RELIGIOUS IDENTITY AND ECONOMIC BEHAVIOR

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Abstract

We find using laboratory experiments that primes that make religion salient cause subjects to identify more with their religion and affect their economic choices. The effect on choices varies by religion. For example, priming causes Protestants to increase contributions to public goods, whereas Catholics decrease contributions to public goods, expect others to contribute less to public goods, and become less risk averse. A simple model implies that priming effects reveal the sign of the marginal impact of religious norms on preferences. We find no evidence of religious priming effects on disutility of work effort, discount rates, or dictator game generosity.

I. Introduction

Scholars since at least Weber (1930) have hypothesized that religious identities affect individuals' economic outcomes. However, measuring the causal effect of religion has been hampered by the difficulty of identifying exogenous variation in religious identities. Religious affiliation, even when inherited from one's parents, is likely to be correlated with many unobserved factors that affect behavior (Pope, Price, & Lillard, 2014). For example, people whose preferences already align closely with a religion's prescriptions may be more likely to choose it or remain in it (Hungerman, 2014). Correlation between behavior and religious affiliation could thus be entirely due to the correlation between exogenous preferences and religion rather than any causal impact of religion itself. In addition, religious affiliation may be correlated with background variables, such as childhood home environment, that affect behavior but are unobserved or measured imprecisely. The considerable selection on observable characteristics into religious affiliation raises concerns that selection on unobservable characteristics is also significant (Altonji, Elder, & Taber, 2005).

In this paper, we use a technique from experimental psychology to create exogenous variation in one channel through which religion could affect behavior: the salience of religious identity to the person affiliated with that identity. We measure how experimental

subjects' choices in the laboratory differ after religious identity is randomly made salient to them versus not by using a sentence-unscrambling task where the unscrambled sentences either do or do not contain religious content. Religious identity will be salient at least some of the time to anybody who belongs to a religion. Indeed, many practices—such as Muslims praying five times per day, Catholics praying the rosary according to a schedule tied to the day of the week, and the Jewish observance of the Sabbath—regularly make religious identity salient. Laboratory identity salience manipulations allow us to measure how behavior is likely to be affected by identity affiliation during salient moments.

Why does identity salience affect behavior? A leading perspective in the psychology literature is self-categorization theory, which has a long history (James, 1890; Turner, 1985). According to this theory, each person belongs to multiple social categories, such as religion, gender, and occupation, each with its own set of norms about how a person in the category should behave. Behavior in a given moment is more powerfully affected by the norms of categories that are salient than the norms of categories that are not salient. If an environmental cue, or a prime, makes a certain category temporarily more salient, behavior shifts toward the salient category's norms. A simple economic model of identity salience effects based on self-categorization theory implies that priming effects reveal the sign of the marginal impact of category norms on steady-state preferences (Benjamin, Choi, & Strickland, 2010). Given this model, religious priming effects are of interest not only because of their direct influence on behavior but also because of what they may tell us about the norms associated with religious identity.

From the twenty outcomes listed by the survey articles of Iannaccone (1998) and McCleary and Barro (2006) as possibly being affected by religion, we study the six that can be measured with workhorse experimental economics tasks: public good contributions, trust, thrift, risk aversion, generosity, and work ethic. Our main analysis is on Protestants and Catholics, since a validation experiment provides the strongest evidence that our priming instrument affects the salience of their religious identity.

Our results suggest that the cross-sectional correlations between religion, contributions to public goods, and trust documented by Putnam (1993), La Porta et al. (1997), and others are at least partially due to a causal channel from religious norms to the two behavioral outcomes. We find that religious identity salience increases Protestants' contributions to a laboratory public good, while it causes Catholics to decrease contributions. For Catholics, religious identity salience decreases trust as measured by expectations of others' contributions to the public good, and it has no statistically significant effect on contributions once its impact on trust is controlled for. For Protestants, however, religious identity salience does not affect trust, suggesting that its positive impact on Protestant public good contributions does not operate through a trust channel.

In accordance with religious identity norms being a causal contributor to the correlation between religion and financial risk taking documented by Barsky et al. (1997) and others, we find that priming religion has a different effect on risk taking for Catholics than for Protestants. For Catholics, we find that religious identity salience increases risk taking. Many Protestant denominations regard gambling as a sinful activity, but in our data,

religious identity salience has no effect on Protestant risk taking, suggesting that the stated antigambling norm is either ignored in practice or considered inapplicable to general financial risk taking.

We find no evidence for effects of religious identity salience on discount rates and generosity, nor do we find religious priming effects on work ethic among Catholics or Protestants. These null results are inconsistent with the Weber (1930) hypothesis that Protestant identity norms promote thrift and hard work while discouraging generosity.

Among Jews, we find that priming religion increases the rate at which workers raise their work effort in response to higher wages in the gift exchange game. Among agnostics and atheists, we find that our manipulation reduces risk aversion. Our validation experiment, however, does not provide any direct evidence supporting the hypothesis that our priming instrument affects religious identity salience for Jews and agnostics/atheists—although our small Jewish sample size makes us unable to reject a large salience effect for Jews, and it is unclear that our validation experiment's measure of religious identity salience would capture this construct for agnostics and atheists. We therefore interpret the priming effects we find for Jews and agnostics/atheists more tentatively than for Protestants and Catholics.

Other researchers have manipulated religious identity salience and measured changes in subsequent choices (Pichon, Boccato, & Saroglou, 2007; Randolph-Seng & Nielsen, 2007; Mazar, Amir, & Ariely, 2008; Hilary & Hui, 2009; Toburen & Meier, 2010; Ahmed & Salas, 2011; Horton, Rand, & Zeckhauser, 2011). To our knowledge, we are the first to measure the impact of religious priming on public good contributions, expectations of others' public good contributions, gift exchange choices, and discount rates. When our outcome measures overlap with the prior literature, our experiment is distinguished by its much larger sample size and by the fact that our measures are elicited using incentive-compatible procedures. In addition, unlike previous research, we measure separate effects for each religion. We also contribute to the literature on religious priming by validating a commonly used priming instrument, confirming that it indeed increases the salience of religious identity but does not also increase the salience of a range of other identities.

In related experimental economics work using Indian caste and other nonreligious identities, Eckel and Grossman (2005), Hoff and Pandey (2006, 2014), Charness, Rigotti, and Rustichini (2007), Chen and Chen (2011), Cohn, Fehr, and Maréchal (2014), Chen et al. (2014), and Cohn, Maréchal, and Noll (2015) find that group identity effects on behavior strengthen with the salience of group membership. Chen and Yeh (2014) manipulate which norms (expressed by legal rulings) subjects are exposed to and study how that affects their self-identification.

Our paper proceeds as follows. Section II reviews the literature on religious effects in the domains we study. Section III describes a theoretical framework for interpreting identity salience effects. Section IV describes our priming instrument and the experiment we use to confirm that it has the desired effect on the salience of religious identity. As we discuss in section IV and in an online appendix, we find some evidence that could be interpreted as indicating that our priming instrument affects whether subjects report themselves to be

Protestant or Catholic, but we believe this effect is unlikely to drive our main experiment's results. Section V describes the methodology of our main experiment, and section VI describes our main experiment's sample. Section VII presents the main experiment's empirical results for Protestants and Catholics. Section VIII discusses the main experiment's results for Jews and agnostics/atheists. Section IX describes a joint hypothesis test that partially addresses concerns about type I error due to multiple hypothesis testing. Section X concludes.

II. Literature Review on Religious Effects

In this section, we characterize some key similarities and differences across Catholicism, Protestantism, and Judaism along three institutional and belief dimensions—organizational structure, beliefs about the afterlife, and moral teachings—that have been theorized to matter for the economic choices we study. For each of these characteristics, we discuss their hypothesized effects. We note, however, that when our later empirical results are consistent with one of these previously hypothesized mechanisms, we cannot rule out the possibility that other mechanisms associated with the religion actually generate the effects we find.

We also discuss the prior empirical work that does not use priming techniques, the vast majority of which is correlational. We close this section with a discussion of those who identify with no religion, a substantial minority of our experimental sample.

A. Organizational Structure

Catholicism is organized as a centralized and vertical hierarchy that exerts considerable authority down to the local level. In contrast, Protestant churches and mainstream North American synagogues are more autonomous and horizontally organized. Putnam (1993) argues that horizontally organized groups foster trust and contributions to public goods because the horizontal social networks they create allow norms of cooperation to be sustained through transmission of information about and collective punishment of defectors. In contrast, the hierarchical organization of Catholicism causes "vertical bonds of authority [to be] more characteristic" (p. 107). Putnam continues, "A vertical network, no matter how dense and no matter how important to its participants, cannot sustain social trust and cooperation. Vertical flows of information are often less reliable than horizontal flows, in part because the subordinate husbands information as a hedge against exploitation. More important, sanctions that support norms of reciprocity against the threat of opportunism are less likely to be imposed upwards and less likely to be acceded to, if imposed" (p. 174).

B. Beliefs about the Afterlife

Catholicism holds that there exist heaven, hell, and an intermediate state called purgatory where those who are ultimately destined for heaven but are insufficiently purified undergo temporary punishment. The Catholic Church's sixteenth-century Council of Trent wrote that a person's final destiny is determined by "the good works that he performs by the grace of God and the merit of Jesus Christ."

Protestantism rejects the existence of purgatory, but in the United States, Protestants are a little more likely than Catholics to believe in heaven and hell (Exline, 2003). In contrast to

Catholicism, Protestantism espouses the doctrine of *sola fide* ("by faith alone"): one enters heaven solely through faith in Jesus rather than through good works performed during one's life. However, an absence of good works is a sign that one's faith is not a saving faith.

There is considerable disagreement regarding what Judaism teaches about the existence of an afterlife (Raphael, 2009). In practice, Jews are much less likely to believe in life after death than Protestants and Catholics (Raphael, 2009; Klenow & Bolin, 1989–1990).

Becker and Mulligan (1997) predict that belief in the afterlife will lower a person's discount rate, since putting greater weight on the future increases utility when that future is anticipated to be pleasant. The most famous theory of how afterlife beliefs affect economic behavior comes from Weber (1930), whose thesis is that Calvinist theology, by taking *sola fide* to its purest extreme, had a major impact on Protestants' economic norms, thereby launching the Industrial Revolution (but see Giddens, 2002, for a summary of numerous criticisms that Weber's theory is based on misunderstandings of Protestant and Catholic ethics and theology). John Calvin argued that God has predestined each person to be saved or damned, and that because salvation is by faith and not works, it is impossible to know from one's actions what one's eternal destiny is. Weber argues that such radical uncertainty was psychologically intolerable to Calvin's followers, and so ironically, they came to regard personal diligence, frugality, calculated risk taking, and economic success as signals of whether one was saved. These thus became norms regarding how one ought to behave. In addition, according to Weber, Calvinism discouraged charity, since inequality in economic outcomes—like inequality in eternal destinies—is divinely ordained, and "poverty is very often a symptom of sinful slothfulness" (Weber, 1930, p. 281).

