Religious Identity and Economic Behavior*

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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. We find that Protestantism increases contributions to public goods, and there is suggestive evidence that it increases reciprocity in a labor market gift-exchange game. Catholicism decreases contributions to public goods, increases reciprocity, and decreases risk aversion. Judaism increases reciprocity. We find no evidence of religious identity effects on 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 increase 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, and the maximum feasible size of corporations. 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 introduce 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 elicitations. 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. Therefore, the marginal behavioral effect of religious norms can be identified by the change in behavior induced by increasing religious identity salience. This methodology has previously been used to identify economic effects of racial, ethnic, and gender identity norms by Benjamin, Choi, and Strickland (forthcoming).

[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., forthcoming). Becker and Woesmann (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.

We examine the effects of religious identity on contributions to a laboratory public good, wages offered by the manager and effort expended by the worker in a gift-exchange game, amounts given away in a dictator game, discount rates, and risk aversion. We find that Protestantism increases contributions to the public good, and there is suggestive evidence that it increases reciprocity in the gift-exchange game—that is, the rate at which worker effort increases in response to higher wage offers by the manager. Catholicism decreases contributions to public goods, increases gift-exchange reciprocity, and decreases risk aversion. Judaism increases gift-exchange reciprocity. We find no evidence of religious identity effects on discount rates or generosity in a dictator 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. 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]

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. 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

[2] There have been many other studies examining the effect of identity salience itself, include Reicher and Levine (1994), Forehand, Deshpandé, and Reed II (2002), and LeBoeuf, Shafir, and Belyavsky (forthcoming).

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 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 bigger 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 bigger 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 Norenzyan (2007) to study the effect of priming religious identity, 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.

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-nine percent of subjects in the religious-salient condition listed a religious identity in response, compared to only 23 percent of subjects in the control condition. This difference is significant at the 1 percent level (t-statistic = 2.67).

III. Main Experiment Procedure

Participants in the main experiment were 442 Cornell University students. All sessions were administered by computer using the program z-Tree (Fischbacher, 2007).

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-unscrambling task, they made choices in strategic games and incentive-compatible preference elicitations that are standard in experimental economics. We describe these games and elicitations below. Subjects were told at the beginning of the session that any

interactions they had with other subjects would be anonymous one-shot interactions. In order to avoid excessively long experimental sessions, each subject engaged in only a subset of the games and elicitations. We varied across sessions the order in which we administered the games and elicitations.

Public goods game

Each subject was assigned to a group of four and endowed with $1. Each subject could contribute any fraction of his or her dollar to a group account. 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.

We also asked subjects to give their best guess of how much the other three members of their group would contribute on average. Subjects' payments did not depend upon the value of this guess.

Labor market gift exchange game

We model our labor market gift-exchange game on one of the implementations in 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. 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, the Employee sees the wage and chooses a work quantity to supply that is an integer between 1 and 10, inclusive. 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 – wage paid) × work quantity provided by the Employee ÷ 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.[3] 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.

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. Each subject was also the receiver of another subject's dictator gift.

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,

[3] 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.

borrowing on installment accounts and credit cards, rapid exhaustion of food stamps, delayed application to an MBA program, and defaulting on loans.[4]

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.[5]

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.[6]

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.

IV. Main Experiment Results

A. Subject characteristics

Table 1 presents summary statistics on our subjects, separately by experimental condition. Our sample consists of 151 Protestant/other Christians (whom we refer to collectively as "Protestants"), 115 Catholics, 53 Jews, and 123 atheists/agnostics. The non-Jewish sample is majority female, whereas the Jewish sample is majority male; the average combined SAT I math

[4] 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 (2009, forthcoming), and Reuben, Sapienza, and Zingales (2009).
[5] Any money participants earned from their risk choices was paid with a check that could be cashed immediately.
[6] See Barsky et al. (1997), Guiso and Paiella (forthcoming), Dohmen et al. (2005), and Sahm (2007).

and verbal scores is a little under 1400; and most subjects come from affluent backgrounds, as over half report parental income greater than $80,000. As expected due to randomization, there are no significant differences in these characteristics across experimental conditions.

