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Does Church Attendance Cause People to Vote? Using Blue Laws' Repeal to Estimate the Effect of Religiosity on Voter Turnout
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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, which is statistically significant and fairly robust across model specifications, is consistent with the large effect of church attendance on turnout reported in the literature, and suggests that church attendance may have significant causal influence on voter turnout.

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Introduction

For most 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 (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 non-presidential year.

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 newcomers are less

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

likely to vote. A larger point is that any catalogue of particular conjectures about how church goers may differ from non-church goers runs the risk of overlooking important sources of difference.

We measure the causal 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 (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 1 percentage point fall in the percentage of the population that turns out to vote. Additionally, there is little evidence that repeals in Blue Laws were preceded by declines in voter participation or declines in religious participation; the results here are not driven by “reverse causality.” We thus conclude that the relationship between Blue Laws and voting plausibly operates through Blue Laws’ impact on religious attendance. The findings here therefore provide much more compelling evidence of a true relationship between attendance and voting than does prior research. We also examine whether there are any partisan differences in the effect of the Blue laws’ repeal. We find that the decline in turnout appears to affect Democratic but not Republican vote shares, a result we present in section 3 and briefly discuss in the conclusion.

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 and therefore a decline in citizen involvement in such activities has serious and broad consequences (e.g., Putnam 2000). Robert Putnam, a

leading proponent of the view that the level of social capital plays a critical role in societal performance along many dimensions, argues that religious organizations are of special importance: "Faith communities in which people worship together are arguably the single most important repository of social capital in America." (Putnam 2000, 66). Concern over a decline in church attendance is premised on the belief that churchgoing causes rather than is merely associated with increases in social capital. More generally, 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 does in fact have a causal effect that spills over into 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, if it was real, the mobilizing effect of church attendance might counteract some of the class biases observed 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),

improved self-reported measures of well-being (Ellison, 1991; Hout and Greeley, 2003), and greater marital stability (Lehrer and Chiswick, 1993). Religiosity is strongly correlated with self-reports of well-being (Ellison 1991, Hout and Greeley 2003), and recent work has found that differences in those reporting never attending to 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 a citizen's 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). 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 (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 interest in and capacity for participation. Church goers are exposed to a basic political education through political information from other members and the clergy. Those who attend church participate in decision making processes regarding church affairs, plan meetings, or give speeches. These activities help develop general civic skills that might lead to political involvement outside of church. Second, church members are easily

mobilized. Church members are part of a community and may be relatively easily encouraged to participate by requests from other church members or the church leadership. These requests may be especially effective due to social pressure to participate in political causes that are of common concern.

Recent work in voter turnout provides evidence to support both of these channels. 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 the public encouragement to adhere to the social norm of voting may be effective at increasing turnout. Recent experimental studies on the effect of social pressure on voter turnout confirm earlier work in social psychology showing that social pressure can induce compliance with behavior that is supported by social norms (Cialdini and Goldstein 2004, Cialdini and Trost 1998, Scheff 2000). 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 these studies, it is also possible that those who are more likely to be politically active are the very individuals who 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. 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 routinely interpret the correlation between church-going and turnout as evidence church attendance causes turnout. Researchers minimize the problem of drawing causal conclusions in this area or assume that the available control variables were adequate to eliminate concerns (Rosenstone and Hansen, 1993, p172, Huckfeldt and Sprague, 1993; Verba, et al 1995).[2] However, some differences between churchgoers and others (such as tastes for organizational involvement or listening to sermons and speeches) may be difficult to observe, so that adding control variables will 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

[2] Verba et al. for instance discuss the possibility of spurious correlations at length, but ultimately dismiss the plausibility and relevance of the typical objections to ascribing a causal interpretation to the association between institutional 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.

likely to join both churches and unions and such people also participate in politics at higher rates) or real mechanisms..." (Brady, Schlozman, Verba, and Luks, 2008).

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, a result in agreement with the results of previous cross sectional regression using the American National Election Study and the Current Population Survey.

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.[3] It is these general statewide prohibitions on retail activity which 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 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." After the ruling, Blue Laws began to be

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

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 of retail activity on Sundays. Some states' laws were (or are) decided at the county or city level, making collection of these data infeasible.[4] 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.[5] 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.

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

[4] 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.
[5] These states include Illinois, Massachusetts, Maine, New York, West Virginia, and Wisconsin

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 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 repealing 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. Retail establishments in the same state sometimes supported Blue Laws and sometimes 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 what economic and social forces are 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 Blue Laws' repeal, 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, as these individuals are those most likely to attend services on Sunday (these individuals make up nearly 90 percent of the sample).[6] 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,

[6] 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.

which corresponds to monthly attendance. (Monthly attendance is also the median response.) The table also shows that GSS somewhat over-samples female respondents.[7]

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 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.

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. 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: it is half as large as the well-noted higher rate of religious attendance for married individuals, for example. 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.[8] 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% of the mean. Since Blue Laws likely impact "marginal" churchgoers more than others, the

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[7] 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.

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

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.

The last 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 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 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 Blue Laws repeal also lead to a decline in voter turnout.

