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THE CHURCH VS. THE MALL: WHAT HAPPENS WHEN RELIGION FACES INCREASED SECULAR COMPETITION?

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ABSTRACT

Recently economists have begun to consider the causes and consequences of religious participation. An unanswered question in this literature is the effect upon individuals of changes in the opportunity cost of religious participation. In this paper we identify a policy-driven change in the opportunity cost of religious participation based on 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 Many states have repealed these laws in recent years, raising the opportunity cost of religious participation.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, under fairly general conditions, 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 nonreligious organizations after a state repeals its laws. We then assess the effects of changes in these laws on drinking and drug use behavior in the NLSY. We find that repealing blue laws leads to an increase in drinking and drug use, and that this increase is found only among the initially religious individuals who were affected by the blue laws. The effect is economically significant; for example, the gap in heavy drinking between religious and non religious individuals falls by about half after the laws are repealed.

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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] Religious organizations in the United States and throughout much of the world face competition from other religions, and a large literature has focused on the nature of religious market structure and religious competition.[2] Yet religion also exists in constant competition with other secular demands on individual time, from work to leisure. There has been little work modeling the nature of the competition between religious participation and other secular activities, but understanding this nexus of the secular and religious worlds is crucial for drawing normative conclusions about economic factors which might impinge on both.

In this paper, we pose a simple test of the effect of secular competition on religious participation: the removal of restrictions on Sunday shopping through the repeal of the so-called "blue laws." In many U.S. cities and states it was traditionally against the law to pursue commerce on Sunday, the day of religious observance for the vast majority of U.S. citizens. The repeal of these laws in cities and states substantially increases 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 model that considers an individual allocating temporal and monetary resources between secular and religious activities. Initially, the presence of blue laws restricts individuals' ability to pursue secular activities on Sundays. 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 more secular consumption. Second, there is an ambiguous effect on religious contributions. On the one had incomes may rise due to new work

[1] See Iannaccone (1998) for information on the prevalence and determinants of religious participation.
[2] Examples include Barrow and McCleary (2005 and 2003), Finke and Stark (2005), McBride (2005), Montgomery (2003), and Voas, Olson, and Crockett (2002).

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. The fact that our measure of competition comes from a policy-driven change in a state's laws allows us to clearly identify and model variation in competition, helping us surpass the empirical problems faced by prior work on inter-religious competition.

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 also 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. 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. Thus, secular competition does matter for religious participation: increased secular opportunities for work and leisure on Sundays lead to less time at church and lower religious contributions.

One additional 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 repealing blue laws leads to an increase in drinking and drug use behavior. To confirm the causal nature of these findings, we compare the impacts on individuals who were attending church before the law changes, and were therefore affected, with those

who were non-attendees, and we find the effects are concentrated in the former group. The effect is economically significant; for example the gap in heavy drinking between religious and non religious individuals falls by about half after the laws are repealed.

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 and drug use. 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.[3] 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 middle of the 20th century. These laws frequently prohibited "labor" or "all manner of public selling," but often made exceptions for acts of charity.[4] Blue laws prohibiting very specific types of activities, such as barbering or the sale of alcohol were also fairly common.[5]

In 1961 the Supreme Court issued a number of decisions on the constitutionality of blue laws.

[3] The discussion here draws primarily from Goos (2005) and Laband and Heinbuch (1987).
[4] 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.
[5] 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.

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).

Some economic work has considered the impact blue laws have on labor and profitability. Tullock (1975) makes the point that firms in communities with blue laws may see their profits fall as the laws are repealed, although Moorhouse (1984) presents empirical evidence which contradicts Tullock's hypothesis. Gradus (1996) shows that in theory changes in blue laws may lead to an increase in employment. Using multiple datasets, Goos (2005) provides evidence that repealing blue laws generally had a positive impact on employment in industries which had been subject to the laws, although this may have been offset to some extent by falls in employment elsewhere. No work has examined the relation between these laws and either religious participation or other behavioral measures.

To examine the effects of these repeals on religious participation, 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 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 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.[6]

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.[7] 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.