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 442 excludes three subjects who guessed that the study had something to do with religion.[7]

B. Public goods game

Panel A of Table 2 shows the results from a regression 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 prevent us 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 17 cents more to the public good than control Protestants (p-value = 0.010), whereas primed Catholics decrease their contributions by 15 cents (p-value = 0.051). Jewish subjects' contributions are unaffected by the prime. Interestingly, the prime affects atheists and agnostics, increasing their contributions by 15 cents (p-value = 0.058). This increase is driven primarily by agnostics. Since it seems likely that there are no strong norms associated with an agnostic identity, the agnostic/atheist priming effect may be due to the activation of residual religious norms present among these subjects.

Putnam (1993) and La Porta et al. (1997) argue that the channel through which religion affects public goods provision is trust. Our results provide some support for this hypothesis.

[7] We also asked subjects whether they believed that their experimental choices would affect their payments as the instructions specified. Over 95 percent of subjects reported believing these payment promises. Greater than 85 percent of Christians and agnostic/atheist subjects also reported believing that their choices would affect other participants' payments exactly as we had specified, and 80 percent of Jewish subjects believed this.

Panel B of Table 2 indicates that primed Protestants expect their group members to contribute 7 cents more on average, and primed Catholics expect their group members to contribute 7 cents less, although these effects are not statistically significant. Panel C shows that these changes in expectations affect contributions; 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 does not appear to operate exclusively through expectations of others' behavior. The coefficients on the religion-salient dummy show that even after controlling for the effect priming has on expectations, priming Protestant identity increases contributions by an additional 16 cents, and priming Catholic identity decreases contributions by an additional 22 cents. These coefficients are not statistically different from zero, but we can reject their equivalence with each other at the 5 percent level. The combination of priming's effects on expectations and priming's direct effects on contributions adds up to statistically significant effects on overall contributions.

C. Labor market gift-exchange game

Panel A of Table 3 shows that priming religious identity causes Protestants, Catholics, and Jews to increase the wage they offer as Managers in the gift-exchange game by 21 to 49 cents. None of these effects are statistically significant when the three religions are analyzed separately, but pooling these observations, the difference is significant at the 10 percent level (p-value = 0.053). In contrast, atheists and agnostics reduce their wage by 49 cents when religion is made salient, a difference that is significant at the 5 percent level. This reduction is strongest among atheists, which may indicate an atheist norm of expecting selfish behavior in labor markets with incomplete contracts.

In Panel B, we analyze the work choices of Employees. The dependent variable is the work-related cost Employees choose to incur, averaging across all possible wage offers. Priming religion significantly increases mean work-related expenditures by 6 cents among Catholics and 12 cents among Jews. There is a statistically insignificant increase among Protestants of 3 cents, and a statistically insignificant decrease among atheists and agnostics of 4 cents.

Insight into the driver of these average work supply shifts can be found in Figure 1, which plots the average amount Employees chose to expend on work-related costs for each wage

offer. There are several noteworthy features of these graphs. First, for all religious groups and experimental conditions, minimal labor is supplied at a $0 wage offer; in the entire sample, only one Protestant provides anything more than the minimum. Therefore, shifts in the purely altruistic component of labor supply play no role in the average labor supply effect of the prime. Second, 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. Third, this slope of work expenditures with respect to wage is larger when religion is salient among Protestants, Catholics, and Jews, but smaller when religion is salient among atheists and agnostics. Thus, the average labor supply effects of the prime are driven by its effects on Employees' reciprocity. Finally, work expenditures are often smaller at a $4 wage 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 offered a $4 wage purely represents money-burning as a gesture of goodwill and appreciation.[8]

We formally analyze the priming effect on Employees' reciprocity in Panel C of Table 3. The dependent 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. (We exclude work expenditures at $4 because of their money-burning aspect.) Priming Catholic identity significantly increases this slope from 0.05 to 0.09; that is, unprimed Catholics increase their work expenditure by 5 cents for every dollar of wage increase, while primed Catholics increase their work expenditure by 9 cents for every dollar of wage increase. Priming Jewish identity has even stronger effects, increasing the slope from 0.03 to 0.10. Primed Protestants' slope increases by 0.03, and primed agnostics and atheists' slope decrease by 0.04, but these effects are not statistically significant.

