

Religious Identity and Economic Behavior*

Daniel J. Benjamin
Cornell University, Institute for Social Research, and NBER

James J. Choi
Yale University and NBER

Geoffrey Fisher
Cornell University

Current draft: February 14, 2010

Abstract

We identify the marginal effect of religious identity on economic choices by measuring how laboratory subjects' choices change when their religious identity is made salient to them. Using this method, we test six hypotheses derived from prior literature about the causal effect of religion. 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 work effort in an anagram-solving task at a fixed piece-rate wage, discount rates, or generosity in a dictator game.

JEL Classification: C91, C92, Z12

Keywords: religion, identity, norm, gift exchange, public goods, dictator game

* We thank Azim Shariff and Ara Norenzayan for sharing with us the religious identity priming instrument. We are grateful to Stefano DellaVigna, Kirabo Jackson, and participants at the NBER's Economics of Religion Conference, U.C Santa Barbara's Behavioral / Experimental Economics Conference, and the University of Maryland's Labor/Public Economics Seminar for helpful comments and suggestions. We thank Mario Basora, Maria Bodiu, Kristin Brinley Conner, Evan Buntrock, Jim Casteleiro, John Farragut, Isabel Fay, Joshua Funt, Arjun Gokhale, Jesse Gould, Rebecca Hausner, Ben Hebert, Liying Huang, Ahmed Jaber, Bige Kahraman, Anqi Kang, Philip Kauders, June Kim, Xiaoying Lin, Michael Luo, Max Mihm, Gregory Muenzen, Christopher Nieves, Collin Raymond, Alex Rees-Jones, John Schemitsch, Nathaniel Schorr, Dennis Shiraev, Nichole Szembrot, Russell Toth, Elizabeth Truax, and Ryan Yamada for their research assistance. We thank the National Institute on Aging (grant P30-AG012810) for financial support.

E-mail: db468@cornell.edu, james.choi@yale.edu, geoffrey.w.fisher@gmail.com.

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 attribute to the salutary effect of such beliefs on work ethic, honesty, trust, and thrift. Putnam (1993) and La Porta et al. (1997) argue that hierarchical religions such as Catholicism inhibit trust, which has negative effects on GDP growth, government efficiency, the production of public goods, and the maximum feasible size of corporations. Relatedly, Ruffle and Sosis (2007) find that participation in collective religious rituals is associated with greater trust and cooperativeness with others. 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 beliefs. Religious beliefs 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 by varying the salience of religious identity in laboratory subjects using a method from social psychology. We then observe their subsequent choices in strategic games and incentive-compatible preference elicitation. According to “self-categorization theory” (James, 1890; Turner, 1985), making a social category more salient—that is, “priming” the category—causes a person's behavior to shift towards the salient category's norms. The marginal behavioral effect of religious norms can therefore be identified by the change in behavior induced by increasing religious identity salience. This methodology has previously been used to identify economic

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.

effects of racial, ethnic, and gender identity norms by Benjamin, Choi, and Strickland (forthcoming).

We examine six specific 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 Protestantism and increased by Catholicism (Kumar, Page, and Spalt, 2009), or may be reduced by religion in general (Hilary and Hui, 2008).
- H4.* Thrift and capital accumulation is promoted by religion (Weber, 1930; Guiso, Sapienza, and Zingales, 2003; Barro and McCleary, 2003 and 2006).
- H5.* Generosity is increased by religion (Friedrichs, 1960; Langford and Langford, 1974; Batson, Schoenrade, and Ventis, 1993).
- H6.* Work ethic is increased by Protestantism (Weber, 1930).

To operationalize these hypotheses in a laboratory experiment, we map each of the six domains into behaviors that can be measured using standard methods: (1) contributions in a public goods game, (2) expectations about *others'* contributions in a public goods game, (3) elicited risk aversion, (4) elicited discount rates, and (5) amount given away in a dictator game. Since work ethic could be interpreted as marginal utility-cost of effort or as reciprocity toward an employer, we measure both of these concepts: (6) the number of anagrams attempted at a fixed piece-rate wage, and effort expended as the worker in a labor market bilateral gift-exchange game.

