Do Blue Laws Save Lives? The Effect of Sunday Alcohol Sales Bans on Fatal Vehicle Accidents

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Abstract

This paper analyzes the effect of state-level Sunday alcohol sales restrictions ("blue laws") on fatal vehicle accidents, which is an important parameter in assessing the desirability of these laws. Using a panel data set of all fatal vehicle accidents in the U.S. between 1990 and 2009 combined with 15 state repeals of blue laws, we show that restricting alcohol sales on Sunday has at most a small effect on fatal accident rates. Using American Time Use Survey data, we find no effect of blue laws on the location of consumption, and we show that the group whose drunk driving behavior is most affected by these laws is underage men. Overall, these results suggest that Sunday alcohol sales restrictions have fewer secular public health benefits, at least in terms of vehicle fatalities, than previously believed.

INTRODUCTION

Blue laws, or bans on certain types of commerce only on Sundays, have been widely debated throughout U.S. history. While few broad restrictions on Sunday commerce remain as of 2011, laws targeting only the sale of alcohol persist in many states.[1] For example, in 1990, 29 states and the District of Columbia had a Sunday alcohol sales ban, and as of 2011, 14 states and the District of Columbia still maintained such a ban (Distilled Spirits Council of the United States [DISCUS], 2009). A common justification for these laws put forth by policymakers is that they provide a secular benefit to society by curtailing drinking and thereby reducing alcohol-related crimes. If banning alcohol sales on Sunday has secular benefits, most notably reducing traffic fatalities by limiting drunk driving, then these laws may serve a purpose to society that goes beyond their Christian religious roots. However, if these restrictions do not have public health benefits, they become harder to justify because they entail potentially large costs in terms of lost tax revenue and consumer deadweight loss. The prevalence of these restrictions and the growing movement among states to repeal them suggests a need to understand what benefits, if any, blue laws have to secular society.[2]

[1] Throughout this paper, we use the terms "blue law" and "Sunday sales ban" interchangeably. While blue laws historically referred to a range of restrictions on Sunday, in the time period of this analysis, they pertain exclusively to Sunday alcohol sales bans.
[2] Gruber and Hungerman (2008) show that repeal of general commerce bans on Sunday in the 1960s and 1970s was associated with lower church attendance and less charitable giving. They also find that relaxing these restrictions caused an increase in drinking and illegal drug use. Our analysis focuses on Sunday alcohol sales bans, which are distinguished from these general sales bans, because during the time period of our analysis, blue laws take the form of restricting alcohol purchases almost exclusively. The only other form of commerce restricted on Sunday is vehicle sales in 13 states. There is little reason to believe that changing vehicle sales restrictions on Sunday will affect fatal accidents, however.

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There is limited evidence in the literature that alcohol sales restrictions provide public health benefits. Horverak (1983) found that a temporary closure of state-run liquor stores in Norway during a worker strike had a small negative effect on violent crime. However, this alcohol restriction applied to all days of the week, and it is unclear if a restriction on Sundays alone would cause such reductions. Olsson and Wikstrom (1982) show that the number of police incidents with intoxicated individuals decreased significantly on Saturday following implementation of a Saturday alcohol sales ban in Sweden.

This paper analyzes one of the most important potential secular benefits to society stemming from Sunday alcohol sales bans: reduced drunk driving. It is unclear *ex ante* what effect a blue law might have on this outcome, since Sunday sales bans restrict the sale of alcohol in "off-premise" locations only, such as grocery stores, convenience stores, and liquor stores. However, these laws allow the sale of alcohol "on-premise," which means customers can purchase beer and liquor for consumption in bars and restaurants on Sundays.[3] Because these laws restrict only off-premise sales, it is not obvious that one should expect them to reduce drunk driving. If, for example, enacting a blue law induces more people to consume alcohol in a bar on Sunday rather than in their homes, these restrictions could increase drunk driving even if they decrease total alcohol consumption. Whether blue laws reduce drunk driving thus is an empirical question.

Three previous analyses examine the effect of alcohol sales restrictions on drunk driving using blue law repeal variation, and they find mixed results. Ligon and Thyer (1993) estimate that a Sunday sales ban in Georgia reduced DUI arrests, but that they find negative effects on all days of the week suggests the regulation is correlated with unobserved trends in drunk driving in the region.[4]

Lapham and McMillan (2006) conducted a study between 1990 and 2000 on the consequences of repealing a blue law in New Mexico for alcohol-related vehicle fatalities and non-fatal car crashes in the state. They find surprisingly large effects of repealing a blue law: On Sundays, alcohol-related crashes increased by 29 percent and alcohol-related crash fatalities went up by 42 percent. No other days of the week showed changes in accidents or fatalities post-repeal. This study has been quite influential in the policy debate over repealing Sunday alcohol restrictions. For example, it was cited as the primary source of evidence against repealing Georgia's blue law in an op-ed written by then-Governor Sonny Perdue during a heated debate over blue laws in the state in 2008 (Perdue, 2008). Despite the policy importance of this study, its methodological approach is problematic: The authors use a single-state interrupted time series analysis due to the fact that they have data from only one state and observe only one policy change. With this methodology, it is very difficult to disentangle policy effects from spurious trends or secular shocks.[5]

Stehr (2010) shows further evidence of the limitations of the findings of Lapham and McMillan (2006). He finds that the New Mexico liquor ban repeal had a larger effect in New Mexico than in other states that repealed these laws. Furthermore, he compares changes in alcohol-related fatalities to changes in non–alcohol-related fatalities surrounding blue law repeals and finds they change similarly in all states, including New Mexico. These results cast doubt on the efficacy of Sunday alcohol sales bans for reducing drunk driving fatalities. However, the comparison of alcohol- versus non–alcohol-related fatalities is complicated by the finding that

[3] While there is some cross-state variation in when alcohol is allowed to be served on-premise, we ignore this variation because we have no historical data on such laws.
[4] It also is possible that a Sunday sales ban reduces drinking (and thus drunk driving) on all days of the week. The evidence in the literature is inconsistent with a general decrease in drinking due to an alcohol sales ban, however (Carpenter & Eisenberg, 2009).
[5] Maloney and Rudbeck (2009) provide some evidence in favor of this contention. They examine changes in alcohol-related relative to non–alcohol-related fatal crashes on Sunday in New Mexico surrounding repeal of the sales ban and find the two types of accidents change similarly.

non–alcohol-related crashes increase following blue law repeal. While such a response could be driven by omitted variables that are correlated with blue law repeals and with fatal accident trends, another compelling explanation is the existence of measurement error in the Fatal Accident Reporting System (FARS) alcohol involvement designation. Alcohol involvement is based on the responding officer's assessment of whether alcohol played a role in the fatal accident, and if this designation is subject to measurement error that is in any way correlated with blue laws, it could lead to a finding that both alcohol- and non–alcohol-related crashes increase when blue laws are repealed. Since policymakers and courts have stressed the importance of policies that reduce drunk driving,[6] the effect of Sunday alcohol sales bans on fatal accidents is an important factor in assessing the desirability of these laws. A broader examination of the connection between blue laws and fatal vehicle crashes that employs different identifying assumptions than in either Lapham and McMillan (2006) or Stehr (2010) is warranted.

This paper analyzes the effect of state-level blue laws on fatal car crashes using 15 blue law repeals over the period from 1990 to 2009 and a state-level panel of fatal vehicle crashes constructed from the FARS. We make several contributions to the literature. First, we employ difference-in-difference models that compare accident rates before and after the sales restriction repeals on Sundays across states that do and do not repeal a blue law in addition to models that compare these differences across Sundays and weekdays. Our use of a state-level panel with multiple repeals occurring in different years provides an important contribution to the literature in that we are able to employ fixed effects that make our estimates more robust to unobserved secular variation in vehicle crash rates than some previous work that has used one policy change in one year and state. Our estimates also are robust to measurement error in alcohol involvement designation. We further estimate difference-in-difference-in-difference (DDD) models that compare changes in fatal accidents surrounding blue law repeals on Sundays versus weekdays. The main identifying assumption underlying such a specification is that weekday fatal crashes are unresponsive to Sunday alcohol sales laws. We provide evidence that this assumption holds in the data in the section titled "Baseline Parameter Estimates."

In contrast to Lapham and McMillan (2006) but consistent with Stehr (2010), our results indicate at most a small effect of repealing Sunday alcohol sales restrictions on fatal accidents. In our preferred model, which compares changes across Sundays and weekdays and allows for Sunday and weekday specific state time trends, we find repealing such a law is associated with a statistically nonsignificant increase in fatal accidents of about 1 percent.