D. Dictator game

Table 4 contains regression results where the dependent variable is the amount of money given away in the dictator game. The constant terms indicate that when unprimed, subjects on average give away a positive amount from the dollar with which they are endowed, but the proportion given is far less than half. This is a typical result for dictator game experiments (e.g., Forsythe et al., 1994). Depending on the religious group, the average amount given away is between 14 and 24 cents.

[8] Alternatively, they may represent subject confusion about the payoff formulas.

The insignificant coefficients on the religion-salient dummy indicate that religious identity does not induce subjects to give away more. The point estimates of the priming effect are in fact negative. The absence of 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.

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. Our implementation differs from theirs in that their subjects were told they were dividing a $10 endowment. Their study also involved deception; subjects' choices were not actually implemented. These differences may explain our non-replication.

E. Discount rate

In the time preference task, 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.

Because we observe binary choices at 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. 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 5 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. We cluster standard errors in the interest rate regression by subject (Froot, 1989; Rogers, 1993).

Contrary to the hypothesis that religious identity promotes thrift and capital accumulation (Weber, 1930; Barro and McCleary, 2003 and 2006), in no case do we find that religious identity affects discount rates in a statistically significant way. Except for Catholics, the point estimates of the priming effect are positive. The negative point estimate of the Catholic discount rate effect is small; for example, priming causes Catholics' median reservation interest rate for delaying immediate payment for one week to fall only from 4.05 percent to 3.99 percent.

F. Risk aversion

In the risk preference task, 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 $(2.40 \times 0.5 - 1)/1 = 0.20$.[9] As in the discount rate elicitation, we only observe binary risk choices at a finite number of risk premia. Therefore, we use interval regressions to estimate subjects' reservation risk premia.[10]

Table 6 shows that priming religious identity causes the average risk premium required to entice Catholics to forego a sure \$1 to fall from 26 percent to 8 percent, an effect significant at the 10 percent level. This finding is consistent with Kumar, Page, and Spalt's (2009) conclusion that Catholicism encourages investment in lottery-like stocks. However, we cannot reject the hypothesis that the –18 percent Catholic priming effect is equal to the statistically insignificant –4 percent Protestant priming effect. The two Christian priming point estimates are inconsistent with Hilary and Hui's (2009) argument that both Protestantism and Catholicism discourage risk-taking. We find no significant identity effects for Jews and atheists/agnostics.

G. 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

[9] In this formula, we treat the risk choice as investing a \$1 endowment into either the risk-free or risky asset.
[10] We do not restrict the reservation risk premium to be positive because we observe some risk-seeking behavior among our subjects.

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 because religious service attendance is 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.

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 for public good contribution amounts, reciprocity as the Employee in the gift-exchange game, and risk aversion. The explanatory variables are a religion-salient dummy, the strength of belief in divine punishment, and the interaction between these two. The regression for Jewish subjects' risk aversion is omitted due to insufficient sample size.

The one significant interaction is for Protestants' contribution to the public good. Protestants with the mean belief in divine punishment significantly increase their contribution by 16 cents when primed, going from 55 cents to 71 cents. Protestants with a belief in divine punishment one standard deviation above the mean start from a lower unprimed baseline but have a larger increase in their contribution when primed, so that their primed contribution is similar to (but slightly larger than) the average Protestant; these subjects' contribution rises from 40 cents to 73 cents. The negative relationship between belief in divine punishment and unprimed contributions is significant. These findings are consistent with two different explanations:

(1) Protestants' preferred contribution in the absence of identity considerations (x_0) is negatively correlated with belief in divine punishment, depressing unprimed

contributions of Protestants with high belief in divine punishment. Protestant public good contribution norms (x_C) are positively correlated with belief in divine punishment, which is why primed contributions among Protestants with high divine punishment beliefs are slightly larger than primed contributions among those with average divine punishment beliefs.[11] This explanation is the closest to that of Barro and McCleary (2003, 2006), where the salutary effects of belief in divine punishment operate through changes in norms.

