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D. Mark Anderson
Daniel I. Rees

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D. Mark Anderson
Montana State University

Daniel I. Rees
University of Colorado Denver and IZA

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

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The Office of National Drug Control Policy (ONDCP) recently announced a goal of reducing drugged driving by 10 percent within three years. In an effort to achieve this goal, ONDCP is encouraging all states to adopt per se drugged driving laws, which make it illegal to operate a motor vehicle with a controlled substance in the system. To date, 16 states have passed per se drugged driving laws, yet little is known about their effectiveness. The current study examines the relationship between these laws and traffic fatalities, the leading cause of death among Americans ages 5 through 34. Our results provide no evidence that per se drugged driving laws reduce traffic fatalities.

JEL Classification: I10, I18

Keywords: drugged driving, per se laws, traffic fatalities, marijuana

Corresponding author:

Daniel I. Rees
University of Colorado Denver
Department of Economics
Campus Box 181
P.O. Box 173364
Denver, CO 80217-3364
USA
E-mail: Daniel.Rees@ucdenver.edu

1. INTRODUCTION

Arizona was the first state to pass a per se drugged driving law. As of June 28, 1990 it became illegal to operate a motor vehicle in Arizona with detectable levels of cocaine, marijuana, methamphetamine, phencyclidine (i.e., PCP) or any other controlled substance in the system. Arizona drivers who test positive for a controlled substance are presumed to be impaired and can be prosecuted without additional evidence.

Since 1990, 10 more states have passed zero tolerance per se drugged driving laws, and 5 states have passed laws that specify nonzero thresholds for controlled substances (or their metabolites) above which a driver is automatically considered impaired (Table 1). Nevada, Ohio, and Pennsylvania specify nonzero thresholds for marijuana and a variety of other controlled substances. Virginia specifies nonzero thresholds for cocaine, methamphetamine and phencyclidine, but does not specify thresholds for marijuana or tetrahydrocannabinol, the primary psychoactive agent in marijuana. The Washington law, which was passed on November 6, 2012 and came into effect one month later, specifies a nonzero threshold for tetrahydrocannabinol but no other controlled substance.

R. Gil Kerlikowske, the director of the Office of National Drug Control Policy (ONDCP), has called drugged driving “a significant problem” (Westall 2010). Indeed, according to data from the National Survey on Drug Use and Health, 10.6 million Americans drove under the influence of an illicit drug in 2010; in comparison, 28.8 million Americans reported that they drove under the influence of alcohol (U.S. Department of Health and Human Services 2011).

According to Compton and Berning (2009), who analyzed data from the 2007 National Roadside Survey, more than 15 percent of drivers on weekend nights test positive for drugs.[1]

The ONDCP recently announced that it would like to "make preventing drugged driving a national priority on par with preventing drunk driving." Its specific goal is to reduce drugged driving in the Unites States by 10% within three years (White House 2012b). In an effort to achieve this goal, the ONDCP is encouraging all 50 states to adopt per se drugged driving laws.[2] However, aside from anecdotal evidence that these laws make drugged driving easier to prosecute, next to nothing is known about their effectiveness (Walsh et al. 2004).

Using data from the Fatality Analysis Reporting System (FARS) for the period 1990-2010, the current study examines the relationship between per se drugged driving laws (hereafter referred to as "per se laws") and traffic fatalities. Our results suggest that per se laws are negatively related to traffic fatalities in the cross section. Controlling for unobserved heterogeneity at the state level, the estimated relationship between per se laws and traffic fatalities becomes positive, but is statistically indistinguishable from zero. We conclude that, as currently implemented, making it illegal to operate a motor vehicle with drugs (or drug metabolites) in the system, has no discernible impact on traffic fatalities.

2. BACKGROUND

2.1. Substance use and driving

[1] There is evidence that driving under the influence of marijuana is especially prevalent among teenagers and young adults (Lacey et al. 2007). According to data collected by Monitoring the Future, high school seniors are now more likely to drive after smoking marijuana than to drive after consuming alcohol (White House 2012a).

