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State Marijuana Laws and Traffic Fatalities*

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Abstract: We examine the relationship between traffic fatalities and state marijuana laws using data from 1985 through 2019 and Poisson difference in difference models that allow effects to vary over time. We show numerous attributes of state marijuana laws are captured by a single underlying dimension, implying interdependence and potential inseparability of the effects of recreational marijuana laws (RMLs) and prior medical marijuana laws (MMLs). Controlling for MMLs, we find no statistically significant change in fatalities associated with RMLs, while, as in earlier work, MMLs are associated with lower fatalities. There is a statistically insignificant decline in fatalities in states bordering RML states. Type 1 error rate inflation is present in this context, but it is not strong enough to overturn the finding that lower fatalities followed MMLs.

Keywords: recreational marijuana, medical marijuana, driving under the influence, traffic safety, fatal crashes
JEL Codes: I-18, K-32, and R41

1. INTRODUCTION

As of March 2021, thirty-five U.S. states and the District of Columbia (hereinafter 51 states) had legalized marijuana use for medical purposes. Fifteen of those had also legalized recreational use (National Conference of State Legislatures, 2019). The movement towards liberalization is expected to continue (Lopez, 2020; Weed, 2020). Opponents of liberalization express concerns that increased marijuana use may lead to detrimental social consequences (Ricketts, 2020). This is despite potential therapeutic uses of marijuana (Marmor, 1998; Watson et al., 2000; Amar, 2006; Baron, 2015; Whiting et al., 2015) and high costs associated with enforcement of its prohibition and resultant incarcerations (Hickey and McLaughlin, 2019; DeVylder et al., 2021).

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An increase in traffic fatalities due to an increase in driving under the influence of marijuana is one such concern, and there are grounds for it. Marijuana exposure is associated with indicators of driver impairment (Ronen et al., 2008; Sewell et al., 2009; Li et al., 2012; Hartman and Huestis, 2013; Brady and Li, 2014; Hartman et al., 2016). However, individuals intoxicated by marijuana use may overestimate their impairment (Berghaus et al., 1995) and compensate by avoiding difficult and dangerous driving maneuvers (Kelly et al., 2004). Further, if some individuals substitute marijuana use for alcohol use, traffic fatalities might fall for either of two reasons (Anderson et al., 2013). First drivers might be less dangerous under the influence of marijuana than alcohol. Second, since alcohol is regularly consumed in public, but marijuana is not, alcohol might be more associated with driving under the influence than marijuana.

We cannot yet tally the full consequences of changing marijuana laws because the laws and their implementation continue to evolve, and it may take a long time to adjust to these changes (Pacula and Sevigny, 2014; Hall and Weier, 2015). However, California passed its medical marijuana law (MML) in 1996, and Colorado and Washington legalized recreational use in 2012. As of 2019, MMLs had been in effect at least a decade in sixteen states, and recreational marijuana laws (RMLs) had been in effect at least four years in nine. If liberalization has led to a large decline in traffic safety, there should be evidence of it in traffic fatality counts.

A growing literature addresses this issue. Hall and Weier (2015) found an increase in fatal accidents involving cannabis impaired drivers after the adoption of MMLs . However, this is an assessment of the frequency of measured marijuana use given fatal crashes, not the frequency of fatal crashes given marijuana use, and would occur if use increased even if there were no associated increase in crashes. Further, this may be due in part to increased and improved testing for marijuana (Armentano, 2012). Anderson et al. (2013) show MMLs were associated with decreased traffic fatalities . They also present evidence of substitution of marijuana for alcohol following MMLs, which may largely account for the decline. Baggio et al. (2017) and Miller and Seo (2021) present further evidence that marijuana liberalization is associated with reduced alcohol consumption. Santaella-Tenorio et al. (2017) also found a reduction in traffic fatalities following MMLs.

Several recent papers have presented early evidence on the impacts of RMLs on traffic fatalities. Hansen et al. (2020a) and Aydelotte et al. (2017) find no statistically significant effects in Colorado and Washington. Santaella-Tenorio et al. (2020) find a statistically significant increase in fatalities in Colorado but not in Washington. Aydelotte et al. (2019) find a statistically significant increase in both Colorado and Washington after commercialization, which followed recreational legalization with a lag. Kamer et al. (2020) examine changes in Alaska, Colorado, Oregon, and Washington following commercialization and find a statistically significant increase in fatalities. Given the limited number of state-years under RMLs and the challenges involved in disentangling the effects of RMLs from other factors, it is not surprising that these results are mixed.

We contribute to this literature by working to disentangle the interrelated provisions of state marijuana laws and the timing of policy changes. We develop composite measures of marijuana permissiveness based on several features of state statutes. Using data from the Fatality Analysis Reporting System (National Highway Traffic Safety Administration, 2021), we find evidence MMLs are associated with a decrease in fatalities. This is true whether we control for a binary MML indicator or a composite measure of MML permissiveness. Having controlled for either the presence of an MML or our permissiveness measure, we find no evidence RMLs are associated with further changes in fatalities.

In section 2, we characterize state marijuana laws based on nine distinct attributes, including whether medical dispensaries and adult recreational use are allowed. We show they are all well captured by a single underlying dimension representing permissiveness toward marijuana use. This suggests marijuana laws may reflect prevailing norms, expectations, and behaviors, as well shaping them, and that the various aspects of marijuana laws are interdependent. From this point of view, measuring the effects of one aspect of marijuana laws, such as RMLs, without controlling for other aspects, such as prior MMLs, is a dubious proposition, as changes we may expect to be consequences of RMLs, in fact, may precede them. For example, in 2018, both the Manhattan and Brooklyn district attorneys stopped prosecuting most marijuana offenses even though non-medicinal use had not been legalized (Ransom and Pager, 2018). Similarly, Anderson et al. (2013) and Malivert and Hall (2013) show that the price of high-quality extralegal marijuana fell substantially following implementation of MMLs in states that later implemented RMLs, presumably leading to corresponding increases in extralegal recreational consumption prior to the adoption of an RML.

After discussing our other data sources and variables in section 3, section 4 discusses the specification of our empirical model. Together, four aspects of our model comprise a unique approach to the measurement of the effects of marijuana laws on traffic fatalities:

1. We simultaneously control for either the presence of MMLs, or a measure of their permissiveness, and for RMLs. In some specifications we control for the presence of medical dispensaries and for non-RML states that share a border with RML states.

