

PRELIMINARY AND INCOMPLETE

The Church vs. the Mall:
What Happens When Religion Faces
Increased Secular Competition?

Jonathan Gruber, MIT and NBER
Daniel M. Hungerman, Notre Dame*

Recently economists have begun to consider the causes and consequences of religious participation. A challenge for work in this area is finding attractive variation in the incentive to participate in religious activities. In this paper we identify a policy-driven change in the opportunity cost of religious participation based on changes in state laws that prohibit retail activity on Sunday, known as “blue laws.” Many states have repealed these laws in recent years, raising the opportunity cost of religious participation. We construct a model which predicts that allowing retail activity on Sundays will lower attendance levels, but may increase or decrease religious donations. We then use a variety of datasets to show that when a state repeals its blue laws religious attendance falls, and that church donations and spending fall as well. These results do not seem to be driven by declines in religiosity prior to the law change, nor do we see comparable declines in membership or giving to non-religious organizations after a state repeals its laws. Finally, we assess the effects of changes in these laws on drinking and drug use behavior in the NLSY. We find that after a state repeals its blue laws there is an insignificant change in drinking and drug use in the population. However, when we compare initially religious individuals to other individuals, we find that repealing blue laws leads to an increase in drinking and drug use behavior among initially religious individuals relative to others. The effect is economically significant, suggesting that the gap in heavy drinking between religious and non religious individuals falls by about a third after the laws are repealed.

* We are grateful to Tracy Liu and especially Gabriel Durazo for research assistance, and to the NBER non-profit fellowship for research support (Hungerman). This work was supported by the Metanexus Institute.

Religious service attendance is one of the most popular activities in the United States. In a typical week, 20 percent of the U.S. population attends religious services, and half of the population attends religious services at least once per month. Participation is widespread across all socio-economic groups and locations, and has remained relatively constant through time.¹ Despite being a key aspect of time allocation for individuals and families, social scientists still have little understanding of what drives religious participation.

In particular, there is little understanding about how the secular world influences decisions about religious participation. Is religious participation a fixed taste parameter that is independent of secular influences? Or are there important substitutabilities or complementarities between secular influences and religious participation, so that changes in the secular world can impinge upon religious participation? The answer to these questions not only matters for modeling time allocation, but also for general determination of individual and social well-being. There is a large literature which suggests that there are positive benefits to both the individual and society from increased religious participation, so there could be potential welfare effects if secular activity crowds out religious activity.

In principle, one can assess the extent to which changes in the secular world spill over into religious participation by using a change in the opportunity cost of religious attendance. Such changes are hard to isolate, however. Differences between individuals in the opportunity cost of religious participation, for example due to earnings differences or the religious preferences of a spouse, are correlated with taste parameters that might independently determine religious participation. Attempts to address this concern by adding additional controls, as much of the literature has done, are not entirely satisfying, especially given that characteristics likely to affect the decisions to undertake religious participation (such as the underlying “goodness” of an individual) are difficult to control for.

In this paper we identify a policy-driven change in the opportunity cost of religious participation

¹ See Iannaccone (1998) for information on the prevalence and determinants of religious participation.

based on changes in state laws that prohibit retail activity on Sunday, known as “blue laws.” Many states have repealed these in recent years, thus raising the opportunity cost of religious attendance by offering alternatives for work, leisure, and consumption.

We investigate the impact of this loosening on the constraints of the secular world as it competes with the religious world. We begin by developing a theoretical model of individuals choosing between secular and religious time allocation (secular leisure or work vs. religious attendance) and goods allocation (secular consumption vs. religious contributions). Initially, the presence of blue laws restricts individuals’ ability to pursue secular leisure activities and secular consumption on Sundays due to the blue laws. When the laws are repealed, there are two possible effects. First, time devoted to religious pursuits unambiguously falls, as individuals choose to devote more time to work and non-religious leisure activities. Second, there is an ambiguous effect on religious contributions. On the one hand, incomes may rise due to new work activities, and this could increase contributions. On the other hand, new secular consumption opportunities compete with religious giving for a share of the individual’s budget, and this could decrease contributions.

We then turn to an empirical investigation of the impact of secular competition by studying the large number of states that repealed their blue laws over the past forty years. We begin by using data from the General Social Survey (GSS) on religious attendance to show a very strong reduction in religious attendance when the blue laws are repealed, particularly among those who had been relatively frequent attendees before repeal.

We then use data from the Consumer Expenditure Survey (CEX) to document a strong decline in religious contributions when blue laws are repealed, despite no change in other charitable activity. We confirm these findings using a unique dataset of budget data for four major Christian denominations over the past forty years, which show a significant decline in church expenditures when the blue laws are repealed. Thus, secular competition does matter for religious participation: increased secular

opportunities for work and leisure on Sundays led to less time at church and lower religious contributions.

As noted earlier, one reason we might care about these findings is that changes in the opportunity cost of religious participation may have negative consequences for individuals or society. In the final section, we test this possibility using data from the National Longitudinal Survey of Youth (NLSY) on consumption of alcohol and illegal drugs. We find that the repeal of the blue laws is associated with increased consumption of these goods, particularly among those who were the most frequent church-goers before repeal. This is consistent with social costs from reduced religious participation.

Our paper proceeds as follows. Part I provides a brief history of the blue laws in the U.S., and discusses their repeal in a number of states. Part II develops a model of time and budget allocation which allows us to demonstrate the impact of blue laws-like constraints, and their removal. Part III presents our analysis of secular competition and time allocation, using data on religious attendance in the GSS. Part IV presents our analysis of religious giving in the CEX and of church budget data. Part V documents the impact of blue law repeal on alcohol consumption. Part VI concludes.

Part I: Background on the Blue Laws

This section provides a brief overview on the history of blue laws in the United States.² 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.

Blue laws have a long history in America, dating back to 1610. All thirteen colonies had Sunday closing laws. By the end of the 19th century, nearly every state had at least some law prohibiting certain activities on Sunday. Laws prohibiting general retail activity on Sundays were fairly widespread as of the mid 1900s. These laws frequently prohibited “labor” or “all manner of public selling,” but often made

² The discussion here draws primarily from Goos (2005) and Laband and Heinbuch (1987).

exceptions for acts of charity.³ Blue laws prohibiting very specific types of activities, such as barbering or the sale of alcohol were also fairly common.⁴

In 1961 the Supreme Court issued a number of decisions on the constitutionality of blue laws. The most important of these decisions, 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.”

Following this ruling, blue laws were frequently 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 Arkansas it was permissible on Sunday to sell film and flashbulbs, but not cameras. Some New Jersey counties allowed the sale of disposable diapers, but washable diapers were prohibited (King, 1976).

In the decades following this ruling, most states repealed their blue laws either through judicial or legislative action. Goos (2005) reports that 33 states had general prohibitions against retail activity on Sunday in 1961; the number had fallen to 22 in 1985, and as of 2005 only 8 states prohibited retail activity.

To examine the effects of these repeals, we gathered information on each state’s blue laws from the 1950s until the present. Our goal was to find as many states as possible where we could document a discreet and significant change in the prohibition of retail activity on Sundays. Some states did not satisfy these criteria; the most common reason being that the state’s laws were (or are) decided at the county or city level, making these states unusable with some of our state-level datasets. A few states were not used because we could not verify the exact time that the laws were repealed. Four states were dropped because

³ States sometimes exempted certain types of retail activity, for example by allowing pharmacies to stay open on Sunday. We describe below some states that provide extensive exemptions to their laws.