To test the hypotheses, we compare the behavior of religiously-primed and unprimed subjects. Consistent with *H1*, we find that Protestantism increases contributions to the public good, while Catholicism decreases contributions. In partial support of *H2* and *H3*, we find that Catholicism decreases expectations of others' contributions to public goods and decreases risk aversion, but we find no corresponding effects for Protestants. We do not find support for *H4*, *H5*, or *H6*. However, we do find a “work ethic” effect for Judaism; among Jews, priming

religion increases the reciprocity of workers in response to higher wages in the gift-exchange game.

We are aware of three prior psychology papers that manipulate religious 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 spend more time trying to solve unsolvable anagrams for which no payment is offered. 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.

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 particular social category reveals the marginal effect of increasing the strength of affiliation with that category. Let x be some action choice, such as how much to contribute to a public good. An individual belongs to a social category C , such as Protestantism, with strength $s \geq 0$. Let x_0 denote the individual's

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

preferred action in the absence of identity considerations, and let x_C denote the action 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 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 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 according to 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 order of frequency: 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 -values for differences across conditions in the number of subjects who mentioned them 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 unscrambling task. 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 the 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, the subjects who solved anagrams did not participate in any of the games or preference elicitations, and the other subjects each engaged in only a subset of the games and elicitations. When games and elicitations were administered, their order 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 would be 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.

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

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

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 \$3.60. All six choices affected each subject's payment. We measured larger-stakes risk preferences with analogous choices, except that the monetary amounts were multiplied by 100, and there was only a small chance that the subject's choice would be implemented.⁴

Risk aversion measures derived from real-stakes 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.⁵

C. Discount rate elicitation

We measured time preferences by asking participants to make 12 binary choices between \$10 now and some larger amount one week from now, and another 12 binary choices between \$10 one week from now and some larger amount two weeks from now. The larger delayed amounts ranged from \$10.10 to \$15. One of the intertemporal choices was randomly chosen for payment. All payments were made by a check given to the participant immediately following the experiment. Delayed payments were implemented via post-dated check. The section's instructions made it clear that the questions were not intended to evaluate performance: "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 risk preference elicitations.

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,

⁴ Any money participants earned from their risk choices was paid with a check that could be cashed immediately.

⁵ See Barsky et al. (1997), Guiso and Paiella (2008), Dohmen et al. (2005), and Sahn (2007).

borrowing on installment accounts and credit cards, rapid exhaustion of food stamps, delayed application to an MBA program, and defaulting on loans.⁶

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.

E. Labor market tasks

Conceptually, work ethic could be interpreted in terms of economic theory as marginal utility-cost of effort—willingness to exert effort at a contracted-upon wage rate—or as reciprocity toward an employer—a sense of obligation to supply effort in response to a wage even when the labor contract is incomplete. We measure both.

For marginal utility-cost of effort, we ask subjects to solve as many four-letter anagrams as they can in a five-minute period. We pay participants five cents per correctly solved anagram. By restricting the task to five minutes, we reduced the chances that primed subjects' religious identity salience would fade during the task. Because we pay a piece-rate wage, shifts in the amount of effort exerted primarily reflect shifts in the marginal effort cost function.

We measure 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 pays a wage to the employee between \$0 and \$4 that is a multiple of 50 cents. The Manager cannot change the wage later. In the second stage,

⁶ 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).

the Employee sees the wage and chooses a work quantity to supply that is an integer between 1 and 10, inclusive. (Because the employee's work quantity is the choice of a number rather than effort in a real work task, we can measure a subject's reciprocity unconfounded by his marginal utility-cost of effort.) Employees are told that the Manager will be shown their work quantity choice. The Employee's earnings are the wage received minus the cost of the work provided, which is 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 rises from 1 to 10. The Manager's earnings are $(\$4 - \text{wage paid}) \times \text{work quantity provided by the Employee} \div 10$.