We also contribute to the literature by examining the time pattern of potential effects in an event study framework. We find no systematic trends in fatal accidents pre-repeal between our treatment and control groups, validating our methodological approach, which is based on the assumption of no selection on fixed trends. Furthermore, the event study estimates show no effect of repeal on fatal accidents even four years after the laws changed. This finding reinforces the conclusion that blue laws have little effect on fatal accidents, as the average effects are not masking large differences in fatal accident changes over time.

Finally, we add to the existing literature by examining the effect of blue laws on the location of alcohol consumption and on fatal accident rates by driver age and gender. First, using American Time Use Survey data that contain information on time use by day of the week, we show that blue laws do not cause a shift in where individuals drink. The lack of fatal accident responses to blue law repeals thus does

[6] For example, in *Michigan Department of State Police v. Sitz* (1990), the Supreme Court ruled that DUI checkpoints are constitutional because the potential benefits to society from reducing drunk driving overrode any Fourth Amendment concerns that these stops constitute unreasonable search and seizure.

not appear driven by offsetting effects on the location and volume of consumption. Second, we show that fatal accidents involving under-21-year-old male drivers are the most affected by blue law repeals. This finding is of interest because it suggests the group to which anti–drunk driving policies in general and alcohol access restrictions in particular should be targeted.

BLUE LAWS

Blue laws originally were created to mandate observance of a Sunday Sabbath, a tradition rooted in the Fourth Commandment, which requires the faithful to "remember the Sabbath, to keep it holy." As a result of British influence, blue laws were a part of the legal codes in many American colonies, including the first colony of Virginia (Lawrence-Hammer, 2007). The prevalence of these laws continued following American independence and the founding of the United States, but conflict often accompanied enforcement. Most notably, newly elected President George Washington experienced the effects of blue laws in 1793 when he was stopped on his way to church for violating a Connecticut ban on unnecessary travel on Sundays (Roland, 2007). More recently, in 2007, when the Colorado Rockies made it to their first franchise World Series, residents of Colorado were restricted from purchasing alcohol in stores during game four because it fell on a Sunday.

Throughout the 19th century, citizens choosing not to observe a Sunday Sabbath repeatedly challenged the laws. When these blue laws were challenged in court, justices often sided with the state and upheld the restrictions. Efforts were made to deregulate Sundays at times, as in the 1810 Post Office Act permitting mail service on Sundays, but supporters of the blue laws responded fiercely to the measures and often forced stricter regulation (Raucher, 1994). In 1961, the Supreme Court upheld the constitutionality of a general set of blue law sales restrictions in the landmark case *McGowan v. Maryland* (1961). The majority opinion in this case asserted that the blue laws held a secular purpose of providing for "the health, safety, recreation and general well-being of our citizens" through promoting a uniform day of rest, and therefore this general ban on Sunday commerce was not unconstitutional. Beginning in the 1950s in Iowa and as late as 1985 in South Carolina and Minnesota, states began repealing general Sunday sales restrictions (see Gruber & Hungerman, 2008, for a discussion of these general bans). By the 1990s, blue laws in the United States took the more limited form of off-premise alcohol sales restrictions rather than general commerce restrictions.

Although many states recently have repealed Sunday alcohol sales bans, blue laws remain a relevant issue. Currently, 14 states and the District of Columbia maintain a blue law pertaining to alcohol: Alabama, Connecticut, Georgia, Indiana, Minnesota, Mississippi, Montana, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Utah, and West Virginia.[7] Blue laws are somewhat geographically clustered in the South, and tend to exist in states with stronger religious traditions. However, state-level sales bans clearly are still relevant throughout the

[7] We define a blue law as restricting liquor sales on Sundays. Of the 15 states repealing blue laws during the sample, however, Arkansas, Colorado, Delaware, Kansas, New Mexico, Massachusetts, New Mexico, Pennsylvania, and Rhode Island had laws banning beer sales as well. Currently, seven of the states with Sunday alcohol sales bans restrict both off-premise spirits and beer sales. Throughout this analysis, we treat all Sunday sales bans the same. In results not reported, we have examined whether states that also ban beer experience larger fatal vehicle crash increases post-repeal than do states that only ban liquor sales. The estimates are imprecisely estimated, but the point estimates actually suggest that states with beer and liquor bans experience a decrease in fatal accidents after blue law repeal. In no case are the estimates for the two types of states statistically different from zero, and they are consistent with small effects of blue laws on drunk driving. Due to the imprecision of the estimates, we do not report them here. They are available from the authors upon request.

United States.[8] Blue laws pertaining only to alcohol sales have not been challenged directly in front of the Supreme Court, but such a challenge would undoubtedly entail a balancing of any secular benefits of these bans against their entanglement with religion. Determining whether prohibiting alcohol sales on Sunday has secular public safety benefits therefore is critical to assessing the constitutionality of alcohol-related blue laws.

By banning only off-premise sales of liquor and often beer, Sunday sales bans can either increase or decrease fatal accidents. The net impact of these laws on drunk driving behavior will be a function of how many consumers reduce their alcohol consumption due to a Sunday sales ban versus how many are induced to drink outside the home and then drive home drunk. Note that because these restrictions are commonly known and understood, forward-looking consumers can consume alcohol on Sunday at home by purchasing it on Friday or Saturday. Sunday sales bans therefore are not guaranteed to reduce consumption. If they do not restrict drinking behavior, however, it is reasonable to argue they should have negligible effects on drunk driving behavior. Using a state-level panel on alcohol sales, prices, and blue laws from 1990 to 2004, Stehr (2007) estimates that blue laws reduce total beer sales by 2.4 percent and total spirits sales by 3.5 percent. Although data limitations do not allow an analysis of Sunday-specific sales, these results suggest at most a modest effect of blue laws on drinking.

Using individual-level data on alcohol consumption, Carpenter and Eisenberg (2009) find that a blue law repeal in Ontario, Canada, led to a small overall change in alcohol consumption. However, they estimate significant shifting behavior, with consumption increasing by 6.7 percent on Sunday following repeal and decreasing by 4.5 percent on Saturdays. To the extent that these findings are generalizable to the United States, blue law repeals may serve to shift fatal accidents from Saturday to Sunday. We examine this possibility empirically as follows. Even without the shifting behavior identified by Carpenter and Eisenberg (2009), the effect of Sunday sales bans on consumption is small. These results, as well as those by Stehr (2007), call into question the finding by Lapham and McMillan (2006) that Sunday alcohol-related vehicle accident fatalities increased by 42 percent following repeal of a sales ban in New Mexico. The remainder of this paper seeks to determine whether such effects are evident in a national sample with multiple and time-varying sales ban repeals that allow us to account more fully for countervailing and unobserved trends in fatal accidents that might be correlated with these repeals.

EMPIRICAL MODELS

Our goal in this analysis is to estimate the effect of repealing a Sunday alcohol sales ban on fatal accidents in a state. Because these sales bans pertain to Sundays only, we first compare the change in Sunday crash rates from before and after a sales ban repeal to the change in Sunday crash rates in states that did not repeal a blue law. The identification assumption underlying this specification is that non-repealing states provide an accurate counterfactual for underlying trends in Sunday-specific crash rates in repealing states.

The model we use for analyzing Sunday crash rates is:

$$Ln\ (Crash/Pop_{it}) = \beta_0 + \beta_1\ BL_{it} + \partial \Upsilon X_{it} + \phi_t + \kappa_i + \theta_i t + \varepsilon_{it} \quad (1)$$

[8] In addition, many states allow local choice in implementing a Sunday alcohol sales ban. Currently, 19 states allow for this local option. Since no historical information on which areas maintain Sunday sales bans is available, we focus on state-level bans in this analysis. Excluding the states that allow local choice in determining Sunday alcohol sales bans yields results qualitatively and quantitatively similar to those shown as follows.

where *Crash/Pop* is total fatal vehicle crashes per capita in state i and in year t on Sundays. We focus on crashes per capita rather than on fatalities per capita because the number of fatal accidents, rather than the number of fatalities, is the behavioral outcome most directly associated with blue laws. If the policy reduces fatalities, it is because it reduces the number of drunk driving accidents. Using fatalities per capita produces estimates almost identical to using crashes per capita, however. These results are presented in Appendix Table A1.[9]

The variable *BL* in Equation (1) is an indicator equal to 1 if a state has a Sunday sales ban in effect, and we control for a vector of state observable characteristics and alcohol and traffic policies (X), such as the unemployment rate, log number of registered vehicles, log median income, maximum legal blood alcohol content laws, seat belt laws, the maximum state speed limit, and underage zero-tolerance laws. See the section titled "Data" for a description of these variables. Note that these policies only are relevant in the current analysis if the timing of their enactment is correlated with the timing of blue law repeals. Because such controls are common in the literature, we include them in our analysis. Equation (1) also includes state and year fixed effects that render all identifying variation to be within states over time, comparing Sunday fatal accident rate changes in repeal relative to non-repeal states in each year. As discussed as follows, there also are strong secular trends in fatal accidents occurring over this period. Our relatively long pretreatment panel allows us to include state-specific time trends in many specifications that control for separate underlying trends in crash rates in each state.

