Journal of Crime and Justice

ISSN: 0735-648X (Print) 2158-9119 (Online) Journal homepage: https://www.tandfonline.com/loi/rjcj20

Suspended licenses, suspended lives: the impact of drug-related driver's license suspensions on traffic fatalities

Tracy Sohoni, Richard Stringer & Sylwia Piatkowska

To cite this article: Tracy Sohoni, Richard Stringer & Sylwia Piatkowska (2019): Suspended licenses, suspended lives: the impact of drug-related driver's license suspensions on traffic fatalities, Journal of Crime and Justice

To link to this article: https://doi.org/10.1080/0735648X.2019.1690549

Published online: 18 Nov 2019.

JOURNAL OF CRIME AND JUSTICE
https://doi.org/10.1080/0735648X.2019.1690549

Suspended licenses, suspended lives: the impact of drug-related driver's license suspensions on traffic fatalities

Tracy Sohoni[a], Richard Stringer[b] and Sylwia Piatkowska[c]

[a]Department of Sociology and Criminal Justice, Old Dominion University, Norfolk, VA, USA; [b]Department of Criminal Justice, Kennesaw State University, Kennesaw, GA, USA; [c]College of Criminology and Criminal Justice, Florida State University, Tallahassee, FL, USA

ABSTRACT

As part of the War on Drugs, many states passed legislation revoking the driver's licenses of individuals convicted of drug crimes, even for drug offenses in which no driving offense occurred. These restrictions were supported at the national level, with federal legislation in 1992 requiring states to either pass laws suspending the license of anyone convicted of a drug offense for at least 6 months, or risk losing 10% of certain federal highway funds (although states could also formally 'opt out'). Reentry advocates contend that these restrictions on driver's licenses provide a hardship to individuals as they reenter society, making it more difficult for them to maintain stable employment, and placing additional stress on families that shoulder the responsibility of providing transportation. In response, some states have recently chosen to end restrictions on driver's licenses for individuals that are convicted of non-driving related drug offenses. A critical part of this conversation, however, is whether or not these laws have any safety benefits in terms of traffic accidents. We conduct a fixed effects analysis to determine the impact of these laws on both drug-related traffic fatalities and all traffic fatalities.

ARTICLE HISTORY
Received 8 April 2019
Accepted 30 October 2019

KEYWORDS
Social policy fixed effects analysis; crash fatalities; collateral consequence laws; license suspensions

Introduction

The 1990s saw the height of the 'Get Tough on Crime' and the 'War on Drugs' movements in the United States (Mauer 2006). During this time not only did the country see an increase in harsh sentences for crimes, but it also saw an increase in the number of 'collateral consequence' sanctions for criminal actions, that is, sanctions imposing civil restrictions on offenders that are not decided by a judge, but rather are incurred automatically in addition to the criminal sentence (Chesney-Lind and Mauer 2003; Petersilia 2003; Thompson 2008; Travis and Visher 2005). One such collateral consequence is the revocation or suspension of a driver's license in response to a drug conviction. As part of the War on Drugs, many states passed legislation revoking or suspending the driver's licenses of individuals convicted of drug crimes, even for drug offenses in which no driving-related offense occurred, such as drug possession or drug dealing (Marcus 2003; Samuels and Mukamal 2009). These restrictions were supported at the national level, with federal legislation in 1992 requiring states to either pass laws suspending the license of anyone convicted of a drug offense for at least 6 months, or risk losing 10% of federal highway funds. State legislators also had the option of formally 'opting out' of the ban, but the federal legislation in essence required states to formally consider adopting the ban (Marcus 2003; Samuels and Mukamal 2009).

CONTACT Tracy Sohoni tsohoni@odu.edu Department of Sociology and Criminal Justice, Old Dominion University, 6000 Batten Arts and Letters, Norfolk, VA 23529, USA

Reentry advocates contend that restrictions on driver's licenses as a result of a drug-related offense provide a hardship to individuals as they reenter society, making it more difficult for them to maintain stable employment, and placing additional stress on families that shoulder the responsibility of providing transportation (Marcus 2003; Samuels and Mukamal 2009). In response, some states have recently chosen to end restrictions on driver's licenses for individuals that are convicted of non-driving related drug offenses (Aiken 2016; Beitsch 2017), and many states have passed formal resolutions objecting to the federal legislation, as documented on the Legal Action Center's Roadblocks to Reentry website. A critical part of this conversation, however, is whether or not these laws have any safety benefits in terms of traffic accidents. Presumably, the purpose of these laws is to prevent drivers that may be at an increased risk of causing drug-related traffic accidents from driving. To date, however, there has been no analysis as to whether or not the passage of these laws resulted in a corresponding drop in traffic-related fatalities.

In order to address this issue, we examine traffic fatalities between 1985–2016 to determine whether states that imposed laws revoking or suspending the driver's licenses of individuals convicted specifically of non-driving related drug offenses saw reductions in traffic fatalities. We focus on non-driving related drug offenses as these are the offenses that have been focused on by reentry advocates, and as they have a more tenuous connection to potential traffic fatalities than do driving-related drug offenses (such as driving while intoxicated). In order to examine this relationship, we conduct a fixed-effects analysis comparing states that adopted restrictions on non-driving drug offenses, to those that did not have laws suspending licenses for non-driving related drug offenses, to determine the impact of these laws on both drug-related traffic fatalities, and all traffic fatalities.

Literature review

It is estimated that millions of individuals have had their licenses suspended for non-driving related drug offenses (Marcus 2003). Given that roughly 90% of Americans report driving to work (Dews 2013), the suspension of an individual driver's license can cause significant hardship to individuals trying to maintain employment. Surveys and interviews have found that suspension of driver's licenses can result in job loss as well as difficulty in finding a new job, and among those that find a new job, lower income (Carnegie 2007; Zimmerman and Fishman 2001).[1] Furthermore, there is the concern that collateral consequence laws in general disproportionately impact poor and minority citizens (Chin 2002; Pinard 2010). Given that it is well documented that poor and minority are disproportionately likely to be arrested for drug offenses (Tonry 2011; Alexander 2012), it stands to reason that these policies suspending the driver's licenses of individuals convicted of drug crimes will be disproportionately likely to impact the poor and minorities.

Research on traffic safety has not traditionally been included in criminological research (for exceptions see Chamlin 2018; Chamlin and Sanders 2018; Stringer 2016, 2018). Conversely, traffic safety scholars have rarely examined drug policies in their research, choosing instead to focus on the role of alcohol due to its prevalent involvement in crashes compared to illicit drugs. However, given that reducing traffic fatalities was a key goal of state laws restricting the driver's licenses of individuals convicted of non-driving related drug offenses, and of the federal legislation requiring states to respond to a call to pass such legislation or risk losing highway funding, it is of great interest to see if these laws had the intended effect of lowering traffic fatalities by removing risky drivers from the roadway (Marcus 2003).

The implementation of laws to suspend the driver's licenses of individuals that committed non-driving related drug crimes was meant to improve road safety through deterrence. Deterrence theory proposes that individuals will consider the costs of any given action, particularly the certainty and severity of a potential punishment, and this will guide their choice as to whether to engage in the activity (Pratt et al. 2006). These laws were meant to engage in specific deterrence of those that had their driver's license suspended, as individuals would refrain from driving as they would incur additional penalties if they were found to be driving with a suspended license (Marcus 2003; Stafford and Warr 1993). The law would also trigger general deterrence by increasing the punishment of drug crimes and thus should decrease the amount of illegal drug activity (Marcus 2003). By reducing the amount of drug offending, this would also reduce the potential number of individuals that might drive under the influence of drugs.

Research on traffic safety generally finds that enforcing safer driving patterns through increasing potential penalties is effective in reducing traffic fatalities. Traffic safety researchers have examined a host of state policies and legislation geared toward increasing traffic safety such as speed limits (Ossiander and Cummings 2002; Vernon et al. 2004), seatbelt laws (Adams, Cotti, and Tefft 2015; Freeman 2007), motorcycle helmet laws (Mertz and Weiss 2008), distracted driving laws (Ibrahim et al. 2011) and graduated licensing programs (McCartt et al. 2010), generally finding that penalties are effective in deterring dangerous behaviors, thus reducing traffic crash fatalities.