Blum and Dudley (2001) extend Weber's argument by positing that Catholics have weaker incentives to cooperate with people beyond their immediate circle because the divine penalty for defecting can be avoided through the sacrament of penance. For Protestants, however, defection is more costly because it weakens their conviction that they were predestined to be saved, and no absolving sacrament is available to them.

C. Moral Teachings

Catholicism, Protestantism, and Judaism share many of the same sacred texts, which creates substantial commonality in their moral teachings. For example, exhortations to be generous are found in both the Old and New Testaments, so it is natural to hypothesize that Catholicism, Protestantism, and Judaism all have identity norms of generosity (Friedrichs, 1960; Batson, Schoenrade, & Ventis, 1993). We focus the discussion in the remainder of this section on differences across the three religions' moral teachings that may be relevant for the economic behaviors we study.

Moral condemnation of gambling is widespread in U.S. Protestantism and Judaism but not in Catholicism (Bell, 1974). Indeed, other religious bodies have often criticized the Catholic Church for practices that promote gambling (Halek & Eisenhauer, 2001; Hoffmann, 2000), such as the use in many Catholic parishes of games of chance to raise funds. Binde (2007) describes how in southern Italy, San Pantaleone has been widely regarded as the patron saint of lotto players, and various other saints are asked for winning lotto numbers. Kumar, Page,

and Spalt (2011) argue that Protestant and Catholic norms regarding gambling extend to financial investing and corporate policies. In Jewish theology and religious culture, Schnall (2001, p. 62) writes that "a life of simplicity and security is prescribed over one of risk and uncertainty."

Regarding work ethic, Weber (1930) claims that Protestantism developed the concept of one's work as a divine calling, so that working diligently in a worldly occupation became a moral imperative in a way that it did not in Catholicism. In Judaism, Schnall (2001) describes two strands in traditional teachings that are in tension with each other: a high valuation of gainful labor as a religious obligation and an idealization of full-time withdrawal from worldly labor for religious study.

A leading economic model of how religion affects public goods contributions is Iannaccone (1992): religious prohibitions and sacrifices raise the cost of secular activities and the price of entry into the religious community, screening out free riders and thus sustaining the provision of public goods within the religious community. Berman (2000) interprets extended full-time yeshiva attendance by ultra-Orthodox Jewish men as a manifestation of this mechanism. Similar dynamics can operate in Protestant sects that make strict demands of their members. Iannaccone (1992) predicts that larger congregations will tend to be less strict due to their higher costs of monitoring and enforcing distinctive behavioral requirements. Larger congregations will in turn have lower provision of within-congregation public goods. The large average size of Catholic congregations may thus impede provision of public goods within Catholic communities not only because free-rider problems are generically greater in larger populations, but also because the technology of strict prohibitions and sacrifices is less available to Catholic congregations.

D. Prior Empirical Evidence

There is considerable empirical evidence that Catholics trust less and contribute less to public goods than Protestants (Greeley & McManus, 1987; Hodgkinson & Weitzman, 1990; Putnam, 1993; Zaleski & Zech, 1994; Iannaccone, 1998; Inglehart, 1999; Alesina & La Ferrara, 2002; Guiso, Sapienza, & Zingales, 2003; Arruñada, 2010), although some studies find otherwise (Fehr et al., 2002; Bellemare & Kröger, 2007). For Jews, Guiso et al. (2003) find no significant difference relative to the nonreligious in self-reported trust, but Berman (2000) and Ruffle and Sosi (2007) find high levels of cooperation among ultra-Orthodox Israeli communities and religious kibbutzim.

There is also a large body of evidence that Catholics are less risk averse than Protestants (Tec, 1964; Grichting, 1986; Barsky et al., 1997; Hilary & Hui, 2009; Kumar, 2009; Kumar et al., 2011; Shu, Sulaeman, & Yeung, 2012; Schneider & Spalt, 2016, forthcoming), although Renneboog and Spaenjers (2012) find the opposite. For Jews, Barsky et al. (1997) find less risk aversion than for Catholics and Protestants, contrary to what might have been hypothesized based on Schnall's (2001) summary of Jewish teaching on risk taking.

Regarding generosity, nearly all studies find a positive relationship between religious involvement and self-reported giving to both religious and secular causes (Bekkers & Wiepking, 2011). However, studies that observe actual behavior in response to a single

giving opportunity find mixed evidence on the links between religion and altruism (Smith, Wheeler, & Diener, 1975; Annis, 1976; Batson & Ventis, 1982; Batson et al., 1989; Eckel & Grossman, 2003; Eckel & Grossman, 2004; Tan, 2006; Bekkers, 2007).

The existence of a uniquely strong Protestant work ethic has some support, but there is little evidence of a uniquely high Protestant valuation of thrift (Guiso et al., 2003; Arruñada, 2010; Spenkuch, 2011; Renneboog & Spaenjers, 2012). McCleary and Barro (2006) find that across countries and religions, a greater belief in hell is associated with a stronger self-reported valuation of hard work but not of thrift. Evidence on the Jewish work ethic is mixed. Consistent with the presence of competing attitudes toward work in Jewish thought, findings vary across Jewish samples (Lenski, 1963; Chiswick, 1985; Harpaz, 1998; Lynn & Kanazawa, 2008).

E. Atheists, Agnostics, and Religious "Nones"

Kosmin and Keysar (2009) report that in 2008, 15.0% of U.S. adults say they do not belong to a religion. However, only 0.7% self-identify as atheists and 0.9% as agnostics. The remaining 13.4% simply say their religion is "none." Despite not belonging to a religion, religious belief is quite high among those with no religious affiliation. For example, 57% believe there is a God who performs miracles (Keysar & Kosmin, 2007), and 93% pray sometimes (Hout & Fischer, 2002). Sixty-eight percent did identify with a religion at age 12, making it likely that they currently retain many of the norms associated with the religion that they grew up with. Former Catholics and former Protestants are present in similar proportions among nones. The primary distinguishing feature of nones is not their religious beliefs, but their detachment from organized religion. For most religious nones, secularism is not a defining identity but, rather, a "residual category" (Phillips, 2007, p. 29).

Relative to the more religious, one striking trait of people with low religiosity in the West is that they are less risk averse. Miller and Hoffman (1995), Miller (2000), and Miller and Stark (2002) interpret this relationship through the lens of Pascal's wager: irreligion is a risky strategy because one gains little if there is no God but potentially loses a great deal if there is a God. Consistent with this interpretation, irreligion is negatively correlated with risk aversion in predominantly Christian and Islamic countries but not in India and Japan, where the majority religions do not teach that nonparticipation has eternal consequences as dire as those in Christianity and Islam (Miller, 2000).

III. A Theoretical Framework for Interpreting Priming Effects

Aspects of a person's identity organically become more or less salient during the course of everyday life. Identity priming effects are interesting in and of themselves because they reflect how behavior changes during these moments. Moreover, it follows from the psychological theory of self-categorization (James, 1890; Turner, 1985) that we can draw additional conclusions from priming effects. According to this theory, priming one's religious identity temporarily increases the strength of one's affiliation with that identity category, causing one's behavior to shift toward the category's norms. We present here a formalization of this theory, which is inspired by Akerlof and Kranton (2000) and developed in Benjamin, Choi, and Strickland (2010). The formalization shows how comparing primed

and unprimed behavior allows us to draw inferences about the identity category's norms and how they affect steady-state behavior.

Let x be some choice, such as how much to contribute to a public good or how much to trust a stranger. An individual belongs to a social category C, such as Protestant, with strength s 0. Let x_0 denote the individual's preferred choice in the absence of identity considerations, and let x_C denote the choice that is normative for members of social category C.

Norms can be injunctive or descriptive (Cialdini, Reno, & Kallgren, 1990). Injunctive norms explicitly prescribe or prohibit actions (e.g., "thou shalt not kill"). Although studying a religion's stated injunctive norms gives some guidance as to what the injunctive norms tied to that religious identity are, these stated norms may be ignored or not accepted as normative in practice by the religion's followers. For example, despite the Catholic Church's official injunction against contraception, 82% of U.S. Catholics say birth control is morally acceptable, a proportion close to the 90% of non-Catholics who condone birth control (Newport, 2012).

Descriptive norms are derived from what is believed to be "typical" or "normal." Although they do not carry an explicit "ought," people are influenced by descriptive norms because of pressure for social conformity (Asch, 1951; Festinger, 1954) or because other people's actions are interpreted to contain useful information about optimal behavior (Banerjee, 1992; Bikhchandani, Hirshleifer, & Welch, 1992; Ellison & Fudenberg, 1993). For example, low trust among Catholics initially created by Catholicism's vertical organization could engender a descriptive Catholic norm of low trust, even if Catholicism does not contain an injunctive norm against trust. Group behavior that develops in response to some force could remain part of the group's descriptive norms long after the original force is gone, creating hysteresis in behavior (Clark, 2003). For instance, a Protestant denomination whose founding members worked hard because of Calvinist theology could retain a strong work ethic norm even after the denomination abandons Calvinism, since the work ethic of older members creates a descriptive norm for younger members. Due to misperceptions, a descriptive norm need not correspond to the actual average behavior of group members (Borsari & Carey, 2001; Wenzel, 2005). Therefore, the norm may amplify or attenuate the original effect from which it springs.

Regardless of the source of the norm x_C, the formal model states that the individual chooses x to maximize

$$U = -(1 - w(s))(x - x_0)^2 - w(s)(x - x_C)^2, \quad (1)$$

where $0 \quad w(s) \quad 1$ is the weight placed on the norm for social category C in the person's decision. We assume that $w(0) = 0$ and $w' > 0$. Deviating from the norm for one's category causes disutility that is increasing in s. We assume that s has a steady-state value $\bar{s}$ but can be temporarily increased to $\bar{s} + \varepsilon$, where $\varepsilon > 0$, by a category prime that makes the category's norms more cognitively accessible and hence more influential on behavior.

The first-order condition of equation (1) gives the optimal action, $x^*(s) = (1 - w(s))x_0 + w(s)x_C$, which is a weighted average of the preferred action without identity considerations and the category norm. This condition yields several implications:

1. The higher the steady-state strength $\bar{s}$ of the category affiliation, the closer x^* is to x_C in steady state.

2. A category prime also causes x^* to move closer to x_C. Thus, the behavioral effect of priming social category C reveals the marginal behavioral effect of increasing the steady-state strength $\bar{s}$ of C. This is what makes priming manipulations a useful experimental procedure for studying how category norms affect steady-state choices.

