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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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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. The repeal of the laws restricting Sunday retail activity ('blue laws') is used to measure the effects of church-going on political participation. Blue laws' repeal caused a 5 percent decrease in church attendance. Its effect on political participation was measured and it was found that, following the repeal, turnout fell by approximately 1 percentage point. This decline in turnout is consistent with the large effect of church attendance on turnout reported in the literature, and suggests that church attendance may have a significant causal effect on voter turnout.

For a large number of Americans, attending religious services is a routine and important part of life. On an average Sunday roughly a quarter of the population of the United States 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.[2] Over two-thirds of Americans belong to a church or other religious organization.[3] Despite the broad reach and clear importance of religion in American life, 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.[4] Prior work has found that, when socioeconomic characteristics, age, gender, and political conditions are controlled, 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-run and short-run differences in those who choose to attend church and those who

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[1] Figures based on calculations of General Social Survey data from 1973 to 1998.
[2] Andreoni 2006.
[3] Iannaccone 1998.
[4] E.g., Rosenstone and Hansen 1993; Verba, Schlozman, and Brady 1995.

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 conjectures about how churchgoers may differ from non-churchgoers runs the risk of overlooking important sources of difference that might also be relevant for the outcome variable of interest.

Our research measures the effect of church attendance by observing the consequences of the decline in church attendance that followed a policy change. In the closing decades of the twentieth century, long-standing restrictions on Sunday retail activity, often referred to as 'blue laws,' were repealed. Gruber and Hungerman have argued that a consequence of permitting Sunday morning shopping was to reduce the relative appeal of Sunday morning church attendance. They provided compelling evidence that there was a notable decline in church attendance following the repeal of blue laws.[5]

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 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 the repeal of blue laws was preceded by a decline in voter participation or a decline in religious participation; the results here do not appear to be driven by 'reverse causality.'

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.[6] Finding that church attendance does have a causal effect on political participation provides 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.[7]

This article 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.

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

[5] Gruber and Hungerman 2008.
[6] E.g., Putnam 2000.
[7] Brady et al. 2008.

activity,[8] lower rates of delinquency,[9] lower rates of substance abuse,[10] better health status and outcomes,[11] and greater marital stability.[12] Religiosity is also strongly correlated with self-reports of well-being,[13] 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.[14]

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 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.[15] 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 elections and 10.2 percentage points more likely to report voting in mid-term elections than those who say they do not attend religious services.[16] Verba, Schlozman, and Brady perform a similar analysis.[17] Using the data from the survey of 2,500 respondents in the Citizen Participation Study, Verba, Schlozman, and Brady confirm the strong correlation between church attendance and turnout.[18] 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,[19] Latinos,[20] Muslims,[21] and African Americans.[22]

There are two main explanations of 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.[23] Those who attend church have opportunities to interact with and to work with others, and may participate in making decisions regarding church affairs, planning meetings, or giving speeches. These activities help to develop general civic skills that might aid political involvement outside the church. Second, church members are part of a community, and this civic association yields political by-products.[24] As such, churches are 'important conduits of political information and recruitment.'[25] 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

[8] Evans et al. 1995; Hull and Bold 1995; Lipford, McCorkmick, and Tollison 1993.
[9] Bachman et al. 2002; Johnson et al. 2000; Wallace and Williams 1997.
[10] National Center on Addiction and Substance Abuse 2001.
[11] Hummer et al. 1999; Levin and Vanderpool 1987.
[12] Lehrer and Chiswick 1993.
[13] Ellison 1991; Hout and Greeley 2003.
[14] Gruber and Mullainathan 2002.
[15] Rosenstone and Hansen 1993.
[16] Rosenstone and Hansen 1993, Tables D-1 and D-4.
[17] Verba, Schlozman, and Brady 1995.
[18] Verba, Schlozman, and Brady 1995. For additional research on the link between church attendance and political participation see also Peterson (1992), Smidt et al. (2008), Wald (1992), Wald, Kellstedt, and Leege (1993).
[19] Wong, Lien and Conway 2005.
[20] Jones-Correa and Leal 2001.
[21] Jamal 2005.
[22] Alex-Assensoh and Assensoh 2001; Harris 1994.
[23] E.g., Verba, Schlozman, and Brady 1995.
[24] Jones-Correa and Leal 2001; Wald, Owen, and Hill 1988.
[25] Jones-Correa and Leal 2001, 754.

members or the church leadership.[26] These requests may be especially effective due to social pressure to participate in political causes that are of concern to the church community. In addition, those who attend church may be more likely to be targeted for mobilization by outside groups, who may mobilize through the church, or may use an individual's church membership to help infer an individual's political preferences and what messages are more likely to be effective.[27]

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 door to door canvassing in the run up to an election, can increase turnout substantially.[28] 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 effect, and results of this magnitude have been supported by most subsequent studies of canvassing.[29]

Moreover, voter turnout appears to be highly sensitive even to 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.[30] Recent experimental studies on the effect of social pressure on voter turnout confirm these findings. For example, in a field experiment in 2008, 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 ten times the effect of a typical political mailing.[31] Other experimental work has shown that the more personal an encouragement to vote is, the more effective it is at producing higher turnout.[32]

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. Although 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. 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.

