baseline Religiosity	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: This table presents OLS estimates of treatment effects on religiosity and attitudes. Dependent variables are post-treatment measures of the importance of God (0–10 scale), and desire to pray often (binary). The heterogeneity variable indicates whether the baseline importance of God, measured before the treatment was administered, is below or above the sample median. The omitted category is the control group. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels

Table A11: Heterogeneity by Baseline Religiosity

	(1)	(2)	(3)	(4)
	Importance of God		Desire to Pray Often	
Religious Nudge × Baseline Religiosity	0.023	0.024	0.007	0.006
	(0.015)	(0.015)	(0.006)	(0.006)
Scientific Nudge × Baseline Religiosity	0.006	0.007	-0.006	-0.008
	(0.018)	(0.018)	(0.006)	(0.006)
Control Mean	5.02	5.02	0.34	0.34
Control SD	3.77	3.77	0.47	0.47
Individuals	1,136	1,136	1,136	1,136
Baseline Religiosity	✓	✓	✓	✓
Controls		✓		✓

Notes: This table presents OLS estimates of treatment effects on religiosity and attitudes. Dependent variables are post-treatment measures of the importance of God (0–10 scale), and desire to pray often (binary). The heterogeneity variable is the importance of God, which was measured before the treatment. The omitted category is the control group. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels

Table A12: Heterogeneity by Demographic Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Median Age		Gender		BSc Education		Family Income	
	Below	Above	Male	Female	Below	Above	Below	Above
Panel A: Importance of God								
Religious Nudge	-0.010	-0.198*	-0.130	-0.074	-0.148*	0.068	-0.076	-0.140
	(0.099)	(0.109)	(0.113)	(0.100)	(0.081)	(0.156)	(0.090)	(0.128)
Scientific Nudge	-0.089	-0.246**	-0.140	-0.175	-0.140	-0.380**	-0.140	-0.208
	(0.102)	(0.121)	(0.111)	(0.113)	(0.090)	(0.181)	(0.098)	(0.136)
Control Mean	4.91	5.11	5.04	5.12	5.08	4.83	5.25	4.66
Control SD	3.51	3.96	3.71	3.80	3.75	3.86	3.68	3.90
Individuals	578	558	517	609	901	235	721	415
Panel B: Desire to Pray Often								
Religious Nudge	0.072*	-0.089**	0.002	-0.012	0.005	-0.058	0.006	-0.032
	(0.037)	(0.036)	(0.039)	(0.034)	(0.028)	(0.059)	(0.034)	(0.038)
Scientific Nudge	-0.054	-0.047	-0.051	-0.061*	-0.049	-0.094	-0.058*	-0.060
	(0.038)	(0.038)	(0.039)	(0.036)	(0.030)	(0.062)	(0.035)	(0.042)
Control Mean	0.28	0.39	0.29	0.38	0.33	0.38	0.35	0.33
Control SD	0.45	0.49	0.46	0.49	0.47	0.49	0.48	0.47
Individuals	578	558	517	609	901	235	721	415
Baseline Religiosity	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓

Notes: This table presents OLS estimates of treatment effects on religiosity and attitudes. Dependent variables are post-treatment measures of the importance of God (0–10 scale) (Panel A) and desire to pray often (binary) (Panel B). Heterogeneity variables are age (below/above median), gender (male/female), education (below/above BSc), and family income (below/above median). The omitted category is the control group. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table A13: Heterogeneity by Demographic Characteristics (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Marital Status		Religious First Name		Has Grandparent Name		Response in Religious Holiday	
	Relationship	Single	No	Yes	No	Yes	No	Yes
Panel A: Importance of God								
Religious Nudge	-0.018	-0.132*	0.036	-0.174**	0.013	-0.144*	-0.180**	0.047
	(0.191)	(0.076)	(0.168)	(0.080)	(0.176)	(0.080)	(0.084)	(0.141)
Scientific Nudge	-0.254	-0.166**	-0.067	-0.186**	-0.198	-0.172**	-0.207**	-0.065
	(0.227)	(0.080)	(0.173)	(0.090)	(0.192)	(0.087)	(0.091)	(0.154)
Control Mean	6.09	4.61	4.40	5.23	4.76	5.10	4.62	5.91
Control SD	3.77	3.70	3.73	3.77	3.86	3.75	3.76	3.65
Individuals	265	871	329	793	247	889	801	335
Panel B: Desire to Pray Often								
Religious Nudge	-0.070	0.011	0.021	-0.013	-0.022	0.000	0.021	-0.070
	(0.056)	(0.028)	(0.043)	(0.031)	(0.050)	(0.029)	(0.030)	(0.048)
Scientific Nudge	-0.065	-0.053*	-0.031	-0.061*	-0.113**	-0.041	-0.054*	-0.055
	(0.061)	(0.029)	(0.046)	(0.033)	(0.056)	(0.030)	(0.031)	(0.053)
Control Mean	0.44	0.30	0.25	0.37	0.33	0.34	0.30	0.43
Control SD	0.50	0.46	0.44	0.48	0.47	0.48	0.46	0.50
Individuals	265	871	329	793	247	889	801	335
Baseline Religiosity	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓

Notes: This table presents OLS estimates of treatment effects on religiosity and attitudes. Dependent variables are post-treatment measures of the importance of God (0–10 scale) (Panel A) and desire to pray often (binary) (Panel B). Heterogeneity variables are marital status (single, separated, divorced, widowed/married, in a relationship), whether the participant has a religious first name (no/yes), whether the participant has their grandparents name (no/yes), and whether the participant responded during the month of a religious holiday (no/yes). The omitted category is the control group. "Baseline Religiosity" is the baseline raw importance of God (measured at the beginning of the survey and before the treatment was administered) and is included in all specifications. "Controls" include respondent age (years) and indicators for respondent gender (male/female/other), education (high school/BSc/MSc/PhD), relationship status (single/married or long-term relationship/other), family income (above/below median), and religion (no religion/orthodox/other). Robust standard errors are reported. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table A14: Multiple-Hypothesis Testing for Treatment-Effect Heterogeneity

	Religious Nudge vs. Control		Scientific Nudge vs. Control		Scientific vs. Religious Nudge	
Heterogeneity Dimension	Unadjusted p-value	BH q-value	Unadjusted p-value	BH q-value	Unadjusted p-value	BH q-value
Importance of God						
Age Above Median	0.182	0.798	0.344	0.798	0.834	0.977
Female	0.517	0.853	0.846	0.977	0.655	0.914
Below Bachelor's Degree	0.319	0.798	0.346	0.798	0.058*	0.700
Family Income Above Median	0.639	0.913	0.624	0.913	0.961	0.977
Single	0.592	0.888	0.875	0.977	0.502	0.853
Religious First Name	0.271	0.798	0.724	0.965	0.384	0.798
Has Grandparent's Name	0.349	0.798	0.941	0.977	0.289	0.798
Response During Religious Holiday	0.170	0.798	0.410	0.798	0.590	0.888
Baseline Religiosity Above Median	0.207	0.798	0.914	0.977	0.288	0.798
Baseline Religiosity (continuous)	0.120	0.798	0.716	0.965	0.329	0.798
Desire to Pray Often						
Age Above Median	0.002***	0.072*	0.881	0.977	0.002***	0.072*
Female	0.898	0.977	0.941	0.977	0.994	0.994
Below Bachelor's Degree	0.362	0.798	0.472	0.846	0.845	0.977
Family Income Above Median	0.413	0.798	0.848	0.977	0.397	0.798
Single	0.209	0.798	0.885	0.977	0.253	0.798
Religious First Name	0.480	0.846	0.526	0.853	0.930	0.977
Has Grandparent's Name	0.541	0.854	0.191	0.798	0.426	0.800
Response During Religious Holiday	0.102	0.798	0.961	0.977	0.142	0.798
Baseline Religiosity Above Median	0.403	0.798	0.089*	0.798	0.012**	0.240
Baseline Religiosity (continuous)	0.332	0.798	0.180	0.798	0.030**	0.449