3. The sign of the priming treatment effect, $x^*(s + \varepsilon) - x^*(s) \approx (dx^*/ds)\varepsilon = w'(s)(x_C - x_0)\varepsilon$, depends on the sign of $x_C - x_0$. Even if the $\bar{s}$, x_0, and $w(\cdot)$ of an experimental sample differ from those of the general population affiliated with C, the directional effects of priming the sample will generalize to the population as long as $x_C - x_0$ has the same sign for both groups.[1]

Psychologists have tested the category salience mechanism by priming social categories with norms that are believed to be known and confirming that choices shift toward these norms (Reicher & Levine, 1994; Forehand, Deshpandé, & Reed, 2002; LeBoeuf, Shafir, & Bayuk, 2010). We invert the approach in these papers by taking the validity of the category salience mechanism as given, priming categories with unknown norms, and making inferences about the norms from the resulting shift in choices.

Importantly, this framework also provides a way to think about the limitations of priming techniques for identifying norms:

4. Although the direction of the priming effect reliably identifies the sign of $x_C - x_0$, differences in the priming effect's magnitude across people may arise through a number of channels. Assume without loss of generality that $x_C > x_0$. All else equal, priming will have a larger effect if the identity norm is more extreme (i.e., x_C is larger) or the person's preferred action in the absence of identity considerations is more extreme in the opposite direction (i.e., x_0 is smaller). Priming will also have a larger effect if the salience manipulation is more effective at increasing identity salience for that particular person (i.e., ε is larger) or the person's choices are more sensitive to a given change in identity salience (i.e., w' is larger). This latter difference can arise either because the w function has a different shape or because the person has a different steady-state $\bar{s}$, so that the points at which she evaluates the w function differ. Because it is difficult to interpret differences in priming-effect magnitudes, we will focus on estimating the priming effect within a religious identity category rather than a difference-in-difference analysis of priming effects across religious categories.

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[1]The sign of $x_C - x_0$ might differ also because x_C in a sample differs from that in the general population affiliated with C. This could occur, for example, if identity norms differ across subgroups within a religious category—such as different denominations within Protestantism—and the mix of subgroups differs between the sample and the general population affiliated with that category.

5 Priming will not reveal social category effects that operate exclusively through x_0 rather than x_C. For example, suppose that x is the decision to recycle or not, Jews' average income is high because of an injunctive norm in Jewish culture for more education, and warm-glow utility from recycling is increasing in income (Schultz, Oskamp, & Mainieri, 1995). The result would be that Jews recycle more often than the average American due to a higher x_0 that is the result of their category membership. But if recycling behavior is not thought of as a characteristic of Jews, there would be no x_C associated with recycling, so the second additive term in equation (1) is missing. Then, priming the Jewish social category would not cause Jews to recycle more.

6 There will be no difference between primed and unprimed choices in domains where the unprimed behavior is already at the boundary of the action space. For example, a Jew who always keeps kosher will refuse to eat pork even when unprimed. Thus, even if priming Jewish identity increases the weight on the norm of keeping kosher, pork-eating behavior will not be affected.[2]

Points 5 and 6 imply that null priming effects need not be interpreted as definitive evidence against the existence of social category effects in a particular domain.

IV. The Priming Instrument

Our priming instrument, first used by Shariff and Norenzyan (2007) to study the effect of priming religious concepts, is a sentence-unscrambling task where subjects are asked to drop the irrelevant word in a five-word group and rearrange the remainder to form a four-word sentence. For example, "yesterday it finished track he" becomes "he finished it yesterday." Each subject unscrambles ten sentences.

The sentences vary depending on whether the subject is in the religion-salient condition or the control condition. Five of the sentences unscrambled by religion-salient subjects contain religious content. These five sentences are: "she felt the spirit," "the dessert was divine," "give thanks to God," "the book was sacred," and "prophets reveal the future." None of the control subjects' sentences contain religious content.[3] The online appendix lists all of the scrambled sentences in the priming instrument.

As a preliminary empirical exercise, we report the results of an online experiment we conducted to confirm that the Shariff and Norenzyan (2007) priming instrument increases the salience, and hence perceived importance, of religious identity—a proposition that, as far as we are aware, has not been tested in earlier work. For this purpose, we recruited 1,798 subjects during March and April 2013: 1,192 from the Yale School of Management eLab subject pool and 606 from U.S. workers on the Mechanical Turk website.[4] We chose not to target particular religions (or mention religion at all in our recruiting materials) in order to

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[2]In contrast, we hypothesize that priming the Jewish category would make a Jew who occasionally keeps kosher outside the Jewish holidays more likely to refuse pork.

[3]An advantage of this priming instrument is that it is subtle. Compared with blatant primes, subtle primes more reliably cause behavior to conform to norms (Wheeler & Petty, 2001), which aids in interpreting our results within the framework described in section III.

avoid making religious identity salient to all the subjects. Compensation for participating was a one in forty chance of winning a $20 Amazon.com gift certificate for eLab subjects and $1 for Mechanical Turk subjects.

Subjects who visited our experiment website were randomly assigned to complete the religion-salient task or the control task online. Subjects were not aware that this task differed across subjects. On the screen after the sentence unscramble, subjects saw the following text: "An important part of your identity is the groups you belong to that define who you are. These groups are sets of people with similar characteristics as you or sets of people who play a similar role in life as you do. Of the many groups you belong to, please list the 5 that are the most important for defining who you are." Following this were five text-entry boxes in which subjects typed free responses. The identity responses were placed into 27 categories by coders blinded to the experimental condition.[5] The key dependent variable is whether a subject lists a religious category (or secular equivalent) among his or her top five identities. The full set of results, which we summarize, is shown in online appendix table 1.

Across the entire sample, we find that 29.9% of subjects assigned to the religion-salient prime listed a religious identity (including identities tied to morality and philosophy, to accommodate secularism) as one of the five identities versus 25.1% of subjects in the control condition. The difference is significant at the 5% level ($p = 0.023$).[6] This finding suggests that the priming instrument indeed increases the salience of religious identities.[7]

Although whether religion is listed as a top-five identity is a rather crude measure of identity salience (e.g., it only captures movement from outside to inside the top five), we nonetheless attempt to examine whether the effect of priming on this measure varies across religious groups. We elicited religious affiliation using a multiple-choice question (it did not include "none" as an option, though it did include "other"), which was included among questions on the last two screens that asked about participants' demographic characteristics. In the sample were 314 Catholics, 510 Protestants, 89 Jews, 570 agnostics and atheists, and 315 with religious affiliations that did not appear with sufficient frequency in our main experiment's sample to study.

For Catholics and Protestants separately, we do not have enough power to detect significant priming effects, but the point estimate of the effect is similar for both groups: the prime increased religious identity mentions among Catholics from 26.1% to 33.3% ($p = 0.158$) and among Protestants from 41.0% to 47.6% ($p = 0.135$). Pooling Catholics and Protestants together, the priming effect is significant at the 5% level ($p = 0.017$).

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[4]Our target sample size was 2,000 participants. We recruited almost all of the Yale sample first and then the Mechanical Turk sample to get closer to our target. Following best practices of scientific disclosure, we discuss in the online appendix three other experiments we ran to validate the priming instrument.

[5]The online appendix describes our categorization scheme.

[6]All p-values mentioned in this paper are two-sided.

[7]We have also run a tobit analysis where the dependent variable takes the value 1 if a religious identity is the first identity mentioned, 2 if it is the second mention, and so on. If no religious identity is mentioned in the top five, the dependent variable takes on a value of 6 and is coded as right-censored. In this analysis, which imposes stronger functional form assumptions than those we use in the main text's analysis, we estimate that the treatment causes the rank of religious identity to rise by 0.67 places ($p = 0.015$).

For Jews, we find that religious identity mentions are actually higher in the control group (67.4%) than in the primed group (58.7%), but the difference is far from significant ($p = 0.396$), and our estimates are very noisy due to the relatively small sample. At the 5% level, we cannot reject the hypothesis that the prime increases Jewish identity mentions by as much as 12.6%. Nevertheless, the failure to see an increase in Jewish identity mentions in response to the prime makes the interpretation of our later results on Jewish priming effects more tentative.

Finally, for agnostics and atheists, the prime had little effect on religious identity mentions: 10.0% in the control group versus 11.1% in the primed group ($p = 0.669$). However, we believe this dependent variable may be a poor measure of religious identity salience for this group because even if religion is successfully primed, these individuals are unlikely to mention a religious identity as among their top five, since many are in fact religious nones with neither an affirmative secular identity nor a nonsecular religious affiliation. We asked subjects after eliciting their religious affiliation: "In your previous question, you described your religious affiliation. But would your religious affiliation be better described as 'none'?" Among subjects who had categorized themselves as atheists or agnostics, 80.7% answered yes to this follow-up question. To the extent that religious nones in our sample retain nonsecular religious beliefs and norms (as is common for American religious nones), the prime seems likely to heighten their salience, even if this effect does not show up as an increase in religious identity mentions.

Examining how the prime's efficacy differs by religious group may be problematic if the prime affects how participants categorize themselves. A chi-squared test of equality of religious affiliation proportions across treatment and control yields a p-value of 0.037, suggesting that such recategorization may indeed be occurring. The proportion of Christians in the validation sample is well balanced (45.7% in treatment versus 46.0% in control), as is the proportion of Jews (5.1% in treatment versus 4.8% in control); the religious imbalance primarily comes from Protestants being relatively more common than Catholics in the treatment group (30.3% Protestants, 15.4% Catholics) versus the control group (26.5% Protestants, 19.5% Catholics).[8] In the online appendix, we calculate bounds on how the priming instrument's efficacy might vary by religion if it also causes recategorization. However, we believe the difference in religious-affiliation proportions is due to sampling variation rather than to the prime actually causing people to change the religious affiliation they report. Indeed, there is no evidence of such an effect in the main experiment sample described in section V: testing the equality of religious affiliation proportions in the treatment versus control groups in that sample yields a p-value of 0.901. (If we pool the prime validation and main experiment samples, the p-value is 0.206.)