Of particular interest is whether legislation targeted toward reducing impaired driving can also be effective in reducing traffic crash fatalities. Research on the impact of legislation aimed at impaired driving has largely focused on alcohol-impaired driving, and some studies do support the idea that legislation around alcohol can reduce automobile crashes. For instance, Fell et al. (2009) found that several laws targeting underage drinking (including use and lose laws, and zero-tolerance laws) resulted in a reduction in traffic crash fatalities, while Lavoie et al. (2017) found that increases in alcohol sales tax resulted in a reduction in alcohol-positive drivers.

Studies looking at changes in the per-se blood alcohol concentration (BAC) level have had mixed findings. Foss, Stewart, and Reinfurt (2001) found that a lower BAC level had no impact on traffic crash fatalities in North Carolina. However, Chamlin (2018) found that while reducing the BAC limit to 0.8 in New Jersey had no effect on total or driver fatalities, it did have a negative and lasting effect on passenger fatalities. Chamlin and Sanders (2018) found the legislative change in per-se BAC did not have a direct effect on fatal crashes, but that the legislation lead to a decrease in total vehicle miles traveled which then lead to reductions in alcohol-related crashes. Fell and Voas (2014) argue that lowering BAC levels to .05 should have a substantial impact since those at .05-.08 have a higher risk of accidents than those at a .000 level, and indeed the National Transportation Safety Board (NTSB) has recommended lowering the per-se Blood Alcohol Limit (BAC) limit to .05. However, Stringer (2016) found that drivers with low BAC levels have a very low involvement in fatal automobile crashes (though higher than those with a .000 BAC), and suggests that investments might see higher returns in other areas. Furthermore, Stringer (2018) notes that increases in DUI arrests result in lower traffic crash fatalities, but notes that it is not a linear effect, but rather there are diminishing returns. In whole, these findings suggest that legislative changes can reduce impaired driving; however, there are caveats to these findings.

Perhaps most relevant for our current research findings indicate that the implementation of legislation for administrative license suspensions (license suspensions which are enacted upon arrest for DUI) are related to decreases in fatal automobile crashes (Voas, Tippetts, and Fell 2000; Ulmer, Shabanova, and Preusser 2001; Wagenaar and Maldonado-Molina 2007). Interestingly, Wagenaar and Maldonado-Molina (2007) found that policies that allow for pre-conviction such as administrative license suspensions are effective at reducing crashes, but that legislation for post-conviction suspensions have no effect on crashes. The lack of an effect for post-conviction suspensions may be due to low conviction rates or lack of enforcement, as well as the reduced celerity of the sanction. Ulmer (2001) focused on license suspension among youth that were found driving while intoxicated, or in possession of alcohol underage or of a controlled substance, finding that those that had their license suspended were less likely to be involved in a subsequent crash compared to those that did not have their license suspended.

It is important to note that the suspension of one's driver's license is not an absolute mechanism of prevention from driving such as an incarceration, vehicle impoundment, or ignition interlocks, and that it acts merely to deter individuals from driving. In fact, studies show that many offenders continue to drive to some extent after their license is suspended (McCartt, Geary, and Berning 2003; Ross and Gonzales 1988). Therefore, it is possible that license suspensions may have a trivial effect on crashes if it does not prevent the person from driving. However, evidence indicates that drivers with suspended licenses do drive less frequently and drive much more carefully when they choose to drive (Blomberg, Preusser, and Ulmer 1987; McCartt, Geary, and Berning 2003; Voas and Tippetts 1996; Voas, Tippetts, and Taylor 1998), most likely in an effort to avoid police detection and a stop. Thus, even if offenders continue to drive post suspension, reductions in crashes could still be prevalent if these suspensions lead to less or more cautious driving.

Although much of the research on impaired driving, and on the impact of suspended licenses, has focused on impairment related to alcohol, in recent years, concerns of possible increases in 'drugged driving' in response to state marijuana legalization have led to several studies that have explored drug use and its involvement with crashes. Although some studies have found no increased rate of crashes among those driving under the influence of drugs, others have found that driving under the influence of drugs is associated with an increased rate of crashes compared to those driving sober, although this risk is markedly smaller than the risk of driving under the influence of alcohol (Compton and Berning 2015; Li, Brady, and Chen 2013; Romano and Pollini 2013; Romano et al. 2014; Romano and Voas 2011). Only a few studies have examined drug policy, specifically marijuana decriminalization, and crashes, and the results have been mixed with some finding no differences in traffic crash fatalities between criminal and decriminalized policies (Aydelotte et al. 2017; Hansen, Miller, and Weber 2018; Pollini et al. 2015) and others indicating significant differences (Hamzeie et al. 2017; Masten and Guenzburger 2014; Salomonsen-Sautel et al. 2014).

Although we have focused on the impact of legislation on traffic crash fatalities, it is important to also take into account the impact of factors beyond legislation in crashes. For example, negative economic indicators, such as high unemployment and/or low economic growth are associated with lower crash fatalities (Evans and Graham 1988; Wagenaar 1984), while urbanization is associated with lower rates of crash fatalities (O'Neill and Kyrychenko 2006). In fact, O'Neill and Kyrychenko (2006) find that the socioeconomic and demographic factors of percentage living in urban areas, median household income, percentage with a college degree, educational expenditures, and mean vehicle age account for roughly 60–70% of the variance in fatal crashes between states.

This project seeks to ascertain whether laws suspending driver's licenses of individuals convicted of non-driving related drug offenses resulted in lower rates of traffic fatalities. In so doing, this project integrates literature from both the criminological literature on drugs, drug policy, and the harms associated with drug use with the literature on traffic safety in order to make a substantial contribution to both. While both the criminological literature and traffic safety literature are well-developed areas of scholarship, a substantial void is present both with regard to the evaluation of drug policies (e.g., drug conviction license suspensions) and drug-related harms (e.g., automobile crashes). Additionally, since criminologists and traffic safety scholars rarely consider each other's research (DeMichele, Lowe, and Payne 2014), the integration of the extant research from both fields has the potential to greatly advance both as seen in recent scholarship (see Chamlin 2018; Chamlin and Sanders 2018; Stringer 2016, 2018).

Data and methods

The passing of laws suspending the driver's license of individuals convicted of *non-driving related* drug offenses assumes that individuals that are guilty of drug crimes are also more likely to drive while under the influence of illicit substances. However, just as many people that consume alcohol do not drive under the influence, it is possible that people that possess or sell illicit substances do not drive under the influence. This research seeks to answer whether these laws resulted in lower rates of fatal crashes. Presumably, if the laws contribute to public safety, we should expect to see a decrease in drug-related crash fatalities specifically.

To address the research questions outlined above, we utilize pooled cross-sectional time-series data collected from all 50 states in the United States for the 1985–2016 period. These data were compiled from several government sources: the National Highway Traffic Safety Administration

(NHTSA, 2018), the Federal Highway Administration (Federal Highway Administration 2018), the U.S. Census Bureau (U.S. Census Bureau 1980-2017), the FBI's Uniform Crime Reports (U.S. Department of Justice 2017, 2018), the Bureau of Justice Statistics (Bureau of Justice Statistics 2012, 2013-2016), the Bureau of Labor Statistics (Bureau of Labor Statistics 2018), the Surveillance Report of the National Institute of Alcohol Abuse and Alcoholism (Haughwout and Slater 2018), and the Economic Research Service (Butler and Beale 2003). All data are publicly available.

Two dependent variables are considered in the present study. The first dependent variable is the total crash fatality rate. In keeping with previous work (see, e.g.,, Tippetts et al. 2005), we constructed this measure by dividing the total number of crash fatalities in a state by total vehicle miles travelled (VMT), and multiplied the outcome by 10,000. The second dependent variable is the drug crash fatality rate (crash fatalities related to the usage of drugs), which was constructed by dividing the total number of drug crashes in a state by VMT and multiplying the outcome by 10,000. Both crash measures were obtained through NHTSA (2018) and VMT was from the Federal Highway Administration (2018).