2. We employ Poisson regression, a more natural choice than ordinary least squares regression given the count nature of the data. We employ robust standard errors clustered by state to adjust standard errors for over or under dispersion and for panel specific heteroskedasticity and autocorrelation.

3. Following Anderson et al. (2013), we allow before and after effects to vary by time from the change in the law. However, we exploit the additional years of data in our sample to do so simultaneously for MMLs and RMLs, and to allow flexibility for an additional two years prior to the effective year.

4. We include year specific effects that differ for the group of states that had implemented an MML by 2019 and those that had not (no state had adopted an RML without first adopting an MML). Persistent year specific shocks that impact states within a group similarly but states in different groups differently are absorbed by these fixed effects. It is impossible to rule out the possibility that results found using a difference in difference design are driven by persistent idiosyncratic shocks following changes in marijuana laws that create spurious correlation in the state time series, but this is a step in that direction.

Section 4 presents our primary results. We find MMLs are associated with declines in traffic fatalities, in accord with earlier work, though the effect declines after six years. Having controlled for MMLs, we find no additional effect of RMLs. We do find an upward drift in fatalities in states that adopt RMLs from five years prior to the effective year to five years after the effective year. This trend disappears after that, though only Colorado and Washington had RMLs for that long. Effects of RMLs observed by comparing outcomes in the five years after the effective year to any given period prior to adoption may simply reflect this pre-existing upward drift and lack of control for the effects of prior MMLs.

Section five presents evidence regarding cross border effects and type 1 error rate inflation. There is evidence of significant cross border marijuana sales between RML states and non-RML states (Hansen et al., 2020a). Fatalities are slightly lower in non-RML states after an RML becomes effective in a bordering state, but the difference is not statistically significant, and our other results are unchanged.

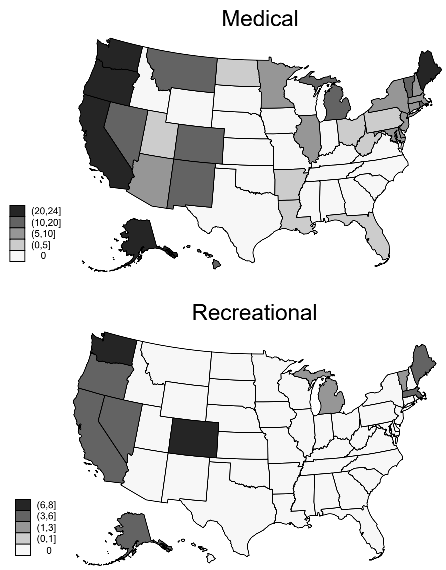
We conduct a placebo test to examine the possibility that our finding regarding the effects of MMLs is driven by persistent idiosyncratic state shocks that occur after changes in laws, creating spurious correlation in the state time series. To test for this, we randomly reassign marijuana law histories among the states 10,000 times, conducting tests of the null hypotheses of no difference in before and after effects of both MMLs and RMLs each time. We find evidence of type 1 error rate inflation, but it is not strong enough to overturn our finding that reductions in traffic fatalities follow MMLs. The inflation of type 1 error rates only strengthens our finding of no statistically significant further effects of RMLs.

2. CHARACTERIZING STATE MARIJUANA LAWS

In our models of traffic fatalities, the explanatory variables of most interest pertain to state marijuana laws. Figure 1 shows the distribution of the number of years MMLs and RMLs had been in effect as of 2019, the last year in our analysis as traffic fatality data was not yet available for later years. The figure is based on information from State Medical Marijuana Laws, maintained by the National Conference of State Legislatures (National Conference of State Legislatures, 2019). Some states allow medical use of marijuana related products but only with very minimal THC content (0.25% or less). For our purposes, such states are treated as not having an MML. Most of the states with MMLs in place for over a decade are in the West. Most that did not have an MML are in the Midwest or Southeast. A similar pattern may be seen in RMLs. The oldest RMLs are in the West, and others are in the North or Northeast. Considerable variation is apparent between states in whether an MML or an RML is present and in the timing of their adoption.

What measures of state marijuana laws should be included in an analysis of traffic fatalities? Is a single indicator of the presence of an MML, or of an RML, sufficient? Previous work suggests the impact of MMLs and RMLs may differ, suggesting both should be included. Pacula et al. (2015) argue it is important to consider multiple components of state marijuana laws and that medicinal dispensaries may be particularly important due to an association with increases in marijuana use. Santaella-Tenorio et al. (2017) found operational dispensaries were associated with a further reduction in fatalities and suggested future analyses consider other aspects of marijuana laws.

Figure 1: Years under Liberalization as of 2019



We record the presence or absence of the nine attributes of state marijuana laws defined in Table 1 from 1985 through 2019. This information was obtained by first consulting State Medical Marijuana Laws (National Conference of State Legislatures, 2019) and then turning to state statutes for additional details. We explore relationships among these attributes in 2019 laws using joint correspondence analysis (JCA). JCA is a technique for data exploration and dimensionality reduction for categorical variables analogous to factor analysis for continuous variables. JCA maps the attributes and states to a lower dimensional space capturing the most variance. The solution obtained by JCA involves the calculation of the singular value decomposition of the matrix of standardized residuals that is derived from the matrix of all cross tabulations of the categorical variables, known as the Burt matrix. The role of the Burt matrix in JCA is analogous to the role of the covariance matrix in factor analysis (Greenacre and Blasius, 2006).