Does the priming manipulation increase the salience of any nonreligious identities? This would confound the inference that the priming effect reflects religious identity norms. For example, priming religious identity in an African American Protestant might prompt the

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[8]As we report in the online appendix, the prime does not make any particular demographic group (we observe age, household income, and full-time student status) more likely to report being Protestant or report being Catholic, and the prime seems to be more effective at raising religious identity salience among lower-income subjects.

subject to think about the African American nature of her religious experience, activating a general African American identity and causing her behavior to shift toward nonreligious African American norms as well. However, we see no evidence that our religious prime increased the salience of nonreligious identities: none of our 26 nonreligious identity categories—such as nationality/ethnicity/race/language and socioeconomic class—are mentioned significantly more often under the prime than under the control.

The online appendix reports evidence that the prime does not disproportionately cause certain identities to fall out of the top five and that negative priming effects—where the prime causes people who reject the norms of a religious identity to move even further away from that category's norms—if they occur at all, are rare.

V. Main Experiment Procedure

Participants in the main experiment were 817 Cornell University students. Again, we did not mention in our recruiting materials that we were looking for people of particular religions. Sessions were administered by computer, most using the program z-Tree (Fischbacher, 2007) but some using VBA for Microsoft Excel, depending on which dependent variables we measured in the session. Sessions occurred between April 2008 and December 2009.

Within each experimental session, we randomly assigned subjects to complete the religion salient or control sentence unscramble. Subjects were not aware that this task differed across subjects. After completing the sentence unscramble, they participated in strategic games, incentive-compatible preference elicitations, and an anagram-solving task. Subjects were told at the beginning of the experimental session that any interactions they had with other subjects would be anonymous, one-shot interactions. In order to avoid excessively long sessions, each subject engaged in only a subset of the post-unscramble tasks.[9] The order in which the post-unscramble tasks appeared varied across sessions.[10]

A. Public Goods Game

We measured the willingness to contribute to a public good by having subjects play a linear public goods game (Marwell & Ames, 1979). We assigned each subject to a group of four and endowed him or her with $1. Subjects could contribute any fraction of their dollar to a group account, which is the laboratory public good. Contributions were doubled and then distributed evenly among the four group members. Subjects kept any money that they did not contribute. Total group earnings are maximized (at $2 per group member) if each member contributes his or her entire dollar to the group account. However, in the absence of other-regarding preferences, it is a dominant strategy to contribute nothing, since the private return on a contribution is −50%.

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[9]We had 211 subjects in sessions that administered only the following four sections after the sentence unscramble: the dictator game, the public goods game, the risk preference elicitation, and the time preference elicitation; 343 subjects in sessions that administered only the following three sections after the sentence unscramble: the dictator game, the public goods game, and the labor market gift exchange game; 120 subjects in sessions that administered only the following three sections: the time preference elicitation, the risk preference elicitation, and the anagram work ethic task; and 143 in sessions that administered only the anagram work ethic task.
[10]We have examined how the strength of the priming effect varied with the length of time elapsed since the priming manipulation. The effects on Jewish gift exchange reciprocity and Catholic risk aversion may weaken over time, while the effects on Protestant and Catholic public goods contributions and Catholic trust appear to strengthen over time, but we put little weight on these possible trends because very few of the interactions between the priming effect and task order are statistically significant.

Before eliciting subjects' own contributions, we asked subjects to give their best guess of how much the other three members of their group would contribute on average—a measure of trust.[11] In order to keep the game's instructions relatively simple, we did not make subjects' payments depend on the value of this guess.[12] Existing evidence suggests that behavior in laboratory public goods games like ours is correlated with contributions to public goods outside the laboratory (Laury & Taylor, 2008; Carpenter & Seki, 2011; Fehr & Leibbrandt 2011).

B. Risk Aversion Elicitation

We elicited small-stakes risk preferences by asking participants to make six binary choices between $1.00 for sure and a 50% chance of a larger amount, ranging from $1.60 to $3.60. The outcome of each gamble was independent, and we paid subjects for all six choices in accordance with their stated preferences; for example, if a subject selected the gamble if and only if winning the gamble yielded $3.60, then she would have a 50% chance of earning $5 and a 50% chance of earning $5 + $3.60 = $8.60 in this section. We measured larger-stakes risk preferences with six analogous choices, where the monetary amounts were 100 times larger than the small-stakes risk choice amounts and there was only a small chance that the subject's choices would be implemented for payment.[13]

Risk-aversion measures derived from incentive-compatible experimental choices such as ours are highly correlated with measures from hypothetical choices, which in turn predict risky behaviors in the field (Barsky et al., 1997; Guiso & Paiella, 2008; Dohmen et al., 2005; Sahm, 2012).

C. Discount Rate Elicitation

We measured time preferences by asking participants to make 12 binary choices between receiving $10.00 now and receiving some larger amount one week from now, and another 12 binary choices between receiving $10.00 one week from now and receiving some larger amount two weeks from now. The larger delayed amounts ranged from $10.10 to $15.00. After a subject made these choices, we randomly selected one of the 24 choices and paid the subject according to his or her stated preference in this choice. All payments were made by a check given to the participant immediately following the experiment. Delayed payments were implemented by postdated check.[14]

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[11]We use the term trust in the sense of La Porta et al. (1997, p. 333): trust is when "people expect certain fair or cooperative behavior of their opponents even when they do not expect to see them again." See also Gächter, Herrmann, and Thöni (2004) for an explication of how trust applies to a simultaneous-move public goods game.

[12]Since an important determinant of contribution choice is the desire to neither free-ride nor be free-ridden on (e.g., Kurzban et al., 2001), the payment associated with the public goods game itself provides an incentive to accurately forecast others' contributions. Paradoxically, it is theoretically possible that paying subjects for accurate forecasts might actually make their reported beliefs further away from their true beliefs due to the incentive to hedge against the risk of an inaccurate forecast that would cause one's contribution to be quite different from others' contributions (Blanco et al., 2010; Fischbacher & Gächter, 2010). Gächter and Renner (2010) find that paying subjects for reporting accurate forecasts has the undesirable side effect of raising public good contributions, whereas eliciting beliefs without incentives for accuracy leaves contributions unaffected.

[13]One of the six large-stakes risk choices was randomly chosen to be paid out if the subject could correctly predict two spins of a roulette wheel, which implied a 1 in 1,444 chance of one large-stakes risk choice being implemented.

[14]If the subject received a delayed payment for this section, then earnings from other sections were paid through a separate check that was immediately cashable.

Our approach to measuring time preferences is standard (Frederick, Loewenstein, & O'Donoghue, 2002). Similar measures predict variation in discounting-related behaviors in the field (Fuchs, 1982; Bickel, Odum, & Madden, 1999; Kirby, Petry, & Bickel, 1999; Petry & Casarella, 1999; Kirby & Petry, 2004; Shapiro, 2005; Ashraf, Karlan, & Yin, 2006; Meier & Sprenger, 2010; Reuben, Sapienza, & Zingales, 2015).

D. Dictator Game

In our implementation of the dictator game (Kahneman, Knetsch, & Thaler, 1986; Forsythe et al., 1994), we endowed each subject with $1 and randomly assigned him or her to another participant in the session. The subject could choose to give any portion of that $1 to the other subject. A profit-maximizing individual would keep the entire dollar for himself, so the amount given away is a measure of pure altruism. Benz and Meier (2008) find that generosity in laboratory dictator games is positively correlated with charitable giving outside the laboratory.

E. Labor Market Tasks

Work ethic can be interpreted as an individual's disutility of effort, which determines the willingness to exert a contracted-on amount of effort at a given wage rate, or the strength of the reciprocity motive toward an employer that causes a worker to supply more effort in response to a higher wage when the labor contract is incomplete. We measure identity effects on both variables.

To measure identity effects on the disutility of effort, we asked subjects to solve as many four-letter anagrams as they could in a five-minute period. We paid participants five cents per correctly solved anagram. Because the piece-rate wage we offered is a contingent payment, subjects' reciprocity norm should not have been strongly activated in this task. Hence, the quantity of effort supplied by subjects equates the marginal cost of effort with the marginal benefit of the expected payment from exerting anagram-solving effort. Shifts in the amount of effort exerted across salience conditions reflect shifts in the marginal effort-cost function.[15] By restricting the task length to five minutes, we reduced the chances that primed subjects' religious identity salience would fade during the task.

We measured work provision in an incomplete contractual setting by running a labor market bilateral gift exchange game (Fehr et al., 1998; Charness, Frechette, & Kagel, 2004). We paired subjects together and told them that one of them would play the role of the Manager and the other the role of the Employee. We described the roles in these terms so that subjects would be more likely to use norms applicable to labor markets in their choices. After playing once, subjects played the game again, but this time in the opposite role and with a different partner.

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[15]Let e be the quantity of effort supplied, with units normalized so that the expected number of anagrams solved in the five minutes allotted is equal to e. Let p be the payment in the numeraire good per anagram solved, and let the increasing convex function $c(e)$ denote the utility cost of supplying effort. The subject's utility function is $U(e; p) = ep - c(e)$. As long as the optimal amount of effort e^* is interior, it is characterized by the first-order condition, $c'(e^*) = p$ Since p is the same across the treatment and control groups, changes in effort supply can be attributed to changes in the c' function.

In the first stage of the game, the Manager paid a wage to the Employee between $0 and $4 that is a multiple of 50 cents. The Manager could not change the wage later. In the second stage, the Employee saw the wage and chose a work quantity to supply that is an integer between 1 and 10, inclusive. Employees were told that the Manager would be shown their work quantity choice. The Employee's cost of work provided was an increasing convex function of work quantity: $0.00, $0.04, $0.08, $0.16, $0.24, $0.32, $0.40, $0.48, $0.60, and $0.72 as work quantity rose from 1 to 10. The Employee's earnings were the wage received minus the cost of work provided. The Manager's earnings were ($4 − wage paid) × work quantity provided by the Employee ÷ 10.

To facilitate calculation, we provided subjects a lookup table that showed the Manager's and the Employee's earnings at each wage and work quantity combination. Managers simply chose one wage, while Employees indicated a contingent work supply choice for each possible wage. After observing the Managers' wage choice, we implemented the Employees' work supply choice based on that wage. The profit-maximizing strategy for the Employee is to always supply the minimum amount of work, since the Manager is unable to contract on effort and has no opportunity to punish the Employee for shirking. If the Manager believes the Employee is a profit maximizer, his or her own profit-maximizing response is to offer a $0 wage. Despite these equilibrium predictions, the prior literature finds that, consistent with the presence of a reciprocity norm, Employees usually supply positive effort that is increasing in the wage offer. Managers anticipate this norm and rarely offer the minimum possible wage.

Because the Employee's work quantity is the choice of a number rather than effort in a real work task and because the cost of providing this work quantity is determined by a function that is the same for all subjects, differences in work provided across salience conditions are driven solely by changes in the strength of subjects' reciprocity norm and not by changes in the utility cost of effort.

