

Religious Identity and Economic Behavior^{*}

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Abstract

Although many scholars (e.g., Weber, 1930) have hypothesized that religious identity norms affect economic outcomes, empirical tests have been hampered by the difficulty of identifying exogenous variation in religion. We *create* exogenous variation by randomly varying religious identity salience in laboratory subjects. The marginal effect of religious identity is the change in subjects' choices when religion is salient. We test six hypotheses from prior literature. We find that Protestantism increases contributions to public goods. Catholicism decreases contributions to public goods, decreases expectations of others' contributions to public goods, and decreases risk aversion. Judaism increases worker reciprocity in a bilateral labor market gift-exchange game. We find no evidence of religious identity effects on disutility of work effort, discount rates, or generosity in a dictator game.

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At least since Weber (1930), scholars have hypothesized that norms tied to religious identities affect economic outcomes. Weber argued that Protestantism encouraged capital accumulation and a strong work ethic, thus leading to the rise of capitalism. More recently, Barro and McCleary (2003, 2006) find evidence that belief in heaven and hell increases GDP growth rates, a result that they hypothesize is due to the salutary effect of this belief on work ethic, honesty, trust, and thrift. Putnam (1993) and La Porta et al. (1997) argue that Catholicism inhibits trust, which has negative effects on GDP growth, government efficiency, the production of public goods, and the maximum feasible size of corporations. Putnam (1993) also argues that Protestantism promotes trust. Relatedly, Ruffle and Sosis (2007) find that participation in collective religious rituals is associated with greater trust and cooperativeness. Stulz and Williamson (2003) show that a country's principal religion is correlated with the strength of its creditor rights; Guiso, Sapienza, and Zingales (2003) find positive correlations between Christian religions and attitudes conducive to economic growth; and Hilary and Hui (2009) and Kumar, Page, and Spalt (2009) argue that religious risk norms affect corporations' investment decisions and individuals' stock portfolios.¹

However, causal inference about the effect of religious identity norms has been hampered by the difficulty of identifying exogenous variation in religious identities. Religious beliefs are endogenous choices and are likely to be correlated with many unobserved factors that have causal impacts on economic outcomes.

In this paper, we *create* exogenous variation in the strength of religious identity norms. According to “self-categorization theory,” a long-standing idea in psychology (James, 1890; Turner, 1985), each person belongs to multiple social categories, such as religion, gender, and occupation, which each has its own set of norms. 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 towards the salient category's norm. We can therefore identify the marginal behavioral effect of religious identity norms by making religious identity salient to a

¹ There is also a large literature finding religiosity has a positive correlation with salutary individual outcomes, which could be due to the causal impact of religious norms. These outcomes include higher educational attainment, higher income, lower levels of welfare receipt and disability, higher marriage probability, lower divorce probability, better health, greater self-reported happiness, and greater resilience to childhood disadvantage (e.g. Freeman, 1986; Ellison, 1991; Levin, 1994; Gruber, 2005; Dehejia et al., 2009). Becker and Woessmann (2009) argue that most of the Catholic-Protestant prosperity gap in late-19th-century Prussia can be accounted for by higher literacy among Protestants, driven by Protestants' desire to read the Bible. Iannaccone (1998) surveys much of this literature.

randomly selected subset of laboratory subjects and observing how these primed subjects' subsequent behavior differs from the behavior of unprimed subjects. This methodology has previously been used to identify economic effects of racial, ethnic, and gender identity norms by Benjamin, Choi, and Strickland (forthcoming).

Using priming techniques, we examine six hypotheses derived from prior literature:

- H1.* Contributions to public goods are affected positively by Protestantism and negatively by Catholicism (Putnam, 1993; La Porta et al., 1997).
- H2.* Trust is affected positively by Protestantism and negatively by Catholicism, and trust is the mechanism explaining the relationship between religion and public goods contributions (Putnam, 1993; La Porta et al., 1997).
- H3.* Financial risk-taking is reduced by religion. Kumar, Page, and Spalt (2009) argue that risk-taking is reduced by Protestantism, whereas Hilary and Hui (2009) argue that risk-taking is reduced by both Protestantism and Catholicism.
- H4.* Thrift and capital accumulation are promoted by religion (Weber, 1930; Guiso, Sapienza, and Zingales, 2003; Barro and McCleary, 2003, 2006).
- H5.* Generosity is increased by religion (Friedrichs, 1960; Batson, Schoenrade, and Ventis, 1993).
- H6.* Work ethic is increased by religion, and especially by Protestantism (Weber, 1930; Barro and McCleary, 2003, 2006).

To operationalize these hypotheses in a laboratory setting, we map them into seven outcomes that can be measured using standard experimental methods. The first five outcomes are contributions to a laboratory public good, expectations about *others'* contributions to the laboratory public good, elicited risk aversion, elicited discount rates, and the amount given away in a dictator game. Since work ethic could be interpreted as a low disutility of effort or as a high reciprocity motive toward an employer when labor contracts are incomplete, we measure two outcomes corresponding to each of these concepts: the number of anagrams attempted at a constant piece-rate wage, and the rate at which effort expended as a worker rises with wage offers in a labor market bilateral gift-exchange game.