⁴ We had hoped to include laws prohibiting alcohol sales in our study, but these laws are very seldom repealed all at once. States allowing alcohol sales often do so through a series of small adjustments and exemptions which are difficult to quantify.

they had too many exceptions to their laws. Finally, seven western states never had any retail blue laws during this time period and were also dropped.

Table 1 lists the remaining states and the year when their laws were repealed, either by judicial action or act of the legislature. The table also lists which states are excluded and the primary reason for exclusion. While many states are not usable, the states whose laws are suitable for analysis make up a fairly diverse group. Figure 1 illustrates the sample of usable states. While there are relatively few states in the west and in New England, we nevertheless have at least some representation in all areas of the country, and there does not appear to be any pattern in the timing of when laws are repealed in any given part of the country.

One may be concerned that blue laws may not have been enforced prior to their repeal. If blue laws were not enforced, then their repeal should not have a significant effect on the opportunity cost or religious participation and this will bias us away from observing any effect empirically. Another concern with blue laws is that they were not repealed exogenously. For example, it might be the case that declining levels of religiosity led to the laws being repealed, rather than the other way around. We address this concern in the empirical section of the paper. First however, we consider the theoretical effects of repealing these laws on religious participation.

Part II: A Model of Religious and Secular Time Allocation and Consumption

In this section, we develop a model to illustrate how blue law repeal might impact religious participation. Our model considers an individual deciding how to spend time and money on Sunday. The purpose of the model is to provide predictions on what effect (if any) blue laws might have on the two key aspects of religious participation that are available in our data: religious attendance and religious contributions.

The laws might work through two channels. First, they may constrain an individual's ability to

work on Sunday. Second, blue laws might constrain an individual's ability to consume secular goods and services on Sunday. It turns out that imposing either type of constraint will generally lead to a rise in church attendance, but that the effects of the constraints on church donations is less clear.

Consider an individual who can spend money on two activities; one is religious and one is secular. Further, the individual can spend his time Sunday working, participating in secular leisure activities, or by attending church. More formally, let individual i 's utility function be given by

$$U_i(x_R, t_R, t_S) + V_i(x_S).$$

where x_R is donations to church, x_S is secular consumption, t_R is time spent in church, and t_S is time spent on secular activities (where $t_R + t_S \leq 1$). We assume utility is increasing in inputs, concave, continuous, and differentiable.

The individual's budget constraint is

$$x_R + p_S x_S + wt_R + wt_S \leq I.$$

Where income $I = w + y$ includes both the earnings potential for working on Sundays, w , as well as income not derived from working on Sunday, y . For example y might include labor income from the rest of the week as well as non labor income. Thus, even if an individual does not work at all on Sunday, the individual may still be able to consume x_R and x_S .⁵

⁵ The inclusion of income not earned on Sunday y raises the valid question of whether other days of the week should be added to the model. It turns out that other days are superfluous. If we considered two days of the week, Sunday and Not Sunday, and rewrote the utility function and budget constraint as

$$U_i(x_R, t_R, t_S, x_R^{NS}, t_R^{NS}, t_S^{NS}, x_S^{NS}) + V_i(x_S)$$

$$x_R + p_S x_S + wt_R + wt_S + x_R^{NS} + p_S x_S^{NS} + wt_R^{NS} + wt_S^{NS} \leq I$$

where the superscript *NS* denotes choice variables for not Sunday, and $I = 2w + y$, none of the intuition that follows would change as long as consumption on Sundays and consumption on Not Sunday are not perfect substitutes (although this would follow from the assumptions we make below). We therefore focus on the simpler situation where there is only one day of the week.

We allow blue laws to have effects in two ways. First, these laws may limit or prohibit work opportunities, so that $t_R + t_S \geq \alpha_w$. Thus, if blue laws prohibit work entirely, then $\alpha_w = 1$ and $t_R + t_S = 1$. We call this restriction the work restriction. Second, blue laws may restrict or prohibit secular consumption. We follow Glaeser and Laibson (2005) in modeling this restriction as a restriction on the quantity of secular consumption allowed, so that $x_S \leq \alpha_S$. If secular consumption is prohibited entirely on Sundays, then $\alpha_S = 0$. We call this restriction the consumption restriction.⁶

The individual, in the presence of these laws, thus maximizes

$$U_i(x_R, t_R, t_S) + V_i(x_S) + \lambda(I - x_R - p_S x_S - w t_R - w t_S) + \mu_S(\alpha_S - x_S) + \mu_w(t_R + t_S - \alpha_w)$$

where the first term is utility, the second is the budget constraint, and the last two terms are non-negativity conditions imposed by the blue laws.⁷

Consider first a situation where the laws have no effect on an individual's decisions; individuals would choose the same outcomes regardless of whether the laws were in place or not. Then the Kuhn-Tucker multipliers μ_S and μ_w are set to zero in the above conditions and (7) and (8) are not relevant. In this case, repealing blue laws will have no affect on an individual's optimum allocation of time and money. Denote the unconstrained solutions as x_R^* , x_S^* , t_R^* , and t_S^* .

Consider next the case where the work restriction is binding, but the consumption restriction is not. That is, $\mu_S = 0$ and $\mu_w \neq 0$. In this case, the individual is at a corner solution and cannot spend as much time working as he would choose were there no blue laws and $t_R + t_S = \alpha_w > t_R^* + t_S^*$. As the work constraint is relaxed and α_w is lowered, it follows that time on work will increase, and aggregate

⁶ Note that these constraints create a set of feasible choices for the individual that is convex, so that the Kuhn Tucker conditions will yield a global maximum (and not merely a local maximum).

⁷ We focus only on the two Kuhn-Tucker conditions related to the laws, and assume that the standard Kuhn-Tucker conditions on non-negativity of the choice variables are not relevant. The solutions are thus "interior" except to the extent that policy constraints create corner solutions.

time spent not working will decrease. The first order conditions show that in equilibrium the marginal rate of substitution between t_R and t_S does not change as α_w is lowered. The following weak assumption is sufficient (but not necessary) to ensure a robust response of both t_R and t_S as α_w falls.

Assumption 1: (a) the marginal rate of substitution between t_R and t_S is not a function of x_R , and (b)

$$\frac{\partial^2 U}{\partial t_i \partial t_j} > \frac{\partial^2 U}{\partial^2 t_i} \text{ for } i, j \in \{S, R\}, i \neq j.$$

This assumption essentially ensures that the two temporal goods are “normal” in that if total expenditures on the two goods rise in equilibrium, but the marginal rate of substitution does not change, then it must be

the case that expenditures on *each* good rises. The assumption that $\frac{\partial^2 U}{\partial t_i \partial t_j} > \frac{\partial^2 U}{\partial^2 t_i}$ is not very strong in

that if it did not hold for either temporal good t_i the Hessian matrix would not be negative semidefinite and the utility function would not be quasiconcave.⁸ Also, this condition also allows the cross-partial

derivative of utility between t_R and t_S to be negative, just so long as it is not less than $\frac{\partial^2 U}{\partial^2 t_i}$.

