Does church attendance cause people to vote?

Using blue laws' repeal to estimate the effect of religiosity on voter turnout

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This Version: May 2010

Abstract

Regular church attendance is strongly associated with a higher probability of voting. It is an open question as to whether this association, which has been confirmed in numerous surveys, is causal. We use the repeal of the laws restricting Sunday retail activity ("blue laws") to measure the effects of church-going on political participation. The repeal of blue laws caused a 5 percent decrease in church attendance. We measure the effect of blue laws' repeal on political participation and find that following the repeal turnout falls by approximately 1 percentage point. This turnout decline is consistent with the large effect of church attendance on turnout reported in the literature, and suggests that church attendance may have significant causal effect on voter turnout. We also find that the decline in turnout appears to affect Democratic but not Republican vote shares, and that the effect of blue laws appears to be stronger for Catholics than for others.

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* We thank Tracy Liu and in particular Gabriel Durazo for assistance in collecting the Blue Laws data, and Shang Ha for research assistance. We also thank seminar participants at Notre Dame for comments. We are grateful to James Snyder, who supplied the election data. This work was supported by the Metanexus Institute and the Yale University Institution for Social and Policy Studies.

Introduction

For a large number of Americans, attending religious services is a routine and important part of life. On an average Sunday roughly one-fourth of the U.S. population attends religious services, and roughly half of the population attends religious services at least monthly.[1] Donations to churches and other religious organizations make up a plurality (and by some estimates a majority) of charitable contributions (Andreoni, 2006). Over two-thirds of Americans belong to a church or other religious organization (Iannaccone, 1998). Despite the broad reach and clear importance of religious observance, there has been relatively little progress in measuring how church attendance shapes the choices people make and the attitudes they hold.

There are strong correlations between the degree of religious observance and a wide variety of pro-social behaviors and positive health outcomes. For example, there is a well-known positive association between attending religious services and political participation; those reporting regular church attendance are much more likely to vote (e.g. Rosenstone and Hansen 1993; Verba, Schlozman, and Brady 1995). Prior work has found that, controlling for socioeconomic characteristics, age, gender, and political conditions, those who report attending church weekly are between 10 and 15 percentage points more likely to vote, a difference roughly equal to the gap in turnout between a presidential and midterm election.

It is unclear how these correlations between religiosity and various outcomes should be interpreted. Do these associations measure the causal effect of church attendance, or do they capture long and short run differences in those who choose to attend church and those who do not? In the case of church attendance and voting, it is quite plausible that a person who enjoys participating in church life (an activity which involves listening to speeches, meeting with others, volunteering, and organizing) would also enjoy participating in politics. Short-run factors may be at work as well. Those who are new to an area may have less religious involvement and it is well established that on average people who have recently moved are less likely to vote. A larger point is that even a seemingly robust catalogue of specific

[1] Figures based on calculations of GSS data from 1973 to 1998.

conjectures about how churchgoers may differ from non-churchgoers runs the risk of overlooking important sources of difference.

We measure the effect of church attendance by observing the consequences of the decline in church attendance that followed a policy change. In recent decades, long-standing restrictions on Sunday retail activity, often referred to as “blue laws,” were repealed. In a recent paper, Gruber and Hungerman argue that a consequence of permitting Sunday morning shopping was to reduce the relative appeal of Sunday morning church attendance. They provide compelling evidence that there was a notable decline in church attendance following the repeal of blue laws (Gruber and Hungerman, 2008).

We extend this earlier work to examine whether the repeal of the blue laws was also associated with a decline in voting in presidential and mid-term elections, which is what is predicted to occur if church attendance promotes political participation. We find that the repeal of the blue laws resulted in an approximately one percentage point fall in the percentage of the population that turns out to vote. Additionally, there is little evidence that the repeal of blue laws was preceded by a decline in voter participation or a decline in religious participation; the results here are not driven by “reverse causality.” We also find that the decline in turnout appears to affect Democratic but not Republican vote shares, and that the effect of blue laws appears to be stronger for Catholics than for others.

These findings have implications for the larger question of how citizen engagement in voluntary associations affects society. Citizen involvement in religious organizations, unions, civic groups, and clubs is often credited with creating networks of communication and fostering trust and reciprocity among members of society. The “social capital” created by such organizations is cited by some as an important determinant of the quality of political and economic performance (e.g., Putnam 2000). If church attendance is determined to have a causal effect on political participation, this would provide a valuable example of how participation in voluntary organizations has a causal effect on the political sphere as social capital theorists maintain.

Establishing whether church attendance has a causal effect on participation also has implications for our understanding of mass politics and for evaluating the full range of consequences that follow from public policy toward religious organizations. For instance, one important feature of churches is that their membership is not concentrated among the highest socioeconomic strata, and so a genuine mobilizing effect from church attendance might counteract some of the class biases in political participation (Brady, Schlozman, Verba, and Luks 2008).

Our paper proceeds as follows. Section 1 reviews the literature linking religiosity and political behavior. Section 2 discusses the history of blue laws and the identification strategy. Section 3 presents the estimation results. Section 4 discusses the implications of our findings, some of their limitations, and directions for future investigation.

Section 1. Literature Review

There is a large body of work documenting correlations between church attendance and various pro-social behaviors. In particular, attending church has been linked to: lower levels of criminal activity (Evans, Cullen, Dunaway and Burton, 1995; Lipford, McCorkmick, and Tollison, 1993; Hull and Bold, 1995), lower rates of delinquency (Bachman et al. 2002; Johnson et al 2000.; Wallace and Williams, 1987), lower rates of substance abuse (National Center on Addiction and Substance Abuse, 2001), better health status and outcomes (Levin and Vanderpool, 1987; Hummer, Rogers, Nam, and Ellison, 1999), and greater marital stability (Lehrer and Chiswick, 1993). Religiosity is also strongly correlated with self-reports of well-being (Ellison 1991, Hout and Greeley 2003), and recent work has found that differences between those reporting never attending church and those reporting attending church weekly is comparable to the boost in happiness from moving from the bottom to the top income quartile (Gruber and Mullainathan, 2002).