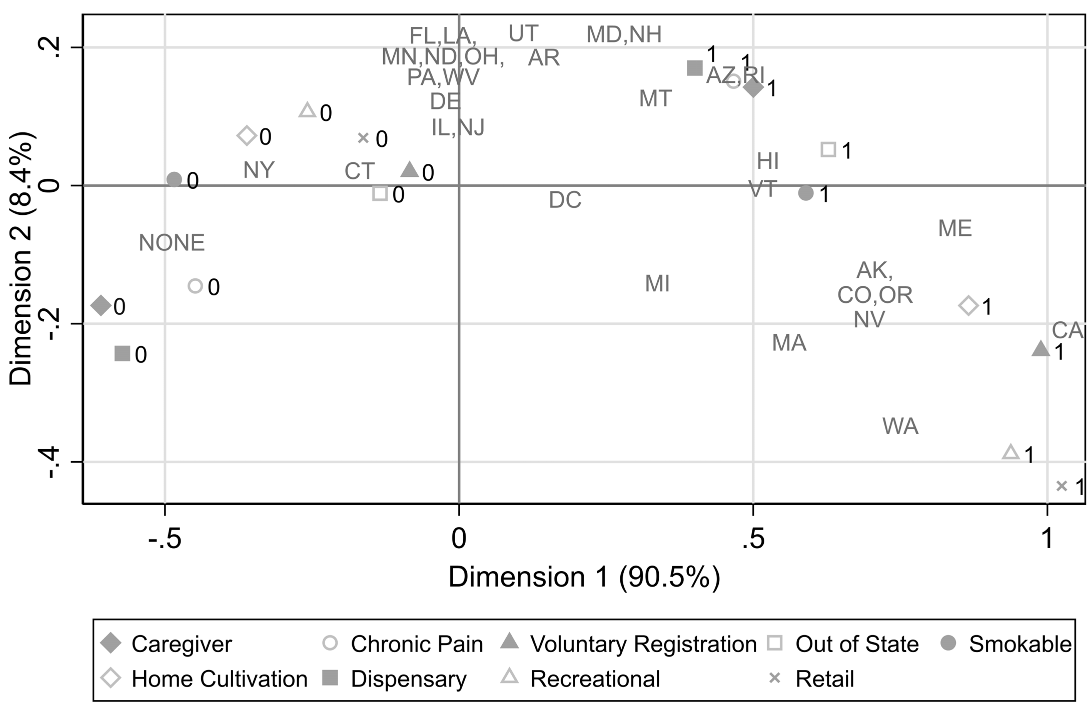
The results of the JCA are shown in Figure 2, which is the projection of the cloud of points in the space generated by all attributes onto the plane of the principal axes 1 and 2. The inertia of a dimension measures the link between the dimension and all the variables. It can be used as a measure of deviation from independence. Dimension 1 captures 90.5% of the inertia in the Burt matrix of state marijuana laws. Dimension 2 captures another 8.4% of the inertia, leaving just over 1% unaccounted for by the position of the state in these two dimensions.

The highest coordinates on dimension 1 are associated with the presence of *Home Cultivation, Voluntary Registration, Recreational, and Retail*. California, the first mover in liberalization, is the highest scoring state on dimension 1. The lowest coordinates on dimension 1 are associated with the absence of *Caregiver, Chronic Pain, Smokable, and Dispensary*. An MML that lacked all four of these measures would be particularly restrictive. States that had not liberalized their marijuana laws at all as of 2019 are the lowest scoring. This suggests the

Table 1: Indicator Variables Characterizing Marijuana Laws

Variable	Equals 1 if:
Caregiver	Patients can designate caregivers to procure or administer.
Chronic Pain	Can be prescribed for chronic pain.
Voluntary Registration	No penalty for possession with prescription and state ID.
Out of State	Procurement allowed with a valid out of state registration (limits apply).
Smokable	Patients allowed to consume by smoking.
Home Cultivation	Patients allowed to grow their own (limits apply).
Dispensary	Provision for medical dispensaries included.
Recreational	Adult recreational use permitted.
Retail	Provision made for retail sale for adult use.

Figure 2: Joint Correspondence Analysis Biplot



1) The location of each state is labeled with the state abbreviation. NONE represents states with no MML or RML. Groups separated by commas share the location.
2) Markers at coordinates of presence (1) or absence (0) of attributes in principal normalization.

first dimension primarily reflects permissiveness toward the use of marijuana and captures most of the difference between states. The second dimension seems to reflect concern for control over details of medical use, with the presence of *Recreational* and *Retail* having the lowest coordinates and the presence of *Caregiver* and *Dispensary* having the highest.

The degree to which state marijuana laws are characterized by a single underlying dimension of permissiveness has implications for modeling the effects of marijuana laws. Separating the effects of different attributes is a dubious proposition due to the close relationship among them. Moreover, it is unclear the degree to which the elements of the law define a state's permissiveness toward marijuana versus merely reflecting it. In the later interpretation the laws are in part proxies for a broader set of norms and practices. Certainly, we must be thoughtful and parsimonious in choosing which attributes, or measures thereof, to include.

We use four variables in the models estimated below, though no more than three at a time. Our first model includes an indicator of the presence of an MML, denoted *MML, Dispensary, and Recreational*. In our second model we replace the binary indicator of the presence of an MML with a continuous measure of its permissiveness. To construct this measure, denoted *MMP*, we conduct a JCA like that depicted in Figure 2 but omitting *Dispensary, Recreational, and Retail*, and extract the state scores on the first dimension. To simplify interpretation, we scale *MMP* to have a minimum of 0 and a maximum of 1.

3. ADDITIONAL DATA

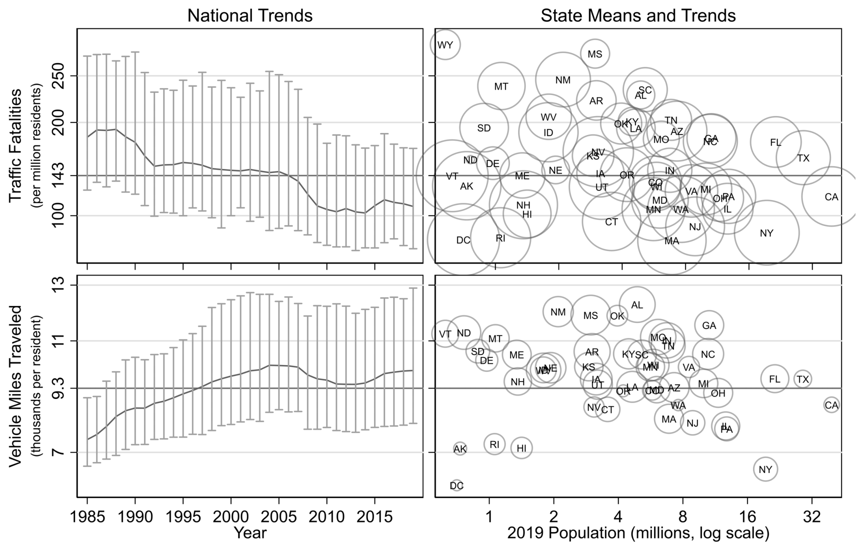
We estimate variations of a difference in difference Poisson regression model with time varying effects. The dependent variable in all instances is *Fatalities*, annual traffic fatalities for the 51 states from 1985 through 2019, comprising 1,785 observations. Data on fatalities is from the Fatality Analysis Reporting System (FARS) database maintained by the National Highway Traffic and Safety Administration.