Because there may be unobserved state-specific shocks that are spuriously correlated with the timing of blue law repeal and occur differentially in repeal states, we also employ an estimator that compares changes in crash rates between repeal states and non-repeal states on Sundays versus weekdays from post-repeal relative to pre-repeal. This estimator is essentially a difference-in-difference-in-difference estimator, but instead of employing state-by-year fixed effects we control directly for the effect of blue laws on weekday fatal accidents.[10] We define a weekday as Monday through Thursday, and we exclude Friday and Saturday in most specifications because of the possibility of spillovers. Weekdays in the same state and year act as a control group for any state-by-year level policies or spurious variation in accident rates that may be correlated with the timing of blue law repeal, but affect all days of the week. We therefore are identifying the effect of blue law repeal on Sunday fatal accident rates relative to fatal accident rates on weekdays. The identification assumption supporting this approach is that weekday fatal accident rates are unaffected by blue law repeals. We show evidence below that the data are consistent with this assumption.

Our model using weekdays as the control group is as follows:

$$\begin{aligned} Ln\ (Crash/Pop_{ijt}) = {} & \beta_0 + \beta_1\, BL_{it} + \beta_2\, Sunday * BL_{it} + \partial X_{it} + \lambda_j \\ & + \phi_t + \kappa_i + \varphi_i\, Sunday * I\,(i) + \phi_t\, Sunday * I(t) \\ & + \gamma_i\, Sunday * I(i) * t + \alpha_i\, Weekday * I(i) * t + \varepsilon_{it} \qquad (2) \end{aligned}$$

where j indexes day of the week and we include fixed effects for state, year, and day of the week. In addition, we include Sunday-specific state fixed effects (φ_i) that control

[9] All appendices are available at the end of this article as it appears in JPAM online. See the complete article at wileyonlinelibrary.com.

[10] The Blue Law variable varies at the state-by-year level, so we cannot include state-by-year fixed effects in Equation (2). However, if we set the Blue Law variable equal to zero on all weekdays, a version of Equation (2) that contains Sunday-by-state, Sunday-by-year, and state-by-year fixed effects is a traditional triple difference estimator. The estimate from this model is identical to four decimal places to the estimate in Table 3, column (ii), but we prefer the estimator shown in that column to the traditional triple difference estimator because it allows us to test directly whether blue law repeals affect traffic fatalities in repeal states on weekdays.

for the possibility that states with blue laws have different baseline fatal accident rates on Sundays than states without blue laws. The Sunday-by-year fixed effects (ϕ_t) control for common shocks to Sunday crash rates relative to weekdays in each year that may be correlated with the timing of when states repealed their blue law. These fixed effects help ensure that β_2, which is our coefficient of interest, is identifying the effect of the sales ban repeal *per se* and is not biased by unobserved factors that affect crash rates on Sundays in all states in a given year. In our preferred model, we also include state-specific linear time trends separately for Sundays (γ_i) and weekdays (α_i). The day–state linear time trends allow for differences in secular trends in weekday crashes compared to trends in Sunday crashes that existed prior to blue law repeal. Including these controls allows us to identify the effect of blue law repeal on Sunday crashes relative to weekday crashes without bias from preexisting differences in trends between Sunday and weekday crashes in each state.

Estimates of β_2 in Equation (2) identify the effect of interest under two main assumptions. The first assumption is that weekday fatal accidents are unaffected by blue law repeal. A test of this assumption is given by β_1, which shows the effect of Sunday sales bans on weekday fatal accident rates. Of particular concern is that β_1 is negative, because this would be evidence that the treatment (i.e., having a Sunday sales ban) reduces control group fatal accident rates. As we demonstrate below, this coefficient typically is positive but not statistically significant, which supports the validity of using this control group.

The second main identifying assumption contained in Equation (2) is that blue law repeals are uncorrelated with trends in the dependent variable. For example, if states that are experiencing decreasing fatal accident rates on Sunday are more likely to repeal Sunday sales bans, our estimate of β_2 will understate the true effect of the law change because it would attribute this secular decline to a program effect.[11] In models that include state-by-day linear trends, any bias is driven by sharp breaks from these trends when blue laws are repealed due to unobserved factors unrelated to the sales bans. We test for this possibility empirically by estimating models in which we include dummy variables for the relative time to repeal and their interactions with Sunday. This event study model allows us to test whether the treatment and control groups are trending similarly pre-repeal, scaling calendar time such that the year of repeal is year zero. We find little evidence of differential pretreatment trends, which supports our methodological approach.

Equations (1) and (2) are log-linear in per capita crash rates.[12] Alternatively, we could estimate negative binomial or Poisson regressions using counts of total vehicle crashes. Count models are most appropriate when one has a dependent variable that takes on only integer values and has a significant number of zeros (i.e., is the sum of a Bernoulli process with a low probability of success). In such cases, the non-normality of the dependent variable gives OLS undesirable properties, making count models more appropriate. In our context, there are no observations with zero crashes, and log of per capita crashes has an approximately normal distribution, so we estimate Equations (1) and (2) using OLS. The results from specifications with number of crashes as the dependent variable estimated using a negative binomial model are similar, however, and they are available from the authors upon request.

[11] We have researched the historical record extensively to try and determine why states repealed their blue laws. The most common arguments given for repealing Sunday sales bans were to increase tax revenue and to make shopping more convenient for consumers. There is no evidence we could find that these laws were repealed because of trends in drunk driving in the state.

[12] The results are virtually identical if we use log number of crashes as the dependent variable and control for log population or if we use $\ln[(Crash/Pop)/(1 - (Crash/Pop))]$ as the dependent variable.

DATA

Vehicle Crashes

The vehicle crash data for this analysis come from the Fatal Accident Reporting System (FARS), a public database that contains information on every fatal vehicle crash in the United States. We analyze data from 1990 to 2009, for a total of 734,509 crashes. These 734,509 observations do not include 192 crashes that were dropped because the day of the week was omitted, while an additional 6,120 observations were dropped because the time of the crash was not recorded. These observations with unrecorded data represent a small percentage of the total observations, and they appear with equal frequency on all days of the week.

As discussed, we do not separate drunk driving fatalities from other non–alcohol-related fatalities in the FARS data due to concerns about measurement error in the alcohol involvement designation variable. Because this determination is subject to the discretion of the officer responding to the crash, it is susceptible to bias.[13] Since blue laws should affect drunk driving but not other types of fatal accidents, if we do find an effect of repealing blue laws on Sunday fatal accident rates relative to other days of the week that is aligned with the timing of blue law repeals, it is unclear what else would be causing this effect other than changes to alcohol-related crashes. There is no other apparent reason unrelated to drunk driving that would systemically change Sunday crashes compared to other days of the week when a blue law is repealed, which would be necessary for non–alcohol-related crashes to identify β_2 in Equation (2).[14]

Each crash is described by a range of variables including the state in which the crash took place, the date and time of the crash, and the number of fatalities. Our analysis focuses on the number of fatal accidents rather than the number of fatalities associated with these accidents. The average fatality per accident is 1.12 and is quite similar across days of the week and across years. We focus on the latter outcome because we believe it is a more accurate measure of the frequency of drunk driving, which is the margin on which alcohol restrictions are most likely to affect behavior.

Critical to our analysis is the ability to determine the day of the week on which each accident took place. In order to avoid attributing fatal accidents from late Saturday night to Sunday, we define a day of the week as starting at 8:00 A.M. and ending at 8:00 A.M. the following day. As an example, our adjusted Sunday begins on Sunday at 8:00 A.M. and lasts until Monday at 8:00 A.M.[15] Overall, crashes are evenly distributed across adjusted days of the week, excluding Friday and Saturday. Fatal accidents on Sunday and Wednesday account for about 20 percent each of all fatal accidents on these five days, Monday and Tuesday account for 19 percent each, and 21 percent occur on Thursdays.

BLUE LAWS

Information on changes in alcohol policy by state is taken from DISCUS (2009), which records and publicizes any changes to blue laws, and the National Institute

[13] For example, there is some evidence that police officers changed their alcohol classifications for teens following minimum legal drinking age increases. See Lovenheim and Slemrod (2010) for a discussion of this issue.

[14] Using total crashes surrounding changes in alcohol availability policies to identify the effect of these policy changes on drunk driving is commonplace in the literature (e.g., Lovenheim & Slemrod, 2010; Dee, 1999; Dee & Evans, 2001; Miron & Tetelbaum, 2009; Mast, Benson, & Rasmussen, 1999).