Under this assumption, as the work constraint is relaxed, it must be the case that both t_R and t_S fall. To see this, take the derivative of the marginal rate of substitution between t_R and t_S , denoted U_{t_R} / U_{t_S} , with respect to t_S . The derivative can be written $(U_{t_R t_S} U_{t_S} - U_{t_R} U_{t_S t_S}) / (U_{t_S})^2$ which is positive by Assumption 1. Similar work can show that the marginal rate of substitution is falling with respect to t_R . Thus, we know that after the blue laws change, the only way the marginal rate of substitution can be set to its equilibrium value is through changes in t_R and t_S (and not through changes

⁸ This follows since negative semidefiniteness requires $\frac{\partial^2 U}{\partial t_i^2} \frac{\partial^2 U}{\partial t_j^2} - \frac{\partial^2 U}{\partial t_i \partial t_j} \frac{\partial^2 U}{\partial t_j \partial t_i} > 0$. If $\frac{\partial^2 U}{\partial t_i \partial t_j} < \frac{\partial^2 U}{\partial t_i^2}$,

then the cross partial is “more negative” than the second derivatives, and the square of the cross partial derivative will consequently be larger than the product of the two second-derivatives and quasiconcavity will be violated.

in x_R and x_S). It follows that, if the sum of t_R and t_S falls as work rises, that *both* t_R and t_S must fall. If one fell and the other increased (or stayed the same), then the marginal rate of substitution would not be the same after the law changed as it had been before the law changed.

As the work constraint is relaxed, the net effect on church donations, x_R , is more difficult. On the one hand, total spending on x_R and x_S combined will rise as “expenditures” on t_R and t_S fall. But it is possible that only secular consumption rises with earned income, and that church donations either stay the same or fall.

As an example, suppose utility can be represented by $\sum_{i=R,S} 2x_i^{1/2} + 2t_i^{1/2}$ and let $p_S = 1$. It is easy to see that these preferences satisfy Assumption 1. Given a binding work constraint, the individual will set $t_R = t_S = \alpha_w / 2$, and will set $x_R = x_S = (I - w\alpha_w) / 2$. Here as the constraint is relaxed and α_w falls, x_R rises.

Now suppose utility can be represented as $(x_R^{1/2} + t_R^{1/2} + t_S^{1/2})^2 + 2x_S$ and again let $p_S = 1$. The term in parentheses is a constant elasticity of substitution function with an elasticity of substitution equal to 2. This example also satisfies all the conditions on preferences we have stated, including Assumption 1. Here, however, under a binding work constraint it can be shown from the first order conditions on x_R and x_S that the individual will set $x_R = 2\alpha_w$, so that as the constraint is relaxed and α_w falls, x_R falls. Thus, the work constraint has a clear affect on temporal activities, but an indeterminate effect on expenditures, and in particular on church donations.

Consider next the case where the consumption restriction is binding but the work restriction is not, so that $\mu_S \neq 0$ and $\mu_w = 0$ and $x_S = \alpha_S < x_S^*$. Then as the consumption constraint is relaxed,

secular consumption will rise, and by the budget constraint it follows that total “expenditures” on the other three choice variables will fall.

Consider first how a relaxation of the consumption constraint will affect time in church, t_R . Let optimal choice of t_R in the face of the consumption constraint be given as t_R^{**} . Given the setup of the utility function, the optimal solution in this case solves

$$\max_{x_R, t_R, t_S} U_i(x_R, t_R, t_S) + \lambda(\tilde{I} - x_R - wt_R - wt_S)$$

where $\tilde{I} = I - p_S \alpha_S$. It follows that $\frac{\partial t_R^{**}}{\partial \alpha_S} = \frac{\partial t_R^{**}}{\partial \tilde{I}} \frac{\partial \tilde{I}}{\partial \alpha_S} = -p_S \frac{\partial t_R^{**}}{\partial \tilde{I}}$. We make the following

assumption on the normality of goods. Again, this assumption is fairly weak:

Assumption 2: (a) The unconstrained choice variable t_R^* is a strictly normal good in potential income I .

(b) The unconstrained choice variable x_S^* is continuous in income, and

$$p_S \frac{\partial x_S^*}{\partial I} \in (0, \phi), \text{ where } 0 \leq \phi < 1.$$

Assumption 2 involves normality in potential income I rather than earned wages income. Intuitively, part (a) of Assumption 2 says that if there were suddenly 25 hours in the day then church attendance would increase. Part (b) of Assumption 2 states that secular consumption is also normal and adds the technical condition that the marginal propensity to consume x_S is less than (but can be arbitrarily close to) unity. Thus, if potential income grows by a dollar, secular consumption rises, but by an amount less than a dollar.

It follows from the conditions in Assumption 2 that t_R^{**} is normal in \tilde{I} . To see this, suppose instead that that t_R^* was normal in I but t_R^{**} was not normal in \tilde{I} . Then there exists income levels \tilde{I}^1 and \tilde{I}^2 , $\tilde{I}^1 < \tilde{I}^2$, such that $t_R^{**}(\tilde{I}^1) > t_R^{**}(\tilde{I}^2)$. Define the function $f(I) = I - p_S x_S^*(I)$. It follows from

the budget constraint and from Assumption 2 that $f(I)$ is continuous, monotonically increasing without bound, and $f(0) = 0$. Therefore, there exists an income level I' where $f(I') = I' - p_S x_S^*(I') = \tilde{I}^1$, and $t_R^*(I') = t_R^{**}(\tilde{I}^1)$. Similarly, there exists an income level I'' where $I'' - p_S x_S^*(I'') = \tilde{I}^2$, and $t_R^*(I'') = t_R^{**}(\tilde{I}^2)$. Since $f(I)$ is monotonically increasing, we know $I' < I''$. Then by normality of t_R^* we have $t_R^*(I') < t_R^*(I'')$. But this is not possible since $t_R^*(I') = t_R^{**}(\tilde{I}^1) > t_R^{**}(\tilde{I}^2) = t_R^*(I'')$, and we have a contradiction.

Therefore, under the weak conditions imposed by Assumption 2, it follows that $\frac{\partial t_R^{**}}{\partial I} > 0$ and

$\frac{\partial t_R^{**}}{\partial \alpha_S} < 0$. Intuitively, as the consumption constraint is weakened, secular consumption will rise and

individuals have less income to spend on other goods. The normality of t_R ensures that time in church will fall as total expenditures on goods besides secular consumption fall. Regarding the other choice variables, it should be clear that similar assumptions on their normality will lead to the same conclusion. The assumption that church donations are normal with respect to potential income thus leads to church donations falling as the consumption restriction is weakened.

Finally, there is the case where both the work restriction and the consumption restriction are binding. This case is simply a composite of the above situations. When the work restriction is relaxed, time on secular and religious activities will each fall and work will increase. Similarly, if church attendance is a normal good then time on religious activities will fall when the consumption restriction is relaxed; the overall first-order effect of the laws on time allocation is therefore unambiguous. For religious donations, however, the effect is less clear. While normality of church donations suggests that weakening the consumption constraint might lead to less donations, it is possible that weakening the consumption constraint would have an effect in the opposite direction.

To summarize, blue laws have the potential to restrict both a person's ability to work and a person's ability to consume secular goods and services. It is possible that neither of these restrictions affects an individual's decisions, and so repealing the laws would have no effect on consumer behavior. In the event that blue laws do affect an individual's decisions, under weak conditions repealing the laws will unambiguously lead to less time spent on religious activities. This is because after the laws are repealed formerly constrained individuals will increase time on work and secular consumption, and both of these crowd out time spent on religious activities. The situation is more complicated for religious donations. On the one hand, individuals who had been facing a restriction on work will respond to the laws being repealed by working more and spending more on both secular consumption and religious donations, and so religious donations could rise. On the other hand, if donations are normal individuals who had been facing a restriction on secular consumption will respond to the laws being repealed by substituting out of religious donations and spending more on secular consumption.

