New Evidence on the Casual Effect of Traffic safety Laws on Drunk Driving Fatalities

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New Evidence on the Causal Impact of Traffic Safety Laws on Drunk Driving Fatalities*

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Abstract

In the United States, about 28 lives are lost daily in motor vehicle accidents that involve an alcohol-impaired driver. While most states have enacted traffic-safety laws to address this phenomenon, little consensus exists on the causal impact of these laws in reducing drunk-driving fatalities. This paper exploits quasi-random variation in state-level laws to estimate the causal effect of select traffic laws on the frequency of fatal accidents involving a drunk driver. This is identified from the discontinuities in policy treatments among homogeneous contiguous-counties that are separated by a shared state border. This approach addresses the econometric issues created due to spatial heterogeneity that may have biased previous studies. We present convincing evidence that the estimates in the literature are prone to an upward bias. Further, if the effective laws were adopted as a federal mandate in 1986, they could have prevented about 24% of drunk-driving motor-vehicle fatalities.

Keywords: traffic-fatalities, drunk-driving, traffic-safety

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1 Introduction

In the United States, having access to a motor vehicle has become one of the essential conveniences of modern life. From 1990, survey estimates have consistently shown over 90% of households having access to this commodity. Operating a motor vehicle, however, is not without its fair share of risks, evident by the National Center for Health Statistics (NCHS) ranking motor vehicle accidents as the number one leading cause of preventable death among teens and the 13th leading cause of preventable deaths in the U.S in 2013. This hazard is compounded by risky behavior by some economic agents, such as speeding or driving while impaired. The National Center for Statistics and Analysis (2015) estimates that across all states in 2014, alcohol-impaired-driving fatalities accounted for between 20 percent (Vermont) and 41 percent (Massachusetts, North Dakota, and Texas) of the total count of traffic fatalities, with a national average of 31 percent. On a daily basis, there are 28 recoded deaths in motor vehicle accidents that are due primarily to an alcohol-impaired driver, with one of such loss of life occurring every 53 minutes. This also carries an estimated annual cost of $44 billion dollars resulting from lost productivity, legal, court and medical related expenses, property damage, and insurance administration. Consequently, the issue of traffic safety is a major health concern not only to those directly involved, but to society as a whole.

Policymakers have long advocated that these outcomes can be mitigated through the use of strict traffic laws that mandate safe motor vehicle operation, adequately punish offenders, and are robustly enforced. As a result, traffic laws have played a dual function of both maintaining the orderly flow of traffic as well as creating an incentive structure which dissuades individuals from engaging in any activity which could potentially lead to harm. Policymakers have attempted to legislate how vehicles are used on public carriageways, including placing speed limits, mandating seatbelt use, and restricting licenses among other measures. Many states have also implemented measures aimed at discouraging certain behavior by targeting those that are most likely to become perpetrators. For example, some states have implemented taxes aimed at making alcohol consumption more expensive; reducing excessive drinking and essentially passing through the cost of the negative externalities created through alcohol consumption to those who cause the externalities. At the extensive margin, alcohol taxes increases the cost associated with alcohol consumption and as such should deter some individuals from drinking or at the very least reduce the quantity of alcohol individuals consume. If this pigovian tax is effective, then it should reduce the level of inefficiency created by alcohol consumption and as such should reduce the frequency of individuals being at risk by driving while intoxicated.

This study exploits the across state variation in traffic laws to establish the causal impact of state-level legislation on traffic and drunk driving related fatalities. We argue that the implementation of and/or removal of pigovian taxes, historical laws, and traditional legislative restrictions creates a natural experiment that can be used to causally identify the effect of alcohol restrictions and drunk-driving punishments on the number of drunk-driving fatalities. This is done within a contiguous county specification which controls for spatial heterogeneity. By restricting our analysis to laws that are quasi-random and our sample to county pairs that share the same border, we are able to exploit the policy discontinuities at the state border to identify the treatment effect of each law of interest.

The contiguous counties identification approach utilized in this paper follows the earlier studies by Huang (2008) and Dube et al. (2010)[1]. In this paper, we present several specifications tests and convincing evidence of the causal relationship that we seek to establish.

In our preferred specification, we found that the most effective laws in reducing drunk-driving fatalities are: alcohol tax rate, open container laws, zero tolerance laws that create a blood alcohol concentration(BAC) limit of 0.02 g/dL and very harsh penalties for the first incidence of driving under the influence. Through a simple simulation using the estimated parameters, we show that available legislative solutions to this problem have been under utilized by policymakers over our sample period. Consequently, our results suggest that these tools can be used to address this widespread public health issue if they are employed at the federal level. Had the effective laws been implemented uniformly across states, the level of drunk driving fatalities would be about 24 percent lower annually. Further, a small increase in the alcohol tax rate over the period would have reduced the level of drunk driving fatalities by approximately 1 percent.

The remainder of this paper is outlined as follows: Section II provides a brief overview of the related literature. Section III describes the data we utilized, the model specification and provides an outline of the identification strategy utilized. Section IV provides the empirical estimates and robustness checks. Section V outlines the policy implications and the major conclusions derived from this study.

2 Literature

Time series and state-level panel anaysis are often used to assess the effectiveness of changing a particular traffic law. While it is possible to control for some relevant factors, these methodologies are particularly prone to the adverse influence of unobserved heterogeneity arising from spatial differences in individual behavior that may be correlated with drunk-driving law changes. For example, the state policymakers decision to increase the beer tax rate or reduce the acceptable BAC limit may be dependent on the trends in drunk-driving fatalities, the political leaning of the state legislature or the general economic environment of the state. The wide-ranging impact of traffic fatalities is reflected in the plethora of studies that have been conducted to assess the impact of traffic laws on traffic safety outcomes. The robustness of these studies have often been questioned due to the variances in results and a lack of a coherent narrative across the studies. The differences in methodology, mainly the extent to which spatial heterogeneity is accounted for, and variation in sample sizes, may have contributed to this lack of consistency. These studies have assessed the impact of alcohol control legislations (Freeman, 2007; Foss et al., 2001; Fell and Voas, 2006; Chaloupka et al., 1993), license laws (Fell et al., 2011; Shope, 2007; Williams and Shults, 2010), driving restrictions and vehicle operation rules (Ossiander and Cummings, 2002).