[15] The results from regressions estimated when we used 6:00 A.M., 7:00 A.M., and 9:00 A.M. as cutoffs did not vary markedly from the results when we used 8:00 A.M. as the benchmark cutoff time for a day. Results of these regressions are available upon request. This adjustment varies slightly from the adjustment to days of the week applied by Lapham and McMillan (2006), who define a day from 12:00 P.M. to 12:00 P.M. the following day. However, adjusting the days to 8:00 A.M. more accurately accounts for late-night crashes without capturing crashes that truly are the result of activities from the following day.

Table 1. Repeals of Sunday alcohol sales restrictions.

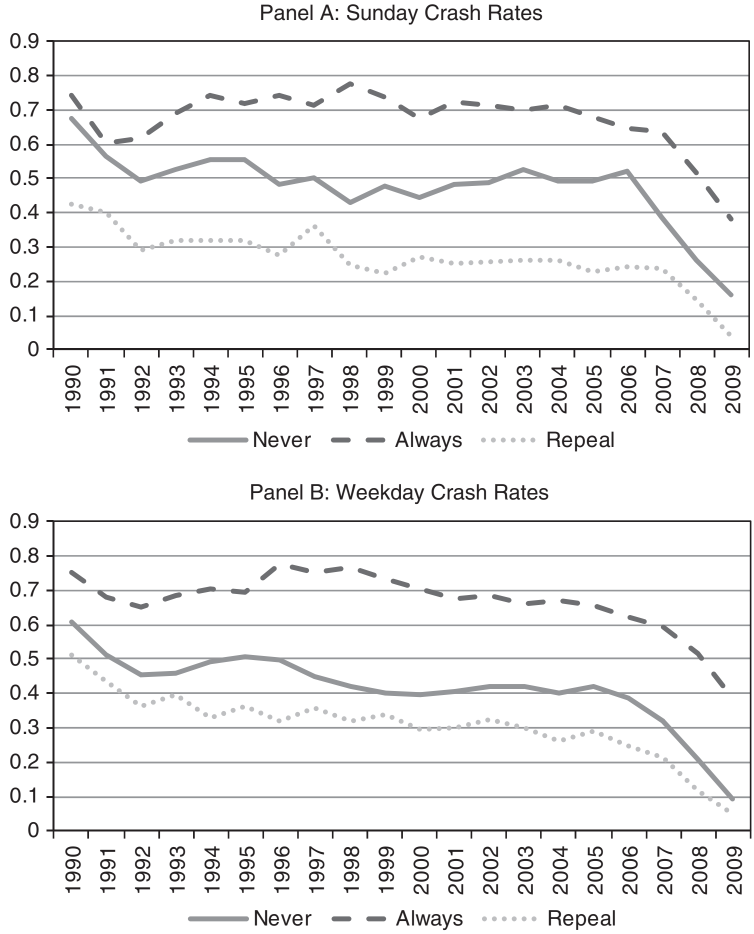
Year of Repeal	Repeal States
1990	
1991	
1992	
1993	
1994	
1995	
1996	NM
1997	
1998	
1999	
2000	
2001	
2002	OR
2003	DE, KS, PA, NY
2004	MA, OH
2005	RI, ID, KY, VA
2006	WA
2007	
2008	
2009	AR, CO

Notes: Stehr (2007) and DISCUS (2009). The year of blue law repeal differs from the calendar year of repeal in the cases in which repeal occurred after June 30th.

on Alcohol Abuse and Alcoholism, which maintains information for the years 1998 through 2009. We define a state as having a blue law if a Sunday liquor sales restriction is in place as of June 30th of each year, although many of these states also ban beer sales. If a state repeals a blue law after June 30th, we apply the change to the subsequent year. Imposing this cutoff is necessitated by the use of a state-by-year panel. In order to determine whether this cutoff affects our results, we estimated a version of Equations (1) and (2) that includes a dummy variable for whether a state changed blue law status during that year. The coefficient on this variable was close to zero and was not statistically significant at conventional levels. Furthermore, coding the midyear changes as fractional values that represent the proportion of the year covered by a blue law does not affect our estimates. Both of these robustness checks suggest that classifying blue laws in the manner we do is innocuous.

Table 1 displays blue law repeals by year. New Mexico was the first state in our sample to repeal its blue law in 1996.[16] The next repeal of a blue law did not occur until 2002, when Oregon repealed its ban on Sunday sales. The majority of changes occurred in 2003 through 2005, when nine states repealed Sunday sales restrictions. Finally, in 2009, Colorado and Arkansas repealed their bans. As Table 1 illustrates, these repeals occurred in all areas of the country and were relatively evenly spaced throughout the first half of the decade. This spacing allows us to make a significant methodological advancement over much of the previous work in this area by controlling for unobserved shocks to fatal accident rates that are correlated with blue law repeals by using within-state weekday crash rates as a control group. In our most restrictive models, we are identifying effects only off of the fact that different states repealed their restrictions in different years.

[16] Referenced dates for blue law repeal for the remainder of the paper refer to the year in which we first code a blue law as being repealed, which will differ from the calendar year of repeal in the cases in which repeal occurred after June 30th.

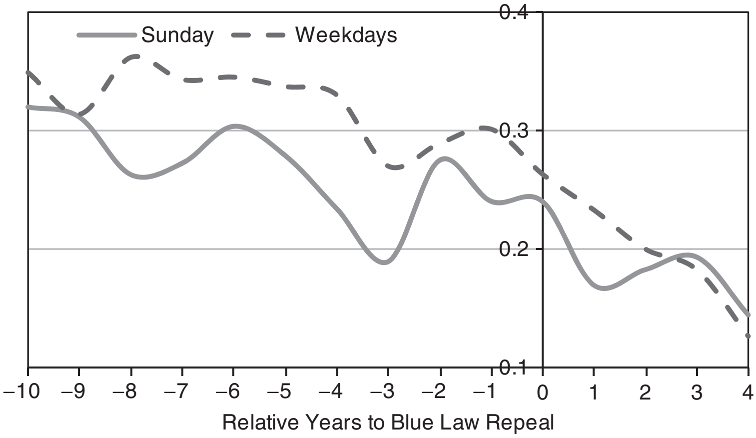


Source: FARS crash data and blue laws discussed in the text. Weekdays consist of Monday through Thursday. "Never" refers to states that do not have statewide sales restrictions during the sample period, "Always" refers to states that maintain sales restrictions throughout, and "Repeal" refers to states that repeal a blue law prior to 2010.

Figure 1. Trends in Log Crashes per 10,000 People on Sundays and on Weekdays, by Blue Law Status.

Figure 1 plots trends in log fatal vehicle crash rates per 10,000 people for states that maintain a blue law throughout the analysis, for states that never restrict Sunday alcohol sales, and for repeal states, separately for Sundays and weekdays. If blue law repeal causes an increase in Sunday fatal accidents, one should observe fatal accident rates increasing throughout the 2000s in repeal states relative to states that did not change their blue law status. Figure 1 shows no evidence of such a pattern; however, the necessity for using state fixed effects is evident as the three types of states have very different underlying per capita crash rates.

Comparing Sunday crash rate trends to weekday trends in repeal states shows that weekday crash rates began to decline in the late 1990s and Sunday crash rates stayed constant until 2006. While this could be evidence of blue law repeals increasing Sunday fatal accident rates, note that the same relative pattern occurs in states that do not ban Sunday sales at any time during the sample period. This finding suggests that the crash rate convergence that occurred in repeal states in the 2000s



Source: FARS crash data and blue law repeal dates discussed in the text. Weekdays consist of Monday through Thursday.

Figure 2. Trends in Log Crashes per 10,000 People on Sundays and on Weekdays by Relative Time to Blue Law Repeal among States that Repeal a Blue Law Between 1990 and 2009.

between weekdays and Sundays was not due to repealing alcohol sales bans. Note as well that there are strong downward trends in crash rates on all days and across all states. On all days, these trends are most pronounced for the never-repeal states and for the repeal states, and the downward trends are somewhat stronger for weekday crashes. The inclusion of state-by-day linear trends accounts for these differential underlying trends, and because most of the policy variation is occurring later in the sample, these trends are identified off of a relatively long pretreatment time trend.

Another way to examine relative fatal crash rate trends is to plot crash rates on weekdays and on Sundays by relative years to sales ban repeal among the repeal states. Figure 2 plots fatal crash rates in relative time, with year zero being the year of repeal. The figure illustrates the strong secular declines in fatal crash rates occurring on both Sundays and weekdays. The declines are stronger for weekdays than for Sundays, and there are no evident breaks from this downward trend surrounding blue law repeal. That weekday crash rates are declining more steeply throughout the period than are Sunday crash rates will bias upward (in absolute value) the estimates of blue law repeals. Post-repeal, Sunday crash rates are increasing relative to weekday crash rates, but this is clearly a continuation of pretreatment trends. The day-by-state specific linear time trends will account for much of this difference and should reduce in absolute value the estimated effect of blue law repeal, which is what we find. Overall, the comparison of crash rate trends in Figures 1 and 2 suggests that repealing Sunday sales bans will have at most a small effect on fatal accident rates and that any effect is occurring relative to a decline in fatal accidents on weekdays, not due to absolute increases on Sundays.