Voter turnout is also strongly associated with religious observance. There is a robust and large positive association between turnout and citizens' frequency of church attendance. Rosenstone and Hansen (1993) pool survey data from several decades of American National Election Studies and regress turnout on reported church attendance and a collection of additional variables, including age, income, gender, and education. They estimate that those who report attending church every week or almost every week are 15.1 percentage points more likely to report voting in presidential election and 10.2 percentage points more likely to report voting in mid-term elections than those who say they do not attend religious services (Rosenstone and Hansen, 1993, Tables D-1 and D-4). Verba, Schlozman, and Brady (1995) perform a similar analysis, predicting turnout in a larger set of elections. Using the data from the 2,500 respondent Citizen Participation Study survey, Verba, Schlozman, and Brady confirm the strong correlation between church attendance and turnout (Verba, Schlozman, and Brady 1995).[2] Other research has focused on the relationship between church going and turnout for particular ethnic or racial subgroups. A positive relationship between religious participation and voting has been demonstrated for Asian Americans (Wong et al, 2005), Latinos (Jones-Correa and Leal, 2001), Muslims (Jamal, 2005), and African Americans (Harris 1994, Alex-Assensoh and Assensoh, 2001).

There are two main explanations for how church attendance might cause greater voter turnout. First, participation in a church builds civic skills and thereby increases a citizen's capacity for participation (e.g. Verba, Schlozman, and Brady 1995). Those who attend church have opportunities to interact with and to work with others, and may participate in decision making processes regarding church affairs, plan meetings, or give speeches. These activities help develop general civic skills that might aid political involvement outside of church. Second, church members are part of a community and there are political by-products of this civic association (Jones-Correa and Leal 2001, Wald, Owen, and Hill 1998). As such, churches are "important conduits of political information and recruitment" (Jones-Correa and Leal, 2001, p754). Church members are exposed to information about community affairs as well as explicitly political messages. Churches may also be used for political mobilization, through the distribution of voting guides or other political material, and members may be especially responsive to requests to participate made by other church members or the church leadership (Green, Rozell, and Wilcox 2001; Guth, Kellstedt, Green, and Smidt 2002; Guth, Beail, Crow, Gaddy, Montreal, Nelson,

[2] For additional research on the link between church attendance and political participation see also Peterson (1992), Wald (1992), Wald, Kellstedt, and Leege (1993), Smidt, den Dulk, Penning, Monsma, and Koopman (2008)).

Penning, and Walz 2003; Campbell 2004). These requests may be especially effective due to social pressure to participate in political causes that are of concern to the church community.

Recent work on voter turnout provides some evidence to support both of these channels of influence. For example, turnout behavior appears to be relatively malleable. Randomized assignment field experiments have shown that some common mobilization tactics, such as pre-election door to door canvassing, can increase turnout substantially (Gerber and Green, 2000). The initial study in the most recent wave of scholarship, conducted in New Haven in 1998, showed an 8 percentage point average treatment on treated effects and results of this magnitude have been supported by most subsequent studies of canvassing (Green and Gerber, 2008). Encouragements to vote might be delivered personally by church members and so might be similarly effective.

Moreover, voter turnout appears to be highly sensitive to even small amounts of social pressure. Members of a church congregation are likely to be reminded about the upcoming election during church services, with the clear implication if not the explicit injunction to vote. The social pressure exerted by public encouragement to adhere to the norm of voting may be effective at increasing turnout. Work in social psychology has demonstrated that social pressure can induce compliance with behavior that is supported by social norms (Cialdini and Goldstein 2004, Cialdini and Trost 1998, Scheff 2000). Recent experimental studies on the effect of social pressure on voter turnout confirm these findings. For example, in a recent field experiment, Gerber, Green, and Larimer examined the effect of different pre-election mailings on the probability a subject voted. Two of the mailings reminded voters that whether they voted or not is a matter of public record. They found these mailings caused over a 5 percentage point increase in the voting rate, approximately 10 times the effect of a typical political mailing (Gerber, Green, and Larimer, 2008). Other experimental work has shown that the more personal an encouragements to vote is, the more effective it is at producing higher turnout (Gerber and Green, 2000; Green and Gerber, 2004).

At the same time, efforts to measure the causal effects of church going are hampered by the non-random nature of differences in church attendance across individuals. While church attendance may be causing the higher turnout reported in observational studies, it is also possible that those who are more likely to be politically active are the very individuals who are likely to attend church. For example, individuals may have a fixed desire for social participation that extends to all arenas, including both political and religious participation. In such a case, the positive correlation observed in other studies may reflect this omitted third factor.[3] This difficulty is compounded by the fact that the most influential work examines correlations between survey measures of church attendance and turnout. Survey work suffers from various forms of measurement error. If those who attend church or report they attend church also exaggerate their pro-social behavior, the relationship between religious attendance and pro-social behavior will tend to be biased upward.

While aware of this difficulty, scholars who specialize in political behavior often interpret the correlation between church-going and turnout as evidence church attendance causes turnout. When the difficulty of drawing causal measurements in this area is explicitly considered, scholars assume that the available control variables are adequate to eliminate concerns (Rosenstone and Hansen, 1993, p172, Huckfeldt and Sprague, 1993; Verba, et al 1995).[4] However, some differences between churchgoers and others (such as tastes for organizational involvement, a tendency toward habitual behavior, or an affinity for religious beliefs and practices) may be difficult to observe. Thus, adding control variables may not fully address whether the association between attendance and voting reflects these omitted factors. Some more recent work is agnostic about whether the correlation between church attendance and voting ought to be interpreted as causal. A review of survey evidence demonstrating the strong positive association between church attendance (as well as union membership) and political participation concludes that "much more work is needed to determine whether the 'effects' we find are simply the result of confounds (such as the possibility that those with a sense of duty are more likely to join both churches and unions

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[4] For instance, Verba et al. discuss the possibility of a spurious correlation at length, but ultimately dismiss the plausibility and relevance of objections to ascribing a causal interpretation to the association between religious involvement and political activity. They note that empirically it is not simply being affiliated with an institution, but how actively the individual is engaged that matters for political participation (p279). This argument does not address the possibility that a taste for participation is expressed in both the extent of involvement in institutions, on one hand, and the extent of involvement in politics, on the other.

and such people also participate in politics at higher rates) or real mechanisms...” (Brady, Schlozman, Verba, and Luks, 2008).