Alcohol control legislations can be divided into two categories, namely preventive or ex-post regulations(Ying et al., 2013). On the one hand, preventative regulations seek to limit the public's access to alcohol by increased prices (tax laws), limiting access (blue laws), and restricting possession (open container laws). On the other hand, ex-post laws seek to punish those that are guilty

[1]These studies utilized this approach in the Financial and Labor Economics literature.

of driving while impaired (DWI) by establishing minimum punishments, gradations of guilt, and licensing restrictions. The effectiveness of these regulations is not consistent across the literature, with variations in data and methodology predicting varying effects.

The estimated impact of the BAC law on traffic fatalities provide just one example of this inconsistency found in the literature. While some studies suggest that BAC laws, specifically the lowering of the BAC threshold, do not have a significant impact on traffic fatalities (Freeman, 2007; Foss et al., 2001), others find that this law can be meaningfully used as policy lever to address this phenomena (Fell and Voas, 2006; Eisenberg, 2003; Wagenaar et al., 2001; Dee, 2001). The differences between the results of these studies is attributed to inconsistencies in the time period employed (Freeman, 2007) and the use of intra or inter-state comparisons (Foss et al., 2001). Similarly, early studies into the effectiveness of beer excise taxes suggested that they were very effective in reducing traffic fatalities, a narrative that has not been consistently supported by later studies. The earlier studies found that increasing the beer tax rate reduced the total level of fatal traffic accidents across all age groups(Cook, 1981; Saffer and Grossman, 1987). These studies were subsequently challenged on the basis that they did not adequately address the issue of unobserved heterogeneity(Dee, 1999). More recent studies have suggested that increasing the beer tax either has a small effect (Chaloupka et al., 1993) or an insignificant impact (Mast et al., 1999; Young and Likens, 2000; Young and Bielinska-Kwapisz, 2006) on traffic fatalities.

The effectiveness of DUI fines and Administrative License Revocations (ALR) have been explored by many studies in the literature. The estimated impact of these laws have varied profusely across states and studies. As such, while some studies have found DUI fines to be effective in reducing traffic fatalities (Chaloupka et al., 1993; Wagenaar et al., 2007; Wilkinson, 1987), other studies find dissimilar results (Young and Likens, 2000). Mixed results have also been found for ALR laws(Ying et al., 2013).

The laws that seek to improve traffic safety and reduce the risk of an accident by regulating how vehicles are operated, such as seat belt and speed limit legislations, are often found to be negatively associated with traffic fatality rates (Freeman, 2007; Cohen and Einav, 2003). However, there is a lack of concurrence on the magnitude and significance of these effects. Cohen and Einav (2003) point out that the effectiveness of seat belt laws is often overestimated in the literature. They posit that more accurate data, in terms of percentage of seat belt usage, and differentiating between primary and secondary enforcement allows for a clearer picture of the effectiveness of seat belt laws. They also point out that the ability of their study to exploit interstate variation in seat belt laws allowed for more accurate modeling of the effect of seat belt laws.

Given the high propensity of young drivers to be involved in fatal crashes, there has been an effort to extend greater control on how this cohort operates motor vehicles. Graduate Driver's License (GDL) laws encompass a number of measures that seek to restrict vehicle operation by young individuals. The extent to which GDL laws are implemented by states vary widely and the significance of their impact on fatal accidents has varied across states and studies as a result(Fell et al., 2011; Shope, 2007). Despite these variances, there is a consensus in the literature that GDL laws are effective in reducing the rate of teen involvement in traffic accidents and fatal crashes, specifically for 16 and 17 year olds (Chen et al., 2006; Fell et al., 2011; Shope, 2007). There is, however, evidence that the benefits of such laws do not extend past the immediately impacted cohort (Chen et al., 2006) and even a suggestion that strong GDL laws were associated with increased involvement in fatal crashes by the 18 year old cohort(Masten et al., 2011).

Despite the plethora of related studies within the field, there remains a high level of inconsistency in the results. The tendency for studies to utilise state-level panels and time series data makes their findings susceptible to issues of spatial heterogeneity and omitted variable bias. Consequently, there remains a need for a more comprehensive analysis of the average effect of these laws, addressing the concerns raised by previous studies. We propose the contiguous-counties model as an approach that could provide meaningful insight about some of the questions raised about previous studies. This approach should yield estimates that are unbiased and consistent across sub-samples.

3 Data and Empirical Specification

In this section, we provide a general overview of the data, selection restrictions and methods we employ in the paper. We begin with a description of the key sources of the data in section 3.1, sample restrictions in section 3.2, descriptive statistics in 3.3 and then concluding with the empirical model specification in section 3.4.

3.1 Data

To be consistent and for ease of comparability with the previous studies in the literature, we utilize data on traffic fatalities from the Fatality Analysis Reporting System (FARS). This dataset is compiled by the National Highway Traffic Safety Administration (NHTSA) using data reported by various state-level agencies. It is comprised of fatal accidents involving a motor vehicle being used on any traffic-way that is open to the public within any of the 50 States and the District of Columbia. To qualify for inclusion, the incident must have resulted in the death of an involved party within 30 days of the crash.