Part III: Secular Competition and Time Allocation

GSS Data and Empirical Methods

Our analysis of the time allocation effect of the blue laws begins with modeling their impact on religious attendance. To carry out this analysis, we turn to the General Social Survey (GSS), the longest-running national survey that gathers data on religious participation. In every year since 1972, this survey has asked a sample of 1,500 to 2,500 respondents about their frequency of religious attendance. There are nine possible responses to this question: never; less than once per year; about once or twice a year; several times a year; about once a month; two to three times a month; nearly every week; every week; and several times a week. For the basic analysis below, we simply use the linear index formed by these responses (with values 0 through 8); given that each interval represents roughly a doubling of attendance frequency, this is akin to a log scale. But we also show as well the effects on particular categories of

responses below.

Our sample covers the years 1973 to 1998. We consider individuals in the states with usable blue laws data listed in Table 1. We limit the sample to individuals who report their “religious preference” as Catholic or Protestant, as these individuals are those most likely to attend services on Sunday (these individuals make up nearly 90 percent of the sample).⁹ Additionally, we drop data from a given state in the year the law changed.

Table 2 is table of means; the first panel presents the means of selected variables of interest from the GSS. The average value of our attendance index, which ranges from 0 (never) to 8 (several times a week) is slightly above 4, which corresponds to monthly attendance. (The median response is attendance 2-3 times a month). The table also shows that GSS somewhat over-samples female respondents.¹⁰

We use these data to estimate models of the form

$$A_{ijt} = \beta Laws_{jt} + \gamma X_{ijt} + \delta Z_{jt} + \phi_j + \nu_t + \varepsilon$$

where A_{ijt} is religious attendance for individual i in state j in year t ; $Laws_{jt}$ is an indicator for whether the blue laws are still in place in state j in year t ; X_{ijt} is a set of characteristics of the individual i (age, age squared, gender, dummies for race, dummies for educational attainment, and a dummy for being married); Z_{jt} is a set of state/year control variables (state percent black, state percent foreign born, and the statewide rate of insured unemployment); $\beta Laws_{jt}$ is an indicator for whether the blue laws are still in place in state j in year t ; ϕ_j is a set of state dummies; and ν_t is a set of year dummies. This “difference-in-difference” equation thus assesses whether repealing the blue laws causes a deviation from a state’s mean of participation relative to other states at this time. Following Bertrand et al. (2004), we cluster our

⁹ We also considered examining the effects of blue laws on Jews, for whom blue laws should not matter since their day of worship is not Sunday. The estimates were insignificant as expected, although the sample was too small for the results to be regarded as reliable.

¹⁰ We compared data on observable characteristics such as age and gender in the sample of states used in the regressions to the sample of states excluded. The means were generally similar.

standard errors at the state level; our standard errors are similar if clustered at the state/year level.

Results

The results from this analysis are presented in Table 3. The first column shows our basic difference-in-difference regression for the religious attendance index. We find a statistically significant negative effect on religious attendance of blue law repeal in the first row; in parentheses under the coefficient is the state-clustered standard error, and in square brackets is the sample mean for the dependent variable. The result indicates that repealing the blue laws reduced attendance by 0.21 index points, or about 5 percent of the sample mean. This is a sizeable effect: it is one-third as large as the well-noted higher rate of religious attendance for women, for example. Thus, the prediction of the model developed above is clearly borne out: raising secular competition lowers religious attendance.

The coefficients on the remaining control variables are as expected and reported in previous studies. Older persons and females are more likely to attend, as are those who are more educated (the excluded category of education). There is little relation to state/year variables.

Table 4 shows the results separately by attendance category, in order to understand the nature of shifts in attendance; each row here is a separate regression where a dummy for that attendance category is used as the dependent variable. There is little effect on those who attend more than weekly; at least two factors may explain this. First, it is possible that such individuals are not constrained by the prohibitions on secular activity that blue laws entail. In the context of the model presented earlier, these are individuals at an “interior” solution at which the work constraint and the consumption constraint do not bind. Alternately, an individual may attend church (for instance) 4 days a week when blue laws are in place, and after they are repealed their attendance may fall to 3 days a week—but since such attendance is still “several times a week” we do not observe this decline.

Panel A of Table 4 shows that there are large effects on all of the other categories of regular attendance; the coefficients are negative and at least marginally significant for attending weekly, about

weekly, 2-3 times per month, at least once a month, or several times per year. The results make clear that the effect of the blue laws is not to induce casual church-goers to forgo attendance, but rather to reduce attendance among regular church-goers.

Specification Checks

The strong correlation between repealing the blue laws and reduced church-going is striking. Our causal interpretation of this finding rests on the assumption that nothing else changed at the same time as the blue laws that also caused a decline in church-going. In this section we present three sets of tests designed to support that assumption.

A common threat to the validity of any difference-in-difference exercise of this nature is underlying trends in the data. If there was a downward trend in church-going at the same time as the repeal of the blue laws, this would appear as a causal effect of the blue laws. Indeed, it is even possible that blue laws were repealed where church-going was weakening, so that there was more demand for secular opportunities on Sundays.

We test this alternative in two ways. First, we include in our model state-specific time trends. If there is an underlying trend in church going over time within the states that repeal the blue laws, these controls will help capture that trend. The second column of Table 3 includes these state-specific trend terms. As this column shows, if anything the addition of these trend terms *strengthens* the results.

Second, we run a falsification test: we include in the model a dummy for the two years *before* the blue law repeal. If blue law repeal is just picking up a pre-existing reduction in demand for church-going, then this should be captured in this “lead” term. In fact, the lead term is insignificant, and our estimated effect of the blue laws is unchanged from the first column.¹¹

¹¹ Adding time trends and the placebo dummy to the regressions done by attendance group in Panel A of Table 4 yields similar results—for each regression the estimates grow stronger when trends are added, and are unchanged by adding a placebo dummy. The placebo dummy is in all cases insignificant.

While these results are compelling, they are unable to rule out a simultaneous change in church-going attitudes and blue law repeal. We have tried to control for simultaneous economic conditions using the state/year disposable income and insured rate of unemployment. As a further test, we note that if there are omitted factors which drive down religious participation, they are likely to reduce participation in other group activities as well. The GSS collects data on whether the respondent is a member of one of a set of organizations, such as fraternal orders, political clubs, sports clubs or hobby clubs – as well as a member of church groups. In the second panel of Table 4, we show that there is a negative effect of blue law repeal on being a member of church groups, but no effect on the odds of being a member of any other group. This suggests that the religious effects are working through the time allocation mechanism we posed.

Part IV: Secular Competition and Religious Contributions

Data and Empirical Methods

While the results in Part III confirmed our conclusion from the theory model about religious attendance, we had no such clear prediction for religious contributions. In this section, we use data from two sources to address the impacts on religious contributions.

The first source is the Consumer Expenditure Survey (CEX). The CEX collects data for a nationally representative set of households on an inventory of their consumption expenditures, including charitable contributions. Charitable contributions are divided into contributions to: religious organizations; educational organizations; political organizations; charitable organizations (e.g. the United Way); and other organizations. The CEX data are collected quarterly, but contributions information is only collected in the last interview, and respondents are asked about their charitable giving over the past year. We use these CEX data for the period from 1980 through 1998.

