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The fatal toll of driving to drink: The effect of minimum legal drinking age evasion on traffic fatalities

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ABSTRACT

There is a sizeable literature on the effect of minimum legal drinking age (MLDA) restrictions on teenage drunk driving. This paper adds to the literature by examining the effect of MLDA evasion across states with different alcohol restrictions. Using state-of-the-art GIS software and micro-data on fatal vehicle accidents from 1977 to 2002, we find that in counties within 25 miles of a lower-MLDA jurisdiction, a legal restriction on drinking does not reduce youth involvement in fatal accidents and, for 18 and 19-year-old drivers, fatal accident involvement actually increases. Farther from such a border, we find results consistent with the previous literature that MLDA restrictions are effective in reducing accident fatalities. The estimates imply that, of the total reduction in teenager-involved fatalities due to the equalization of state MLDAs at 21 in the 1970s and 1980s, for 18-year olds between a quarter and a third and for 19-year olds over 15 percent was due to equalization. Furthermore, the effect of changes in the MLDA is quite heterogeneous with respect to the fraction of a state's population that need not travel far to cross a border to evade its MLDA. Our results imply the effect of lowering the MLDA in select states, such as has been proposed in Vermont, could lead to sizeable increases in teenage involvement in fatal accidents due to evasion of local alcohol restrictions.

1. Introduction

In part to reduce alcohol-related driving fatalities, Congress passed the National Minimum Drinking Age Act in 1984 that mandated all states must increase their minimum legal drinking age (MLDA) to 21 or forfeit federal highway funds. At the time of passage, only 20 states had an MLDA of 21, while 18 states had an MLDA of 19, 8 states (including the District of Columbia) had an 18-year-old MLDA, and 5 states had an MLDA of 20. By 1987, all states had adopted a minimum drinking age of 21.

Although the move to a uniform 21-year-old MLDA occurred more that 20 years ago, it is becoming policy-relevant again today as some states are considering reducing the drinking age. For example, in March 2008 the Vermont State Senate passed legislation creating a task force to consider lowering the MLDA to 18. South Dakota and Missouri also are now discussing whether to lower their drinking ages. Recently, 100 college presidents in the United States called on lawmakers to reduce the national MLDA to 18. One of the critical components of the debate over whether to reduce the legal drinking age is whether to enact a national reduction or whether to leave it up to individual states.

An important but unexamined policy parameter in this debate is the degree to which cross-state *differences* in minimum legal drinking ages induce teenage drunk driving. The introduction of the uniform 21-year-old minimum legal drinking age in the United States has generated a large volume of controversy and research over the effectiveness of this change in reducing teen traffic fatalities, but most of this research addresses the effect of *raising* (in a majority of states) the drinking age to 21, while little attention has been paid to the fact that the National Minimum Drinking Age Act also served to *equalize* drinking ages across most localities in the country. For example, in 1980, the MLDA in Ohio was 18 but was 21 in Michigan, Indiana, Pennsylvania, and Kentucky. These differences were reduced when Ohio raised its MLDA to 19 in 1983 and were eliminated completely in 1987 when Ohio raised its MLDA to 21. While Virginia had an MLDA of 18 until 1983 and then of 19 until 1985, Washington, DC had an 18-year-old MLDA until 1986, when all cross-state differences were eliminated.

If the presence of nearby lower-MLDA localities induces teenagers[1] to avoid local restrictions by crossing a border to buy alcohol, driving to get the alcohol (and more importantly driving

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[1] Throughout this analysis, we refer to "teenagers" as those who are 18, 19, or 20 years old.

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back often under the influence) makes alcohol-related accidents more likely. The act of cross-border evasion of the local MLDA therefore can itself undermine the main objective of state alcohol policies—the prevention of alcohol-related automobile accidents, especially among young drivers. Depending on the extent of cross-border evasion, introducing MLDA variation—for a given average level of the MLDAs—across states can be quite costly in terms of lives lost.

The possibility that variation in state policies can induce cross-border evasion that undermines the effectiveness of individual state policies is widely understood. The extent and impact of cross-border shopping has been studied largely in the context of taxation, where inter-jurisdictional tax differences induce consumers to purchase goods in nearby localities. Much of this literature has focused on avoidance of state excise taxes on cigarettes (Lovenheim, 2008; Stehr, 2005; Merriman, forthcoming; Coats, 1995; Slemrod, 2008; Goolsbee et al., forthcoming) and alcohol (Stehr, 2007; Beard et al., 1997) due to the large interstate excise tax differentials on these commodities, and without exception the literature concludes that this phenomenon is widespread and varies with the potential monetary savings. The tax avoidance is symptomatic of distortionary costs, including the cost of driving to the lower-tax neighboring state. What makes the variation in MDLA laws especially striking is that part of the social cost of avoiding the local law can be measured in terms of not only the lives of young drivers but also of others involved in the fatal crashes of drunk drivers returning from a night on the town.

In this paper we show empirically that, ceteris paribus, the presence of lower-MLDA border states raises youth driving fatalities in areas that are close to lower-MLDA borders. We use Geographic Information System (GIS) software to match with each U.S. county the closest locality in which an 18, 19, or 20-year-old legally can purchase alcohol and measure the population-weighted average distance from the county to that locality. Then, using data from the Fatal Accident Reporting System (FARS) covering 1977–2002, which contains information on every fatal accident in the United States, we first show that accidents involving only older drivers vary systematically with MLDA changes and with the distance to lower-MLDA borders. This variation suggests a difference-in-difference methodology is necessary to control for spurious fatal accident variation that is correlated with the timing of MLDA increases.

We then estimate such a difference-in-difference model, which identifies how the likelihood that an 18, 19 or 20-year-old driver is involved in a fatal accident relative to older drivers varies with MLDA law changes and distance to lower-MLDA borders. The results indicate that, for counties within 25 miles of a lower-MLDA border, the effect of restricting alcohol by raising the MLDA locally *increases* the likelihood that an 18 or 19-year-old (but not a 20-year-old) driver is involved in a fatal accident (relative to all drivers over 25 years old). In contrast, for counties more than 25 miles from a lower-MLDA border, raising the drinking age within a state has a negative and statistically significant effect on the likelihood that a teenage driver is involved in a fatal accident. Furthermore, although we cannot measure alcohol involvement directly, our estimates of the effect of increasing the minimum legal drinking age and of MLDA evasion are due solely to accidents occurring at night, which is consistent with alcohol use.

We conduct simulations based on our empirical estimates that decompose the total observed difference in teen-involved traffic fatalities between 2002 and each year from 1977 to 1988 attributable to MLDA changes into the part due to *raising* the MLDA and the part due to *equalizing* the MLDA. In the late 1970s and early 1980s, about 23 percent of the total MLDA-related decline in traffic fatalities was due to equalization for 18-year-old accident involvement, and for 19-year-old accidents equalization accounted for about 16 percent of the total MLDA-related decline. These estimates imply previous studies that have ignored MLDA evasion have significantly understated the potential reduction in teenage drunk driving due to completely restricting teenagers' access to alcohol, because local restrictions are partly evaded, often with fatal consequences.

Behind the average national effect lie substantial differential effects across states. For example, the existence of unequal MLDA laws raised 18-year-old involvement in fatal accidents by over 5 percent in Alabama, Delaware, New Jersey, South Dakota, and Tennessee in 1980. In contrast, Arizona, California, the District of Columbia, Idaho, Nevada, Oregon, South Carolina, Utah and Washington did not experience increased fatalities due to 18-year olds evading the minimum legal drinking age in that year. These simulations suggest, despite the fact that the effect of evasion on traffic fatalities is localized to counties within 25 miles of lower-MLDA borders, a significant portion of the national fatality reduction attributable to MLDA changes was due to the equalization of MLDAs across states in the late 1970s and early 1980s.

The rest of this paper is organized as follows: Section 2 reviews the previous literature on cross-border shopping and the effects of the MLDA on traffic fatalities. Section 3 discusses our data, and Section 4 presents estimates from a county-by-year level first-difference model with which we motivate the necessity of using our preferred difference-in-difference approach. In Section 5, we present our difference-in-difference estimator, discuss identification and show the results. Section 6 concludes.

2. Previous literature

There is a large literature concerning the effects on drunk driving of minimum legal drinking age restrictions and other traffic safety policies. Much of the early research found negative effects of both minimum legal drinking ages and beer taxes on traffic fatalities (see Wagenaar and Toomey, 2002 for a review of this literature). However, most of these studies fail to control for state fixed effects, year fixed effects, or state-specific linear time trends, which calls into question whether they identify the causal effect of policy changes. In the first study of MLDA changes and traffic fatalities that uses state and year fixed effects, Cook and Tauchen (1984) find evidence that youth auto fatality rates increased in states that lowered their drinking ages in the 1970s by between 7 and 11 percent. Using a state-level panel from 1977 to 1992, Dee (1999) allows for state-specific linear time trends and concludes that an increase in the MLDA to 21 from under 21 reduced 18–20-year-old traffic fatalities by between 9 and 11 percent. In a study using similar data and methodology, Dee and Evans (2001) find 18–19-year-old teen traffic fatalities fell by about 5 percent when states increased their MLDA to 21.[2] Ruhm (1996), Young and Likens (2000), Young and Bielinska-Kwapisz (2008), Mast et al. (1999), and Ponicki et al. (2007) corroborate the conclusion that increases in the MLDA reduce teen traffic fatalities. Miron and Tetelbaum (2009), however, suggest the effect of MLDA laws on traffic fatalities is all due to reductions from states that increased their MLDA prior to the 1984 Federal Highway Aid Act. Focusing on states that increased their drinking age after 1985, when the increases were due to the Federal Highway Aid Act and thus more

[2] The largest differences between Dee (1999) and Dee and Evans (2001) are the inclusion as an explanatory variable of beer taxes in the former analysis and the inclusion in the latter analysis of the log of the 18–19-year-old population and an indicator for a 65 mile-per-hour maximum speed limit.

plausibly exogenous, they find no effect of higher drinking ages on teen fatalities.

Very little research has addressed the extent to which individuals evade local alcohol taxes and alcohol-related laws and how this evasion affects drunk driving and traffic fatalities. This lack of evidence is somewhat surprising given the large volume of tax evasion studies in the cigarette literature. For example, Becker et al. (1994), Coats (1995), Thursby and Thursby (2000), Yurekli and Zhang (2000), Farrelly et al. (2001), and Gruber et al. (2003) all document ways that smuggling and proximity to low-tax neighbors make cigarette sales more sensitive to local tax rate changes. Goolsbee et al. (forthcoming) find that the responsiveness of taxed cigarette sales to cigarette excise tax changes is sensitive to the Internet connectivity rate in a state, suggesting pervasive Internet smuggling. Lovenheim (2008) shows that the sensitivity of cigarette consumption to the home state price varies systematically by how close consumers live to lower-price borders. Using the spatial distribution of littered cigarette packs in Chicago, Merriman (forthcoming) shows a large portion of the cigarettes apparently consumed in the City of Chicago do not have the city tax stamps, and the likelihood of having a tax stamp from lower-tax Indiana is decreasing in the distance to the Indiana border.