To control for exposure, all models include both the natural log of state population, *Population* from the U.S. Census Bureau (U.S. Census Bureau, nd), and the natural log of annual vehicle miles traveled per capita, *VMTpc* from the Federal Highway Administration (Federal Highway Administration, nd). It is important to include both for several reasons. First, not all VMT are created equal. Tourists and trucks passing through low population states are a different matter than heavy interactions on congested roads in more populous states. Second, not all fatalities are in vehicles. Pedestrians and cyclists are killed too but are not reflected in VMT. Third, optimal state investments in traffic safety increase with population in a way that implies fatality rates will rise less than proportionately with population (Dewey et al., 2003). We also control for an indicator of whether a primary seatbelt law was in effect in each state-year, *Seatbelt*, from the Centers for Disease Control and Prevention (Centers for Disease and Control Prevention, nd).

Simple tables of summary statistics do not give a complete or nuanced sense of the panel data under analysis. Figures 1 and 2 provide such information regarding state marijuana laws. Figure 3 provides such information regarding *Fatalities, VMTpc, and Population.* A standard table of summary statistics is provided in Appendix 1.

The top left panel of Figure 3 shows the national mean annual fatality rate and the

Figure 3: Traffic Fatalities, Vehicle Miles Traveled, and Population



Ranges are 10th and 90th percentiles among states. Horizontal lines are national averages over all years.
Fatalities per capita fell for all states. Circle area in the top right panel proportional the average annual rate of decline.
VMT per capita increased for all states. Circle area in the bottom right panel proportional to the average annual rate of growth.
Wyoming is off the scale of the lower right panel at 15,555 VMT per capita.

10th and 90th percentiles among states. Annual fatalities averaged 143 per million residents nationally, falling over time as the range across states narrowed. The top right panel shows state means for annual fatalities per million residents plotted against 2019 population. The size of the circles is proportional to the average annual rate of decline in the fatality rate. For example, Wyoming and Mississippi had the highest mean fatality rates and relatively slow improvement in traffic safety while the District of Columbia, Massachusetts, New York, and Rhode Island had the lowest fatality rates and relatively rapid improvement in traffic safety.

The bottom left panel of Figure 3 plots annual vehicle miles traveled per capita and the 10th and 90th percentiles among states. Annual *VMTpc* averaged 9,322 nationally and increased over time as the range across states widened. The bottom right panel shows the mean for each state plotted against 2019 population. The size of the circles is proportional to the average annual rate of increase in *VMTpc*. For example, Mississippi and Alabama had relatively high mean *VMTpc* and saw relatively rapid increases, whereas the District of Columbia and New York had relatively low mean *VMTpc* and saw much slower increases. Wyoming is literally off the chart at a mean of 15,555 *VMTpc*.

4. MODEL SPECIFICATION

All models include state and year fixed effects. Year fixed effects differ between the group of states that had an MML as of 2019 and the group that did not. All models allow for time varying annual effects before and after a marijuana law goes into effect.

To express our specification precisely, let i index states (1 to 51), $State_i$ denote indicators for each state, t index years (1985 to 2019), $Year_t$ denote indicators for each year, and L_i denote an indicator of whether state i had implemented an MML as of 2019. Define m as the year minus the year the MML became effective, so that it takes on the value 0 during the effective year. Define d and r similarly for $Dispensary$ and $Recreational$, respectively. Define $MMLYear_{mit}$ to be an indicator equal to 1 if it is year m in state i in year t and 0 otherwise. Define $DispensaryYear_{dit}$ and $RMLYear_{rit}$ similarly for $Dispensary$ and $Recreational$. In some models we employ a binary MML indicator, MML, and in others we employ our measure of the permissiveness of the MML, MMP. Let $MMLVariable_{mit}$ represent whichever is used.

The rate for the Poisson process in our models may then be written as follows.

$$\begin{aligned}\ln(Rate_{it}) = &\sum_{m>-6} \beta_m MMLVariable_{mit} MMLYear_{mit} + \sum_{d>-6} \beta_d DispensaryYear_{dit} \\ &+ \sum_{r>-6} \beta_r RMLYear_{rit} + \beta_{SB} Seatbelt_{it} + \beta_{Pop} \ln(Population_{it}) \\ &+ \beta_V \ln(VMTpc_{it}) + \sum_{i>1} \beta_i State_i + \sum_{t>1985} \beta_t Year_t + \sum_{i>1} \sum_{t>1985} \beta_{Lt} L_i Year_t + \beta_0\end{aligned} \quad (1)$$

Model 1 replaces *MMLVariable* with 1 in states with an MML and follows prior work by using the binary indicators *MML, Dispensary,* and *Recreational*. It differs in that it controls for all three simultaneously. Model 2 replaces *MMLVariable* in equation (1) with 1 for $-6 < m < 0$ and *MMP* for $m \geq 0$. Models 3 and 4 drop *DispensaryYear* from models 1 and 2.

In all models we assume effects are zero more than five years before the effective year. Thus, the comparison group includes all state-years for states that never implemented a relevant marijuana law and all state-years more than five years prior to the effective date of the provision for states that did implement one. A clear trend over the years before the law took effect would cast serious doubt on causal interpretations of related effects. For years more than five years following the effective date, we measure only the pooled average effect. That is because the number of state-years observed falls as the lag increases, particularly precipitously for RMLs. We exclude years where m, d, or r equal -1 or 0 when comparing effects before and after changes in laws for two reasons. First, laws go into effect in different months in different states. Second, the effective year is sometimes the year after the law's adoption. However, police and prosecutors may not enforce laws that will soon be inapplicable, and individuals may expect that unofficial change or be confused about the effective year.