In order to construct the variable to measure state laws related to driver's license suspensions for non-driving related drug offenses, we conducted searches using Westlaw on laws related to driver's license suspensions. We also referenced the Legal Action Centers's Roadblocks to Reentry report (Samuels and Mukamal 2009) as well as Google searches to help identify relevant laws. We then cross-referenced our findings with PrisonPolicy.org's report (Aiken 2016) as an additional check. Notably, the implementation period of the suspension law diverges across the U.S. states, with some of the states implementing the law as early as in the late 1980s (i.e., New Jersey, Louisiana), and others implementing it in the second half of the 1990s (i.e., Colorado, Iowa). Moreover, in some of the states, the suspension law has not been implemented at all (i.e., Alaska, Idaho), whereas in the others states it has already been repealed (i.e., Colorado, Indiana; see Table A1 for more detailed information on the implementation law and time period covered in each state). States were coded as suspending drivers' licenses for non-driving related drug offenses when they suspended driver's licenses for convictions of a drug offense other than driving under the influence.[2] This generally included being found in possession of a drug, or of selling drugs. By determining the year, a law was instated, as well as the year a law was repealed (if it was repealed) we were able to assign states a code of '1' for each year they had a law suspending driver's licenses for non-driving related drug offenses. If the law was repealed, states were coded as 0 for each subsequent year. States that did not have laws suspending licenses for non-driving related drug offenses, including those in which suspension only occurred for a driving-related drug or alcohol offense, were coded as '0' for each year. Accordingly, our focal independent variable, the 'Suspension Law', is coded as a dichotomous variable with a value of 1 for the years when the suspension law is in effect, and 0 for otherwise. To ensure correct temporal ordering, this measure, along with the remaining predictor variables, was lagged 1 year. Thus, we expect the effect of Suspension Law and other independent variables on drug and total crash fatality rates to take place in the following year, at time $t + 1$, rather than in the same year.

In order to test for the possibility that other factors that have been shown to have an effect on traffic crash fatalities may also be related to the likelihood of states passing these laws, we also include measures for urbanization (the percentage of residents in urban areas), the percentage of young population (the percentage of population between 15 and 24 years of age), alcohol consumption per capita, and highway funds' rate. For instance, urbanization, percentage young population, and alcohol consumption may all be associated with the perception that drug offending is a greater problem, thus making it more likely that laws restricting the driver's licenses of drug offenders are passed. Similarly, states that have higher levels of highway funding may also systematically differ in their disposition towards passing these laws. As discussed previously these factors have been shown to be related to crash fatalities, and thus it is important to include them in our model (see Tippetts et al. 2005; Stringer 2018; Voas, Tippetts, and Fell 2000). The data on the percentage of young population were available to download from the Census Bureau (U.S. Census Bureau 1980-2017). The data on alcohol consumption per capita were gleaned from the Surveillance

Report of the National Institute of Alcohol Abuse and Alcoholism (Haughwout and Slater 2018). Data for highway funds were obtained from the Federal Highway Administration (2018) and data on urbanization came from the Economic Research Service (Butler and Beale 2003).

We also adjust for economic conditions given the significant effect economic conditions have been shown to have on traffic crash fatalities (O'Neill and Kyrychenko 2006), and the possibility that economic conditions may impact legislators' willingness to pass or maintain these laws. To assess their effects, we constructed an index of economic disadvantage by summing z-scores for the unemployment rates and poverty rates (the percentage of population below the poverty line). The two measures are positively correlated, and on conceptual grounds, both indicate economic adversity (r = .51; Cronbach's alpha = .58). The data source for the unemployment rates was the Bureau of Labor Statistics (Bureau of Labor Statistics 2018). The data on the poverty rate were obtained from the Census Bureau (U.S. Census Bureau 1980-2017, Current Population Survey).

Finally, to capture the plausible effect of punitive environment, we adjusted for violent crime rates, drug arrest rate, and incarceration rates in the present study as states that pass laws suspending the driver's licenses of offenders with non-driving related drug offenses may also pursue other more punitive policies, such as increased drug arrest and increased incarceration rates. These increased arrests and incarceration may remove riskier drivers from the road (as suggested by Gottfredson and Hirschi's discussion of analogous behaviors (1990)) thus reducing the rates of crash fatalities independently of the impact of suspended driver's licenses. This is important because impaired drivers are not limited to DWI (Driving While Impaired) behavior but also engage in global criminality (see Gould and Gould 1992). The data on violent crime rates and drug arrest rates were taken from the FBI's Uniform Crime Reports (U.S. Department of Justice 2017, 2018, 2018b), whereas data on the incarceration rates were available to download from the Bureau of Justice Statistics (Bureau of Justice Statistics 2012, 2013-2016).

The analysis of the impact of legislation on outcome measures such as crash fatalities is frequently estimated using ARIMA interrupted time-series modeling (Box and Tiao 1975; Campbell 1969; Chamlin 2018; McCleary, McDowall, and Bartos 2017; Ross and McCleary 1983). The ARIMA interrupted time-series modeling has an advantage over panel models because it precisely identifies the lag structure between variables, systematically accounting for seasonal variation, thus achieving a high degree of reliability in the estimation of the model parameters (Chamlin 1988; McCleary et al. 1980). To that end, the ARIMA modeling is beneficial because it controls for potentially confounding maturation effects and other stochastic processes while allowing making inferences about the causal effects of the particular events (McDowall et al. 1980).

Nevertheless, we argue that the fixed-effects pooled time-series cross-sectional technique is well suited for the present study. Compared to the ARIMA models, pooled time-series technique is more flexible in terms of assumptions required to identify the model (Greenberg and Kessler 1982; see Chamlin 1988). While the ARIMA model is generally limited to a single series, panel analyses combine time series for several cross-sections, giving a higher variability of data as compared to simple time series or cross-section design research. To that end, the fixed-effects pooled time-series cross-sectional technique is suitable given that the data at hand cover multiple states but uses annual data and so have a relatively low number of observations required in ARIMA models.[3]

The pooled time-series technique allows for greater incorporation of many additional variables as compared to the ARIMA models (Chamlin 1988). As Brandt and Williams (2007, 7) emphasized, the ARIMA model 'ignores the fact that some of the variables in the model may help to proxy dynamics in the others.' This may lead to a severe overfitting of the data. In contrast, including variables in the separate ARIMA models may result in insufficient estimates. Chamlin (2018), for instance, used Ordinary Least Square regression models rather than ARIMA models to examine the causal influence of indicators of target availability and capable guardianship, along with BAC limit reductions, on the volume of deaths resulting from motor vehicle accidents within Pennsylvania using annual data from 1947 through 2014. As Chamlin (2018, 4) pointed out 'if one wants to compare the relative effects of the implementation of some legislative initiative and alternative explanations for some outcome series of interest, then multivariate time series are preferable.'

As noted earlier, in addition to the suspension law, a number of additional variables were incorporated in the present study for the reasons discussed above. The inclusion of these variables is guided by our inquiry of whether it is really about the laws themselves, or is it that states that are more likely to pass those laws also have other characteristics (i.e., certain arrest or incarceration rates) that make have lower traffic fatalities.[4] This called for using fixed effect pooled time-series analytic technique as the suitable modeling strategy.[5]

To take advantage of the data structure, our analyses utilized fixed effects within-state regression (Beck 2001; Wooldridge 2002). To ensure the robustness of our findings, two types of models were estimated:

$$C_{it} = \beta_{i,0} + \beta_{t,0} + \sum_k \beta_{i,k} X_{k,i,(t-1)} + \varepsilon_{i,t} \tag{1}$$

where C is the crash fatality rate for state i in year t, βi,0 is the fixed effect for the state i, βt,0 is the fixed effect for the year t, Xk,i,(t-1) is the kth independent variable for state i in the previous year, βi,k is the coefficient on independent variable k for state i, and εi,t is stochastic error for state i in year t.

$$C_{it} = \beta_{i,0} + \beta_{i,1} + \sum_k \beta_{i,k} X_{k,i,(t-1)} + \varepsilon_{i,t} \tag{2}$$

where C is the crash fatality rate for state i in year t, βi,0 is the fixed effect for the state i, βi,1 is the crash fatality rate trend for state i, Xk,i,(t-1) is the kth independent variable for state i in the previous year, βi,k is the coefficient on independent variable k for state i, and εi,t is stochastic error for state i in year t. The crash fatality rate is autoregressive around linear trend $\varepsilon_{it} = \rho_i + \varepsilon_{it-1} + \omega_{it}$.