(2) The Protestant contribution norm does not vary with belief in divine punishment. Instead, the observed treatment interaction is explained by a positive correlation between divine punishment beliefs and sensitivity to our salience manipulation. Differences in sensitivity can be driven by differences in how much decision weights respond to an identity salience shock of a given magnitude (w') or by differences in how much our priming manipulation raises identity salience (ε). A negative correlation between preferred contributions in the absence of identity considerations (x_0) and belief in divine punishment could explain the observed negative correlation between belief in divine punishment and unprimed contributions. Alternatively, x_0 could be uncorrelated with belief in divine punishment, but steady-state religious identity strength $\overline{s}$ is negatively correlated with belief in divine punishment, reducing unprimed contributions among those with high divine punishment beliefs. The existence of such a negative correlation strikes us as counterintuitive and unlikely.

Unfortunately, our data do not distinguish between these two explanations.

We explore interactions between religious identity effects and religious service attendance in Table 8. The Jewish sample is dropped from the reciprocity analysis because all Jewish subjects with above-median service attendance were randomized into the religion-salient treatment. We again drop the Jewish sample from the risk aversion analysis because of insufficient sample size.

[11] In order for this explanation to be consistent with the stylized facts, $[\tilde{x}_C - \tilde{x}_0 - (x_C - x_0)]w'\varepsilon$ must be greater than $x_0 - \tilde{x}_0 + w[x_C - x_0 - (\tilde{x}_C - \tilde{x}_0)]$, where $\tilde{x}$ denotes values held by those with high divine punishment beliefs and x denotes values held by those with low divine punishment beliefs.

We find no statistically significant interactions between attendance frequency and religious identity effects among Protestants and Catholics. Interestingly, however, the point estimates indicate that the decrease in risk aversion caused by Catholic identity is concentrated among those who attend services less than once a month. Infrequent attendees' reservation risk premium drops by 29 percentage points when primed (significant at the 10 percent level), whereas more frequent attendees' reservation risk premium drops by only 6 percentage points, which is statistically insignificant.

Among atheists and agnostics, we find that the drop in Employee reciprocity in response to the religious prime occurs exclusively among those who never attend religious services. Among those who attend with some positive frequency, there is no change in reciprocity due to the prime. This interaction is caused by the difference in responses between atheists and agnostics. Recall that in Section III.C, we found that among atheists and agnostics, the significant drop in Manager wage offers in response to the religious prime was driven by atheists. Therefore, there may be an atheist norm that drives their behavior towards subgame perfect equilibrium predictions on both sides of labor markets with incomplete contracts.

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 greater behavioral influence when that identity is temporarily 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, and there is suggestive evidence that it increases employee reciprocity in a labor market with incomplete contracts. Catholicism decreases contributions to public goods, increases employee reciprocity, and decreases risk aversion. Judaism increases labor market reciprocity. There is some evidence that Christianity and Judaism increase employer wage offers. However, we find no evidence that religious identity affects discount rates or purely altruistic generosity.

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Table 1. Subject Characteristics

		Protestant	Catholic	Jewish	Agnostic/ Atheist
Male (%)	Control	35.1	49.1	56.7	43.1
	Religion salient	41.9	38.3	70.0	49.2
	p-value of difference	0.392	0.259	0.347	0.500
SAT I Math score (mean)	Control	705	688	716	729
	Religion salient	700	690	714	741
	p-value of difference	0.703	0.878	0.896	0.252
SAT I Verbal score (mean)	Control	676	669	659	698
	Religion salient	655	654	672	700
	p-value of difference	0.148	0.411	0.532	0.859
Parental income > $80,000 (%)	Control	65.8	64.2	89.3	55.4
	Religion salient	52.9	63.5	86.4	61.9
	p-value of difference	0.118	0.942	0.758	0.473
N		151	115	53	123

Note: Some statistics are calculated using fewer observations than indicated in the last row because of non-response.

Table 2. Public Goods Game Results

	Protestant	Catholic	Jewish	Agnostic/Atheist
Panel A. Amount contributed to public good				
Religion salient	0.17***	-0.15*	0.00	0.15*
	(0.07)	(0.08)	(0.12)	(0.08)
Constant	0.55***	0.69***	0.53***	0.43
	(0.05)	(0.05)	(0.08)	(0.06)
N	146	114	48	118
Panel B. Expectation of others' contribution to public good				
Religion salient	0.08	-0.07	0.08	0.03
	(0.05)	(0.06)	(0.08)	(0.06)
Constant	0.59***	0.72***	0.61***	0.55***
	(0.04)	(0.04)	(0.05)	(0.04)
N	146	114	48	118
Panel C. Relationship between own contribution and expectation of others' contribution				
Religion salient	0.16	-0.22	-0.22	0.01
	(0.11)	(0.14)	(0.25)	(0.10)
E(*Others' contribution*)	0.93***	0.82***	0.88***	0.87***
	(0.11)	(0.13)	(0.20)	(0.12)
Religion salient ×	-0.09	0.19	0.22	0.20
E(*Others' contribution*)	(0.15)	(0.18)	(0.35)	(0.16)
Constant	0.00	0.10	0.00	-0.04
	(0.07)	(0.10)	(0.14)	(0.07)
N	146	114	48	118