Consistent with *H1*, we find that Protestantism increases contributions to the laboratory public good, while Catholicism decreases contributions. In partial support of *H2*, we find that Catholicism decreases expectations of others' contributions to the public good, and Catholicism has no statistically significant effect on contributions once its impact on trust is controlled for. However, Protestantism does not affect trust, suggesting that Protestantism's positive impact on public good contributions comes from a norm to unconditionally contribute to public goods.

Our data do not support *H3*. In accordance with Kumar, Page, and Spalt (2009), we do find that religious identity norms cause Protestants to become relatively more risk averse than Catholics, but this is because Catholicism *increases* risk-taking, rather than Protestantism reducing risk-taking as Kumar, Page, and Spalt (2009) hypothesize.

We find no evidence for Judeo-Christian identity effects on discount rates and generosity (*H4* and *H5*). Nor do we find Christian work ethic effects (*H6*). However, we do find a work ethic effect for Judaism; among Jews, priming religion increases the rate at which workers increase their effort in response to higher wages in the gift-exchange game.

We are aware of three prior psychology papers that manipulate religious identity salience and measure changes in subsequent behavior. Shariff and Norenzayan (2007) find that priming religion increases generosity in a dictator game. Even though we use the same priming instrument they do, we are unable to replicate their result in our much larger sample. None of our dictator game treatment effects are statistically significant, and the point estimates indicate that dictator game generosity, if anything, slightly *decreases* when religion is made salient. Toburen and Meier (forthcoming) use the Shariff and Norenzayan (2007) priming instrument and find that religiously primed subjects given unsolvable anagrams to solve as part of a "verbal intelligence test" (for which they were not paid) spend more time on the task. Randolph-Seng and Nielsen (2007) find that priming religion reduces the frequency of high performance in an unmonitored laboratory task, which they interpret as a reduction in cheating.² In the economics literature, Hilary and Hui (2009) find suggestive experimental evidence (not reported separately by religion) that priming religion increases risk aversion in hypothetical risk choices. In our larger sample using incentivized choices, we find evidence in the opposite direction for Catholics.

² There have been many other studies examining the effect of making non-religious identities salient, including Reicher and Levine (1994), Forehand, Deshpandé, and Reed II (2002), and LeBoeuf, Shafir, and Bayuk (2010).

There are also a number of papers that report correlations between religious affiliation and behavior in economics experiments (e.g., Anderson and Mellor, 2009; Tan and Vogel, 2008).

Our paper proceeds as follows. Section I describes a theoretical framework for understanding how priming effects allow us to make inferences about norms. Section II describes the pilot experiment we used to confirm that our priming instrument has the desired effect on identity salience. Section III describes the methodology of our main experiment. Section IV presents the main experiment's empirical results, including a joint hypothesis test to address concerns about Type I error due to multiple hypothesis testing. Section V concludes.

I. A Theoretical Framework

Within our theoretical framework, which is inspired by Akerlof and Kranton (2000) and developed in Benjamin, Choi, and Strickland (forthcoming), priming a social category reveals the marginal effect of increasing the strength of affiliation with that category. 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 Protestantism, with strength $s \geq 0$. Let x_0 denote the individual's preferred choice in the absence of identity considerations, and let x_C denote the choice prescribed for members of social category C . The individual chooses x to maximize

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

where $0 \leq w(s) \leq 1$ 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 prescribed 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 $\bar{s} + \varepsilon$ by a category prime, where $\varepsilon > 0$.

The first-order condition of (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 that guide our analysis.

First, the higher the steady-state strength \bar{s} of the category affiliation, the closer x^* is to x_C in steady state. Second, 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 why priming manipulations are a useful experimental procedure for studying how identity affects steady-state preferences. Third, 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, 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.

Finally, 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 $x_C > x_0$. 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 he or she evaluates the w function differ. Priming will have no effect if there is no identity norm for choices in the measured domain, in which case the person always chooses x_0 regardless of identity salience.

II. Validating the Priming Instrument

The priming instrument, first used by Shariff and Norenzayan (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. 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 and Petty, 2001).

We recruited 91 students at the University of Michigan for a pilot experiment to confirm that the priming instrument increases the strength of religious identity affiliation. Subjects were randomly assigned to complete the religion-salient task or the control task. Subjects were not aware that this task differed across subjects. Immediately after the sentence unscramble, the questionnaire asked: “What five aspects of your identity (such as ‘male/female’ or ‘college student’) are most important to you?” Forty-seven percent of subjects in the religion-salient condition listed a religious identity in response, compared to only 25 percent of subjects in the control condition. This difference is significant at the 3 percent level (z -statistic = 2.16).

In contrast, for each of the other identities listed by subjects (in descending order of frequency, they were: gender, student, nationality / ethnicity, personality trait, family relationship, professional interest, non-family relationship, age group, activity group, political orientation, physical feature, and socioeconomic class), the p -value for a test of differences across conditions in the proportion of subjects who mentioned it varied from 25 to 97 percent. This evidence suggests that the priming instrument increases religious identity salience without affecting the salience of other identities.