5. RESULTS

Full results for all models are available in Appendix 2. For all models, the leave-one-out (n-fold) cross validated root mean square error (LOO-RMSE) of fatalities per million residents is near 18. For the average year from 1985 through 2019 national fatalities per million

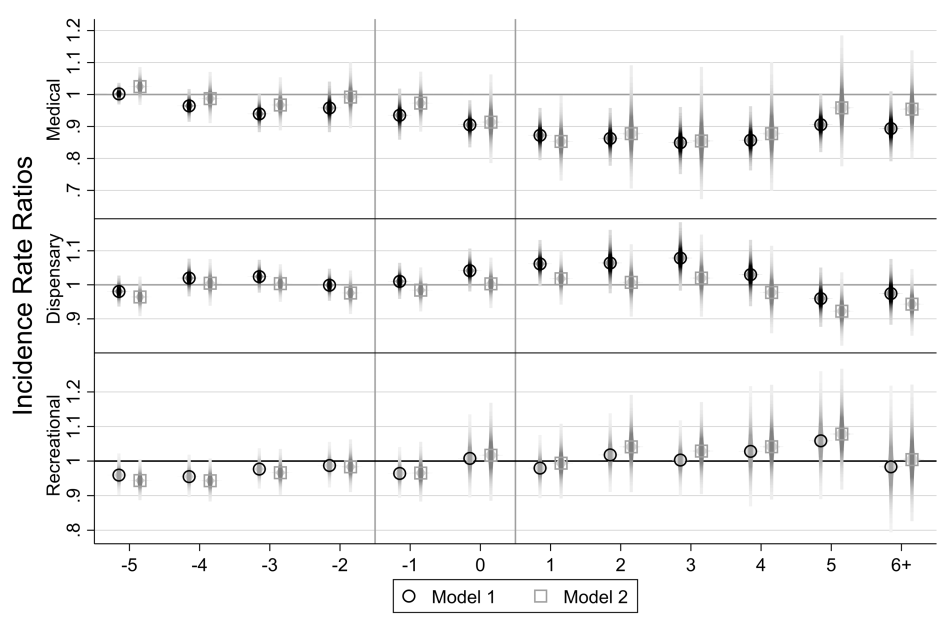
U.S. residents averaged 143, while fatalities per million residents were 157 for the average state-year. Thus, the models are accurate to within about 12% of fatalities.

Before presenting the main results, we briefly discuss the results for the control variables. The coefficients on the log of *Population* are highly consistent, indicating an elasticity between 0.87 and 0.9 that is statistically significant ($p < 0.001$) in all cases. Similarly, the coefficients on the log of *VMTpc* indicate an elasticity between 0.52 and 0.58 that is statistically significant ($p < 0.001$) in all cases. The results for *Seatbelt* indicate approximately a 3% reduction in fatalities when such a law is present, and the effect is statistically significant at the 5% level for a one-tailed test in all cases.

5.1. Primary Results on the Effects MMLs and RMLs

The results of primary interest from models 1 and 2 are presented in Figure 4. The top panel presents the results regarding the effect of MMLs. The middle panel presents the effects of dispensary provisions. The lower panel presents RML effects. Effects are presented as incidence rate ratios (IRR). The IRR is the estimated ratio of the number of fatalities with a marijuana law provision to fatalities without that provision, holding all else equal. The IRR is calculated by exponentiating the estimated coefficients. An IRR less than one indicates a reduction in traffic fatalities, while no effect corresponds to an IRR of 1.

Figure 4: Effects of Marijuana Law Liberalization from Models 1 and 2



1) Model 1 uses MML, Model 2 uses MMP. Both include log population, log vehice miles traveled per capita, a primary seatbelt law indicator, state fixed effects, and separate year fixed effects for states that ever adopted an MML and those that did not.
2) Robust standard errors clustered by state. Smoothed CIs extend to 99%. N=1,785, 50 US States and DC, 1985-2019.
3) Leave one out RMSE, fatalities per million residents: Model 1 = 18.24, Model 2 = 18.08.

The results of models 1 and 2 are very similar. The LOO-RMSE is very slightly improved in model 2, presumably due to the additional information in the continuous variable *MMP* compared to *MML*. However, the confidence intervals for yearly effects are wider in model 2, presumably due to the difficulty in precisely estimating a state's value of the underlying dimension of permissiveness from six binary indicators.

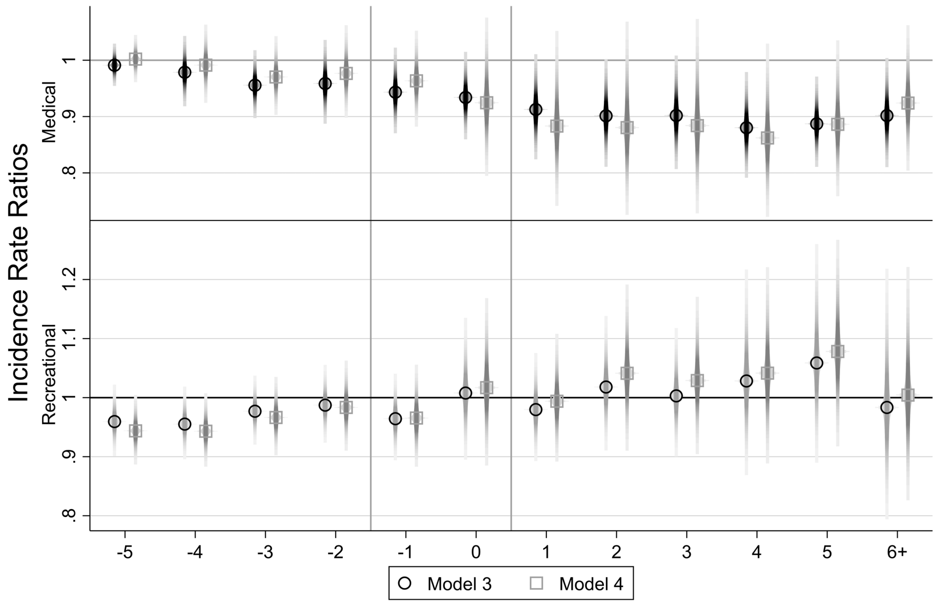
It is important to note how closely the effects of *Dispensary* mirror the effects of *MML* in model 1 and *MMP* in model 2. MML effects and dispensary effects are not independent. We noted above that separating the effects of specific aspects of state marijuana law is a dubious proposition since all attributes we measured appear to largely reflect a single underlying dimension. The pattern seen in the top two panels of Figure 4 is consistent with that observation. Combining the evidence from the top two panels by summing the two effects corroborates previous findings that traffic fatalities decline following an MML. Chi-squared tests of the null hypothesis that the average effect of *Dispensary* from years -5 through -1 is 0 cannot be rejected for model 1 ($p = 0.669$) or model 2 ($p = 0.518$) nor can the hypothesis that the average effect of *Dispensary* from years 1 on is 0 ($p = 0.398$ and $p = 0.623$ respectively). Thus, controlling separately for *Dispensary* merely confuses the results.