The quality of this data should be reasonably high, since coding manuals are provided with a set of instructions on how coders can transfer information from a police accident/crash report to the FARS; several classes are held annually to train coders and system wide meetings are held to encourage uniform coding practices. The data is collected in a very organized manner, with several regional NHTSA offices and several trained FARS analysts collecting, transcribing and transmitting their state's data to the centralized system in a standard form. These analysts consult several records including; death certificates, coroner/medical examiner reports, hospital records, vital statistics police reports and the state highway department data. For each county, we begin by aggregating the total number of fatal accidents involving a drunk driver and the total number of overall fatal accidents[2]. To check the robustness of our estimates and to appropriately address the potential concerns of our readers, we also provide estimates from models involving these outcomes calculated for various days of the week and times of a given day.

[2]It should be noted that the county observed in the FARS dataset is the location where the accident occurred and not the driver's county of residence. This raises concern of a potential migration bias. This is more extensively discussed in section 4

We also utilize data on state traffic laws from the State Health Policy Research Dataset (SHEPRD). This dataset was created to examine state-level public health laws from 1980-2010 and features laws such as taxes on packaged beer, minimum and maximum fines and other forms of traffic rules such as seatbelt and graduated license laws. Compilation of the data set was facilitated by the Robert Wood Johnson Foundation and the United States Department of Health and Human Services, the National Institute of Health and the National Institute on Alcohol Abuse and Alcoholism. We focus on the period 1986-2005, since the majority of the changes in traffic laws occurred during this period and since this time period minimizes the number of missing laws we have in our dataset. This results in a balanced dataset consisting of traffic laws and outcomes over a 20 year period, for 49 states.

Finally, to ensure we are adequately accounting for the size of the county in our analytical framework, we collect county-level population data from the US Census Bureau and land area data from Dube, Lester and Reich (2010) to complete our dataset. Given the method we employ, we have compiled one of the largest aggregated dataset to be used in the literature to date, which should also lead to an efficiency advantage over previous studies.

3.2 Sample Restrictions

The balanced dataset utilized in this study was comprised of observations at the county level over the period 1986-2005. The contiguous border counties sample (CBCS) is further restricted using the population sizes and land area disparities within each contiguous county pair. Given our criticism of the heterogeneous comparison groups utilized in previous studies, this study further restricts the sample by dropping county-pairs where the difference in the size of the land area (in squared miles) and population within the pairs are too large. This saw county-pairs having the top 5% of disparity in land area or population being omitted from the sample. Consequently, for each pair within this sample, we calculate the within pair absolute difference in the land area and population counts. The county pairs belonging to the tail (defined as being beyond the 95th percentile) of the resulting distribution are omitted from our analysis[3]. This relies on the plausible assumption that county-pairs that are comprised of counties with similar land area and population size are more homogeneous. This prevents us from comparing very big counties (either in land area or population) to very small counties that are by nature less similar. Through the use of contiguous county-pairs across state lines, eliminating county pairs where the within pair variation in land area and population are outliers and by using county-pair by year fixed effects, this study has employed the largest combination of measures to address the issue of spatial heterogeneity (common time trend) that studies in this literature have utilized to date. Finally, we omit Hawaii and Alaska from our analysis for obvious reasons.

3.3 Descriptive Statistics

Columns 1 and 2 in table 1 below shows the mean and standard deviation for the contiguous-counties sample and columns 3 and 4 shows the mean and standard deviation for all counties in the US. The restrictions imposed on our CBCS reduces the disparities in the observed characteristics within

[3] The qualitative conclusions do not change when we include the omitted 5%.

each pair by design. This can easily be seen from table 1, where the standard deviations from the contiguous counties sample are sufficiently smaller than those from the sample including all counties, though the mean values from both samples are very similar[4]. Across both samples, the average number of fatal crashes involving an intoxicated driver was approximately 11 per hundred thousand population.

Table 1: County Level Descriptive Statistics 1986-2005

	Mean[1]	Std. Deviation [2]	Mean[3]	Std. Deviation[4]
Number of Fatal Crashes (Drunk)	10.84	14.82	10.72	17.89
GDP Per Capita	27573.07	7791.85	27996.28	7822.51
Poverty Rate	13.32	3.75	13.38	3.61
Motor Vehicle Reg. in 000s	4493.73	3763.26	5470.53	4677.99
Population	56371.44	101678.20	84729.59	279100.10
Land Area (square miles)	1000.40	1230.58	1114.36	3472.10

While these descriptive statistics do not provide conclusive evidence that the CBCS solves the problem of spatial heterogeneity, this table offers two key insights. First, since our preferred specification is identified from the variation in traffic policies within each contiguous border county pair, it is necessary that the counties observed characteristics within each pair are similar. Through our restriction of omitting pairs where the difference in population or land area is 'too large', we are able to reduced the standard deviation in the observed characteristics across all pairs. Since the deviation from the mean characteristics in the CBCS is noticeably smaller than in the sample including all counties, this suggests that there is a higher degree of homogeneity in the former. Second, the restrictions we imposed at the pair level and the use of only contiguous counties led to very large counties (in terms of population size and land area) being omitted. This is necessary since these counties are unlikely to be similar in observed and unobserved characteristics to other counties. Both of these observations offers moderate support to the identification framework we exploit. However, given the multi-dimensional space of the policies/treatment we are evaluating, we are unable to offer stronger evidence of comparability.

Table 2 provides a summary of the policies we examine in this study. Since the identification framework utilized in this paper relies on the changes in policies within each contiguous county pair, this table shows the mean value for these laws across all states and the number of states that have changed each law over the period of study. For each discrete variable (e.g. a law prohibiting open containers in cars), the third column shows the difference in the number of states who had the law in 1986 and those who had the law in 2005. For the continuous variables (e.g. tax rate on packaged beer), we present the change in the mean value of the variable from 1986 to 2005.