III. Main Experiment Procedure

Participants in the main experiment were 827 Cornell University students. To avoid making religious identity salient to all the subjects, 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.

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. We describe the games, elicitations, and anagram task below. 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. The order in which the post-unscramble tasks appeared varied across sessions.

A. Public goods game

We measured the willingness to contribute to a public good by assigning each subject to a group of four and endowing him or her with \$1.³ 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 percent.

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.⁴ Subjects' payments did not depend upon the value of this guess.

Existing evidence suggests that behavior in laboratory public goods games like ours is associated with contributions to public goods outside the laboratory. Carpenter and Seki (forthcoming) find that cooperativeness in a laboratory public good game is positively correlated with the productivity of Japanese fishermen who pool their catch with other fishermen, and Fehr and Leibbrandt (2008) find that Brazilian fishermen who contribute more to a laboratory public good are less likely to over-exploit their common fishing ground. Laury and Taylor (2008) find that the amount contributed to a laboratory public good is positively correlated with the willingness to contribute money to a local tree-planting organization.⁵

B. Risk aversion elicitation

We elicited small-stakes risk preferences by asking participants to make six binary choices between \$1 for sure and a 50 percent chance at a larger amount, ranging from \$1.60 to

³ Marwell and Ames (1979) were the first to run this type of experiment to study the willingness to contribute to public goods.

⁴ 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.

⁵ However, Laury and Taylor (2008) find that this relationship is driven by the most selfish subjects, and that differences in contributions among less selfish subjects do not reliably predict differences in contributions to the field public good. This may be because the choices about contributing to the laboratory public good and the field public good occurred close together in time. Virtuous behavior tends to encourage more selfish behavior subsequently due to the “licensing effect” (Kahn and Dhar, 2006); because an individual has bolstered his self-concept as a virtuous person through the initial virtuous act, he is freed to act selfishly afterwards.

\$3.60. We paid subjects for all six choices in accordance with their stated preferences; for example, if a subject selected the sure \$1 payment if and only if winning the gamble yielded \$3.60, then she would have a 50 percent chance of earning \$5 and a 50 percent 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.⁶ The section's instructions made it clear that the questions were not intended to evaluate subjects normatively: "It's important to keep in mind that there are no right or wrong answers here. Which choice you make is a matter of personal preference." We used this same wording again in the instructions for the discount rate elicitation.

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 such as smoking, drinking, failing to hold insurance, holding stocks rather than Treasury bills, being self-employed, switching jobs, and moving residences.⁷

C. Discount rate elicitation

We measured time preferences by asking participants to make 12 binary choices between receiving \$10 now and receiving some larger amount one week from now, and another 12 binary choices between receiving \$10 one week from now and receiving some larger amount two weeks from now. The larger delayed amounts ranged from \$10.10 to \$15. 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 via post-dated check.⁸

Our approach to measuring time preferences is standard (Frederick, Loewenstein, and O'Donoghue, 2002). Similar measures predict variation in discounting-related behaviors such as drug addiction, cigarette smoking, excessive gambling, use of commitment savings devices,

⁶ 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 one in 1,444 chance of one large-stakes risk choice being implemented.

⁷ See Barsky et al. (1997), Guiso and Paiella (2008), Dohmen et al. (2005), and Sahn (2007).

⁸ 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.

borrowing on installment accounts and credit cards, rapid exhaustion of food stamps, delayed application to an MBA program, and defaulting on loans.⁹

D. Dictator game

In our implementation of the dictator game (Kahneman et al., 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 in terms of economic theory as relating to an individual's disutility of effort, which determines the willingness to exert a contracted-upon 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 anagram-solving effort. Shifts in the amount of effort exerted across salience conditions reflect shifts in the marginal effort cost function.¹⁰ By

⁹ See Fuchs (1982), Bickel, Odum, and Madden (1999), Kirby, Petry, and Bickel (1999), Petry and Casarella (1999), Kirby and Petry (2004), Shapiro (2005), Ashraf, Karlan, and Yin (2006), Meier and Sprenger (2010), and Reuben, Sapienza, and Zingales (2009).

¹⁰ In mathematical notation, 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.

restricting the task length to five minutes, we reduced the chances that primed subjects' religious identity salience would fade during the task.¹¹

We measured work provision in an incomplete contractual setting by running a labor market bilateral gift-exchange game (Fehr et al., 1998), modeled on one of the implementations of Charness, Frechette, and 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.

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 $(\$4 - \text{wage paid}) \times \text{work quantity provided by the Employee} \div 10$.

To facilitate calculation, we provided subjects a lookup table that showed the Manager and 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.

¹¹ Subjects given the anagram-solving task performed no other tasks after the sentence unscramble.

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. Debriefing questionnaire

At the end of the session, after payoffs had been revealed, subjects completed a debriefing questionnaire that collected information about their demographic characteristics, beliefs about the experiment, and religious beliefs. 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. We discuss responses to the relevant questions in further detail in Section IV.