To facilitate calculation, we provided subjects a lookup table that showed the Manager and Employee's earnings at each wage and work quantity combination.⁷ Managers simply chose one wage, while Employees indicated a contingent work supply choice for each possible wage offer. After observing the Managers' wage, 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.

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.A.

⁷ Charness, Frechette, and Kagel (2004) find that providing a lookup table decreases wages, work effort, and the slope of workers' effort with respect to wage.

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.⁸ 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.⁹ 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.

B. Public goods game

Panel A of Table 1 shows results from regressions where the dependent variable is the amount contributed to the public good. Unprimed Catholic subjects contribute the most on average, unprimed agnostics and atheists contribute the least, and unprimed 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 simply by comparing subject choices across religions. To learn about the impact of religion, we instead turn to comparisons between the treatment and control groups.

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

⁸ 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 Catholics, 101 Protestants, 43 Jews, and 136 atheists/agnostics, compared with 134, 98, 52, and 133 in the control group, respectively.

⁹ At the very 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 dictator, public goods, and gift exchange games, 88% reported believing that their choices would affect *other* participants’ payments exactly as we had specified.

could be due to the existence of an atheist or agnostic norm regarding public goods 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 expectations 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. (Analogously, primed atheist and agnostics expect others to contribute 9 cents more, although this effect is significant only at the 10 percent level.) Panel C shows that these changes in expectations affect contributions. The coefficients in this panel are from a regression of contributions on a religion-salient dummy, the expected average contribution of other group members, and the interaction of these two variables. We find that subjects' own contributions increase almost one-for-one with their expectation of their other group members' average contribution, and this relationship does not differ significantly between primed and unprimed subjects. Nevertheless, the effect of religious identity on public goods contribution does not operate exclusively through expectations of others' behavior. Protestants' expectations of others' contributions are unaffected by priming (and the coefficient for Protestants is statistically distinguishable from the coefficient for Catholics; $p < .05$). It seems, therefore, that while the negative effect of religion on public goods provision for Catholics operates through reduced trust in others, there is a Protestant norm to unconditionally contribute to public goods.

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 $(2.40 \times 0.5 - 1)/1 = 0.20$.¹⁰ Because we observe binary choices at

¹⁰ In this formula, we treat the risk choice as investing a \$1 endowment into either the risk-free or risky asset. Our choice to use the “minimum” risk premium as the dependent variable 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 96% of participants behaved consistently, our results are virtually identical if we use the “maximum” risk premium

only a finite number of interest rates, 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 large-stakes gambles—so each subject appears in the regression twice, and we cluster standard errors by subject (Froot, 1989; Rogers, 1993).

Table 2 shows that priming religious identity causes the average risk premium required to entice agnostics/atheists to forego a sure payout to fall by 12 percentage points. For the smaller 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 smaller 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. Our finding is consistent with Kumar, Page, and Spalt's (2009) conclusion that Catholicism encourages investment in lottery-like stocks, but not consistent with their conclusion that Protestantism discourages such investment. The two negative Christian priming point estimates are inconsistent with Hilary and Hui's (2009) argument that both Protestantism and Catholicism discourage risk-taking.¹¹ We find no significant identity effects for Jews.

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

instead. Similarly, our choice to use the “minimum” interest rate in our time preference regressions below does not matter because 98% of participants had a unique interest rate switchpoint.

¹¹ Although it is not the focus of their paper, Hilary and Hui (2009) find suggestive evidence that priming religion increases risk aversion in sample of 120 undergraduates. It is difficult to directly compare their results with ours because they do report only full-sample results and not the religious composition of the sample, they report only p-values (.20 and .08 for their two regressions) rather than effect sizes, they use a different priming instrument, and they pose only two risky decision problems, both hypothetical.

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 (Froot, 1989; Rogers, 1993).