Our discussion has focused on the large literature demonstrating a consistently positive relationship between turnout and church involvement as measured by attendance, but some recent work uses additional measures of church involvement. Driskell, Embry, and Lyon measure weekly church attendance and also construct two additional variables measuring church related activity: an index of involvement in church activities, such as participation in choir or religious education programs, and a measure of participation in church leadership (Driskell, Embry, and Lyon 2008). They include all three of these measures as independent variables in regressions explaining their political participation index (the index is based on ten different participatory acts, including voting, contributing money, participating in a protest, etc.) and find that, controlling for church leadership and participation in church activities, there is no statistically significant relationship between church attendance and the participation index. Because the extent of involvement in various church activities or the decision to seek a leadership role in church may be related to omitted variables correlated with political participation, the concern for omitted variable bias between churchgoers and others extends to studies employing multiple measures of church involvement. Nevertheless, the arguments in Driskell, Embry, and Lyon’s work suggest that studies constrained to employ church attendance as the participation measure, such as our study and previous studies, should consider church attendance as a proxy variable for more nuanced indicators of church involvement.

The strategy that we will pursue in this paper is to use a policy change, repeal of the blue laws, as a shifter of religious participation that may impact voting. Several recent studies have used policy changes to study voter turnout. Dee (2004) and Milligan, Moretti, and Oreopolous (2004) measure the effect of education on voter turnout using the change in educational attainment caused by compulsory education laws and changes in child labor laws; they conclude that education has a positive effect on voter turnout in the United States. Milligan, Morettie, and Oreopolous note that their methodology does not allow them to exhaustively explore the *mechanisms* through which education and voting are related;

the same is true here. But our study *will* explore whether the relationship between religion and voting is largely driven by omitted characteristics or whether the two causal mechanisms mentioned above are of foremost importance. The next section discusses the history of blue laws in more detail.

Section 2: A Brief History of Blue Laws

This section provides some background on blue laws in the United States. For more information, readers should consult Goos (2005), Laband and Heinbuch (1987), and Gruber and Hungerman (2008).

Blue laws, also called Sunday closing laws, are laws which restrict various activities on the Sabbath. Such laws have been fairly common throughout the nation's history. All of the original colonies had Sunday closing laws, and every state had at least some law prohibiting certain activities on Sunday. By the mid 1900s, over 30 states had laws prohibiting retail activity on Sundays. These laws frequently prohibited "labor" or "all manner of public selling," but often made exceptions for acts of charity.[5] These general statewide prohibitions on retail activity will be the focus here.

In 1961 the Supreme Court issued a key decision regarding the constitutionality of blue laws in the case *McGowan v. Maryland*. The court upheld the constitutionality of blue laws, but stated that they could be found unconstitutional if their classification of prohibited activities rested "on grounds wholly irrelevant to the achievement of the State's objective." After the ruling, blue laws began to be challenged on the basis that they failed this constitutional test (Theuman, 2005). These challenges were often successful since blue laws could be confusing in their classification of what activities were allowed and what activities were not. For example, in Texas it was possible to sell hammers, but not nails, on a Sunday (King, 1985). In the decades following this ruling, most states repealed their blue laws either through judicial or legislative action (Goos, 2005).

To study these laws, we gathered information on each state's blue laws from the 1950s until the present. We identified states that witnessed a discreet and significant statewide repeal in the prohibition

[5] States sometimes also exempted certain retail activities, for instance by allowing pharmacies to stay open on Sunday.

of retail activity on Sundays. Some states' laws were (or are) decided at the county or city level, making collection of these data infeasible.[6] A few states were not used because we could not verify the exact time that the laws were repealed, or because the states gradually added exceptions to their laws over time, making it difficult to assess in any particular year whether the laws in place could be regarded as effective.[7] Eight western states (Arizona, California, Colorado, Idaho, Nevada, New Mexico, Oregon, and Wyoming) never had any retail blue laws during this time period. Since these states do not directly contribute to identification in the results that follow, one would hope that their inclusion is irrelevant. We investigate whether the results are sensitive to including these western states below.

Panel A of Table 1 lists the usable states and the year when their laws were repealed, either by judicial action or act of the legislature. The states with usable laws make up a fairly diverse group. While there are relatively few states in the west and in New England, we nonetheless have state representation in all areas of the country, and there is no clear pattern in the timing of when laws are repealed in any given part of the country. We are also optimistic that results from this sample would be generalizeable to other areas. First, our sample (including the western states listed below Panel A of Table 1) includes nearly half of the states in the U.S. and slightly over half the U.S. population. Second, the characteristics of GSS respondents in our set of states are very close to other states; for example average age (45.9 in our sample versus 45.3 in other states), fraction female among GSS respondents (0.58 versus 0.57), or fraction with a high school degree (0.33 versus 0.33) are all very similar.

Even if the collection of states appears reasonably diverse and the timing of repeal appears nonsystematic, there are a few other important questions concerning the use of these laws. First, one may wonder if these laws were enforced before their repeal. If they were not enforced, then their repeal would not have an effect on religious or voting behavior and this will bias us away from finding an impact of these laws. Fortunately, we were able to uncover newspaper stories and other evidence indicating the

[6] These states include 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.
[7] These states include Illinois, Massachusetts, Maine, New York, West Virginia, and Wisconsin

significance of changes in the laws for a number of states (e.g., Merry, 1983; McGee, 1991; Hansard, 1985; The New York Times, 1970; and the Associated Press, 1984).

Second, one may wonder whether the timing of blue laws' repeal is coincident with other phenomena. For example, it might be the case that declining levels of civic participation lead to the laws being repealed. We address this concern in the empirical section of the paper. But fortunately it appears that the phenomena contributing to a state's decision to repeal its blue laws are varied and state-specific. Some states repealed their laws only after court battles that lasted years. Other states changed their laws by legislative action without court involvement.

It is also hard to generalize about the role of special interest groups in repealing blue laws. Some retail establishments supported blue laws while others did not. 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 supported and argued against Sunday closing laws (Merry, 1983). Price and Yandle (1987) investigate the economic and social forces associated with the repeal of these laws. After considering the share of women in the workforce, the presence of labor unions, a state's political makeup, and various other state socioeconomic characteristics, they do not find any covariates consistently associated with the presence of blue laws.