These CEX data have the disadvantage that due to the time period covered, and the states reliably

sampled by the CEX, we only have three state changes to study, those in Tennessee in 1981 and in Minnesota and Texas in 1985, and only 9 control states. Because of the small number of states included in these data, the results were more sensitive to clustering standard errors by state versus state and year. When the state-level controls were excluded, results were very similar from both approaches. But when they are included, the standard errors using state clustering grow by 50 percent, although the coefficient is unchanged. We therefore present results here showing both state/year and state clustering.

We therefore turn to a more complete source of data from the yearbooks collected by Christian denominations themselves. In particular, we have compiled church expenditures at the state and year level for four major Christian denominations from the period 1950 to 2000. We chose for our analysis any of the largest denominations which reported annual data at the state level (or at a finer level of detail that could be aggregated up to states) over the 1950 to 2000 period. The resultant sample includes the Lutheran Church Missouri Synod (in 1971 this denomination was the 7th largest in the country), the Southern Baptist Convention (2nd), the United Church of Christ (9th), and the United Methodist Church (3rd).¹²

While it is likely these denominations are not perfectly representative of all churchgoers in the United States, all four of these denominations are among the largest in the country, and they represent a fairly diverse group of adherents in terms of regional location, growth trends over time, and religious and social conservatism. These denominations are fairly widespread; the data cover 15 of the 16 states for which we have usable blue laws data (the lone state which is not covered by any of our denomination data is Utah). For most of the states we have data multiple denominations.

These data do not measure our variable of most interest, donations to the church. But they do measure total church spending, which serves as an alternate measure of the resources available to a church. We therefore model whether church spending per member rises or declines when blue laws are

¹² Data on denomination sizes came from the Glenmary Research Center (1974).

repealed. Our empirical model is the same type of difference-in-difference model used earlier (although the regression does not include individual-level regressors, as our unit of observation is all the congregations in a given denomination, in a given state and year). As above, all standard errors are clustered at the state level.

CEX Results

Table 5 presents the results of this analysis for the CEX. We show results for total dollars of religious contributions, the log of contributions (conditional on making any), and the probability of contributing. When using total dollars, we censor the top 1 percent of observations at the 99th percentile value to minimize the influence of outliers. The regression includes the control variables noted earlier, but we just show the coefficients of interest.

The results show a sizeable decline in religious charitable contributions when blue laws are repealed. Repeal is associated with a decline in religious charitable contributions of \$109 on average, which is about 25 percent of the sample mean of \$433. This consists of a large 13 percent reduction in the amount of giving, conditional on giving, and a small and insignificant reduction in the odds of giving. These estimates are significant when we cluster standard errors at the state/year level, but not so when we cluster at the state level. Once again, this is consistent with the notion that those attached to church scale back their participation, but do not sever ties completely.

The next three columns demonstrate that this result is not driven by unobserved factors which were reducing charitable activity at the time of blue laws repeal by looking at charitable giving to sources other than religious organizations. There are actually large increases in other forms of charitable giving, which could represent some reallocation in response to the blue laws, but at a minimum does not suggest underlying taste shifts towards charitable giving.

The second panel of the Table shows the results when we add state-specific time trends.¹³ The estimated effect on the level of spending rises, while the estimated effect on the log of giving falls. None are significant, but the general pattern of findings is similar. Thus, for the limited set of repeals that we are able to study in the CEX data, we see a sizeable decline in religious charitable contributions.

Church Spending Results

Table 6 turns to our alternative source of data, church spending per member. This source has the disadvantage that it does not measure what we would really like, which is donations. But it has the advantage of extending over a very long time period across states, allowing us to study the full set of repeals in this time period. Our data is at the level of denomination/state/year, so we include in the model, in addition to the usual set of state and year fixed effects, a full set of denomination fixed effects; interactions between the denomination fixed effects and the state fixed effects; and interactions between the denomination fixed effects and the year fixed effects. We weight the regressions by church membership in the denomination/state/year cell, and once again cluster the standard errors by state. Our model also includes controls for the percent of the state population that is foreign-born and the percent that is black.

Panel A of Table 6 shows our basic findings. In the first column, we find that church spending per member fell by 6.3 percent when the blue laws were repealed; the result is statistically significant. In the second column, we add the number of members in the denomination/state/year cell as a control, to capture potential scale effects in giving/member. This has no effect on the estimates.

The next two columns of panel A pursue the specification checks we have used throughout the paper: including state-specific linear trends and undertaking a falsification check by putting in as well a dummy which becomes unity two years *before* the repeal. Neither of these changes has much effect on

¹³ We are not able to estimate the falsification using the “lead” term here since one of our three changes happens so early in the sample period.

the result.

One might wonder whether these denominational results are driven by a particular denomination. The sample sizes and incomplete geographic coverage of each denomination makes running the regressions on one denomination at a time problematic. However, we can remove each denomination from the pooled result and see whether the results change, and in so doing assess the results' sensitivity to each denomination. Panel B of table 6 assesses whether any particular denomination is driving this result by excluding one denomination at a time. In every case the coefficient remains negative, and in most cases remains significant. The most noticeable change is a reduction in the coefficient to about two-thirds of its size when the Southern Baptist Convention is excluded. That the results are most sensitive to this denomination is not too surprising given that it is the largest denomination in the dataset. However, when the placebo dummy or time trends are added, the results are similar regardless of what denominations are excluded, and in all cases the result is at least marginally significant.

Part V: Implications for Outcomes

The results thus far suggest that repealing the blue laws causes a reduction in religious participation along both the giving and going margins. This is important because it suggests that the secular and religious worlds do effectively compete for time allocation among Americans. But it might have additional implications as well, if religious participation has positive effects on behavior and outcomes. As reviewed in Gruber (2005), there is an enormous literature outside of economics which documents strong correlations between religious participation and positive outcomes, such as improved health and reduced criminal activity. This literature has generally, however, not dealt with the important identification concerns raised by this correlation. Gruber (2005) attempts to do so by using the ethnic/religious composition of the area as an instrument for religious participation, and he finds very large positive effects on economic outcomes such as education and income, but he does not explore other

outcomes.

For this section, we turn to the National Longitudinal Survey of Youth (NLSY), which follows a cohort of youth initially ages 14-21. The survey began in 1979 and has continued through the 1980s and 1990s. The NLSY has information on consumption of a variety of substances, including drinking, marijuana use, and cocaine use. In addition, the NLSY has a particular advantage for our purposes: it contains information on prior religious attendance. As Table 3 suggests using GSS data, the effects of changes in blue laws should be particularly acute for those who were the most frequent church attendees. We cannot investigate the effects of blue laws on outcomes such as drinking and drug use in the GSS, however, since church attendance is itself endogenous, so in categorizing individuals as frequent or infrequent attendees we would be using a selected sample. The NLSY, however, is longitudinal data, and provides data on religious attendance in 1979, before the blue laws were repealed for a number of states in our sample. Thus, we can use pre-existing religious attendance as a basis for dividing the sample by attendance category.