The results from the Hausman test preferred fixed effects specification for the estimated models. Accordingly, in both equations, we control for all unknown and/or unmeasured state characteristics that are constant over time with fixed effects for the states. Yet, the two equations differ in their treatment of time. In the first model, we adjust for time by removing unknown and/or unmeasured between-year differences in total and drug crash fatality rates that are constant over states. In the second model, we control for time by modeling the dynamics of crash fatality rate for each state. These two strategies are widely employed to adjust for the unmeasured sources of trends (Greenberg 2014), and yet each of them has its own advantages and disadvantages. As Greenberg (2013) points out, the benefit of the first strategy is its non-parametric form, wherein the precise temporal dependence of the omitted variables does not have to be specified in the model. This may take place at the expense of utilizing many degrees of freedom, however. In contrast, the second model preserves a larger number of degrees of freedom; nevertheless, it may provide a worse fit. Accordingly, we estimate the two types of models depicted above.

Finally, while the results from early diagnostics revealed that nonstationarity was not an issue in the present analysis, heteroscedasticity, and contemporaneous correlation of error were found to be present in the pooled, state-level data set employed. Accordingly, all models were estimated with Generalized Least Squares using panel-corrected standard errors (PCSE) to correct for these problems (Beck 2001; Beck and Katz 1995). Incorporating these adjustments is critical given that the pooled data violate the spherical assumption of Ordinary Least Square regression. All models were estimated in the econometrics software, Eviews 9.

The total number of cases for total and drug fatality rates was equal to 1600 and 1409, respectively. These numbers have been reduced in the regression models due to missing values recorded for the some of the predictor variables (drug arrest rates and incarcerations rates; see also Table 1). The final tally resulted in 1530 country/year combinations in Models 2 and 4 in Table 3, and 1345 country/year combinations in Models 2 and 4 in Table 4.

Table 1. Descriptive statistics for variables used in the analysis.

Variables	*N*	Mean	SD	Minimum	Maximum
Dependent variable					
Total crash fatalities (per 10,000 VMT)	1600	148.33	50.88	52.34	392.23
Drug crash fatalities (per 10,000 VMT)	1409	6.14	6.67	0	40.92
Independent variable					
Suspension law	1600	.33	.47	0	1
Control variables					
Drug arrest rate (per 100,000 pop.)	1533	428.14	247.94	0	3881.71
Violent crime rate (per 100,000 pop.)	1600	440.91	220.50	47	1244.3
Alcohol consumption per capita	1600	2.38	.52	1.19	4.99
Economic disadvantage	1600	0	1.74	−4.02	6.72
% of young population	1600	14.48	1.40	11.54	25.24
Hwy fund rate (per 100,000 population)	1600	13,978.72	10,870.55	2830.26	97,979.13
Incarceration rate	1595	375.95	203.82	53	1420
% of urbam population	1600	.70	.20	.13	1.00

Results

Table 1 displays the descriptive statistics of the variables used in the analysis. The results reveal that the mean of total crash fatalities per 10,000 VMT was 148.33 with a standard deviation of 50.88, whereas the mean of drug crash fatality rate was 6.14 per 10,000 VMT with a standard deviation of 6.67. This indicates that there is a great deal of variation in both total crash fatalities and drug crash fatality rate across the United States. The univariate analysis also revealed that drug crash fatality rate was highly skewed (skewness = 1.86, kurtosis = 7.05). Accordingly, we applied a log transformation to this measure to reduce its skewness. With a natural logarithm, the mean of drug crash fatality rate was 1.34 with a standard deviation of 1.12.[6]

Table 1 further reveals a great deal of variation with respect to some of the independent variables. The mean of drug arrest rate, for instance, was 428.14 with a standard deviation of 247.94, whereas the mean of highway fund rates (per 100,000 population) was 13,978.72 with a standard deviation of 10,870.55. All two variables, drug arrest rate and highway fund rate, were found to be right-tailed in the univariate analysis. Accordingly, we converted these variables into a natural logarithm, which resulted in a mean drug arrest rate of 5.91 with a standard deviation of .61, and a mean highway fund rate of 9.36 with a standard deviation of .56. Finally, Table 1 shows that our focal independent variable, Suspension Law, has a mean of .33 with a standard deviation of .47, indicating that for 33% of the time points across the 50 States, Suspension Laws were in place.

Table 2 demonstrates the bivariate correlations between the variables used in the present study. The results presented in this table reveal that our focal independent variable, the Suspension Law, is significantly associated with the total crash fatality rate ($r = -.18$; $p.<.001$) in the theoretically predicted direction (negative). In contrast, there is no significant relationship between the Suspension Law and the drug crash fatality rate ($r = -.04$; $p. = .142$). This observation is supported by the Independent Samples *T*-test, which has revealed a significant difference in the total crash fatality rates between states with and without the Suspension Law, but not in the drug crash fatality rates (see Table B1). Table 2 also shows that none of the covariates included in the analysis were highly correlated with any of the other measures.[7]

Table 3 displays the coefficients for the total crash fatality rate regressed on the Suspension Law and the control variables.[8] As noted, the models included differ in their treatment of time. Specifically, Models 1 and 2 include state fixed effects and fixed-effects for time, whereas Models 3 and 4 include state fixed effects, linear time, and autoregressive term AR(1). In addition, Models 1 and 3 demonstrate the regression model in which the total crash fatality rate is predicted only by the Suspension Law and the fixed-effects for time and states. Models 2 and 4, meanwhile, incorporate the structural covariates into the regression equation.

Table 2. Bivariate correlations for the pooled sample.

	X1	X2	X3	X4	X5	X6	X7	X8	X9	X10	X11
Total crash fatality rate	1										
Drug crash fatality rate (ln)	−.01	1									
Suspension law	−.18***	−.04	1								
Drug arrests rate (ln)	−.20***	−.03	.22***	1							
Violent crime rate	.30***	−.16***	.16***	.33***	1						
Alcohol consumption	.02	.13***	−.23***	−.03	.01	1					
Economic disadvantage	.29***	.07**	.05*	−.02	.26***	−.22***	1				
% of young population	.38***	.01	−.01	−.12***	−.04	−.14***	.14***	1			
Hwy funds rate (ln)	−.28***	.39***	−.08***	−.01	−.36***	.14***	−.09***	−.09***	1		
Incarceration rate	−.19***	.27***	.36***	.39***	.21***	−.05*	.24***	−.14***	.24***	1	
% of urban population	−.42***	−.11***	.29***	.42***	.42***	.02	−.04	−.19***	−.35***	.22***	1

**p* <.05; ***p* < .01; ****p* < .001.

Table 3. Modeling total crash fatality rate on suspension law and control variables.

	Model 1	Model 2	Model 3	Model 4
Constant	147.24***	134.03***	5789.61***	5544.22***
	(.447)	(21.48)	(501.07)	(476.57)
Suspension law	−4.11**	−6.61***	−7.29***	−7.30***
	(1.34)	(1.45)	(2.20)	(2.04)
Drug arrests rate (ln)		−2.22*		.264
		(1.00)		(1.20)
Violent crime rate		.010		−.012
		(.006)		(.009)
Alcohol consumption		33.42***		22.27***
		(3.85)		(5.42)
Economic disadvantage		−.821		−1.56*
		(.638)		(.704)
% of young population		2.61***		3.20***
		(.476)		(.702)
Hwy funds rate (ln)		−2.93		−4.33
		(2.18)		(2.52)
Incarceration rate		−.006		.002
		(.008)		(.007)
% of urban population		−93.16***		−40.36
		(15.99)		(22.44)
AR(1)	-	-	.650***	.556***
			(.035)	(.038)
Year	-	-	−2.82***	−2.71
			(.250)	(.245)
Adjusted R^2	.893	.906	.928	.930
Log-likelihood	−6456.49	−6070.29	−5887.85	−5582.51
N	1600	1530	1600	1530

**p* <.05; ***p* <.01; ****p* <.001. Standard errors in parentheses. Models 1 and 2 models include statefixed effects and fixed-effects for time. Models 3 and 4 include state fixed effects, linear time, and autoregressive term AR(1).

The results depicted in Table 3 show that the bivariate correlations displayed in Table 2 largely persist when controlling for the structural covariates in the regression model. Specifically, Model 1 and Model 3 reveal that the Suspension Law, our focal independent variable, yields a significant effect on the total crash fatality rate in the theoretically predicted direction. The Suspension Law is significant and negative in sign in Model 1 (Model 1: b = –4.11; p < .01) that controls for time with fixed effects as it is in Model 3 (Model 3: b = –7.29; p < .001) that treats time as a linear function. Notably, the significant effect of Suspension Law persists once the structural covariates are incorporated into the regression equation in Model 2 (Model 2: b = –6.61; p < .001) and Model 4 (Model 4: b = –7.30; p < .001). This indicates that the implementation of the Suspension Law significantly

Table 4. Modeling drug crash fatality rate on suspension law and control variables.