Although 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,

[26] Campbell 2004; Green, Rozell, and Wilcox 2001; Guth et al. 2002.
[27] Rosenstone and Hansen 1993.
[28] Gerber and Green 2000.
[29] Green and Gerber 2008.
[30] Cialdini and Goldstein 2004; Cialdini and Trost 1998; Scheff 2000.
[31] Gerber, Green, and Larimer 2008.
[32] Gerber and Green 2000; Green and Gerber 2008.

scholars assume that the available control variables are adequate to eliminate concerns.[33] 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 and such people also participate in politics at higher rates) or real mechanisms …'.[34]

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.[35] 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 about omitted variable bias due to differences 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 interpret church attendance as a proxy variable for more nuanced indicators of church involvement.

The strategy that we will pursue in this article is to consider a policy change, repeal of the blue laws, as a means of shifting religious participation that may have an impact on voting. Several recent studies have used policy changes to study voter turnout. Milligan, Moretti, and Oreopolous, as well as Dee, have measured 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 concluded that education had a positive effect on voter turnout in the United States.[36] Milligan, Morettie, and Oreopolous noted that their methodology does not allow them to explore the *mechanisms* through which education and voting are related exhaustively; the

[33] Huckfeldt and Sprague 1993; Rosenstone and Hansen 1993, 172; Verba, Schlozman, and Brady 1995. For instance, Verba, Schlozman, and Brady 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 (p. 279). 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.

[34] Brady et al. 2008.

[35] Driskell, Embry, and Lyon 2008.

[36] Dee 2004; Milligan, Moretti, and Oreopolous 2004.

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. Our focus will be on blue laws in the United States, but the methodology here could extend to other nations as well. Studies have documented changes in blue laws in, for example, Belgium, Canada, Germany, and the Netherlands.[37]

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, Gruber and Hungerman, and Laband and Heinbuch.[38]

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 thirty 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.[39] 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.[40] These challenges were often successful since blue laws could be confusing in their classification of what activities were allowed and what activities were not. In the decades following this ruling, most states repealed their blue laws through either judicial or legislative action.[41]

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 as we know of no source of information on blue laws at the local level.[42] 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.[43] 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 hopes that their inclusion is irrelevant. We investigate whether the results are sensitive to including these western states below.

[37] Ferris 1991; Gradus 1996; Jacobsen and Koormean 2005; Tanguay, Vallée, and Lanoie 1995.

[38] Goos 2005; Gruber and Hungerman 2008; Laband and Heinbuch 1987.

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

[40] Theuman 2005.

[41] Goos 2005.

[42] 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. We discuss the implications of county-level laws more below.

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

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

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

Panel C: Summary Statistics on Voting Data

Variable	Mean	Std. Dev.
County population	70,789	260,000
Percentage of population that votes	39.6	8.84
Percentage of population voting for Dem. candidate	16.3	5.6
Percentage of population voting for Rep. candidate	21.0	7.5
Percentage of population voting for Ind. candidate	2.3	3.5

Notes: 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.

[†]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.

[‡]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 percentage 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 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 could be generalized to other areas. First, our sample (including the western states listed below Panel A of Table 1) includes nearly half of the United States of America and slightly over half the US population. Second, the characteristics of GSS (General Social Survey) respondents in our set of states are very close to other those of other states; for example, average age (45.9 in our sample versus 45.3 in other states), proportion female among GSS respondents

(0.58 versus 0.57), proportion with a high school degree (equal at 0.33), or the proportion attending religious services weekly (0.31 versus 0.30) are all very similar.

Even if the collection of states appears reasonably diverse and the timing of repeals 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 introduce a bias against 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.[44]

Second, one may wonder whether the timing of blue laws' repeal is coincident with other phenomena. In our context, for blue laws to be suitably exogenous, the repeal of blue laws should not coincide with other social phenomena that would themselves directly affect church attendance or voter turnout. For example, regarding blue laws and attendance, it might be a concern that parts of the country that traditionally have high attendance (or turnout) also traditionally favor blue laws. Fortunately, the empirical model specifications provided below will control in a very flexible way for this type of cross-sectional correlation. Another concern might be that the passage of blue laws coincides with broad temporal trends, such as declines in social capital or trends in secularism over time, leading to lower religious attendance (and voter turnout). We propose specifications that will investigate the potential importance of such effects in the data.