Moreover, the phenomena contributing to a state's repealing its blue laws are varied and somewhat idiosyncratic. The Supreme Court's 1961 decision set the groundwork for finding such laws unconstitutional, but legal challenges to blue laws occurred both before and after this ruling (Raucher, 1994). Some states, such as Vermont, Minnesota, and Utah, had laws which were repealed but were first the subject of court battles that lasted years. Some states, such as North Dakota, repealed their laws through legislative action that came immediately after court rulings. Other states, such as Texas, changed their laws in part because of lobbying by regulated industries. In some states a particular individual played a key role in repealing the laws. Blue laws in South Carolina were repealed in part because of the efforts of former governor Richard Riley (King, 1985), and blue laws were maintained in many communities in and around New Jersey through the efforts of the businessman Sydney Schlesinger (Lynch, 1978).

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[6] We have re-estimated the main regressions on each dataset below adding these western states in, and in every case the results are similar to those presented here.

[7] In comparison to the table of laws presented in Appendix A of Goos (2005), there are a few differences here. First, Goos identifies several states that still have laws on the books; most of these states (Alabama, Kentucky, Maine, New Hampshire, Oklahoma, Rhode Island, and West Virginia) are excluded from our analysis for other reasons—for instance, they either set their laws by county or they provide so many exceptions to the laws over time that it is unclear when and if the law stopped being enforced. For South Carolina, Goos states that laws are still on the books but that some counties can overturn these laws. However, the amendments to Section 53-1-5 of South Carolina's state code allowed businesses to open for most of the day Sunday, and subsequent amendments gave counties the ability to further *weaken* the law. Moreover, South Carolina exempted a number of industries from Sunday regulation entirely in 1985, although individuals were still allowed to object to working on Sundays without facing any penalty from their employers. Our categorization of South Carolina consequently differs from Goos'.

It is also difficult to generalize about the role of special interest groups in repealing blue laws. Retail establishments sometimes supported blue laws and sometimes did not. Often different retailers in the same state disagreed about the value of the laws (Hansard, 1985). Support for the laws could vary even among similarly-sized businesses in a state (Barmash, 1986), although small businesses were more likely to support the laws (Laband and Heinbuch, 1987). Labor unions have both argued for and against the value of blue laws (Merry, 1983). In a study that underscores the heterogeneous circumstances associated with blue laws' repeal, Price and Yandle (1987) examined what economic and social forces are correlated with the presence of state blue laws. Looking at 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.

While changes in blue laws may not be consistently associated with any particular socioeconomic trend, repealing the laws may not be an exogenous event. Repealing the laws may coincide with changes in economic conditions or other circumstances that lower a population's taste for "social capital." Or, it may be the case that declining levels of religiosity led to the laws being repealed, rather than the other way around. We address these concerns in the empirical section of the paper first by including a number of controls for a state's socioeconomic circumstances. Additionally, we also test for reverse causality and for blue laws coinciding with a decline in other types of social participation. These tests are described more in parts III, IV, and V of the paper. Part V, which documents the impact of blue laws on alcohol and drug use, also includes results from very strong specifications which control for all observed and unobserved phenomena varying across states in a given year.

Another concern is 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. However, for many states we were able to uncover newspaper stories and other evidence indicating the economic significance of changes in the laws (a sample includes McGee, 1991; Reinhold, 1985; The New York

Times, 1970; and the Associated Press, 1984). The following section theoretically explores how these changes in the laws may affect religious attendance and donations.

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 blue laws might affect individuals' decisions through two channels. First, they may constrain an individual's ability to work on Sunday. Second, they 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 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. We assume utility is increasing in inputs, concave, continuous, and differentiable. Additive separability in x_S allows us to avoid excessive machinery; its usefulness should become clear during the analysis. Importantly, we will not make any explicit assumptions about the relationship between t_R and x_R in the above utility function; this relationship has been a point of debate in the literature (Gruber, 2003).