The attendance measure in the NLSY is similar to that we used for the GSS. In 1979 individuals were asked how often they had attended religious services in the past year. Possible answers were not at all, infrequently, once per month, 2-3 times per month, once per week, and more than once per week.¹⁴

Our measure of heavy drinking equals unity if a respondent reports having had six or more drinks in one sitting sometime in the past month. Between 1979 and 2000, this variable is available in the surveys taken in 1982, 1983, 1984, 1985, 1988, 1989, and 1994. Our measure of marijuana use is a dummy for whether a respondent has used marijuana in the past 30 days. Our measure of cocaine use equals unity if an individual has ever used cocaine; we focus on this measure rather than use in the past 30 days because fewer individuals report having used cocaine in the past 30 days. Measures of marijuana/cocaine consumption are available in 1984 and 1988. Thus, for drinking, we show the results

¹⁴ If attendance were available every year, we would do a 2SLS analysis. However, attendance was only asked one other year (1982), and so 2SLS is not feasible.

both for all waves and restricted to the same two waves for which other measures are available.¹⁵

We begin by estimating the same type of difference-in-difference models described earlier. These regressions drop observations in the state and year that a law was changed, include the same individual level controls used in the GSS, and cluster residuals by state. The results of this analysis are shown in the top panel of Table 7. For alcohol consumption, using all waves of available data, we find that repealing the blue laws leads to a rise in the odds of drinking of 1.4 percentage points, which is not significant. This represents about a 4 percent increase relative to the sample mean. When we restrict our analysis to the waves for which the marijuana and cocaine data are available (1984 and 1988), we find even weaker effects.

The next two columns show the results for marijuana and cocaine consumption. Here we find a positive and significant effect on marijuana consumption of 3.2 percentage points. This is a very large effect which is more than 20 percent of the sample mean. For cocaine, the result is wrong-signed but only as large as its standard error.

We then turn to estimating models where we interact the repeal dummy with dummies for frequent or some attendance. Since we are considering the effect on groups within-state, we can actually include a full set of state*year interactions in these models, so that we are identifying the effect of repeal on those who are high/somewhat frequent attendees relative to those who are non/infrequent attendees in the same state. This is an even richer specification than we have used earlier, controlling for any state-wide factors correlated with blue laws repeal. These results are shown in the second panel of Table 7.

The results here are striking: for each of these three behaviors, we find significant effects of repeal on those who attend church more frequently, relative to those who do not. For example, for

¹⁵ One might also consider whether NLSY respondents change their hours worked after blue laws are repealed. We examined both intensive and extensive work decisions in the NLSY, but in all cases found that blue laws have no effect. This is possible if individuals who start working on Sundays substitute out of working on other days. Goos (2005) also finds an insignificant effect of blue laws on weekly employment when looking at all industries over the period 1982 to 1992 (see Table IV in his paper).

drinking, we find that those who attend most frequently are 6.5 percent more likely to drink than those who don't attend, which is about 15 percent of the sample mean and about a third of the difference in heavy drinking between weekly attendees and non attendees.¹⁶ Those who attend somewhat frequently are 3.3 percent more likely to drink, which is about 10 percent of the sample mean and roughly half the gap in heavy drinking observed between sometimes attendees and non attendees. The fact that the effect is rising in frequency of attendance is comforting for our causal interpretation, but these estimates are not significantly different from each other. The next column shows the drinking results restricted to the 1984 and 1988 panels, which are larger and more similar across the two categories of participation.

The next two columns show the results for marijuana and cocaine consumption. Once again, we obtain large and (at least marginally) significant interaction effects. For marijuana, the effects are very large, suggesting a 10.5 percent increase for very frequent attendees and a 6.7 percent rise for somewhat frequent attendees. These represent increases of 70 percent and almost 50 percent relative to the mean and the majority of gap in marijuana use between weekly attendees and non attendees.¹⁷ For cocaine, the results are more modest but still large, with a roughly equal 4.1-4.3 percentage point effect on both very and somewhat frequent attendees, which is about 20 percent of the sample mean and again represents a sizeable portion of the gap in cocaine use between weekly, sometimes, and non attendees. If the results for drinking are any indication, these very large results may be dampened with more data availability. But the key point here is that there are strong effects of repeal on the most frequent attendees, relative to those with low or no church attendance.

One problem with these interaction models is that the interaction with religious participation may be reflecting some other differential effect. For example, females are more likely to attend church, and perhaps all we are capturing is differential effects of the blue laws on females. To address this concern, in

¹⁶ The mean of the heavy drinking variable for weekly attendees is 0.27, for sometimes attendees it is 0.35, and for non attendees it is 0.40.

¹⁷ The average for the marijuana variable for high, sometimes, and non attendees is 0.10, 0.15, and 0.23, respectively. For cocaine the respective means are 0.11, 0.20, and 0.25.

Table 8 we include a full set of interactions of the repeal dummy with other key demographic characteristics: marital status; college graduate; gender; and race. Doing so has little effects on our estimates of interest; clearly, we are picking up the differential effect on participation and not some other correlate.

Part VI: Conclusions

The results presented above confirm the powerful role that secular competition can have on religious behavior: when the blue laws are repealed, there is a significant drop in both religious attendance and religious contributions. This is a striking finding which has a number of interesting implications.

First, this finding serves to validate economic models of religiosity, as discussed extensively by Iannaccone (1998). Religious participation is not independent of economic influences such as the opportunity cost of church-going. This builds on a growing empirical literature which has highlighted the interaction between economic decisions and the religious sector (Chen 2005; Gruber and Hungerman, 2005; Hungerman, 2005).

Second, this finding can be a valuable input into the discussion of the regulation of religion and substitutable activities. Absent strong negative externalities, there seems little argument for restricting the days of the week that commerce can take place. But religious participation may be one of those activities with such externalities. As such, secular regulations such as blue laws which promote religious participation can have external effects. Whether those external effects are sufficiently large to justify restrictions on commerce is an excellent question for future research.

References

- Chen, D. (2005) "Club Goods and Group Identity: Evidence from Islamic Resurgence During the Indonesian Financial Crisis" Working paper.
- Glaeser, E. and Shleifer, A. (2001) "A Case for Quantity Regulation," NBER working paper 8184.
- Glenmary Research Center (1974) "Churches and Church Membership in the United States, 1971" Glenmary Research Center, Cincinnati, Ohio
- Goos, M. (2005) "The Impact of Shop Closing Hours on Labor and Product Markets," Working paper.
- Gruber, J. (2003) "Pay or Pray? The Impact of Charitable Subsidies on Religiosity" *Journal of Public Economics* 88, 2635-2655.
- Gruber, J. (2005) "Religious Market Structure, Religious Participation, and Outcomes: Is Religion Good for You?" *Advances in Economic Analysis & Policy*, 5(1) Article 5.
- Gruber J., and Hungerman, D. (2006) "Faith-Based Charity and Crowd Out during the Great Depression" NBER working paper 11332
- Hungerman, D. (2005) "Are Church and State Substitutes? Evidence from the 1996 Welfare Reform" *Journal of Public Economics* 89, 2245-2267
- Iannaccone, L. (1998) "Introduction to the Economics of Religion" *Journal of Economic Literature*, 36(3) 1465-1495.
- King, W. "Selling on Sunday Rising Despite Confusing Laws" *The New York Times*, May 22, pg 1.
- Laband, D. and Heinbuch, D. (1987) *Blue Laws*. Lexington Books: Lexington, Massachusetts
- Theuman, J. (2005) "Validity, Construction, and Effect of 'Sunday Closing' or 'Blue' Laws—Modern Status" *American Law Reports*, ALR4th, 246-332

Table 1: Sample of States and Time of Repeal

State	Year of Repeal	State	Year of Repeal
Florida	1969	South Carolina	1985
Iowa	1955	South Dakota	1977
Indiana	1977	Tennessee	1981
Kansas	1965	Texas	1985
Minnesota	1985	Utah	1973
North Dakota	1991	Virginia	1975
Ohio	1973	Vermont	1982
Pennsylvania	1978	Washington	1966

States were excluded from the sample for the following reasons:

Laws Decided at the City/County Level (12 states): Arkansas, Georgia, Kentucky, Louisiana, Maryland, Michigan, Mississippi, Missouri, New Hampshire, New Jersey, North Carolina, and Rhode Island.