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. 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 3. Gift-Exchange Game Results

	Protestant	Catholic	Jewish	Agnostic/Atheist
Panel A. Wage offered as manager				
Religion salient	0.34	0.22	0.50	-0.49**
	(0.24)	(0.23)	(0.32)	(0.23)
Constant	1.40***	1.18***	0.94***	1.52***
	(0.15)	(0.18)	(0.22)	(0.18)
N	69	67	31	59
Panel B. Mean work cost expended as employee				
Religion salient	0.04	0.06*	0.12**	-0.05
	(0.14)	(0.03)	(0.05)	(0.04)
Constant	0.14***	0.10***	0.06*	0.17
	(0.02)	(0.02)	(0.04)	(0.03)
N	69	67	31	59
Panel C. Slope from regression of work cost on wages between $0 and $3.50				
Religion salient	0.03	0.04*	0.07**	-0.04
	(0.02)	(0.02)	(0.03)	(0.02)
Constant	0.08***	0.05***	0.03	0.11***
	(0.01)	(0.01)	(0.02)	(0.02)
N	69	67	31	59

Note: This table shows regression results where the dependent variable is the wage offered as a manager (Panel A), average work cost expended as an employee (Panel B), or the subject-specific slope coefficient from a regression of work cost expended as an employee on managerial wage offered (Panel C). *Religion salient* is a dummy for being in the religion-salient condition. 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 4. Dictator Game Results

	Protestant	Catholic	Jewish	Agnostic/Atheist
Religion salient	-0.02	-0.03	-0.01	-0.04
	(0.05)	(0.05)	(0.07)	(0.04)
Constant	0.24***	0.17***	0.14***	0.16***
	(0.03)	(0.03)	(0.05)	(0.03)
N	151	115	53	123

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. Standard errors are in parentheses below the point estimates.

Table 5. Discount Rate Results

	Protestant	Catholic	Jewish	Agnostic/Atheist
Religion salient	0.31	-0.01	1.13	0.40
	(0.42)	(0.69)	(0.77)	(0.69)
1 week vs. 2 weeks	-0.01	-0.01	-0.44	-0.32
	(0.13)	(0.22)	(0.37)	(0.21)
Constant	-2.69***	-3.21***	-3.78***	-4.77***
	(0.29)	(0.49)	(0.64)	(0.56)
$\hat{\sigma}$	1.91	2.36	1.86	2.53
	(0.20)	(0.31)	(0.24)	(0.26)
N	164	96	44	128

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 6. Risk Aversion Results

	Protestant	Catholic	Jewish	Agnostic/ Atheist
Religion salient	-0.04	-0.18*	0.06	-0.05
	(0.07)	(0.09)	(0.07)	(0.07)
Larger Stakes	0.29***	0.32***	0.31***	0.27***
	(0.05)	(0.07)	(0.11)	(0.06)
Constant	0.22***	0.26***	0.05	0.12***
	(0.06)	(0.07)	(0.06)	(0.04)
$\hat{\sigma}$	0.38	0.38	0.28	0.34
	(0.03)	(0.04)	(0.04)	(0.03)
N	164	96	44	128

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 standardf 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 7. Religion-Salience Treatment Interactions with Belief in Divine Punishment