OTHER DATA

In our empirical models, we control for several state-level, time-varying characteristics that might differentially affect weekday versus Sunday fatal accident rates or may be correlated with blue law repeals. We include log of total vehicle registrations in Equations (1) and (2), which were taken from the Federal Highway Administration. We also control for statewide economic indicators, such as the

Table 2. Means of analysis variables.

Variable	Mean	Standard Deviation	Minimum	Maximum
Ln(*Crashes/Population*)	0.560	0.389	−2.290	2.094
*Sunday*BlueLaw*	0.069	0.253	0	1
BlueLaw	0.480	0.500	0	1
Sunday	0.143	0.350	0	1
Monday	0.143	0.350	0	1
Tuesday	0.143	0.350	0	1
Thursday	0.143	0.350	0	1
Friday & Saturday	0.286	0.452	0	1
Ln(*Population*)	15.968	0.899	13.025	17.425
UnemploymentRate	5.665	1.631	2.300	13.600
Ln(*RegisteredVehicles*)	15.715	0.889	12.290	17.340
Ln(*MedianIncome*)	10.813	0.136	10.306	11.190
.08BACLaw	0.547	0.498	0	1
SeatBeltLaw	0.477	0.499	0	1
Zero Tolerance Law	0.752	0.432	0	1
Max Speed Limit	68.062	6.028	55	90

Note: The data come from multiple sources, as discussed in the text. All means are averages over the years 1990 to 2009 and are weighted by state-by-year total population.

unemployment rate and median income. The unemployment rate is measured by the Bureau of Labor Statistics, and median income for each state and year is taken from the Census Bureau and is inflation adjusted to 2009 dollars using the Consumer Price Index for All Urban Consumers (CPI-U).

We collected data related to the use of other traffic safety laws by state and year as well. We control for the existence of a 0.08 percent blood alcohol content (BAC) law, the information on which we collected from the National Conference of State Legislatures (2004). Seat belt laws are coded as a dummy variable and are collected from the Insurance Institute for Highway Safety (2011). These laws related to seat belts are generally classified as either primary enforcement or secondary enforcement. Under primary enforcement, motorists can be pulled over by police officers for not wearing a seat belt, while under secondary enforcement, motorists must be pulled over for another reason for a seat belt citation to be issued. We classify a state as having a seat belt law only if the law falls under primary enforcement. Whether the state has a "zero-tolerance" policy for underage drinkers also is included in the models, the enactment dates for which were taken from Hingson, Heeren, and Winter (1994) and augmented using LexisNexis searches of state statutes. Finally, we control for the maximum speed limit listed in the FARS data for each state and year. Table 2 displays the means and standard deviations of the variables used in this analysis.

RESULTS

BASELINE PARAMETER ESTIMATES

Results from estimation of Equations (1) and (2) from 1990 to 2009 are reported in Table 3; columns (i) and (ii) exclude state–day linear time trends, and these trends are included in the estimates in columns (iii) to (v). Each regression in this table (and throughout this analysis) is weighted by total state-by-year population in order to make results representative of the U.S. population rather than of the states. Our results are not sensitive to using weights, however, as the unweighted estimates are quantitatively and qualitatively similar to those presented as follows. Because observations

Table 3. OLS estimates of the effect of blue laws on fatal accident rates.

	Dependent Variable: Ln (*Crashes per Capita*)				
Independent Variable	Sundays Only (i)	DDD (ii)	Sundays Only (iii)	DDD (iv)	Friday & Saturday (v)
(*Blue Law*) * *Sunday*	−0.004	−0.041	−0.047	−0.012	
	(0.032)	(0.030)	(0.035)	(0.024)	
Blue Law		0.039		−0.034	−0.022
		(0.039)		(0.043)	(0.029)
Sunday		0.042		0.040	
		(0.043)		(0.043)	
Monday		−0.043**		−0.043**	
		(0.008)		(0.008)	
Tuesday		−0.034**		−0.034**	
		(0.007)		(0.007)	
Thursday		0.069**		0.069**	
		(0.004)		(0.004)	
Saturday					−0.020**
					(0.008)
Ln(*Median Income*)	0.298*	0.362**	0.263**	0.311**	0.321**
	(0.155)	(0.114)	(0.128)	(0.091)	(0.099)
Unemployment Rate	−0.009	−0.004	−0.013	−0.007*	−0.007
	(0.007)	(0.007)	(0.009)	(0.004)	(0.005)
0.08 BAC Law	0.054*	0.040**	0.002	0.002	−0.004
	(0.030)	(0.016)	(0.024)	(0.015)	(0.016)
Seat Belt Law	−0.030	−0.043*	−0.062**	−0.060**	−0.079**
	(0.033)	(0.024)	(0.029)	(0.020)	(0.020)
Zero Tolerance Law	0.081**	0.028*	0.062**	0.013	−0.002
	(0.023)	(0.017)	(0.018)	(0.014)	(0.015)
Maximum Speed Limit	0.002*	0.002**	0.0003	0.0004	−0.0005
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(*Registered Vehicles*)	0.072	0.021	0.119*	0.027	0.006
	(0.067)	(0.062)	(0.067)	(0.049)	(0.068)
State–day trends	No	No	Yes	Yes	Yes

Notes: Authors' calculations, as described in the text. Standard errors clustered at the state level are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level. The unit of observation in each regression is the day of week by state by year. The estimates in columns (i) and (iii) contain only observations on Sundays and are based on 969 observations. The estimates in columns (ii) and (iv) contain observations from Sundays and weekdays and are based on 4,845 observations. The estimates in column (v) contain observations from Friday and Saturday and are based on 1,938 observations. In columns (iii) and (v), "State–day trends" are state-specific linear year trends. In column (iv), we control separately for state-specific linear year trends on Sundays and on all weekdays together. All regressions are weighted by state-by-year populations.

are unlikely to be independent within states over time due to serial correlation, all standard errors are clustered at the state level.

The results from estimation of Equation (1) indicate a weak correlation between blue law repeals and fatal accident rates. In column (i), the coefficient on the blue law dummy is −0.004, and it is not statistically significantly different from zero at conventional levels. In column (iii) we control for state-specific linear time trends, which changes the coefficient to −0.047. This estimate also is not statistically different from zero even at the 10 percent level. However, the lower bound of the 95 percent confidence interval is −0.067 in column (i) and is −0.116 in column (iii), which suggests at most modest effects of blue laws on fatal accidents that are substantively very different from the 42 percent effect found by Lapham and McMillan (2006).

Columns (ii) and (iv) of Table 3 present estimates of Equation (2) that use within-state and year weekday crash rates as an additional control group. The importance of including Sunday-by-state fixed effects is evident from Figure 1. In repeal states, Sunday crash rates relative to weekday crashes rates are 37 percent lower than in states that never have a Sunday sales ban and are 20 percent lower than states that maintain a sales ban throughout our sample period. Since states that never have sales bans have higher relative average Sunday crash rates than states that always maintain a sales ban, this cross-state variation will tend to cause an overstatement in absolute value of the effect of repealing a blue law. The state-by-Sunday fixed effects allow for β_2, which is the coefficient on (*Blue Law*) * *Sunday*, to be identified only off of within-state variation in blue law status over time.

In column (ii), we find that repealing a Sunday sales ban increases fatal accidents per capita by 4.1 percent. This estimate is not significant even at the 10 percent level. In column (iv), which is our preferred estimate, we control for state–Sunday and state–weekday trends and the coefficient on (*Blue Law*) * *Sunday* decreases in absolute value by about 70 percent relative to the estimate in columns (ii), to -0.012. The lower bound of the 95 percent confidence interval is -0.059, which indicates that this estimate is inconsistent with all but small effects of alcohol sales bans on fatal accidents. The reason including state–day trends reduces the magnitude of the estimate is evident in Figure 2. In repeal states, both Sunday and weekday crashes begin decreasing several years prior to repeal, but weekday crashes trend downward more sharply. Without state–day trends, some of the variation that identifies β_2 in Equation (2) is coming from differences between preexisting trends in Sunday and weekday crashes. Because the differences in trends in weekday and Sunday crash rates are evident throughout the sample period, we believe these differences alone are not evidence of a treatment effect and that state–day trends therefore are appropriate. Regardless of whether the state–day trends are included in the model, the results indicate that fatal accidents are unresponsive to blue law changes.