One might also wonder, if blue laws' repeal is not well predicted by broad trends in social capital (or trends toward secularity), what *does* drive blue laws repeal? For example, if repeal was associated with the ascension of a particular special interest group that had exceptional voting turnout behavior, this could bias our estimates. This does not typically appear to be the case. Theuman states that most commonly the fate of blue laws has been determined by the minutiae of the laws themselves, as typically the laws have been 'challenged on the basis of their classifications of businesses, commodities, or persons covered by the law or excluded from its operation.'[45] The failure of the laws' classifications to meet constitutional standards led to courts over-ruling blue laws in Pennsylvania, Vermont, and Utah. Some states, such as Kansas, North Dakota, and Washington, legislatively repealed their blue laws after court rulings on the validity of the laws. These details of the laws matter for court rulings but can also affect public sentiment; support for blue laws' repeal in Texas became high after it was publicized that the law forbade the sale of baby bottles while allowing the sale of beer bottles.[46] In some cases, a particular individual played a pivotal role in preserving blue laws (such as Sydney Schlesinger),[47] or in their repeal (such as Richard Riley).[48]

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,[49] although small businesses were more likely to support the laws[50] and often larger retailers supported the laws.

[44] E.g., Associated Press 1984a; Hansard 1985; McGee 1991; Merry 1983; *New York Times* 1970.

[45] Theuman 2005.

[46] Attlesey 1985; King 1985. Texas law also allowed the sale of hammers but not nails, wooden ladders but not aluminum ones, blank cassette tapes but not pre-recorded tapes, and 'a truckload of lumber but not a power saw' (King 1985).

[47] Lynch 1978.

[48] King 1985. Idiosyncratic events may also have influenced repeal. On Christmas Eve in 1984 a South Carolina man named W. Thomas Moseley was arrested, while wearing a Santa Claus suit, for trying to open up his clothing store; the story of Santa Claus's arrest on Christmas Eve in South Carolina received national attention (Associated Press 1984a; Associated Press 1984b). The next year, with Moseley acting as a crusader against the laws, the state repealed its blue law (Coyne 1986; Reed 1985).

[49] Barmash 1986.

[50] Laband and Heinbuch 1987.

Labor unions have both supported and argued against Sunday closing laws.[51] Price and Yandle investigate the economic and social forces associated with the repeal of these laws.[52] After considering the date of statehood, the proportion of Southern Baptists and Catholics in a state, the percentage of the state legislature controlled by the majority party, the number of large retail employers (100 or more employees) and the number of small employers (four employees or less), the number of drug stores in a state with twenty or more employees, state tourism revenue, the proportion of women in the workforce, and the presence of labor unions, 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 Sundays; 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 legal 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 in blue laws create an empirically attractive change in the opportunity cost of religious participation.

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.

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 GSS, a long-running national survey that gathers data on religious participation.[53] 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 a 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 fifty-two 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

[51] Merry 1983.

[52] Price and Yandle 1987.

[53] The American National Election Study (ANES) also asks questions on attendance (and voting). While qualitatively similar results can be obtained in the ANES, the ANES has several drawbacks including a smaller sample size, a more parsimonious measure of attendance, and several changes in its measure of attendance over time; we found that results from the ANES could be very sensitive to the measure of attendance constructed. We focus here on the larger sample and finer (and more consistent) attendance measure available in the GSS.

on Sunday.[54,55] 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 also shows that the GSS somewhat over-samples female respondents.[56]

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 (percentage of population of state black, percentage of foreign born persons in the state, 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, Duflo, and Mullainathan, we cluster our standard errors at the state level; there is further discussion of this below.[57]

The key coefficient is δ, relating the effect of repealing blue laws on attendance. As mentioned earlier in the section on blue-laws history, 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,[58] 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.)

[54] 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. Including all faiths (not just Catholic and Protestant) in the regressions produces very similar results to those shown.

[55] We also estimated regressions on the entire sample (all faiths) where the dependent variable was a dummy for Catholic or Protestant adherence; the coefficient on the repeal dummy was negative (suggesting a fall in adherence to these groups) but not significant. This is potentially compatible with some prior work; for example Gruber and Hungerman (2008) find that the fall in church attendance after blue laws' repeal is driven by an increase in people attending church only a few times a year. That is, when shopping opportunities increase, people may go from attending church frequently to only attending occasionally, and they may still identify as Christian when asked.