The table shows that there is sufficient variation in state traffic legislations over the period to identify the model we present below. The only laws which offer a cause for concern is mandated prison time for drivers committing their first DUI[5].

[4]This conclusion does not hold for the population and land area variables, since these were used to restrict our CBCS. Therefore, the two samples will be different along these dimensions by design

[5]A closer inspection of this variable show changes in the average across years. This suggest that some states are implementing the policy while others are repealing it.

Table 2: **State Level Descriptive Statistics**

	Mean [1]	Std. Deviation[2]	**Change 2005-1986**[3]
BAC Limit of 0.08 g/dL (adults)	0.3	0.46	48
Underage BAC Limit of 0.02 g/dL	0.57	0.49	45
State Beer Tax ($ per gallon)	0.23	0.17	0.05
Law Prohibiting Open Containers in Cars	0.50	0.50	27
Beer Keg Registration Required	0.25	0.43	22
Mandated Min Fine 1st DUI	139.35	186.01	131.63
Mandated Comm. Service 1st DUI	0.38	0.49	-12
License Susp. Pre-Con. 1st DUI	0.64	0.48	21
Mandated prison time 1st DUI	0.33	0.47	1
Marginal GDL	0.15	0.36	14
Good GDL	0.18	0.38	31
Bicycle Helmet Use	0.21	0.40	18
Seatbelt Fine	0.82	0.38	0.71

3.4 Empirical Model Specification

3.4.1 Traditional Specification

Previous studies have predominantly utilized a panel of states to evaluate the effect of traffic legislations in explaining traffic fatalities. The results obtained by these studies are potentially biased since they utilize econometric specifications which typically compare the fatality rates in states that have enacted a particular traffic law(s) to those that did not. These approaches assume that each state legislature's decision to implement these laws are independent of all omitted factors (state's political and economic environment) which potentially influences individuals' decision to drive while intoxicated or the count of fatal accidents in the state. Consequently, the extent of the bias will depend on the degree to which the pre-implementation trend in fatality counts in the state(s) which enact various traffic laws differ from those who do not implement these laws and the ability of previous studies to include variables which control for all the factors driving these differences. Due to the high degree of heterogeneity between the states in these samples, the usual practice of including time and state fixed effects yield little additional benefits in providing estimates that can be interpreted causally. Studies in the literature that have utilized time series approaches may suffer from similar issues of model misspecification or changing trends in fatalities over time. Time series studies rely on the assumption that the intervention is exogenous, which is usually not satisfied in this environment. While a majority of the previous studies provide us with key descriptive insights in understanding the impact of traffic rules on driving related fatalities; there is still a need for robust estimates produced using a method which address the limitations pointed out in the previous literature. We propose that the method utilized in this study may be able to bridge this gap in a meaningful way. We begin by estimating models which replicates the previous studies using several methods employed in previous works.

We begin with the model that is most often utilized. This specification can be expressed as follows:

$$F_{st} = \alpha + T_{st}\pi + X_{st}\phi + \theta_s + \theta_t + \varepsilon_{st}$$

Where F_{st} is the number of total fatalities or fatalities involving a drunk driver in state s at time t, T is a vector of traffic laws, θ_s is state-level fixed effect and θ_t are time fixed effects. To test this approach robustly, we also include other state-level controls that potentially explain the differences in the fatality trends observed across states and may also be correlated with the timing of the implementation of traffic laws. However, under even the most robust specification of this model, there is a high likelihood that the necessary condition $E(\varepsilon_{st}|T_{st}, X_{st}) = 0$ is not satisfied due to the high degree of heterogeneity which exists among states. Before presenting our most preferred empirical specification, we also show that utilizing all counties in the united states would also provide estimates that are expectedly unreliable. This is because we are still comparing very distinct groups, each possessing a time trend in the outcome variable that may be distinctive. Therefore, the differences in the time trend between counties pre- and post-interventions are likely to lead us to conclude a false positive in judging the effectiveness of the intervention. The specification we are using for the all counties sample is:

$$F_{ct} = \alpha + T_{st}\pi + X_{st}\phi + \theta_c + \theta_t + \varepsilon_{ct}$$

Identification in this model requires that $E(\varepsilon_{ct}|T_{st}, X_{st}) = 0$, which is not satisfied. There are a number of unobservables that are potentially correlated with T and also affect driving fatalities. Therefore, differences in wealth, education or even racial composition could easily violate this exogeneity of treatment assumption that is required to estimate causal parameters.

3.4.2 Identification from Discontinuities at Contiguous State Borders: Contiguous Counties Approach

Our preferred specification proposes a more generalized difference in difference strategy which exploits the variation in adopted traffic laws between contiguous counties pairs across border states to causally identify our parameters of interest. In advancing this approach in the current literature, we cautiously subscribe to the view that a contiguous county method utilizes more homogeneous control groups than approaches which employ a contiguous states approach, those just randomly comparing states in general or any approach utilizing all counties in the US. Additionally, this panel approach allows us to difference out between county-pair variation which may affect the outcome. By including county-pair specific fixed effects, we identify our results using only the variation in the policies of interest which occur within the thousands of contiguous county pairs we utilize. That is, this approach relies on the changes in traffic legislation which occurs within each across-state contiguous county-pair for identification of the parameters. As such, the identifying assumption is that policy differences within county-pairs are uncorrelated with differences in residual driving fatalities in either county.

