Does Religious Proscription Cause People to Act Differently? Evidence from a Theory-Based Test

August 2010—Preliminary, Please do not Cite

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Abstract

Hundreds of studies have shown that highly religious individuals have different outcomes than others. These differences may reflect an impact from religious adherence, since religious institutions often proscribe certain behaviors. But it is possible that individuals with high tastes for religion coincidentally have low tastes for activities that religions prohibit; researchers in this area lack an accepted instrument for religion and have struggled to separate causation from correlation. This paper uses the canonical model of religious participation to develop an empirical test to separate causation and correlation. The test relies on exogenous variation in secular activities, and thus does not require an instrument for religious participation. Empirical applications of the test to blue laws, casino openings, and the legal drinking age are then conducted. Despite a concern for type-II error, the results suggest that religious proscription has a causal effect on outcomes.

Introduction

Religious individuals often have different outcomes than other individuals. For example, highly religious individuals are less likely to drink heavily or use illegal drugs, are less likely to engage in risky sexual behavior, are less likely to engage in delinquent or criminal activities and have lower recidivism upon leaving the criminal justice system, are more likely to be married, report better health outcomes along a vast number of dimensions, live longer, have higher levels of civic participation, and are more likely to make charitable contributions. These differences have been well established by hundreds of studies across the social sciences.[1] Moreover, these differences are not only statistically identifiable, but are often large in magnitude; religiosity is one of the strongest known predictors for outcomes such as charitable giving, civic participation, and risky behavior.[2]

A crucial question unanswered by this body of research is whether the relationship between religion and other outcomes is causal. It is certainly plausible that religious participation affects outcomes and decisions, as many religious organizations actively proscribe certain activities and behaviors. But it could be that individuals with a preference for religion could also have preferences against things like binge drinking, in which case the behavior of the religious may reflect a correlation of preferences rather than a true impact of religion on behavior.

Typically, an economist might address this issue by developing an instrumental variable (IV) that affects religious behavior but does not otherwise affect outcomes of interest; variation in religiosity created by changes in the instrument could then be used to establish causality. But there is no accepted

[1] Perhaps the best starting place to survey work on differences in outcomes between religious and non-religious is Johnson, Tompkins, and Webb (2002), which surveys nearly 800 articles and a host of outcomes. More recent surveys include Wallace, Myers, and Osai (2004) who survey work on religion and substance abuse; Regnerus (2003) who surveys work on religion and various outcomes for adolescents; Ferguson, Wu, Spruijt-Metz and Dyrness (2007) who survey work faith-based social service provision; Marks (2005) who surveys work on religion and various health outcomes; Wong, Rew, and Slaikeu (2006) who survey work adolescent religiosity and mental health; Dew et al. (2008) who survey work on religion and adolescent psychiatric symptoms; Rew and Wong (2006) who survey work on adolescent health and religion; Johnson (2008) who discusses work on prisoner recidivism and religion; and Lillard and Price (2007) who discuss work on religion and outcomes for youth in disadvantaged families.

[2] For a discussion of the importance of religion in civic behavior, see Gerber, Gruber, and Hungerman (2010). For a discussion of the importance of religion and charitable giving, see Andreoni (2006). For evidence of the magnitude of the religion-risky behavior relationship, see Gruber and Hungerman (2008).

instrumental variable for religious practice. One reason for a lack of instruments in religion is that many instruments are based on policy changes, but in many countries laws respecting an establishment of religion are prohibited. Hungerman (forthcoming) (who looks at religious markets), Altonji, Elder, and Taber (2005) (who look at religious education) and Lillard and Price (2007) (who look at religious practice more generally) all review instruments or other identification strategies that have been proposed and in each case the authors find that available identification strategies have serious limitations.

In this paper I consider a new methodology for establishing whether or not religious affiliation changes the behavior of adherents. Instead of pursuing an instrumental variables strategy, I develop an empirical test based on the best-known model of religious affiliation, by Laurence Iannaccone (1992). Iannaccone's model depicts religious organizations as a club where religiosity has a positive externality: each individual's religious participation affects overall club quality and thus the wellbeing of other members. Since religious participation generates a positive externality for others in the club there is a standard externality problem: religious participation will be inefficiently low. Congregations can counteract this problem by proscribing their adherents' behavior—for example, forbidding excessive drinking or mandating that adherents refrain from labor activities on the Sabbath. These actions by the congregation serve to increase the opportunity cost, or price, of secular consumption, but this price increase can actually improve member utility because it solves the externality problem.

I show that such a price increase will be especially effective in overcoming the externality problem under two (not mutually exclusive) situations. In the first situation, individuals derive very high utility from the quality of the religious good, and they derive low utility from secular activities. Hence, they are very willing to face a higher price for secular consumption if they can then enjoy a higher-quality religious club. But even absent religious instruction, these individuals would be reluctant to substitute into behaviors that the religion forbids. Indeed, they are willing to forgo the activities forbidden by the religious group precisely because they place low value on the forbidden activities anyways. One might think of these as "correlation" members; their secular outcomes reflect not a causal effect of religion but rather a correlation of tastes for religion and against secular activities.

But there is a second situation wherein congregations can successfully prohibit secular activities. In the second situation, adherents view religious consumption and proscribed secular activities as substitutes: for these individuals the cross-price elasticity of religious participation with respect to the cost of the secular activity is very high. Unlike correlation members, these "causal members" change their behavior in the face of religious instruction—in fact, they change their behavior so much that the positive externality generated by their actions leads to efficiency gains for the club overall.

The model thus predicts that highly religious individuals will undertake less binge drinking and other prohibited behavior. However, the model allows both for individuals who do not respond to the congregation's efforts to alter secular consumption (correlational members) and for individuals who are very responsive (causal members). The key difference between correlational members and causal members is that the latter view secular activities and religious activities as substitutes, and hence their religiosity is responsive to incentives to engage in secular behavior. Thus empirical evidence on whether highly religious individuals respond to secular incentives can be exploited to distinguish causation and correlation. Importantly, it is variation in *secular* incentives that is needed to test for causality. From an econometric standpoint, exogenous variation in religiosity (i.e., an instrumental variable for religion) is not needed.

In the second part of the paper, I consider a battery of tests of whether highly religious individuals are responsive to secular incentives. I define "highly religious" using both denominational strictness and ex-ante measures of religious consumption. I use three sources of variation in secular incentives: changes in blue laws, casino openings, and variation in minimum legal drinking age laws. I consider changes in attendance, religious spending, and heavy drinking as outcomes using a variety of datasets.

In every case but one I find strong evidence that highly religious individuals are in fact very responsive to changes in secular incentives. For example, churches in the most conservative denominations see a 1 percent fall in spending when a nearby county opens a casino, weekly attendance falls by 4.5 percentage points when blue laws are repealed, and those who initially attend worship weekly or more are about 3 percentage points more likely to become heavy drinkers (a 10 percent effect) upon

reaching the legal drinking age. (The lone exception to these tests are regressions on religious attendance in the face of casino openings, which are statically insignificant; I discuss these more in section 4). Strikingly, in many specifications it is the *most* religious individuals who are the *most* responsive to changes in blue laws, casino availability, or the legal drinking age. These counter-intuitive results suggest that highly religious individuals are responsive to incentives to engage in prohibited activities. Highly religious individuals are thus "marginal" when it comes to prohibited activities, and consequently the differences in behaviors among religious and non-religious are not merely driven by individuals who coincidentally happen to like religion and to not like prohibited secular activities.

The methodology outlined here could be applied in any situation where differences in behavior might reflect religious instruction in a way consistent with the model, and where there is suitable secular variation in the data. I discuss in the conclusions a number of possible settings where work such as this could be extended. I also discuss the fact that the model is not limited to religious practice but could also include secular clubs as well. Extensions to the model might have to deal with concerns for type II error for reasons described in section 2. But the empirical results presented here give optimism that such error can be overcome.

The paper proceeds as follows. Section 2 lays out the model and the theoretical foundation for the test. Section 3 discusses the sources of secular incentives to be used. Section 4 reports specifications and empirical test results. Section 5 concludes.

2. The Model

The model is based off Iannaccone (1992). My exposition of the model will be brief as I add nothing substantial to the theoretical environment. Instead, my contribution is to recast Iannaccone's central result in a more intuitive way. Aside from providing clearer intuition for the model's central result, my derivation shows that Iannaccone's result holds in a counterintuitive setting that generates a useful empirical test—a test that is not obvious from the original derivation. Those unfamiliar with the model may wish to consult Iannaccone (1992), as well as Iannaccone (1994); these papers provide excellent rationale for many of the model's features. Dozens of other studies have also discussed

enriching Iannaccone's model in a number of ways, such as Berman (2000), Makowski (forthcoming), McBride (2007) or Finke and Stark (2006).

The impetus of the model is that religious organizations are largely voluntary, and yet many religious organizations make strong and seemingly irrational demands of their members. The discussion here will be especially concerned with religious demands that impinge on economically relevant secular behaviors and outcomes. There are a number of examples. For instance, many monotheistic religions argue for a Sabbath day wherein members should voluntarily refrain from productive labor activities. To quote the 4th Commandment: "Remember the Sabbath day by keeping it holy. Six days you shall labor and do all your work, but the seventh day is a Sabbath to the Lord your God". Muslims have a similar obligation to "drop all business" for Friday prayer, although Friday prayer is not the same as the Sabbath.

Many religious traditions also argue against excessive alcohol consumption or any alcohol consumption at all. To quote the religious text Isaiah 5:22: "Woe to those who are heroes at drinking wine and champions at mixing drinks." Similarly, in the Qur'an: "In them (alcohol & gambling) is a great sin, and (some) benefit for men, but the sin of them is greater than their benefit." Religious groups which prohibit alcohol altogether include the church of Latter Day Saints (the Mormon Church), many traditions in Islam, Sikhism, Seventh-Day Adventist, and some groups in Buddhism, Hinduism, and Protestantism. Beyond religious texts, religious groups may attempt to enforce these beliefs through the timing of religious services, promoting social interactions that make subterfuge difficult, and through social activism. The model presented here shows why these steps may be rational actions for religious organizations to take, and why these actions may or may not have a significant impact on adherent behavior.[3]

Before pursuing the formal model, however, consider first a simple example that illustrates the rough intuition. The example here is somewhat simplistic and omits certain key elements of the model, but it nonetheless gives a rough impression of the model's logic. Panel I of Figure 1 depicts an individual choosing a consumption bundle consisting of two goods: a religious good and a "risky" good (e.g.,

[3] Some religious traditions take these prohibitions more seriously than others; this is discussed more below.

alcohol). The individual faces a budget constraint and has standard (i.e., quasiconcave) preferences. In addition to points on or inside of the budget constraint, however, suppose that there is a religious club that the individual can join. For simplicity, suppose the club offers members a single alternative consumption bundle, point A. This bundle provides greater religious consumption than the individual can obtain on his own, but the religious group forbids risky consumption. The individual in Figure 1 would choose not to join the club, and instead would choose point B.

Panels II and III of Figure 1 illustrate two alternative situations wherein the individual would instead choose point A and would join a religious group that forbids risky consumption. In panel II, the indifference curves have been "rotated" and made steeper. This individual would give up large amounts of risky consumption to obtain a small increase in religion and would clearly be best off choosing point A. If there was no religious club available, this individual would choose point B—which, like point A, is a point with low risky behavior. While this individual chooses to the join the club and forgoes risky behavior, the individual's risky consumption would be negligible regardless of whether the religious club exists or not—this is a "correlation" member. Now consider a drop in the price of risky behavior that shifts the budget constraint out to the new dashed line. If point A is still available, this individual will continue to choose point A even after this change in price occurs.

In panel III, the indifference curves have been "flattened" out so that this individual views risky and religious behavior as substitutable. This individual will also choose point A over bundles on the solid budget constraint. However, if the religious club did not exist and point A was not available, the individual would be best off at point B—a point where the risky consumption is much greater than at point A (the indifference curve passing through point B has been omitted to avoid clutter). For this individual, unlike the individual in panel II, the availability of the club has an important impact on risky behavior. But consider a drop in the price of the risky good for this individual. Unlike the individual in panel II, in panel III a drop in the price of the risky behavior may cause a radical deviation in consumption—from A to A'. The figure thus illustrates the well-established empirical fact that religious individuals (like those at point A in panels II and III) undertake less risky consumption. The figure shows

how this could represent either a correlation of preferences (panel II) or an effect of the club (panel III), and the figure illustrates how secular price changes can be used to distinguish between correlation and causation.

The model below provides this intuition more rigorously. While Figure 1 provides the basic intuition, the figure's depiction is simplistic. Figure 1 is a "partial equilibrium" picture that ignores how one person's choice might affect club decisions and outcomes, but the model allows for interactions among club members by distinguishing between one's own religious consumption and the benefit one receives from other's consumption. In fact, it is the externality-based benefit one receives from other's consumption—which is not depicted in simple two-good scenarios in Figure 1—that allows religious proscription to increase club member utility in equilibrium. The addition of an externality that is not depicted in Figure 1 thus plays a key role in the logic of model.