Notes: This table reports omnibus tests of treatment-effect heterogeneity across the listed outcomes and heterogeneity dimensions. The first two columns test whether the effect of the religious nudge differs across values or categories of the corresponding heterogeneity variable, relative to the control group. The third and fourth columns report the analogous tests for the scientific nudge relative to the control group. The final two columns test whether the difference between the scientific and religious nudges varies across the corresponding heterogeneity dimension, with the religious nudge serving as the omitted treatment group. For categorical heterogeneity variables, each entry is based on a joint test that all relevant treatment-by-heterogeneity interaction terms are equal to zero. For continuous baseline religiosity, the reported test concerns the corresponding treatment-by-baseline-religiosity interaction coefficient. Benjamini–Hochberg (BH) q-values adjust for multiple testing across the full family of heterogeneity tests reported in the table. Stars are based separately on the corresponding unadjusted p-value or adjusted q-value: ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

B Appendix: Survey Structure and Questionnaire (English Translation)

Block 1: Baseline Respondent Characteristics

1. Participation consent.
   - Agree
   - Disagree
2. What is your gender?
   - Man
   - Woman
   - Non-binary
   - Prefer not to say
3. What is your year of birth?
   (Dropdown selection: e.g., 1985)
4. This means you are _____ years old.
   (Text entry: age in two digits.)
5. What is your marital status?
   - Single
   - Married
   - Cohabiting
   - Divorced
   - Separated
   - Widowed
   - Prefer not to say
6. What is the highest education level you have attained?
   - Some primary school grades
   - Primary school certificate
   - 3-year middle-school certificate
   - Secondary school certificate (high-school diploma)
   - Institute of Vocational Training (IEK)
   - Diploma from Technological Educational Institute (TEI)
   - Studies for at least 1 year at university, but no degree
   - University degree
   - Master's degree
   - PhD
   - Prefer not to say
7. What is your city of residence (e.g., Kallithea, Attica)?
   (Text entry.)
8. What is your father's prefecture of origin?
   (Dropdown selection.)
9. What is your mother's prefecture of origin?
   (Dropdown selection.)

10. What is your city/town of origin (e.g., Piraeus, Attica)?
    (Text entry.)

11. What is your annual net household income (approximately)?
    - €0–9,999
    - €10,000–19,999
    - €20,000–29,999
    - €30,000–39,999
    - €40,000–49,999
    - €50,000–59,999
    - €60,000–69,999
    - €70,000+
    - Don't know

12. What is your formal first name (for example, report Eleni instead of Lena, i.e., avoid nicknames)?
    (Text entry.)

13. Is your first name the same as your grandfather's or grandmother's?
    - Yes
    - No

14. If no was selected in Question 13: What was your grandparent's first name?
    (Text entry.)

15. Do you belong to a religion? If yes, which?
    - None
    - Roman Catholic
    - Protestant
    - Orthodox
    - Jewish
    - Muslim
    - Hindu
    - Buddhist
    - Prefer not to answer

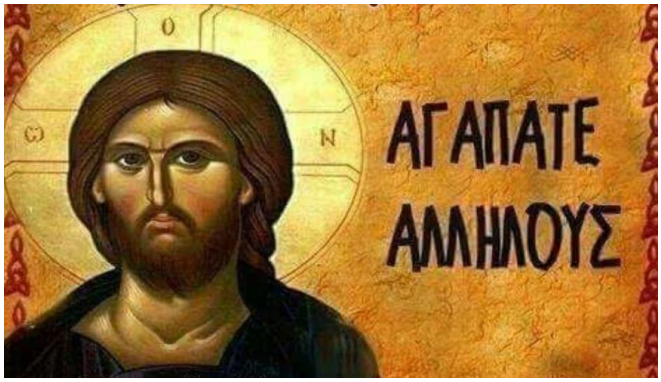
16. How important is God in your life?
    Slider: Scale from 0 (not very important) to 10 (very important).