A parallel literature focuses on alcohol tax evasion and consumption. Beard et al. (1997) find that per-capita beer sales, but not liquor sales, are higher in areas estimated to have larger amounts of cross-border shopping. Stehr (2007) presents evidence that the responsiveness of taxed liquor sales (but not beer sales) to changes in home-state prices is sensitive to differences between the home state and border state prices; of an estimated taxed liquor sales elasticity of −1.79, 17.6 percent is due to cross-border purchasing behavior. He also finds that repealing Sunday alcohol sales bans in states both acted to increase home state sales and increase the export of liquor to other states, a finding consistent with cross-state shopping.

A smaller literature has examined the relationship of alcohol access to drunk driving and its consequences. Blose and Holder (1987) show traffic crashes increased when liquor by the drink restrictions were repealed in North Carolina in 1978, but Powers and Wilson (2004) find no evidence that Arkansas counties that prohibit alcohol sales have lower DUI arrest rates than "wet" counties. In an analysis of alcohol sale restrictions in Texas, Baughman et al. (2001) find that making a county "wet" reduces accidents when county fixed effects and county-specific trends are included in their empirical specification. This result is consistent with individuals drinking in nearby localities that allow alcohol sales and driving home intoxicated.

Finally, in the analysis most closely related to our own, Kreft and Eppling (2007) compare underage to non-underage vehicle accident fatalities in Michigan, both before and after Michigan increased its MLDA to 21 in 1979. Using a difference-in-difference-in-difference methodology, they find increasing the MLDA in Michigan did not have differential effects on underage driving fatalities in counties "close" (less than 90 miles) to a lower-MLDA border for 19 and 20-year olds. Note, though, that our results suggest counties more than *25* miles from lower-MLDA borders do not experience higher fatalities due to MLDA differentials, so defining "close" as within 90 miles may attenuate their results. Furthermore, much of their variation is due to driving to Canada rather than to other states in the United States. Because, as we elaborate on below, the likelihood of being caught while driving drunk across a national border is higher than the likelihood of being caught while driving drunk across a state border (due to customs administration), their results do not provide a complete picture of MLDA evasion in the United States, which is the aim of our analysis.

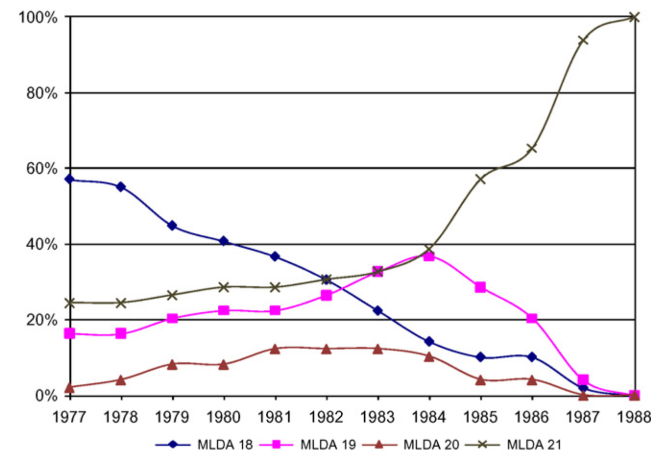

Fig. 1. MLDA distribution as of January 1 of each year, 1977–1988.
Source: State-specific minimum legal drinking age laws. After 1988, all states had an MLDA of 21.

3. Data

The accident and fatality data used in this analysis come from the Fatal Accident Reporting System (FARS), which is a census of all vehicle crashes that involved fatalities in the United States compiled by the National Highway Traffic Safety Administration (NHTSA). The FARS data set contains information on the number and age of all passengers and drivers, the county of accident and the time of day.[3]

We combine these data with state-level information on minimum legal drinking ages for each year from 1977 to 2002. Because we know the exact date of the accident, we match each accident to the MLDA regime in effect at the time of the accident, accounting for cases in which the MLDA changed in the middle of a calendar year.[4] This methodology allows a more detailed analysis of the relationship between the timing of MLDA changes and traffic fatalities than the state-level yearly average used uniformly in the previous literature.

Fig. 1 presents the distribution of minimum legal drinking ages as of January 1 in each year between 1977 and 1988. From 1988 onward, there is no MLDA variation as all states have set their MLDA to 21. Fig. 1 shows the large cross-sectional and time-series variation in minimum drinking ages during the late 1970s and 1980s. For example, in 1977, almost 60 percent of states had an MLDA

[3] FARS contains information on blood alcohol content (BAC) and police-reported alcohol involvement. However, the former measure suffers from a large amount of missing data, and the police-reported alcohol measure may be endogenous. For example, if police officers are more likely to report alcohol involvement among teens after an MLDA increase because now the teenage drunk driver is breaking multiple laws, using the alcohol involvement measure will bias our MLDA estimates toward zero. On the other hand, police officers may be less likely to report alcohol involvement after an MLDA increase due to the potential severity of increased punishment, which would bias our estimates upward. While there is no evidence on the direction or extent of bias in the self-reported alcohol or the BAC measures, we do not use either in our analysis because of the potential endogeneity and measurement error they would introduce.

[4] Many states had grandfather clauses that allowed, for example, individuals who were 18 when the MLDA increased to legally purchase alcohol. Incorporating this information into our analysis would require knowledge of each driver's birth date, which is not included in our data. In order to assess the consequences of ignoring grandfathering provisions, we tested the robustness of our estimates to including a dummy variable equal to 1 for accidents that occur in a time period in which a teenager of a given age potentially could purchase alcohol legally due to grandfathering. Our estimates were unaffected by adding this variable, which implies ignoring grandfathering has a negligible effect on our results. These estimates are available upon request.

of 18 and less than 20 percent had an MLDA of 21. Between 1977 and 1984, many states increased their MLDA from 18 to 19 or 20. In 1984, only 16.3 percent of states had an 18-year-old MLDA, but 34.7 percent had an MLDA of 19 and 12.2 percent had an MLDA of 20. While there is a noticeable rise in the proportion of states with an age-21 MLDA between 1977 and 1984, the largest rise in the prevalence of the age-21 drinking age occurred between 1984 and 1987.

Fig. 1 underscores the importance of examining accident rates separately for different ages. While restrictions increased for 18-year olds from 1977 to 1988, most restrictions on 19 and 20-year olds were enacted in the three years after 1984. The differential timing of MLDA changes suggests that aggregating all teenage accidents together might yield misleading results if restrictions have heterogeneous effects across different age groups.[5]

In order to examine the determinants of accident probabilities of each age group, we construct three separate dummy variables equal to one if an accident involves an 18-year-old driver, a 19-year-old driver, and a 20-year-old driver, respectively. Note that an accident can involve both an 18 and 19-year-old driver, in which case the first two dummy variables would both equal one.[6] These three indicator variables are the dependent variables we use in our difference-in-difference empirical model. Although this specification of the dependent variable is unique in this literature, in Section 5 we explain that the identifying variation is similar to the variation used to identify the count models employed in most previous work.

Combining the FARS data with state-by-date MLDA information, we construct a set of variables called *restricted* that equal one if a driver of age 18, 19 or 20 is below the MLDA in the state where the accident occurred on the day of the accident. For example, if a state-year MLDA is 19, *restricted* would equal one for 18-year-old drivers and equal zero for 19 and 20-year-old drivers.

We then construct another measure of access to alcohol for those restricted in their home state—the distance to a lower-MLDA locality. This lower-MLDA locality can be either another state or another country, i.e., Mexico or Canada. Because the most specific level of geographic identification in the FARS data is the county, we construct county-level population-weighted average distances to lower-MLDA borders. We follow the methodology used in Lovenheim (2008) for a similar application regarding cigarette smuggling, which entails, for each Census block point, finding the minimum crow-flies distance to a road crossing into another state. Taking a population-weighted average at the county level across block points yields the population-weighted average distance. This methodology has the benefits of measuring distance from the population center of a county rather than a more arbitrary county seat or geographic center. Road crossings and Census block point locations and populations are taken from the 2000 Census Tiger files.[7] For each accident, we match the county in which the accident occurred with the closest lower-MLDA border where an 18, 19, or 20-year-old can drink legally on that date. Note the closest lower-MLDA border is often, though not always, a border state. We include the Canadian and Mexican borders in our analysis; because of the uniform U.S. age-21 MLDA after 1987, in that period all distances are to either Canada or Mexico.[8]

In addition to measuring the distance to the closest lower-MLDA state, we construct a dummy variable (*Border County*) equal to one if the county where an accident occurs borders a state with a higher MLDA. For example, when the variable of interest is the likelihood of a 19-year-old being involved in a fatal accident, *Border County* will equal 1 if the county of the accident borders a state with an MLDA over 19 and if the MLDA in that county is 18 or 19. This variable identifies the counties into which individuals evading MLDA restrictions most likely will go to drink legally.

We also control for a standard set of state-level anti-drunk-driving policies in order to account for changes in other policies that may affect teenage drunk driving relative to drunk driving among older drivers. In particular, we construct dummy variables for enactment dates for 0.08 illegal per se laws taken from a compilation by the National Conference of State Legislatures (2004). These laws made it illegal *per se* to be driving with a BAC over 0.08, which increased the legal stringency and the ease of prosecution for drunk driving among those over 21. We therefore expect these laws to reduce fatal accidents with drivers over 21 relative to fatal accidents with teen drivers. Because we expect them to reduce the relative frequency of teen involvement in fatal accidents, we also control for "zero tolerance" underage drinking laws that make it illegal for an underage drinker to have either a greater than zero or a greater than 0.02 blood alcohol content. Enactment dates were taken from Hingson et al. (1994) and augmented and updated using LexisNexis searches of state statutes.

We make use of a dummy variable equal to one if a state has either a primary or secondary seatbelt law; dates of enactment are taken from the Insurance Institute for Highway Safety (2008). Because states in which there is more vehicle use may have higher non-teenage accident rates, we control for annual vehicle miles traveled per capita at the state level. Vehicle miles traveled (VMT) come from the Federal Highway Administration's *Highway Statistics* compilations from 1977 to 2002, and state-level populations are taken directly from the U.S. Census Bureau. Finally, we construct a measure of average state beer taxes in each year, in real 2005[9] cents per gallon, from the World Tax Database, maintained by the Office of Tax Policy Research at the University of Michigan. It is unclear *ex ante* whether beer taxes will affect teen drunk driving differently than drunk driving by those over 21. We include beer taxes as control variables in our analysis because it is common to do so in the literature and because they could have heterogeneous impacts on the behavior of different age groups.

For all legal variables other than MLDA laws and beer taxes, when a law was enacted in the middle of a year, we set the given indicator variable equal to one if the law was enacted prior to July 1 of the year, if not, it is set to zero for that year and one for each subsequent year.

Table 1 contains means and standard deviations of the variables used in our analyses and charts the timing of the MLDA changes and their effect on the time path of the *restricted* and distance variables.

[5] The timing of MLDA changes is one explanation for why Miron and Tetelbaum (2009) do not find MLDA effects post-1985. As most states had already increased their MLDA to 19 or higher, 18–20-year-old fatalities will respond less to changes post-1984 than pre-1984. This does not mean, however, that 19-year-old fatalities responded less to these MLDA changes.

[6] There are few accidents that involve multiple teen drivers. Only 4.6 percent of accidents involving 18-year-old drivers also involve a 19 or 20-year-old driver. Similarly, only 4.6 percent of accidents involving a 19-year-old also involve an 18 or 20-year-old. Among accidents involving 20-year olds, 4.7 percent involve an 18 or 19-year-old as well.