	Model 1	Model 2	Model 3	Model 4
Constant	1.29***	−1.55	−104.40***	−92.29***
	(.028)	(1.28)	(15.67)	(22.21)
Suspension law	.138	.109	−.176	−.124
	(.078)	(.082)	(.121)	(.114)
Drug arrests rate (ln)		.047		−.032
		(.071)		(.058)
Violent crime rate		−.000		−.001
		(.000)		(.000)
Alcohol consumption		.310		.465
		(.199)		(.240)
Economic disadvantage		−.008		−.002
		(.029)		(.031)
% of young population		.049		.051
		(.028)		(.040)
Hwy funds rate (ln)		.119		.007
		(.131)		(.127)
Incarceration rate		−.000		−.000
		(.000)		(.000)
% of urban population		.159		.054
		(.605)		(.918)
AR(1)	-	-	.587***	.564***
			(.041)	(.043)
Year	-	-	.053***	.046***
			(.008)	(.011)
Adjusted R^2	.506	.516	.671	.671
Log-likelihood	−1624.16	−1521.87	−1225.60	−1154.08
N	1409	1345	1409	1345

**p* <.05; ***p* < .01; ****p* < .001. Standard errors in parentheses. Models 1 and 2 models include state fixed effects and fixed-effects for time. Models 3 and 4 include state fixed effects, linear time, and autoregressive term AR(1).

contributed to a reduction of the total crash fatality rate, regardless of the treatment of time in the given model and net of the other predictors included in the models.

Looking at the parameter estimates of the structural covariates included in the regression equation, both Model 2 and Model 4 show that alcohol consumption per capita (Model 2: b = 33.42; p < .001; Model 4: b = 22.27; p < .001) and percentage of young population yield positive effects on the total crash fatality rate (Model 3: b = 2.61; p < .001; Model 3: b = 3.20; p < .001). That is, increases in alcohol consumption per capita and the percentage of population young are associated with increases in the total crash fatality rate, regardless of the utilized modeling strategy. Some differences emerge, however. Specifically, the results in Model 2 show a significant and negative effect of the drug arrest rate (Model 2: b = −2.22; p < .05), and percent urban (Model 2: b = −93.16; p < .001). Yet, these three measures exhibit no effect in Model 4. In contrast, economic disadvantage is negatively associated with the total crash fatality rate in Model 4 only.

In Table 4 we display the coefficients for the drug crash fatality rate regressed on the Suspension Law and control variables. This table has an identical organization as the previous table, wherein Models 1 and 2 models include state fixed effects and fixed-effects for time and Models 3 and 4 include state fixed effects, linear time, and autoregressive term AR(1). As before, in Models 1 and 3, we report the results for the Suspension Law only. We then increase the empirical rigor of the models by entering the structural covariates in Models 2 and 4, as in the previous table. The results in Table 4 reveal that none of the regression coefficients is statistically significant across the models, including the regression coefficient for our focal independent variable, the Suspension Law. This indicates, then, that while Suspension Law is related to a reduction in the total crash fatality rate, it is not related to the drug crash fatality rate in the present study.

Discussion

While driver's license suspensions result from a wide variety of actions, ranging from driving while intoxicated to a failure to pay fines, we focus specifically on the impact of driver's license suspensions resulting from non-driving related drug offenses. Our findings offer tentative support that laws suspending the driver's licenses of individuals convicted of drug offenses, even non-driving related drug offenses, may offer some safety benefit. However, this conclusion must be tempered with caution given that this finding is only in models for the overall crash fatality rate, and is not found when examining the drug crash fatality rate specifically. This is unexpected given that we would expect any reduction to occur specifically through drug crash fatalities.

One possible explanation for finding a reduction among the total crash fatality rate but not among the drug crash fatality rate is that individuals that are willing to break the law with drug offending, may also be more likely to engage in analogous behaviors, such as risky driving including exceeding the speed limit or driving recklessly (Gottfredson and Hirschi 1990). Thus, suspending the driver's licenses of these individuals may result in a reduction of risky driving behaviors – either due to reduced levels of driving, or more cautious driving as they try to avoid detection of driving with a suspended driver's license, leading to a reduction in overall traffic fatalities. Just as some research indicates that the legalization of marijuana may result in higher traffic fatalities (Hamzeie et al. 2017; Masten and Guenzburger 2014; Salomonsen-Sautel et al. 2014), our research speaks to the need for future research examining the impact of collateral consequence laws related to driving on traffic fatalities.

Although these findings do indicate some support that suspending the driver's licenses of individuals convicted of non-driving related drug crimes may improve safety, it is worth noting that this is not the only, or even the best option, to improve traffic safety. Furthermore, these laws come at the cost of making it more difficult for individuals to maintain legitimate employment, and put a strain on families of those with a suspended license. Options exist however, that could both help individuals that are reentering society along with their families, and that could also improve transportation safety. For example, increased infrastructure providing the greater availability of public transportation options could not only provide safer transportation options (given that public transportation tends to be significantly safer per vehicle miles travelled than private vehicles) (Litman 2014), but would also help support individual's efforts to maintain stable employment by offering transportation options beyond private vehicles. Furthermore, increased public transportation options would offer wide-reaching safety improvements beyond just those realized among this specific population.

Notes

1. Although others find that no statistical differences in employment or income between those that had administrative license suspensions and those that did not (Knoebel and Ross 1997; Wells-Parker and Cosby 1988).
2. A few states had laws in which suspension could be invoked when a motor-vehicle was used, even if the motor-vehicle was not being driven at the time; such as when a person was found to be in possession of drugs, or selling drugs while in a vehicle, but not driving the car under the influence. (Arizona, Maine, Minnesota, Rhode Island, South Dakota, and Wyoming). In addition, two states (Missouri and Wyoming) allow the suspension of driver's licenses for non-driving related offenses, but only for minors (under the age of 21 and 19, respectively). As suspensions for non-driving drug offenses only when a motor vehicle was present, and suspensions for non-driving related drug offenses for minors only, are expected to result in a very small number of suspensions (compared to laws have more inclusive categories of non-driving related drug offenses), they are coded as not having a suspension law.
3. As Chamlin (1988) and McCleary et al. (1980) argue, a rather long time series is required in ARIMA models to produce reliable parameter estimates. To achieve trustworthy results, it is generally said that a minimum of 50 and preferably 100 observations should be included in the ARIMA model (Box and Jenkins 1970; Greenberg 2014). This would require monthly pre-and post-intervention data. While data are available on traffic crash total count at the monthly level, due to difficulties in determining the exact month of implementation and/or repeal of the law in many states, our data use annual data. We also considered ARIMA modeling using yearly data for individual states. Nevertheless, the number of observations in such analysis (32; from 1985 to 2016) would have been too small to achieve trustworthy estimates (Chamlin 1988; Greenberg 2014; McCleary et al. 1980).

4. One limitation of the multivariate models is missing data for the control variables. This problem is particularly true for the longitudinal analysis that often uses data from the US Census or Current Population Survey, which are provided at set intervals only. Given that, researchers frequently use linear interpolation to impute missing values for the intervening years (Chamlin and Sanders 2018). This is done especially for measures such as the age structure and urbanization. In the present study, the annual data for these two measures from the US Census Bureau (U.S. Census Bureau 1980-2017) and from the Economic Research Service (Butler and Beale 2003) did not have missing values (see Table 1). We acknowledge that the Census Bureau and the Economic Research Service developed the intercensal estimates by applying a mathematical formula to take into account differences between the postcensal time-series population estimates and results of the decennial censuses of interests, but note that the techniques used by the Census Bureau are well established and commonly accepted for use in academic research. As a check, however, in the additional analysis, we excluded these two measures to assess the robustness of our findings. The substantial results of this analysis were virtually the same as those presented in the study (results available upon request).
5. Following Chamlin and Sanders (2018, 4) we acknowledge that ARIMA interrupted time-series models have an advantage over multivariate time-series procedures in that prewhitening effectively addresses the omitted variable bias. The benefit of a modeling strategy with state fixed effects is that it controls for any spurious effects caused by the omission of unknown or unmeasured between-unit differences, wherein each observation (e.g., state) is used as its own control group. This does not extend to variation over time, however. Accordingly, some of the results could be due to model misspecification when relevant factors are not accounted for in the model. Nevertheless, as discussed earlier, multivariate time series are preferable when alternative explanations of the outcome series (in addition to the implementation of the law) are of interest for the study (Chamlin 2018).
6. In additional analysis, we also estimated models using the drug crash fatality rate in an original metric (without applying a natural logarithm). Substantively identical results were obtained in this analysis.
7. The results of variance inflation factors (VIF) among the covariates showed that multicollinearity was not an issue in the present analysis, indicating the highest value of 1.65 for the incarceration rate in Table 2 and 1.60 in Table 3 for percent urban. This is below the recommended cut off (10).
8. We also estimated models adjusting for the drug overdose mortality rate (per 100,000) as provided by CDC WONDER Underlying Cause-of-Death Mortality Files. This measure exhibited no effect on both the total and drug crash fatality rate, and did not change the substantive results of the model (including our focal independent variable, Suspension Law). As CDC WONDER indicates, a large share of data points on the drug overdose mortality rate are not reliable. Moreover, these data have been only recently collected in a systematic matter, which resulted in many missing values in the estimated models. Accordingly, we excluded this measure from the final analysis. As data continue to improve, we encourage future studies to examine the above relationships while accounting for the drug overdose mortality rate.