In sum, we focus on states where we can identify a significant change in statewide prohibitions of retail activities on Sunday; these laws create immediate and significant changes in the opportunity cost of religious participation. A number of diverse states have witnessed such a change; there does not appear to be any systematic pattern in the timing or location of the law changes in these states. Prior research has failed to identify social or economic factors that are consistently related to the repeal of blue laws', and anecdotal evidence suggests that the factors leading to a state repealing its blue laws are varied. All of this suggests that changes blue laws create an empirically attractive change in the opportunity cost of religious participation.

Section 3: Specification and Results

This section presents empirical results on the impact of blue laws' on church attendance and voting. We begin first with a discussion of blue laws and attendance, using data from the GSS.

A. GSS Data and Empirical Methods

Our empirical analysis begins with examining blue laws' 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. We start by simply using 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. We also convert answers into estimated weeks of annual attendance (so for example we estimate that a person who attends every week attends 52 times a year).

Our sample covers the years 1973 to 1998. We consider individuals in the states with usable blue laws data listed in Table 1 (including western states which never had blue laws). We limit the sample to individuals who report their "religious preference" as Catholic or Protestant (nearly 90 percent of the sample), as these individuals are most likely to attend services on Sunday.[8] Additionally, we drop data from a given state in the year the law changed (as it is not clear how to categorize such cases).

Panel B of Table 1 reports 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 (monthly attendance is also the median response). The table

[8] We have also tried 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, but the sample was too small for the results to be regarded as reliable.

also shows that GSS somewhat over-samples female respondents.[9]

We use these data to estimate models of the form

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

where A_{ijt} is religious attendance for individual *i* in state *j* in year *t*; $Laws_{jt}$ is an indicator for whether 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. Following Bertrand et al. (2004), we cluster our standard errors at the state level.

The key coefficient is δ, relating the effect of repealing blue laws on attendance. As mentioned in Section 2, perhaps the greatest concern in interpreting this coefficient involves whether blue laws are correlated with other phenomena that might affect religiosity. For instance, certain parts of the country with traditionally high attendance may also traditionally favor blue laws, creating a spurious cross-sectional relationship between the laws and religious behavior. An advantage of our difference-in-difference approach is that we can address this concern by including state dummies, so that identification comes not from cross-sectional variation but instead from changes within states across time.

But even time-based variation may be problematic. For instance, religious behavior may be "trending down" in the United States over this period (cf. Norris and Inglehart, 2004), while at the same time blue laws were becoming less common; to the extent that these trends are merely coincident, estimates of δ could be misleading. To address this concern, the regressions use a set of year dummies that nonparametrically control for any relevant time-varying phenomena. (This set of year dummies would subsume a linear or quadratic time trend.)

[9] A small number of observations each year have missing values for variables and are omitted. We compared mean age and fraction female (when available) for omitted respondents to included respondents and they were very similar across the two groups.

Even this specification could be susceptible to a state-specific pre-existing trend, however. For instance, it may be the case that declines in social capital specific to one state over time lead to lower religious attendance, and also lead to a change in blue laws. We can test for the prevalence of this phenomenon using an "early repeal dummy" regression. This regression adds an "early" repeal dummy that goes from zero to unity in the years *before* a state repeals its blue laws; the early repeal dummy stays at unity thereafter. If changes in the laws are driven by pre-existing declines in civic participation or religiosity, then this "early" repeal dummy would be negative and significant, and/or it would attenuate the observed effect of the actual law change. If the results are robust to the inclusion of the early repeal dummy, however, it is evidence against this pre-existing trend concern.

The results from this analysis are presented in Table 2. The first column shows our basic difference-in-difference regression for the religious attendance index.[10] There is a statistically significant negative effect on religious attendance of blue law repeal. The result indicates that repealing the blue laws reduced attendance by 0.25 index points, a little over 5 percent of the sample mean. This is a sizeable effect: for example, it is half as large as the well-noted higher rate of religious attendance for married individuals. The rest of the column shows selected coefficients on other control variables; they are as expected.

Column 2 reports estimates when the dependent variable is estimated weeks of attendance per year.[11] We find that on average blue laws' repeal reduces attendance by a few weeks a year. The average number of estimated weeks of attendance is about 30, so the 2-week impact estimated in column 2 is about 6 percent of the mean. Since blue laws likely impact "marginal" churchgoers more than others, the 2-week estimate likely understates the drop in churchgoing observed by those affected by the repeal (although, with a repeated cross-section of data like the GSS, we cannot formally verify this). Both columns 1 and 2 point to a non-negligible impact of blue laws on attendance.

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[10] The term "difference in difference" refers to the fact that our coefficient is not identified by simple differences in religiosity over time, nor is it identified by simple cross-sectional differences in religion across states. Here, the regressions are identified by comparing state trends over time. See Wooldridge (2002) for a discussion of difference-in-difference estimation.

[11] For these results we estimate that individuals in the highest attendance category attend twice a week.

The next two columns test our estimates for reverse causality. In these columns we include in the model a dummy that goes from zero to unity starting two years *before* a state repeals its blue laws. If the blue law repeal coefficient 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 in all cases unchanged. The results here thus show that the repeal of blue laws led to a statistically and economically significant decline in religious attendance. In the next subsection, we see if repeal of blue laws also led to a decline in voter turnout.

The last two columns present an additional test for the concern that blue-laws repeal is correlated with state characteristics. First, using state-level data we ran a probit regression where the dependent variable was a dummy for whether a state repealed its blue laws; the right-hand side included a number of state characteristics.[12] From this regression we calculated the estimated probability that any state in any year would have repealed its blue laws. We then added this predicted probability of repeal to the right hand side of our attendance regressions. If blue laws' repeal is driven by states with similar characteristics repealing their laws around the same time, and if such a "diffusion" was correlated with attendance, than this new variable might eliminate the predictive power of the true law change.[13] The results are very similar to before; it is the true law change and not changes in state characteristics coincident with law changes that drive our results.