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 3 presents the regression evidence on how priming religion 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 the dictator game usually gave away a positive amount of money to the subject with whom they were paired, although the proportion given away is far less than half on average. This is a typical result for dictator game experiments (e.g., Forsythe et al., 1994). Table 4 contains regression results where the dependent variable is the amount of money given away. 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. The absence of positive religious identity effects on pure altruism in the dictator game is consistent with the lack of religious identity effects on purely altruistic labor provision in the gift-exchange game at a \$0 wage.

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 Norenzayan 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 ($\beta = .238, s.e. = .0645, N = 50$), their Study 2 ($\beta = .200, s.e. = .0810, N = 50$), and our experiment ($\beta = -.034, s.e. = .0214, N = 553$), the combined priming effect estimate on dictator game giving for a \$1 endowment is \$0.004 with a standard error of \$0.019.¹²

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 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 the number of anagrams attempted by 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 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. 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.¹³ The

¹² 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 subject population or procedure 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 while religion is related to self-reported helping behavior, the relation with 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.

¹³ At a \$4 wage, work expenditures are usually smaller than at a \$3.50 wage. This is because when the Manager offers \$4, his or her 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). 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 difference between primed and unprimed Catholic work expenditures at a \$4 wage is not statistically significant, however.

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 3. 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 \$3.50.¹⁴ Jewish identity significantly increases this slope from 0.05 to 0.11, but there are no effects for any other group.

Panel B of Table 3 examines the effect of priming religious identity on wages offered by Managers. To our knowledge, there is no clearly articulated hypothesis in the prior literature about religion's effect on managerial wage offers, so we report these results mainly for completeness. One might expect that religious effects on trust and altruism would affect Manager wage offers, but we find no significant effects.

G. Multiple hypothesis testing and Type I error

Although all of the main hypotheses we have tested (with the exception of managerial wage offers in the gift-exchange game) 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 goods contributions, anagrams attempted, gift-exchange reciprocity, manager wage offers, dictator game generosity, discount rates, and risk aversion are jointly equal to zero.¹⁵ We reject this hypothesis at $p = 0.015$.

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

¹⁴ We exclude work expenditures at \$4 because of their money-burning aspect mentioned in the previous footnote.

¹⁵ To run this test, we pool all of our observations into a single interval regression and control for task type dummies, task type dummies \times religion dummies, and task type dummies \times religion dummies \times additional task-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 task type and cluster standard errors by subject.

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 indicate 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.¹⁶ To keep the number of interaction regressions manageable, we limit attention to the dependent variables and religious groups where we found statistically significant main effects of priming. We also omit regressions involving agnostics and atheists since 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

¹⁶ These regressions would be problematic if the priming manipulation 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 in divine punishment ($p < .05$). However, we do not run regressions among Jews where we interact the treatment dummy with belief in divine punishment.

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*.¹⁷

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. 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.

¹⁷ Because we varied the order of experimental games and preference elicitations across sessions, another question we can ask is how the strength of priming effect on a task varied with the length of time elapsed between the priming manipulation and the task. Based on visual inspection of the point estimates, 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 on Catholic trust appear to strengthen over time, but we attach little weight to these possible trends because very few of the interactions between the priming treatment and task order are statistically significant.