There appear to be no clear pre or post RML effects in model 1 or model 2, although excluding years 6+ there is the appearance of slight upward drift beginning before the RML and continuing through year 5. The hypothesis that the average effect from years -5 through -2 does not differ from the average effect from year 1 on cannot be rejected for model 1 ($p = 0.371$). In model 2 the difference appears marginally statistically significant ($p = 0.098$); however, this must be taken with caution since we show below that type 1 error rates are inflated in these models. Confidence intervals widen as years from the effective year increase simply due to the sparsity of the sample of states with long lived RMLs as of 2019.

Given that *Dispensary* was statistically insignificant in models 1 and 2, and that its relationship with *MML* and *MMP* muddies the results, we drop it and re-estimate the models. Having dropped *Dispensary*, we recalculate *MMP* by including *Dispensary* in the JCA of MML provisions. The top panel of Figure 5 shows the results for *MML* and *MMP* in models 3 and 4. MMLs are clearly associated with reductions in traffic fatalities. The null hypothesis of no difference in before and after effects is rejected in both model 3 ($p = 0.002$) and model 4 ($p = 0.021$), however, the effect is smaller from year 6 onward than in years 1 through 5.

The lower panel of Figure 5 shows the effects of RMLs. We do not distinguish between RMLs with and without retail sale provisions, since the two are so closely related. However, since retail sales follow RMLs with a lag, we might expect to see a change a few years after the effective year. Indeed, as of 2019 only Alaska, Colorado, Oregon, Washington, and the District of Columbia had 4 or more years under an effective RML, all of those but the District of Columbia had retail sales provisions for those years, and there was a modest increase in fatalities for year 4 and a slightly larger increase for year 5. Taking a broader view, that increase can be seen as part of an upward drift in fatalities in states implementing RMLs that starts five years before the effective year. Findings of an increase in traffic fatalities following an RML may simply reflect this pre-existing upward drift. The increase associated with this upward drift disappears in year 6 on, although only Colorado and Washington had RMLs that long as of 2019.

Figure 5: Effects of Marijuana Law Liberalization from Models 3 and 4



1) Model 3 uses MML, Model 4 uses MMP. Both include log population, log vehice miles traveled per capita, a primary seatbelt law indicator, state fixed effects, and separate year fixed effects for states that ever adopted an MML and those that did not.
2) Robust standard errors clustered by state. Smoothed CIs extend to 99%. N=1,785, 50 US States and DC, 1985-2019.
3) Leave one out RMSE, fatalities per million residents: Model 3 = 18.16, Model 4 = 18.03.

Even with the upward drift, the null hypothesis of no difference in average before and after RML effects cannot be rejected in model 3 ($p = 0.396$) or model 4 ($p = 0.148$). It is again worth noting the increase in the width of the confidence intervals from year 4 on, which reflects the small number of states with RMLs of that age. Keeping that limitation in mind, RMLs are not associated with any changes in traffic fatalities as of 2019.

5.2. Cross Border Spillovers from Recreational Marijuana Sales

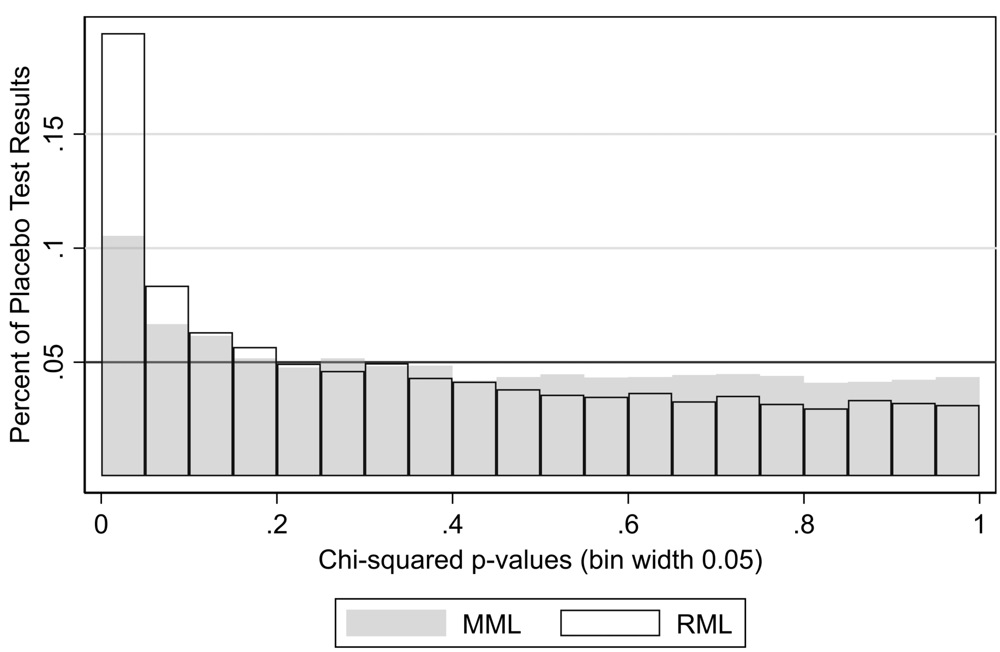
Hansen et al. (2020b) present compelling evidence that individuals travel from bordering states to states with legal retail marijuana sales to purchase marijuana. This suggests potential spillover effects from recreational legalization. We check to see whether this might alter our finding that RMLs have no impact on traffic fatalities by adding an indicator variable for whether a state that has not legalized recreational use borders one that has to the specifications of models 1-4, though we do not allow this effect to change over time. The full results of these models are included in Appendix 2. In all cases, bordering a state with an RML is associated with lower fatalities, though the difference is not statistically significant. The resulting IRR estimates range from 0.96 to 0.97 with p-values from 0.164 to 0.3. We find the same effect if we control instead for bordering a state with legal retail sales, and whether we lag the indicator two years to reflect the typical lag between legalization and commercialization.