F. Postexperimental Questionnaire

At the end of the session, after payoffs had been revealed, subjects completed a questionnaire that collected information about their demographic characteristics, beliefs about the experiment, and religious beliefs. As in the prime-validation experiment in section IV, we elicited religious affiliation using a multiple-choice question that did not include "none" as an option but did include "other." We also included numerous decoy questions to mask the purpose of the study, so that subjects would not contaminate future subjects by telling them that we were running an experiment about religion.

VI. Main Experiment Sample

Our sample consists of 254 Protestants, 199 Catholics, 95 Jews, and 269 agnostics/atheists. [16] We find that religious affiliations are almost perfectly balanced across treatment and control groups. In the treatment group, there are 126 Protestants, 101 Catholics, 43 Jews,

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[16]We count those who classified themselves as "Christian–Other" as Protestants after excluding a small number of Orthodox Christians and Mormons.

and 136 agnostics/atheists, compared with 128, 98, 52, and 133 in the control group, respectively. The results from section IV suggest that most of these agnostics and atheists would have chosen "none" if it had been an option in the multiple-choice question on religious affiliation. We analyze agnostics and atheists as a single group because their responses to questions about religious belief suggest that the difference between these groups is not sharp: although fewer self-categorized atheists have nonsecular religious beliefs than self-categorized agnostics, a nonnegligible fraction of atheists nonetheless hold nonsecular religious beliefs. For example, 11.3% of atheists and 17.9% of agnostics in our sample rate their agreement with the statement, "I believe in a God who watches over me," as 4 or higher on a 6-point scale, where higher numbers indicate greater agreement. On agreement with, "There is life after death," 15.3% of atheists and 33.6% of agnostics give a rating of at least 4. Note that these rates of belief in the supernatural are substantially lower than among the general U.S. population of religious nones, which may indicate that our sample of agnostics and atheists is more likely to hold an affirmative secular identity and that priming religion is less likely to activate nonsecular religious norms in them. Because we did not collect information on any previous religious affiliations of agnostics and atheists, we are unable to say which nonsecular religion's norms, if any, are important for this group.

In the postexperimental questionnaire, we asked subjects, "What do you think this study is about?" To avoid estimating treatment effects that are driven by experimenter demand effects, the above sample of 817 excludes 4 subjects who guessed that the study had something to do with religion.[17] In addition, we dropped 4 subjects who left more than half of the sentence-unscramble responses blank, since they were likely not to have been properly primed. Our results are not sensitive to including these dropped subjects.

Table 1 shows some summary statistics on our sample's characteristics. The genders are fairly evenly balanced, but compared to the average U.S. individual, our Cornell student subjects are less likely to have been born in the United States, come from wealthier families, and have higher SAT scores. These characteristics are typical of elite U.S. college students.

Overall, our sample practices religion less fervently than the U.S. population. Thirty-one percent of Protestants, 21% of Catholics, 5% of Jews, and 0% of agnostics and atheists in our sample attend religious services at least once per week. Thirty-nine percent of Protestants, 20% of Catholics, 4% of Jews, and 1% of agnostics and atheists pray daily. These percentages are lower than the 50% of Protestants, 42% of Catholics, 16% of Jews, and 5% of religious nones in the U.S. population who attend religious services at least weekly, and the 69% of Protestants, 58% of Catholics, 26% of Jews, and 22% of religious nones in the U.S. population who pray daily (Pew Forum on Religion and Public Life, 2008).

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[17]In the postexperimental questionnaire, we also asked subjects whether they believed that their experimental choices would affect their payments as the instructions specified. Ninety percent of subjects reported believing these payment promises. Among the subjects who participated in the dictator, public goods, and gift exchange games, 86% reported believing that their choices would affect other participants' payments exactly as we had specified.

VI. Main Experiment Results on Protestants and Catholics

The focus of our analysis is on Protestants and Catholics, since the validation experiment provided the strongest evidence that the priming instrument increases religious identity salience for these two groups.

A. Public Goods Game

Table 2A shows coefficients from regressing the amount contributed to the public good on a dummy for being in the religion-salient condition. The constant coefficients indicate that among unprimed subjects, Catholics contribute more than Protestants. However, selection into our sample of Cornell students is not random. And even if our sample were representative of each religion's members, the many unobserved variables that are correlated with religious affiliation would prevent us from inferring any causal effects of religion by simply comparing subject choices across religions. We instead turn to comparisons between the treatment and control groups within each religion, which identify the causal impact of religious identity salience using randomized variation.

In our data, religious identity salience increases public good contributions by 16 cents for Protestants but reduces them by 18 cents for Catholics. These results support the hypothesis that Protestantism is associated with norms that increase contributions to public goods, while Catholicism is associated with norms that decrease these contributions.[18]

Putnam (1993), La Porta et al. (1997), and Ruffle and Sosis (2007) argue that the channel through which religion affects public goods provision is trust. Among Catholics, identity salience does appear to affect public good contributions by changing the propensity to trust. Table 2B, which contains regressions of subjects' expectations of other group members' average contribution on a religion-salient dummy, indicates that relative to control Catholics, primed Catholics expect the average member of their group to contribute 12 cents less. However, there is no significant effect of priming on Protestant expectations. This suggests that Protestant identity salience's positive effect on public good contributions in panel A is not driven by a change in Protestants' trust.

Table 2C shows that subjects' reported expectations are strongly predictive of their choices. The coefficients in this panel are from a regression of contributions on a religion-salient dummy and the subject's expectation of other group members' average contribution. We find that subjects' own contributions increase almost one-for-one with their expectations. Once expectations are controlled for, primed Catholics do not contribute significantly less than unprimed Catholics, suggesting that the Catholic priming effect on contributions operates largely through Catholicism's negative effect on trust. In contrast, primed Protestants contribute a highly significant 13 cents more than unprimed Protestants even after controlling for expectations, suggesting the existence of a Protestant norm for unconditional cooperation. The Protestant priming coefficient is statistically distinguishable from the Catholic priming coefficient at the 5% level. The unconditional nature of the effect on

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[18]Interpreted through the lens of the model in section III, the facts that (a) these two priming effects go in opposite directions, (b) control Protestants contribute less than control Catholics, and (c) primed Protestants contribute more than primed Catholics suggest that the prime is activating different identities rather than a common ecumenical identity.

Protestants may suggest that the Protestant cooperation norm has its roots in an unconditional motive for cooperativeness, such as the notion that defection is costly because it is a negative signal about one's predestined salvation status, as in Blum and Dudley (2001). The evidence seems inconsistent with the Protestant norm originating from organizational mechanisms that assure members that they can contribute without being exploited because free riders will be punished or excluded, as in Putnam (1993) or Iannaccone (1992), although such mechanisms could play a role in keeping the norm viable. [19]

B. Risk Aversion

In the risk preference regression, our dependent variable is the minimum risk premium—that is, the expected return offered by the gamble in excess of the risk-free return—that the subject requires to accept the gamble. For example, if $X = \$2.40$ is the smallest X for which a subject would choose to gamble for a 50% chance of receiving X rather than accept a sure $1.00, then the reservation risk premium is $(2.40 \times 0.5 - 1)/1 = 0.20$.[20] Because we observe binary choices over only a finite number of risk premiums, we use an interval regression (Stewart, 1983), which is a generalization of the tobit estimator that accommodates dependent variable values that are not precisely observed but are known to lie somewhere within an interval. We observe two risk premiums for each subject—one for the small-stakes gamble and one for the larger-stakes gamble—so each subject appears in the regression twice, and we cluster standard errors by subject (Froot, 1989; Rogers, 1993).

Table 2D shows that religious identity salience causes Catholics' required risk premium to fall by 11 percentage points. This is a drop from 21% to 10% for the small-stakes gamble. The effect is significant only at the 10% level in this regression specification, but if we also control for a gender dummy (which is highly significant) to reduce residual variance, the priming effect point estimate becomes significant at the 5% level. We find no significant identity salience effects on risk taking for Protestants.

Our results are consistent with religious identity contributing to the documented spread in risk tolerance between Catholics and Protestants. However, we find no evidence that antigambling norms in Protestantism depress risk tolerance. In our data, the wedge identity salience creates is instead driven by Catholics becoming more risk seeking.[21] This finding is consistent with the hypothesis that Catholicism promotes gambling.

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[19]These organizational mechanisms were hypothesized to work on cooperation among coreligionists, so they may nonetheless be operative in such interactions, which our experiment does not study.

[20]In this formula, we treat the risk choice as investing a $1 endowment into either the risk-free or risky asset. Our decision to use the minimum risk premium as the dependent variable instead of the maximum risk premium that induces a safe choice matters only when participants' risk choices are inconsistent, switching between choosing the safe option and the risky option at more than one value of X. Since 95% of participants behaved consistently, our results are virtually identical if we use the maximum risk premium instead. Similarly, our choice to use the minimum interest rate in our time preference regressions in section VI.C does not matter because 97% of participants had a unique interest rate switch point.

[21]Although it is not the focus of their paper, Hilary and Hui (2009) find suggestive evidence that priming religion increases risk aversion in a sample of 120 undergraduates. It is difficult to directly compare their results with ours because they report only full-sample results and not the religious composition of their sample, they report only p-values (0.20 and 0.08 for their two regressions) rather than effect sizes, they use a different priming instrument, and their subjects made only two risk choices that are both hypothetical.

C. Discount Rate

In the time preference regressions, our dependent variable is the log of the minimum continuously compounded weekly interest rate that the subject requires to choose the later payment. That is, we apply the log operator once to transform the reservation gross interest rate into the continuously compounded net interest rate, and then we apply the log operator again. The second application of the log operator causes the estimator we describe below to assume that reservation continuously compounded interest rates are conditionally log normal, thus ruling out negative discount rates. Each subject appears in the regression twice because we have two discount rate observations: now versus one week in the future and one week versus two weeks in the future. Therefore, we cluster the standard errors by subject.

As in the risk preference elicitation, we only observe binary choices at a finite number of interest rates. Therefore, we use interval regressions to estimate subjects' reservation interest rates. In the interest rate regressions that follow, if the coefficients imply that a certain set of explanatory variable values is associated with a mean log continuously compounded interest rate of $\hat{\mu}$, then the median continuously compounded interest rate is $\exp(\hat{\mu})$.