IV. Main Experiment Results

A. Sample selection

Our sample consists of 264 Protestant/other Christians (whom we refer to collectively as “Protestants”), 199 Catholics, 95 Jews, and 269 atheists/agnostics.¹² In the debriefing 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 827 excludes four subjects who guessed that the study had something to do with religion.¹³ In addition, we have dropped four 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.

¹² Since we analyze results separately by religion, it would be problematic if the priming manipulation affected how participants categorized themselves, e.g., causing marginal atheist/agnostics to identify themselves with the religious tradition in which they were raised. We find that religious affiliations are almost perfectly balanced across treatment and control groups. In the treatment group, there are 130 Protestants, 101 Catholics, 43 Jews, and 136 atheists/agnostics, compared with 134, 98, 52, and 133 in the control group, respectively.

¹³ At the end of the experiment, 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, 88 percent reported believing that their choices would affect *other* participants’ payments exactly as we had specified.

B. Public goods game

Panel A of Table 1 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 the most on average, agnostics and atheists contribute the least, and Protestants and Jews are in the middle. 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 the causal effect of religion by simply comparing subject choices across religions. To learn about the impact of religion, we instead turn to comparisons between the treatment and control groups within each religion.

We find, consistent with the conclusions of Putnam (1993) and La Porta et al. (1997), that Protestantism increases the supply of public goods while Catholicism suppresses it. Protestants for whom religious identity is salient contribute 15 cents more to the public good than control Protestants, whereas primed Catholics decrease their contributions by 18 cents. Jewish subjects' contributions are unaffected by the prime. Interestingly, the prime has a marginally significant effect on atheists and agnostics, increasing their contributions by 12 cents (p -value = 0.08). This could be due to the existence of an atheist or agnostic norm regarding public good contributions, but it may alternatively arise from the activation of residual religious norms present among subjects who were raised in a faith and have subsequently abandoned it.

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, our data support this hypothesis. Panel B of Table 1, 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. Primed atheists and agnostics expect others to contribute 9 cents more, a marginally significant result. However, there is no significant effect of priming on Protestant expectations. This suggests that Protestantism's positive effect on public good contributions in Panel A is due to a Protestant norm to *unconditionally* contribute to public goods, rather than a Protestant norm to trust others.

Panel C shows that subjects' reported expectations are not just cheap talk. 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.¹⁴ Once expectations are controlled for, primed Catholics do not contribute significantly less than unprimed Catholics, indicating that the Catholic priming effect on contributions operates entirely through Catholicism's negative effect on trust. Similarly, the positive priming effect on atheist and agnostic contributions is no longer significant after controlling for expectations. In contrast, primed Protestants contribute a highly significant 12 cents more even after controlling for expectations. The Protestant priming coefficient is statistically distinguishable from the Catholic priming coefficient at the 5 percent level.

C. 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 the subject would choose to gamble for a 50 percent chance of receiving X rather than accept a sure \$1 if X is at least \$2.40, then the reservation risk premium is $(2.40 \times 0.5 - 1)/1 = 0.20$.¹⁵ Because we observe binary choices over only a finite number of risk premia, 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 premia 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).

¹⁴ In untabulated regressions, we find that this relationship does not differ significantly between primed and unprimed subjects in any of the religious groups.

¹⁵ 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 only matters 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 percent 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 IV.D does not matter because 97 percent of participants had a unique interest rate switch point.

Table 2 shows that priming religious identity causes the average risk premium required to entice agnostics and atheists to forego a sure payout to fall by 12 percentage points. For the small-stakes gamble, this represents a fall from 16 percent to 4 percent. We also find evidence that religious identity salience causes Catholics' risk premium to fall by 11 percentage points. This is a drop from 21 percent to 10 percent for the small-stakes gamble. The effect is significant only at the 10 percent level in this regression specification, but if we additionally control for a gender dummy (which is highly significant), the priming effect point estimate becomes significant at the 5 percent level. We find no significant identity effects on risk-taking for Protestants and Jews.

Our results are consistent with Kumar, Page, and Spalt's (2009) conclusion that religious norms raise the risk-taking of Catholics relative to Protestants. But whereas they argue that this effect occurs because of risk-averse Protestant norms, we find that the effect is instead driven by risk-seeking Catholic norms. Our two negative Christian priming point estimates are inconsistent with Hilary and Hui's (2009) argument that both Protestantism and Catholicism discourage risk-taking.¹⁶

D. Discount rate

In the time preference regression, 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 our 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

¹⁶ 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 (.20 and .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.

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 3 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 the hypothesis that religious identity promotes thrift and capital accumulation (Weber, 1930; Guiso, Sapienza, and Zingales, 2003; Barro and McCleary, 2003 and 2006), in no case do we find that religious identity affects discount rates in a statistically significant way. Moreover, the point estimates of the priming effect are positive for all religions.

E. 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 4 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 between 14 and 22 cents, depending on the religious group.