Another main benefit of using this approach is that the county pair specific time trend absorbs the unobserved heterogeneity that previous approaches have ignored. The preferred specification using this approach is given by:

$$F_{cpt} = \alpha + T_{st}\pi + X_{st}\phi + \theta_c + \nu_{pt} + \varepsilon_{cpt}$$

Where c, p, t indexes counties, pairs and time; ν_{pt} is the county-pair by year fixed effects and our outcome and errors are now defined at the county-time level within each contiguous county pair extending across a state border. Under this specification,a causal interpretation is more likely to be achieved as the assumption $E(\varepsilon_{cpt}|T_{st}, X_{st}, \nu_{pt}) = 0$ is now satisfied.

4 Empirical Findings

In this section, we present results from the conventional specifications and contrast these with the estimates from the contiguous-border county-pair specification we propose in this study. In all models, where possible, we include controls for smoking legislations (laws restricting cigarette ads, bar and indoor smoking bans and cigarette taxes); State GDP per capita, measures of state ideological leanings, minimum wage, population and multiple measures of policing. The outcome of interest in all models is the number of drunk driving fatalities per one-hundred thousand population.

In the main specifications, we present the estimated impact of the laws that are most directly related to drunk driving prevention. In table 3-6, rows 1-2 focus on law restricting alcohol use prior to operating a vehicle. This includes dummy variables capturing the presence of a BAC limit of 0.08g/dl for adults and a BAC limit of 0.02 g/dl for drivers under legal drinking age. Row 3 captures the impact of directly increasing the price of alcoholic beverages. Rows 4-5 capture the impact of laws restricting where an individual may possess an open alcoholic beverage and who can purchase a Keg. Finally, rows 6 to 9 capture the penalty for being caught driving under the influence (minimum fine, community service, license suspension, mandatory prison time).

4.1 Traditional and Contiguous-Counties Estimates

We begin the empirical analysis by estimating the effect of traffic laws on drunk-driving fatalities using the traditional specifications. These results are presented in table 3. The estimated parameters presented in Column 1 utilises state-level panel data, with added controls for state and year fixed effects. Columns 2 and 3 then utilise data for all counties in the US, with controls for state-level time trends and county level fixed-effects, respectively. We also include the set of controls discussed above where possible. All standard errors are clustered at the state-level.

The estimates from table 3 suggests that only laws restricting alcohol use prior to operating a vehicle are effective in reducing drunk-driving fatalities. These estimates suggest that having a BAC limit of 0.08 g/dL for adults and 0.02 g/dL for minors each reduced drunk-driving fatalities by about .4 per one-hundred population. For the other laws of interest, while most had the expected negative sign, they were not significant at conventional levels. Additionally, while the point estimates are consistent across all specifications, the county-level estimates are less precise. However, the estimates in this table are unreliable because there is credible evidence against the exogeneity assumption being satisfied in the state and all-counties specifications. For each law, the high degree of heterogeneity between treated and untreated states/counties could induce bias in the estimated parameters in a manner we cannot intuitively predict.

Turning our attention to the paper's main contribution to the literature, table 4 presents the estimates from the contiguous-counties specification. Column 1 shows the baseline specification that controls for county and state-level time-varying covariates and any factors that are fixed within-county over the sample period. However, this model does not control for the unobserved time varying factors within each contiguous-counties border-pairs. Column 2 then introduces the county pair specific time trend which restricts our empirical identification to the variation in traffic laws within each contiguous counties border-pair. We present Column 2 as our preferred specification, since it relies on within pair variation in traffic laws and controls for any unobserved time-varying factors common to both counties within each pair.

The estimates from the contiguous-counties border-pair specification in table 4 are much larger relative to those presented in table 3. The specification in column 1 of table 4 is comparable to that of column 3 in table 3, though the former uses all counties and the latter uses only contiguous counties. Comparing these two results, we can discern that just the act of using a more homogeneous sample increases the estimated impact of the laws. For example, the impact of taxes on packaged beer and open container laws increase in both magnitude and statistical significance moving from the all-counties sample in column 3 of table 3 to the contiguous-counties sample in column 1 of table 4. This suggests that spatial heterogeneity in the state-level and all-counties samples induce an upward bias in the estimates.

However, while the contiguous-counties border-pair sample reduces the degree of the heterogeneity between counties, the specification in table 4 column 1 only corrects for the fixed county-specific and time-specific unobservable factors affecting drunk driving fatalities. As such, the unobserved time varying factors correlated with the implementation of various traffic legislations may yet be another source of endogeneity. To address this issue, we include a county-pair specific time fixed effect in the results presented in table 4 column 2. This is identified from the assumption that differences in traffic legislation within each contiguous county-pair is uncorrelated with the differences in the residual levels of driving fatalities between the counties in each pair. Under our preferred specification, we find that open container, tax rate on packaged beer, BAC limits for young drivers, and mandatory prison for first DUI violation are effective in reducing drunk driving fatalities. If these estimates are more reflective of the population parameters, it indicates that the traditional specifications are biased upwards. As such, using the traditional approaches may lead to misleading conclusions about the effectiveness of each traffic law.

In general, the contiguous-border county-pair estimates suggest that the most effective legislations in reducing drunk-driving fatalities are the non-pecuniary measures directly related to alcohol use, drunk driving or accident prevention, such as open container law, beer tax, license suspension or prison time for the first DUI incident, and alcohol limits for young drivers. This suggests that a mixture of preventative and ex-post laws can be effective in limiting the incidents of drinking and driving. Additionally, the result highlights the effectiveness of taxes on packaged beer in forcing individuals to internalize some of the negative externalities created when they drink and drive. Such excise sin taxes disincentives individuals from engaging in risky behavior that may result in traffic fatalities. In contrast, it suggests that drivers do not change their risky behavior in response to harsh pecuniary penalties. This is supported by the insignificant result found for the minimum fine.