[7] See Lovenheim (2008) for a more complete discussion of the distance calculation. One complication with this methodology is that we fix the population distribution at 2000 levels. While data limitations necessitate this method, unless populations are shifting contemporaneously with MLDA changes and systematically with respect to lower-MLDA borders, within-county population changes over time will not bias our results.

[8] In Section 5.4, we present evidence that, if anything, including Canada and Mexico attenuates our results.

[9] We use the CPI-U to convert beer taxes into real 2005 cents.

Table 1
Means of selected variables by year.

Variable	1980		1990		2000	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Average number of accidents with 18-year-old drivers	0.948	2.300	0.690	1.716	0.638	1.436
P(Driver = 18)	0.082	0.275	0.061	0.239	0.060	0.237
Restricted 18	0.529	0.499	1.000	0.000	1.000	0.000
Restricted 18*$I(D < 25)$	0.092	0.289	0.019	0.138	0.015	0.123
Restricted 18*$I(25 < D < 50)$	0.090	0.287	0.004	0.066	0.005	0.068
Restricted 18*$I(50 < D < 75)$	0.067	0.250	0.008	0.089	0.007	0.084
Restricted 18*$I(75 < D)$	0.279	0.449	0.968	0.176	0.973	0.162
Border County 18	0.078	0.268	0.000	0.000	0.000	0.000
Average number of accidents with 19-year-old drivers	1.060	2.715	0.737	2.033	0.633	1.596
P(Driver = 19)	0.091	0.288	0.065	0.246	0.059	0.236
Restricted 19	0.392	0.488	1.000	0.000	1.000	0.000
Restricted 19*$I(D < 25)$	0.092	0.288	0.034	0.186	0.028	0.167
Restricted 19*$I(25 < D < 50)$	0.061	0.240	0.017	0.128	0.014	0.119
Restricted 19*$I(50 < D < 75)$	0.047	0.212	0.029	0.168	0.025	0.156
Restricted 19*$I(75 < D)$	0.192	0.394	0.918	0.274	0.932	0.252
Border County 19	0.072	0.259	0.000	0.000	0.000	0.000
Average number of accidents with 20-year-old drivers	0.974	2.633	0.670	2.030	0.587	1.507
P(Driver = 20)	0.085	0.278	0.059	0.236	0.055	0.228
Restricted 20	0.362	0.481	1.000	0.000	1.000	0.000
Restricted 20*$I(D < 25)$	0.078	0.269	0.036	0.186	0.029	0.168
Restricted 20*$I(25 < D < 50)$	0.051	0.220	0.017	0.128	0.014	0.118
Restricted 20*$I(50 < D < 75)$	0.044	0.205	0.029	0.167	0.024	0.154
Restricted 20*$I(75 < D)$	0.189	0.392	0.919	0.273	0.932	0.251
Border County 20	0.063	0.242	0.000	0.000	0.000	0.000
Log VMT Per Capita	−9.460	1.112	−9.033	1.119	−8.520	1.058
Seatbelt Law	0.000	0.000	0.843	0.364	0.997	0.056
Zero Tolerance Law	0.000	0.000	0.022	0.146	1.000	0.000
0.08 BAC Law	0.000	0.000	0.142	0.349	0.442	0.497
Log Real Beer Tax	4.626	0.302	4.256	0.339	4.552	0.288
Number of fatalities	1.130	0.448	1.119	0.417	1.117	0.421
Number of accidents	43,552		39,427		37,220	

Source: Authors' calculations as described in the text.

In 1980, 52.9 percent of accidents occurred in areas in which an 18-year-old could not drink. Overall, 9.2 percent occurred in counties within 25 miles of a lower-MLDA border, 9.0 percent occurred between 25 and 50 miles of a lower-MLDA border, and 6.7 percent occurred between 50 and 75 miles of a lower-MLDA border. Furthermore, 7.8 percent of accidents occurred in counties in which the MLDA was 18 but that bordered a state in which the MLDA exceeded 18.

The distance distributions in 1980 are quite similar across age groups, but as a proportion of total "restricted" accidents, the proportion within 25 miles is higher for 19 and 20-year olds. Also, note that the fraction of total fatal accidents involving an 18, 19, or 20-year-old driver is much higher than their share of the population, but this fraction declines over the time period we are studying. For example, in 1980, 18-year-old and 19-year-old drivers were involved in 8.2 and 9.1 percent of accidents, respectively. By 1990, these percents had dropped to 6.1 and 6.5. Thus, the likelihood of teen involvement in a fatal accident declined during the period when MLDA changes were occurring.[10] Left unexplored in Table 1 is how much of these concurrent declines can be attributed to raising the MLDA, how much can be attributed to equalizing the MLDA, as well as how much of the decline was unrelated to MLDA changes. The remainder of this paper seeks answers to these questions.

4. County-level aggregate analysis

The majority of previous work on minimum legal drinking ages has used as a dependent variable either state-level counts of teen traffic fatalities (Cook and Tauchen, 1984; Dee and Evans, 2001; Dee, 1999; Chaloupka et al., 1993; Saffer and Grossman, 1987; Miron and Tetelbaum, 2009; Young and Likens, 2000; Young and Bielinska-Kwapisz, 2008) or state-level counts of fatalities in which teen drivers were involved (Kreft and Eppling, 2007).[11] In such models, spurious shocks to drunk driving behavior or traffic safety within states that are correlated with the timing of MLDA laws may bias the estimated MLDA treatment effect. Nevertheless, in order to establish a point of comparison with the past literature and to motivate our preferred difference-in-difference methodology, in what follows we estimate first-difference county-by-year aggregate count models of the number of accidents with drivers of

[10] Note that there were small further declines, particularly for 19 and 20-year olds, between 1990 and 2000 when there were no changes in state MLDAs.

[11] Previous research has not distinguished between examining 18–20-year-old fatalities and fatalities caused by 18–20-year-old drivers. Although the former has been studied more frequently, we believe the latter will more accurately capture teen drunk driving because an intoxicated teen may cause a fatal accident in which there are no 18–20-year-old fatalities, and an 18–20-year-old fatality can occur in an accident in which there are no underage drivers. Cook and Tauchen (1984) make this same argument. Furthermore, we analyze the number of fatal accidents rather than the number of fatalities caused by these accidents. In the aggregate, this difference is not significant because the mean number of fatalities per accident is slightly over one and has changed little over time. We have estimated our models using the number of fatalities as the dependent variable, and the results are quantitatively and qualitatively similar. These estimates are available upon request from the authors.

a given age:

$$Numacc^{a}_{jst} = \exp(\alpha_0 + \alpha_1 restricted_{st} + \pi_1 I(D < 25)_{jt} * restricted_{st} + \pi_2 I(25 < D < 50)_{jt} * restricted_{st} + \pi_3 I(50 < D < 75)_{jt} * restricted_{st} + \delta(Border\ County)_{jt} + \theta X_{st} + \lambda_j + \tau_t + \phi_s * t + \varepsilon_{jst}), \quad (1)$$

where $Numacc^a$ is the number of accidents with a driver of age a in county j in state s and in year t. We estimate this specification using a fixed effects Poisson model separately for ages 18, 19, and 20.

The distance indicator variables, $I(D<25)_{jt}$, $I(25<D<50)_{jt}$ and $I(50<D<75)_{jt}$, measure the availability of alcohol for those whose access is restricted in their home state. These variables are set to one if the distance to the closest lower-MLDA locality in which an individual of age a legally can purchase alcohol is in the given range of miles. Parametric specifications of the correlation between distance and MLDA evasion (as in Lovenheim, 2008) are complicated by the possibility of a non-linear relationship, because counties that are farther away from lower-MLDA borders may be less likely to have evasion traffic, but any evasion traffic in those counties may be at a higher risk for a drunk driving accident because the affected individuals are (and have been) driving longer distances. Specifying dummy variables as in Eq. (1) allows us to examine the relationship between MLDA restrictions and cross-border alcohol access without putting any strong parametric structure on this relationship.

The parameter α_1 is an estimate of the effect of MLDA restrictions in counties more than 75 miles away from the closest lower-MLDA border in which a person of age a can drink, and the π coefficients allow for heterogeneous effects of MLDA restrictions depending on the county's distance from such a border. Under the assumption that counties more than 75 miles away from a lower-MLDA border experience no evasion traffic, a negative estimated value of α_1 would indicate that MLDA restrictions reduce the number of fatal collisions with a driver of age a, and positive values of π would be evidence of consequential MLDA evasion behavior.

Our estimates of α_1 and π (in common with estimates from similar studies) unavoidably include the impact of unmeasured responses of state and local law enforcement policies to the MLDA changes. For example, when New York State raised its drinking age from 19 to 21 in 1984, they at least temporarily increased highway patrols along the border with Vermont, where the legal drinking age was 18. When Vermont increased its MLDA to 21 in 1986, both Vermont and New York increased highway patrols near the Canadian border to attempt to reduce cross-border drinking and driving. The likely behavioral responses of teenagers to these endogenous policy changes suggest, if anything, we are understating the potential effect of MLDA differentials on youth traffic fatalities, holding other policy changes constant.

Note also that, although we can measure accurately the location of an accident, we do not know in which county the teenage driver lives. Our estimates can only be used to calculate how far individuals are willing to travel to evade local MLDA laws to the extent that the location of the accident is correlated with the residence of the drivers.

Eq. (1) includes county fixed effects (λ_j), year fixed effects (τ_t), and state-specific linear time trends ($\phi_s{}^*t$). The variation in the distance indicators is thus due to within-county changes over time caused by a change in the MLDA in the county's home state or another state. Including year fixed effects allows us to account for contemporaneous year-specific shocks to teen-driver fatal accidents. It is possible to include these year effects because there is a large amount of cross-time variation in the timing of MLDA changes (see Fig. 1). The state-specific linear time trends control for spurious correlation between secular changes in teen drunk driving, teen driver safety and MLDA changes. As Table 1 illustrates, the likelihood of a teen being involved in a fatal accident and the number of accidents involving teens declined significantly between 1980 and 1990 and continued to decline, although more slowly, between 1990 and 2000. While some of this reduction likely was due to MLDA changes, the rise of Mothers Against Drunk Driving also occurred during this time period, and their lobbying and educational efforts arguably reduced drunk driving among teenagers. Including state-specific linear time trends identifies MLDA effects off of short-run breaks from state-level trends that are not confounded by longer-run secular declines occurring over our 25-year panel.

Our use of a Poisson fixed effects count model is justified by the non-normality of the dependent variable due to many county-years without fatal accidents with teen drivers of a given age. However, as Table 1 illustrates, the data are over-dispersed as it is always the case that the variance of the dependent variable is larger than its predicted variance (which, in a Poisson distribution, equals the mean). This over-dispersion will cause our standard errors to be understated. We therefore report standard errors that are the average of 200 bootstrap replications, clustered at the county level. This bootstrapping method adjusts for the standard error estimation biases stemming both from over-dispersion and from within-county serial correlation.