The coefficients on the religion-salient dummy show that Protestant, Catholic, and Jewish identities do not induce subjects to give away more money. The Protestant and Catholic priming effect point estimates are in fact negative. Among atheists and agnostics, the priming effect is negative and significant at the 10 percent level.

Thus, we fail to replicate the Shariff and Norenzyan (2007) finding that priming religion increases generosity in a dictator game, even though we use the same priming instrument they do. Shariff and Norenzyan do not report results separately by religion, but a rough meta-analysis using full-sample results points to a precisely estimated zero effect overall. Assuming independence between their Study 1 ($\beta = 0.238$, $s.e. = 0.0645$, $N = 50$), their Study 2 ($\beta = 0.200$, $s.e. = 0.0810$, $N = 50$), and our experiment ($\beta = -0.034$, $s.e. = 0.0214$, $N = 553$), the combined

estimate of the priming effect on dictator game giving for a \$1 endowment is \$0.004 with a standard error of \$0.019.¹⁷

F. Labor market tasks

In the anagram solving task, our subjects on average supplied answers for around 30 anagrams during the five minutes allotted to them. But contrary to the hypothesis that religious identity norms decrease the cost of work effort, the regression in Table 5 of anagrams attempted on a dummy for being in the religion-salient condition shows that priming religious identity does not significantly affect the number of anagrams attempted among any of our religious groups. The point estimate of the priming effect on Protestants, who were the object of Weber's (1930) Protestant work ethic hypothesis, is in fact negative, although this is not significantly different from zero. In untabulated analysis, we have used the number of anagrams correctly answered as the dependent regression variable instead 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 all religious groups and experimental conditions, minimal labor is supplied at a \$0 wage offer.¹⁸ 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 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 wage represents pure money burning as a gesture of goodwill and appreciation (or confusion about the payoff formulas).¹⁹

¹⁷ To conduct this calculation, we divide Shariff and Norenzayan's coefficients and standard errors by 10, since their subjects were told they were allocating a \$10 endowment instead of a \$1 endowment. In analyzing their Study 2, we ignore their second treatment group (which received a non-religious prime). Of course, differences in procedure or subject population may explain our non-replication. A potentially relevant difference is that their study involved deception; subjects' choices were not actually implemented. Batson, Schoenrade, and Ventis (1993) review laboratory evidence that religion is related to self-reported helping behavior, but its relationship to actual behavior is much weaker. This pattern may help explain the divergent findings if incentive-compatibility was less credible to subjects in Shariff and Norenzayan's experiment.

¹⁸ The absence of positive religious identity effects on purely altruistic labor provision in the gift-exchange game at a \$0 wage is consistent with the lack of religious identity effects on pure altruism in the dictator game.

¹⁹ At a \$4 wage, work expenditures are usually smaller than at a \$3.50 wage. Primed Catholics appear to behave quite differently than other groups with respect to money burning. Unlike every other religion \times experimental condition cell, primed Catholics slightly increase their work expenditures as the wage goes from \$3.50 to \$4. The

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, increases greatly among primed Jews relative to unprimed Jews. Other religious groups' reciprocity does not appear to be nearly as affected by priming. We formally analyze the priming effect on Employees' reciprocity in Panel A of Table 6. The dependent regression variable is the subject-specific slope coefficient from a regression of the subject's work-related costs on wage offers from \$0 to \$3.50. Jewish identity significantly increases this slope from 0.05 to 0.11, but there are no significant effects for any other group.

Panel B of Table 6 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, so we report these results mainly for completeness.²⁰ The regression of managerial wage offers on a dummy for being in the religion-salient condition shows no significant effects of religious identity.

G. Multiple hypothesis testing and Type I error

Although all of the main hypotheses we have tested correspond to existing hypotheses in the literature, 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 address this possibility, we test whether the priming effects of each religion 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 zero.²¹ We reject this hypothesis at $p = 0.006$.²²

difference between primed and unprimed Catholic work expenditures at a \$4 wage is not statistically significant, however.

²⁰ However, since one motivation for a Manager to pay a positive wage is that he or she expects the Employee to reciprocate the kindness, one might expect that hypotheses about religious identity effects on trust in the public goods context would also apply to Managerial wage offers. Our results suggest Catholic norms about trust are specific to public good contexts.

²¹ To run this test, we pool all of our observations into a single interval regression and control for outcome type dummies, outcome type dummies \times religion dummies, and outcome type dummies \times religion dummies \times 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 \times outcome type and cluster standard errors by subject.

H. Treatment interactions with belief in divine punishment and religious service attendance

In this subsection, we examine whether the priming effects we have identified as significant for Christians or Jews differ for subjects with a stronger belief in divine punishment or more regular religious service attendance. Barro and McCleary (2003, 2006) find that the positive association between religion and GDP growth appears to operate most strongly through belief in the existence of hell, which could be a powerful motivator of behavior. In contrast, they find a negative association between GDP growth and the frequency of religious service attendance. Although Barro and McCleary interpret this negative relationship as arising from religious service attendance being a proxy for real resources being diverted to religious activities rather than economic production, it is possible that frequent attendees have different norms than infrequent attendees, even holding beliefs about the afterlife fixed. Differences in priming effect sizes across individuals could be due to differences in norms. But in Section I, we discussed other reasons why priming effect sizes could vary, so we interpret these treatment interactions with caution.