17. It is difficult for me to forgive others for their mistakes.
    Slider: Scale from 0 (not very difficult) to 10 (very difficult).

Block 2: Randomized Treatment

Participants were randomly assigned to one of three experimental conditions: A religious nudge, a scientific nudge, or a control condition with no nudge. Participants assigned to either treatment were shown the corresponding image and message below. Participants assigned to the control condition proceeded directly to the subsequent questions.

(a) Religious Nudge



Jesus Christ taught us to:

- Love one another (John 13:35)
- Love your enemies (Luke 6:27–28)

(b) Scientific Nudge



"Research from the University of California concludes that people can increase their happiness through simple positive activities, such as expressing gratitude or interacting with other people (Lyubomirsky & Layous, 2013)."

(c) Participants assigned to the control condition were not shown an image or message.

All participants then proceeded to the following questions.

18. Ideally, how often would you like to pray?
    - Never/almost never
    - Rarely
    - Once a year
    - Only on special religious holidays
    - Only when attending religious services
    - Several times a week
    - Once a day
    - Several times a day
19. How important is God in your life?
    Slider: Scale from 0 (not very important) to 10 (very important).

Block 3: Religiosity and Social Attitudes

20. Which of the following, if any, do you believe in?
For each item: Yes or No.
    - God
    - Afterlife
    - Hell
    - Heaven

21. Please tell us whether you strongly agree, agree, disagree, or strongly disagree with each of the following statements.
Scale: Strongly agree, Agree, Disagree, Strongly disagree.
    - Whenever science and religion conflict, religion is always right.
    - The only acceptable religion is my religion.

22. Apart from weddings and funerals, how often do you attend church nowadays?
    - Never/almost never
    - Rarely
    - Once a year
    - Only on special religious holidays
    - Once a month
    - Once a week
    - More than once a week

23. For each of the following actions, indicate whether you think it is never justified, always justified, or somewhere in between.
Slider: Scale from 0 (never justified) to 10 (always justified).
    - Abortion
    - Premarital sex
    - Homosexuality

24. It is difficult for me to forgive others for their mistakes.
Slider: Scale from 0 (not very difficult) to 10 (very difficult).

25. Were you vaccinated against COVID-19?
    - Yes
    - No

26. Which political ideology best represents you?
    - Left
    - Centre-left
    - Centre
    - Centre-right
    - Right
    - No ideology / not interested in politics

27. What is your attitude toward immigrants in our country?
    - Very positive

- Positive
- Neutral
- Negative
- Very negative

28. What is your position on legalizing same-sex marriage?
    - Strongly support
    - Somewhat support
    - Neutral
    - Somewhat oppose
    - Strongly oppose

29. How willing are you to take revenge if you are treated very unfairly?
    Slider: Scale from 0 (not very willing) to 10 (very willing).

30. How willing are you to punish a person for unfair behavior toward:
    Slider: Scale from 0 (not very willing) to 10 (very willing)
    - Oneself
    - Someone else

Block 4: Preferences

31. In your choices, how much do you compare yourself to others?
    Slider: Scale from 0 (not very) to 10 (very).

32. Compared with others, how willing are you to sacrifice today to benefit in the future?
    Slider: Scale from 0 (not very) to 10 (very).

Block 5: Conclusion

33. If you would like to enter the draw for the 100 projectors and be included in similar studies (some with monetary payment), please provide your contact details below.
    Mobile phone: Text entry.