Table 2 contains the results from estimation of Eq. (1) on the 1977–2002 FARS data. The estimates show evidence that (i) MLDA evasion significantly increases the number of fatal accidents with teenage drivers and that (ii) a binding MLDA significantly reduces teenage involvement in fatal accidents far from borders. For example, in column (i), we find that within 25 miles of a lower-MLDA border, increasing the MLDA actually *increases* the number of fatal accidents with 18-year-old drivers by 5.7 percent (−0.122 + 0.179). We also see evidence of MLDA evasion reducing the effectiveness of MLDA restrictions in the 25–50 mile range. However, after 50 miles to a lower-MLDA border, restricting access of 18-year olds to alcohol significantly reduces the number of fatal accidents with drivers of this age. Similar effects are evident for 19-year olds in Table 2, although for 20-year olds we do not find statistically significant evidence of evasion.

Eq. (1) identifies the effects of restricting teen access to alcohol on the number of accidents with teenage drivers under the assumption that changes to MLDAs (and changes in the distance to a lower-MLDA border) are conditionally exogenous. In other words, conditional on the observables, there are no spurious trends or secular shocks in fatal accidents correlated with changes in MLDA laws. In order to test this assumption, we estimate a version of Eq. (1) with the number of fatal accidents with 21–25-year-old drivers and no drivers under 21 and the number of fatal accidents with drivers over 26 only.

Table 3 shows our estimates of the effect of MLDA law changes on fatal accidents only with older drivers. Particularly for 18-year-old restrictions, there is strong evidence that all fatal accidents are reduced far from lower-MLDA borders when the MLDA is raised above 18. Furthermore, these effects differ systematically with the distance to these borders. Recall that most of the changes to 18-year-old MLDA states occurred prior to the imposition of a uniform 21-year-old drinking age by the federal government in 1984. The results in Table 3 suggest that these changes were correlated with overall declines in fatal accidents that occurred differentially

Table 2
Poisson estimates of the effect of MLDA changes on the number of fatal accidents with drivers of a given age, aggregate county-level estimates from 1977 to 2002.

Independent variable	Dependent variable: number of fatal accidents		
	Driver age		
	18 (i)	19 (ii)	20 (iii)
Restricted	−0.122**	−0.092**	−0.076**
	(0.023)	(0.028)	(0.031)
Restricted*$I(D<25)$	0.179**	0.102**	0.064
	(0.035)	(0.036)	(0.048)
Restricted*$I(25<D<50)$	0.078**	0.073	0.007
	(0.031)	(0.049)	(0.037)
Restricted*$I(50<D<75)$	0.036	−0.059	−0.068*
	(0.037)	(0.037)	(0.039)
Log VMT Per Capita	−0.519**	−0.637**	−0.641**
	(0.047)	(0.050)	(0.051)
Seatbelt Law	−0.010	0.037*	0.020
	(0.023)	(0.022)	(0.025)
Zero Tolerance Law	−0.010	0.021	−0.007
	(0.032)	(0.029)	(0.032)
0.08 BAC Law	0.024	0.044	0.079**
	(0.025)	(0.030)	(0.028)
Log Real Beer Tax	−0.463**	−0.480**	−0.464**
	(0.054)	(0.070)	(0.073)
Border County	0.047	0.034	0.012
	(0.041)	(0.038)	(0.036)
Number of observations	70,997	70,997	70,991
Number of clusters	3,108	3,108	3,108
Mean of Dep. Var.	0.724	0.747	0.707

Source: Authors' estimation of Eq. (1) as described in the text. The estimates in columns (i), (ii) and (iii) use the total number of fatal accidents with 18, 19 and 20-year-old drivers, respectively, as the dependent variable.
All estimates include county and year fixed effects as well as state-specific linear year trends.
Standard errors are the average of 200 bootstrap replications clustered at the county level. **Significance at the 5-percent level. *Significance at the 10-percent level.

by distance to lower-MLDA borders. Because the dependent variables exclude all accidents with drivers under the age of 21, we believe it is unlikely these coefficients are picking up a causal effect of MLDA changes on fatal accidents with older drivers. A more reasonable interpretation of the estimates in Table 3 is that changes in MLDA laws were spuriously correlated with changes in traffic safety enforcement. That fatal accidents with older drivers decreased less (or increased) close to lower-MLDA borders suggests that the geographic heterogeneity in response increasing the MLDA among teenage drivers is not fully due to evasion.

While we are unable to determine why accidents not involving teenage drivers vary systematically with MLDA law changes and distance to a lower-MLDA border, the estimates in Table 3 suggest a first-difference approach as given by Eq. (1) overstates (in absolute value) the effect of MLDA increases and evasion on fatal accidents with teenage drivers. A difference-in-difference approach therefore will give a more credible estimate of these effects, because it will force the change in teenage fatal accidents to be relative to the change in fatal accidents among older drivers that are unlikely responding to MLDA restrictions.

5. Accident-level difference-in-difference analysis

5.1. Empirical model

The main impediment to conducting a difference-in-difference analysis in the current framework is the non-linearity of the count model. In a linear setting, one could estimate this difference-in-difference model by using interactions with driver age and the restriction indicator variables. In Eq. (1), however, interpreting interaction terms becomes very difficult due to non-linearities.[12] We therefore restrict ourselves to a linear model and analyze the proportion of accidents that include a driver of a given age. Furthermore, because several MLDA changes occur in the middle of the year, we disaggregate the data to the accident level in order to maximize the amount of variation we can use.[13] In disaggregated form, our focus is on the likelihood an accident includes a driver of a given age[14]:

$$\begin{aligned} I(Age_{ijst}=a) = {} & \beta_0 + \beta_1 restricted_{st} + \gamma_1 I(D<25)_{jt} * restricted_{st} \\ & + \gamma_2 I(25<D<50)_{jt} * restricted_{st} \\ & + \gamma_3 I(50<D<75)_{jt} * restricted_{st} \\ & + \delta(Border\ County)_{jt} + \theta X_{st} + \lambda_j + \tau_t + \phi_s * t + \varepsilon_{ijst}, \end{aligned} \tag{2}$$

where $I(Age_{ijst}=a)$, is an indicator variable equal to one if accident i in county j in state s and in year t includes a driver of age a.

Eq. (2) differs from Eq. (1) in two basic but fundamental ways.[15] First, Eq. (2) is linear whereas Eq. (1) is exponential. As previously discussed, we use the linear model to make the difference-in-difference estimator more tractable. The second major difference between the models is that Eq. (2) identifies the parameters of interest, β_1 and γ_1 through γ_3, as the change in fatal accident involvement among teenage drivers relative to older drivers when minimum legal drinking ages increase. In Eq. (1), the estimates identify only the change in fatal accidents with teen drivers due to an MLDA increase. Thus, for the specification with $a=18$, γ_1 divided by the mean of the dependent variable should be approximately equal to the difference between the π_1 estimate using 18-year-old driver accidents and the π_1 estimate using older driver accidents.[16] While one could perform this subtraction manually using Tables 2 and 3, one cannot undertake hypothesis tests in this manner, which necessitates the use of Eq. (2).

It is important to emphasize that the identifying variation used to estimate the parameters in Eqs. (1) and (2) is identical. The only differences between Eq. (2) and models that use the number of

[12] For example, in a simple model with $E[y|x]=\exp(\beta_0+\beta_1x_1+\beta_2x_2+\beta_3x_1x_2)$, $(\partial^2 E[y|x]/\partial x_1\partial x_2)=\beta_3E[y|x]+(\beta_3x_1+\beta_2)E[y|x](\beta_1+\beta_3x_2)$, not β_3 as in a linear model.

[13] Table A1 shows parameter estimates from a county-by-year aggregate model in which the dependent variable is the proportion of accidents with a driver of a given age. These estimates are qualitatively and quantitatively similar to the disaggregated estimates we show below. Furthermore, Appendix Table A1 shows our estimates change negligibly when we include county-specific linear year trends in the model. Due to the computational burden of including these trends and the fact that secular declines in drunk driving were more likely the result of state-level policies and educational campaigns regarding drunk driving, we do not include them in our analysis.

[14] The analysis sample extends well beyond the time that all states had increased their MLDA to 21. We use this time frame in order to obtain more precise estimates of state fixed effects, state-specific time trends, and the effect of other policy variables. In Appendix Table A2, we show results from models estimated over the period 1977–1994; although they exclude several of the uniform 21-year-old MLDA years, they are similar to our main results presented below.

[15] A third but minor difference between the two estimating equations is that the dependent variable used in Eq. (2) is subject to "division bias." Because an increase in teen fatal accidents increases all fatal accidents, the percent change in teen-involved fatal accidents as a proportion of all fatal accidents will be slightly smaller than the percent change in teen fatal accidents for a given policy response. This bias is small and its effect on our estimates are second-order. For example, if there are 100 accidents and 9 involve teenagers, an increase of 1 fatal accident represents an 11.1 percent increase (=1/9) in the total number of teen fatal accidents but a 10.0 percent increase (=((10/101) − 0.09)/0.09) in the proportion of fatal accidents with a teen drunk driver.

[16] Our estimates in Table 4 below are very similar to what one would obtain from performing the described subtraction of estimates manually across columns in Tables 2 and 3.

Table 3
Poisson estimates of the effect of MLDA changes on the number of fatal accidents with drivers over 20, aggregate county-level estimates from 1977 to 2002.

Independent variable	Dependent variable: number of fatal accidents					
	Driver age					
	21–25 (i)	21–25 (ii)	21–25 (iii)	26+ (iv)	26+ (v)	26+ (vi)
Restricted 18	−0.047**			−0.048**		
	(0.020)			(0.014)		
(Restricted 18)*I(D < 25)	0.106**			0.066**		
	(0.022)			(0.014)		
(Restricted 18)*I(25 < D < 50)	0.076**			0.080**		
	(0.029)			(0.014)		
(Restricted 18)*I(50 < D < 75)	0.051**			0.018		
	(0.021)			(0.018)		
Border County 18	0.047*			0.010		
	(0.025)			(0.017)		
Restricted 19		−0.012			−0.038**	
		(0.019)			(0.015)	
(Restricted 19)*I(D < 25)		0.040*			0.011	
		(0.026)			(0.020)	
(Restricted 19)*I(25 < D < 50)		0.015			0.046*	
		(0.033)			(0.025)	
(Restricted 19)*I(50 < D < 75)		−0.048**			−0.004	
		(0.020)			(0.016)	
Border County 19		0.026			−0.003	
		(0.026)			(0.016)	
Restricted 20			−0.026			−0.040**
			(0.019)			(0.015)
(Restricted 20)*I(D < 25)			0.035			0.009
			(0.029)			(0.021)
(Restricted 20)*I(25 < D < 50)			0.017			0.051*
			(0.037)			(0.026)
(Restricted 20)*I(50 < D < 75)			−0.034			−0.002
			(0.022)			(0.016)
Border County 20			0.015*			−0.003
			(0.027)			(0.016)

Source: Authors' estimation of Eq. (1) as described in the text. The estimates in columns (i), (ii) and (iii) use the total number of fatal accidents with 21–25-year-old drivers and no drivers under 21. The estimate in columns (iv)–(vi) use the total number of fatal accidents with drivers over 25 only.
All estimates include county and year fixed effects, state-specific linear year trends, and control for log VMT per capita, seatbelt law, zero tolerance law, 0.08 BAC law and log real state beer tax.
Standard errors are the average of 200 bootstrap replications clustered at the county level. **Significance at the 5-percent level. *Significance at the 10-percent level.

accidents or fatalities, such as Eq. (1), are functional form and the use of non-teen involved accidents as a control group. The count model given by Eq. (1), which is similar to those used in the majority of previous work, is a first-difference model, whereas Eq. (2) is a difference-in-difference model with the second difference defined by accidents that do not include teenage drivers. Relative to Eq. (1), the estimates in Eq. (2) will be robust to any county-specific shock that affects the treatment and control groups equally. Identification of the effect of MLDA changes on teen traffic fatalities thus is achieved under less restrictive assumptions using Eq. (2) than when a first-difference model is employed.[17]

Note that selection issues, in particular, are not different across the two models. Analyzing the FARS data embodies an implicit selection criterion, which is that a recorded accident has to include a fatality. The passage of an MLDA law can affect this selection by changing the total number of drunk driving accidents and by changing the likelihood that an accident will have a fatality: both will alter the total number of fatalities. For example, if a given policy reduces fatal accidents involving 18-year-old drivers, the number of fatal accidents with 18-year-old drivers will decrease in the sample and the proportion of fatal accidents with 18-year-old drivers (or, alternatively, the likelihood of observing an 18-year-old driver) relative to fatal accidents with older drivers also will decrease. Sample selection is thus not a concern; indeed, it is precisely the selection of drivers into and out of the FARS data that identifies the treatment effects of interest. What bears emphasis, however, is that the variation identifying the parameters of interest in Eqs. (1) and (2) only will differ if fatal accidents among the control group in Eq. (2) vary systematically with MLDA law changes, as is the case here.