References

- Akerlof, George A., and Rachel E. Kranton. 2000. "Economics and Identity." *Quarterly Journal of Economics* 115(3): 715-733.
- Ashraf, Nava, Dean S. Karlan, and Wesley Yin. 2006. "Tying Odysseus to the Mast: Evidence from a Commitment Savings Project in the Philippines." *Quarterly Journal of Economics* 121(2): 635-672.
- Barro, Robert J., and Rachel M. McCleary. 2003. "Religion and Economic Growth." *American Sociological Review* 68: 760-781.
- Barro, Robert J., and Rachel M. McCleary. 2006. "Religion and Economy." *Journal of Economic Perspectives* 20: 49-72.
- Barsky, Robert B., F. Thomas Juster, Miles S. Kimball, and Matthew D. Shapiro. 1997. "Preference Parameters and Behavioral Heterogeneity: An Experimental Approach in the Health and Retirement Study." *Quarterly Journal of Economics* 112(2): 537-579.
- Batson, C. Daniel, Patricia A. Schoenrade, and W. Larry Ventis. 1993. *Religion and the Individual: A Social-Psychological Perspective*. New York: Oxford University Press.
- Becker, Sascha O., and Ludger Woessmann. 2009. "Was Weber Wrong? A Human Capital Theory of Protestant Economic History." *Quarterly Journal of Economics* 124: 531-596.
- Benjamin, Daniel J., James J. Choi, and A. Joshua Strickland. Forthcoming. "Social Identity and Preferences." *American Economic Review*.
- Bickel, Warren K., Amy L. Odum, and Gregory J. Madden. 1999. "Impulsivity and Cigarette Smoking: Delay Discounting in Current, Never, and Ex-Smokers." *Psychopharmacology* 146(4): 447-454.
- Charness, Gary, Guillaume R. Frechette, and John H. Kagel, 2004. "How Robust is Laboratory Gift Exchange?" *Experimental Economics* 7: 189-205.
- Dehejia, Rajeev, Thomas DeLeire, Erzo F. P. Luttmer, and Josh Mitchell. 2009. "The Role of Religious and Social Organizations in the Lives of Disadvantaged Youth." In *An Economic Perspective on the Problems of Disadvantaged Youth*. Jonathan Gruber, ed. Chicago: University of Chicago Press.
- Dohmen, Thomas, Armin Falk, David Huffman, Uwe Sunde, Jürgen Schupp, and Gert G. Wagner. 2005. "Individual Risk Attitudes: New Evidence from a Large, Representative, Experimentally-Validated Survey." IZA Discussion Paper No. 1730.
- Ellison, Christopher G. 1991. "Religious Involvement and Subjective Well-Being." *Journal of Health and Social Behavior* 32: 80-99.

- Fehr, Ernst, Erich Kirchler, Andreas Weichbold, and Simon Gächter. 1998. "When Social Norms Overpower Competition: Gift Exchange in Experimental Labor Markets." *Journal of Labor Economics* 16(2): 324-351.
- Fischbacher, Urs. 2007. "z-tree: Zurich Toolbox for Ready-Made Economic Experiments." *Experimental Economics* 10: 171-178.
- Forehand, Mark R., Rohit Deshpandé, and Americus Reed II. 2002. "Identity Salience and the Influence of Differential Activation of the Social Self-Schema on Advertising Response." *Journal of Applied Psychology* 87(6): 1086-1099.
- Forsythe, Robert, Joel L. Horowitz, N. E. Savin, and Martin Sefton, 1994. "Fairness in Simple Bargaining Experiments." *Games and Economic Behavior* 6: 347-369.
- Frederick, Shane, George Loewenstein, and Ted O'Donoghue. 2002. "Time Discounting and Time Preference: A Critical Review." *Journal of Economic Literature* 40(2): 351-401.
- Freeman, Richard B. 1986. "Who Escapes? The Relation of Churchgoing and Other Background Factors to the Socioeconomic Performance of Black Male Youths from Inner-City Tracts." In *The Black Youth Employment Crisis*. Richard B. Freeman, ed. Chicago and London: University of Chicago Press, pp. 353-376.
- Friedrichs, Robert W. 1960. "Alter versus Ego: An Exploratory Assessment of Altruism." *American Sociological Review* 25(4): 496-508.
- Froot, Kenneth A. 1989. "Consistent Covariance Matrix Estimation with Cross-Sectional Dependence and Heteroskedasticity in Financial Data." *Journal of Financial and Quantitative Analysis* 24(3): 333-355.
- Fuchs, Victor. 1982. "Time Preference and Health: An Exploratory Study." In Victor R. Fuchs, editor, *Economic Aspects of Health*: 93-120. Chicago: University of Chicago Press.
- Gruber, Jonathan H. 2005. "Religious Market Structure, Religious Participation, and Outcomes: Is Religion Good For You?" *Advances in Economics Analysis and Policy* 5(1), Article 5.
- Guiso, Luigi, and Monica Paiella. 2008. "Risk Aversion, Wealth, and Background Risk." *Journal of the European Economic Association* 6(6): 1109-1150.
- Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2003. "People's Opium? Religion and Economic Attitudes." *Journal of Monetary Economics* 50: 225-282.
- Hilary, Gilles, and Kai Wai Hui. 2009. "Does Religion Matter in Corporate Decision Making in America?" *Journal of Financial Economics* 93: 455-473.
- Iannaccone, Laurence R. 1998. "Introduction to the Economics of Religion." *Journal of Economic Literature* 36: 1465-1496.
- James, William. 1890. *The Principles of Psychology*. New York: Henry Holt.