Table 2E presents the regression evidence on how priming religious identity affects discount rates. The explanatory variables are a dummy for being in the religion-salient condition and a dummy for the intertemporal choice being between payments deferred for one week versus two weeks. Contrary to Weber's (1930) hypothesis, in neither case do we find that religious identity salience affects discount rates in a statistically significant way. Moreover, the point estimates of the priming effect are positive for both Catholics and Protestants. For example, the median interest rate required to delay receipt of money from today to one week later is 4.9% for unprimed Protestants and 6.9% for primed Protestants, and 2.9% for unprimed Catholics and 4.5% for primed Catholics. Our findings suggest that if Christian religions promote thrift and capital accumulation, they do so through channels other than identity-related norms.

D. Dictator Game

Despite having incentives to keep their entire $1 endowment for themselves, dictators in our dictator game usually gave away a positive amount of money to the subject with whom they were paired, although the proportion given away was far less than half on average. This is a typical result for dictator game experiments (e.g., Forsythe et al., 1994). Table 2F contains coefficients from regressing the amount of money given away on a dummy for being in the religion-salient condition. The constant terms indicate that when unprimed, the average amount given away is 17 or 21 cents, depending on the religious group.

The coefficients on the religion-salient dummy show that the prime does not induce either Protestants or Catholics to give away significantly more money. The priming effect point estimates are in fact negative but insignificant (–2 and –4 cents for Protestants and Catholics, respectively). We interpret the evidence overall as suggesting that Christian religious norms neither increase nor decrease generosity. We thus add to the body of work that finds only a weak correlation between religion and observed (as opposed to self-reported) generosity.

We fail to replicate the Shariff and Norenzayan (2007) and Ahmed and Salas (2011) findings that priming religion increases generosity in a dictator game, even though we use the same priming instrument as Shariff and Norenzayan and a similar one as Ahmed and Salas. Neither paper reports results separately by religion, but a rough meta-analysis using our full-sample results (which include the Jewish and agnostic/atheist results presented in section VIII) points to a very small, positive, and insignificant effect overall. Assuming independence between Shariff and Norenzayan's study 1 ($\beta = 0.238$, SE = 0.0645, $N = 50$), their study 2 ($\beta = 0.200$, SE 0.0810, $N\beta = = 50$), Ahmed and Salas's experiment (β 0.0714, SE = 0.0217, $N = 224$), and our experiment ($\beta = -0.033$, SE = 0.0215, $N = 554$), the combined estimate of the priming effect on dictator game giving for a \$1 endowment is \$0.022 with a standard error of \$0.016 ($p = 0.169$).[22]

E. Labor Market Tasks

In the anagram solving task, our Protestant and Catholic subjects on average supplied answers for around thirty anagrams during the five minutes allotted to them. But contrary to the hypothesis that religious identity salience will decrease the cost of work effort, the regression of anagrams attempted on a dummy for being in the religion-salient condition (in table 2G) shows that priming religious identity does not significantly affect the number of anagrams attempted among either religious group. The point estimate of the priming effect on Protestants is actually negative, although the coefficient is small (-0.71) and not significantly different from 0. In untabulated analysis, we instead used the number of anagrams correctly answered as the dependent regression variable and find qualitatively similar results.

Labor supply effects in the bilateral gift exchange game are summarized in figure 1, which plots the average amount Employees chose to expend on work-related costs for each wage offer. Among both Christian groups and experimental conditions, minimal labor is supplied at a \$0 wage offer. The amount Employees are willing to expend on work-related costs rises nearly linearly with wage offers between \$0 and \$3.50, indicating a strong reciprocity norm. In measuring reciprocity, we will ignore labor supply at a \$4.00 wage because at this wage, the Manager's earnings are always \$0 regardless of how much labor the Employee supplies. Therefore, any positive work-related expenditures by the Employee at a \$4.00 wage represents either intentional money burning as a gesture of goodwill and appreciation or confusion about the payoff formulas.[23]

It is readily apparent from figure 1 that the strength of reciprocity, as reflected in the slope of work expenditures with respect to wages between \$0 and \$3.50, does not appear to be greatly affected by priming. We formally analyze the priming effect on Employees' reciprocity in table 2H. The dependent variable in the regression is the subject-specific slope coefficient from a regression of the subject's work-related costs on wage offers from \$0 to

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[22]To conduct this calculation, we divide Shariff and Norenzayan's and Ahmed and Salas's coefficients and standard errors by their stake sizes in order to put all of the findings in units of allocating a \$1 endowment. In analyzing Shariff and Norenzayan's study 2, we ignore their second treatment group (which received a nonreligious prime).
[23]Primed Catholics appear to behave different from the other three religious groups with respect to money burning. Unlike every other religion × experimental condition cell, primed Catholics slightly increase their work expenditures as the wage goes from \$3.50 to \$4.00. The difference between primed and unprimed Catholic labor supply at a \$4.00 wage is not statistically significant, however.

$3.50. There are no significant effects for either Protestants or Catholics. These results are collectively inconsistent with the notion that there is a uniquely high Protestant work ethic norm.

Table 2I examines the effect of priming religious identity on wages offered by Managers. There is to our knowledge no clearly articulated hypothesis in the prior literature about religion's effect on managerial wage offers, but we report these results for completeness. The regression of managerial wage offers on a dummy for being in the religion-salient condition shows no significant effects of religious identity.

F. Treatment Interactions with Belief in Divine Punishment and Religious Service Attendance

Belief in divine punishment is a particular feature of religious belief that has been hypothesized to affect economic behavior. Religious service attendance is a costly action that signals stronger identification with the religion and serves as a channel through which religious messages, values, norms, and interactions with coreligionists are received. Therefore, in this section, we examine whether the priming effects we have identified as significant differ for subjects with a stronger belief in divine punishment or more frequent religious service attendance.

Understood through the lens of the theory in section III, a priming effect that changes sign across subpopulations can demonstrate heterogeneity in the primed category's norm. To see this point, let x_0^j be the optimal choice in the absence of identity considerations for members of subpopulation j and x_C^j be the religious identity category norm for this sub-population. Let $x^{j*}(0)$ be the subject's observed choice when unprimed and $x^{j*}(1)$ be the observed choice when primed. Recall that priming religion causes choices to move away from x_0^j and toward x_C^j.

Suppose we observe that priming causes the choices of subpopulations A and B to diverge from each other: $x^{A*}(1) < x^{A*}(0) \quad x^{B*}(0) < x^{B*}(1)$ the. Then we can infer that norm of A is different from the norm of B : $x_C^A < x_C^B$. Another instance where we can infer a difference in norms is when priming causes choices to move toward each other and cross: $x^{A*}(0) < x^{B*}(0)$, $x^{A*}(0) < x^{A*}(1)$, $x^{B*}(1) < x^{B*}(0)$, and $x^{B*}(1) < x^{A*}(1)$. In this case, we can conclude that $x_C^B < x_C^A$.

If a priming effect does not change sign across subpopulations, the treatment interaction is harder to interpret: as discussed in section III (implication 4), there are many different reasons why the magnitude of the priming effect might vary across individuals. Therefore, inferences about norms from significant treatment interactions that do not cause the treatment effect's sign to flip should be made cautiously. Similarly, null treatment interaction effects could arise from homogeneity in a norm or from heterogeneity in other factors that masks norm heterogeneity.

To enable us to study treatment-effect interactions empirically, we asked a subset of our subjects on a post-experimental questionnaire to rate on a six-point Likert scale their agreement with the statement, "God punishes people for their sins." We normalize this variable so that within each religious group, it has a 0 mean and unit variance. We also asked all subjects how often they attend religious services. Possible answers were "never," "less than once a month," "once a month," "a few times a month," "once a week," "a few times a week," "once a day," and "more than once a day." We create an indicator variable for whether the subject's attendance frequency is above the median for his or her religious group. Median attendance frequency is once a month for Protestants and less than once a month for Catholics.

Table 3 shows regressions where the explanatory variables include an interaction of the religion-salient dummy with either the strength of belief in divine punishment or with the indicator for greater-than-median frequency of religious service attendance. To reduce the number of hypothesis tests, we limit attention to the dependent variables and religious groups where we found statistically significant main effects of priming. We omit from the table regressions with Catholic trust as the dependent variable for the sake of brevity, since these results are similar to those from regressions with Catholic public good contributions as the dependent variable.

The treatment interactions with divine punishment belief and religious service attendance frequency are insignificant for both Protestants and Catholics. The null interactions with religious service attendance frequency could indicate that even if Protestant and Catholic norms in the examined domains had their genesis in intracongregational interactions (Putnam, 1993), their transmission today may not depend heavily on such interactions. However, as already discussed, null interaction effects need not indicate that norms are uncorrelated with the variable being interacted.

VIII. Main Experiment Results for Jews and Agnostics/Atheists

Table 4 consolidates all of the priming effect regressions for Jews and agnostics/atheists.

We might expect Judaism, due to its horizontal organization, to be associated with norms that increase contributions to public goods. We find no significant priming effect for Jews, but the small number of Jews in our sample makes it difficult to say whether the null Jewish effect is due to a true absence of an effect or a lack of statistical power; at the 5% level, we cannot reject equality with either the positive Protestant or the negative Catholic priming effect. Atheist and agnostic subjects' contributions are also not significantly affected by the prime, although the p-value of the treatment effect is less than 0.10.

We also find no sign that Judaism's prescription of "a life of simplicity and security" has an effect on Jews' risk choices, which is in accord with the nonexperimental evidence that Jews are not more risk averse than non-Jews. But the religious prime causes the average risk premium required for agnostics and atheists to forgo a sure payout to fall significantly by 12 percentage points. For the small-stakes gamble, this represents a fall from 16% to 4%. This result may suggest that the nonexperimental correlation between irreligiousness and low risk

aversion cannot be entirely explained by a causal effect from risk aversion to religiosity, as per the Pascal's wager hypothesis, but is at least partially due to a causal effect of norms among the irreligious. In untabulated regressions run separately for agnostics and atheists, we find that the priming effect is driven almost entirely by the agnostics ($\beta = -0.169$, $p = 0.003$) rather than the atheists ($\beta = -0.049$, $p = 0.517$).

How could a risk-tolerant norm for agnostics develop when very few of them hold an affirmative secular identity? One possibility (albeit highly speculative) regarding how Pascal's wager might in fact play a role is as follows: when a person takes the risk of turning away from religion, cognitive dissonance causes him to conceive of himself as the kind of person who is less risk averse, so that the initial low-risk aversion that motivated his choice becomes even lower (Festinger, 1957, 1964). Then, whenever religion is made salient, that self-concept of low risk aversion becomes more highly activated.[24]

We do not find any significant priming effects on discount rates, dictator game generosity, number of anagrams attempted, or wages offered as the Manager in the gift exchange game. But there appears to be a high Jewish work ethic norm based on reciprocity to the employer. Jews' subject-specific slope coefficient from a regression of the subject's work-related costs on wage offers from $0 to $3.50 increases significantly upon priming from 0.05 to 0.11. Such a norm is consistent with the Jewish teaching of gainful labor as an obligation. Perhaps unsurprisingly, given that Jews in our sample are not members of communities that study religion full time, that strand of thought seems to outweigh any countervailing subordination of the value of worldly labor to religious study.