4.2 Robustness Checks

The main result questions the validity of the estimates in the literature. While it may be easy to convince readers that any estimation procedure that fails to account for the issues of spatial heterogeneity or omitted variables would yield parameters that are biased, it is not as easy to convince them that the contiguous county-pair specification adequately addresses this issue. To make the case for the empirical design we employed in this study, we present falsification tests in this section to show the robustness of the main results. To achieve this, we focus on time periods where we expect particular laws to have a predictable impact on individual behavior. Particularly, we examine the impact of traffic legislations in preventing drunk driving fatalities during Fridays and Saturdays 12AM to 3AM, Holidays (Fourth of July, Thanksgiving and New Years Day) and rush hour periods (Monday to Thursday from 3PM to 6PM). The first and second period of interest have the most notable spikes in the level of drunk driving fatalities. During these periods, we have a highly selective sample inclusive of riskier drivers who are less likely to respond to traffic laws and more likely to be intoxicated. That is, when we restrict our attention to a time period when drivers are less likely to be responsive to traffic rules, we should find that these rules are less ineffective. For example, it is intuitive that it would take a much larger increase in the beer tax rate to dissuade the average individual from drinking during happy hour than is necessary during the normal work hours. On the other hand, during the Monday to Friday rush hour period, it is plausible to expect that the risk preferences of the average driver to be closer to our core sample. Under these conditions, it would be counterintuitive if any law(s) that was insignificant in the main specification, is now found to have an impact.

Table 5 show the results of the robustness checks for the state-level specification. In the subsamples focusing on Holidays, and Friday and Saturdays 12-3AM, more laws become statistically significant though the magnitude of the effect reduces across all laws. In the rush hour subsample, all laws were found to be statistically insignificant. As such, the conventional estimates suggest that traffic laws are more effective when considering sub-populations with a higher risk tolerance. In contrast, the contiguous county-pair result in table 6 finds that the laws became generally ineffective in curbing drunk driving fatalities in each subsample. Across all subsamples using this econometric specification, the estimated effect reduces. Also, all previously ineffective laws continue to have no impact on this phenomena. As such, while the contiguous counties econometric specification produce intuitive results across all subsamples.

The second robustness check looks at the impact of laws that are not directly related to drunk-driving on drunk-driving fatalities. This is done for the main sample and for the three sub-samples above. We also compare the performance of the conventional estimates to the contiguous-counties estimates under these demanding conditions. We expect that these laws should have no impact on drunk driving fatalities during holidays, rush hour intervals, and during Fridays and Saturdays 12-3AM. The results are presented in table 7[6]. The laws of interest in this falsification test: whether the state possesses a good graduated licensing system, bike helmet laws, law imposing a fine for seat-belt non-use, and a law which reduces damages for seatbelt non-use.

The conventional state-level specification produce implausible estimates when used to estimate

[6]All specifications in table 7 include the previous laws and the full set of controls discussed above where possible.

laws that do not directly impact drunk-driving fatalities. For example, the conventional estimates suggest that a law that reduces accidental damages for seatbelt non-use increases drunk-driving fatalities. In contrast, the contiguous counties specification produces estimates that are plausible and intuitive across all sub-samples. In general, the preferred specification show that these laws had no impact on drunk driving fatalities when assessing sub-samples with a higher share of risky drivers.

These falsification tests show that given the main results, the contiguous county-pair specification produces plausible estimates under restrictive conditions. In contrast, the traditional specification fails to consistently produce plausible estimates when used to estimate the effect of traffic laws in restrictive environments. These results support the argument that there are major concerns about the robustness of the traditional specification.

4.3 Main Threats to Causality: Migration Bias

Estimating the true effect of a policy becomes troublesome when the treatment is based on geographical location and individuals can migrate either to avoid or to be included in the treatment. In the context of our study, this would be a problem if there exists sufficient migration across state borders in response to changes to traffic legislations such that the counties within each contiguous county-pair become highly heterogeneous. Consider two contiguous counties, A belonging to state X and B belong to state Y. Let us assume that both counties are identical in terms of their observed (including their traffic laws) and unobserved characteristics. Let us suppose that state X decides to reform its traffic law system and state Y does not change its law in response to the reform in X. A researcher exploiting these policy changes to estimate the impact of seatbelt enforcement on traffic fatalities would produce coefficients affected by the migration bias if this reform induces the riskiest individuals who are most prone to accidents in county B to move to county A. This movement of individuals across the state border due to the change in policy leads to the counties no longer being identical as is required. We strongly believe that this is not a major concern in this paper due to the high direct and psychic cost associated with migration. Therefore, the share of individuals induced to relocate to nearby states due to a change in traffic rules is infinitesimal and should pose no threat to the identification framework we employ.

There is also a likelihood that the county where the fatal accident occurs and is counted in our data is different from the driver's county of residence. This only poses a problem if it is done in a systematic manner which inflates the fatality counts for one of the contiguous counties within a given border pair[7]. For example, if state X imposes a steep beer tax, this may induce some individuals to drive to the bordering county (Y) to drink. As such this could inflate the accident figures in state Y and deflate the counts in state X. Mechanically, this would make the beer tax appear more effective than it actually is.

There are two arguments that suggest that it is unlikely that this mechanism accounts for the results from the contiguous-county specification. First, to be plausible, the increased expected penalty imposed by the new legislation must offset the time and monetary cost of driving across the bor-

[7]If the reason for driving across the border is independent of the existing laws, then this does not induce a bias. For example, if an individual drives to the bordering county to visit families or for work and is involved in a drunk driving fatal crash, this would not bias the specification.

der[8]. As such, this would require very steep increases in the cost of drunk-driving associated with law changes. We do not believe this could reasonable explain the big differences observed between the estimates in tables 3 and 4. Secondly, legislations such as open container laws focus mainly on where alcohol can be consumed, and should not induce big behavioral changes such as traveling to a border state. However, the conventional estimates do not find this law to be effective, while the contiguous-counties border-pair estimates suggest that this law reduces drunk-driving fatalities[9].