B. Specification and Estimation of Voting

In this section we examine how changes in blue laws and the resultant decline in religious participation impacts voter turnout. We use county-level data on voter turnout; the unit of observation is

[12] The right-hand side controls from the probit regression included the fraction of the state population that was black, the fraction foreign-born, the rate of insured unemployment, per-capita disposable income, the fraction of the state that was Southern Baptist in 1952, the fraction Methodist in 1952, the fraction Catholic in 1952, and a set of region-specific time trends. The regression sample included all useable states from 1950 to 2000.

[13] To some extent, the controls we have already included as regressors up until now address this concern, but the-probit based approach has the added benefit that it relaxes the assumption of a linear relationship between state characteristics and the likelihood of repeal.

thus a given county in a given year. Our key dependent variable is voter turnout for presidential elections between 1952 and 2000. The regression we estimate is:

$$turnout_{ct} = \delta repeal_{ct} + \beta X_{ct} + \phi_c + \theta_{ry} + \varepsilon \,.$$

Here $turnout_{ct}$ is the percent of the population voting in the presidential election in year *t* for county *c*, $repeal_{ct}$ is a dummy that equals unity if a state has repealed its blue laws (and zero otherwise), X_{ct} is a matrix of regressors, and the terms ϕ_c and θ_{ry} are county and region-by-year dummies (with the turnout data we have multiple repeals of blue laws in all four regions of the country, meaning that with θ_{ry} we will be able to exploit within-region variation in blue laws' prevalence across time for identification). We will measure $turnout_{ct}$ in both levels and logs. The key coefficient is δ, which captures how a change in blue laws (and thus the opportunity cost of religious participation) impacts voter turnout.

The regressors in *X* will help control for other determinants of voter turnout. These include dummy variables for whether senatorial and gubernatorial elections are being held in a given state and year, and the county's population (in both levels and logs). The population data come from the Decennial Censuses; we linearly interpolate each county's population for years between the censuses. We also include a measure of whether a state is a "battleground" state: the difference between the share of the state voting Democratic and the national share voting Democratic. We include the square of this "battleground" variable as well.

The key coefficient is δ, relating the effect of repealing blue laws on turnout. As with the attendance data, the regression uses a difference-in-difference approach where identification comes not from cross-sectional variation but instead from changes within counties across time. Thus the results will not be driven by spurious cross sectional correlation (e.g., parts of the country that traditionally have high turnout also traditionally favor blue laws) nor will they be driven by coincident time trends in religious and voting behavior. In particular, national or regional trends in turnout from whatever cause, including national or regional trends in religious observance, are accounted for by the region-year dummy variables.

We will also again consider the “early” repeal dummy specification which adds a dummy that goes from zero to unity starting the election before a state repeals its blue laws.

Most of the results which follow will use weighted regressions; this is for two reasons. First, as the dependant variable is essentially a measure of per-capita turnout, more populous counties are more informative and this should be exploited to improve the regression’s efficiency. Second, as shown below, the weighted model fits the data better.

Panel C of Table 1 gives means for some key variables. As shown in the table, we also have data on total votes cast for each presidential candidate; this will allow us to examine how changes in the opportunity cost of religious participation affect voter support for members of different political parties.

Table 3 presents results from our basic regression. All residuals correct for heteroskedasticity and are clustered at the state level. The dependent variable is the percent of the population voting in presidential elections from 1952 to 2000 (in levels). The first column presents our estimates of the baseline specification. The key coefficient, for whether blue laws have been repealed, is negative and significant. The coefficient suggests that the percent of the population voting in presidential elections falls by about one point after blue laws are repealed. This is a bit less than 3 percent of the mean of the dependent variable; the effect is thus reasonable but significant in magnitude.

Comparing magnitude of the effects in Table 3 to those in Table 2 is somewhat difficult since the bases are different. Roughly speaking, we find that attending church about 2.5 fewer weeks per year leads to a one percentage point decline in the odds of voting.[14] This result is remarkably consistent with Rosenstone and Hansen (1993), who find that attending nearly every week raises the likelihood of voting by 15 percentage points. In comparison, our estimates suggest that going from zero to fifty weeks of

[14] Readers familiar with instrumental-variables (IV) estimation could also interpret our findings from an IV framework. In particular, a Wald-estimate can be constructed by dividing the “reduced-form” estimates in Table 3 by the “first stage” estimates in Table 2. Dividing the coefficient in column 1 of Table 3 by the coefficient in column 2 of Table 2 yields a Wald estimate of -0.986/-2.456 = 0.40. The estimate suggests that an additional week of church attendance raises the likelihood of voting by a little less than half a percentage point. We also considered IV estimation using self-reported voting with GSS data; while point estimates were very similar to the coefficients reported here, the measurement error in the GSS data was extensive enough that the results were sometimes imprecise. See section 2 of Angrist and Evans (1998) for an example of the use of a Wald Estimate in an IV setting.

attendance a year would raise the likelihood of voting by about 20 percentage points; this result therefore seems compatible with a large but not implausible relationship between religious participation and voting.

Turning to the other regressors, one sees that the senatorial and gubernatorial election dummies are both insignificant. The last two variables capture the role of "battleground" states; the difference in the share of a state's vote for the Democratic candidate and the share of the national vote is negative and significant; the coefficient for the square of this difference is positive but small and insignificant. Together the coefficients suggest that "blowout states" (where the Democratic candidate was doing either especially well or poorly) have lower turnout than other states.

The second column reports results from an identical regression except that observations are not weighted by county population. The coefficient is once again negative and significant. It is reassuring that the relationship between voting and the cost of religious participation is similar regardless of whether weights are used. As suggested by the R^2, the weighted model fits the data better than the unweighted model.

The third column repeats the baseline estimation but only uses the states where blue laws have changed; the regression drops western states (listed below Panel A of Table 1) which never had blue laws. Since these western states do not directly contribute to the identification of the repeal dummy coefficient, their exclusion should not diminish the results. This turns out to be the case—the repeal dummy coefficient in the third column is similar to the coefficient in the first column.