C Appendix: Mapping the Preregistration to the Reported Analyses

This appendix maps the public AEA RCT Registry entry (AEARCTR-0016648) to the design, outcomes, and analyses reported in the manuscript. The purpose is to distinguish elements implemented as registered from implementation details, deviations, and analyses that are best interpreted as secondary or exploratory.

Our confirmatory evidence is the intent-to-treat comparison of the scientific treatment package with the no-message control on the post-treatment 0–10 importance-of-God item, interpreted as an immediate self-reported religiosity outcome. Other analyses are reported to examine robustness, measurement, heterogeneity, and possible interpretation. Where analyses were not clearly specified in the public registration, we label them as secondary or exploratory.

Table C1: Mapping the AEA RCT Registration to the Reported Analyses

AEA Registration Item	**Implementation in the Manuscript**	**Goal and Interpretation**
conveyed prosocial appeals attributed to religious and scientific sources. **Treatment Implementation.** The registration describes brief prosocial messages framed through religious or scientific sources.	The implemented treatments conveyed prosocial appeals attributed to religious and scientific sources. Each message was accompanied by an image consistent with its framing, while the control group received neither a message nor an image. Participants completed baseline survey questions before treatment assignment.	We interpret the estimated effects as those of the implemented religious and scientific treatment packages relative to the no-message control condition. Figure 1 presents the stimuli shown to participants.
Outcomes. The registration lists the survey outcomes subsequently examined in the paper, including measures of religiosity, religious beliefs and practices, social and political attitudes, and behavioral preferences.	We analyze and report all outcomes listed in the registration. Our main religiosity outcomes are the importance of God and desired prayer frequency. For desired prayer frequency, we report both the original ordered response and a binary indicator for wanting to pray at least once per day.	The registered outcomes are reported in the preregistered-analysis section and associated appendix tables. Any transformations of the original survey measures are described explicitly in the text and table notes.
Treatment Comparisons. The registration specifies the three experimental conditions but does not state separate formal hypotheses for all pairwise treatment comparisons.	Given the three-arm design, we report the three natural pairwise comparisons (either in the text or in the tables directly): Scientific vs. Control, Religious vs. Control, and Scientific vs. Religious.	The first two comparisons estimate the effect of each message relative to receiving no message. The Scientific vs. Religious comparison directly tests whether responses differ between the two treatment conditions.
Estimation and Covariates. The registration describes intent-to-treat comparisons across randomly assigned treatment groups and two-sided statistical tests.	We estimate OLS models for continuous outcomes and linear probability models for binary outcomes, with heteroskedasticity-robust standard errors. Our preferred specifications control for baseline religiosity and demographic characteristics; specifications without the additional demographic controls are also reported.	Because treatment assignment is randomized, covariates are included to improve precision rather than for identification. Results are generally similar with and without the additional controls.
Sample Construction and Exclusions. The registration describes a voluntary online survey of respondents aged 18 or older and an intended sample of approximately 1,000 participants.	The initial dataset contains 1,228 observations. We exclude 50 respondents who did not reach the randomization stage, 12 respondents younger than 18, and 30 respondents whose completion time exceeded 60 minutes, yielding a final analytic sample of 1,136 participants.	The realized sample size, exclusion rules, and participant flow are reported transparently in Recruitment and Sample Construction.

AEA Registration Item	Implementation in the Manuscript	Goal and Interpretation
Exploratory and Additional Analyses. The registration does not specify the treatment-effect heterogeneity analyses or the additional validation exercises reported in the paper.	We treat all heterogeneity analyses as exploratory, including heterogeneity by baseline religiosity and respondent characteristics. The paper also reports complementary validation exercises using first names and the World Values Survey and discusses potential mechanisms underlying the treatment effects.	These analyses are explicitly distinguished from the registered outcome analyses. Heterogeneity results are interpreted cautiously and accompanied by multiple-testing adjustments. The validation exercises are descriptive and assess cross-sectional properties of the religiosity measures, while potential mechanisms are presented as interpretations rather than channels identified by the experimental design.