As such, while we cannot rule out migration bias affecting the estimated parameters, the evidence suggest that it does not violate the main conclusions.

4.4 Policy Implications

In this section, we provide back of the envelope calculations of the effectiveness of the various laws we found to be effective in reducing driving related fatalities. To be consistent with the usual statistics documented by the National Highway Traffic Safety Administration(NHTSA), table 8 provides the actual and potential effect of traffic laws in reducing total fatalities. The assumptions utilized in our simulation mirrors those employed by the NHTSA. The actual lives saved can be interpreted as the number of lives that would have been lost if the states that enacted the particular law, had decided not to do so. This is simply calculated as the beta coefficient from panel 2 of table 4 weighted by the population in the states that enacted these laws. We only present the result for the estimates that are statistically significant. In contrast, the potential lives saved should be interpreted as the total number of lives that would have been saved had the particular law been enacted in all states.

Rows 1 to 2 of this table provides yearly average estimates over the period 1986-2005. Columns 1 to 4 shows the effect of each law on the actual number of lives saved for all states who enacted the law and the potential lives that could have been saved if the law was enacted as a federal mandate[10]. The simulation suggest that on average, the four laws have saved about 3000 lives yearly. However, there are a number of lives that were not saved because some of these laws are not adopted universally[11]. As such, the effective laws only saved 48 percent of the lives that could be saved from drunk-driving accidents with available legislative tools. With drunk-driving fatalities averaging about 13,400 annually over the period, this suggests that these laws could have prevented approximately 24 percent of these fatal accidents if they were uniformly utilized over the sample period.

The evidence also suggests that there are gains to be made from adopting a uniform tax rate on packaged beer. If the federal government had imposed a mandatory minimum of 20 cents per gallon in 1986 (the average alcohol tax in 1986), they could have saved about 135 more lives annually[12]. This small increase in the beer tax rate over the period would have reduced the level of drunk driving fatalities by approximately 1 percent annually.

[8]There is also an informational cost for individuals to determine the laws in the bordering state.

[9]This is also supported by the plausible estimates we find in table 7 when we focused on laws unrelated to the cost of consuming alcohol.

[10]These are presented as yearly averages over the sample period. The total is a multiple of 19.

[11]The zero tolerance law was the only law that was adopted universally before the end of the sample period.

[12]Calculated as the product of the affected population (per hundred thousand) with a beer tax below 0.2 over the period, the average gap between the actual beer tax and 0.2 and the estimated impact of the beer tax on traffic fatalities.

Overall, the simulations indicate that there is still room to use existing laws to reduce drunk driving fatalities. However, these laws must be further evaluated against any social cost they impose.

5 Conclusion

In this paper, we present new evidence on the effectiveness of traffic legislation on the rate of drunk-driving fatalities. To robustly evaluate this issue and to assess the empirical limitations of previous studies, the contiguous county-pair approach is employed. We argue that this approach offers significant insight into the causal effect of each policy because of our ability to directly address the issue of spatial heterogeneity that has biased previous estimates. Additionally, contiguous-counties border-pair level analysis benefits from several empirical and efficiency advantages, that state-level panels and time series analysis do not.

Our findings suggest that the the most effective legislations in reducing drunk-driving fatalities are the non-pecuniary measures directly related to alcohol use, drunk driving or accident prevention; such as a open container law, beer tax, license suspension or prison time for the first DUI incident and alcohol limits for young drivers. Consequently, these laws can be employed as policy tools by decision-makers to address the widespread public health issue of motor vehicle fatalities. A simple simulation suggests that policymakers could have prevented about 24 percent of these fatal accidents if these laws were uniformly enacted in 1986.

While this study does not solve all the problems in the literature and may have several limitations of its own, we are optimistic that it will be insightful to future efforts examining this question. In order for policymakers to implement policy aimed at combating this phenomenon, there is a need for robust studies exploring the best practices that are most effective. This study makes a single step in arriving at these solutions.

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6 Appendices

Table 3: Impact of Traffic Laws on Drunk Driving Fatalities (Conventional)

	[1]	[2]	[3]
BAC Limit of .08 g/dL	-0.44***	-0.37	-0.36
	(0.21)	(0.40)	(0.41)
Underage BAC Limit of .02 g/dL	-0.39*	-0.57*	-0.55*
	(0.20)	(0.29)	(0.30)
Beer Tax Rate	-0.94	-3.58	-3.69
	(1.81)	(2.87)	(2.93)
Open Container Law	-0.30	-0.50	-0.50
	(0.30)	(0.46)	(0.47)
Keg Registration	0.15	-0.40	-0.39
	(0.26)	(0.39)	(0.41)
1^{st} DUI: Min Fine	-0.00008	-0.0002	-0.0002
	(0.0005)	(0.0009)	(0.001)
1^{st} DUI: Community Service	-0.098	-0.23	-0.23
	(0.17)	(0.30)	(0.31)
1^{st} DUI: License Suspension	-0.31	-0.36	-0.35
	(0.41)	(0.47)	(0.49)
1^{st} DUI: Mandates Prison	-0.20	-0.48	-0.48
	(0.22)	(0.29)	(0.30)
Obs	879	56,088	56,088
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	No	No
State-Time Trend	No	Yes	No
County FE	No	No	Yes
R-squared	0.81	0.06	0.26

Table 4: Impact of Traffic Laws on Drunk Driving Fatalities (Contiguous)