Laws added exceptions over time, making it unclear in any year the extent to which the laws constrained activities (6 states): Illinois, Massachusetts, Maine, New York, West Virginia, Wisconsin

Unverifiable information on presence of laws or timing of law changes (8 states): Alaska, Alabama, Connecticut, Delaware, Hawaii , Montana, Nebraska, Oklahoma

Never had laws (8 states): Arizona, California, Colorado, Idaho, Oregon, Nevada, Wyoming, New Mexico

The above categories are not necessarily mutually exclusive. For example some states may have been excluded both because they had laws at the city level and because the state's information could not be verified for a given time period.

Table 2: Means of Selected Variables for Each Dataset

General Social Survey

Variable	Regression Sample
Attendance	4.38 (2.56)
Age	46.1 (17.9)
Sex (1= female)	0.58 (0.49)

Standard deviations in parentheses. Observations: 11720. The regression sample includes Catholics and Protestants, and excludes respondents surveyed the year a state repealed its laws.

Consumer Expenditure Survey

Variable	Regression Sample
Religious Giving (dollars)	434 (11472)
Religious Giving (logged dollars)	6.10 (1.46)
Dummy for religious Giving	0.40 (0.50)
Nonreligious Giving (dollars)	133 (457)
Nonreligious Giving (logged dollars)	4.7 (1.55)
Dummy for Nonreligious Giving	0.40 (0.49)

Standard deviations in parentheses. Observations: 29,104 except for the logged variables, there observations equal 11615 for non religious giving and 11725 for religious giving.

National Longitudinal Survey of Youth, 1979

Variable	Regression Sample
Had 6+ drinks at once in past month	0.33 (0.47)
Ever used Marijuana in life (1 = Y)	0.14 (0.35)
Ever used Cocaine in life (1 = Y)	0.11 (0.31)
Age	24.7 (4.4)
Sex (1 = female)	0.54 (0.50)

Standard deviations in parentheses. Observations: 22,332, except for the marijuana and cocaine responses (only available in 1984 and 1988); those observations are 6,879. See Part V of the text for more information on the sample of observations used for the NLSY.

Table 3: Church Attendance: Basic Index

	Basic	Time Trends	w/Placebo Dummy
Repeal Dummy	-0.209 [0.092]	-0.35 [0.121]	-0.222 [0.088]
Placebo Dummy	-	-	0.023 [0.146]
Age	0.014 [0.010]	0.014 [0.010]	0.014 [0.010]
Age squared ÷ 100	0.001 [0.001]	0.001 [0.001]	0.001 [0.001]
Dummy for Female	0.676 [0.034]	0.678 [0.032]	0.676 [0.033]
Dummy for White	-0.313 [0.208]	-0.305 [0.211]	-0.313 [0.207]
Dummy for Black	0.468 [0.261]	0.48 [0.265]	0.468 [0.261]
Dummy for Some College	-0.434 [0.044]	-0.434 [0.043]	-0.434 [0.044]
Dummy for High School	-0.741 [0.058]	-0.745 [0.058]	-0.741 [0.058]
Dummy for Drop Out	-1.273 [0.108]	-1.28 [0.109]	-1.273 [0.108]
Dummy for Married	0.517 [0.064]	0.518 [0.064]	0.517 [0.064]
State Percent Black	-0.067 [0.136]	0.363 [0.174]	-0.063 [0.152]
State Percent Foreign Born	-0.003 [0.025]	-0.142 [0.185]	-0.003 [0.049]
Rate of Insured Unemployment	-0.002 [0.048]	-0.001 [0.062]	-0.006 [0.064]
Disposable Income	-0.007 [0.062]	0.056 [0.093]	-0.002 [0.029]
Time Trends?	No	Yes	No
Observations	11720	11720	11720
R-squared	0.09	0.09	0.09

Robust standard errors in brackets. Regressions with time trends include linear and quadratic time trends for each state. All regressions include state dummies and year dummies. Dependent variable is measure of how often an individual attends church, ranging from 0 (never) to 8 (multiple times a week). The repeal dummy is set to unity once a state repeals its Blue Laws. The “Placebo dummy” is set to unity two years before the blue laws changed. Data are from the GSS.

Table 4: Blue Laws in the GSS: Extensions

Panel A: Regressions on Each Attendance Level Individually.

	More Than Weekly	At least Weekly	At least About Weekly	At least 2-3 times a Month	At least Once a Month	At least Several Times a Year	At least 1-2 Times a Year	At least more than Never
Repeal Dummy	-0.004 [0.010]	-0.039 [0.018]	-0.036 [0.016]	-0.038 [0.015]	-0.048 [0.016]	-0.029 [0.017]	-0.008 [0.011]	-0.008 [0.012]

Each column represents a separate regression. Coefficients are from a linear probability regression. In the first column, the dependant variable equals unity if an individual attends church more than weekly. In the second column the dependant variable equals unity if an individual attends *at least* weekly (i.e., either weekly or more than weekly). Thus, the measure of attendance used in each column subsumes all of the attendance measures in columns to the left.

Panel B: Church vs. Non Church Membership

	Church Membership	Membership other than Church Membership
Repeal Dummy	-0.041 [0.023]	0.01 [0.016]
Time Trends?	No	No
Observations	6323	6323
R-squared	0.1	0.14

The dependent variable in the second column equals unity if a respondent is a member a non-church organization such as fraternal orders, political clubs, sports clubs or hobby clubs. The sample size is somewhat smaller than before because this variable is not available in all years, and because this variables is missing for some of the observations used in Table 3.

Table 5: Blue Laws and the CEX

Without Time Trends

	Religious Giving			Other Giving		
	Total Giving	Log of Giving	Giving Dummy	Total Giving	Log of Giving	Giving Dummy
Repeal Dummy	-109.9	-0.133	-0.019	46.51	0.073	0.001
(state/year clusters)	[47.78]	[0.080]	[0.023]	[16.21]	[0.068]	[0.027]
(state clusters)	[95.17]	[0.101]	[0.053]	[14.35]	[0.045]	[0.01]
Time Trends?	No	No	No	No	No	No
Observations	29104	11725	29104	29104	11615	29104
R-squared	0.09	0.16	0.09	0.07	0.17	0.11

With State Time Trends

	Religious Giving			Other Giving		
	Total Giving	Log of Giving	Giving Dummy	Total Giving	Log of Giving	Giving Dummy
Repeal Dummy	-129.45	-0.081	0.0177	31.02	0.096	-0.028
(state/year clusters)	[83.08]	[0.156]	[0.035]	[27.12]	[0.124]	[0.043]
(state clusters)	[74.96]	[0.18]	[0.046]	[30.44]	[0.087]	[0.050]
Time Trends?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	29104	11725	29104	29104	11615	29104
R-squared	0.09	0.16	0.10	0.07	0.17	0.11

Standard errors in brackets. Regressions include controls for age, age squared, gender, race, educational attainment, and marital status, as well as for interview month. Data include 12 states over the period 1980-1998 (see text). Regressions omit observations in states the year that blue laws changed.