The model develops the simple intuition in Figure 1 in a number of other ways as well. Importantly, the model below shows why religious groups might endogenously choose to offer their members bundles that limit risky consumption; the picture provides no such rationale for the location of point A. The fact that religious groups endogenously choose incentives is important to the intuition behind the test, as it provides a nonstandard connection between substitutability and causality, as discussed below. The formal model provides a mechanism by which religious groups can increase religious consumption, and relaxes the assumption that the club simply offers a single consumption bundle to members; instead the model depicts a club offering a separate technology (ie a set of feasible consumption choices) to members by which they optimize.

Consider a "religious club" of $N + 1$ identical individuals. The assumption that individuals within a club are identical is mainly done for convenience; I discuss in the appendix extending the result to heterogeneity within clubs. Each club member i derives utility from three things: secular consumption S^i, individual i's religious participation R^i, and the overall "quality" of the club, Q^i:

$$U^i = U(S^i, R^i, Q^i),$$

where *U* is twice continuously differentiable, strictly quasi-concave, and strictly increasing in all its arguments. Individuals choose their secular and religious consumption subject to a budget constraint:

$$\pi_s S^i + \pi_r R^i \leq I^i \tag{1}$$

where *I* is endowed resources and prices are given by π_s and π_r.[4] Group quality, which individual *i* takes as given, is determined by:

$$Q^i = F(\bar{R}^i, N), \tag{2}$$

where $\bar{R}^i = \sum_{k \neq i} \frac{R^k}{N}$ is the average level of *R* for all club members other than *i*. Assume that the technology *F* is twice continuously differentiable, concave, and increasing in its arguments. Thus individuals' religious participation R^i benefits others in the club, as it affects club quality for other members. Intuitively, individuals value being associated with a club where other members are dedicated to club activities so that the quality of worship service and social interaction is high. It is straightforward to show that this externality leads to a Nash Equilibrium outcome that is not Pareto optimal, as individuals fail to account for the external benefits from consuming R^i. The congregation may be able to address this inefficiency by raising the cost of secular activity, that is, increasing π_s.[5] To see this, consider a symmetric Nash Equilibrium. The typical club member's indirect utility function is given by

$$V(I, \pi_s, \pi_r, Q) = \max_{R,S} \left\{ U(S, R, Q) \middle| \pi_s S + \pi_r R \leq I \right\}. \tag{3}$$

Equilibrium quality *Q* will satisfy the expression

$$F[R(I, \pi_s, \pi_r, Q^e), N] = Q^e. \tag{4}$$

The equilibrium will be stable if $(\partial F / \partial \bar{R})(\partial R / \partial Q)$ is less than unity.

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[4] The equation is comparable to equation (4) in Iannaccone (1992). As Iannaccone shows, the model is easily adapted to both monetary and temporal resources at once; here the distinction is unimportant and readers can consider consumption as either in time or money.

[5] One might also wonder whether congregations could address this issue by lowering π_r (subsidizing religion); this may be impractical for a number of reasons, including the fact that true religiosity may be hard to observe. See Iannaccone (1992) for a discussion of the feasibility of alternatives to raising the price π_s.

If the price π_s increases, the equilibrium change in utility is:

$$\frac{dU^e}{d\pi_s} = \frac{\partial V}{\partial \pi_s} + \frac{\partial V}{\partial Q^e}\frac{dQ^e}{d\pi_s}. \tag{5}$$

One can show that equilibrium utility will increase when the price of secular consumption rises if the following inequality holds: [6]

$$\varepsilon_{r\pi_s}\frac{U_Q^e Q^e}{U_S^e S^e} > \frac{1-\varepsilon_{Fr}\varepsilon_{rq}}{\varepsilon_{Fr}}, \tag{6}$$

where ε_{ab} is the elasticity of *a* with respect to *b*, Q^e and S^e are the equilibrium levels of *Q* and *S*, and $U_a^e = \left.\frac{\partial U}{\partial a}\right|_{\substack{R=R^e \\ S=S^e \\ Q=Q^e}}$.[7] The right hand side is an unobservable group-idiosyncratic expression governed by the group technology and stability ensures that the right hand side of (6) is positive. The left-hand side of (6) shows that there are essentially two factors which determine when a congregation can raise welfare by increasing the costs of secular activities, a ratio of utilities and the elasticity $\varepsilon_{r\pi_s}$.

Consider first the ratio $\frac{U_Q^e Q^e}{U_S^e S^e}$. The numerator of this ratio is the marginal utility of club quality in equilibrium, multiplied by equilibrium quality. This gives a first-order approximation of *total* utility derived by quality in equilibrium. The denominator of this ratio correspondingly represents utility derived from secular consumption. If the expression in (6) is driven by members having large values of this ratio, then the religious group could effectively raise prices even if individuals are largely unresponsive to the price change—that is, the cross-price elasticity $\varepsilon_{r\pi_s}$ is small. In this case, individuals may attend very strict religious organizations where the cost of secular consumption is very high, but these individuals are not responsive to the strictness of the religious group. Intuitively, these individuals

[6] The appendix shows how to derive (6) from (5).

[7] The expression (6) is closest to expression (14) in Iannaccone's paper, although his expression is based on expenditure share terms instead of equilibrium utility. Note also that Iannaccone does not provide intuition for the expenditure share term k_j in his derivation.

derive a very high amount of utility from religious quality; these individuals are thus happy to have secular activities discouraged, as this results in an increase in group quality that they could not obtain on their own (since quality is taken as given). Individuals in this case may be described as "correlation" individuals who happen to "like" religion and "not like" secular consumption.

But there is a second term in (6), $\varepsilon_{r\pi_s}$. This shows that groups can profit from raising secular costs when members have especially large cross-price elasticities. In this case, members are *extremely* responsive to religious prohibitions; these individuals view secular and religious consumption as highly substitutable. A strict religious group can thrive with such individuals because the group gets a big "bang for its buck" when it raises the cost of secular activity. A change in π_s results in a large change in individuals' religious consumption and hence a large increase in the externality. These high-substitution individuals *are* affected by religious participation; indeed, it is their responsiveness to religious advocacy that promotes efficiency when secular costs are raised. The model thus suggests that differences in behavior among the very religious might be driven by correlation, or by causation.

Consider the competing roles of causation and correlation in a numerical example based on a CES utility function, $U = \left(S^{\beta} + \gamma K^{\beta}\right)^{1/\beta}$, $K = R^{\alpha} Q^{1-\alpha}$. Here the parameter β determines how "substitutable" secular and religious consumption are (although β itself is not the cross-price elasticity), and the term γ determines the weight individuals put on religious consumption. Let the technology for creating group quality be $Q = \overline{R}$ and let each individual face a budget constraint $\pi_S S + R = I$ so that *R* is a numeraire good. The symmetric Nash Equilibrium solutions are $R^e = I / \left(1 + \pi_s^{\beta/(1-\beta)} \left(\gamma\alpha\right)^{1/(1-\beta)}\right)$ and $S^e = I / \left(\pi_s + \left(\pi_s \gamma\alpha\right)^{1/(1-\beta)}\right)$. Consider an equilibrium where $I = 1$, $\alpha = 0.3$, and $\pi_s = 4$. The parameters β and γ will vary.

Figure 2 depicts the equilibrium cross-price elasticity $\varepsilon_{r\pi_s}$ for various values of β and γ. (The variable β ranges from 0.2 to 1 and the variable γ varies from 1 to 6.) The figure shows a spike in the

price elasticity as β and γ both approach 1. As these parameters approach unity the utility function becomes increasing linear and individuals place equal weight on religious and secular consumption. Individuals with these parameters would thus be radically responsive to religious changes in π_s as they view secular consumption and religious consumption as nearly perfect substitutes. The point with the lowest elasticity, where individuals are the least responsiveness to religious instruction, is in the opposite corner, where β is low and γ is high. These individuals view secular and religious consumption as not substitutable and place high amount of weight on religious consumption, making them unwilling to alter their religious consumption when the price of secular consumption changes.

Figure 3 depicts the derivative of equilibrium utility with respect to π_s. Strikingly, there are two clear peaks in the figure and they are in opposite corners. The largest gains in utility from an increase in the cost of secular consumption are found for groups whose members view secular consumption as the *most* substitutable with religion and those whose members view it as the *least* substitutable with religion. The responsiveness of religious participation to secular incentives here distinguishes those who are affected by religious institutions and those who are not. This responsiveness is empirically observable and it forms the basis of the empirical tests below.

Consider, for example, the case of heavy drinking as a secular activity. Intuitively, empirical evidence that highly religious individuals are very responsive to incentives for engaging in heavy drinking would be evidence that a low incidence of heavy drinking among the religious is *not* merely driven by the religious having low tastes for drinking—because if they had low tastes for drinking, it follows that they would be unresponsive to incentives to drink. The empirical test proposed here is thus a test of whether highly religious individuals are in fact highly marginal individuals when it comes to religiously-proscribed behavior. Evidence that highly religious individuals are responsive to secular incentives can be taken as evidence that religious adherence has a causal effect on behavior.

The value of this test is that it is variation in *secular* incentives which can be used to distinguish causal members from correlational members. Thus empirically exogenous variation in secular incentives,

rather than exogenous variation in religion, is sufficient. The relative availability of exogenous variation in secular activities, combined with the general nature of the model, gives this test potentially broad applicability.

It is important to note that this test is based on a theoretical result that gives an endogenous rationale for altering prices. The model's depiction of a club that alters members incentives allows for a nonstandard connection between substitutability and causation. Hot dogs and hamburgers may be substitutes, but generally it would be incorrect to conclude from this that eating hot dogs causes one to eat fewer hamburgers. The difference here is that inequality (6) shows that religious groups might optimally choose to affect member incentives by proscribing behavior—and thus individuals inside a religious club will face a set of endogenously determined prices different from those outside of the club. There is no analogy to this in a simple hot-dog-hamburger story, where the prices of hot dogs and hamburgers are the same for all individuals. In the case where religious individuals face different incentives, evidence of substitutability can establish whether the different incentives imposed by the club have an effect on behavior.

As mentioned earlier, the two conditions which admit strict religiosity in inequality (6) may not be mutually exclusive; it could be that empirically there are both correlation members and causation members in the data at the same time. There is a chance in such a situation that the unresponsive correlational members could "drown out" the responsiveness of causal members, so that a researcher looking for evidence of causality could not reject the hypothesis that members are unresponsive to incentives even though in truth some members were responsive—type II error. The test described here is thus potentially low-power. The importance of this is likely situation-dependent and will be considered more below.

As is well known, this model makes a number of other predictions that have been empirically validated.[8] The key empirical test proposed here—to see whether highly religious individuals are

[8] For instance, the model predicts that stronger levels of religious participation should be associated with less risky or other religiously prohibited behavior, that strict religious groups should demand "sacrifices" from members or

extremely responsive to incentives for secular behavior— has not been explored and indeed is somewhat counter-intuitive. One might regard the model's prediction of high substitutability as an "Alcoholics Anonymous"-style depiction of religion. The model suggests that religious groups may in some circumstances successfully be able to make strong demands of their members regarding secular behavior (e.g., prohibiting drinking) because these members respond to the demands with intense religious devotion.[9]

It is important to note that this test, like the model used to derive it, is developed for strict religious groups, meaning groups that are especially concerned with directing members' behavior and establishing tension between members and nonmembers. This focus seems reasonable given prior work showing that that different outcomes for religious individuals are often salient among the highly religious who adhere to strict, high-tension faiths (cf. Grasmick, Kinsey, and Cochran, 1991; Hammond, Cole, and Beck; 1993; Beyerlein, 2004; Berman, 2000; Cochran and Beeghley, 1991). Of course, some religious congregations may not qualify as "strict" churches in this sense and the model has limited predictive power for these more moderate organizations.[10] The empirical work below will thus attempt to distinguish between strict and moderate religion. The following section describes the empirical tests to be conducted in more detail.

3. Empirical Tests

The prior section presented a theory of religious participation where religious groups might

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stigmatize members, that strict congregations will have higher participation (Iannaccone, 1992), that strict congregations will be smaller & attract those with more limited outside options (e.g., lower wages, schooling) (Argyle and Beit-Hallahmi 1975, Berman, 2000), and that conversion will be more common/radical for strict congregations (Levine, 1984; Roof and McKinney 1987).

[9] The third step in the original 12 step AA program for alcoholism was to make "a decision to turn our will and our lives over to the care of God as we [understand] Him." Alcoholics Anonymous was co-founded by William Wilson in Akron, Ohio, in 1935 following a profound religious experience; Wilson found that his religious devotion was central to his ability to forgo drinking—he substituted one for the other. See chapters 1 and 3 of Wilson (1955) for more on the role of religion in Alcoholics Anonymous. In addition to the "alcoholics anonymous" analogy, readers have also offered other analogies for causation in the model here. One is the story of the "preacher's daughter," who after being raised in a strict religious upbringing engages in risky behavior upon leaving home. Another is the Amish tradition of "Rumspringa", wherein adolescents may choose to leave the Amish tradition and subsequently engage in illegal or unhealthy activities, as depicted in the film *the Devil's Playground*. The extent to which these stories meaningfully reflect the model is debatable, but they may provide some intuition for the strong substitutability observed among the highly religious that drives the main result.

[10] Some work has tried to extend this model to "moderate" religious groups, cf. Makowski (2010).

profitably alter secular incentives when members view religious and secular consumption as substitutes. Empirical evidence of substitutability can be taken as evidence against a story where highly religious individuals simply have no interest in the secular consumption which has been subject to proscription. In this section I outline a number of tests of substitutability between religious and secular consumption among the highly religious.