Our debriefing questionnaire asked a subset of our subjects 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 zero 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, less than once a month for Catholics and Jews, and never for atheists and agnostics.

Table 7 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.²³ To keep the

²² 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 0.015.

²³ Interpreting the interaction coefficient could be problematic if the prime affected self-reports of belief in divine punishment or frequency of religious service attendance. We find no systematic differences in either of these variables across treatment and control groups, with the exception that primed Jews report substantially higher belief

number of interaction regressions manageable, we limit attention to the dependent variables and religious groups where we found statistically significant main effects of priming. For the sake of brevity, we omit from the table the regressions with Catholic trust as the dependent variable, since these results are similar to those from the regressions with Catholic public good contributions as the dependent variable. We also omit regressions involving agnostics and atheists because interactions with religious belief or religious service attendance are difficult to interpret for this group.

There is a marginally significant interaction for Protestants' contribution to the public good. Protestants with the mean belief in divine punishment marginally increase their contribution from 52 cents when not primed to 66 cents when primed. For Protestants with a belief in divine punishment one standard deviation above the mean, the treatment effect appears to be 26 cents instead of 14 cents. This effect, if real, is consistent with Barro and McCleary's (2003, 2006) hypothesis that the salutary effects of belief in divine punishment operate through changes in norms.

We find no statistically significant interactions between attendance frequency and religious identity effects among Protestants and Catholics. Among Jews, we find that the increase in Employee reciprocity in response to the religious prime occurs primarily among those who attend religious services relatively *infrequently*.²⁴

IV. Conclusion

The debate about religion's effect on economic outcomes has been hindered by the difficulty in identifying exogenous variation in religion. In this paper, we created exogenous variation by experimentally manipulating the salience of religious identity in laboratory subjects. The long-standing psychological theory of self-categorization predicts that norms associated with an identity have a temporarily greater behavioral influence when that identity is salient.

in divine punishment ($p < .05$). However, we find that the coefficients on belief in divine punishment and its interaction with the treatment dummy are essentially equal to zero in the gift-exchange reciprocity regression among Jews in Table 7.

²⁴ Because we varied the order of experimental games and preference elicitation across sessions, another question we can examine is 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.

Therefore, we can identify the marginal directional effect of religious identity norms on economic choices by seeing how those choices change when religious identity salience varies exogenously.

We find that Protestantism increases contributions to public goods. Catholicism decreases contributions to public goods, expectations of others' contributions to public goods, and decreases risk aversion. Judaism increases labor market reciprocity. However, we find no evidence that religious identity affects discount rates or purely altruistic generosity.

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Table 1. Public Goods Game Results

	Protestant	Catholic	Jewish	Agnostic/Atheist
Panel A. Amount contributed to public good				
<i>Religion salient</i>	0.15** (0.06)	-0.18*** (0.07)	0.01 (0.12)	0.12* (0.06)
Constant	0.53*** (0.05)	0.70*** (0.05)	0.56*** (0.08)	0.49*** (0.04)
<i>N</i>	180	138	56	168
Panel B. Expectation of others' contribution to public good				
<i>Religion salient</i>	0.03 (0.05)	-0.12** (0.05)	0.08 (0.08)	0.09* (0.05)
Constant	0.59*** (0.03)	0.73*** (0.04)	0.61*** (0.05)	0.52*** (0.03)
<i>N</i>	180	138	56	168
Panel C. Relationship between own contribution and expectation of others' contribution				
<i>Religion salient</i>	0.12*** (0.04)	-0.07 (0.05)	-0.07 (0.09)	0.04 (0.05)
<i>E(Others' contribution)</i>	0.90*** (0.05)	0.94*** (0.06)	1.01*** (0.11)	0.95*** (0.07)
Constant	-0.00 (0.04)	0.01 (0.06)	-0.06 (0.06)	-0.01 (0.04)
<i>N</i>	180	138	56	168

Note: This table shows regression results where the dependent variable is the amount contributed to the public good (Panels A and C) or the expectation of others' average contribution to the public good (Panel B). *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. Huber-White standard errors are in parentheses below the point estimates. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Table 2. Risk Aversion Results

	Protestant	Catholic	Jewish	Agnostic/ Atheist
<i>Religion salient</i>	-0.03 (0.06)	-0.11* (0.07)	0.02 (0.06)	-0.12** (0.05)
<i>Larger Stakes</i>	0.27*** (0.04)	0.31*** (0.05)	0.26*** (0.07)	0.26*** (0.04)
Constant	0.23*** (0.05)	0.21*** (0.05)	0.09** (0.04)	0.16*** (0.03)
$\hat{\sigma}$	0.39 (0.03)	0.36 (0.03)	0.26 (0.03)	0.32 (0.02)
<i>N</i>	242	154	80	196