Table 6: Blue Laws and Total Church Expenditures

Panel A: All denominations together

	No Membership Control	Basic	w/Trend	w/Placebo
Repeal Dummy	-0.063 [0.020]	-0.059 [0.028]	-0.059 [0.018]	-0.07 [0.026]
Placebo Dummy	-	-	-	0.019 [0.041]
Control for Log of Membership?	No	Yes	Yes	Yes
State Time Trends?	No	No	Yes	No
State-by-Denomination Dummies?	Yes	Yes	Yes	Yes
Year-by-Denomination Dummies?	Yes	Yes	Yes	Yes
Observations	1381	1381	1381	1381
R-squared	0.91	0.92	0.96	0.92

Each column represents a different regressions

Panel B: Dropping each denomination from the sample

	No Membership Control	Basic	w/Trend	w/Placebo
No Lutheran Church-Missouri Synod	-0.064 [0.021]	-0.056 [0.033]	-0.074 [0.019]	-0.071 [0.028]
No Southern Baptist Convention	-0.031 [0.029]	-0.019 [0.034]	-0.051 [0.026]	-0.042 [0.028]
No United Church of Christ	-0.073 [0.020]	-0.068 [0.028]	-0.067 [0.021]	-0.077 [0.028]
No United Methodist Church	-0.055 [0.021]	-0.055 [0.020]	-0.066 [0.021]	-0.069 [0.024]

Each cell represents a different regression.

Dependent variable is log of total church expenditures, per member. Regressions are weighted by church membership. All regressions use state-by-denomination and year-by-denomination dummies and drop the year the laws changed. The unit of observation is all the churches in a given denomination, in a given state and year.

Table 7: Blue Laws and Risky Behavior

Panel A: Simple Difference-in-Difference Estimates

	Drinking All Years	Drinking in 1984 and 1988	Marijuana	Cocaine
Repeal Dummy	0.014 [0.017]	-0.018 [0.018]	0.032 [0.017]	-0.034 [0.034]
Observations	22332	6879	6879	6879
R-squared	0.15	0.44	0.07	0.08

Panel B: Blue laws effect by attendance

	Drinking All Years	Drinking in 1984 and 1988	Marijuana	Cocaine
High Attendance*Repeal Dummy	0.065 [0.022]	0.116 [0.046]	0.103 [0.033]	0.041 [0.027]
Some Attendance*Repeal Dummy	0.033 [0.016]	0.096 [0.029]	0.065 [0.033]	0.043 [0.024]
State Dummies?	Yes	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes
State-by-Year Dummies?	Yes	Yes	Yes	Yes
Full Set of Individual Controls?	Yes	Yes	Yes	Yes
Observations	22332	6879	6879	6879
R-squared	0.15	0.44	0.07	0.08

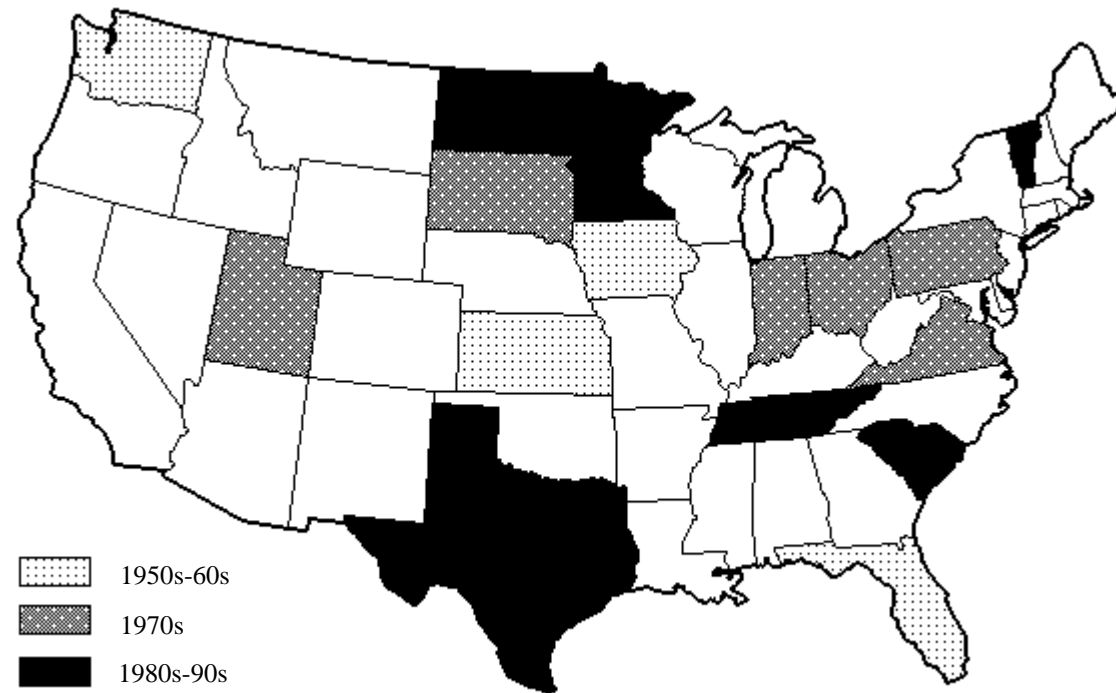
In the drinking regressions, dependent variable is unity if respondent has had six or more drinks in one sitting in past 30 days. In marijuana and cocaine regressions, dependent variable is unity if respondent has ever tried marijuana in the past 30 days and cocaine in their lifetime, respectively. Drinking data are available for the years 1982, 1983, 1984, 1985, 1988, 1989, and 1994. Marijuana and cocaine data are available in the years 1984, and 1988. Regressions omit observations in states the year that blue laws changed. Standard errors are clustered at the state level. Regressions include controls for age, age squared, gender, race, educational attainment, and marital status,

Table 8: Blue Laws and Drinking: More Interactions

	Drinking All Years	Drinking in 1984 and 1988	Marijuana	Cocaine
High Attendance*Repeal Dummy	0.054 [0.020]*	0.098 [0.045]	0.094 [0.032]	0.045 [0.028]
Some Attendance*Repeal Dummy	0.025 [0.018]	0.082 [0.028]	0.059 [0.033]	0.047 [0.026]
Married*Repeal Dummy	-0.005 [0.019]	0.027 [0.020]	0.016 [0.015]	-0.054 [0.012]
College Graduate*Repeal Dummy	-0.005 [0.022]	0.030 [0.011]	0.009 [0.028]	0.037 [0.015]
Women*Repeal Dummy	0.045 [0.027]	0.076 [0.012]	0.031 [0.024]	-0.02 [0.017]
Black*Repeal Dummy	0.033 [0.026]	0.014 [0.032]	0.018 [0.033]	-0.015 [0.025]
State Dummies?	Yes	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes
State-by-Year Dummies?	Yes	Yes	Yes	Yes
Full Set of Individual Controls?	Yes	Yes	Yes	Yes
Observations	22332	6879	6879	6879
R-squared	0.15	0.15	0.07	0.08

Standard errors in brackets. Data is from the NLSY; see Table 7 for more details.

Figure 1: Timing & Distribution of Blue Laws



Only one state (Iowa) repealed its blue laws in the 1950s, and only one state (North Dakota) repealed its laws in the 1990s. See Table 1 for more details.