Acknowledgments

The authors thank Sarah Rajec from William and Mary Law School for her assistance on the legal research for this paper, as well as Dr. Tse-Chuan Yang from the University of Albany, SUNY for his helpful suggestions.

Disclosure statement

No potential conflict of interest was reported by the authors.

Funding

This research received no specific grant from any funding agency in the public, commercial, or not-for-profit sectors.

Notes on contributors

Tracy Sohoni is an assistant professor at Old Dominion University. Her research interests focus on race and crime, including the role of race in white collar crime, and the measurement of the race gap in offending using self-reported measures of offending. She is also interested in the impact of collateral consequence laws and issues of reentry. Her work has been published in *Theoretical Criminology, Journal of Crime and Justice, Journal of Research in Crime and Delinquency*, and *Sociological Quarterly*.

Richard Stringer, Ph.D. is an assistant professor in the Department of Sociology and Criminal Justice at Kennesaw State University in Kennesaw, GA. His research interests include drug and alcohol policy, courts and sentencing, policing, and advanced quantitative methods. His prior research has been funded by the U.S. Department of Justice and has appeared in outlets such as the *Journal of Drug Issues, American Journal of Criminal Justice*, and the *Journal of Crime and Justice*. He also serves as Associate Editor for the *Journal of Criminal Justice & Law*.

Sylwia Piatkowska is an assistant professor in the College of Criminology and Criminal Justice at Florida State University. Her areas of interest include hate crime, suicide and suicidal behavior, immigration and crime, and both international and comparative criminology. She has recently published in *Justice Quarterly, Journal of Research in Crime and Delinquency, Sociological Quarterly, Deviant Behavior, International Journal of Comparative and Applied Criminal Justice, International Criminal Justice Review, International Journal of Comparative Sociology* and *European Sociological Review*.

References

Adams, S., C. Cotti, and N. Tefft. 2015. “Seatbelt Use Amoung Drunk Drivers in Different Legislative Settings.” *Economic Inquiry* 53 (1): 758–772. doi:10.1111/ecin.12155.

Aiken, J. 2016. *Reinstating Common Sense: How driver’s license suspensions for drug offenses unrelated to driving are falling out of favor*. A Prison Policy Initiative Report. Accessed 12 December 2016. https://www.prisonpolicy.org/driving/national.html

Alexander, M. 2012. *The New Jim Crow: Mass Incarceration in the Age of Colorblindness*. New York, NY: New Press.

Aydelotte, J. D., L. H. Brown, K. M. Luftman, A. L. Mardock, P. G. Teixeira, B. Coopwood, and C. V. Brown. 2017. “Crash Fatality Rates after Recreational Marijuana Legalization in Washington and Colorado.” *American Journal of Public Health* 107 (8): 1329–1331. doi:10.2105/AJPH.2017.303848.

Beck, N. 2001. ““Time-series–Cross-section Data: What Have We Learned in the Past Few Years?” *Annual Review of Political Science* 4: 271–293. doi:10.1146/annurev.polisci.4.1.271.

Beck, N., and J. N. Katz. 1995. “What to Do (And Not to do) with Time-Series Cross-Section Data.” *American Political Science Review* 89 (2): 634–647. doi:10.2307/2082979.

Beitsch, R. 2017. *States Reconsider Driver’s License Suspensions for People With Drug Convictions*. A Pew Stateline Article. Accessed 31 January 2017. https://www.pewtrusts.org/en/research-and-analysis/blogs/stateline/2017/01/31/states-reconsider-drivers-license-suspensions-for-people-with-drug-convictions

Blomberg, R. D., D. F. Preusser, and R. G. Ulmer. 1987. *Deterrent Effects of Mandatory License Suspension for DWI Conviction (No. DOT-HS-807-138)*. USA: National Highway Traffic Safety Administration.

Box, G., and G. Jenkins. 1970. *Time Series Analysis: Forecasting and Control*. San Francisco: Holden-Day.

Box, G. E. P., and Tiao. 1975. “Intervention Analysis with Applications to Economic and Environmental Problems.” *Journal of the American Statistical Association* 70 (349): 70–79. doi:10.1080/01621459.1975.10480264.

Brandt, P. T., and J. T. Williams. 2007. *Multiple Time Series Models*. Thousand Oaks, California: Sage Publications.

Bureau of Justice Statistics. 2012. *Rate (per 100,000 resident population) of Sentenced Prisoners under Jurisdiction of State and Federal Correctional Authorities on December 31*. Sourcebook of Criminal Justice Statistics. Accessed December 17, 2018. http://www.albany.edu/sourcebook/

Bureau of Justice Statistics. 2013-2016. “Correctional Populations in the United States.” Accessed 10 December 2018. http://www.bjs.gov/

Bureau of Labor Statistics. 2018. “Local Area Unemployment Statistics 1985-2016.” Accessed 10 December 2018. https://www.bls.gov/lau/

Butler, M. A., and C. L. Beale. 2003. *Rural-urban Continuum Codes for Metro and Nonmetro Counties*. Washington, DC: US Department of Agriculture, Economic Research Service. https://www.ers.usda.gov/data-products/rural-urban-continuum-codes/

Campbell, D. T. 1969. “Reforms as Experiments.” *American Psychologist* 24 (4): 409–429. doi:10.1037/h0027982.

Carnegie, J. A. 2007. *Driver’s License Suspensions, Impacts and Fairness Study*. Trenton, NJ: New Jersey Department of Transportation. (No. FHWA-NJ-2007-020).

Chamlin, M. B. 1988. “Crime and Arrests: An Autoregressive Integrated Moving Average (ARIMA) Approach.” *Journal of Quantitative Criminology* 4 (3): 247–257. doi:10.1007/BF01072452.

Chamlin, M. B. 2018. “An Interrupted Time Series Analysis of the Differential Impact of New Jersey’s BAC Legislation on Driver and Passenger Crash Fatalities.” *Journal of Crime and Justice* 40 (4): 542–549. doi:10.1080/0735648X.2016.1158660.

Chamlin, M. B., and B. A. Sanders. 2018. “Social Policy and Crash Fatalities: A Multivariate Time Series Analysis.” *Journal of Crime and Justice* 41 (3): 322–333. doi:10.1080/0735648X.2017.1360194.

Chesney-Lind, M., and M. Mauer, eds. 2003. *Invisible Punishment: The Collateral Consequences of Mass Imprisonment*. New York, NY: New Press.

Chin, G. J. 2002. “Race, the War on Drugs, and the Collateral Consequences of Criminal Conviction.” *Journal of Gender Race and Justice* 6: 253.

Compton, R. P., and A. Berning. 2015. “Drug and Alcohol Crash Risk.” *Journal of Drug Addiction, Education, and Eradication* 11 (1): 29.

DeMichele, M., N. C. Lowe, and B. K. Payne. 2014. "A Criminological Approach to Explain Chronic Drunk Driving." *American Journal of Criminal Justice* 39 (2): 292–314. doi:10.1007/s12103-013-9216-4.