	Protestant	Catholic	Jewish	Agnostic/Atheist
Panel A. Amount contributed to public good				
Religion salient	0.16**	-0.13	0.00	0.04
	(0.08)	(0.09)	(0.15)	(0.10)
Religion Salient ×	0.17**	-0.05	-0.03	-0.12
Divine punishment	(0.08)	(0.09)	(0.15)	(0.08)
Divine punishment	-0.15***	0.06	0.04	-0.12
	(0.05)	(0.07)	(0.10)	(0.08)
Constant	0.55***	0.69***	0.48	0.55***
	(0.05)	(0.07)	(0.10)	(0.07)
N	97	79	33	77
Panel B. Slope from regression of work cost on wages between \$0 and \$3.50				
Religion salient	0.03	0.03*	0.07**	-0.04
	(0.02)	(0.02)	(0.03)	(0.02)
Religion Salient ×	-0.00	0.00	0.01	0.01
Divine punishment	(0.02)	(0.02)	(0.03)	(0.02)
Divine punishment	-0.01	-0.01	0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)
Constant	0.08***	0.06***	0.03	0.11***
	(0.01)	(0.01)	(0.02)	(0.02)
N	69	67	31	59
Panel C. Risk aversion				
Religion salient	-0.01	-0.40**	--	0.01
	(0.10)	(0.16)		(0.12)
Religion Salient ×	-0.12	-0.32	--	0.08
Divine punishment	(0.09)	(0.24)		(0.09)
Divine punishment	0.04	0.22	--	-0.10
	(0.07)	(0.21)		(0.07)
Larger stakes	0.21***	0.62***	--	0.18***
	(0.06)	(0.15)		(0.06)
$\hat{\sigma}$	0.21	0.34	--	0.30
	(0.04)	(0.04)		(0.05)
Constant	0.18**	0.36***	--	0.19***
	(0.09)	(0.12)		(0.06)
N	66	26	--	46

Note: Panel A shows regression results where the dependent variable is the amount contributed to the public good. Panel B 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 C shows interval regression results 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. *Divine punishment* is the self-reported belief in divine punishment, normalized to have zero mean and unit standard deviation within each 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 variable is denoted by $\hat{\sigma}$. Standard errors are in parentheses below the point estimates; these are Huber-White standard errors, clustered by subject, for the risk aversion regressions. The number of observations corresponds to the number of subjects in Panels A and B, and the number of reservation risk premium intervals observed in Panel C.

Table 8. Religion-Salience Treatment Interactions with Religious Service Attendance

	Protestant	Catholic	Jewish	Agnostic/Atheist
Panel A. Amount contributed to public good				
Religion salient	0.16*	-0.14	0.03	0.10
	(0.09)	(0.11)	(0.14)	(0.09)
Religion Salient ×	0.02	-0.02	-0.20	0.18
(*Attendance > median*)	(0.13)	(0.15)	(0.33)	(0.17)
Attendance > median	-0.09	0.03	0.15	-0.21*
	(0.09)	(0.11)	(0.26)	(0.12)
Constant	0.59	0.67***	0.51***	0.49***
	(0.06)	(0.08)	(0.09)	(0.07)
N	146	114	48	118
Panel B. Slope from regression of work cost on wages between \$0 and \$3.50				
Religion salient	0.03	0.03	--	-0.07**
	(0.02)	(0.03)		(0.03)
Religion Salient ×	-0.01	0.00	--	0.09*
(*Attendance > median*)	(0.04)	(0.04)		(0.05)
Attendance > median	0.02	-0.01	--	-0.05
	(0.03)	(0.03)		(0.03)
Constant	0.07***	0.06***	--	0.13***
	(0.02)	(0.02)		(0.02)
N	69	67	--	59
Panel C. Risk aversion				
Religion salient	-0.07	-0.29*	--	0.02
	(0.11)	(0.15)		(0.07)
Religion Salient ×	0.06	0.23	--	-0.46***
(*Attendance > median*)	(0.16)	(0.19)		(0.17)
Attendance > median	-0.04	-0.02	--	0.18*
	(0.12)	(0.15)		(0.09)
Larger stakes	0.29***	0.32***	--	0.27***
	(0.05)	(0.07)		(0.06)
$\hat{\sigma}$	0.38	0.27	--	0.33
	(0.03)	(0.04)		(0.03)
Constant	0.25**	0.27**	--	0.09**
	(0.10)	(0.13)		(0.04)
N	164	96	--	128

Note: Panel A shows regression results where the dependent variable is the amount contributed to the public good. Panel B 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 C shows interval regression results 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. *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 variable is denoted by $\hat{\sigma}$. Standard errors are in parentheses below the point estimates; these are Huber-White standard errors, clustered by subject, for the risk aversion regressions. The number of observations corresponds to the number of subjects in Panels A and B, and the number of reservation risk premium intervals observed in Panel C.