- Langford, Barbara Johnson, and Charles C. Langford. 1974. "Church Attendance and Self-Perceived Altruism." *Journal for the Scientific Study of Religion* 13(2): 221-222.
- Kahneman, Daniel, Jack Knetsch, and Richard H. Thaler. 1986. "Fairness and the Assumptions of Economics." *Journal of Business* 59: S285-300.
- Kirby, Kris N., and Nancy M. Petry. 2004. "Heroin and Cocaine Abusers Have Higher Discount Rates for Delayed Rewards than Alcoholics or Non-Drug-Using Controls." *Addiction* 99(4): 461-471.
- Kirby, Kris N., Nancy M. Petry, and Warren K. Bickel. 1999. "Heroin Addicts Have Higher Discount Rates for Delayed Rewards than Non-Drug-Using Controls." *Journal of Experimental Psychology* 128(1): 78-87.
- Kumar, Alok, Jeremy Page, and Oliver Spalt. 2009. "Religious Beliefs, Gambling Attitudes, and Financial Market Outcomes." University of Texas working paper.
- La Porta, Rafael, Florencio Lopez-de-Silanes, Andrei Shleifer, and Robert W. Vishny. 1997. "Trust in Large Organizations." *American Economic Review* 87: 333-338.
- LeBoeuf, Robyn A., Eldar Shafir, and Julia Belyavsky Bayuk. 2010. "The Conflicting Choices of Alternating Selves." *Organizational Behavior and Human Decision Processes* 111(1): 48-61.
- Levin, Jeffrey S. 1994. "Religion and Health: Is There an Association, Is It Valid, and Is It Causal?" *Social Science Medical Journal* 24: 589-600.
- Marwell, Gerald, and Ruth E. Ames. 1979. "Experiments on the Provision of Public Goods. I. Resources, Interest, Group Size, and the Free-Rider Problem." *American Journal of Sociology* 84(6): 1335-1360.
- Meier, Stephan, and Charles Sprenger. 2010. "Present-biased preferences and credit card borrowing." *American Economic Journal: Applied Economics* 2(1): 193-210.
- Petry, Nancy M., and Thomas Casarella. 1999. "Excessive Discounting of Delayed Rewards in Substance Abusers with Gambling Problems." *Drug and Alcohol Dependence* 56(1-2): 25-32.
- Putnam, Robert. 1993. *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton, NJ: Princeton University Press.
- Randolph-Seng, Brandon, and Michael E. Nielsen. 2007. "Honesty: One Effect of Primed Religious Representations." *International Journal for the Psychology of Religion* 17: 303-315.
- Reicher, Stephen, and Mark Levine. 1994. "Deindividuation, Power Relations Between Groups and the Expression of Social Identity: The Effects of Visibility to the Out-Group." *British Journal of Social Psychology* 33(2): 145-163.