Dews, F. 2013. "Ninety Percent of Americans Drive to Work." Accessed March 13, 2019. https://www.brookings.edu/blog/brookings-now/2013/10/28/ninety-percent-of-americans-drive-to-work/

Evans, W., and J. D. Graham. 1988. "Traffic Safety and the Business Cycle." *Alcohol, Drugs, and Driving* 4 (1): 31–38.

Federal Highway Administration. 2018. "Highway Statistics Series. U.S. Department of Transportation." *Office of Highway Policy Information*. Accessed 1 November 2018. https://www.fhwa.dot.gov/policyinformation/statistics.cfm

Fell, J. C., D. A. Fisher, R. B. Voas, K. Blackman, and A. S. Tippetts. 2009. "The Impact of Underage Drinking Laws on Alcohol-Related Fatal Crashes of Young Drivers." *Alcoholism: Clinical and Experimental Research* 33 (7): 1208–1219. doi:10.1111/acer.2009.33.issue-7.

Fell, J. C., and R. B. Voas. 2014. "The Effectiveness of a 0.05 Blood Alcohol Concentration (BAC) Limit for Driving in the United States." *Addiction* 109 (6): 869–874. doi:10.1111/add.12365.

Foss, R. D., J. R. Stewart, and D. W. Reinfurt. 2001. "Evaluation of the Effects of North Carolina's 0.08% BAC Law." *Accident Analysis and Prevention* 33 (4): 507–517. doi:10.1016/S0001-4575(00)00064-6.

Freeman, D. G. 2007. "Drunk Driving Legislation and Traffic Fatalities: New Evidence on BAC 08 Laws." *Contemporary Economic Policy* 25 (3): 293–308. doi:10.1111/coep.2007.25.issue-3.

Gottfredson, M. R., and T. Hirschi. 1990. *A General Theory of Crime*. Redwood City, CA: Stanford University Press.

Gould, L. A., and K. H. Gould. 1992. "First-time and multiple-DWI Offenders: A Comparison of Criminal History Records and BAC Levels." *Journal of Criminal Justice* 20 (6): 527–539. doi:10.1016/0047-2352(92)90062-E.

Greenberg, D. F. 2014. "Studying New York City's Crime Decline: Methodological Issues." *Justice Quartely* 31 (1): 154–188. doi:10.1080/07418825.2012.752026.

Greenberg, D. F., and R. C. Kessler. 1982. "The Effect of Arrests on Crime: A Multivariate Panel Analysis.social." *Forces* 60 (3): 771–790.

Hamzeie, R., I. Thompson, S. Roy, and P. T. Savolainen. 2017. "State-level Comparison of Traffic Fatality Data in Consideration of Marijuana Laws." *Transportation Research Record: Journal of the Transportation Research Board* (2660): 78–85. doi:10.3141/2660-11.

Hansen, B., K. Miller, and C. Weber. 2018. "Early Evidence on Recreational Marijuana Legalization and Traffic Fatalities." *Economic Inquiry*. doi:10.1111/ecin.12751.

Haughwout, S. P., and M. E. Slater. 2018. *Surveillance Report# 110: Apparent per Capita Alcohol Consumption: National, State, and Regional Trends, 1977–2016*. Bethesda, MD: National Institute on Alcohol Abuse and Alcoholism.

Ibrahim, J. K., E. D. Anderson, S. C. Burris, and A. C. Wagenaar. 2011. "State Laws Restricting Driver Use of Mobile Communications Devices: Distracted-driving Provisions, 1992–2010." *American Journal of Preventive Medicine* 40 (6): 659–665. doi:10.1016/j.amepre.2011.02.024.

Knoebel, K. Y., and H. L. Ross. 1997. "Effects of Administrative License Revocation on Employment." *Accident Analysis and Prevention* 29 (5): 595–611. doi:10.1016/S0001-4575(97)00012-2.

Lavoie, M. C., P. Langenberg, A. Villaveces, P. C. Dischinger, L. Simoni-Wastila, K. Hoke, and G. S. Smith. 2017. "Effect of Maryland's 2011 Alcohol Sales Tax Increase on Alcohol-Positive Driving." *American Journal of Preventive Medicine* 53: 17–24. doi:10.1016/j.amepre.2016.12.011.

Li, G., J. E. Brady, and Q. Chen. 2013. "Drug Use and Fatal Motor Vehicle Crashes: A Case-control Study." *Accident Analysis and Prevention* 60: 205–210. doi:10.1016/j.aap.2013.09.001.

Litman, T. 2014. "A New Transit Safety Narrative." *Journal of Public Transportation* 17 (4): 8. doi:10.5038/2375-0901.

Marcus, A. J. 2003. "Are the Roads a Safer Place because Drug Offenders Aren't on Them: An Analysis of Punishing Drug Offenders with License Suspensions." *Kan. JL and Pub. Pol'y* 13: 557.

Masten, S. V., and G. V. Guenzburger. 2014. "Changes in Driver Cannabinoid Prevalence in 12 US States after Implementing Medical Marijuana Laws." *Journal of Safety Research* 50: 35–52. doi:10.1016/j.jsr.2014.03.009.

Mauer, M. 2006. *Race to Incarcerate*. New York, NY: New Press.

McCartt, A. T., E. R. Teoh, M. Fields, K. A. Braitman, and L. A. Hellinga. 2010. "Graduated Licensing Laws and Fatal Crashes of Teenage Drivers: A National Study." *Traffic Injury Prevention* 11 (3): 240–248. doi:10.1080/15389580903578854.

McCartt, A. T., L. L. Geary, and A. Berning. 2003. "Observational Study of the Extent of Driving while Suspended for Alcohol Impaired Driving." *Injury Prevention* 9 (2): 133–137. doi:10.1136/ip.9.2.133.

McCleary, R., D. McDowall, and B. J. Bartos. 2017. *Design and Analysis of Time Series Experiments*. New York, NY: Oxford Press.

McCleary, R., R. Hay, E. E. Meidinger, and D. McDowall. 1980. *Applied Time Series Analysis for the Social Sciences*. Beverly Hills: Sage.

McDowall, D., R. McCleary, E. Meidinger, and R. Hay Jr. 1980. *Interrupted Time Series Analysis*. Beverly Hills, CA: Sage.

Mertz, K. J., and H. B. Weiss. 2008. "Changes in Motorcycle-related Head Injury Deaths, Hospitalizations, and Hospital Charges following Repeal of Pennsylvania's Mandatory Motorcycle Helmet Law." *American Journal of Public Health* 98 (8): 1464–1467. doi:10.2105/AJPH.2007.123299.

National Highway Traffic Safety Administration (NHTSA). 2018. *Fatality Analysis Reporting System (FARS)*. United States Department of Transportation. Accessed July 22, 2018. https://www.nhtsa.gov/research-data/fatality-analysis-reporting-system-fars

O'Neill, B., and S. Y. Kyrychenko. 2006. "Use and Misuse of Motor-vehicle Crash Death Rates in Assessing Highway-safety Performance." *Traffic Injury Prevention* 7 (4): 307–318. doi:10.1080/15389580600832661.

Ossiander, E. M., and P. Cummings. 2002. "Freeway Speed Limits and Traffic Fatalities in Washington State." *Accident Analysis and Prevention* 34 (1): 13–18. doi:10.1016/S0001-4575(00)00098-1.

Petersilia, J. 2003. *When Prisoners Come Home: Parole and Prisoner Reentry*. New York, NY: Oxford University Press.

Pinard, M. 2010. "Collateral Consequences of Criminal Convictions: Confronting Issues of Race and Dignity." *New York University Law Review* 85: 457.

Pollini, R. A., E. Romano, M. B. Johnson, and J. H. Lacey. 2015. "The Impact of Marijuana Decriminalization on California Drivers." *Drug and Alcohol Dependence* 150: 135–140. doi:10.1016/j.drugalcdep.2015.02.024.

Pratt, T. C., F. T. Cullen, K. R. Blevins, L. E. Daigle, and T. D. Madensen. 2006. "The Empirical Status of Deterrence Theory: A Meta-analysis." *Taking Stock: the Status of Criminological Theory* 15: 367–396.

Romano, E., P. Torres-Saavedra, R. B. Voas, and J. H. Lacey. 2014. "Drugs and Alcohol: Their Relative Crash Risk." *Journal of Studies on Alcohol and Drugs* 75 (1): 56. doi:10.15288/jsad.2014.75.56.

Romano, E., and R. A. Pollini. 2013. "Patterns of Drug Use in Fatal Crashes." *Addiction* 108 (8): 1428–1438. doi:10.1111/add.12180.