Turning to treatment interactions with beliefs in divine punishment and religious service attendance frequency in domains where there is a significant average treatment effect, table 5 shows that among Jews, there is no relationship between belief in divine punishment and the effect of priming on Employee reciprocity, but there is a negative treatment interaction with the frequency of religious service attendance. Priming causes the slope of work expenditures with respect to wages to rise from 0.04 to 0.14 among Jews who attend services less than once a month or never, but to fall from 0.08 to 0.02 among Jews who attend services more frequently. Note that (a) the sign of the priming effect is positive for infrequent attenders and negative for frequent attenders and (b) unprimed infrequent attenders reciprocate less than unprimed frequent attenders, but primed infrequent attenders reciprocate more than primed frequent attenders. These point estimates suggest that there is a lower work reciprocity norm for Jews who attend religious services more often, consistent with the notion that Jews who devote more time to nonsecular activities place less weight on the norm of regarding worldly labor as an obligation than on the competing norm of devaluing worldly labor relative to religious study.

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[24]If this mechanism is at play, then one may wonder why (by a seemingly analogous argument) holding a nonsecular religious identity does not generically increase risk aversion due to cognitive dissonance. A difference may arise from the fact that in the United States, the default is to be religious, so most of those who are nonsecular did not actively engage with Pascal's wager, whereas most of those who are secular actively engaged with Pascal's wager. Another possibility is that countervailing norms within the religion offset the cognitive dissonance effect.

Among agnostics and atheists, neither belief in divine punishment nor religious service attendance frequency interacts significantly with the priming effect on risk aversion. The lack of an interaction with divine punishment beliefs may argue against the speculative hypothesis that the combination of Pascal's wager and cognitive dissonance is responsible for the negative priming effect on risk aversion within this group. However, it is not clear what the sign of this interaction should be. Holding fixed the level of irreligiousness, those with a stronger current belief in divine punishment may have a greater sense that they are taking a risk by not being religious; on the other hand, those who more thoroughly reject the possibility of divine punishment may be taking a bigger risk if punishment is increasing in deviation from orthodox belief.

IX. Multiple Hypothesis Testing and Type I Error

Although we have restricted our main hypothesis tests to behavioral domains already hypothesized by the previous literature to be affected by religion, the number of tests we have run is large, and many of the priming effects are insignificant. This raises the concern that our significant priming effects are chance artifacts arising from the large number of tested hypotheses.

To partially address this possibility, we test whether the main priming effects we estimated for our four religious groups on public good contributions, expectations of others' public good contributions, risk aversion, discount rates, dictator game generosity, anagrams attempted, and gift exchange reciprocity are jointly equal to 0. We pool all of our observations into a single interval regression and control for outcome-type dummies (e.g., a dummy for the dependent variable being a public good contribution amount, a dummy for the dependent variable being an amount given away in the dictator game, etc.) × religion dummies, outcome-type dummies × religion dummies × a religion-salient treatment dummy, and outcome-type dummies × religion dummies × additional outcome-specific control variables. Interval regressions can accommodate dependent variables that are point observations as a special case. We allow the residual's variance to vary by religion × outcome type and cluster standard errors by subject. This procedure causes every individual priming coefficient value we previously estimated to appear in a single coefficient vector, allowing us to run a Wald test for their joint equality with 0. We reject this hypothesis at $p = 0.005$.[25] This finding indicates that the identity salience=manipulation has some effect, although it does not establish the robustness of any particular result we report. As with any empirical research, our confidence in particular results will increase or decrease depending on the outcome of future replication attempts.

X. Conclusion

In our data, when Catholics have their religious identity made salient to them, they decrease their contribution to public goods. Consistent with the theory of Putnam (1993) that vertically organized religions decrease trust, Catholics' trust falls when religion is primed,

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[25]We exclude the priming effect on Managers' wage offers from this joint hypothesis test because there is no strong ex ante hypothesis from the literature about how religion would affect this dependent variable. If we include it, the *p*-value for the joint hypothesis test is 0.012.

and the decrease in Catholic public good contributions under priming is entirely explained by this fall in trust. But the companion hypothesis that horizontally organized religions such as Protestantism are associated with high-trust norms receives no support in our data, since Protestant trust is unaffected by priming. The fact that priming causes Protestants' public good contributions to increase despite no change in trust suggests that there exists a Protestant norm of high unconditional public good contributions. Such a norm is consistent with Blum and Dudley's (2001) hypothesis that high Protestant cooperation has its roots in afterlife beliefs that make uncooperative behavior especially costly. But notably, Weber's (1930) famous hypothesis about a Protestant ethic of hard work and thrift emerging from afterlife beliefs receives no support in our data.

A leading hypothesis regarding why irreligion is correlated with low risk aversion also involves afterlife beliefs: people with low risk aversion select into irreligiousness, since irreligiousness amounts to making the risky choice in Pascal's wager (Miller & Hoffman, 1995). Our finding that priming religion increases agnostic/atheist risk aversion suggests that there is a norm of low risk aversion for this group. This in turn suggests that selection into irreligiousness via Pascal's wager is not entirely responsible for the correlation between risk aversion and irreligiousness.

Moral teachings may shape the response to religious priming of Catholic risk aversion and Jewish labor supply reciprocity. It has been argued that Catholic practice encourages gambling. Consistent with a low-risk-aversion norm, Catholics in our sample become less risk averse when primed. Jewish teaching has two strands of thought that are in tension with each other: a view of gainful labor as a religious obligation and an idealization of full-time withdrawal from worldly labor for religious study. We find that the rate at which Jews increase work effort in response to higher wages goes up when primed, but only if they have below-median religious service attendance frequency, consistent with these two teachings carrying different weights in populations that devote different amounts of time to religious activities. If moral teachings to be generous— common across all the religions we study— were reflected in identity norms, then we would expect religious priming to increase generosity, but we do not find any effect. Similarly, Jewish and Protestant injunctions against risk taking are not reflected in the null priming effects on risk aversion that we find within these samples.

We conclude with two notes of caution in interpreting our results. First, when we fail to find an effect of identity salience, it does not necessarily rule out the effect's existence. It is possible that we did not have sufficient statistical power to detect the effect or that a stronger salience manipulation would have produced a detectable effect. It is also possible that the strength of our identity salience manipulation varies by religious group, making us more likely to find null effects in some groups than others. Our own direct evidence regarding the priming instrument's success in increasing religious identity salience among Jews, atheists, and agnostics is weak. Another possibility is that certain identity salience effects apply only to domains that do not overlap with what we measure. For example, making religious identity salient might increase work effort in a person's main job, which she considers to be her divine calling, but not in a laboratory task she is doing in her spare time. For all of these reasons, our positive findings merit greater emphasis than our null findings.

Second, while a key advantage of identity salience manipulations is that they can be randomly assigned, exogenously manipulating identity salience is not the same as exogenously manipulating religious affiliation. Identity salience does not provide information about channels other than identity norms through which religion might affect behavior. Identity salience manipulations would not detect, for example, the effect Catholic affiliation has on the likelihood of getting a better education due to the increased probability of attending a Catholic school and the consequent changes in choices and outcomes.

Despite these caveats, we believe that identity salience manipulations are worth exploring as a potentially useful tool for learning about some of the effects of religious identity. Religious identity will be salient outside the laboratory at least some of the time, so identity salience manipulations allow us to measure how behavior is likely to be affected during those times. Interpreting identity salience effects through the lens of self-categorization theory allows us to make additional inferences about what religious identity norms are and their marginal directional effect on steady-state behavior.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

Acknowledgments

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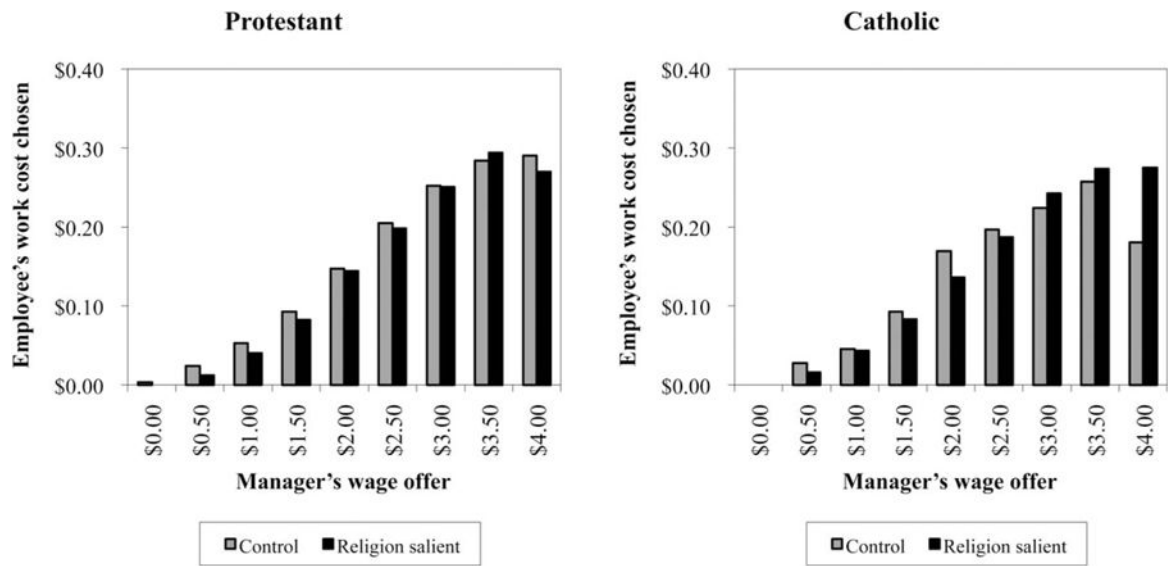
Figure 1.
Average Employee Work Costs Chosen in Response to Managerial Wage Offers in Gift Exchange Game,Protestants and Catholics

Table 1

Main Experiment Subject Characteristics

	Protestant	Catholic	Jewish	Agnostic/Atheist	Full Sample
% male	42%	46%	61%	49%	48%
% born in United States	79%	80%	96%	66%	77%
Median parental income	$80,000–$99,999	$80,000–$99,999	Over $100,000	$80,000–$99,999	$80,000–$99,999
Mean SAT Math score	710	707	722	743	723
Mean SAT Verbal/Critical Reading score	671	675	675	704	684
% who attend religious services at least once per week	31%	21%	5%	0%	15%
% who pray daily	39%	20%	4%	1%	18%
N	254	199	95	269	817

Subjects selected one of six income ranges to indicate their parents' annual income: under $20,000; $20,000–$39,999; $40,000–$59,999; $60,000–$79,999; $80,000–$99,999; and over $100,000. The row labeled "Median parental income" shows the range chosen by the median subject.