5.3. Placebo Tests and Type 1 Error Rate Inflation

Since we are working with panel data, there is a possibility that persistence over time of unobserved time varying idiosyncratic shocks may inflate type 1 error rates. We conduct a placebo test to get a sense of how likely it is that such a mechanism is behind our finding that MMLs are associated with reduced traffic fatalities. Specifically, we randomly exchange the state level marijuana law histories 10,000 times, each time estimating model 3 and saving the p-value from the tests of no-difference in before and after MML and RML effects. Anderson et al. (2013) conducted a similar test.

The distributions of the resulting p-values are shown in Figure 6. If there were nothing systematic in the rearranged data, the distribution in Figure 6 would tend to be uniform. Since some states may be randomly assigned their own series, and since *MML* and *Recreational* may share a similar time-path in some states, we may not expect the distribution to be precisely uniform. However, a high probability of finding statistically significant results with randomly rearranged time series would suggest inflated type 1 error rates from standard tests.

Figure 6: Placebo Test Results



The MML and RML time series were randomly reassigned among states 10,000 times. Each time model 3 was estimated and Chi-squared tests of the null hypothesis of no difference between before and after effects for MML and RML were conducted. The distribution of p-values is shown.

The results indicate inflated type 1 error rates, particularly for tests of the effects of RMLs. Given the near four-fold increase in p-values less than 0.05 shown for RMLs, none of the results above for RML effects are anywhere close to statistically significant. That is not so for the MML effects. The p-value for the test of no difference in before and after effects in model 3 was 0.002, and p-values less than or equal to 0.002 occurred in only 130 of 10,000 trials in our placebo test. In model 4 the p-value from the test of the null hypothesis of equal before and after effects was 0.021, and p-values less than 0.021 occurred in only 560 of 10,000 trials.

6. CONCLUSION

We find lower state traffic fatalities following implementation of MMLs, consistent with earlier work. This is true whether we employ a simple MML indicator or a continuous indicator of the permissiveness of state medical marijuana laws. The effect weakens in later years. We showed type 1 error rates are likely inflated in our models, and so likely inflated in related work employing similar difference in difference analysis of traffic fatality data. This tendency is not strong enough to overturn the conclusion that traffic fatalities decreased following MMLs.

Controlling for prior MMLs, we find no evidence of a statistically significant association between RMLs and traffic fatalities. We find no evidence of association between traffic fatalities and cross border recreational legalization. Those null results are strengthened by our finding that type 1 error rates are likely inflated in this context. There is upward drift in fatalities in states that implemented RMLs from five years prior to the effective year through five years after. Findings of statistically significant before and after effects of RMLs may simply reflect this upward drift.

Examining nine attributes of state marijuana laws, including numerous provisions related to medical use, recreational legalization, and provision for retail sales, we found they are all largely captured by a single underlying dimension measuring permissiveness toward marijuana use. This provides important context for interpreting findings regarding the effects of MMLs and RMLs. Even if RMLs are later found to be associated with higher traffic fatalities, the net effect of both MML and RML provisions is the appropriate measure of the impact on traffic fatalities associated with liberalization of marijuana laws. Moreover, from this point of view, RMLs simply reflect the same underlying permissiveness as do MMLs. In states with a very permissive stance, reflected in expansive MMLs adopted prior to RMLs, recreational legalization and retail sales may not lead to as drastic a change in behavior as we might otherwise expect.

Identifying the effects of RMLs is complex and the available data is yet limited. The effects following liberalization in other states with different histories, policies, and norms may differ from the effects associated with liberalization so far. Liberalization may eventually be shown to lead to more fatalities, at least under some sets of circumstances, as more and different states legalize recreational use, and more data accrues. However, as of 2019, we find liberalization has been associated with lower traffic fatalities, not higher.

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APPENDIX 1: SUMMARY STATISTICS

Table A.1: 1785 Observations, 50 U.S. States and the District of Columbia from 1985 through 2019.

Variable	Mean	Standard Deviation	Minimum	Maximum
Fatalities (per million residents)	156.927	61.543	23.625	384.318
MML	0.200	0.400	0	1
MMP	0.122	0.263	0	1
RML	0.030	0.171	0	1
Dispensary	0.162	0.369	0	1
Population (millions)	5.598	6.285	0.454	39.512
VMTpc (thousands)	9.788	1.917	5.079	18.296
Seatbelt	0.388	0.487	0	1