	[1]	[2]
BAC Limit of .08 g/dL	-0.33	0.46
	(0.21)	(0.31)
Underage BAC Limit of .02 g/dL	-1.14***	-1.40***
	(0.23)	(0.33)
Beer Tax Rate	-16.24***	-9.09**
	(2.49)	(3.55)
Open Container Law	-0.84***	-1.02***
	(0.21)	(0.31)
Keg Registration	-0.13	0.34
	(0.27)	(0.38)
1^{st} DUI: Min Fine	-0.001*	0.0005
	(0.0007)	(0.001)
1^{st} DUI: Community Service	-0.31	-0.24
	(0.20)	(0.29)
1^{st} DUI: License Suspension	-0.16	-0.49*
	(0.19)	(0.27)
1^{st} DUI: Mandates Prison	-0.39*	-0.73**
	(0.21)	(0.29)
Controls	Yes	Yes
County FE	Yes	Yes
Year FE	Yes	No
County-Pair Year FE	No	Yes
R-squared	0.28	0.64
Obs	38,128	38,128

Table 5: Robustness Checks-Impact of Traffic Laws on Drunk Driving Fatalities (State)

	Holidays[1]	Fri and Sat 12-3AM[2]	Rush Hour (Evening)[3]
BAC Limit of .08 g/dL	-0.07**	-0.033	-0.023
	(0.031)	(0.029)	(0.016)
Underage BAC Limit of .02 g/dL	-0.09***	-0.014	0.025
	(0.032)	(0.03)	(0.017)
Beer Tax Rate	-0.31*	-0.62***	-0.041
	(0.18)	(0.15)	(0.12)
Open Container Law	-0.07**	-0.048*	-0.009
	(0.03)	(0.029)	(0.016)
Keg Registration	-0.009	-0.008	0.019
	(0.035)	(0.033)	(0.019)
1^{st} DUI: Min Fine	-0.00003	-0.00006	0.000007
	(0.00008)	(0.00007)	(0.00004)
1^{st} DUI: Community Service	-0.015	-0.04	0.006
	(0.029)	(0.027)	(0.016)
1^{st} DUI: License Suspension	-0.045	-0.006	-0.022
	(0.03)	(0.03)	(0.016)
1^{st} DUI: Mandates Prison	-0.07**	-0.008	-0.02
	(0.03)	(0.029)	(0.016)
Obs	879	879	879
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
R-squared	0.55	0.62	0.45

Table 6: Robustness Checks-Impact of Traffic Laws on Drunk Driving Fatalities (Contiguous)

	Holidays[1]	Fri and Sat 12-3AM[2]	Rush Hour (Evening)[3]
BAC Limit of .08 g/dL	0.22	0.075	-0.073
	(0.16)	(0.11)	(0.076)
Underage BAC Limit of .02 g/dL	-0.31**	-0.10	0.088
	(0.13)	(0.16)	(0.066)
Beer Tax Rate	0.82	-2.19	-1.90
	(1.27)	(1.45)	(1.18)
Open Container Law	0.0858	-0.12	0.045
	(0.12)	(0.15)	(0.074)
Keg Registration	-0.14	0.065	0.064
	(0.15)	(0.16)	(0.087)
1^{st} DUI: Min Fine	0.0002	-0.00065	0.00044
	(0.0006)	(0.0005)	(0.0003)
1^{st} DUI: Community Service	-0.117	-0.12	0.008
	(0.11)	(0.11)	(0.07)
1^{st} DUI: License Suspension	-0.018	0.18	-0.15***
	(0.091)	(0.14)	(0.058)
1^{st} DUI: Mandates Prison	-0.028	-0.12	-0.022
	(0.15)	(0.11)	(0.06)
Obs	38,128	38,128	38,128
Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	No	No	No
County-Pair Year FE	Yes	Yes	Yes
R-squared	0.54	0.54	0.54

Table 7: Robustness Checks, Non-Alcohol Related Laws

	Conventional (State)	Contiguous-Counties
Universe		
Good GDL	-0.30*	-0.25
	(0.17)	(0.35)
Bike Helmet Laws	-0.32*	-0.59*
	(0.167)	(0.35)
Seat-Belt Fine	-0.32*	-1.58*
	(0.167)	(0.74)
Reduced Damages, SB No-Use	.38	0.76
	(0.36)	(0.68)
Holidays		
Good GDL	-0.01	0.09
	(0.17)	(0.13)
Bike Helmet Laws	-0.07**	0.15
	(0.033)	(0.13)
Seat-Belt Fine	0.003	0.21
	(0.07)	(0.25)
Reduced Damages, SB No-Use	0.16***	-0.001
	(0.058)	(0.26)
Rush Hours		
Good GDL	-0.02	0.03
	(0.02)	(0.07)
Bike Helmet Laws	-0.01	-0.13
	(0.02)	(0.10)
Seat-Belt Fine	-0.03	0.002
	(0.04)	(0.17)
Reduced Damages, SB No-Use	0.094***	0.09
	(0.031)	(0.14)
Fri & Sat 12-3AM		
Good GDL	-0.03	0.025
	(0.03)	(0.12)
Bike Helmet Laws	-0.03	0.05
	(0.03)	(0.14)
Seat-Belt Fine	-0.03	-0.44*
	(0.07)	(0.26)
Reduced Damages, SB No-Use	-0.043	-0.16
	(0.054)	(0.27)
Controls	Yes	Yes
State FE	Yes	No
Year FE	Yes	No
County-Pair Year FE	No	Yes
County FE	No	Yes

Table 8: Effectiveness of Traffic Legislations in Reducing Motor Vehicle Accidents

	[1] License Suspension (DUI)	[2] Mandatory Prison Time (DUI)	[3] Open Container	[4] BAC Limit of 0.02 (Underage)
Total Lives Saved	785	460	1,576	227
Pot Lives Saved	1,297	1,932	2,700	371
Lives Saved (%)	60.5	24	58	61