- Reuben, Ernesto, Paola Sapienza, and Luigi Zingales. 2009. "Procrastination and Impatience." Northwestern University working paper.
- Rogers, William. 1993. "Regression Standard Errors in Clustered Samples." *Stata Technical Bulletin* 13: 19-23.
- Ruffle, Bradley J., and Richard Sosis. 2007. "Does it Pay to Pray? Costly Ritual and Cooperation." *The B.E. Journal of Economic Analysis & Policy* 7(1) (Contributions), Article 18.
- Sahm, Claudia R. 2007. "Stability of Risk Preference." Federal Reserve Board Finance and Economics Discussion Series 2007-66.
- Shariff, Azim F., and Ara Norenzayan. 2007. "God Is Watching You: Priming God Concepts Increases Prosocial Behavior in an Anonymous Economic Game." *Psychological Science* 18: 803-809.
- Shapiro, Jesse M. 2005. "Is There a Daily Discount Rate? Evidence from the Food Stamp Nutrition Cycle." *Journal of Public Economics* 89(2-3): 303-325.
- Stewart, Mark B. 1983. "On Least Squares Estimation when the Dependent Variable is Grouped." *Review of Economic Studies* 50(4): 737-753.
- Stulz, Rene M., and Rohan Williamson. 2003. "Culture, Openness, and Finance." *Journal of Financial Economics* 70: 313-349.
- Toburen, Tina, and Brian P. Meier. Forthcoming. "Priming God-Related Concepts Increases Anxiety and Task Persistence." *Journal of Social and Clinical Psychology*.
- Turner, John C. 1985. "Social Categorization and the Self-Concept: A Social Cognitive Theory of Group Behavior." In *Advances in Group Processes*, Vol. 2, ed. E. J. Lawler, 77-121. Greenwich, CT: JAI Press.
- Weber, Max, 1930. *The Protestant Ethic and the Spirit of Capitalism*. London: Allen and Unwin.
- Wheeler, S. Christian, and Richard E. Petty. 2001. "The Effects of Stereotype Activation on Behavior: A Review of Possible Mechanisms." *Psychological Bulletin* 127(6), 797-826.
- Williamson, Oliver. 1985. *The Economic Institutions of Capitalism*. New York: Free Press.

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>	0.18** (0.09)	-0.16 (0.10)	-0.14 (0.14)	-0.05 (0.09)
<i>E(Others' contribution)</i>	0.95*** (0.07)	0.87*** (0.10)	0.96*** (0.17)	0.88*** (0.11)
<i>Religion salient</i> × <i>E(Others' contribution)</i>	-0.09 (0.11)	0.12 (0.13)	0.11 (0.22)	0.15 (0.15)
Constant	-0.03 (0.05)	0.06 (0.09)	-0.03 (0.08)	0.03 (0.06)
<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 expectation of others' 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 risk premium required 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 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 if the intertemporal choice was 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 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.

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 work ethic 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 10% level. ** 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 aversion		Gift-exchange reciprocity	
<i>Religion salient</i>	0.14* (0.07)	0.14* (0.08)	-0.18** (0.08)	-0.20* (0.10)	-0.13 (0.09)	-0.15 (0.09)	0.06* (0.03)	0.10*** (0.03)
<i>Religion Salient</i> × <i>Divine punishment</i>	0.12* (0.07)		-0.06 (0.08)		-0.08 (0.09)		0.01 (0.03)	
<i>Divine punishment</i>	-0.11** (0.05)		0.04 (0.05)		0.06 (0.06)		-0.01 (0.02)	
<i>Religion Salient</i> × <i>(Attendance > median)</i>		0.02 (0.12)		0.03 (0.14)		0.08 (0.13)		-0.16** (0.07)
<i>Attendance > median</i>		-0.10 (0.09)		0.00 (0.10)		0.05 (0.10)		0.04 (0.06)
<i>Larger stakes</i>					0.38*** (0.08)	0.30*** (0.05)		
Constant	0.52*** (0.05)	0.58*** (0.06)	0.71*** (0.06)	0.70*** (0.07)	0.20*** (0.06)	0.19*** (0.06)	0.05*** (0.02)	0.04** (0.02)
$\hat{\sigma}$					0.35 (0.03)	0.35 (0.03)		
<i>N</i>	131	180	103	138	84	154	40	40

Note: The column headings indicate the dependent variable and religious group 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 risk premium required to accept a gamble. The public goods and reciprocity coefficients are from an OLS regression; the risk aversion 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 if the sure payout in the risky choice was \$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.