The control group in Eq. (2) is defined by the estimation sample. Eq. (2) is a difference-in-difference estimator, with the first difference being within county over time and the second difference being between teens and older drivers. The ages of the older drivers we include in the estimation sample defines a control group in each regression. For example, if the estimation sample includes all accidents with 18-year-old drivers and with only drivers over 25, the accidents with drivers only over the age of 25 are a control group.

In the estimates presented below, we use two estimation samples that define the relevant control groups. The first sample

[17] Our estimates will be biased if minimum legal drinking ages changed endogenously based on trends in teen fatal accident involvement. Appendix Table A2 shows estimates of Eq. (2) over the time period 1984–1994. The post-1984 MLDA variation is arguably less subject to endogeneity because it is driven by states involuntarily raising their drinking ages due to the passage of the Federal National Minimum Drinking Age Act. While the standard errors become noticeably larger due to dramatically reducing the sample size, the point estimates are qualitatively and quantitatively similar to the results based on the sample from 1977 to 2002 shown in Table 4 below.

Table 4
Linear probability model estimates of the effect of MLDA changes on the probability of a teenager being involved in a fatal accident, estimates from 1977 to 2002.

Independent variable	Dependent variable: dummy = 1 if accident includes a driver of the given age					
	Driver age					
	18-year-old		19-year-old		20-year-old	
	Control group driver ages					
	21–25 (i)	26+ (ii)	21–25 (iii)	26+ (iv)	21–25 (v)	26+ (vi)
Restricted	−0.011**	−0.006**	−0.012**	−0.006**	−0.007*	−0.005**
	(0.004)	(0.002)	(0.004)	(0.003)	(0.004)	(0.002)
Restricted*$I(D<25)$	0.010*	0.011**	0.007	0.007**	0.001	0.004
	(0.006)	(0.004)	(0.006)	(0.003)	(0.006)	(0.004)
Restricted*$I(25<D<50)$	−0.001	−0.002	0.010*	0.002	−0.002	−0.003
	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)	(0.003)
Restricted*$I(50<D<75)$	−0.002	0.002	0.001	−0.002	−0.009	−0.009**
	(0.007)	(0.004)	(0.007)	(0.004)	(0.007)	(0.004)
Log VMT Per Capita	0.021**	−0.007	−0.002	−0.018**	0.001	−0.017**
	(0.008)	(0.004)	(0.008)	(0.004)	(0.008)	(0.004)
Seatbelt Law	−0.005	0.001	0.001	0.004**	−0.001	0.003
	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)
Zero Tolerance Law	−0.001	−0.002	0.005	−0.000	−0.000	−0.002
	(0.005)	(0.002)	(0.005)	(0.002)	(0.005)	(0.002)
0.08 BAC Law	0.000	0.002	0.003	0.002	0.008**	0.004**
	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)
Log Real Beer Tax	−0.024**	−0.014**	−0.030**	−0.014**	−0.028**	−0.013**
	(0.009)	(0.005)	(0.011)	(0.005)	(0.010)	(0.006)
Border County	0.001	0.004	0.001	0.003	−0.003	0.000
	(0.007)	(0.004)	(0.006)	(0.003)	(0.006)	(0.003)
Number of observations	262,540	614,305	264,255	616,020	261,256	613,021
Number of clusters	3,095	3,108	3,096	3,108	3,098	3,109
Mean of Dep. Var.	0.208	0.089	0.213	0.091	0.204	0.087

Source: Authors' estimation of Eq. (2) as described in the text. Both control groups exclude accidents involving an 18, 19 or 20-year-old driver.
Standard errors clustered at the county-level are in parentheses. **Significance at the 5-percent level. *Significance at the 10-percent level.

includes any accident with a driver of age *a* and any accident without a driver under 21 years old but with a driver between 21 and 25. The implicit control group in this sample is thus the 21–25-year olds. Using a control group close in age to the treatment group increases the likelihood they will be subject to more similar county-specific shocks. However, if MLDA laws increase spillovers across ages[18] or cause shifts in drunk driving to later ages (as found in Dee and Evans, 2001), 21–25-year olds constitute a poor control group. For this reason, our preferred specification includes any accident with a driver of age *a* and any accident *without* a driver under 26 years old but *with* a driver who is over 25. The control group in this specification is accidents with at least one driver over 25 and no driver under 26 involved.[19]

As Eq. (2) and the above discussion illustrates, although the unit of observation is an accident,[20] all of the independent variables vary at either the state or county level so that, within counties, there is no independent variation. The estimates based on the accident-level data we present below therefore are accompanied by standard errors that are clustered at the county level.[21]

5.2. Parameter estimates

Table 4 presents the results from estimation of Eq. (2) that control for general variation in fatal accidents that are spuriously correlated with the timing of MLDA changes. The results in Table 4 are consistent with those in Table 2, although as expected they are somewhat attenuated. While counties more than 25 miles from a lower-MLDA border experienced significant declines in teenage drivers' involvement in fatal accidents, involvement of teenagers within 25 miles of the border with an unrestricted drinking age did not decline – or actually increased – for 18 and 19-year olds, but not for 20-year olds. For example, the estimates shown in column (ii) of Table 4 imply that restricting alcohol access to 18-year olds reduces 18-year-old driver involvement by −0.6 percentage points (compared to a mean of 8.9 percent) in counties more than 75 miles from the border. Within 25 miles of a lower-MLDA border, however, the likelihood of an 18-year-old being involved in a fatal accident actually *increased* by 0.5 percentage points (−0.6 + 1.1). This finding is consistent with 18-year olds evading alcohol restrictions by driving to states where they can legally purchase alcohol, thereby increasing fatalities close to lower-MLDA borders.

[18] For example, raising the MLDA to 21 may reduce drinking among 21–25 year olds if they share a peer group with those who are no longer allowed to purchase alcohol legally. However, the presence of these same peer groups could also reduce the effectiveness of the MLDA restrictions, as those over 21 will be able to purchase alcohol for those who are under 21.

[19] The state-specific time trends in Eq. (2) control for the possibility that there are state-specific trends in the ratio of teenagers to the control groups. If these trends had non-linear elements that are spuriously correlated with the timing of MLDA changes, our results could be biased. To assess this possibility, we perform sensitivity analyses in which we control for the ratio of the state-level population of 18, 19 or 20-year olds to the state-level population of the control group (this ratio can be interpreted as the underlying exposure rate). Our results are unchanged, which suggests this linearity assumption is innocuous. These results are available from the authors upon request.

[20] Our estimates will be representative of teenage involvement in fatal accidents, not teen-driver production of fatalities. However, when we weight Eq. (2) by the number of fatalities from each fatal accident, it does not alter our findings.

[21] Because MLDA laws vary by state, one might argue it is more appropriate to cluster standard errors at the state level. While this level of clustering increases the size of standard errors slightly, it does not affect the results or conclusions of the analysis. However, because there is substantial variation in MLDA effects across counties, we believe it is more appropriate to cluster at the county level.

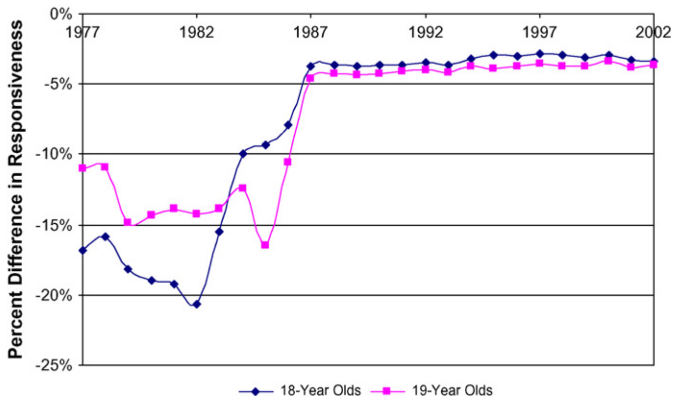

Fig. 2. Difference between evasion and non-evasion MLDA responses for 18 and 19-year olds.
Source: Authors' calculations as described in the text. Each line represents the difference in the sensitivity of fatal accident involvement of drivers of a given age to alcohol restrictions with and without MLDA evasion.

Results for 19-year-old drivers are similar to those of 18-year-old drivers. Beyond 75 miles from a lower-MLDA border, MLDA restrictions reduced the likelihood of a 19-year-old being involved in a fatal accident by 0.6 percentage points. Within 25 miles of the border, however, the likelihood increased by 0.1 percentage points. Thus, while the effects of MLDA restrictions are more muted for 19-year olds, the results for both 18 and 19-year olds show evidence of cross-border evasion.

For both 18 and 19-year-old drivers, the results using the 21–25-year-old control group and the 26 plus control group are similar, particularly in percentage terms.[22] The only notable difference is that the coefficient on the *restricted***I*($D < 25$) variable is not significant at the 5 percent level when the 21–25-year-old control group is employed. However, the point estimates are consistent with MLDA evasion, and given the potential spillovers between 21 and 25-year olds and teenagers, we believe drivers over 25 years old form a more natural control group. The odd-numbered columns in Table 4 show that our use of this control group is not fully generating our results.

The results for 20-year-old drivers show little evidence of cross-border effects. While the coefficient on *restricted***I*($D < 25$) in columns (v) and (vi) are both positive, neither is significant at even the 10 percent level, and both are small in magnitude. The coefficient on *restricted*, however, is negative and significant in both columns, suggesting that restricting 20-year olds access to alcohol causes reductions in drunk driving accidents that are not offset by cross-border evasion.

In all columns, the estimates on *Border County* are not statistically significant, although as expected they are positive in all but column (v). A positive coefficient is indicative of cross-border evasion because those "destination" counties into which teens go to drink exhibit higher fatal accident involvement rates than other counties. The estimates in Table 4 are suggestive that these counties experience higher teen fatal accident involvement, but the estimated magnitudes are too small and too imprecisely estimated to make a definitive determination. The largest effect of MLDA evasion occurs in the counties *from* which teens travel, not *to* which teens travel.