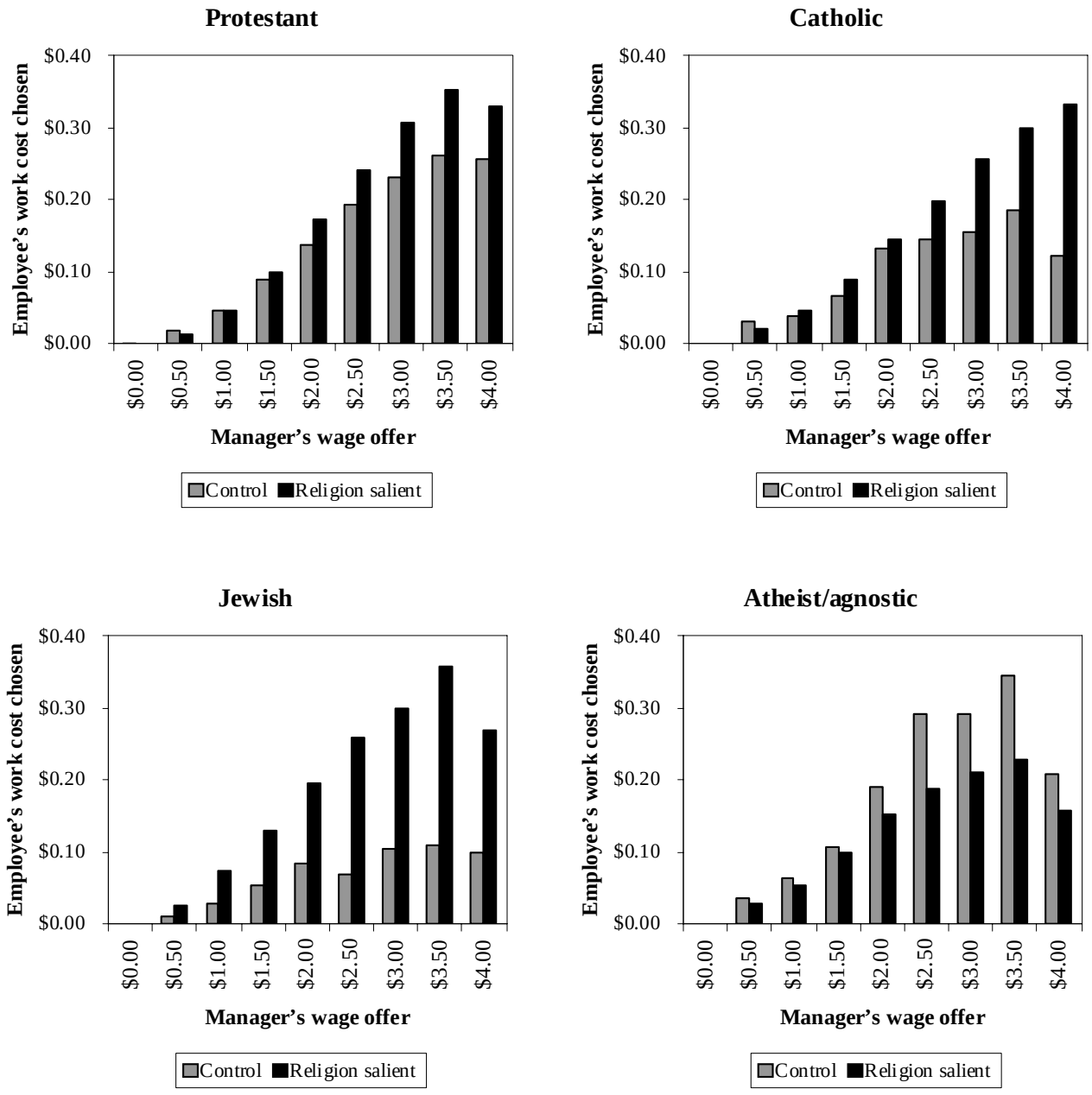


Figure 1. Employee work costs chosen in response to managerial wage offers in gift exchange game.