The amount of available time on Sunday is set to unity, so $t_R + t_S \leq 1$. The individual earns a wage rate *w* by working on Sunday; thus wage earnings from Sunday are $w(1 - t_R - t_S)$. The individual's budget constraint is

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

where potential 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.[8]

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 Shleifer (2001) 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.

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

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

[8] 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.

where the first two terms are utility, the third is the budget constraint, and the last two terms are non-negativity conditions imposed by the blue laws.[9]

In a situation where there are no blue laws, the Kuhn-Tucker multipliers μ_S and μ_w are set to zero. Denote the *unconstrained* solutions in this case as x_R^*, x_S^*, t_R^*, and t_S^*. We assume these choice variables are strictly normal in potential income *I*, so that if $I' < I''$ then $x_R^*(I') < x_R^*(I'')$, and similarly for the other three choice variables.[10] We also assume that unconstrained solutions are differentiable in potential income. It could also be the case that blue laws are in place but individuals would choose the same outcomes regardless of whether the laws were in place or not. Then, μ_S and μ_w are again set to zero and the optimal solutions are the same as the unconstrained solutions. In this case, repealing blue laws will have no affect on an individual's optimum allocation of time and money.

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 time spent not working will decrease. The easily derived first order conditions show that in equilibrium the marginal rate of substitution between t_R and t_S is unity and 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.

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[9] 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.

[10] Recall that *I* reflects potential income, rather than just wage earnings. An increase in *I* might be construed as an increase in the value of all the resources (temporal and monetary) at an individual's disposal; it consequently seems reasonable to assume that the choice variables are normal in *I*.

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

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

Part (a) of Assumption 1 is satisfied by a number of well-known utility functions, such as a constant elasticity of substitution utility function. Part (b) of Assumption 1 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. Also, this condition allows the cross-partial derivative of utility between t_R and t_S to be negative, just so long as it is not more negative than $\frac{\partial^2 U}{\partial t_i^{\,2}}$.

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 the first order conditions and 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 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), than 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, instead, suppose utility can be represented as $\left(x_R^{1/2} + t_R^{1/2} + t_S^{1/2}\right)^2 + 2x_S$ 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 (in particular, additive separability), 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 technical assumption on the wealth expansion path of secular consumption. Again, this assumption is fairly weak:

Assumption 2: *In the unconstrained case, the marginal propensity to consume x_S^* does not converge to unity as potential income I goes to infinity.*

Assumption 2 ensures that as potential income *I* grows without bound that in the unconstrained case expenditures on all goods other than x_S also grow without bound.

It follows from Assumption 2, and the assumption that all goods are strictly normal in *I* in the unconstrained case, 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, the assumption that the unconstrained solutions are strictly normal and differentiable in *I*, 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 condition imposed by Assumption 2, it follows that $\frac{\partial t_R^{**}}{\partial \tilde{I}} > 0$ 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 the similar and equally reasonable assumption of their normality leads 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. Assumption 2 is important because it ensures that any observed outcome in the constrained case can be reproduced in the unconstrained case. This might not happen if total expenditures on donations and temporal goods in the unconstrained case asymptotically approached a finite amount as potential income grew. In such a situation, constraining x_S may force an individual to spend more on donations and temporal goods than they ever would otherwise, and in such a situation the response of these goods to a change in $\tilde{I}$ would be unclear.

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 work 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 affect 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. The next part of the paper begins the empirical analysis on the impact of blue laws on the allocation of time and money.

Part III: Secular Competition and Time Allocation

GSS Data and Empirical Methods

Our empirical 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 most years 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.

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).[11] Additionally, we drop data from a given state in the year the law changed. Given the sample period covered, we have 10 states with policy changes (Indiana, Minnesota, North Dakota, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Virginia, and Vermont), and 6 states that can serve as controls because policy didn't change in this period (Florida, Iowa, Kansas, Ohio, Utah and Washington)

Table 2 is a 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.[12]

We use these data to estimate models of the form

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

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, inflation-adjusted per-capita disposable income, and the statewide rate of insured unemployment); ϕ_j is a set of state dummies; and υ_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 standard errors at the state level; our standard errors are similar if clustered at the state/year level.