Note: This table presents interval regressions where the latent dependent variable is the minimum risk premium required for a subject to accept a gamble. We pool each subject's two risk choices together. *Religion salient* is a dummy for being in the religion-salient condition. *Larger stakes* is a dummy for if the sure payout in the risky choice was \$100. The estimated conditional standard deviation of the latent dependent variable is denoted by $\hat{\sigma}$. Huber-White standard errors, clustered by subject, are reported in parentheses below the point estimates. The final row shows the number of reservation risk premium intervals in the regressions. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table 3. Discount Rate Results

	Protestant	Catholic	Jewish	Agnostic/Atheist
<i>Religion salient</i>	0.21 (0.36)	0.43 (0.50)	0.33 (0.57)	0.08 (0.51)
<i>1 week vs. 2 weeks</i>	0.01 (0.11)	-0.17 (0.17)	-0.37 (0.27)	-0.30* (0.17)
Constant	-2.96*** (0.26)	-3.53*** (0.36)	-3.62*** (0.44)	-4.40*** (0.42)
$\hat{\sigma}$	1.99 (0.16)	2.18 (0.21)	1.85 (0.20)	2.36 (0.18)
<i>N</i>	242	154	80	196

Note: This table presents interval regressions where the latent dependent variable is the log continuously compounded interest rate required to defer payment receipt. We pool each subject's two intertemporal choices together. *Religion salient* is a dummy for being in the religion-salient condition. *1 week vs. 2 weeks* is a dummy for the intertemporal choice being between payments deferred for one week versus two weeks. The estimated conditional standard deviation of the latent dependent variable is denoted by $\hat{\sigma}$. Huber-White standard errors, clustered by subject, are reported in parentheses below the point estimates. The final row of each panel reports the number of discount rate intervals in the regressions. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table 4. Dictator Game Results

	Protestant	Catholic	Jewish	Agnostic/Atheist
<i>Religion salient</i>	-0.03 (0.04)	-0.04 (0.04)	0.01 (0.07)	-0.05* (0.03)
Constant	0.22*** (0.03)	0.17*** (0.04)	0.14*** (0.04)	0.17*** (0.02)
<i>N</i>	186	139	62	176

Note: This table shows regression results where the dependent variable is the amount given away. *Religion salient* is a dummy for being in the religion-salient condition. Huber-White standard errors are in parentheses below the point estimates. * Significant at the 10 percent level. *** Significant at the 1 percent level.

Table 5. Number of Anagrams Attempted

	Protestant	Catholic	Jewish	Agnostic/Atheist
<i>Religion salient</i>	-1.48 (2.53)	1.76 (3.98)	4.92 (5.99)	2.12 (2.63)
Constant	29.80*** (1.89)	31.09*** (2.89)	33.65*** (4.48)	32.02*** (1.78)
<i>N</i>	78	60	33	93

Note: This table shows regression results where the dependent variable is the number of anagrams attempted in the anagram-solving task. *Religion salient* is a dummy for being in the religion-salient condition. Huber-White standard errors are in parentheses below the point estimates. *** Significant at the 1% level.

Table 6. Bilateral Gift-Exchange Game Results

	Protestant	Catholic	Jewish	Agnostic/Atheist
Panel A. Slope from regression of work cost on wages between \$0 and \$3.50				
<i>Religion salient</i>	0.01 (0.02)	0.01 (0.02)	0.06** (0.03)	-0.02 (0.02)
Constant	0.08*** (0.01)	0.08*** (0.01)	0.05*** (0.02)	0.09*** (0.01)
<i>N</i>	104	91	40	113
Panel B. Wage offered as manager				
<i>Religion salient</i>	0.02 (0.19)	0.01 (0.19)	0.42 (0.29)	-0.28 (0.18)
Constant	1.41*** (0.13)	1.28*** (0.14)	0.98*** (0.21)	1.57*** (0.12)
<i>N</i>	104	91	40	113

Note: This table shows regression results where the dependent variable is the subject-specific slope coefficient from a regression of work cost expended as an Employee on managerial wage offered (Panel A) or the wage offered as a Manager (Panel B). *Religion salient* is a dummy for being in the religion-salient condition. Huber-White standard errors are in parentheses below the point estimates. ** Significant at the 5% level. *** Significant at the 1% level.