In what follows I identify highly religious individuals using two separate criteria. First, I will consider religious groups that prior research has identified as strict. Second, I will consider the behavior of individuals who report high ex ante measures of religious participation—in particular, high attendance—separately from individuals with low or moderate attendance. Prior work has shown that both group strictness and high religious attendance are strongly associated with the characteristics of the model (Dynes, 1955; Harrison and Lazerwitz, 1982; Hoge and Yang, 1994; Iannaccone, 1988, 1992, 1998) and so the behavior of highly religious individuals as defined either by group strictness or prior attendance will be suitable for the empirical tests.

The model shows that substitutability here is defined by changes in religious practice in response to changes in incentives for secular consumption. In what follows I consider three different sources of variation in incentives: blue laws (that impact incentives to engage in secular activities during the Sabbath), casino openings (that impact incentives to gamble), and minimum-legal-drinking-age laws (that affect incentives for underage drinking). I discuss each source of variation below:

3A. Blue Laws

This section provides a brief overview on the history of blue laws in the United States, additional discussion can be found in Cohen-Zada and Sander (2009); Hungerman (forthcoming); Geber, Gruber, and Hungerman (2010); Goos (2005); Laband and Heinbuch (1987); and especially Gruber and Hungerman (2008).

Blue laws, or Sunday closing laws, refer to laws which restrict certain activities on the Sabbath. The origin of the term "blue laws" is disputed. Laws banning general retail activity—for instance, by prohibiting "labor" or "all manner of public selling"—on Sundays were widespread as of the middle of

the 20th century.[11] In 1961 the Supreme Court issued a number of decisions on the constitutionality of blue laws. The decision for the case *McGowan v. Maryland* upheld the constitutionality of blue laws, but in so doing stated that blue laws could be found unconstitutional if their classification of prohibited activities rested "on grounds wholly irrelevant to the achievement of the State's objective." Blue laws were subsequently challenged on the basis that they did not satisfy this constitutional test (Theuman, 2005). These challenges were successful on a number of occasions because blue laws were sometimes confusing in their classification of prohibited activities. For example, in New Jersey some counties allowed the sale of disposable diapers, but washable diapers were prohibited (King, 1976).

As blue laws are repealed, the cost of secular labor and leisure activities declines. Whether religious individuals (in particular, Christians, who make up the vast majority of the religious in the United States) are responsive to this change in incentives depends upon how substitutable these labor and leisure opportunities are with traditional religious worship. Prior work (e.g., Cohen-Zada and Sander; 2009; and Gerber, Gruber, and Hungerman 2010) has shown that the repeal of blue laws was associated with a decline in religious attendance and giving. Conventional wisdom might suggest that this affect was driven by marginal members with modest levels of religious participation to begin with. The new and counter-intuitive prediction here is that the *marginal* members substituting traditional worship with labor and leisure could be the members with the *strictest and strongest* religious devotion initially. Evidence that the most religious members view religious and secular activities as substitutes would be evidence that strict religious groups in this context do significantly alter member behavior. Alternately, evidence that highly religious individuals are unresponsive to blue laws would be evidence that highly religious individuals are correlational members.

Following prior work, I will examine the effect of blue laws using the sample of states that saw a discreet and significant change in the prohibition of retail activity on Sundays, or states that never had

[11] States sometimes exempted certain types of retail activity, for example allowing acts of charity or allowing pharmacies to stay open. Blue laws prohibiting specific types of activities, such as barbering or the sale of alcohol were also common but as with almost all prior research on blue laws the focus here is on regulation of general commerce.

blue laws. There are 24 such states; they are listed below Panel A of Table 1.[12] This group of 24 states makes up a reasonably diverse group, with variation in the timing of laws and in the locations of the states themselves.

A concern with empirical work relating the effect of blue-laws' repeal to religiosity is that this work may be driven by underlying trends in religious behavior.[13] Fortunately, most prior work on blue laws has established that the repeal of these laws does not seem correlated with underlying trends in religiosity or other observables in a way that might confound the analysis. Price and Yandle (1987) examine what economic and social factors are associated with the presence of state blue laws, including the political makeup of a state, the fraction of women in the workforce, the strength of labor unions, and other state socioeconomic characteristics; they found that none of their covariates were consistently associated with the presence of blue laws. Gerber, Gruber, and Hungerman (2010) and especially Gruber and Hungerman (2008) consider underlying trends and reverse causation in depth and find that blue laws' repeal seems unrelated to preexisting religious trends. However, the analysis below will also consider this issue by testing the specifications' sensitivity to various strong controls for trends. Further, the regressions using data from the National Longitudinal Survey of Youth will allow for a number of very demanding specifications including ones with year-by-state dummies and individual fixed effects. Fortunately, the findings here and in most past work suggest that blue laws are not driven by trends in religiosity.

3B. Casino Openings

The second source of secular variation used will be casino openings. Nevada was the only state with casinos up to 1978. Since then, and especially since the late 1980s, there has been a dramatic growth

[12] The most common reason a state is excluded from the analysis is that the state's laws were (or are) decided at the county or city level, making these states unusable with our state-level datasets. A few states were not used because the exact time that the laws were repealed is unverifiable. See Gruber and Hungerman (2008) for more detail on the selection of states. Dropping the 8 states that never had blue laws does not affect the results below.

[13] Also a concern is that blue laws may not have been enforced before to their repeal. Lack of enforcement will bias the empirical results towards zero, and prior work has shown that this is not a concern. Further, anecdotal evidence for a number of states indicates the significance of changes in the laws (a sample includes McGee, 1991; Reinhold, 1985; The New York Times, 1970; and the Associated Press, 1984).

in the number of casinos in the United States. As of 1999, 28 states had legalized casinos (NGISC, 1999). In 1998, the majority of all legal gambling revenue in the United States was from casinos (Barron, Staten, and Wilshusen, 2002)[14] and in 1999 nearly one third of Americans reported having gambled at a casino in the past year (NGISC, 1999).[15]

A 1987 Supreme Court decision, *California* v. *Cabazon Band of Mission Indians*, played a major role in the growth of casinos.[16] Indian tribes are sovereign nations, and the Supreme Court ruling confirmed that states lacked the power to regulate commercial gambling on Indian reservations. The federal government responded to this ruling by clarifying the regulatory framework for casinos with the Indian Gaming Regulatory Act (IGRA) in 1988. This act divided gaming into three classes: Class I, social games "solely for prizes of minimal value" or traditional Indian games identified with ceremonies and celebrations; Class II, bingo and "other games similar to bingo;" and Class III, "all forms of gaming that are not Class I gaming or Class II gaming," such as blackjack, slot machines, roulette, or other casino games.[17] Figure 4 shows the extraordinary growth in the number of counties with a Class III gambling establishment following this ruing; the figure shows a surge in the prevalence of these establishments throughout the 1990s. Figure 4 also shows a slight delay in this rise following the passage of the IGRA. This may reflect the fact that the IGRA stated that Class III gaming was allowed on reservations only if gaming had been agreed upon by a tribal state compact. There were in some cases delays as states and tribes established such compacts.[18] Further, tribes can only offer Class III games when states allow these games elsewhere, meaning not all states saw growth in casinos over this period. In some situations, such as in the state of Connecticut, whether a state allowed gaming elsewhere was a point of dispute leading to lengthy court cases. The *Cabazon* ruling and IGRA act thus led to considerable casino growth, but this

[14] The term "gambling revenue" refers to gross dollars wagered minus dollars paid out in winnings.
[15] See Figure 1-2 in the NGISC report for reported gambling habits in the past year by selected types of gambling games.
[16] See Evans and Topoleski (2002) for an excellent discussion of this decision and the circumstances surrounding the case.
[17] 25 U.S.C. 2701-2721; see § 2703 for these definitions.
[18] In a few instances, notably California, some tribes established gaming before a compact was actually completed. The IGRA placed the burden on states to bargain in good faith and stated that tribes could sue the state for not bargaining in good faith, although this provision of the law has been challenged in court.

growth was idiosyncratic across states and time.

Many major religious traditions advocate against the dangers of gambling, and have expressed concern about this recent increase.[19] Research has also shown that religious individuals gamble less than others and that those attending religious services most frequently are much less likely to gamble heavily (cf. Diaz, 2000; Lam, 2006; Hoffman, 2000). The model in section 2 suggests that despite the low level of gambling among the religious, it could be that highly religious individuals would be willing to substitute gambling for religious consumption if the availability of gambling changed. The empirical work below will thus investigate whether the diffusion of casinos within a state lowers the religiosity of initially religious individuals. As with blue laws, work in the following section will also consider whether casino openings are predicted by underlying religious trends. Other work, considering non-religious outcomes, has found that casino openings have important impacts on surrounding areas: Grinols and Mustard (2006) show that casinos may be associated with higher crime rates, Evans and Topoleski (2002) and Wenz (2008) find that casino openings may increase employment, and Anderson (2009) finds that casino openings lower assistance income among low-educated female householders.

The data for casino availability here was provided by Earl Grinols and David Mustard, and was used in Grinols and Mustard (2006). The data report for every county in the United States whether a Class III casino is in operation and when the first Class III casino opened. The data used in their paper covered the period 1977 to 1996; for this study the authors provided data for two additional years, so the study here covers 1977 to 1998. Their data includes tribal casinos as well as riverboat casinos, other land-based casinos, and "boats to nowhere" that travel outside of U.S. boundaries so passengers can gamble.[20]

[19] For example, the regressions below consider the response to casinos from 4 denominations: the Lutheran Church Missouri Synod, the Southern Baptist Convention, the United Methodist Church, and the United Church of Christ. At least three of these denominations have made explicit statements expressing concern over the rise of gambling following the *Cabazon* decision. The Office of the President of the Lutheran Church Missouri Synod (1999) issued a statement asking congregation lay leaders to communicate "the profoundly harmful impact that state-sponsored and legalized gambling is having on our nation and on our communities," the Southern Baptist Convention passed a resolution (1996) stating the church's "frequent opposition to the gambling industry and the devastating effects of gambling upon the moral and economic life of our nation;" the United Methodist Church (2004) has stated that "gambling is a menace to society" and "deadly to the best interests of moral, social, economic, and spiritual life."

[20] The term "riverboat" casino refers to a boat that "is capable of self-contained operation away from land whether or not it ever leaves the dock," see chapter 2 of NGISC (1999), for more background on this type of casino.

If a Class II casino converts to a Class III casino, this is captured in the data as a new Class III casino. Evans and Topoleski (2002) show that there is considerable variation in the size of Class III casinos, with the median tribal casino having 450 slots and 27,000 square feet; while the biggest tribal casinos include some of the world's largest casinos, such as Foxwoods in Connecticut, with 5,700 slots and 315,000 square feet.

3C. Minimum Legal Drinking Age Laws

A third source of variation in the cost of secular activities will be the Minimum Legal Drinking Age, or MLDA. Following the passage of the 21st amendment in 1933, most states imposed a 21-year age minimum for the purchase and possession of alcohol (Rooney and Schwartz, 1977). In the early 1970s, however, over half of all states lowered the minimum age for the purchase of alcoholic beverages. A number of research studies suggested that these changes led to an increase in fatal car accidents,[21] and in the late 1970s some states began to re-raise the MLDA (Du Mouchel, Williams, and Zador, 1987). In 1984, the federal government enacted the Uniform Drinking Age Act, which provided for the withholding of 5 percent of highway aid from states not having a minimum alcohol purchase age of 21 for all alcoholic beverages by October 1, 1986; 10 percent of funds would be withheld from states not having a minimum purchase age of 21 in 1987. By 1988, all states had established an MLDA of age 21 (Wagenaar and Toomey, 2002).

A large amount of research has explored whether these laws impacted youth drinking and youth health outcomes. While some studies are inconclusive, Wagenaar and Toomey (2002) exhaustively review prior work and conclude that "compared with a wide range of other programs and efforts to reduce drinking among teenagers, increasing the legal age for purchase and consumption of alcohol to 21 appears to have been the most successful effort to date." Weschsler, Lee, Nelson and Kuo (2002) also conclude that "the minimum legal drinking age (MLDA) law may be the single most effective method to

[21] For instance, Williams et al., (1975), Cook and Tauchen (1984), and Smart and Goodstadt (1977)

combat alcohol use and its adverse consequences among young people."[22] Carpenter and Dobkin (2009) use a regression discontinuity analysis to show that MLDA laws reduce drinking by 11 to 21 percent, a large effect.

Work has shown that highly religious individuals are less likely to drink heavily than others, and that religious affiliation is a predictor against alcohol abuse among adolescents (cf. Johnson, Tompkins, and Webb, 2002, and see below). The model from section 2 indicates that this empirical relationship may be driven by religious adolescents having a low taste for drinking or because religious adolescents substitute out of drinking in response to religious instruction. Evidence that religious youth are highly responsive to minimum legal drinking age laws would be evidence for the latter scenario, where religious youth are willing to substitute into and out of heavy drinking.