[11] 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.

[12] 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.

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 brackets under the coefficient is the state-clustered standard error. 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 column 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.

There is, however, a very large negative coefficient on the effect of attending weekly, which represents a 15 percent decline in the prevalence of that category. There are then no effects on attending about weekly, 2-3 times per month, or once per month, positive and significant effects on attending

several times a year or 1-2 times per year, and no effect on not attending at all. Thus, the results appear to indicate a shift down the distribution: those attending weekly move to lower attendance, raising the prevalence of nearby categories (such as about weekly), but individuals in those categories reduce attendance as well, leading to an offsetting decrease and no net effect, as well as an increased prevalence of rare attendance. The fact that there is no change in the "no attendance" category implies that individuals are not dropping out of church-going altogether, but rather that they are simply going less frequently. Of course, without panel data, we cannot prove that these shifts are occurring. Nevertheless, the most consistent interpretation of these results is that repeal of the blue laws does not induce casual church-goers to forgo attendance entirely, but rather serve 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 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. As mentioned earlier, 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 (a "placebo" dummy). 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.[13]

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. These findings are once again inconsistent with a spurious interpretation of our results on attendance.

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

[13] 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.

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 also 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; the collection of these data is described in much more detail in Gruber and Hungerman (2006). 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).[14]

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

[14] Data on denomination sizes came from the Glenmary Research Center (1974).

is Utah). For most of the states we have data for 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 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). 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. This is consistent with the notion that those attached to church scale back their participation, but do not sever ties completely. These estimates are significant when we cluster standard errors at the state/year level, but not so when we cluster at the state level.

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.[15] 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 the level of 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 only has a proxy for our variable of interest (donations), which is church spending. 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. Our model also includes the state-level controls used in the previous section. We weight the regressions by church membership in the denomination/state/year cell, and once again cluster the standard errors by state.

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 spending per member. This has no effect on the estimates.

The next two columns of panel A pursue the specification checks we have used throughout the

[15] 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.

paper: including state-specific linear trends and undertaking a falsification check by putting in a dummy which becomes unity two years *before* the repeal. Neither of these changes has much effect on 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 such 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. 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. If there is a link between participation and other outcomes, than changes in blue laws may impact other activities besides attendance and donations. Moreover, as highlighted by Table 4, changes in blue laws should have effects only for church-goers, and not for those who do not attend religious services.

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. We cannot investigate the effects of blue laws on outcomes such as drinking and drug use in the GSS 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 in1979, 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 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.[16]

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, and similarly for cocaine. Measures of marijuana/cocaine consumption are available in 1984, 1988, 1992 and 1994.[17]

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

[16] If attendance were available every year we could do a 2SLS analysis. However, only lagged measures of attendance are available and so 2SLS is not feasible.

[17] 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).

that repealing the blue laws leads to a rise in the odds of drinking of 1.5 percentage points, which is not significant. This represents about a 4 percent increase relative to the sample mean.

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 coefficient is again positive and significant, and the marginal effect is nearly the same as the sample mean.

We then turn to estimating models which interact the repeal dummy with a dummy for whether the individual attended church in 1979. The results from Tables 3 and 4 suggest that these individuals will attend church less when the blue laws are repealed. 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 attendees (that is, those reporting a level of attendance above "not at all") relative to those who are non attendees in the same state (attendees make up about 80 percent of the entire sample). 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, relative to those who do not. For example, for drinking, we find that attendees are 5.5 percent more likely to drink than those who don't attend, which is about 16 percent of the sample mean.[18] The coefficient represents about half of the difference in heavy drinking between attendees and non attendees.[19]

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 large,

[18] For the alcohol regressions, about 80 percent of the sample is of legal drinking age. While these regressions include controls for age, we have also verified the robustness of the regression to using only those of legal drinking age.