The fourth column provides a test for whether our results are driven by pre-existing trends. The column includes the early repeal dummy that goes from zero to unity starting the election before a state repeals its blue laws; the early repeal dummy stays at unity thereafter. The coefficient on the early dummy is wrong signed, very small in magnitude, and insignificant. The result shows that voting turnout declined immediately after blue laws were repealed, not before. This mirrors the results from using the early-repeal dummy in regressions on religious participation in Table 2; together these results suggest that the estimates are not manifestations of pre-existing trends in social capital or religious participation.[15]

Column 5 tests for whether repeal is diffused among similar states and this diffusion confounds the results. Once again, when we add the predicted likelihood of repeal (taken from the probit regression on state characteristics described above) as a regressor, this variable does not attenuate the true laws' effect—in fact the coefficient on repeal actually gets stronger. If anything, states repealing their blue laws have observable characteristics associated with higher voting turnout, not lower turnout.

The last two columns report results where the dependent variable is logged; once again, the result suggests that an increase in the cost of religious participation leads to a decrease in voter turnout. The coefficients are consistent with a 2.7 to 2.9 percent decline in voter turnout. As mentioned before, the levels result in the baseline regression suggests an effect that is a little less than 3 percent of the mean of the dependent variable. The result is thus extremely similar in magnitude regardless of whether logs or levels are used.

C. Other Results

We have also estimated a variety of models of the impacts of the blue laws on other aspects of voter turnout. Table 4 shows a series of such regressions.[16] We first report results on turnout for contested gubernatorial and senatorial elections between 1952 and 2000.[17] The results for both gubernatorial and senatorial elections are qualitatively similar to those on presidential elections in Table 3, but the standard errors are larger so that the findings are not significant.

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[15] One difficulty with interpreting this result is that presidential elections occur 4 years apart, so that a preexisting trend may be made manifest between two elections. However, the results of Table 2 show no evidence of a pre-existing trend even with higher-frequency attendance data. This lessens the concern that the regression here is somehow masking inter-election phenomena.

[16] These results include a dummy for whether or not a presidential election was being held.

[17] An election is defined as uncontested if either the Republican or Democratic candidate received above 80% or below 20% of the vote. About 6% of the observations for both senatorial and gubernatorial elections are regarded as participating in uncontested elections by this measure. Results using other cutoffs (or all observations) are qualitatively similar.

One might also wonder whether changes in the opportunity cost of religion affect different types of voters in different ways. The next two columns look at how blue laws' repeal impacts the percent of the population voting for Republican and Democratic presidential candidates. Interestingly, we find that the strong negative effect of the blue laws is concentrated in those voting for Democratic candidates, with a positive and insignificant effect on voting for Republican candidates. Thus, the "marginal churchgoers"—those whose behavior is most likely to be affected by blue laws—are relatively more likely to vote for Democratic candidates.

Religion has also been linked to political preferences, though attendance has only been a strong predictor of candidate preferences since the early 1990s.[18] Table 5 explores whether our results vary across religious traditions, and in particular the table compares Catholic and Protestant respondents. The first two columns of the table repeat the basic attendance regressions from columns 1 and 2 of Table 2, except now we include two new regressors: a dummy for whether a respondent is Catholic, and an interaction of the Catholic dummy with the repeal dummy.[19] (While the denominational information from the GSS is limited in some years, we also considered regressions that allowed various Protestant denominations to respond heterogeneously to blue laws. These results did not produce any consistently significant differences among Protestant denominations, and so for brevity here we focus on the Catholic/Protestant distinction.) Table 5 shows that this new interacted variable is negative and significant, while the uninteracted repeal variable (which represents the effect of blue laws on Protestants) is smaller and insignificant. In other words, data from the GSS suggest that Catholics are primarily the ones responsive to blue laws.[20]

The last column of Table 5 attempts to see whether our voter turnout result is also stronger for highly Catholic communities. Since this is aggregate data, we cannot create a dummy variable that

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[18] Since 1992, those who never attend church are much more likely to vote Democratic than those who report attending church every week (ANES 2004, Cumulative file). In contrast, in the 1960s, those who attended church regularly were no more likely to vote Democratic than those who never attended.
[19] About 25 percent of the regression sample reports being Catholic.
[20] We also explored whether the response to blue laws varied by income level; our estimates suggested that individuals with different levels of income responded to blue laws in similar ways.

separates Catholic individuals and Protestants for this regression. Instead, we create a dummy variable that equals unity for highly Catholic counties, which we define as counties where 35 percent or more of the population was Catholic in 1952, a year near the start of our study.[21] We then interact the highly-Catholic dummy with a repeal dummy.[22] There are 99 counties from 16 states in the regression sample that are highly Catholic.[23] The results from column 3 confirm the importance of the Catholic response in the GSS regressions—the decline in voter turnout in highly-Catholic counties (which is found by adding the two coefficients together) is nearly twice as large as in the average not-highly-Catholic county (which is represented by the uninteracted repeal coefficient). Thus, Catholics report a greater decline in attendance after blue laws are repealed, and voter turnout falls more in highly Catholic counties after blue laws are repealed.

Why might Catholics attendance be especially responsive to blue laws repeal? At first blush this fact may be especially surprising given that Catholics in the GSS over this time period report higher attendance levels than all other mainline protestant groups. However, this finding is compatible with Iannaccone's (1992) noted economic model of religious participation. Put briefly, his model suggests that high-participation groups will be especially attractive to individuals who view religious and secular activities as highly substitutable with each other; Hungerman (2010) tests this prediction of the model and finds strong evidence supporting it. Thus, those in high attendance groups can actually be more "marginal" than those with moderate religious participation, which matches the evidence here.

There is also work exploring the importance of faith in influencing Catholic political and civic participation (cf. Leege, 1988). About 56 percent of Catholic respondents in the regression sample in the GSS report themselves as democratically affiliated, compared to only 46 percent of non-Catholics. Thus

[21] Choosing other cutoffs than 35 percent produces similar estimates, although (not surprisingly) cutoffs based on a higher percent Catholic often produce slightly stronger results while cutoffs using a lower percent Catholic produce weaker results. Data on percent Catholic come from the Glenmary study (Whitman, 1956) and were made available by the Association of Religion Data Archives.
[22] The uninteracted highly-Catholic dummy is differenced out by our county dummy variables.
[23] The 16 states are Arizona, California, Colorado, Indiana, Iowa, Kansas, Minnesota, Nevada, New Mexico, North Dakota, Ohio, Pennsylvania, South Dakota, Texas, Vermont, and Wyoming. About 6 percent of the total regression sample is made up of counties that are highly Catholic.

the results in Table 5 may be able to partly explain the strong effect on Democratic turnout. Furthermore, an interesting paper by Smith (2005) finds that Catholic political attitudes are influenced by the attitudes of Catholic clergy—but this relationship is restricted entirely to politically liberal Catholics. This also suggests that the marginal Catholics in Table 5 may play an important role in driving the Democratic turnout effect in Table 4. The findings here might also reflect that Democratic voters are more marginal in their *voting* behavior; similarly, Gomez, Hansford, and Krause (2007) use variation in weather on Election Day to argue that Democratic-leaning voters are more responsive to changes in incentives to vote than other voters.