For this empirical test, data on MLDA laws were taken from O'Malley and Wagenaar (1991) and verified against other sources, including Du Mouchel, Williams, and Zador (1987). The laws are for the minimum legal purchase age for any alcoholic beverage. The data used here account for the fact that some states included "grandfather clauses" in their laws, so that if the MLDA was raised to 21, an individual just under age 21 who had already reached the prior legal drinking age would maintain the ability to purchase alcohol. The main data for testing these laws (described more in the next section) will be the 1979 National Longitudinal Survey of Youth, a survey of individuals 14 to 22 in 1979. The NLSY's time period and sample are fortuitous given that the early 1980s saw a large number of MLDA law changes and many of these changes affected cohorts in the NLSY sample. Furthermore, MLDA laws affect some individuals in a given state and year but not others; this plus the panel nature of the NLSY will allow for very strong specifications to control for any omitted phenomenon that might be associated with changes in MLDAs.

[22] Other methods of deterrence might include greater enforcement or taxation. Grossman, Chaloupka, Saffer and Laixuthai (1994) provide an interesting discussion of the differing economic implications of these various deterrence methods, although they state that increases in the legal drinking age have been "the major element of programs to deter adolescent alcohol abuse."

To summarize, the work below will pursue three different sources of variation in incentives for secular activity: blue laws, casino openings, and minimum drinking age laws. All of these sources of variation affect activities that prior work suggests might (or might not) substitute for religious behavior, and thus all will be useful testing grounds for substitutability. In all three cases the variation in incentives is largely driven by state legislation, federal court rulings, federal legislation, or all three. Furthermore, the three tests are varied in their time periods of secular change (from 1955 to 1991 for blue laws, the 1990s for casinos and the early 1980s for MLDAs), varied in the populations and states affected, and varied in whether they made secular activities more attractive or less attractive. Consistent evidence across all three tests would thus provide extremely compelling evidence for or against the ability of religious practice to change the behavior of its members.

4. Results

This section presents evidence of whether religious individuals respond to secular incentives. The variation in secular incentives will come from blue laws, casino openings, and the minimum-legal-drinking-age. "Religious Individuals" will be identified by denominational affiliation and prior religious attendance. The outcomes used for testing responsiveness will include religious spending, religious attendance, and heavy drinking. The results are organized by outcome; the first subsection presents specifications and results for testing the model by using data on religious spending

4A. Results from Religious Spending

This subsection reports results from religious spending, a suitable measure of congregations' financial resources and one strongly related to other measures such as donations (Hungerman,2008). . The data on religious spending are the same as in Gruber and Hungerman (2008) and are taken from denominational yearbooks for the second half of the twentieth century. Gruber and Hungerman took data from any large denomination providing annual information at the state level (or a finer level that could be converted up to state level data); this yielded four large denominations with usable data: the Lutheran Church Missouri Synod, or LCMS (this was the 7th largest denomination in the country in 1971, near the

midpoint of the sample); the Southern Baptist Convention, or SBC (2nd), the United Church of Christ, or UCC (9th), and the United Methodist Church, or UMC (3rd).[23]

These four denominations are potentially not representative of all churchgoers. However, they are all among the largest denominations in the country, and are all fairly widespread. Panels A and B of Table 1 show the years and states available for each denomination in these regressions. There is wide coverage over place and time. Utah is the only state repealing a blue law that is unavailable in the regressions and the regressions on casino openings include 45 states (missing states are listed under panel B). For most states there are multiple observations. One cautionary note is that data for the LCMS denomination are only available up through 1991, which is just before the strong growth in casinos. Data from the LCMS should be interpreted with caution when examining the casino regressions.[24]

These denominations also vary in their strictness. McBride (2007), Iannaccone (1994), and Hoge (1979) all discuss the relative strictness of these denominations and all present evidence that the Southern Baptist Convention is the strictest, followed by the Lutheran Church Missouri Synod, the United Methodist Church, and the United Church of Christ. Hoge's study is based on a questionnaire sent to a group of experts including church historians, sociologists of religion, denominational and ecumenical leaders, and seminary educators.[25] Iannaccone (1994) argues that one question in the survey is especially relevant to ranking denominations in the context of the model; this question asked experts to rank denominations based on "whether the denomination emphasized a distinctive life-style or morality involving such matters as dress, diet, drinking, entertainment, use of time, marriage, sex, childrearing, and the like;" the experts ranked the SBC, LCMS, UMC, and UCC from most to least strict in that order. Furthermore, experts consistently ranked the SBC as one of the strictest large denominations in America,

[23] Data on denomination size come from the Glenmary Research Center (1974).
[24] There is at least some variation in the LCMS data, however. New Jersey has casinos starting in 1978 and by the final year in the LCMS data, 1991, there are 7 LCMS states with at least one casino.
[25] See endnote 4 in his paper for a complete list.

and the UCC as one of the least strict.[26] The four denominations here thus represent a broad spectrum of strictness among large denominations.

The regressions will investigate whether church resources, as measured by total spending, change in response to blue laws and casino openings.[27] For blue laws the baseline regression is:

$$lspend_{dsy} = \gamma strict_d * repeal_{sy} + \delta repeal_{sy} + \beta X_{sy} + \phi_{dy} + \theta_{ds} + \varepsilon_{dsy}, \quad (7)$$

where, for denomination *d* in state *s* in year *y*, the variable *lspend* is the log of total per member spending by a church, *repeal* is a dummy that equals unity if a state has repealed its blue laws, *strict* is an index that ranks denominations by strictness (0 for UCC, 1 for UMC, 2 for LCMS, and 3 for SBC), *X* is a set of controls, ϕ_{dy} is a set of denomination-by-year dummies, and θ_{ds} is a set of denomination-by-state dummies. The controls in *X* include the rate of insured unemployment, the fraction of the population foreign born, per capita disposable income, the percent of the population black, the percent of the population under age 5, ages 6-18, ages 45-64, and over 64. The set of dummies in ϕ_{dy}, which would subsume a simple set of year or denomination dummies, allow each denomination's spending to follow its own nonlinear trend over time. (These dummies would also subsume a noninteracted strictness index.) The set of dummies in θ_{ds} allow for different levels of spending across states for each denomination. The unit of observation is all churches within a denomination, in a given state and year.

The coefficients of interest in the above regression are δ and γ. The coefficient δ shows the effect of blue laws' repeal for a denomination whose strictness score is zero (i.e., the UCC denomination). The coefficient γ shows how moving one point up the scale alters the impact of blue laws on spending. Thus, for the strictest denomination (the SBC), the impact of blue laws can be evaluated by plugging in

[26] Among very large denominations, the only one ranked as more strict than the SBC was the Church of Latter Day Saints, and the only one ranked less strict than the UCC was the Episcopal Church.

[27] Since the data here cannot be broken down by churchgoer age, and since MLDA laws directly affect only a relatively small fraction of younger adherents, the tests with these spending data will focus on blue laws and casino openings.

the SBC's value on the strictness scale and using the two coefficients: $3\gamma + \delta$. In what follows some specifications will relax the assumption that strictness interacts with blue laws in a linear fashion.

For the regressions on casinos, the above specification will be slightly altered to estimate

$$lspend_{dsy} = \gamma strict_d * casinos_{sy} + \delta casinos_{sy} + \beta X_{sy} + \phi_{dy} + \theta_{ds} + \varepsilon_{dsy}, \quad (8)$$

where now the variable *casinos* represents the number of counties in a state operating a Class III gaming establishment. The samples used for estimating equations (7) and (8) are given in panels A and B of Table 1.

Table 2 reports estimates for equations (7) and (8). Robust standard errors, clustered by state are reported in brackets. Regressions weight observations by church membership and all monetary figures are in year 2000 dollars. Panel A reports regression results from the blue laws regressions. The noninteracted repeal dummy coefficient is positive, small, and statistically insignificant. This coefficient shows that blue laws repeal has little impact on the least conservative denomination, the UCC. For the UMC denomination, the strictness index equals unity and so the effect of the blue laws repeal is the sum of the coefficients, giving a coefficient of about -0.03. This suggests that for the UMC denomination, a repeal of blue laws leads to fall in church spending of about 3 percent. A Wald test shows that the sum of these two coefficients is not statistically different from zero, with the test-statistic having a *p* value of 0.5.

The fact that the coefficient on the interacted term is negative suggests that the impact of blue laws is greater for the more conservative denominations. Blue laws are estimated to have lowered LCMS spending by about 6 percent and SBC spending by a larger 9 percent, and the Wald tests show that these effects *are* statistically different from zero. The fact that the coefficient on the interacted variable is insignificant indicates that the effect of blue laws does not significantly differ between any two congregations one-digit apart on the strictness index, but the regressions nonetheless show that blue laws have a significant negative effect on the *strictest* denominations and a wrong-signed, insignificant effect on the least strict denomination.

The set of denomination-by-year and denomination-by-state dummies in these regressions is very strong, but one can further investigate whether underlying trends are affecting the results by including state specific time trends. The second column of panel A shows that the results are similar to before with the inclusion of these trends. Column 3 uses an even more demanding denomination-by-state specific trend, and the results are again unaffected; this strongly suggests that underlying trends across denomination or place are not driving these results. The estimates from column 3 show that the effect on spending of blue laws ranges from almost nothing for the UCC to -0.0188*3 - 0.007 = -0.063 (or a 6 percent decline) for the SBC denomination, a reasonably large effect.

The results from panel B on casino openings tell the same story: the decline in religious spending is *largest* for the *strictest* denominations, and the results using trends show that the effect of casino openings on strict denominations is statistically different from zero. Looking at the strongest specification in column 3, the results suggest that when a county opens a casino, UCC spending in the state increases by an insignificant 0.2 percent, while SBC spending *falls* by about one percent, and this decline is significantly different from zero. The results from table 2 provide evidence that the strict denominations see falls in religious activity when secular opportunities increase—evidence of substitution and evidence against a correlation story.

Table 3 provides an additional test of the blue laws spending regressions. The first column reports the basic regression from panel A of Table 1, but now the repeal dummy is interacted with a dummy variable for each denomination; this provides a unique estimate of the effect of repeal for each denomination without the linearity assumption imposed by the strictness index. Columns 2 and 3 add in the time trends from before. Clearly, the regressions show that the response of blue laws is strong for the strictest denominations and weaker for the least strict. The results in column 3, from the strongest specification, strikingly show that the effect of blue laws gets progressively stronger as the denomination gets stricter. Once again, strict denominations, rather than having members unresponsive to changes in secular incentives, have members who are very responsive.

Table 4 repeats the specifications of Table 3 for the regressions on casinos. Once again, the results show that the strictest denominations see declines in spending when casinos open. The strongest specification in column 3 shows again a pattern where an increase in strictness is associated with a greater response to casino openings.

The overall result from Tables 2, 3, and 4 is that denominations respond to changes in secular incentives and that these responses are strong among the adherents of the strictest congregations. This fits with the scenario depicted in the model wherein individuals view strict religious adherence and secular opportunities as substitutes—and, as the model demonstrates, where individuals may thus significantly alter their behavior in the face of religious instruction. The next subsection considers the effects of blue laws and casino spending on a different outcome: religious attendance.

4B. Results from Religious Attendance

This subsection considers how changes in secular incentives affect religious attendance. The attendance data will be taken from 1973-1998 waves of the General Social Survey, the longest-running nationally representative survey that regularly asks questions on religious attendance. Unlike the data used in the prior section, here the data include individuals from a national sample and will reflect a variety of religious traditions. The GSS is a (roughly) biennial survey that asks individuals how often they have attended religious worship in the prior year. Responses are broken down into three categories: high attendance (weekly or more), low attendance (attendance in the past year of less than weekly and more than never), and no attendance reported in the past year (these categories will make the GSS results similar to the NLSY results reported in the following subsection). The GSS is a repeated cross section, not a panel or longitudinal study.

As with spending, the tests here will examine how attendance responds to blue laws and casino openings. The specifications to test the effects of blue laws is:

$$atd_{isy} = \delta * repeal_{sy} + \beta X_{isy} + \theta_s + \varphi_y + \varepsilon_{isy} \quad (9)$$

where for individual *i*, in state *s* and year *y*, the variable *atd* represents a measure of religious attendance, the variable *repeal* is a dummy for whether a state has repealed its blue laws, *X* is a set of controls, θ_s is a set of state dummies, and φ_y is a set of year dummies. The controls in *X* include regressors for respondent's gender, race, educational status, marital status, dummies for age, the percent of individuals in the respondent's state black , the percent foreign born, the rate of insured unemployment, and per capita disposable income.

Estimates of (9) will be reported for three different dependent variables. First, the dependent variable will equal unity if an individual reports *high* attendance—weekly attendance or more—and equal zero otherwise. Second, the dependent variable will be a dummy for *low* attendance, the variable will equal unity of a respondent reports attendance in the past year that is less than weekly but more than never (and the variable will equal zero otherwise). Third, the dependent variable will be a dummy for *no* attendance, the variable will equal unity if a respondent reports no attendance (and equal zero otherwise).

The variable δ will show how the repeal of blue laws impacts the likelihood that an individual reports being in any given attendance category. If the coefficient δ is negative when the dependant variable is a dummy for high attendance, this would indicate that blue laws' repeal lowers the likelihood that individuals report high attendance and would be consistent with highly religious individuals substituting religious consumption with secular opportunities. An insignificant or positive δ coefficient would be evidence against this scenario. Note that the three dependent variables include all possible categories, so that the three δ coefficients estimated from each variable must sum up to unity—a fall in the likelihood of one level of attendance must be compensated by an increase in the likelihood of some other level of attendance. The GSS regressions on blue laws will limit the sample to Catholics and Protestants (those who worship on Sunday and presumably are affected by the laws) and again will use the states listed under Panel A of Table 1. Panel C of Table 1 shows selected means from the GSS, about 30 percent of respondents attend weekly, about 57 percent between weekly and never, and about 14 percent report never attending.