Appendix 2: Model Results

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MML Variable	*MML*	*MMP*	*MML*	*MMP*	*MML*	*MMP*	*MML*	*MMP*
Dispensary Year	Yes	Yes	No	No	Yes	Yes	No	N0
RML Border	No	No	No	No	Yes	Yes	Yes	Yes
MML Year								
-5	0.002	0.025	-0.009	0.002	0.002	0.025	-0.009	0.003
	(0.013)	(0.022)	(0.015)	(0.016)	(0.013)	(0.022)	(0.015)	(0.016)
-4	-0.036	-0.013	-0.022	-0.009	-0.036	-0.012	-0.024	-0.01
	(0.021)	(0.031)	(0.025)	(0.027)	(0.021)	(0.031)	(0.025)	(0.027)
-3	-0.062*	-0.033	-0.045	-0.03	-0.062*	-0.032	-0.047	-0.031
	(0.025)	(0.033)	(0.024)	(0.028)	(0.025)	(0.033)	(0.025)	(0.028)
-2	-0.043	-0.008	-0.042	-0.024	-0.042	-0.007	-0.046	-0.026
	(0.032)	(0.04)	(0.03)	(0.033)	(0.032)	(0.04)	(0.03)	(0.032)
-1	-0.067*	-0.027	-0.058	-0.037	-0.068*	-0.027	-0.063*	-0.04
	(0.033)	(0.037)	(0.031)	(0.034)	(0.033)	(0.037)	(0.031)	(0.033)
0	-0.099**	-0.09	-0.068*	-0.078	-0.101**	-0.087	-0.073*	-0.081
	(0.032)	(0.059)	(0.032)	(0.059)	(0.031)	(0.06)	(0.032)	(0.057)
1	-0.136***	-0.159**	-0.091*	-0.124	-0.138***	-0.156*	-0.096*	-0.128
	(0.036)	(0.06)	(0.04)	(0.068)	(0.036)	(0.062)	(0.039)	(0.066)
2	-0.148***	-0.13	-0.104*	-0.127	-0.147***	-0.128	-0.104*	-0.126
	(0.041)	(0.085)	(0.041)	(0.075)	(0.04)	(0.083)	(0.041)	(0.075)
3	-0.163***	-0.157	-0.103*	-0.123	-0.162***	-0.153	-0.103*	-0.121
	(0.048)	(0.093)	(0.043)	(0.075)	(0.048)	(0.092)	(0.044)	(0.076)
4	-0.154***	-0.13	-0.128**	-0.148*	-0.151***	-0.127	-0.125**	-0.143*
	(0.045)	(0.088)	(0.041)	(0.069)	(0.044)	(0.087)	(0.041)	(0.069)
5	-0.099*	-0.042	-0.120***	-0.121*	-0.094*	-0.035	-0.116***	-0.114
	(0.039)	(0.082)	(0.035)	(0.060)	(0.038)	(0.082)	(0.035)	(0.061)
6+	-0.112*	-0.047	-0.103*	-0.079	-0.112*	-0.044	-0.102*	-0.075
	(0.047)	(0.068)	(0.042)	(0.054)	(0.046)	(0.068)	(0.041)	(0.053)
RML Year								
-5	-0.041	-0.058*	-0.056	-0.064*	-0.047*	-0.063*	-0.06	-0.067*
	(0.024)	(0.024)	(0.035)	(0.031)	(0.024)	(0.024)	(0.033)	(0.029)
-4	-0.046	-0.058*	-0.065*	-0.070**	-0.054*	-0.067*	-0.073*	-0.077**
	(0.025)	(0.026)	(0.030)	(0.026)	(0.025)	(0.027)	(0.029)	(0.025)
-3	-0.023	-0.034	-0.047*	-0.048**	-0.032	-0.043	-0.055**	-0.056**
	(0.023)	(0.027)	(0.019)	(0.018)	(0.024)	(0.028)	(0.020)	(0.018)
-2	-0.013	-0.017	-0.036*	-0.032	-0.004	-0.009	-0.024	-0.019
	(0.026)	(0.030)	(0.017)	(0.019)	(0.030)	(0.034)	(0.020)	(0.025)
-1	-0.036	-0.035	-0.060**	-0.051*	-0.027	-0.027	-0.047*	-0.038
	(0.029)	(0.035)	(0.02)	(0.024)	(0.033)	(0.039)	(0.022)	(0.029)
0	0.008	0.017	-0.017	-0.001	-0.005	0.004	-0.031	-0.013
	(0.046)	(0.054)	(0.031)	(0.039)	(0.047)	(0.056)	(0.032)	(0.04)
1	-0.02	-0.006	-0.046	-0.027	-0.033	-0.019	-0.061*	-0.04
	(0.036)	(0.042)	(0.030)	(0.033)	(0.037)	(0.045)	(0.030)	(0.032)
2	0.018	0.041	-0.011	0.017	0.002	0.025	-0.028	0.001
	(0.043)	(0.052)	(0.034)	(0.040)	(0.045)	(0.055)	(0.035)	(0.040)
3	0.003	0.029	-0.023	0.008	-0.012	0.014	-0.039	-0.007
	(0.042)	(0.050)	(0.038)	(0.043)	(0.044)	(0.053)	(0.039)	(0.043)
4	0.028	0.041	0.005	0.025	0.014	0.027	-0.011	0.011
	(0.065)	(0.062)	(0.059)	(0.062)	(0.065)	(0.063)	(0.059)	(0.060)
5	0.057	0.075	0.036	0.06	0.044	0.062	0.02	0.045
	(0.068)	(0.063)	(0.061)	(0.062)	(0.068)	(0.064)	(0.061)	(0.060)
6+	-0.017	0.004	-0.033	-0.002	-0.03	-0.01	-0.05	-0.019
	(0.083)	(0.076)	(0.070)	(0.067)	(0.083)	(0.078)	(0.071)	-0.066

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MML Variable	*MML*	*MMP*	*MML*	*MMP*	*MML*	*MMP*	*MML*	*MMP*
Dispensary Year	Yes	Yes	No	No	Yes	Yes	No	N0
RML Border	No	No	No	No	Yes	Yes	Yes	Yes
Dispensary Year								
-5	-0.02	-0.036			-0.02	-0.037		
	(0.018)	(0.023)			(0.018)	(0.023)		
-4	0.02	0.005			0.018	0.003		
	(0.021)	(0.026)			(0.021)	(0.026)		
-3	0.024	0.004			0.021	0.001		
	(0.018)	(0.021)			(0.018)	(0.022)		
-2	-0.001	-0.024			-0.006	-0.029		
	(0.018)	(0.025)			(0.018)	(0.027)		
-1	0.01	-0.016			0.007	-0.019		
	(0.020)	(0.025)			(0.02)	(0.026)		
0	0.041	0.003			0.038	-0.001		
	(0.023)	(0.029)			(0.024)	(0.03)		
1	0.060^{*}	0.018			0.057^{*}	0.013		
	(0.025)	(0.030)			(0.025)	(0.032)		
2	0.062	0.007			0.062	0.007		
	(0.034)	(0.041)			(0.033)	(0.04)		
3	0.076^{*}	0.02			0.076^{*}	0.019		
	(0.036)	(0.046)			(0.035)	(0.045)		
4	0.03	-0.023			0.031	-0.021		
	(0.037)	(0.051)			(0.035)	(0.048)		
5	-0.041	-0.081			-0.04	-0.079		
	(0.035)	(0.045)			(0.034)	(0.043)		
6+	-0.026	-0.058			-0.021	-0.055		
	(0.038)	(0.04)			(0.036)	(0.038)		
log (Population)	0.879^{***}	0.893^{***}	0.872^{***}	0.884^{***}	0.877^{***}	0.891^{***}	0.872^{***}	0.883^{***}
	(0.093)	(0.096)	(0.094)	(0.098)	(0.090)	(0.093)	(0.091)	(0.095)
log (VMTpc)	0.533^{***}	0.581^{***}	0.534^{***}	0.578^{***}	0.523^{***}	0.572^{***}	0.522^{***}	0.567^{***}
	(0.104)	(0.114)	(0.104)	(0.116)	(0.098)	(0.107)	(0.097)	(0.107)
Seatbelt	-0.029	-0.033	-0.028	-0.031	-0.031	-0.035^{*}	-0.031	-0.034
	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)	(0.017)
RML Border					-0.034	-0.032	-0.041	-0.039
					(0.029)	(0.031)	(0.029)	(0.031)

$N = 1785$. Standard errors in parentheses under coefficients. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$