Table 2

Priming Effects for Protestants and Catholics

	Protestant	Catholic
A. Amount Contributed to Public Good		
Religion salient	0.16** (0.06)	−0.18*** (0.07)
Constant	0.53*** (0.05)	0.70*** (0.05)
N	171	138
B. Expectation of Others' Contribution to Public Good		
Religion salient	0.03 (0.05)	−0.12** (0.05)
Constant	0.60*** (0.03)	0.73*** (0.04)
N	171	138
C. Relationship between Own Contribution and Expectation of Others' Contribution		
Religion salient	0.13*** (0.05)	−0.07 (0.05)
E(Others' contribution)	0.90*** (0.06)	0.94*** (0.06)
Constant	−0.01 (0.05)	0.01 (0.06)
N	171	138
D. Risk Aversion		
Religion salient	−0.03 (0.06)	−0.11 (0.07)
Larger stakes	0.27*** (0.05)	0.31*** (0.05)
Constant	0.23*** (0.05)	0.21*** (0.05)
$\hat{\sigma}$	0.39 (0.03)	0.36 (0.03)
N	232	154
E. Discount Rate		
Religion salient	0.35 (0.36)	0.43 (0.50)
1 week versus 2 weeks	0.00 (0.11)	−0.17 (0.17)
Constant	−3.02*** (0.26)	−3.53*** (0.36)
$\hat{\sigma}$	1.93 (0.16)	2.18 (0.21)
N	232	154
F. Dictator Game		

	Protestant	Catholic
Religion salient	−0.02 (0.04)	−0.04 (0.04)
Constant	0.21 *** (0.03)	0.17 *** (0.04)
N	177	139

G. Number of Anagrams Attempted

	Protestant	Catholic
Religion salient	−0.71 (2.44)	1.76 (3.98)
Constant	29.03 *** (1.77)	31.09 *** (2.89)
N	77	60

H. Gift Exchange: Slope from Regression of Work Cost on Wages between $0 and $3.50

	Protestant	Catholic
Religion salient	0.00 (0.02)	0.01 (0.02)
Constant	0.09 *** (0.01)	0.08 *** (0.01)
N	99	91

I. Gift Exchange: Wage Offered as Manager

	Protestant	Catholic
Religion salient	0.05 (0.19)	0.01 (0.19)
Constant	1.39 *** (0.13)	1.28 *** (0.14)
N	99	91

This table shows regression results where the dependent variable is the amount contributed to the public good (panels A and C), the expectation of others' average contribution to the public good (panel B), the minimum risk premium required for a subject to accept a gamble (panel D), the log continuously compounded interest rate required to defer payment receipt (panel E), the amount given away (panel F), the number of anagrams attempted (panel G), the subject-specific slope coefficient from a regression of work cost expended as an Employee on managerial wage offered (panel H), or the wage offered as a Manager (panel I). "Religion salient" is a dummy for being in the religion-salient condition. "E(Others' contribution)" is the subject's expectation of other group members' average contribution to the public good. "Larger stakes" is a dummy for if the sure payout in the risky choice was $100. "1 week versus 2 weeks" is a dummy for the intertemporal choice being between payments deferred for one week versus two weeks. Ordinary least squares regressions were run for each panel except panels D and E, where interval regressions were run, pooling each subjects' two risk choices or two intertemporal choices together. in these latter two panels, the estimated conditional standard deviation of the latent dependent variable is denoted by $\hat{\sigma}$. Huber-White standard errors are in parentheses below the point estimates. These standard errors are clustered by subject in panels D and E. Significant at

**
5%,

***
1%.

Table 3

Religion-Salience Treatment Interactions with Belief in Divine Punishment and Religious Service Attendance, Protestants and Catholics

	Protestant		Catholic			
	Public Good Contribution	Public Good Contribution	Public Good Contribution	Public Good Contribution	Risk Premium	Risk Premium
Religion salient	0.14 (0.07)	0.14 (0.09)	−0.18** (0.08)	−0.20 (0.10)	−0.13 (0.09)	−0.15 (0.09)
Religion salient × Divine punishment	0.11 (0.07)		−0.06 (0.08)		−0.08 (0.09)	
Divine punishment	−0.09 (0.05)		0.04 (0.05)		0.06 (0.06)	
Religion salient × (Attendance > median)		0.02 (0.12)		0.03 (0.14)		0.08 (0.13)
Attendance > median		−0.13 (0.09)		0.00 (0.10)		0.05 (0.10)
Larger stakes					0.38*** (0.08)	0.30*** (0.05)
Constant	0.52*** (0.05)	0.59*** (0.07)	0.71*** (0.06)	0.70*** (0.07)	0.20*** (0.06)	019*** (0.06)
$\hat{\sigma}$					0.35*** (0.03)	0.35*** (0.03)
N	124	171	103	138	84	154

The column headings indicate the religious group and the dependent variable in the regression. The dependent variables are the amount contributed to the public good or the minimum risk premium required to accept a gamble. The public good coefficients are from an OLS regression; the risk premium coefficients are from an interval regression where we pool each subject's two risk choices together. "Religion salient" is a dummy for being in the religion-salient condition. "Divine punishment" is the self-reported belief in divine punishment, normalized to have 0 mean and unit standard deviation within each religious group. "Attendance > median" is a dummy for whether the subject reports religious service attendance frequency that is above the median for his or her religious group. "Larger stakes" is a dummy for the sure payout in the risky choice being $100. The estimated conditional standard deviation of the latent dependent reservation risk premium is denoted by $\hat{\sigma}$. Huber-White standard errors are in parentheses below the point estimates; these are clustered by subject for the risk aversion regressions. The number of observations corresponds to the number of subjects for the public good regressions, and the number of reservation risk premium intervals observed for the risk aversion regressions. Significant

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5%,

***
1%.

Table 4

Priming Effects for Jews and Agnostics/Atheists

	Jewish	Agnostic/Atheist
A. Amount Contributed to Public Good		
Religion salient	0.01 (0.12)	0.12 (0.06)
Constant	0.56*** (0.08)	0.49*** (0.04)
N	56	168
B. Expectation of Others' Contribution to Public Good		
Religion salient	0.08 (0.08)	0.09 (0.05)
Constant	0.61*** (0.05)	0.52*** (0.03)
N	56	168
C. Relationship between own Contribution and Expectation of Others' Contribution		
Religion salient	−0.07 (0.09)	0.04 (0.05)
E(*Others' contribution*)	1.01*** (0.11)	0.95*** (0.07)
Constant	−0.06 (0.06)	−0.01 (0.04)
N	56	168
D. Risk Aversion		
Religion salient	0.02 (0.06)	−0.12** (0.05)
Larger stakes	0.26*** (0.07)	0.26*** (0.04)
Constant	0.09** (0.04)	0.16*** (0.03)
$\hat{\sigma}$	0.26 (0.03)	0.32 (0.02)
N	80	196
E. Discount Rate		
Religion salient	0.33 (0.57)	0.08 (0.51)
1 week versus 2 weeks	−0.37 (0.27)	−0.30 (0.17)
Constant	−3.62*** (0.44)	4.40*** (0.42)
$\hat{\sigma}$	1.85 (0.20)	2.36 (0.18)
N	80	196
F. Dictator Game		
Religion salient	0.01 (0.07)	−0.05 (0.03)

	Jewish	**Agnostic/Atheist**
Constant	0.14***	0.17***
	(0.04)	(0.02)
N	62	176
G. Number of Anagrams Attempted		
Religion salient	4.92	2.12
	(5.99)	(2.63)
Constant	33.65***	32.02***
	(4.48)	(1.78)
N	33	93
H. Gift Exchange: Slope from Regression of Work Cost on Wages between $0 and $3.50		
Religion salient	0.06**	−0.02
	(0.03)	(0.02)
Constant	0.05***	0.09***
	(0.02)	(0.01)
N	40	113
I. Gift Exchange: Wage Offered as Manager		
Religion salient	0.42	−0.28
	(0.29)	(0.18)
Constant	0.98***	1.57***
	(0.21)	(0.12)
N	40	113

This table shows regressions corresponding to the regressions found in table 2 for Jews and agnostics/atheists. See the table 2 note for the methodology and variable definitions.

Table 5

Religion-Salience Treatment Interactions with Belief in Divine Punishment and Religious Service Attendance, Jews and Agnostics/Atheists

	Jewish		Agnostic/Atheist	
	Gift Exchange Reciprocity		**Risk Premium**	
Religion salient	0.06 (0.03)	0.10*** (0.03)	−0.13 (0.07)	−0.08 (0.06)
Religion salient × Divine punishment	0.01 (0.03)		0.06 (0.06)	
Divine punishment	−0.01 (0.02)		−0.05 (0.05)	
Religion salient × (Attendance > median)		−0.16** (0.07)		−0.23 (0.12)
Attendance > median		0.04 (0.06)		0.15 (0.08)
Larger stakes			0.23*** (0.04)	0.26*** (0.04)
Constant	0.05*** (0.02)	0.04** (0.02)	0.21*** (0.05)	0.13*** (0.04)
$\widehat{\sigma}$			0.30*** (0.03)	0.31*** (0.02)
N	40	40	116	196

The column headings indicate the religious group and the dependent variable in the regression. The dependent variables are the subject-specific slope coefficient from a regression of work cost expended as an Employee on managerial wage offered, and the minimum risk premium required to accept a gamble. The reciprocity coefficients are from an OLS regression; the risk premium coefficients are from an interval regression where we pool each subject's two risk choices together. "Religion salient" is a dummy for being in the religion-salient condition. "Divine punishment" is the self-reported belief in divine punishment, normalized to have 0 mean and unit standard deviation within each religious group. "Attendance > median" is a dummy for whether the subject reports religious service attendance frequency that is above the median for his or her religious group. "Larger stakes" is a dummy for the sure payout in the risky choice being $100. The estimated conditional standard deviation of the latent dependent reservation risk premium is denoted by $\widehat{\sigma}$. Huber-White standard errors are in parentheses below the point estimates; these are clustered by subject for the risk aversion regressions. The number of observations corresponds to the number of subjects for the reciprocity regressions, and the number of reservation risk premium intervals observed for the risk aversion regressions. Significant

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5%,

***
1%.