In both the difference-in-difference models and the triple difference models, we find little evidence that blue laws reduce fatal vehicle crashes. Recall that these estimators differ by the fact that the latter eliminates state- and year-specific shocks to fatal accidents that are correlated with blue law repeal but that are occurring evenly on weekdays and Sundays. The double difference estimator is not robust to such variation, and the fact that we find qualitatively and quantitatively similar estimates in both models suggests that there are not large state-level secular shocks in fatal crashes coinciding with the repeal of blue laws. However, there is no *ex ante* reason to believe such shocks will not be present, and so Equation (2) is still our preferred model because it identifies the effect of blue laws on fatal accidents under less stringent conditions.

One of the key assumptions underlying identification of β_2 in Equation (2) is that weekday fatal accidents are unaffected by sales ban repeals. The coefficients on *Blue Law* in columns (ii) and (iv) of Table 3 are a test of this assumption. Across columns, there is no statistically significant evidence of a relationship between blue laws and fatal accident rates on weekdays. These estimates support the validity of using Mondays through Thursdays as a control group to identify the effect of blue laws on fatal accident rates on Sunday.[17]

[17] A related issue is whether our estimates are robust to using different weekdays as a control group rather than all weekdays. Appendix Table A2 shows estimates that use each weekday separately as a control group. As the table demonstrates, our results are not particularly sensitive to which weekday is used as the control day, suggesting that grouping all weekdays together does not affect our substantive conclusions. Note that because each of the models in Table A2 includes day-by-state specific time trends, the estimates will not average to the estimate in column (iv) of Table 3. In Table 3, we include weekday-by-state time trends that are the average time trend across all weekdays. In Table A2, each day receives its own state-specific time trend. The similarity of the estimates across tables suggests that not allowing state time trends to vary across weekdays is not seriously affecting our estimates. All appendices are available at the end of this article as it appears in JPAM online. See the complete article at wileyonlinelibrary.com.

Table 4. Predicted change in fatal accidents and deaths on Friday, Saturday, and Sunday from repeal of blue laws among states with sales bans in 2010.

State	Predicted Change in Number of Crashes from Repeal	Predicted Percent Change in Crashes from Repeal	2009 Average Fatalities per Crash	Predicted Change in Number of Fatalities
Alabama	7.83	1.96	1.10	8.61
Connecticut	2.24	1.94	1.08	2.42
D.C.	0.35	1.94	1.04	0.36
Georgia	11.16	1.96	1.10	12.28
Indiana	5.82	1.95	1.11	6.46
Minnesota	3.75	2.02	1.16	4.35
Mississippi	6.43	1.99	1.09	7.01
Montana	1.57	1.89	1.11	1.74
North Carolina	12.41	1.99	1.09	13.53
Oklahoma	6.05	2.03	1.13	6.84
South Carolina	8.34	1.97	1.09	9.09
Tennessee	8.87	1.99	1.10	9.76
Texas	27.92	1.98	1.12	31.27
Utah	1.82	1.91	1.14	2.07
West Virginia	2.87	1.99	1.16	3.33

Notes: Authors' calculations, as described in the text. The states listed in the table all have blue laws restricting Sunday alcohol sales as of January 1, 2010. "Predicted Percent Change in Crashes from Repeal" is the percent change in fatal accidents from repeal taken from columns (iv) and (v) of Table 3, weighted by the proportion of accidents on Sunday versus Friday and Saturday in 2009. "Predicted Change in Number of Crashes from Repeal" is this percent multiplied by the total number of accidents in 2009 on Friday, Saturday, and Sunday in each state. "Predicted Change in Number of Fatalities" is the predicted change in the number of crashes times the average crashes per fatality.

SPILLOVERS TO WEEKEND FATAL ACCIDENTS

Despite our finding that blue laws have at most a small impact on fatal accident rates on Sundays, repeals could alter the timing of when consumers drink on the weekends. In column (v) of Table 3, we examine the effect of blue law repeals on fatal accident crash rates for Fridays and Saturdays to determine whether an alcohol restriction on Sundays could affect fatal crashes on weekends. The coefficient on *Blue Law* represents the average effect for both Fridays and Saturdays and suggests that blue laws do not lead to an increase in Friday and Saturday crashes. The coefficient is small, at -0.022, and it is not statistically distinguishable from zero.

For a policymaker deciding whether to repeal a Sunday sales ban, an important parameter is how total fatal accidents would be influenced by repeal in his or her state. In Table 4, we present results from simulations of blue law repeals in each of the 14 states (plus the District of Columbia) that maintain Sunday sales restrictions throughout our sample period. We calculate the expected change in total fatal accidents on Friday, Saturday, and Sunday using the estimates from columns (iv) and (v) of Table 3 to predict the number of accidents if each state's blue law were repealed. The predicted percent change is the weighted average of the estimates from columns (iv) and (v), where the weights are the proportion of total Friday, Saturday, and Sunday crashes on each day in each state in 2009. The predicted change in the number of crashes is equal to the percent change times the number of crashes on these three days in each state in 2009.

Consistent with the results in Table 3, Table 4 shows that repealing a blue law will increase fatal accidents very slightly in states that ban Sunday sales. The percent effects vary little across states, ranging from 1.91 percent in Utah to 2.03 percent in Oklahoma. Translating these percent effects into numbers of accidents shows that a full repeal of Sunday alcohol sales bans in all states would increase fatal accidents by about 107 per year across the country. The last column of Table 4 shows that this would lead to about 119 more deaths per year from fatal vehicle accidents. As Table 3 demonstrates, one cannot rule out statistically that repealing blue laws would have no effect on fatalities as well, but the point estimates indicate a small total effect of alcohol sales bans on fatal accidents and on vehicle fatalities.

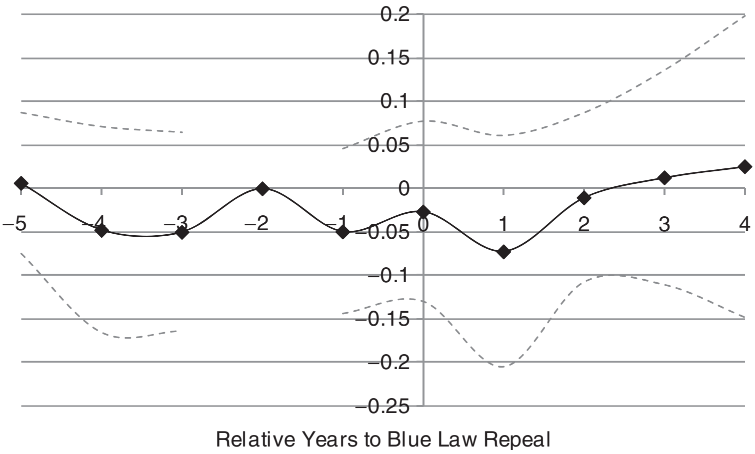
EVENT STUDY ESTIMATES

A potential bias in our empirical approach is that states that repeal Sunday sales bans may have different counterfactual relative trends in fatal accident rates over time, even after allowing for differential state-specific time trends on Sundays and weekdays. In particular, if repeal states experience declines in Sunday crashes relative to other states or relative to weekdays within the state and this decline induces them to repeal these laws, we will find a smaller effect (in absolute value) of repeal on fatal accident rates than actually occurred. We address this possibility by estimating event study models of the effect of blue law repeal on fatal crashes. We estimate the following model using Sundays and weekdays:

$$\begin{aligned} LN(Crash/Pop_{ijt}) = \beta_0 &+ \sum_{k=-5}^{4} \gamma_k I(year - repeal\_year = \tau \\ &+ \sum_{k=-5}^{4} \alpha_k * Sunday * I(year - repeal\_year = \tau) \\ &+ \delta X_{it} + \lambda_j + \phi_t + \kappa_i + \varphi_i\, Sunday * I(i) \\ &+ \theta_t\, Sunday * I(t) + \gamma_i\, Sunday * I(i) * t \\ &+ \alpha_i\, Weekday * I(i) * t + \varepsilon_{ijt} \end{aligned}$$

where I ($year - repeal\_year = \tau$) are a set of dummy variables equal to 1 if the relative time to repeal is τ. For states that do not repeal or that never have a blue law, these dummy variables are set to zero. The coefficients on the relative time dummies show mean differences in fatal crash rates on Sunday versus weekdays in each relative year to repeal. Because of the timing of repeals, we are forced to truncate the post-repeal analysis period to four years after repeal. Since a full set of relative time dummies always sums to 1 for a repeal state, we drop the relative year −2 dummy such that all estimates are relative to that year. We chose two years prior to repeal as our baseline because one year prior may be contaminated by the treatment due to midyear changes (see the section on “Data”).