The regressions on casino openings will be similar to the blue laws regressions, except that here the sample can include all individuals in all states:

$$atd_{isy} = \delta * casinos_{sy} + \beta X_{isy} + \theta_s + \varphi_y + \varepsilon_{isy} \quad (10)$$

where all variable are as in equation (9) except for the key regressor *casinos*, which will equal the number of counties with a Class III casino operating in the state.

The results from linear probability estimates of equations (9) and (10) are given in Table 5. Again, standard errors are robust and clustered by state. Panel A reports estimates from the blue laws specification. The first column shows the estimated coefficient for δ when the dependent variable is a dummy for high (weekly or more) attendance. The coefficient is negative and significant, showing that there is a fall in weekly or greater attendance when secular opportunities increase on Sunday. The estimated 4.5 percentage point decline in the likelihood of weekly attendance is modest but nontrivial. The coefficients in columns 2 and 3 show that this fall is compensated by an increase in low attendance and (to a lesser extent) no attendance. The next trio of columns includes state specific trends to again explore the possibility that trends in attendance are confounding the estimates. Inclusion of these trends makes the result *stronger*. Once again, the results clearly suggest that high levels of religious participation are affected by secular incentives.

The second panel of Table 5 shows results from the casino regressions. While the baseline coefficient on weekly attendance here is negative, it is small and clearly insignificant, as are the low attendance and no attendance regressions. Adding trends produces qualitatively similar estimates. Overall, the estimates suggest that casino openings do not impact religious attendance.

The insignificant effects from panel B stand in contrast to the results in Tables 2, 3, 4 and panel A of Table 5. This might partly be because highly religious individuals do not view casino gaming and religious attendance as substitutes (although the earlier results suggest that they *do* view gaming and religious spending as substitutes). This could be an instance of type II error, as discussed earlier. Alternately, the problem here could be one of measurement error in the independent variable *casinos*.

Grinols and Mustard (2006) show that casinos have important impacts on their own communities, but have weaker or negligible effects on neighboring counties. If the individuals in the GSS are largely located in communities without casinos, it could follow that the aggregate data used in the spending regressions captures the local effects of casinos but that the GSS data largely miss these effects. From 1993 on, only about 10 percent of GSS respondents live in a county with a casino (the fraction living near a casino cannot be calculated for prior years because county of residence information is not available before 1993, but the fraction would surely be much smaller in earlier years since casino prevalence was much lower in the 1970s and 1980s). If the sample size of the GSS were large enough, it might be possible to find an effect using regressions based on county of residence to identify exposure to casinos, rather than state of residence. Unfortunately, county information is only available for a few years (1994, 1996, 1998, and some of 1993), with the number of respondents in any available year reporting that they live in a county with a casino ranging from a few dozen to a few hundred. The local effect of casinos combined with the small sample size of the GSS may thus mask the impact of casinos on religious attendance.

Overall, the results from Table 5 show that religious attendance is responsive to changes in labor and leisure opportunities on Sundays, and that it is *high* levels of attendance that drop in response to increased secular opportunities. As with the results on church spending, this is consistent with a scenario where strict religious members view secular and religious opportunities as substitutes. However, the results on casinos and church attendance are highly imprecise. The next section presents a final set of tests, based on heavy drinking data.

4C.Results from Heavy-Drinking Data

This subsection presents tests from a final outcome: heavy drinking. The prior sections established that, at least in some settings, highly religious individuals substitute religious and secular activities by lowering religious consumption when secular opportunities increase. This section will provide further evidence that risky *secular* consumption increases as religious consumption falls, again indicating that the low levels of risky behavior among the religious are not simply a results of correlation.

Data on heavy drinking will be taken from the 1979 National Longitudinal Survey of Youth, or NLSY. The NLSY is a nationally representative survey begun in 1979 with a sample of 12,686 respondents aged 14 to 22. The initial wave of the NLSY asked respondents about their religious participation, and subsequent waves asked individuals about their alcohol consumption. The results here are from the 1983, 1984, 1985, 1988, 1989, and 1994 waves, which all included questions on alcohol consumption.

The 1979 wave of the NLSY included a question on the frequency of attendance, and answers were coded into a set of categories. Individuals will be grouped into categories comparable with those used in section 4B: those reporting weekly or greater religious attendance (high attenders), those reporting attendance less than weekly but more than never (low attenders) and those reporting no attendance. Individuals in subsequent waves were asked whether they had consumed six or more alcoholic drinks on one occasion in the past month; this question will serve as the measure of heavy drinking. Panel D of Table 1 shows that about a third of the sample report high attendance, about half report low attendance, and about a sixth report no religious attendance.

Panel D also shows that about a third of respondents report heavy drinking in the past month, although this differs by religious attendance. Rates of heavy drinking are lower for high attenders (29 percent) than for low attenders (36 percent) or non attenders (40 percent). The fact that the strongest attenders have the lowest levels of drinking could indicate that religious participation discourages drinking or that these individuals have low tastes for drinking. Evidence that religious individuals drink more when incentives change would be evidence against the latter scenario. This section considers two changes in incentives: blue laws and the legal drinking age.

The NLSY regressions are based on a panel. Furthermore, since the data is at the individual level, one can compare differences in the behavior of individuals in the same state and year. To exploit these features, the estimates will be based on the following differenced regression:

$$\Delta had6_{isy} = \gamma attend_i * \Delta repeal_{sy} + \beta \Delta X_{isy} + \Delta \varepsilon_{isy}, \quad (11)$$

where for individual *i* in state *s* in year *y* the variable *had* 6 is a dummy for heavy drinking and the variable $\Delta had6$ is the difference between the heavy-drinking dummy from the year in question and the prior interview year for individual *i*, *attend* is a set of three dummy variables for high, low, or no attendance in 1979, $\Delta repeal$ is the difference in the repeal dummy between the current year and the prior interview year, ΔX is a set of controls including change in marital status or educational attainment from the prior interview, the percent of the state that is black, percent foreign born, per-capita disposable income, and the rate of insured unemployment (these state variables are included in levels and in first differences), a set of age dummies, and a set of dummies for years since the prior interview.

The key coefficients of interest in the above regression are in the vector γ, the set of coefficients for the variables interacting initial religiosity with a change in blue laws. (Since our three attendance categories include all respondents, there is no need to include a noninteracted *repeal* variable in this regression as it is subsumed by the interaction terms.) The attendance measures are taken from the initial survey year and are not differenced in the interaction term in (11).[28] Thus the interacted terms here reflect how *changes* in blue laws affect *changes* in drinking behavior, and this relationship is allowed to vary by initial religiosity. As in an individual fixed-effects regression, the first differenced regression will remove any unobserved individual-specific effects (note that this includes all fixed characteristics of the individual, including the noninteracted *attendance* dummies, as well as controls for gender and race), which are differenced out of (11). For the baseline specification, state and year dummies are omitted (of course, their meaning in a first-differenced regression is different than in a levels regression) but regressions including these controls will be considered. Even more strongly, regressions including state-by-year dummies will be considered; this will allow trends in drinking to vary across place and time.

A similar specification will be pursued using data from MLDA laws

$$\Delta had6_{isy} = \gamma attend_i * \Delta overage_{isy} + \beta \Delta X_{isy} + \Delta \varepsilon_{isy}, \quad (12)$$

[28] Note that the initial attendance terms do not vary over time and cannot be differenced. Moreover, the interaction term above provides a straightforward interpretation, as discussed in the text.

where variables are defined as before except that *overage* is a dummy for whether individual *i* is old enough to legally purchase alcohol, and $\Delta overage$ is the difference between this dummy for the year in question and in the previous interview year. Once again, noninteracted controls for $\Delta overage$ and *attend* would be subsumed by the first differencing and the full set of dummies in the vector *attend*. As discussed in section 3, the considerable variation in MLDA laws over this period allow for variation across ages, states, and years to contribute to the identification.[29] State-by-year dummies can also be added to this specification.

The blue laws regression results are reported in Table 6. As before, standard errors are robust and clustered by state. Column 1 reports the baseline specification; the results clearly show that high attendees increase their heavy drinking after a change in blue laws. The estimated increase in the likelihood of heavy drinking is between 8 and 9 percentage points, a reasonably large effect—and one suggesting nontrivial willingness of the initially religious to substitute into drinking. This effect is four times larger than the estimate for low religious attendees, a result which is strikingly nonintuitive but consistent with the model. The effect of blue laws' repeal on non attendees also appears to be large, which may be driven by non religious individuals responding to newly-available labor and leisure opportunities.[30] But the crucial result for testing the model is simply whether highly religious individuals are responsive, and they are. Indeed, even the regression with state-by-year dummies shown in column 3—an extraordinarily strong specification—provides evidence of greater heavy drinking among those who were initially the most religious.

The last column tests whether the results so far are truly picking up an increase in drinking among the most religious. It could be (for instance) that some women are highly religious and some women respond strongly to the laws; the regressions in the first three columns constrain all women to have the

[29] About 3,000 observations in the NLSY regression are from underage respondents, including about 40 percent of 19 year olds and 36 percent of 20 year olds. There are also about 130 respondents in the sample who temporarily *lose* their legal drinking age status when either changes in MLDAs were not grandfathered or when they move from a low MLDA state to a high MLDA state.

[30] Goos (2005), and Gruber and Hungerman (2008) both discuss changes in secular labor and leisure opportunities for the overall population. They find limited evidence that total employment increases after blue laws' repeal, but that there may be shifts in times of employment during the week as stores open on Sunday.

same response and thus the strong result for high religiosity could simply be driven by changes among women. The last column investigates this story by considering new interactions for the baseline regression. The regressions interact a change in the repeal of blue laws with a dummy for a college education, a dummy for gender, and a dummy for marital status. These new interactions do not erase the effect for highly religious; in fact the estimated effect is *stronger* than before. The new interaction terms are all smaller than the coefficients of interest. Further, only the married term is significant; this may reflect that, relative to unmarried individuals, married people respond to new labor and leisure opportunities with relatively less heavy drinking, all else equal.[31] But clearly Table 6 suggests that highly religious individuals are marginal individuals when it comes to incentives to drink.

Table 7 examines whether these results are also found using MLDAs. The first column again shows that highly religious individuals drink more when they reach their MLDA. The 2.4 percentage point increase is a little under a 10 percent effect, a number similar and slightly smaller in magnitude than the effects of MLDA laws found in Carpenter and Dobkin (2009). The next two columns show that finding is preserved with the addition of state dummies and year dummies, and even (in column 3) with the addition of state-by-year dummies.

The final column in Table 7 once again tests the robustness of this result by adding in additional interaction terms. As with blue laws, the interactions terms actually *strengthen* the main result; now highly religious individuals have a 4.5 percentage point increase in heavy drinking upon reaching the legal drinking age. The other interacted coefficients are qualitatively similar to those in the blue laws regressions; with only the married interaction being negative. This suggests that, relative to unmarried individuals, married individuals see a relative decline in heavy drinking upon reaching the age of 21. But as before these results suggest that that initially highly religious individuals *do* increase their drinking, and in particular *heavy* drinking, upon reaching the legal drinking age, and that this increase is economically large and robust.

[31] Gruber and Hungerman (2008) report that a similar coefficient on marriage is not significant across alternate specifications, although the effect of marriage is also found here using MLDA laws.

To summarize, the results of this section present a battery of tests for whether highly religious adherents are willing to substitute between religious and secular consumption; the model in section 2 identifies this as the key metric for differentiating causation from correlation. The tests here measure "highly religious" using initial religious attendance and denominational affiliation, measure substitution by looking at changes in religious attendance, religious spending, and heavy drinking, and use three sources of variation in incentives: blue laws, casino openings, and MLDAs. Thus the behaviors, prices changes, measures of religiosity, datasets, specifications, time periods, populations, and sources of variation are different across these tests. In one setting—examining the response of religious attendance to casinos opening in a person's state of residence—there is no evidence of substitutability. In every other instance, however, the results suggest that highly religious individuals *do* substitute between religious and secular consumption when incentives change. This substitution behavior was robust to various tests and was consistently economically large—in fact in many settings highly religious individuals were more responsive than moderate or nonreligious individuals, a strikingly counterintuitive result. The results overall clearly suggest that religious individuals are in some circumstances willing to substitute secular and religious activities, which indicates that in some settings such as gambling and heavy drinking, the differences between religions and non religious are at least partly causal.

Conclusions

This paper proposes a general methodology for investigating the behavior of religious individuals, and in particular whether the behaviors of religious individuals reflect an impact from the religions themselves or are instead driven by correlations in preferences. The method is based on the canonical model of religion and suggests that causal effects can be identified by the willingness of highly religious individuals to substitute religious and secular consumption. The paper then tests for substitutability in a variety of settings and the results verify that religious adherence does affect member behavior, at least in some cases.