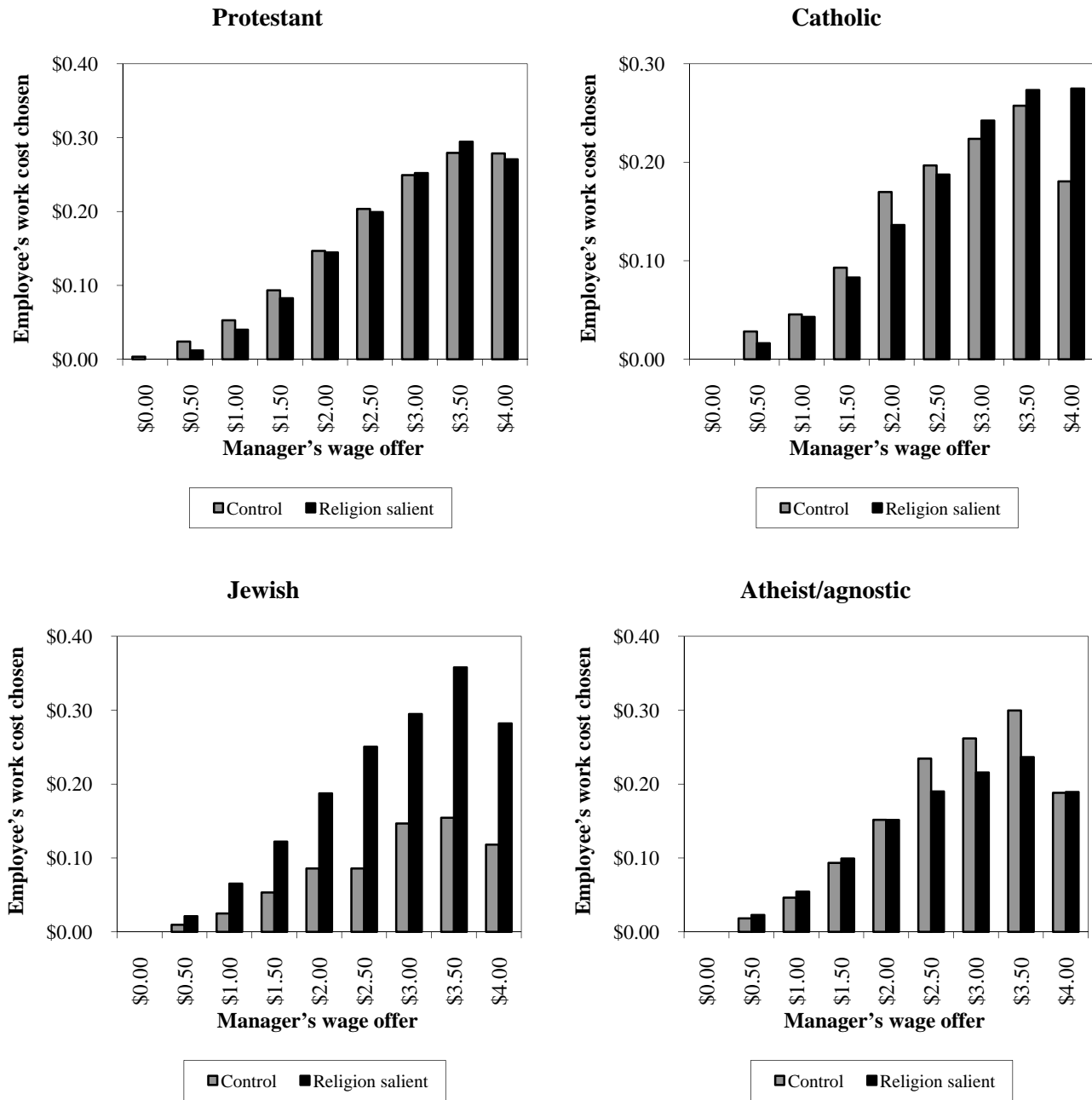


Figure 1. Average Employee work costs chosen in response to managerial wage offers in gift exchange game.