Thus, the results here suggest that the effect of blue laws is especially strong on Catholics and Democratic voters, and this is compatible with prior work suggesting that (a) high attendance groups (such as Catholics, in the sample used here) may be especially responsive to blue laws or other changes in the incentive to participate in religious activities, (b) politically liberal voters may be more responsive to incentives to vote, and even more specifically (c) politically liberal Catholics may be especially responsive to their clergy's political influence.

Conclusions

Several decades of research on political behavior has uncovered a number of strong and robust associations between individual experiences on the one hand, and voter behavior on the other. Among the most important findings from a generation of research are the strong positive associations between individual voter turnout and education, union membership, and church attendance (e.g., Wolfinger and Rosenstone 1980, Rosenstone and Hansen 1993, Verba, Schlozman, and Brady 1995.). These associations have generally been treated as if they were causal effects. However, the foundation for this interpretation is weak. Nearly all of the research rests on cross sectional regressions using survey data and in recent years voting scholars (among others) have shown greater appreciation for the vulnerability of such analysis to bias. This concern is heightened when the independent variable of interest is an individual's choice, such as the decision to attend church or stay in school, which may be affected by unobserved individual attributes or circumstances correlated with political attitudes or behavior.

The U.S. is a highly religious country and an individual's level of religious observance is often positively correlated with a range of desirable outcomes. There is increasing interest in pressing further to consider whether these correlations may be given causal interpretations. One promising strategy for doing so is to find changes in the environment which impact religious participation but not other relevant behaviors, and then to trace through the effects on other aspects of life, such as political participation. The repeal of the blue laws provides an example of such a change. Following Gruber and Hungerman (2008), we show that repeal of the blue laws does indeed lead to less church attendance. We then show that repeal is associated with lower voter turnout, confirming earlier studies that documented higher turnout for those who attend church services more often.

Beyond this methodological contribution, the finding that church attendance appears to cause a change in turnout has substantive implications for political theory. The "social capital" created by citizen involvement in voluntary organizations is often credited with creating networks of communication and fostering bonds of trust and reciprocity, which in turn provides an environment conducive to high levels of political and economic performance. Theorists for centuries have singled out religious practices as of special importance (cf. Tocqueville's extensive discussion of religion in Democracy in America), and have noted Americans are religious and conjectured that this societal feature has broad implications.

Social capital is sometimes treated as having a uniform influence in civic life, but changes in the level of participation in voluntary organizations may have effects that differ markedly across citizens. We find that the turnout effects are largest in terms of voting for Democratic candidates. The point estimates in Table 4 imply that the net effect of repeal of blue laws is to reduce the Democratic share of the presidential vote by approximately one and a half percentage points. To put this magnitude in context, in two of the last eight presidential elections the candidates were separated by about two percentage points or less. Organizations such as unions and churches, which reach citizens across socioeconomic strata, might serve to mobilize their membership and thereby counteract some of the class biases observed in political participation (Brady, Schlozman, Verba, and Luks, 2008). The finding that there are partisan implications to changes in the blue laws suggests partisan differences across the types of individuals whose church attendance is affected by blue law repeal or differences in the strength of their behavioral response to reduced church attendance. Clarifying the mechanism that produces the partisan effects is a research question that merits further attention.

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Table 1: Blue Laws Information and Summary

Panel A: State and Year 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	Vermont	1982
Ohio	1973	Virginia	1975
Pennsylvania	1978	Washington	1966

See text for reasons why various states were not included. Eight other states which never had blue laws are also included in the regressions: Arizona, California, Colorado, Idaho, Nevada, New Mexico, Oregon, and Wyoming

Panel B: Summary Statistics on GSS Data

Variable	Mean	Std. Dev.
Attendance	4.2	2.61
Age	45.9	17.7
Sex (1= female)	0.58	0.49

Observations: 16,143. 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). The percent of respondents reporting particular attendance levels are: Never (10.4) Less than Once a Year (8.1), 1-2 Times a Year (13.2), Several Times a Year (13.1), Once a Month (7.7), 2-3 Times a Month (9.8), About Weekly (6.2), Weekly (22.6), More Than Weekly (8.9).

Panel C: Summary Statistics on Voting Data

Variable	Mean	Std. Dev.
County Population	70,789	260,000
Percent of Population that Votes	39.6	8.84
Percent of Population that Votes for Democratic Candidate	16.3	5.6
Percent of Population that Votes for Republican Candidate	21.0	7.5
Percent of Population that Votes for Independent Candidate	2.3	3.5

Total observations: 19,019. Means are unweighted. Sample includes 1,585 counties from the 1952 through the 2000 presidential elections. Data on only 611 and 977 counties are available in 1952 and 1956, respectively.