[56] 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.

[57] Bertrand, Duflo, and Mullainathan 2004.

[58] Cf. Norris and Inglehart 2004.

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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 0 to 1 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.[59] 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.[60] 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 thirty, so the two-week impact estimated in column 2 is about 6 percent of the mean. Since blue laws likely impact 'marginal' churchgoers more than others, the two-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 next two columns test our estimates for reverse causality. In these columns we include in the model a dummy that goes from 0 to 1 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.[61] From this regression we calculated the estimated probability that any state in any year would have repealed its blue laws. We then

[59] 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.

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

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

TABLE 2 *Blue Laws and Attendance*

	Index	Estimated weeks	Index	Estimated weeks	Index	Estimated weeks
Repeal dummy	−0.245 [0.094]	−2.456 [0.963]	−0.242 [0.087]	−2.554 [1.182]	−0.258 [0.105]	−2.457 [1.314]
Early repeal dummy	–	–	−0.006 [0.129]	0.168 [1.391]	–	–
Predicted repeal	–	–	–	–	0.084 [0.399]	0.005 [3.835]
Age	0.007 [0.009]	0.17 [0.115]	0.007 [0.009]	0.17 [0.115]	0.007 [0.009]	0.170 [0.115]
Age squared	0.0001 [0.0001]	0.001 [0.001]	0.0001 [0.00001]	0.001 [0.001]	0.0001 [0.0001]	0.0008 [0.001]
Dummy for fem.	0.68 [0.041]	7.402 [0.472]	0.68 [0.041]	7.401 [0.471]	0.680 [0.0406]	7.402 [0.471]
Dummy for white	−0.77 [0.240]	−6.354 [2.530]	−0.77 [0.240]	−6.356 [2.528]	−0.770 [0.240]	−6.354 [2.531]
Dummy for black	0.034 [0.263]	1.064 [2.983]	0.034 [0.263]	1.063 [2.982]	0.0340 [0.263]	1.064 [2.98]
Dummy for mar.	0.505 [0.048]	5.287 [0.627]	0.505 [0.048]	5.287 [0.626]	0.505 [0.048]	5.287 [0.628]
Observations	16,143	16,143	16,143	16,143	16,143	16,143
R^2	0.09	0.07	0.09	0.07	0.09	0.07

Notes: 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 percentage 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 1 two years before the blue laws changed. Data are from the 1973–98 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.

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, then this new variable might eliminate the predictive power of the true law change.[62] 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.[63]

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.[64] The regression we estimate is:

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

Here $turnout_{ct}$ is the percentage of the population voting in the presidential election in year t for county c, $repeal_{ct}$ is a dummy that equals 1 if a state has repealed its blue laws (and 0 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) affects 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 actual levels and logarithmic values). 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 proportion of the state voting Democratic and the national proportion 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 rather from changes within counties across time. Thus, the results will not be driven by spurious cross sectional correlation (for example, when

[62] 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.

[63] A referee suggested that repeals from court rulings might *prima-facie* be considered more exogenous than legislative rulings. Three states (Pennsylvania, Vermont, and Utah) had their laws repealed by judicial action. We redid the baseline estimate in column 1 including a new variable that interacts a repeal dummy with a dummy for these three states, allowing a comparison of legislative and judicial repeals. The uninteracted coefficient is in this case −0.23 [s.e. = 0.10], and the new interacted coefficient is −0.08 [0.12]. The results suggest that court and legislative repeals have similar effects on attendance, though court repeals have a slightly more negative effect, but the difference is not statistically significant. We thank the *Journal*'s referee for this suggestion.

[64] Our findings appear to be robust to use of VEP (McDonald and Popkin 2001) rather than population or VAP as the denominator in the analysis. Details are available upon request. Also, one might wonder about turnout for other elections. Congressional house districts do not map cleanly over time to the data we use, but in an earlier version of this article we reported senatorial and gubernatorial results; these were qualitatively similar but less precise than the results in Table 3.

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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 for 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 0 to 1 starting with 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 dependent 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.

The dependent variable here is measured at the county level while the key regressor measuring blue laws is at the state level. There are several noteworthy points on this difference. First, since our key regressor varies in a systematic way across our sample (such as across all counties within a state and year), the residuals from these regressions will be heteroskedastic and will fail the standard assumption of 0 correlation across observations. We will address this concern by clustering standard errors at the state level (as we did with the GSS results, where there is a similar problem). This is a stronger approach than clustering at the county level as state-level clusters will account for any within-county correlation in residuals and also account for correlation between counties in a state; county-level clusters would only account for the latter. (Both methods control for heteroskedasticity.)