[19] The mean of the heavy drinking variable for attendees is 0.315, for non attendees it is 0.403.

suggesting an 11 percent increase for attendees relative to non attendees. These represent increases equal to most of the mean and essentially the entire gap in marijuana use between weekly attendees and non attendees.[20] For cocaine, the results are more modest but still large, with a roughly 3.6 percentage point effect on both very and somewhat frequent attendees, which is slightly larger than the sample mean and again essentially closes the gap in cocaine use between attendees and non attendees (although the standard errors are somewhat large). Clearly there are strong effects of repeal on the most frequent attendees, relative to those with low or no church attendance.

Are these results evidence of a causal effect on religious participation? The regressions in Table 7 are very strong in that they include state-by-year dummies that will net out any statewide impact of the laws, or any statewide phenomenon associated with the laws' repeal. But 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 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 effect on our estimates of interest; it appears that we are picking up the differential effect on participation and not some other correlate.

Part VI: Conclusions

The results presented above confirm the important 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

[20] The average for the marijuana variable for attendees and non attendees is 0.09, and 0.18, respectively. For cocaine the respective means are 0.016 and 0.03.

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, 2006; Hungerman, 2005). Moreover, this also shows that religious participation clearly *is* affected by competitive forces, and that secular opportunities do compete with religious opportunities for temporal and monetary resources.

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.

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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, and/or information on local laws not available (20 states): Alaska, Alabama, Arkansas, Connecticut, Delaware, Georgia, Hawaii , Kentucky, Louisiana, Maryland, Michigan, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Jersey, North Carolina, Oklahoma, 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

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

[†] The main regressions in the paper are similar if these states are included (see text).

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: 11,720. The regression sample includes Catholics and Protestants, and excludes respondents surveyed the year a state repealed its laws. For the basic results attendance is measured by an index (see text).

Consumer Expenditure Survey

Variable	Regression Sample
Religious Giving (dollars)	434 (1142)
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 11,615 for non religious giving and 11,725 for religious giving. Excludes the year a state repealed its laws.

National Longitudinal Survey of Youth, 1979

Variable	Regression Sample
Had 6+ drinks at once in past month	0.33 (0.47)
Used Marijuana past month (1 = Y)	0.11 (0.31)
Used Cocaine in past month (1 = Y)	0.02 (0.13)
Age	24.7 (4.4)
Sex (1 = female)	0.54 (0.50)

Standard deviations in parentheses. Observations: 22,470, except for the marijuana and cocaine responses (available in 1984, 1988, 1992 and 1994); those observations are 12,318. Excludes the year a state repealed its laws. 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	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 per-capita	-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	Weekly	About Weekly	2-3 times a Month	Once a Month	Several Times a Year	1-2 Times a Year	more than Never
Repeal Dummy	-0.004	-0.035	0.002	-0.002	-0.01	0.019	0.021	0.004
	[0.010]	[0.014]	[0.011]	[0.013]	[0.008]	[0.009]	[0.008]	[0.006]

Each column represents a separate regression. Coefficients are from a linear probability regression. In the first column, the dependent variable equals unity if an individual attends church more than weekly. In the second column the dependent variable equals unity if an individual attends weekly, and similarly for all the other categories. Regressions include all the regressors shown in Table 3.

Panel B: Church vs. Non Church Membership

	Church Membership	Membership other than Church Membership
Repeal Dummy	-0.041	0.01
	[0.023]	[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 variable is missing for some of the observations used in Table 3. Regressions include all the regressors shown 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, marital status, state percent black, state percent foreign born, inflation-adjusted per-capita disposable income, the statewide rate of insured unemployment, and dummies 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 regression.

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. Regressions include state percent black, state percent foreign born, inflation-adjusted per-capita disposable income, and the statewide rate of insured unemployment. All regressions use state-by-denomination and year-by-denomination dummies. Regressions omit observations in states the year that blue 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	Marijuana	Cocaine
Repeal Dummy	0.015 [0.017]	0.032 [0.013]	0.022 [0.008]
Observations	22470	12318	12318
R-squared	0.14	0.07	0.02