Figure 3 shows the relative time coefficients (α_τ) from estimation of Equation (3). The black dots in each graph show the coefficient estimates and the dotted lines are the bounds of the 95 percent confidence interval. The break in the confidence interval for relative year −2 reflects the fact that this zero coefficient is imposed rather than estimated. The results show little evidence of differential pre-repeal trends in fatal accidents per capita once Sunday- and weekday-specific state trends are controlled for. Post-repeal, only the coefficients in years 3 and 4 are positive, although the standard errors are quite large due to the small number of states identifying each repeal age coefficient. Overall, these estimates validate our identification assumptions because there is no evidence of endogenous repeals in our preferred model. The point estimates reconfirm the finding from Table 3 that blue laws have



Source: Authors' estimation of Equation (3) as described in the text. Each point is the coefficient on the relative year to blue law repeal interacted with Sunday. Estimates come from regressions that include state-by-Sunday and year-by-Sunday fixed effects as well as separate controls for state-specific time trends on Sunday and on weekdays. The dotted lines give the bounds of the 95 percent confidence interval that is calculated from standard errors that are clustered at the state level. Relative year −2 was dropped such that all coefficients are relative to two years prior to repeal. We have included this point as 0 in the graphs above, but the gaps in the standard error bounds show that this point is imposed rather than estimated.

Figure 3. Event Study Estimates of the Effect of Blue Law Repeal on Fatal Accident Crash Rates.

at most small effects on fatal accidents; examining average effects does not mask heterogeneous effects occurring as a function of time since repeal. This is the first analysis that examines whether there is a time pattern of blue law treatment effects, and the absence of a pattern is consistent with blue laws having no influence on fatal accident rates.

EFFECTS ON WHERE CONSUMERS CONSUME

It is possible that our estimates of the effect of blue laws on fatal accidents are small because blue law repeals may both increase consumption and switch consumption from on-premise to off-premise locations. If the former are associated with more drunk driving, as one must get home from a bar or restaurant, then the net effect of blue laws may be zero even though the behavioral effects may be large. Although we lack information on alcohol consumption or sales by day, we use American Time Use Survey (ATUS) data from 2003 through 2009 to examine whether blue law repeals are associated with changes in the location of consumption. Table 5 presents estimates of the effect of blue law repeals on time spent at bars and restaurants, time spent at home eating and drinking, and whether the individual spent any time at a bar. Because the ATUS was conducted on specific days, we are able to estimate versions of Equations (1) and (2) that compare effects on Sunday in repeal states relative to other states or to weekdays.[18]

[18] We exclude linear state and day time trends from these models due to the fact that these outcomes do not exhibit strong trends over time differentially by day of the week and across states. Consequently, including these trends reduces statistical power without altering the magnitude of the coefficients, so we exclude them from the results in Table 5.

Table 5. OLS estimates of the effect of blue laws on number of minutes spent at bars and eating/drinking at home from the American Time Use Survey.

	Time Spent at Bars		Time Spent at Home Eating/Drinking		Any Time at a Bar?	
Independent Variable	Sundays Only	All Days	Sundays Only	All Days	Sundays Only	All Days
(*Blue Law*)**Sunday*	−0.405	−0.882	2.086	4.652*	0.004	0.003
	(2.854)	(2.878)	(2.285)	(2.511)	(0.024)	(0.029)
(*Blue Law*)**Weekend*		−3.790**		0.562		−0.040*
		(1.637)		(2.008)		(0.021)
Blue Law		0.238		−2.437**		0.001
		(0.862)		(1.056)		(0.012)
Dep. var. mean—Sunday		14.720		50.688		0.211
Dep. var. mean—Friday		17.903		26.880		0.250
Dep. var. mean—Saturday		16.983		31.552		0.249
Number of observations	25,080	98,774	25,080	98,774	25,080	98,774

Notes: Authors' calculations, as described in the text, from the 2003 to 2009 American Time Use Surveys. All models include state and year fixed effects and controls for log state median income, state unemployment rate, 0.08 BAC laws, state maximum speed limit, and zero tolerance laws. The models for all days include state-by-Sunday, state-by-Friday, state-by-Saturday, year-by-Sunday, year-by-Friday, and year-by-Saturday fixed effects as well. Standard errors clustered at the state level are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level. All regressions are weighted by Current Population Survey sample weights.

Our estimates suggest little to no location switching behavior.[19] The coefficients are universally small and not statistically significant for Sundays. For Fridays and Saturdays, we find some evidence that blue laws are associated with less drinking at bars and restaurants, although no change in drinking and eating at home is evident. However, as Table 3 shows, these changes do not translate into changes in fatal accident rates on these days. Overall, the estimates indicate that blue law repeals do not change the location of consumption, which suggests they also cause little change in the amount of consumption because of their small to negligible effect on fatal accident rates.

EFFECTS BY DRIVER AGE AND GENDER

Finally, we examine whether there are differences across the age and gender distribution of drivers in their response to blue law repeals. In Table 6, we estimate versions of equations (1) and (2) in which the dependent variables are the log of the number of drivers in fatal accidents in each state–day–year cell of each age group divided by the total number of residents in that age group–state–year cell. Each estimate in the table comes from a separate regression, and all regressions contain the same set of controls used throughout the analysis. These models reported in table 6 do not include state–day trends due to the smaller sample sizes for the segmented population, but the coefficient estimates remain largely unchanged if these trends are included.

For males and females in panel A, most of the effect of blue law repeal is occurring through its effect on drivers under 21. Although the standard errors are large, blue law repeal is associated with a 7.0 percent increase in fatal accident rates among under-21-year-old drivers on Sundays relative to weekdays and is associated with a

[19] We also estimate models using the ATUS data segmented by age and gender, none of which indicates changes in the location of consumption among any group. These results are available upon request.

Table 6. OLS estimates of the effect of blue laws on fatal accident rates by driver age and gender.

	Driver under 21		Driver Between 21 and 29		Driver Between 30 and 54		Driver over 54	
Independent Variable	Sundays	Sundays & Weekdays	Sundays	Sundays & Weekdays	Sundays	Sundays & Weekdays	Sundays	Sundays & Weekdays
Panel A: Full Sample								
(*Blue Law*)**Sunday*	−0.077	−0.070*	0.001	−0.001	−0.035	−0.030	−0.005	−0.014
	(0.067)	(0.040)	(0.043)	(0.040)	(0.037)	(0.041)	(0.038)	(0.026)
Panel B: Male Drivers								
(*Blue Law*)**Sunday*	−0.119*	−0.134**	−0.014	−0.053	−0.042	−0.033	−0.030	−0.042
	(0.070)	(0.064)	(0.048)	(0.041)	(0.035)	(0.039)	(0.033)	(0.030)
Panel C: Female Drivers								
(*Blue Law*)**Sunday*	0.066	0.108	0.007	0.045	0.005	−0.008	0.059	0.071
	(0.091)	(0.067)	(0.041)	(0.051)	(0.063)	(0.063)	(0.054)	(0.051)

Notes: Authors' calculations, as described in the text. Each cell is a separate regression, and the dependent variables are the log of the number of drivers in a state and year in the given age and gender category involved in a fatal accident divided by the total population in that state–age–gender–year cell. All models include state and year fixed effects and controls for log state median income, state unemployment rate, 0.08 BAC laws, seat belt laws, zero tolerance laws, state maximum speed limit, and log number of registered vehicles. The models for Sundays and weekdays include state-by-Sunday and year-by-Sunday fixed effects as well. Standard errors clustered at the state level are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level. All regressions are weighted by state-by-year populations of the given ages and gender included in the regression.

7.7 percent increase for this age group relative to non-repeal states on Sunday. The only other group exhibiting relative increases post-repeal is 30- to 54-year-olds, but estimated effects for this group are much smaller than the under-21-year-old driver estimates. For the other age groups, there is no evidence that blue law repeals increase fatal accident crash rates; indeed, most coefficients are small and none is statistically significantly different from zero even at the 10 percent level.

Panels B and C examine fatal accident rates among male and female drivers separately. There is clear evidence that all effects are driven by men, and again it is among the under-21-year-old drivers that the effects are most evident. For example, relative to weekdays, blue law repeal is associated with a 13.4 percent increase in fatal accident rates among underage male drivers. However, for female under-21-year-old drivers, the coefficient on (*blue law*) * *Sunday*, 0.108, is positive but is not statistically significant at conventional levels. It is not apparent why blue laws should cause an increase in Sunday fatal crashes relative to weekday crashes for this female age group. However, given the relatively large standard errors accompanying the point estimates, we do not want to overemphasize this finding.