Romano, E., and R. B. Voas. 2011. "Drug and Alcohol Involvement in Four Types of Fatal Crashes." *Journal of Studies on Alcohol and Drugs* 72 (4): 567. doi:10.15288/jsad.2011.72.567.

Ross, H. L., and P. Gonzales. 1988. "Effects of License Revocation on Drunk-driving Offenders□." *Accident Analysis and Prevention* 20 (5): 379–391. doi:10.1016/0001-4575(88)90020-6.

Ross, H. L., and R. McCleary. 1983. "Methods for Studying the Impact of Drunk Driving Laws." *Accident Analysis and Prevention* 15: 415–428. doi:10.1016/0001-4575(83)90027-1.

Salomonsen-Sautel, S., S.-J. Min, J. T. Sakai, C. Thurstone, and C. Hopfer. 2014. "Trends in Fatal Motor Vehicle Crashes before and after Marijuana Commercialization in Colorado." *Drug and Alcohol Dependence* 140: 137–144. doi:10.1016/j.drugalcdep.2014.04.008.

Samuels, P., and D. Mukamal. 2009. *After Prison, Roadblocks to Reentry: A Report on State Legal Barriers Facing People with Criminal Records. 2009 Update*. New York, NY: Legal Action Center.

Stafford, M., and M. Warr. 1993. "A Reconceptualization of General and Specific Deterrence." *Journal of Research in Crime and Delinquency* 30 (2): 123–135. doi:10.1177/0022427893030002001.

Stringer, R. J. 2016. "Are Buzzed Drivers Really the Problem?: A Quasi-Experimental Multilevel Assessment of the Involvement of Drivers with Low Blood Alcohol Levels in Fatal Crashes." *Criminal Justice Policy Review* 29 (5): 464–488. doi:10.1177/0887403416637187.

Stringer, R. J. 2018. "Policing the Drunk Driving Problem: A Longitudinal Examination of DUI Enforcement and Alcohol Related Crashes in the US (1985–2015)." *American Journal of Criminal Justice* 44 (3): 474–498. doi:10.1007/s12103-018-9464-4.

Thompson, A. C. 2008. *Releasing Prisoners, Redeeming Communities: Reentry, Race, and Politics*. New York, NY: NYU Press.

Tippetts, A. S., R. B. Voas, J. C. Fell, and J. L. Nichols. 2005. "A Meta-analysis Of. 08 BAC Laws in 19 Jurisdictions in the United States." *Accident Analysis & Prevention* 37 (1): 149–161. doi:10.1016/j.aap.2004.02.006.

Tonry, M. H. 2011. *Punishing Race: A Continuing American Dilemma*. Oxford, UK: Oxford University Press.

Travis, J., and C. Visher, eds. 2005. *Prisoner Reentry and Crime in America*. Cambridge, UK: Cambridge University Press.

U.S. Bureau of the Census, Current Population Survey, Annual Social and Economic Supplements.1980-2017. "Table 21. Number of Poor and Poverty Rate, by State: 1980 to 2017." Accessed 10 December 2018. https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-people.html

U.S. Census Bureau. 1980-2017. "State Intercensal Tables: 1980-1990," "State and County Intercensal Tables: 1990-2000," "State Intercensal Tables: 2000-2010," "State Population by Characteristics: 2010-2017," Educational Attainment Tables: 1985-2016." Accessed 18 December 2018. https://www.census.gov

U.S. Department of Justice. 2017. "Uniform Crime Reporting Statistics - UCR Data Online." Accessed 10 December 2018. https://www.ucrdatatool.gov/

U.S. Department of Justice. 2018. "2016 Crime in the United States." Accessed 10 December 2018. https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016

U.S. Department of Justice. 2018b. Federal Bureau of Investigation. Uniform Crime Reporting Program Data: Arrests by Age, Sex, and Race, Summarized Yearly, United States (1985-2016). Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor]. Accessed 1 November 2018. https://www.icpsr.umich.edu/icpsrweb/ICPSR/series/57

Ulmer, R. G., V. I. Shabanova, and D. F. Preusser. Preusser Research Group. 2001. *Evaluation of Use and Lose Laws*. USA: National Highway Traffic Safety Administration. No. DOT-HS-809-285.

Vernon, D. D., L. J. Cook, K. J. Peterson, and J. M. Dean. 2004. "Effect of Repeal of the National Maximum Speed Limit Law on Occurrence of Crashes, Injury Crashes, and Fatal Crashes on Utah Highways." *Accident Analysis and Prevention* 36 (2): 223–229. doi:10.1016/S0001-4575(02)00151-3.

Voas, R., and A. Tippetts 1996. "*Unlicensed Driving by DUIs—A Major Safety Problem? TRB ID No. CR077*." Paper presented at the 73rd Annual Meeting. Landover, MD: Transportation Research Board.

Voas, R. B., A. S. Tippetts, and J. Fell. 2000. "The Relationship of Alcohol Safety Laws to Drinking Drivers in Fatal Crashes." *Accident Analysis and Prevention* 32 (4): 483–492. doi:10.1016/S0001-4575(99)00063-9.
Voas, R. B., A. S. Tippetts, and E. P. Taylor. 1998. "Impact of Ohio Administrative License Suspension." *Annual Proceedings/ Association for The Advancement of Automotive Medicine* 42: 401–415.
Wagenaar, A. C. 1984. "Effects of Macroeconomic Conditions on the Incidence of Motor Vehicle Accidents." *Accident Analysis and Prevention* 16 (3): 191–205. doi:10.1016/0001-4575(84)90013-7.
Wagenaar, A. C., and M. M. Maldonado-Molina. 2007. "Effects of Drivers' License Suspension Policies on Alcohol-Related Crash Involvement: Long-Term Follow-Up in Forty-Six States." *Alcoholism: Clinical and Experimental Research* 31 (8): 1399–1406. doi:10.1111/j.1530-0277.2007.00441.x.
Wells-Parker, E., and P. J. Cosby. 1988. "Behavioral and Employment Consequences of Driver's License Suspension for Drinking Driving Offenders." *Journal of Safety Research* 19 (1): 5–20. doi:10.1016/0022-4375(88)90028-X.
Wooldridge, J. M. 2002. *Econometric Analysis of Cross-Section and Panel Data*. Cambridge, MA: MIT Press.
Zimmerman, K., and N. Fishman. 2001. *Roadblock on the Way to Work: Driver's License Suspension in New Jersey*. Newark, NJ: New Jersey Institute for Social Justice.

Appendix

Table A1. Suspension law by state.

State	Law	Implementation	State	Law	Implementation
Alabama	Yes	1993–Present	Montana	No	
Alaska	No		Nebraska	No	
Arizona	No		Nevada	No	
Arkansas	Yes	1991–Present	New Hampshire	No	
California	No		New Jersey	Yes	1988–Present
Colorado	Yes	1995–2009	New Mexico	No	
Connecticut	No		New York	Yes	1993–Present
Delaware	Yes	1990–2014	North Carolina	No	
Florida	Yes	1989–Present	North Dakota	No	
Georgia	Yes	1990–2015	Ohio	Yes	1993–2016
Hawaii	No		Oklahoma	Yes	1990–2010
Idaho	No		Oregon	No	
Illinois	No		Pennsylvania	Yes	1993–Present
Indiana	Yes	1990–2014	Rhode Island	No	
Iowa	Yes	1996–Present	South Carolina	Yes	1990–2011
Kansas	No		South Dakota	No	
Kentucky	No		Tennessee	No	
Louisiana	Yes	1989–Present	Texas	Yes	1991–Present
Maine	No		Utah	Yes	1993–Present
Maryland	No		Vermont	No	
Massachusetts	Yes	1989–2016	Virginia	Yes	1992–Present
Michigan	Yes	1993–Present	Washington	No	
Minnesota	No		West Virginia	No	
Mississippi	Yes	1991–Present	Wisconsin	Yes	1993–2009
Missouri	No		Wyoming	No	

Table B1. *T*-tests for dependent variables by suspension law.

	Two-sample *T*-test				
	Law		No law		
	Mean	SD	Mean	SD	*T*-test
Total crashfFatality rate	135.08	41.23	154.93	53.88	7.48***
Drug crash fatality rate	5.74	5.61	6.36	7.17	1.73

*$p < .05$, **$p < .01$, ***$p < .001$, two-tailed test.