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 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. 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 regressions include county-level dummies, so that identification comes from changes within counties across time. The regressions also use a very strong set of region-by-year dummies, where "region" refers to the four geographic regions as defined by the census. These variables thus control for any relevant time-varying phenomena for each region in the country. (Of course, this set of region-by-year dummies subsumes a typical set of year dummies). We have multiple repeals of Blue Laws in all four regions of the country, meaning that for each region we will exploit within-region variation in Blue Laws' prevalence across time for identification.

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 percent decline in the odds of voting. This result is remarkably consistent with Rosenstone and Hansen (1993), who find that attending nearly every week raises the odds of voting by 15%. Our 2.5 week estimate is roughly one-twentieth of the weeks in the year, and our estimated impact is one-fifteenth of theirs. 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. For instance, it may be the case the declining social capital in a state over time leads to less voting, and also leads to a change in Blue Laws. The fourth column tests this story by including an "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. If changes in the laws are driven by pre-existing declines in civic participation, then this "early" repeal dummy would be negative and significant, and/or it would attenuate the observed effect of the actual law change. But it is clear from Table 3 that this is not the case. 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 are repealed, not before.[9]

The last column reports results where the dependent variable is logged (and weights are used). Once again, the result suggests that an increase in the cost of religious participation leads to a decrease in voter turnout. The coefficient is consistent with a 2.7 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.[10] We first report results on turnout for contested gubernatorial and senatorial elections between 1952 and 2000.[11] 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.

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

[9] 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.
[10] These results include a dummy for whether or not a presidential election was being held.
[11] 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.

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.[12] This finding is consistent with recent work by Brady et al showing that the effect of church attendance on voting is greater among lower income voters than for high income ones (Brady et al, 2008).

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 religiosity is correlated with a range of positive outcomes. There is increasing interest in converting these correlations into causal interpretations. One

[12] Religion has also been linked to political preferences, though attendance has only been a strong predictor of candidate preferences since the early 1990s. 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. Our result that liberal churchgoers are on the margin is thus compatible with, but does not directly substantiate, the observed decline in liberal religious attendance in recent US history.

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 excellent 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 important 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 (see, for example, 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. Despite this, there have been few previous attempts, and little evidence to show, that church attendance actually causes mass behavior (for a recent assessment, see Brady, Schlozman, Verba, and Luks 2008).

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 the Blue Law repeal 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
Repeal Dummy	-0.245	-2.456	-0.242	-2.554
	[0.094]	[0.963]	[0.087]	[1.182]
Early Repeal Dummy	-	-	-0.006	0.168
			[0.129]	[1.391]
Age	0.007	0.17	0.007	0.17
	[0.009]	[0.115]	[0.009]	[0.115]
Age squared	0.0001	0.001	0.0001	0.001
	[0.0001]	[0.001]	[0.00001]	[0.001]
Dummy for Female	0.68	7.402	0.68	7.401
	[0.041]	[0.472]	[0.041]	[0.471]
Dummy for White	-0.77	-6.354	-0.77	-6.356
	[0.240]	[2.530]	[0.240]	[2.528]
Dummy for Black	0.034	1.064	0.034	1.063
	[0.263]	[2.983]	[0.263]	[2.982]
Dummy for Married	0.505	5.287	0.505	5.287
	[0.048]	[0.627]	[0.048]	[0.626]
Observations	16143	16143	16143	16143
R-squared	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, ranging from 0 (never) to 8 (multiple times a week). Regressions omit observations in states the year that Blue Laws changed. 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.

Table 3: Basic Results

	Baseline	No Weights	Only States with Laws	Early Dummy	Logged, No Weights	Logged
Repeal Dummy	-0.986	-1.224	-1.226	-1.116	-0.029	-0.027
	[0.573]	[0.592]	[0.626]	[0.589]	[0.013]	[0.014]
Early Repeal Dummy	-	-	-	0.261	-	-
				[0.587]		
Senatorial Election Dummy	0.063	0.324	-0.014	0.055	0.055	0.002
	[0.189]	[0.197]	[0.233]	[0.194]	[0.194]	[0.006]
Gubernatorial Election Dummy	-0.703	-0.737	-0.37	-0.731	-0.731	-0.011
	[0.703]	[0.991]	[0.858]	[0.728]	[0.728]	[0.026]
State Share Democrat – National Share (abs. Value)	-15.247	-8.199	-7.335	-15.378	-15.378	-0.355
	[10.832]	[12.445]	[14.283]	[10.829]	[10.829]	[0.310]
State Share Dem. – National Share, squared	4.375	13.46	-51.467	3.314	3.314	-0.24
	[57.850]	[70.532]	[77.210]	[58.392]	[58.392]	[1.757]
Weights?	Yes	No	Yes	Yes	No	Yes
County Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Include All Possible States?	Yes	Yes	No	Yes	Yes	Yes
Population Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-Region Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19019	19019	15618	19019	19019	19019
R-squared	0.88	0.85	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. Early repeal dummy goes from zero to unity in the election before Blue Laws changed, and stays at unity thereafter. 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: Additional 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.