Table 7. Religion-Salience Treatment Interactions with Belief in Divine Punishment and Religious Service Attendance

	Protestant		Catholic				Jewish	
	Public good contribution		Public good contribution		Risk premium		Gift-exchange reciprocity	
<i>Religion salient</i>	0.14*	0.14*	-0.18**	-0.20*	-0.13	-0.15	0.06*	0.10***
	(0.07)	(0.08)	(0.08)	(0.10)	(0.09)	(0.09)	(0.03)	(0.03)
<i>Religion Salient</i> × <i>Divine punishment</i>	0.12*		-0.06		-0.08		0.01	
	(0.07)		(0.08)		(0.09)		(0.03)	
<i>Divine punishment</i>	-0.11**		0.04		0.06		-0.01	
	(0.05)		(0.05)		(0.06)		(0.02)	
<i>Religion Salient</i> × <i>(Attendance > median)</i>		0.02		0.03		0.08		-0.16**
		(0.12)		(0.14)		(0.13)		(0.07)
<i>Attendance > median</i>		-0.10		0.00		0.05		0.04
		(0.09)		(0.10)		(0.10)		(0.06)
<i>Larger stakes</i>					0.38***	0.30***		
					(0.08)	(0.05)		
Constant	0.52***	0.58***	0.71***	0.70***	0.20***	0.19***	0.05***	0.04**
	(0.05)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.02)	(0.02)
$\hat{\sigma}$					0.35	0.35		
					(0.03)	(0.03)		
<i>N</i>	131	180	103	138	84	154	40	40

Note: 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, 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 public good and 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 zero 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 and reciprocity regressions, and the number of reservation risk premium intervals observed for the risk aversion regressions.

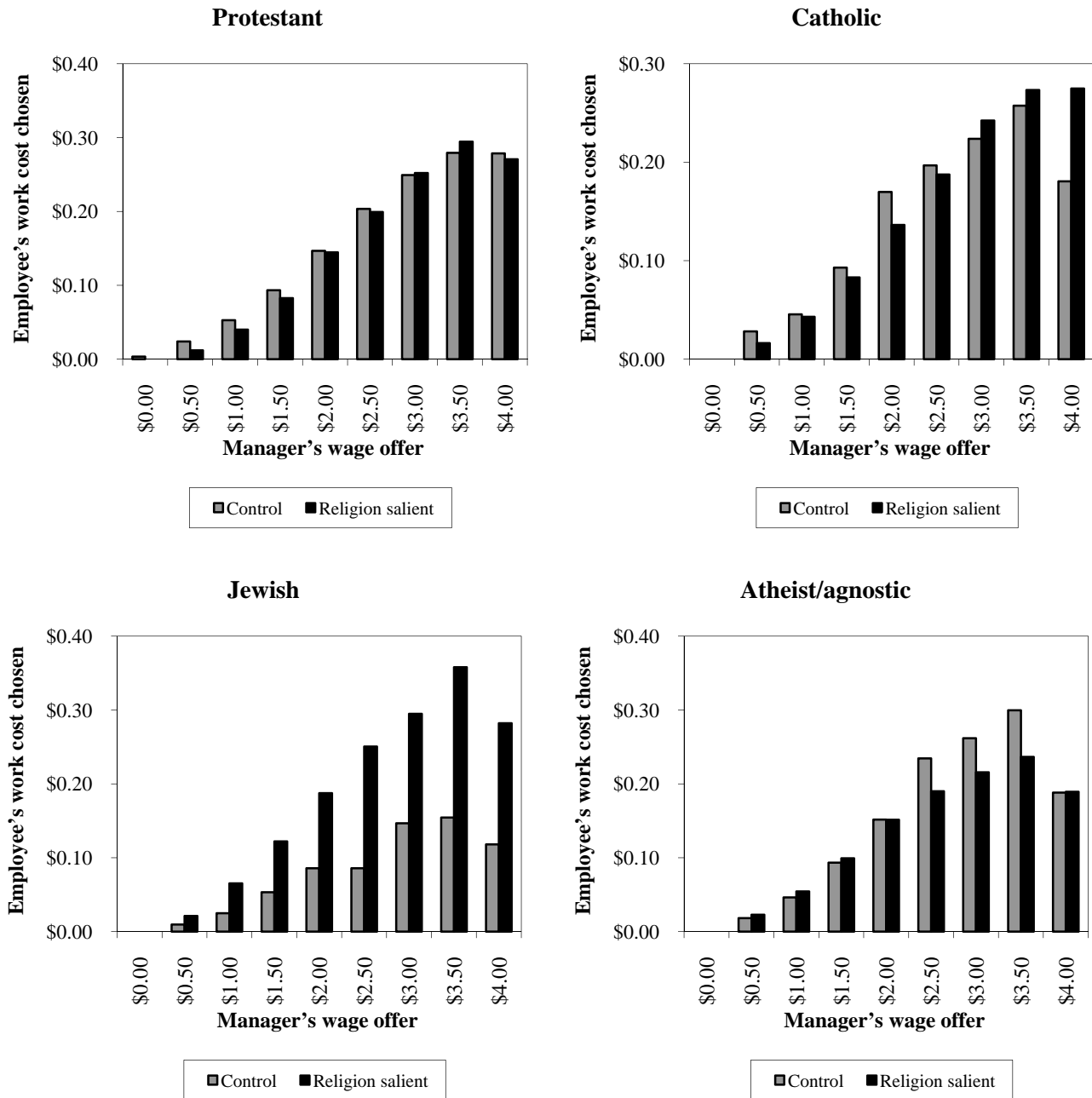


Figure 1. Average Employee work costs chosen in response to managerial wage offers in gift exchange game.