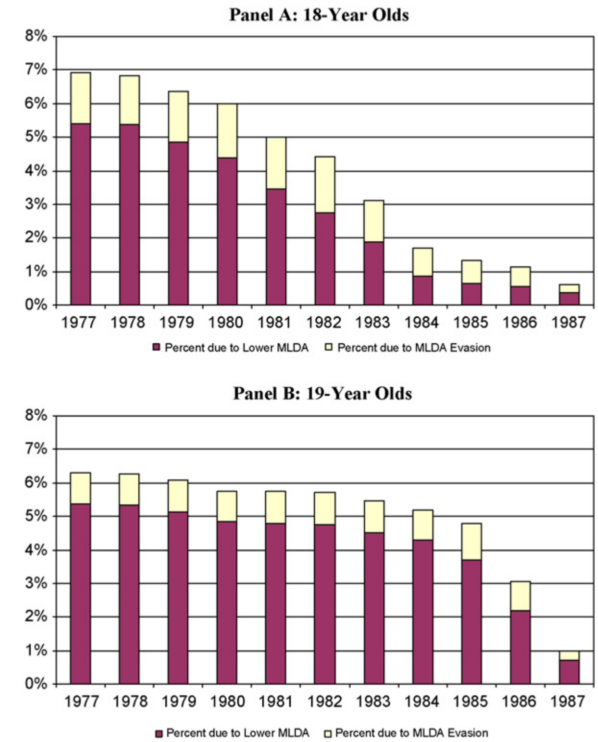

Fig. 3. Simulated percent changes in the proportion of accidents with a teen driver of a given age from lowering and un-equalizing MLDAs relative to 2002. Panel A: 18-year olds. Panel B: 19-year olds.
Source: Authors' calculations as described in the text. The height of each bar represents the total percent increase in fatal accident involvement of drivers of a given age due to implementing the historical MLDA distribution from each year in 2002. The upper and lower sections of each bar decompose the total increase into the part due to MLDA evasion and the part due to lowering the MLDA, respectively.

5.3. Interpreting the results

While the results presented in Table 4 show evidence of cross-border MLDA evasion, they do not reveal what effect evasion has on the aggregate responsiveness of teen fatal accidents to MLDA changes.[23] One way to assess the magnitude of the effect of MLDA evasion on teen traffic fatality involvement is to examine how much larger (in absolute value) the responsiveness to the MLDA change would have been had there been no evasion. The estimated change in responsiveness for each individual can be expressed as follows:

$$\frac{\gamma_1 * I(D < 25) * restricted + \gamma_2 * I(25 < D < 50) * restricted + \gamma_3 * I(50 < D < 75) * restricted}{\beta_1}$$

The denominator is the estimated responsiveness of teen fatal accident involvement in the absence of cross-border evasion of MLDA rules, and the numerator is the estimated change in responsiveness due to evasion. This expression is therefore the percentage

[22] Though the point estimates are typically larger on the *restricted* dummy in the specifications that use the 21 to 25-year-old control group, the mean of the dependent variable is larger when this control group is employed. In percentage terms, the coefficients on *restricted* across the two specifications are quite similar for all ages.

[23] This is because the proportion of counties within 25 miles of a lower-MLDA border changes each year.

Table 5
Simulated percent changes in the proportion of accidents with a teen driver of a given age from raising and equalizing MLDAs in 1980 relative to 2002, by state.

State	1980 MLDA	18-year-old drivers			19-year-old drivers		
		Total from MLDA change	From lowering MLDA	From unequal MLDA	Total from MLDA change	From lowering MLDA	From unequal MLDA
Alabama	19	6.35%	0.00%	6.35%	9.62%	9.62%	0.00%
Arkansas	19	4.16%	0.00%	4.16%	3.63%	0.00%	3.63%
Arizona	21	0.00%	0.00%	0.00%	5.82%	5.20%	0.62%
California	21	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
Colorado	18	10.85%	9.11%	1.74%	8.56%	8.06%	0.51%
Connecticut	18	11.33%	8.91%	2.42%	8.71%	7.32%	1.39%
Washington, DC	20	8.37%	8.37%	0.00%	6.86%	6.86%	0.00%
Delaware	18	16.08%	0.00%	16.08%	10.33%	0.00%	10.33%
Florida	18	9.53%	9.29%	0.25%	7.37%	7.37%	0.00%
Georgia	18	11.04%	10.26%	0.78%	9.76%	9.76%	0.00%
Iowa	19	1.93%	0.00%	1.93%	12.93%	12.61%	0.32%
Idaho	21	0.00%	0.00%	0.00%	12.55%	10.37%	2.18%
Illinois	21	1.01%	0.00%	1.01%	2.14%	0.00%	2.14%
Indiana	19	3.02%	0.00%	3.02%	0.59%	0.00%	0.59%
Kansas	18	11.32%	9.89%	1.43%	10.81%	10.00%	0.81%
Kentucky	21	3.77%	0.00%	3.77%	4.28%	0.00%	4.28%
Louisiana	18	10.56%	9.96%	0.60%	9.79%	9.40%	0.39%
Massachusetts	20	4.69%	0.00%	4.69%	6.19%	0.00%	6.19%
Maryland	18	10.95%	8.62%	2.34%	8.52%	7.16%	1.36%
Maine	20	0.36%	−0.23%	0.59%	−0.43%	0.28%	−0.71%
Michigan	21	1.09%	0.00%	1.09%	0.47%	0.01%	0.45%
Minnesota	19	3.67%	0.00%	3.67%	9.32%	9.09%	0.24%
Missouri	18	0.86%	0.00%	0.86%	0.52%	0.00%	0.52%
Mississippi	21	13.21%	11.17%	2.04%	13.06%	12.52%	0.54%
Montana	19	−0.05%	−0.11%	0.05%	13.32%	13.13%	0.19%
North Carolina	19	10.39%	10.17%	0.21%	9.66%	9.66%	0.00%
North Dakota	21	3.02%	0.00%	3.02%	10.75%	0.00%	10.75%
Nebraska	20	3.92%	0.00%	3.92%	11.17%	11.05%	0.12%
New Hampshire	19	2.80%	0.13%	2.68%	3.79%	−0.19%	3.98%
New Jersey	21	7.74%	0.00%	7.74%	8.79%	7.67%	1.13%
New Mexico	18	4.50%	0.20%	4.31%	3.87%	−0.20%	4.07%
Nevada	18	0.00%	0.00%	0.00%	4.30%	0.00%	4.30%
New York	21	10.24%	8.25%	1.99%	6.44%	6.16%	0.28%
Ohio	18	10.53%	9.12%	1.41%	8.90%	8.03%	0.87%
Oklahoma	18	10.69%	10.10%	0.59%	10.31%	9.90%	0.40%
Oregon	21	0.00%	0.00%	0.00%	0.20%	0.00%	0.20%
Pennsylvania	21	4.53%	−0.03%	4.56%	5.60%	0.00%	5.60%
Rhode Island	18	12.93%	8.81%	4.11%	10.26%	7.74%	2.52%
South Carolina	18	10.73%	10.73%	0.00%	10.06%	10.06%	0.00%
South Dakota	18	17.43%	11.85%	5.58%	17.79%	16.75%	1.04%
Tennessee	19	5.40%	0.00%	5.40%	9.43%	8.56%	0.87%
Texas	18	7.28%	7.03%	0.25%	5.73%	5.60%	0.13%
Utah	21	−0.17%	0.00%	−0.17%	1.06%	0.00%	1.06%
Virginia	18	10.46%	10.19%	0.27%	10.62%	10.51%	0.11%
Vermont	18	9.79%	7.22%	2.57%	16.31%	13.29%	3.02%
Washington	21	0.00%	0.00%	0.00%	0.48%	−0.08%	0.56%
Wisconsin	18	11.23%	9.58%	1.64%	10.01%	9.35%	0.67%
West Virginia	18	11.32%	10.55%	0.77%	12.56%	11.95%	0.61%
Wyoming	19	4.94%	0.00%	4.94%	15.12%	14.27%	0.85%

Source: Authors' calculations using coefficient estimates from columns (ii) and (iv) in Table 4 as described in the text.

change in the probability that a teen is involved in a fatal accident due to MLDA changes if evasion had been eliminated. Taking the average of this statistic in a given year (or in a given state in a given year) yields the change in the responsiveness of the accident involvement rate of teenagers due to MLDA changes when evasion is eliminated.

Fig. 2 presents the results from this calculation separately for 18 and 19-year-old drivers for each year from 1977 to 2002.[24] For 18-year olds, eliminating MLDA evasion would have increased the impact of MLDA increases by between 16 and 21 percent between 1977 and 1982. After 1982, when more states began increasing their MLDAs to over 18 (and thus reducing evasion opportunities), the impact of eliminating evasion fell from 21 percent in 1982 to 4 percent in 1987. For 19-year olds, eliminating evasion opportunities would have increased the impact of MLDA increases on accident involvement by 11 percent in 1977 and 1978. This percent increased to between 13 and 16 between 1980 and 1985, due largely to many states increasing their MLDA to 19. After 1985, the percent change in responsiveness decreased to −4%, where it remained largely stable until 2002. Note that the percent difference in responsiveness after 1987 for both 18 and 19-year olds reflects the existence of lower drinking ages in Canada and Mexico.

Another way to examine the total effect of MLDA evasion on teen fatal accident involvement is to decompose the total MLDA effect into the part due to increasing the MLDA and the part due to equalizing the MLDA. We undertake this decomposition by simulating 18 and 19-year olds fatal accident involvement rates under two counterfactuals. The first counterfactual is what the teenage involvement rate would have been had the MLDA distribution in 2002 been the same as the MLDA distribution in

[24] Because 20-year-old drivers exhibit little evasion behavior, we exclude them from Fig. 2 and from the subsequent analyses in this section.

Table 6
Linear probability model estimates of the effect of MLDA changes on the probability of a teenager being involved in a fatal accident by locality, estimates from 1977 to 2002.