The method developed here requires suitable variation in incentives to undertake secular consumption; an instrument for religion is not required. The method developed here could be applied to

any situation where (a) behavior may be potentially related to some aspect of religious proscription or instruction (b) there is suitable variation in secular incentives to test for substitutability between religious and secular consumption. Examples might include unilateral divorce laws, access to broadband internet pornography, restrictions on abortion or birth control, and "sin taxes" on cigarettes or other merchandise.

Furthermore, while the model is focused on strict groups, they need not be strict *religious* groups. The empirical test developed above could also test for causal differences among those adhering to strict secular groups emphasizing costly behaviors that differentiate members from nonmebmers. Examples might include fraternities, sororities, other social clubs, or terrorist organizations.

The theory here also highlights the difference between *average* participation and *marginal* participation. Counterintuitively—but crucially—the model shows that those with extremely high participation may also be the most marginal; and this is repeatedly confirmed in the data. The model takes group size as fixed; relaxing this feature would likely exacerbate this interesting distinction. Intuitively, religious groups here offer their members a separate technology for consumption, one which provides greater religious consumption and less secular consumption. An optimization problem where individuals not only choose the best bundle within a technology, but choose between technologies themselves, can lead to high instability if a slight alteration of one technology causes individuals to defect. This fact is also consistent with members of high tension groups experiencing radical conversion experiences, as discussed in Iannaccone (1992).

One shortcoming of the test here is that it is subject to type II error. Relatedly, it may be that the behavior of religious individuals is driven in part by the religious institutions but partly by correlations in preferences. While the test here is valuable in that it can potentially rule out a simple "correlation" story, the reduced-form nature of the test does not allow a comparison of the relative value of causation versus correlation. While the results above suggest that the substitution effect for the religious can be quite strong, suggesting a large role for causation, measuring the relative importance of causation is left for the future.

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Appendix

Part 1—Deriving Expression (6) from (5)

First, note that the problem solved by individuals in the Nash Equilibrium here is:

$$\max_{R,S} U(S,R,Q) + \lambda(I - \pi_s S - \pi_r R). \tag{A1}$$

Next, totally differentiating (4) yields

$$\frac{dQ^e}{d\pi_j} = \left(\frac{\partial F}{\partial \bar{R}}\frac{\partial R}{\partial \pi_j}\right) / \left(1 - \frac{\partial F}{\partial \bar{R}}\frac{\partial R}{\partial Q}\right). \tag{A2}$$

Plugging (A2) into (5), noting that $\frac{d\log(U^e)}{d\log(\pi_j)} = \frac{dU^e}{d\pi_j}\frac{\pi_j}{U^e}$, using Roy's Identity, and some algebra show:

$$\frac{d\log(U^e)}{d\log(\pi_j)} = \left(-k_j + k_q \frac{\varepsilon_{Fr}\varepsilon_{r\pi_j}}{1 - \varepsilon_{Fr}\varepsilon_{rq}}\right)\varepsilon_{VI} \tag{A3}$$

where again ε_{ab} is the elasticity of *a* with respect to *b*, k_j denotes the expenditure share of commodity *j*, and $k_q = \left(Q\frac{\partial V}{\partial Q}\right) / \left(I\frac{\partial V}{\partial I}\right)$. The term ε_{VI} is positive; by assumption ε_{Fr} is positive and by stability $1 > \frac{\partial F}{\partial \bar{R}}\frac{\partial R}{\partial Q} = \frac{\partial F}{\partial \bar{R}}\frac{\bar{R}}{F}\frac{\partial R}{\partial Q}\frac{Q}{R} = \varepsilon_{Fr}\varepsilon_{rq}$, where the first equality holds given symmetry and since $F = Q$.

Thus the expressions in (A3) and (5) will be positive if the term in parentheses is positive or equivalently if $(k_q / k_j)\varepsilon_{r\pi_j} > (1 - \varepsilon_{Fr}\varepsilon_{rq}) / \varepsilon_{Fr}$. This inequality matches (6) except for k_q / k_j.

The expression k_q / k_j can be written for $j = s$ as $\left(Q\frac{\partial V}{\partial Q}\right) / \left(S\pi_s \frac{\partial V}{\partial I}\right)$. From the first-order condition for (A1) we have $\frac{\partial U}{\partial S} = \lambda\pi_s = \frac{\partial V}{\partial I}\pi_s$, where the second equality follows from the Envelope Theorem. Letting $\frac{\partial V}{\partial I} = \frac{\partial U}{\partial S}\frac{1}{\pi_s}$,

$$\frac{k_q}{k_j} = \left(Q\frac{\partial V}{\partial Q} \right) / \left(S\frac{\partial U}{\partial S} \right).$$

Plugging this in for k_q / k_j in equilibrium and noting that $\frac{\partial V}{\partial Q} = U_Q^e$ yields expression (6).

Part 2—Heterogeneous Preferences

Heterogeneous preferences affect the model by introducing different levels of religiosity in the religious average term $\overline{R}$, which then affects the derivative of group quality used in (5). However, since the derivative of an average is the same as the average of the component derivatives, this heterogeneity has little impact on the intuition and mainly necessitates notational changes.

Consider a situation where there are two types of atomistic individuals in the club, types "a" and "b". Let the fraction of type-a individuals be θ and consider a Nash Equilibrium that is symmetric within types. [32]

The key impact heterogeneity in types has on equation (5) is through the term $\frac{dQ^e}{d\pi_s}$. With heterogeneity in types, expression (4) becomes:

$$Q^e = F[\theta R_a(I, \pi_s, \pi_r, Q^e) + (1-\theta)R_b(I, \pi_s, \pi_r, Q^e), N] = F[\overline{R}(I, \pi_s, \pi_r, Q^e), N]. \qquad \text{(A4)}$$

Here R_j denotes the equilibrium choice of *R* for a type *j* individual. Totally differentiating (A4) gives:

$$\frac{dQ_i^e}{d\pi_s} = \left(\frac{\partial F}{\partial Q}\frac{\partial \overline{R}}{\partial \pi_s} \right) / \left(1 - \frac{\partial F}{\partial \overline{R}}\frac{\partial \overline{R}}{\partial Q} \right), \qquad \text{(A5)}$$

[32] The case of non-atomistic members is more cumbersome because different members have different perceptions of the equilibrium *Q*, which has no meaningful impact on the intuition and even for small groups is likely unimportant. Recalling that the total number of individuals in the group is $N + 1$, a non-atomistic type-a individual's perception of quality is $Q_a^e = F[(\widehat{\theta}R_a + (1-\widehat{\theta})R_b), N]$ where R_j is the equilibrium *R* for a type *j* individual and $\widehat{\theta} = ((N+1)\theta - 1)/N$ shows for a type-a individual the fraction of the group *excluding himself* that is also type a. A type-b individual views the fraction of those in the group other than herself who are type a as $\tilde{\theta} = \theta(N+1)/N$. The difference between $\widehat{\theta}$ and θ is $(1-\theta)/N$ and the difference between $\tilde{\theta}$ and θ is θ / N; both of which will typically be negligible even for small religious groups. A non-atomistic analysis approximating $\tilde{\theta}$ and $\widehat{\theta}$ with θ is similar to the derivation of (A6) given here.

where $\frac{\partial \overline{R}}{\partial \pi_s} = \theta \frac{\partial R_a}{\partial \pi_s} + (1-\theta)\frac{\partial R_b}{\partial \pi_s}$ and $\frac{\partial \overline{R}}{\partial Q} = \theta \frac{\partial R_a}{\partial Q} + (1-\theta)\frac{\partial R_b}{\partial Q}$.

Using expression (A5) yields a slightly altered version of (A3):

$$\frac{d\log(U^e)}{d\log(\pi_j)} = \left(-k_j + k_q \frac{\overline{\varepsilon}_{Fr}\overline{\varepsilon}_{r\pi_j}}{1-\overline{\varepsilon}_{Fr}\overline{\varepsilon}_{rq}} \right) \varepsilon_{VI}, \tag{A6}$$

where $\overline{\varepsilon}_{Fr}\overline{\varepsilon}_{r\pi_j} = \frac{\partial F}{\partial \overline{R}}\frac{\overline{R}}{F}\frac{\partial \overline{R}}{\partial \pi_j}\frac{\pi_j}{\overline{R}}$ and $\overline{\varepsilon}_{Fr}\overline{\varepsilon}_{rq} = \frac{\partial F}{\partial \overline{R}}\frac{\overline{R}}{F}\frac{\partial \overline{R}}{\partial Q}\frac{Q}{\overline{R}}$. The rest of the derivation of (6) proceeds as before.