The statistically significant estimate for under-21-year-old male drivers is consistent with the limited evidence in the literature that underage drinkers are more likely to subvert minimum legal drinking age restrictions through off-premise purchases. Britt et al. (2006) performed a field experiment in 1997 and 1998 in which consumers who appear underage attempted to purchase alcohol at on-premise and off-premise sites. Participants were 4 percentage points (or 17 percent) more likely to be able to purchase alcohol at an off-premise site, such as a liquor or convenience store. Wagenaar et al. (1993) surveys teens about how they obtain alcohol. The most frequent sources were friends who could legally purchase alcohol and convenience stores. These studies suggest that restricting off-premise sales will have a disproportionate effect on teen alcohol access, which is consistent with the findings of our analysis.

From a policy perspective, that blue laws primarily act through restricting underage access to alcohol is an important result. Alcohol-related blue laws are associated with potentially large social costs through lost tax revenue and distorted consumer behavior. To the extent these laws reduce drunk driving, they appear to do so mostly for underage drinkers. However, states already have laws that restrict this group's access to alcohol. The findings of our analysis suggest that any increases in fatal accidents following blue law repeal could be at least partially mitigated by policies that better enforce restrictions on alcohol access among those under 21. The exact mechanisms by which states might tighten enforcement of such laws are important topics for further study.

CONCLUSION

Using panel data on fatal accident crashes combined with 15 recent repeals of state-level Sunday alcohol sales bans, this paper analyzes whether alcohol-related blue laws provide a secular public safety benefit in reducing fatal accidents on Sundays. We employ a difference-in-difference empirical strategy that compares changes in fatal accidents surrounding repeal of a blue law on Sundays versus non-repeal states on Sunday and a triple difference estimator that compares these differences between Sundays and weekdays. The fact that different states repealed their laws in different years allows us to control for shocks occurring uniformly within each state and year from fatal accidents, which helps us to more credibly isolate the effect of Sunday alcohol sales bans on fatal accidents than previous studies in this area.

We find little evidence that Sunday alcohol sales bans reduce fatal accidents. An event study approach suggests that there are no time-varying effects, at least within four years of repeal, and our estimates show that repealing blue laws does not affect the location of alcohol consumption. Finally, we examine effects by driver age and gender and find that the most affected group is underage male drivers, who are already restricted from purchasing alcohol absent the blue law. Overall, our results are inconsistent with a significant public safety benefit from alcohol-related blue laws, at least with respect to traffic fatalities.

While beyond the scope of this analysis, determining the optimality of Sunday sales bans requires a full accounting of the costs and benefits of these laws. Sunday liquor sales restrictions entail potentially large deadweight losses related to changes in consumer behavior as well as state tax revenue loss. If these bans are optimal, it must be the case that the secular benefits outweigh these costs. A reduction in fatal accidents is but one potential secular benefit of a blue law, albeit a particularly important one, and our results are provocative in the sense that they overturn many of the commonly held beliefs about how blue laws affect drunk driving behavior. However, there are other potential secular benefits of Sunday alcohol sales bans, including reductions in crime[20] and improved public health outcomes through reduced alcohol consumption. Thus, our estimates are informative about one particular potential benefit of a sales ban, but they do not constitute a sufficient statistic for the desirability of these laws, as some policymakers have argued.

Any determination of the costs and benefits of sales bans should take into account the fact that the choice of Sunday to ban liquor sales is rather arbitrary from a secular perspective. The policy implications of findings from previous studies that show blue laws reduce fatal accidents on Sundays are not necessarily clear because one cannot identify the effect of a Sunday sales ban relative to other counterfactual policy environments, such as bans on all days or bans on weekends. If a Sunday-specific ban passes a cost–benefit test, it may be the case that it is optimal to ban liquor sales on all days of the week. If it does not, it may be hard to justify a sales ban on any day of the week. Determining the optimal liquor sales policy entails understanding what combination of days (or hours) to ban liquor sales. Our estimates indicate that banning Sunday liquor sales has negligible effects on fatal car crashes, and they suggest that *any* day-specific ban would have a similarly small effect. Relative to these estimates, however, restricting alcohol sales on multiple days of the week could increase the marginal benefit of the restriction, as it becomes harder to subvert the ban through buying on non-restricted days, but it also would increase consumer deadweight loss and decrease state tax revenue. Thus, the results from this analysis do not necessarily speak directly to this larger policy question, but they do indicate that Sunday-specific liquor sales bans do not reduce fatal car crashes. Such a parameter is important in the policy discussion of the optimal alcohol sales policy, and it suggests that in order for these bans to provide Pareto improvements in social welfare, they must have significant effects on other secular outcomes in order to justify their potentially high private and social costs.

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[20] For evidence of the effect of alcohol access restrictions on crime, see Carpenter (2007), Biderman, De Mello, and Schneider (2010), Carpenter and Dobkin (2008), Horverak (1983), and Olsson and Wikstrom (1982). Carpenter and Dobkin (2010) also provide an extensive review of the literature on alcohol and crime.

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APPENDIX

Table A1. OLS estimates of the effect of blue laws on fatality rates.

	Dependent Variable: Ln(*Fatalities per Capita*)				
Independent Variable	Sundays Only (i)	DDD (ii)	Sundays Only (iii)	DDD (iv)	Friday & Saturday (v)
(*Blue Law*)**Sunday*	−0.016	−0.054*	−0.065	−0.013	
	(0.031)	(0.032)	(0.039)	(0.023)	
Blue Law		0.040		−0.048	−0.024
		(0.038)		(0.047)	(0.030)
Sunday		0.081*		0.040	
		(0.047)		(0.043)	
Monday		−0.040**		−0.040**	
		(0.009)		(0.009)	
Tuesday		−0.035**		−0.035**	
		(0.007)		(0.007)	
Thursday		0.074**		0.074**	
		(0.005)		(0.005)	
Saturday					−0.011
					(0.018)
Ln(*Median Income*)	0.278*	0.347**	0.245*	0.292**	0.350**
	(0.161)	(0.114)	(0.136)	(0.089)	(0.111)
Unemployment Rate	−0.010	−0.005	−0.012	−0.008*	−0.008
	(0.008)	(0.007)	(0.011)	(0.004)	(0.005)
0.08 BAC Law	0.046	0.038**	−0.010	0.0	−0.002
	(0.031)	(0.015)	(0.024)	(0.014)	(0.017)
Seat Belt Law	−0.034	−0.043*	−0.074**	−0.059**	−0.074**
	(0.034)	(0.023)	(0.028)	(0.018)	(0.021)
Zero Tolerance Law	0.092**	0.031*	0.076**	0.016	−0.004
	(0.023)	(0.016)	(0.020)	(0.014)	(0.015)
Maximum Speed Limit	0.003*	0.002**	0.001	0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(*Registered Vehicles*)	0.066	0.022	0.094	0.031	0.011
	(0.074)	(0.059)	(0.074)	(0.051)	(0.067)
State-day trends	No	No	Yes	Yes	Yes

Notes: Authors' calculations, as described in the text. Standard errors clustered at the state level are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level. The unit of observation in each regression is the day of week by state by year. In columns (iii) to (v), "State–day trends" are state-specific linear year trends. In column (iv), we control separately for state-specific linear year trends on Sundays and on weekdays. All regressions are weighted by state-by-year populations.

Table A2. OLS estimates of the effect of blue laws on fatal accident rates using each weekday as a separate control group.

	Dependent Variable: Ln(*Crashes per Capita*)			
Independent Variable	(i)	(ii)	(iii)	(iv)
(*Blue Law*)**Sunday*	−0.020	−0.034	−0.052	−0.030
	(0.060)	(0.042)	(0.032)	(0.029)
Blue Law	−0.023	−0.013	0.006	−0.017
	(0.086)	(0.034)	(0.032)	(0.029)
Sunday	0.052	0.101**	0.055	−0.078*
	(0.058)	(0.041)	(0.042)	(0.045)
Ln(*Median Income*)	0.334**	0.284**	0.222*	0.334**
	(0.109)	(0.114)	(0.104)	(0.100)
Unemployment Rate	−0.007	−0.009	−0.017**	−0.005
	(0.007)	(0.006)	(0.006)	(0.006)
0.08 BAC Law	0.012	−0.0004	-0.001	−0.003
	(0.015)	(0.019)	(0.018)	(0.018)
Seat Belt Law	−0.052**	−0.076*	−0.054**	−0.060**
	(0.022)	(0.029)	(0.025)	(0.020)
Zero Tolerance Law	0.026*	0.027	0.036**	0.036**
	(0.015)	(0.019)	(0.018)	(0.012)
Maximum Speed Limit	0.0003	−0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(*Registered Vehicles*)	0.025	0.100	0.083*	0.039
	(0.061)	(0.065)	(0.049)	(0.051)
Control day	Monday	Tuesday	Wednesday	Thursday

Notes: Authors' calculations, as described in the text. Standard errors clustered at the state level are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level. All regressions are weighted by state-by-year populations and include state and year fixed effects, state-by-Sunday and year-by-Sunday fixed effects, and state-specific linear time trends separately for Sunday and for each weekday.