Panel B: Blue Laws' Effect by Attendance

	Drinking	Marijuana	Cocaine
Attendee*Repeal Dummy	0.055 [0.012]	0.107 [0.030]	0.036 [0.020]
State Dummies?	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes
State-by-Year Dummies?	Yes	Yes	Yes
Full Set of Individual Controls?	Yes	Yes	Yes
Observations	22470	12318	12318
R-squared	0.14	0.07	0.02

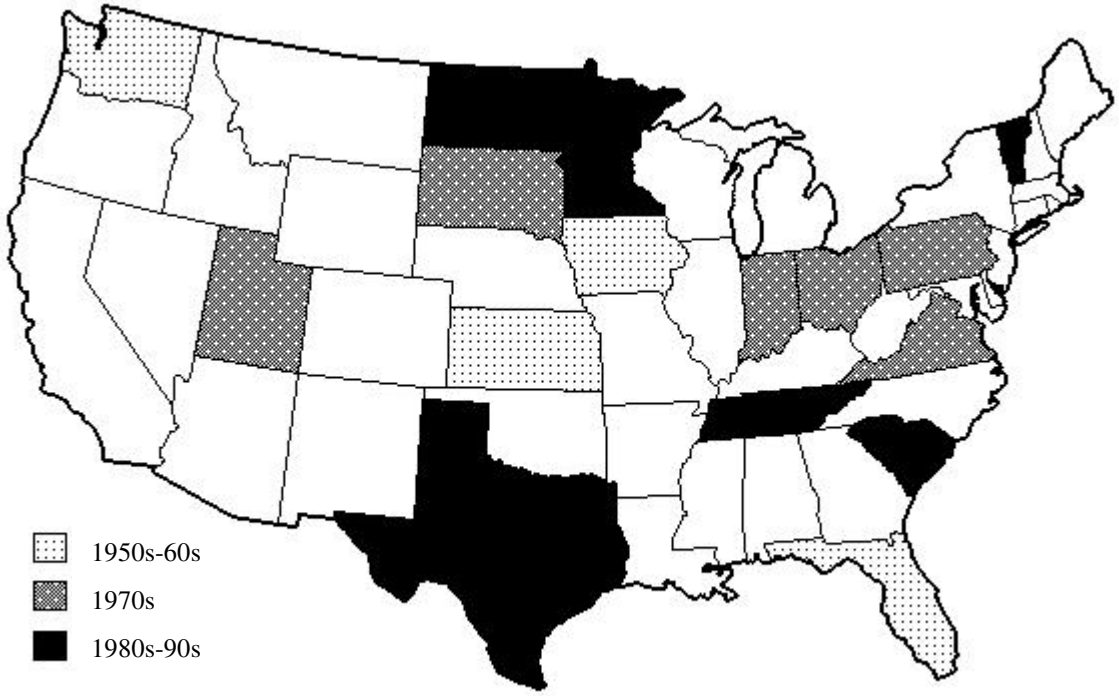
In the drinking regressions, dependent variable is unity if respondent has had six or more drinks in one sitting in past 30 days. In marijuana and cocaine regressions, dependent variable is unity if respondent has ever tried marijuana in the past 30 days and cocaine in the last 30 days, respectively. 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, 1988, 1992 and 1994. Attendees make up about 80 percent of the entire sample. A not-interacted dummy for attendees is also included in all of the above regressions. 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 Risky Behavior: More Interactions

	Drinking All Years	Marijuana	Cocaine
Attendee*Repeal Dummy	0.046 [0.014]	0.097 [0.029]	0.037 [0.021]
Married*Repeal Dummy	-0.005 [0.020]	0.025 [0.011]	-0.003 [0.006]
College Graduate*Repeal Dummy	-0.005 [0.019]	0.019 [0.029]	0.008 [0.005]
Woman*Repeal Dummy	0.041 [0.028]	0.054 [0.025]	-0.02 [0.005]
Black*Repeal Dummy	0.036 [0.027]	-0.014 [0.026]	0.015 [0.007]
State Dummies?	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes
State-by-Year Dummies?	Yes	Yes	Yes
Full Set of Individual Controls?	Yes	Yes	Yes
Observations	22470	12318	12318
R-squared	0.14	0.07	0.02

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.