Figure 1:
Rough Intuition for the Model

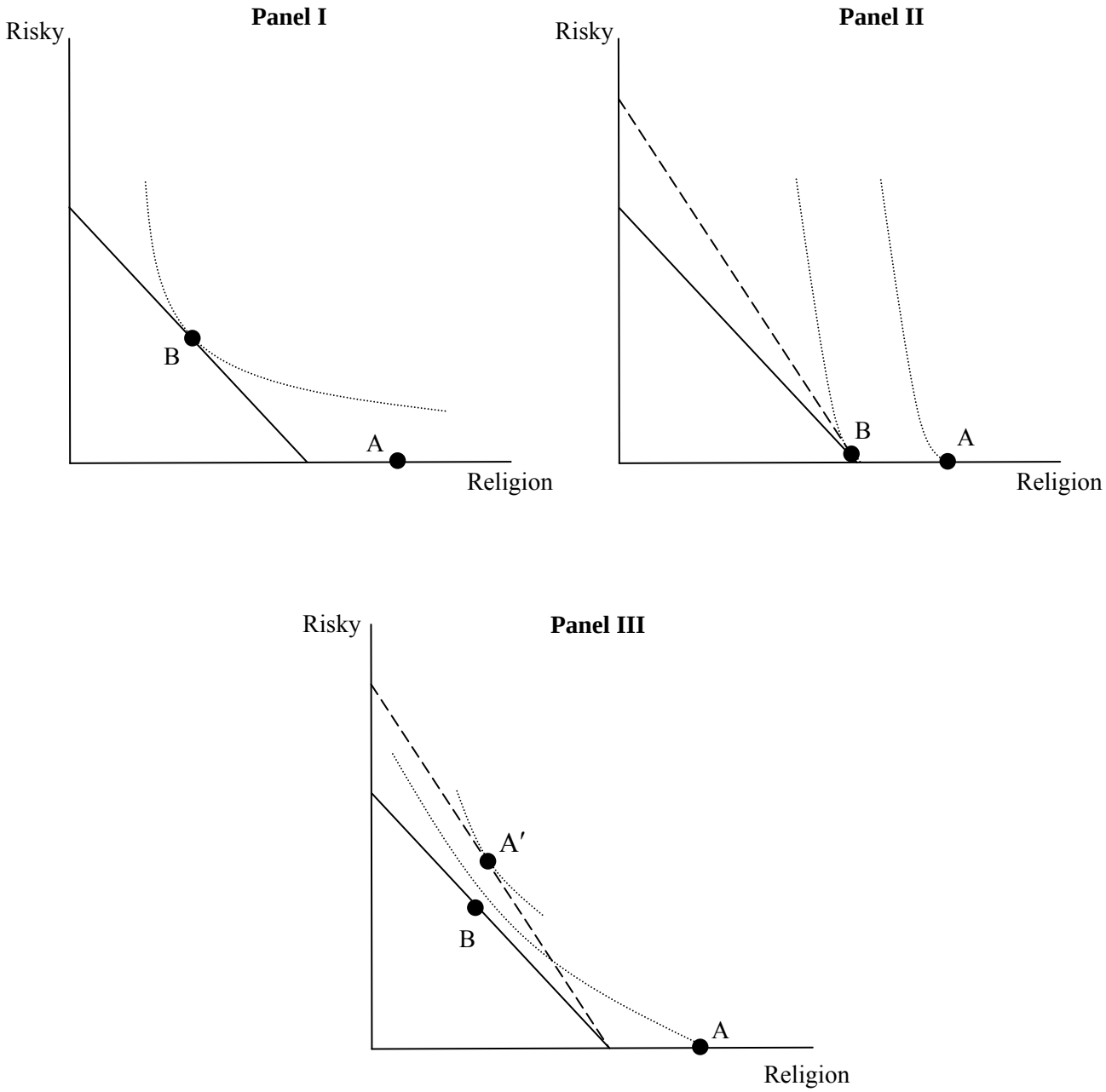
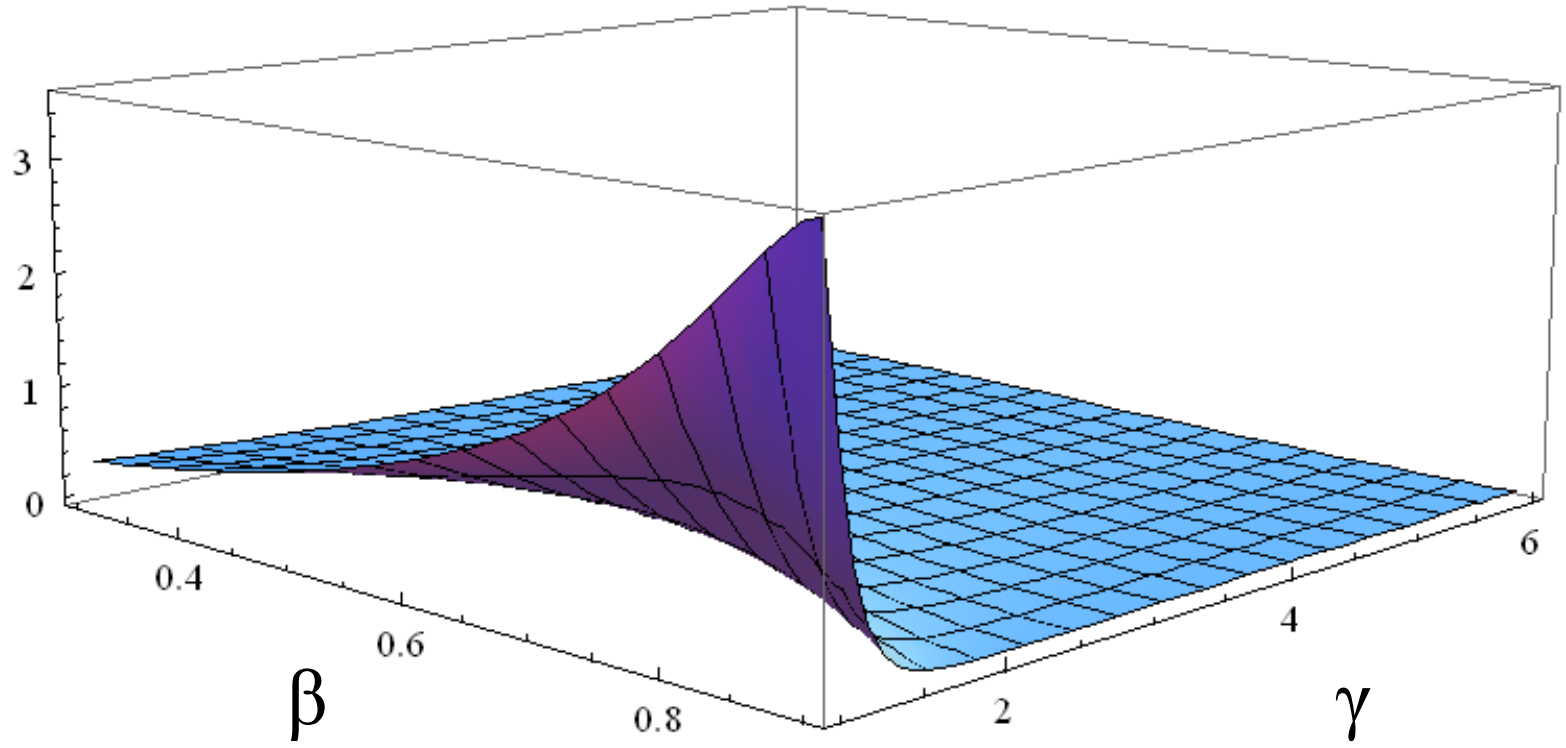
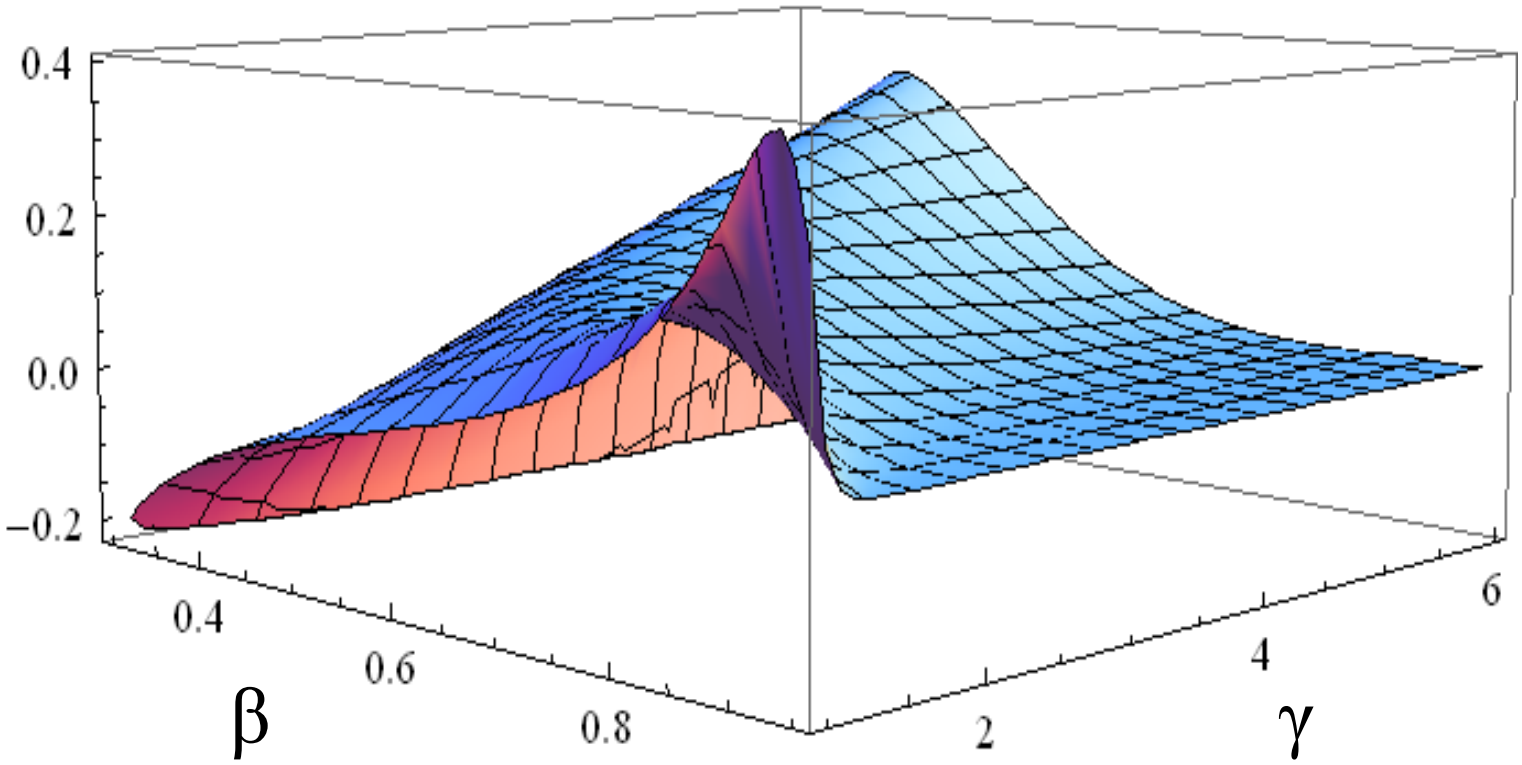


Figure 2: Cross-Price Elasticities in Equilibrium



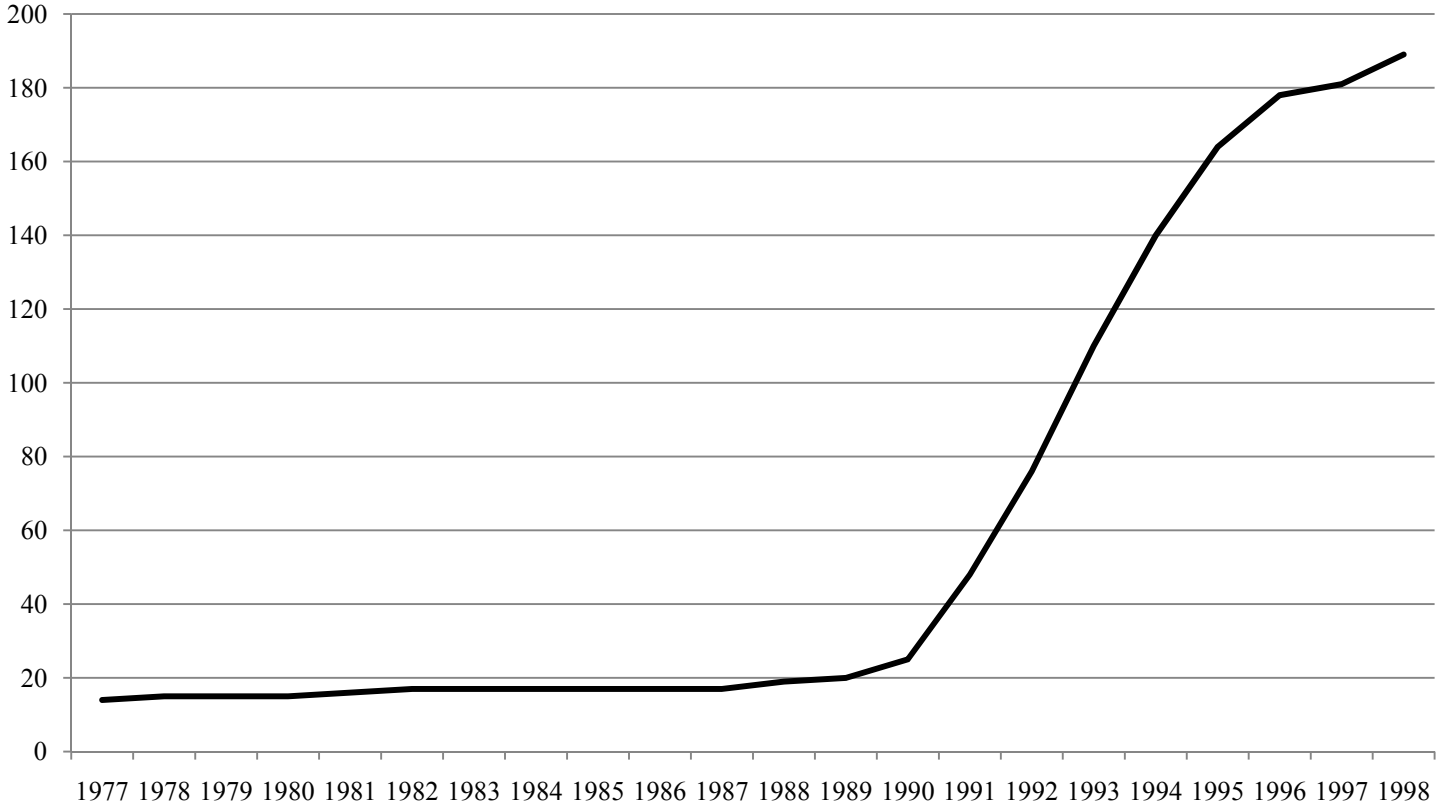
The figure shows the cross-price elasticity of religion with respect to the price of secular consumption, $\varepsilon_{r\pi_s}$, as the parameters β (which determines the elasticity of substitution) and γ (which determines the weight put on religious consumption in the utility function) vary in a symmetric Nash Equilibrium.

Figure 3: The Change in Utility from an Equilibrium Increase of π_S



The figure shows the derivative of the indirect utility function with respect to secular prices, $\frac{\partial V}{\partial \pi_s}$, as the parameters β (which determines the elasticity of substitution) and γ (which determines the weight put on religious consumption in the utility function) vary in a symmetric Nash Equilibrium.

Figure 4: Counties with a Casino



The figure shows the number of counties with a casino each year from 1977 to 1998. Casinos here refer to Class III casinos (see text).

Table 1: Datasets and Selected Descriptive Statistics

Panel A: Church Spending Data for Blue Laws

Denomination	Years Data Available	Blue-Law States Available
Southern Baptist Convention	1968-2000	AZ, CA, CO, FL, IA, IN, KS, NM, NV, OH, SC, TN, TX, VA, WY
Lutheran Church Missouri Synod	1951-1991	CA, FL, IA, IN, KS, MN, ND, OH, SD, TX
United Methodist Church	1962-2000	CA, CO, FL, IA, IN, KS, MN, OH, OR, PA, SC, TN, TX, VA, WA
United Church of Christ	1950-2000	AZ, CA, CO, FL, IA, IN, MN, OH, PA, TX, VT

There are 16 states that repealed blue laws: FL (blue law repealed in 1969), IA (1955), IN (1977), KS (1965), MN (1985), ND (1991), OH (1973), PA (1978), SC (1985), SD (1977), TN (1981), TX (1985), UT (1973), VA (1975), VT (1982), WA (1966). There are 8 states that never had blue laws: AZ, CA, CO, ID, NV, NM, OR, and WY. Of these, the only states not covered by any denomination any year are Utah and Idaho. For some denominations, data on a state are only available for a subset of years.

Panel B: Church Spending Data for Casino Regressions

Denomination	Years Data Available	States Available
Southern Baptist Convention	1968-1998	AL, AR, AZ, CA, CO, FL, GA, IA, IL, IN, KS, KY, LA, MI, MO, MS, NC, NM, NV, NY, OH, OK, SC, TN, TX, VA, WV, WY
Lutheran Church Missouri Synod	1951-1991	CA, FL, IA, IL, IN, KS, MI, MN, MO, MT, ND, NE, NJ, NY, OH, OK, SD, TX, WI
United Methodist Church	1962-1998	AL, AR, CA, CO, FL, GA, IA, IL, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, NE, NJ, NY, OH, OK, OR, PA, SC, TN, TX, VA, WA, WI, WV
United Church of Christ	1950-1998	AZ, CA, CO, CT, FL, IA, IL, IN, MA, ME, MI, MN, NE, NH, NY, OH, PA, RI, TX, VT, WI

For some denominations, data on a state are only available for a subset of years. There are 45 states included by at least one denomination; the 5 states not included are Alaska, Delaware, Hawaii, Idaho, and Utah.