Second, there may be measurement error in the level of blue laws used here. In a classical measurement-error scenario, such measurement error would confound our estimates of our treatment and control groups and bias our key coefficient toward 0. Unlike with the GSS results, our turnout data is not at the individual but rather at the county level. However, as noted in Hanushek, Rivkin, and Taylor, standard measurement error concerns would again bias our results downwards in this case.[65] Further, our elimination of states where state code clearly identifies local governments as the source of blue laws lessens the concern for measurement error. The fact that our law changes are often associated with newspaper accounts and stories describing the law changes also lessens this concern. Finally, while we cannot disaggregate our turnout data, we *can* aggregate our GSS data; aggregate results from there provide nearly identical estimates to those in Table 2.[66]

Table 3 presents results from our basic regression. The dependent variable is the percentage of the population in each county 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 at the 10 percent level. The coefficient suggests that the percentage of the population voting in presidential elections falls by about 1 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 the 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 1 percentage point decline in voting. This result is remarkably consistent with that of Rosenstone and Hansen, who find that attending nearly every week raises the likelihood of voting by 15 percentage points.[67] In comparison, our estimates suggest that the

[65] Hanushek, Rivkin and Taylor 1996.

[66] Redoing the first column of Table 2 with state-level aggregate data yields a coefficient of –0.23 [s.e. = 0.10]; redoing column 2 yields a coefficient of –2.2 [1.06].

[67] Rosenstone and Hansen 1993.

TABLE 3 *Basic Results*

	Baseline	No weights	Only states with laws	Early dummy	Predicted change	Logged, no weights	Logged
Repeal dummy	−0.986 [0.573]	−1.224 [0.592]	−1.226 [0.626]	−1.116 [0.589]	−1.479 [0.499]	−0.029 [0.013]	−0.027 [0.014]
Early repeal dum.	–	–	–	0.261 [0.587]	–	–	–
Predicted repeal	–	–	–	–	3.748 [1.179]	–	–
Senate Elect dummy	0.063 [0.189]	0.324 [0.197]	−0.014 [0.233]	0.055 [0.194]	0.066 [0.199]	0.008 [0.006]	0.002 [0.006]
Gov Elect dummy	−0.703 [0.703]	−0.737 [0.991]	−0.370 [0.858]	−0.731 [0.728]	−0.492 [0.769]	−0.006 [0.031]	−0.011 [0.026]
St. share Dem. – national share (absol. value)	−15.247 [10.832]	−8.199 [12.445]	−7.335 [14.283]	−15.378 [10.829]	−13.681 [11.074]	−0.150 [0.346]	−0.355 [0.310]
St. share Dem. – national share^2	4.375 [57.850]	13.460 [70.532]	−51.467 [77.210]	3.314 [58.392]	−12.451 [58.623]	−0.151 [2.053]	−0.240 [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	19,019	19,019	15,618	19,019	19,019	19,019	19,019
R^2	0.88	0.85	0.88	0.88	0.88	0.86	0.88

Notes: Dependent variable is percentage of population voting in presidential election. Robust standard errors, clustered by state, in brackets. Repeal dummy equals 1 if a state has repealed its blue laws. The Early Repeal dummy goes from 0 to 1 in the election before blue laws changed, and stays at 1 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.

population going from no attendance to fifty weeks of attendance a year would raise voting turnout 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, the senatorial and gubernatorial election dummies both appear 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 0 to 1 starting with the election before a state repeals its blue laws; the early repeal dummy stays at 1 thereafter. The coefficient on the early dummy is wrongly 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.[68]

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 results suggest 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 suggesting 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.[69]

[68] One difficulty with interpreting this result is that presidential elections occur four years apart, so that a pre-existing 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.

[69] As in the prior section, we redid the baseline estimate including a new variable for judicial blue laws repeal. The uninteracted repeal dummy in this case is –0.970 [0.608], and the judicial repeal dummy is –0.294 [1.163]. As before, the estimates suggest that judicial repeals have slightly larger effects, but the difference is not statistically significant.

CONCLUSIONS

Several decades of research on political behavior have 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.[70] These associations have generally been treated as if they were causal effects. However, the foundation for this interpretation may not be as strong as would be ideal. 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. The danger of bias 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 United States of America 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, we show that repeal of the blue laws does indeed lead to less church attendance.[71]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. Our work here supports this idea, although as noted earlier the methodology we propose could extend to other countries as well. We leave such an extension for future work.

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[70] E.g., Rosenstone and Hansen 1993; Verba, Schlozman, and Brady 1995; Wolfinger and Rosenstone 1980.
[71] Gruber and Hungerman 2008.

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