Table 2: Blue Laws and Attendance

	Index	Estimated Weeks	Index	Estimated Weeks	Index	Estimated Weeks
Repeal Dummy	-0.245	-2.456	-0.242	-2.554	-0.258	-2.457
	[0.094]	[0.963]	[0.087]	[1.182]	[0.105]	[1.314]
Early Repeal Dummy	-	-	-0.006	0.168	-	-
			[0.129]	[1.391]		
Predicted Repeal	-	-	-	-	0.084	0.005
					[0.399]	[3.835]
Age	0.007	0.17	0.007	0.17	0.007	0.170
	[0.009]	[0.115]	[0.009]	[0.115]	[0.009]	[0.115]
Age squared	0.0001	0.001	0.0001	0.001	0.0001	0.0008
	[0.0001]	[0.001]	[0.00001]	[0.001]	[0.0001]	[0.001]
Dummy for Female	0.68	7.402	0.68	7.401	0.680	7.402
	[0.041]	[0.472]	[0.041]	[0.471]	[0.0406]	[0.471]
Dummy for White	-0.77	-6.354	-0.77	-6.356	-0.770	-6.354
	[0.240]	[2.530]	[0.240]	[2.528]	[0.240]	[2.531]
Dummy for Black	0.034	1.064	0.034	1.063	0.0340	1.064
	[0.263]	[2.983]	[0.263]	[2.982]	[0.263]	[2.98]
Dummy for Married	0.505	5.287	0.505	5.287	0.505	5.287
	[0.048]	[0.627]	[0.048]	[0.626]	[0.048]	[0.628]
Observations	16143	16143	16143	16143	16143	16143
R-squared	0.09	0.07	0.09	0.07	0.09	0.07

Robust standard errors, clustered by state, in brackets. All regressions include state dummies and year dummies, controls for educational attainment, and controls for state-level income, unemployment, and percent foreign born. Dependent variable "index" is a measure of how often an individual attends church; see text for details. The repeal dummy is set to unity once a state repeals its blue laws. The Early Repeal dummy is set to unity two years before the blue laws changed. Data are from the 1973-1998 GSS. The Predicted Repeal variable measures the likelihood a state has repealed its blue laws based on a probit regression on state characteristics; see text for details.

Table 3: Basic Results

	Baseline	No Weights	Only States with Laws	Early Dummy	Predicted Change	Logged, No Weights	Logged
Repeal Dummy	-0.986	-1.224	-1.226	-1.116	-1.479	-0.029	-0.027
	[0.573]	[0.592]	[0.626]	[0.589]	[0.499]	[0.013]	[0.014]
Early Repeal Dummy	-	-	-	0.261	-	-	-
				[0.587]			
Predicted Repeal	-	-	-	-	3.748	-	-
					[1.178]		
Senatorial Election Dummy	0.063	0.324	-0.014	0.055	0.066	0.055	0.002
	[0.189]	[0.197]	[0.233]	[0.194]	[0.199]	[0.194]	[0.006]
Gubernatorial Election Dummy	-0.703	-0.737	-0.37	-0.731	-0.492	-0.731	-0.011
	[0.703]	[0.991]	[0.858]	[0.728]	[0.768]	[0.728]	[0.026]
St. Share Dem. – National Share (abs. Value)	-15.247	-8.199	-7.335	-15.378	-13.68	-15.378	-0.355
	[10.832]	[12.445]	[14.283]	[10.829]	[11.06]	[10.829]	[0.310]
St. Share Dem. – National Share, squared	4.375	13.46	-51.467	3.314	-12.45	3.314	-0.24
	[57.850]	[70.532]	[77.210]	[58.392]	[58.57]	[58.392]	[1.757]
Weights?	Yes	No	Yes	Yes	Yes	No	Yes
County Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Include All Possible States?	Yes	Yes	No	Yes	Yes	Yes	Yes
Population Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-Region Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19019	19019	15618	19019	19019	19019	19019
R-squared	0.88	0.85	0.88	0.88	0.88	0.86	0.88

Dependent variable is percent of population voting in presidential election. Robust standard errors, clustered by state, in brackets. Repeal dummy equals unity if a state has repealed its blue laws. The Early Repeal dummy goes from zero to unity in the election before blue laws changed, and stays at unity thereafter. The Predicted Repeal variable measures the likelihood a state has repealed its blue laws based on a probit regression on state characteristics; see text for details. States included are given in Panel A of Table 1. Population controls include county population both in levels and in logs. Year-by-Region dummies subsume a regular set of year dummies.

Table 4: Extensions to Turnout Results

	Gubernatorial		Senatorial		Presidential	
	Levels	Logs	Levels	Logs	Democratic	Republican
Repeal Dummy	-0.548	-0.008	-0.77	-0.02	-1.224	0.339
	[0.758]	[0.033]	[0.632]	[0.019]	[0.287]	[0.536]
Weights?	Yes	Yes	Yes	Yes	Yes	Yes
County Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Population Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-Region Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19243	19243	22178	22178	19019	19019
R-squared	0.91	0.89	0.89	0.91	0.87	0.86

Robust standard errors, clustered by state, in brackets. Dependent variable in the first four columns is the percent of population voting in either a gubernatorial or a senatorial election. Regressions in the first four columns omit "uncontested" elections; an election is defined as uncontested if either the Republican or Democratic candidate received above 80% or below 20% of the vote; results using other cutoffs are qualitatively similar. Repeal dummy equals unity if a state has repealed its blue laws. See Table 2 for more details. The dependent variables in the last two columns is the percent of the population voting for the Democratic and Republican candidate in presidential elections.

Table 5: Results by Religiosity

	Attendance Index	Attendance Estimated Weeks	Voter Turnout
Repeal Dummy	-0.107	-0.869	-1.043
	[0.099]	[0.962]	[0.599]
Repeal Dummy*Catholic Dummy	-0.505	-6.391	-
	[0.168]	[2.110]	
Repeal Dummy*Catholic County	-	-	-0.910
			[0.552]
Observations	16143	16143	18776
R-squared	0.092	0.068	0.882

Robust standard errors, clustered by state, in brackets. The regressions in the first two columns are comparable to the first two regressions of Table 2; the regression in column 3 is comparable to the first regression in Table 3. The "Repeal Dummy*Catholic Dummy" variable in columns 1 and 2 is an interaction of the blue laws repeal dummy and a dummy if an individual in the GSS reports being Catholic; 25 percent of the sample reports being Catholic. The "Repeal Dummy*Catholic County" variable in column 3 is an interaction of the blue laws repeal dummy with a dummy that equals 1 if at least 35 percent of a state's population in 1952 was Catholic, as measured by Whitman (1956). There are 99 counties from 16 states that are highly catholic by this measure. The regressions in columns 1 and 2 include an uninteracted dummy for whether a respondent was Catholic; a uninteracted catholic-county dummy is differenced out of column 3 by the county dummy variables. Regressions exploring the influence of Protestant groups typically produce statistically insignificant results.