Panel C: Select GSS Means

Variable	Mean
Dummy for at Least Weekly Worship Attendance	0.29
Dummy for Worship Attendance less than Weekly but more than Never	0.57
Number of Counties in Respondent's State with a Casino	0.97
Dummy for whether Blue Laws have been Repealed in Respondent's State (1 = Repealed)	0.81

Data are from 1973-1998 waves of the General Social Survey. All variables except for the blue-laws dummy are based on a sample of 34,760 respondents. The blue-laws dummy is based on the sample of respondents in the 16 usable blue laws states listed under panel A; the sample size for this variable is 11,720.

Panel D: Select NLSY Means

Variable	Mean
Had 6 or more alcoholic drinks on one occasion in the past month	0.34
Attended religious services at least once a week in past year (in 1979)	0.33
Attended religious services less than weekly but more than never in the past year (in 1979)	0.48

Data are from the NLSY 79 with a total of 56,969 observations. The sample used for these means matches the sample in column 1 of Table 7. The mean of the drinking variable for weekly attendees is 0.288; for moderate attendees it is 0.362 and for nonattendees it is 0.396.

Table 2: Interacted Specifications

Panel A: Blue Laws and Religious Spending

	Interacted	State Trends	State/Den. Trends
Repeal*Conservative Index	-0.0352 [0.0308]	-0.0202 [0.0190]	-0.0188 [0.0203]
Repeal Dummy	0.0071 [0.0667]	-0.00546 [0.0434]	-0.00685 [0.0395]
Observations	1824	1824	1824
R-squared	0.902	0.935	0.954
Wald Test: Index = 1 *p value*	0.468 $p = 0.501$	0.756 $p = 0.395$	1.064 $p = 0.314$
Wald Test: Index = 2 *p value*	4.891 $p = 0.038$	3.62 $p = 0.071$	3.944 $p = 0.06$
Wald Test: Index = 3 *p value*	5.23 $p = 0.033$	4.301 $p = 0.051$	3.33 $p = 0.082$

Panel B: Casinos and Religious Spending

	Interacted	State Trends	State/Den. Trends
Casinos*Conservative Index	-0.00399 [0.00268]	-0.00616 [0.00275]	-0.00446 [0.00164]
Counties with Casinos	0.00607 [0.00577]	0.00334 [0.00537]	0.00193 [0.00349]
Observations	3657	3657	3657
R-squared	0.913	0.944	0.956
Wald Test: Index = 1 *p value*	0.244 $p = 0.624$	0.795 $p = 0.377$	0.754 $p = 0.39$
Wald Test: Index = 2 *p value*	0.224 $p = 0.639$	12.8 $p = 0.001$	4.822 $p = 0.033$
Wald Test: Index = 3 *p value*	1.179 $p = 0.283$	12.88 $p = 0.001$	7.634 $p = 0.008$

Robust standard errors, clustered by state, in brackets. The dependent variable in both panels A and B is total religious spending, per member, logged. All regressions include as regressors the percent of individuals in a state under age 5, between 5 and 18, between 45 and 64, and over 65, the percent of individuals in a state black , the percent foreign born, the rate of insured unemployment, per capita disposable income, denomination-by-state dummies, and denomination-by-year dummies. The unit of observation is all churches in a given denomination, in a given state and year. The blue laws regressions omit state/year observations the year that a state repeals its law. The denominations used are the Southern Baptist Convention (SBC), Lutheran Church Missouri Synod (LCMS), United Methodist Church (UMC), and United Church of Christ (UCC). The conservative index is set to 0 for the UCC denomination, 1 for the UMC, 2 for LCMS and 3 for SBC; higher values correspond to a more conservative denomination. The repeal dummy in panel A equals 1 in a given state and year where blue laws have been repealed; the "Counties with Casinos" variable in panel B reports the number of counties in a given state and year operating a Class III casino. In panel A, the first row of Wald tests reports F-statistics of a test that the sum of (a) the repeal-dummy coefficient and (b) the repeal*index coefficient equals zero when the index has a value of 1. The second row of Wald tests reports F-statistics from the same test when the index has a value of 2; for the third row of tests the index has a value of 3. The Wald tests in panel B are analogous (see text for additional information). In panel A, the F statistic is distributed $F(1, 14)$, with a 95 percent critical value of 4.6. In panel B the F statistic is distributed $F(1, 44)$, with a 95 percent critical value of 4.06. Regressions are weighted by total membership. All monetary figures are in year 2000 dollars. In the top panel mean per-member spending in levels is about $380, in the bottom panel it is about $370. The mean number of counties with a casino in the sample is 0.6; the maximum number of counties with casinos is 15.

Table 3:
Blue Laws and Spending, Denomination-by-Denomination

	Basic	State Trends	State/Den. Trends
Repeal * SBC Dummy	-0.104 [0.0467]	-0.0604 [0.0356]	-0.0653 [0.0369]
Repeal * LCMS Dummy	-0.069 [0.0481]	-0.0851 [0.0734]	-0.0351 [0.0225]
Repeal * UMC Dummy	-0.014 [0.0544]	-0.0238 [0.0275]	-0.0256 [0.0294]
Repeal * UCC Dummy	-0.0613 [0.0809]	0.00119 [0.105]	-0.0128 [0.0254]
State Trends?	No	Yes	Yes
State-by-Denomination Trends?	No	No	Yes
RHS Controls?	Yes	Yes	Yes
Denomination*State Dums?	Yes	Yes	Yes
Denomination*Year Dums?	Yes	Yes	Yes
Observations	1824	1824	1824
R-squared	0.902	0.935	0.954

Robust standard errors, clustered by state, in brackets. The dependent variable is total religious spending, per member, logged. The unit of observation is all churches in a given denomination, in a given state and year. The sample includes data from any available state that repealed its blue laws (or never had blue laws), see panel A of Table 1 for more information. The blue laws regressions omit state/year observations the year that a state repeals its law. The denominations used are the Southern Baptist Convention (SBC), Lutheran Church Missouri Synod (LCMS), United Methodist Church (UMC), and United Church of Christ (UCC). Regressions are weighted by total membership. Right hand side controls include the percent of individuals in a state under age 5, between 5 and 18, between 45 and 64, and over 65, the percent of individuals in a state black , the percent foreign born, the rate of insured unemployment, and per capita disposable income. All monetary figures are in year 2000 dollars. Mean per-member spending, in levels, is $383.

Table 4:
Casinos and Spending, Denomination-by-Denomination

	Basic	State Trends	State/Den. Trends
Casinos * SBC Dummy	-0.00739 [0.00584]	-0.0163 [0.00473]	-0.0113 [0.00422]
Casinos * LCMS Dummy	0.0001 [0.00929]	0.00529 [0.00680]	-0.00649 [0.00359]
Casinos * UMC Dummy	0.00671 [0.00603]	0.000147 [0.00518]	-0.00281 [0.00401]
Casinos * UCC Dummy	-0.00527 [0.00353]	-0.00398 [0.00490]	0.00268 [0.00380]
State Trends?	No	Yes	Yes
State by Denomination Trends?	No	No	Yes
RHS Controls?	Yes	Yes	Yes
Denomination*State Dums?	Yes	Yes	Yes
Denomination*Year Dums?	Yes	Yes	Yes
Observations	3657	3657	3657
R-squared	0.913	0.944	0.956

Robust standard errors, clustered by state, in brackets. The dependent variable is total religious spending, per member, logged. The unit of observation is all churches in a given denomination, in a given state and year. The denominations used are the Southern Baptist Convention (SBC), Lutheran Church Missouri Synod (LCMS), United Methodist Church (UMC), and United Church of Christ (UCC). Regressions are weighted by total membership. Right hand side controls include the percent of individuals in a state under age 5, between 5 and 18, between 45 and 64, and over 65, the percent of individuals in a state black , the percent foreign born, the rate of insured unemployment, and per capita disposable income. All monetary figures are in year 2000 dollars. Mean per-member spending, in levels, for the full regression sample, is $367. The mean number of counties with a casino in the sample is 0.6; the maximum number of counties with casinos is 15.

Table 5: Church Attendance

Panel A: Blue Laws

	Basic			With Trends		
	High Attendee	Low Attendee	Non Attendee	High Attendee	Low Attendee	Non Attendee
Repeal Dummy	-0.0445 [0.0158]	0.0327 [0.0147]	0.0118 [0.0122]	-0.0714 [0.0236]	0.0477 [0.0192]	0.0236 [0.0148]
State Trends?	No	No	No	Yes	Yes	Yes
RHS Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16143	16143	16143	16143	16143	16143
R-squared	0.07	0.048	0.048	0.072	0.049	0.05

Panel B: Casinos

	Basic			With Trends		
	High Attendee	Low Attendee	Non Attendee	High Attendee	Low Attendee	Non Attendee
Casino Counties	-0.0010 [0.00135]	0.00046 [0.0014]	0.00053 [0.001]	0.0015 [0.0023]	0.00083 [0.0029]	-0.0023 [0.0015]
State Trends?	No	No	No	Yes	Yes	Yes
RHS Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34760	34760	34760	34760	34760	34760
R-squared	0.071	0.03	0.051	0.073	0.032	0.052

Robust standard errors, clustered by state, in brackets. All regressions are linear probability regressions. The dependent variable for "high attendee" regressions is a dummy that equals 1 if an individual reports attending worship at least once a week. The dependent variable for "low attendee" regressions equals 1 if attendance is greater than never but less than weekly. The dependent variable for the "non-attendee" regressions equals 1 if an individual does not attend worship. All regressions include regressors for respondent's gender, race, educational status, marital status, age, the percent of individuals in a state black , the percent foreign born, the rate of insured unemployment, per capita disposable income, state dummies, and year dummies. All regressions are run on GSS data from 1973 to 1998. The regressions in panel A here are limited to the states listed below panel A of Table 1. The blue laws regressions omit state/year observations the year that a state repeals its law. The Casino Counties variable in panel B reports the number of counties in a given state and year operating a Class III casino.

Table 6:
Blue Laws & Heavy Drinking

	Baseline	w/State & Yr Dummies	w/State-by-Yr Dummies	Extra Interactions
High Attendee * Δ Repeal	0.0876 [0.0409]	0.0840 [0.0397]	0.0639 [0.0402]	0.0777 [0.0448]
Low Attendee * Δ Repeal	0.0216 [0.0449]	0.0161 [0.0462]	-0.0017 [0.0483]	0.011 [0.0420]
Non Attendee * Δ Repeal	0.1506 [0.0584]	0.1406 [0.0615]	0.1157 [0.0583]	0.1440 [0.0527]
College * Δ Repeal	-	-	-	-0.0146 [0.0271]
Woman * Δ Repeal	-	-	-	0.0521 [0.0330]
Married * Δ Repeal	-	-	-	-0.0401 [0.0183]
State-by-Year Dummies?	No	No	Yes	No
First-Differenced Data?	Yes	Yes	Yes	Yes
State Dummies?	No	Yes	Yes	No
Year Dummies?	No	Yes	Yes	No
RHS Controls?	Yes	Yes	Yes	Yes
Observations	28103	28103	28103	28103
R-squared	0.029	0.039	0.046	0.029

Robust standard errors, clustered by state, in brackets. The dependent variable equals unity if a respondent reports having 6 or more drinks in one sitting in the past 30 days. The regression sample is from the 1983, 1984, 1985, 1988, 1989, and 1994 waves of the NLSY 79. The regressions include the first differences of a set of individual and state controls, a set of age dummies, a set of dummies for years since last interview, and (non-differenced) controls for the percent of individuals in a state black, the percent foreign born, the rate of insured unemployment, per capita disposable income. These regressions are limited to the 24 states below panel A of Table.

Table 7:
Legal Drinking Age & Heavy Drinking

	Baseline	w/State & Yr Dummies	w/State-by-Yr Dummies	Extra Interactions
High Attendee * Δ Overage	0.024 [0.0162]	0.0303 [0.0165]	0.0282 [0.0162]	0.0452 [0.0240]
Low Attendee * Δ Overage	0.018 [0.0200]	0.0206 [0.0188]	0.0193 [0.0191]	0.0377 [0.0221]
Non Attendee * Δ Overage	-0.0123 [0.0334]	-0.0038 [0.0328]	-0.0023 [0.0326]	0.0084 [0.0367]
College * Δ Overage	-	-	-	0.0019 [0.0480]
Woman * Δ Overage	-	-	-	-0.024 [0.0258]
Married * Δ Overage	-	-	-	-0.0432 [0.0224]
State-by-Year Dummies?	No	No	Yes	No
First-Differenced Data?	Yes	Yes	Yes	Yes
State Dummies?	No	Yes	Yes	No
Year Dummies?	No	Yes	Yes	No
RHS Controls?	Yes	Yes	Yes	Yes
Observations	56969	56969	56969	56969
R-squared	0.031	0.042	0.05	0.031

Robust standard errors, clustered by state, in brackets. The dependent variable equals unity if a respondent reports having 6 or more drinks in one sitting in the past 30 days. The regression sample is from the 1983, 1984, 1985, 1988, 1989, and 1994 waves of the NLSY 79. The regressions include the first differences of a set of individual and state controls, a set of age dummies, a set of dummies for years since the last interview, and (non-differenced) controls for the percent of individuals in a state black, the percent foreign born, the rate of insured unemployment, per capita disposable income