Independent variable	Dependent variable: dummy = 1 if accident includes a driver of the given age					
	Driver age					
	18-year-old		19-year-old		20-year-old	
	Control group driver ages					
	21–25 (i)	26+ (ii)	21–25 (iii)	26+ (iv)	21–25 (v)	26+ (vi)
Restricted*USA	−0.015**	−0.008**	−0.016**	−0.009**	−0.010**	−0.007**
	(0.004)	(0.003)	(0.004)	(0.002)	(0.005)	(0.003)
Restricted*USA*$I(D<25)$	0.013**	0.012**	0.012*	0.011**	0.006	0.008**
	(0.006)	(0.004)	(0.007)	(0.004)	(0.007)	(0.004)
Restricted*USA*$I(25<D<50)$	0.001	−0.003	0.012*	0.002	−0.001	−0.003
	(0.006)	(0.004)	(0.007)	(0.005)	(0.007)	(0.003)
Restricted*USA*$I(50<D<75)$	0.001	0.004	0.002	−0.001	−0.005	−0.005
	(0.007)	(0.004)	(0.008)	(0.004)	(0.009)	(0.004)
Restricted*Mexico	0.001	0.002	−0.003	0.003	−0.001	0.003
	(0.006)	(0.003)	(0.006)	(0.004)	(0.006)	(0.004)
Restricted*Mexico*$I(D<25)$	−0.006	0.002	0.006	0.013	−0.004	0.008
	(0.013)	(0.013)	(0.002)	(0.014)	(0.013)	(0.009)
Restricted*Mexico*$I(25<D<50)$	0.047	0.049*	−0.071**	−0.002	−0.036	0.016
	(0.056)	(0.027)	(0.036)	(0.013)	(0.024)	(0.010)
Restricted*Mexico*$I(50<D<75)$	−0.097	−0.041	0.005	0.005	−0.007	−0.003
	(0.065)	(0.032)	(0.007)	(0.006)	(0.015)	(0.007)
Restricted*Canada	−0.011**	−0.010**	−0.014**	−0.008**	−0.006	−0.005
	(0.005)	(0.003)	(0.006)	(0.004)	(0.005)	(0.003)
Restricted*Canada*$I(D<25)$	−0.047	−0.038*	−0.023*	−0.018**	−0.013	−0.008
	(0.056)	(0.020)	(0.013)	(0.007)	(0.018)	(0.010)
Restricted*Canada*$I(25<D<50)$	−0.007	−0.011	0.020	0.007	0.003	0.001
	(0.041)	(0.015)	(0.014)	(0.009)	(0.014)	(0.007)
Restricted*Canada*$I(50<D<75)$	−0.010	−0.006	0.004	−0.004	−0.013	−0.016*
	(0.025)	(0.014)	(0.012)	(0.006)	(0.015)	(0.009)
Log Real Beer Tax	−0.019**	−0.010**	−0.029**	−0.014**	−0.027**	−0.013**
	(0.009)	(0.005)	(0.010)	(0.005)	(0.010)	(0.006)
Border County	−0.001	0.003	−0.001	0.002	−0.004	0.000
	(0.007)	(0.004)	(0.006)	(0.003)	(0.006)	(0.003)

Source: Authors' estimation of Eq. (2) as described in the text. Both control groups exclude accidents involving an 18, 19 or 20-year-old driver.
Standard errors clustered at the county-level are in parentheses. **Significance at the 5-percent level. *Significance at the 10-percent level.
All models include controls for county fixed effects, year fixed effects, state-specific linear time trends, Log VMT per Capita, seatbelt laws, zero tolerance laws, 0.08 BAC laws, real state average beer taxes and border county dummies.
USA is a dummy variable equal to 1 if the closest lower-MLDA border is a U.S. state border, Canada is a dummy variable equal to 1 if the closest lower-MLDA border is with Canada, and Mexico is a dummy variable equal to 1 if the closest lower-MLDA border is with Mexico.

each of the years 1977–1988. We call this counterfactual $\hat{P}_{2002}^{\text{MLDA}=t}$, where t is 1977 through 1988. The second counterfactual is what the first counterfactual accident rate would have been had there been no MLDA evasion.[25] We label this counterfactual $\hat{P}_{2002}^{\text{no-evade}}$. If $\hat{P}_{2002}$ is the actual fitted value of the accident rate from Eq. (1) in 2002, then the percentage change in the accident rate due to lowering the MLDA is $((\hat{P}_{2002}^{\text{no-evade}} - \hat{P}_{2002})/\hat{P}_{2002})$, and the percentage change in the accident rate due to having unequal MLDAs is $((\hat{P}_{2002}^{\text{MLDA}=t} - \hat{P}_{2002}^{\text{no-evade}})/\hat{P}_{2002})$. Thus, taking the observable characteristics in place in 2002, we are able to simulate what accident rates would have been like under varying MLDA distributions, both with and without evasion.

Results from these decompositions are shown in Fig. 3. Panel A contains results for 18-year-old drivers and Panel B contains results for 19-year-old drivers. In each panel, the top section of each bar represents the percent change in the proportion of accidents with teen drivers due to evasion, and the bottom portion of each bar shows the percent change due to lowering the MLDA. The height of each bar is the total percent change in accident involvement attributable to MLDA changes. For 18-year olds, eliminating MLDA evasion was responsible for lowering the proportion of accidents involving 18-year olds by over 1.5 percentage points between 1977 and 1983. This represents between 21.6 and 37.8 percent of the total reduction in accident involvement due to MLDA changes over this period. For 19-year olds, the effects of evasion are somewhat smaller: eliminating cross-state evasion lowered 19-year-old accident involvement by almost 1 percentage point between 1977 and 1986, which represents between 15 and 27 percent of the total reduction in 19-year-old accident involvement due to MLDA changes.

As Fig. 3 illustrates, a substantial portion of the reduction in teen drunk driving due to the MLDA increases in the 1970s and 1980s can be attributed to the equalization of drinking ages rather than to the increases themselves. Furthermore, Fig. 3 implies previous studies that have ignored cross-border evasion may have significantly understated the total effect of MLDA increases on traffic fatalities.

Because MLDA evasion is a function of distance to a lower-MLDA border, MLDA changes have had heterogeneous effects across states. Table 5 shows the same simulations as Fig. 3, but broken down by state for 1980. For example, if we impose the MLDA distribution from 1980 on 2002 accident rates, the likelihood of an 18-year-old being involved in an accident would increase in Colorado by 10.9 percent, 9.1 percent of which is due to lowering the MLDA to 18 and 1.7 percent of which is due to evasion from neighboring states. In contrast, in a state such as New Jersey that had an

[25] Mechanically, we construct this counterfactual by simulating accident rates if all counties had been more than 75 miles from a lower-MLDA border in year t. We do not set the MLDA to be equal across all states, but rather keep the observed MLDA distribution and impose the no smuggling condition on each observation.

Table 7
Linear probability model estimates of the effect of MLDA changes on the probability of a teen driver being involved in a fatal accident relative to a driver over 25 years old, estimates from 1977 to 2002 by time of accident.

Independent variable	Dependent variable: dummy = 1 if accident includes a driver of the given age					
	Driver age					
	18-year-old		19-year-old		20-year-old	
	Night (i)	Day (ii)	Night (iii)	Day (iv)	Night (v)	Day (vi)
Restricted	−0.018**	0.000	−0.021**	0.002	−0.010**	−0.002
	(0.005)	(0.003)	(0.005)	(0.003)	(0.004)	(0.002)
Restricted*$I(D<25)$	0.019**	0.005	0.013**	0.003	0.007	0.002
	(0.007)	(0.004)	(0.006)	(0.004)	(0.007)	(0.004)
Restricted*$I(25<D<50)$	−0.001	−0.002	0.005	0.002	0.006	−0.006*
	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)	(0.003)
Restricted*$I(50<D<75)$	0.008	−0.002	0.005	−0.005	−0.010	−0.006
	(0.007)	(0.004)	(0.006)	(0.004)	(0.008)	(0.004)
Log VMT Per Capita	−0.001	−0.008*	−0.014*	−0.016**	−0.022**	−0.012**
	(0.007)	(0.004)	(0.008)	(0.004)	(0.009)	(0.004)
Seatbelt Law	0.001	0.000	0.002	0.005**	0.006	0.001
	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)
Zero Tolerance Law	0.001	−0.003	−0.000	−0.000	0.002	−0.003
	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)
0.08 BAC Law	0.009**	−0.003*	0.008**	−0.003	0.007*	0.002
	(0.003)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)
Log Real Beer Tax	−0.001	−0.013**	−0.012**	−0.011**	−0.003	−0.014**
	(0.009)	(0.005)	(0.010)	(0.005)	(0.010)	(0.005)
Border County	0.006	0.003	0.005	−0.000	−0.003	0.001
	(0.008)	(0.004)	(0.006)	(0.003)	(0.007)	(0.004)
Number of observations	186,480	422,834	187,861	423,125	186,360	421,651
Number of clusters	3,100	3,105	3,098	3,104	3,100	3,105
Mean of Dep. Var.	0.123	0.075	0.129	0.075	0.122	0.072

Source: Authors' estimation of Eq. (2) as described in the text. Both control groups exclude accidents involving an 18, 19 or 20-year-old driver.
Standard errors clustered at the county-level are in parentheses. **Significance at the 5-percent level. *Significance at the 10-percent level.
"Night" is defined as any accident occurring between 9:00 PM and 3:59 AM. "Day" is defined as any accident occurring at any other time.

MLDA of 19 in 1980, the 1980 MLDA distribution would increase 18-year-old involvement by 7.7 percent relative to the 2002 distribution, and all of this increase is due to evasion (as Maryland and New York both had MLDAs of 18). For 19-year olds in New Jersey, the 8.8 percent increase in the accident involvement rate that would occur if one were to go back to 1980 MLDA levels is mostly due to lowering the MLDA, with some accident involvement increases due to evasion in neighboring Delaware and Pennsylvania.

Overall, simulations of accident involvement rates in 2002 under the 1980 MLDA distribution show that some states, such as Alabama, Delaware, New Jersey, South Dakota, Tennessee, and Wyoming would experience increases in accident involvement among 18-year olds of 5 percent or more due to evasion. We estimate that Delaware, Massachusetts, North Dakota, and Pennsylvania would witness similar increases among 19-year-old drivers due to evasion. As shown in Table 5, there are a number of states in which no evasion would occur, and most states would experience an increase in teen drunk driving due to lowering their MLDA to the 1980 level. These results suggest that allowing states to set different minimum drinking ages could be quite costly in terms of lives lost due to teenage drunk driving accidents.

5.4. Sensitivity analyses

In the results presented thus far, we have made no distinction between state and international borders. Treating all borders the same might be incorrect, however, because interstate travel is unregulated, whereas crossing into Canada or Mexico requires clearing customs. Because of the increased cost and oversight, teens might be less likely to drive drunk across the Canadian or Mexican borders than they would across the Michigan-Ohio border, for example. If the Canadian and Mexican borders are more restrictive in terms of MLDA evasion, our average estimates in Table 4 understate the magnitude of within-U.S. MLDA evasion.

To better understand the differences in behavioral response between state and international border types, we ran a specification of Eq. (2) where we interacted the *restricted* and the *restricted*distance* dummy variables with a dummy variable for the type of the closest lower-MLDA border: USA, Mexico, or Canada. Results from these regressions are presented in Table 6. Consistent with our expectations, the U.S. border results are more pronounced than those reported in Table 4, particularly for 19 and 20-year olds. For 20-year olds, there is now statistically significant evidence of cross-border evasion across U.S. state borders. Across all age groups, the results for the Mexican border show little evidence of MLDA effects or of cross-border shopping.

In contrast, results from the Canadian border show a statistically significant effect of raising the MLDA for 18 and 19-year olds that is consistent in magnitude with the coefficient estimates on *restricted* in Table 4. Surprisingly, there is a large negative estimate of MLDA restrictions within 25 miles of the Canadian border—youth driving fatalities fell relatively more in these areas—although it is only statistically significant at the 5 percent level in column (iv). This finding is driven by the counties in upstate New York and northern Vermont, where state police responded to the creation of MLDA border differentials by increasing patrols and checkpoints.[26] These

[26] For example, an article in *Newsday* in November 1985, 2 weeks before New York increased its MLDA to 21, made the following claim: "State troopers and upstate police departments are planning to beef up patrols along the borders of Vermont and Quebec, where the legal drinking age is 18, and Ontario, where it is 19. Lt. Michael Wright said state troopers will shift more of their 20 'sobriety checkpoints'—

Table 8
Linear probability model estimates of the effect of MLDA changes on the probability of a teenager being involved in a fatal accident, separating effects by the State's MLDA, using accidents only with drivers over 25 as the control group from 1977 to 2002.

Independent variable	Dependent variable: dummy = 1 if accident includes a driver of the given age		
	Driver age		
	18-year-old (i)	19-year-old (ii)	20-year-old (iii)
Restricted*I(MLDA 19)	−0.005* (0.003)		
Restricted*I(D < 25)* I(MLDA 19)	0.008* (0.005)		
Restricted*I(25 < D < 50)* I(MLDA 19)	−0.003 (0.006)		
Restricted*I(50 < D < 75)* I(MLDA 19)	−0.003 (0.006)		
Restricted*I (MLDA 20)	−0.006 (0.005)	0.002 (0.007)	
Restricted*I(D < 25)* I(MLDA 20)	0.020* (0.011)	0.010 (0.010)	
Restricted*I(25 < D < 50)* I(MLDA 20)	0.022** (0.009)	0.021* (0.012)	
Restricted*I(50 < D < 75)* I(MLDA 20)	0.030** (0.012)	0.017 (0.013)	
Restricted*I (MLDA 21)	−0.007** (0.003)	−0.006** (0.003)	−0.005** (0.002)
Restricted*I(D < 25)* I(MLDA 21)	0.012** (0.004)	0.007* (0.004)	0.004 (0.004)
Restricted*I(25 < D < 50)* I(MLDA 21)	−0.005 (0.004)	−0.000 (0.004)	−0.003 (0.003)
Restricted*I(50 < D < 75)* I(MLDA 21)	0.002 (0.005)	−0.004 (0.004)	−0.009** (0.004)

Source: Authors' estimation of Eq. (2) as described in the text. The control group excludes accidents involving an 18, 19 or 20-year-old driver.
All models include controls for county fixed effects, year fixed effects, state-specific linear time trends, Log VMT per Capita, seatbelt laws, zero tolerance laws, 0.08 BAC laws, real state average beer taxes and border county dummies.
Standard errors clustered at the county-level are in parentheses. **Significance at the 5-percent level. *Significance at the 10-percent level.

patrols may have been effective in reducing drunk driving among 18 and 19-year olds.

The results presented in Table 6 confirm that there are indeed differences between U.S. state borders and international borders. While the inclusion of these international borders attenuates our estimates, the results in Table 4 are being driven predominantly by cross-border traffic within the United States.

Another concern with our research strategy is that we may not observe accurately whether drivers involved in fatal accidents are intoxicated. This is a concern because a central identifying assumption of our analysis is that the change in fatal accident involvement among teens around the time of the MLDA changes is due solely to reductions in drunk driving. To test this assumption, we employ a similar strategy to Dee (1999) and estimate models separately for daytime and nighttime accidents.[27]

The results, reported in Table 7 for the specifications using the 26+ control group, support the interpretation that our estimates reflect changes in drunk driving. The coefficients on *restricted* and on *restricted**I*(*D* < 25) are only sizeable and statistically significant at the 5 percent level in the nighttime specifications. During the day, alcohol restrictions and distance have little to no effect on the likelihood a teen driver is involved in a fatal car accident. Moreover, the coefficients on *0.08 BAC Law* are now positive and significant at the 5 or 10 percent level at night, but not during the day. Because these laws are targeted at legal drinkers, they should decrease fatal accident involvement among the control group by more than among the treated group, which is what our results indicate.

Finally, treating all MLDA restrictions equally might induce measurement error in our estimates because those who are close in age to the MLDA may be able to more easily evade the restriction. To assess the relevance of this concern, we interact the *restricted* and distance dummy variables with dummy variables for each state's MLDA. The estimates, presented in Table 8, suggest that the effects of MLDA increases are smaller when an individual is within a year of the drinking age. For example, in the second column of Table 8, there is no effect of a 20-year-old MLDA on the likelihood of a 19-year-old driver being involved in a fatal accident more than 75 miles from a lower-MLDA border. However, there is a negative and statistically significant effect of a 21-year-old-MLDA on 19-year-old fatal accident involvement. A similar difference, although less pronounced, is evident for fatal accident involvement of 18-year olds.

6. Conclusion

The availability of different policies just across the border—be they lower excise taxes or less stringent legal restrictions—can compromise the impact of a jurisdiction's own policies and cause efficiency costs as some residents cross borders to evade their own state's policies. In the case of legalized drinking, being able to drink legally across the border has an additional implication for social costs, because the act of drinking and then driving home drunk can itself be dangerous, even fatal, both to the cross-border consumers and other unfortunate drivers and pedestrians.

Using state-of-the-art GIS software and micro-data on fatal vehicle accidents from 1977 to 2002, we evaluate the effect of minimum legal drinking age state polices since 1977. We find that in counties within 25 miles of a lower-MLDA jurisdiction, a legal restriction on drinking does not reduce youth involvement in fatal accidents and, for 18 and 19-year-old drivers, fatal accident involvement actually increases. Farther from such a border, we find results consistent with the previous literature that MLDA restrictions are effective in reducing accident fatalities. The estimates imply, of the total reduction in teenager-involved fatalities due to the equalization of state MLDAs at 21 in the 1970s and 1980s, between a quarter and a third was due to the equalization for 18-year olds and over 15 percent was due to equalization for 19-year olds. Furthermore, the effect of changes in the MLDA is quite heterogeneous across states, depending on the fraction of a state's population that need not travel far to reach a state with a lower MLDA.

Our results suggest that, by ignoring MLDA evasion, previous studies have underestimated the total effect of MLDA increases on teenage drunk driving. That unequal policies across unmonitored borders can induce the very behaviors the restrictions are meant to eliminate has been documented previously with respect to cigarettes (Lovenheim, 2008). When the behavior in question is teenage drunk driving, evasion itself can exact a toll in terms of lives. While determining the full costs and benefits of a given minimum legal drinking age is outside of the scope of our analysis, our

roadblocks where all drivers are stopped—to the two border areas" (Bunch and Fresco, 1985).

[27] Dee (1999) defines "day" to be between 7:00 AM and 2:59 PM, while "night" is 12:00 AM to 4:59 AM. Instead of excluding accidents occurring in the remainder of the hours of the day, we split each day into 2 periods: "night" is defined as 9:00 PM to 3:59 AM, and "day" is defined as all other hours. Our results are similar when we use the Dee (1999) definitions of night and day. Furthermore, our results are robust to the inclusion of month dummy variables, which control for the fact that dusk and dawn occur differentially around the cutoffs in different times of the year.

results imply that there are significant costs in terms of lives lost to having unequal drinking age restrictions across states in the United States. Other things equal, these results argue for setting a standard minimum legal drinking age across all states.

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Appendix A.

Tables A1 and A2 .

Table A1
OLS estimates of the effect of MLDA changes on the proportion of fatal accidents with a teenage driver, aggregate county-level estimates using accidents only with drivers over 25 as the control group from 1977 to 2002.

Independent variable	Dependent variable: proportion of accidents involving a driver of the given age					
	Driver age 18-year-old		19-year-old		20-year-old	
	County-specific linear time trends?					
	No (i)	Yes (ii)	No (iii)	Yes (iv)	No (v)	Yes (vi)
Restricted	−0.006**	−0.006**	−0.005**	−0.006**	−0.004	−0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Restricted*$I(D<25)$	0.012**	0.012**	0.007*	0.007*	0.003	0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Restricted*$I(25<D<50)$	−0.002	−0.002	0.002	0.003	−0.005	−0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
Restricted*$I(50<D<75)$	0.004	0.003	−0.003	−0.003	−0.009**	−0.009**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Log VMT Per Capita	−0.007*	−0.007*	−0.018**	−0.018**	−0.017**	−0.017**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Seatbelt Law	0.001	0.001	0.004**	0.004**	0.002	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Zero Tolerance Law	−0.002	−0.002	−0.000	−0.000	−0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
0.08 BAC Law	0.001	0.001	0.002	0.002	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Log Real Beer Tax	−0.014**	−0.013**	−0.014**	−0.014**	−0.013**	−0.013**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Border County	0.004	0.004	0.003	0.002	0.000	0.000
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Number of observations	70,997	70,997	70,991	70,991	70,916	70,916
Number of clusters	3,108	3,108	3,108	3,108	3,109	3,109

Source: Authors' estimation of Eq. (2) as described in the text. The control group excludes accidents involving an 18, 19 or 20-year-old driver.
All regressions are weighted by the number of accidents that constitute each county-year observation.
Standard errors clustered at the county-level are in parentheses. **Significance at the 5-percent level. *Significance at the 10-percent level.

Table A2
Linear probability model estimates of the effect of MLDA changes on the probability of a teenager being involved in a fatal accident, estimates using accidents only with drivers over 25 as the control group.

Independent variable	Dependent variable: dummy = 1 if accident includes a driver of the given age					
	Driver age					
	18-year-old		19-year-old		20-year-old	
	Analysis years					
	1984–1994 (i)	1977–1994 (ii)	1984–1994 (iii)	1977–1994 (iv)	1984–1994 (v)	1977–1994 (vi)
Restricted	−0.013	−0.007**	−0.009**	−0.009**	−0.007*	−0.009**
	(0.009)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
Restricted*$I(D<25)$	0.013	0.008**	0.003	0.005	0.003	0.004
	(0.009)	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)
Restricted*$I(25<D<50)$	−0.010	−0.005	−0.006	0.000	−0.004	−0.002
	(0.007)	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)
Restricted*$I(50<D<75)$	0.001	−0.001	−0.003	−0.002	−0.006	−0.007*
	(0.009)	(0.004)	(0.006)	(0.004)	(0.005)	(0.004)
Log VMT Per Capita	0.015*	−0.000	0.010	−0.011**	−0.003	−0.009
	(0.009)	(0.006)	(0.008)	(0.005)	(0.008)	(0.006)

Table A2 (*Continued*)

Independent variable	Dependent variable: dummy = 1 if accident includes a driver of the given age					
	Driver age					
	18-year-old		19-year-old		20-year-old	
	Analysis years					
	1984–1994 (i)	1977–1994 (ii)	1984–1994 (iii)	1977–1994 (iv)	1984–1994 (v)	1977–1994 (vi)
Seatbelt Law	0.001	0.001	0.006**	0.003	0.003	0.003
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Zero Tolerance Law	−0.001	−0.001	−0.003	−0.005	0.001	−0.002
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
0.08 BAC Law	0.005	0.007*	0.013**	0.010**	0.010**	0.007**
	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.003)
Log Real Beer Tax	−0.008	−0.002	0.010	−0.003	−0.008*	0.002
	(0.011)	(0.007)	(0.012)	(0.008)	(0.012)	(0.007)
Border County	−0.006	0.003	−0.007	0.002	−0.002	−0.001
	(0.011)	(0.004)	(0.006)	(0.003)	(0.006)	(0.004)
Number of observations	257,538	416,049	258,331	417,922	257,551	415,950
Number of clusters	3,100	3,104	3,099	3,105	3,102	3,106

Source: Authors' estimation of Eq. (2) as described in the text. The control group excludes accidents involving an 18, 19 or 20-year-old driver.
Standard errors clustered at the county-level are in parentheses. **Significance at the 5-percent level. *Significance at the 10-percent level.

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