[2] In addition to the ONDCP, the Governors Highway Safety Association and the Institute for Behavior and Health have expressed strong support for per se drugged driving laws. Recently, R. Gil Kerlikowske and the President of Mothers Against Drunk Driving, Jan Withers, announced a new partnership to raise public awareness regarding the consequences of drugged driving. Kerlikowske has argued that per se drugged driving laws "can help to keep drugged drivers off the road" and therefore reduce traffic fatalities (Kerlikowske 2012).

Alcohol impairs driving-related functions such as concentration, hand-eye coordination, and reaction time (Kelly et al. 2004; Sewell et al. 2009). Not surprisingly, simulator, driving-course, and etiological studies, which are typically based on police crash and medical examiner reports, provide strong evidence that alcohol consumption leads to an increased risk of collision (Kelly et al. 2004; Sewell et al. 2009). Drivers under the influence of alcohol tend to underestimate the degree to which they are impaired (MacDonald et al. 2008; Marczinski et al. 2008; Robbe and O'Hanlon 1993; Sewell et al. 2009), drive faster, and take unnecessary risks (Burian et al. 2002; Ronen et al. 2008; Sewell et al. 2009).

Laboratory studies have shown that, like alcohol, tetrahydrocannabinol (THC) impairs driving-related functions (Kelly et al. 2004; Sewell et al. 2009). However, simulator and driving-course studies provide little evidence that marijuana use leads to an increased risk of collision (Kelly et al. 2004; Sewell et al. 2009), perhaps because drivers under the influence of marijuana tend to overestimate the degree to which they are impaired (Kelly et al. 2004; Sewell et al. 2009).[3] Although some etiological studies have shown a positive association between marijuana use and the risk of collision, they have been described as "fraught with methodological problems" (Sewell et al. 2009, p. 189). More than 10 percent of U.S. drivers killed in traffic accidents test positive for cannabinols (Brady and Li 2012), but it is exceedingly difficult to account for the influence of other, difficult-to-observe, factors potentially correlated

[3]According to Sewell et al. (2009, p. 186):

> Many investigators have suggested that the reason why marijuana does not result in an increased crash rate in laboratory tests despite demonstrable neurophysiologic impairments is that, unlike drivers under the influence of alcohol, who tend to underestimate their degree of impairment, marijuana users tend to *overestimate* their impairment, and consequently employ compensatory strategies.

with marijuana. Such factors could include, but are certainly not limited to, personality and an individual's attitude towards risk.[4]

Nine percent of U.S. drivers killed in traffic accidents test positive for stimulants and 6 percent test positive for narcotics (Brady and Li 2012). Despite the fact that these drugs are used by a non-trivial fraction of drivers in the Unites States and other developed countries, very little is known about their impact on road safety (Kelly et al. 2004). Only a handful of etiological studies in this area have examined substances other than alcohol and marijuana, and even fewer simulator or driving course studies have been conducted.[5] However, the consensus opinion among experts appears to be that, in high doses, most drugs are "likely to increase accident risk" (Kelly et al. 2004, p. 332).

2.2. Per se laws and traffic fatalities

Currently, all 50 states prohibit driving a motor vehicle with a blood alcohol concentration (BAC) of 0.08 or greater. Drivers found to have a BAC greater than 0.08 are presumed to be impaired and can be prosecuted without having to introduce additional evidence. In contrast, most states do not set specific thresholds for controlled substances. As a

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[4] A recent meta-analysis concluded that acute cannabis consumption nearly doubled the risk "of being involved in a motor vehicle collision resulting in serious injury or death" (Asbridge et al. 2012, p. 4). However, the authors of this study noted that:

> Although we restricted positive cannabis results to drivers that showed the presence of tetrahydrocannabinol in the absence of other drugs or alcohol, other potentially important confounders were probably not controlled for. These hidden confounders, as well as the differing study designs used, might have affected the results of the individual studies and hence the estimates of the pooled odds ratios (pp. 4-5).

[5] Driving course and simulator studies have found evidence of benzodiazepine-induced impairment in driving performance (Kelly et al. 2004), but, to our knowledge, no simulator or driving course study has examined the impact of opioids or stimulants.

consequence, in order to prove impairment, the prosecution must rely on the results of a field sobriety test or evidence that the motorist was driving erratically.

Per se laws are intended to make the job of prosecuting drugged drivers easier, and a number of state officials have reported that they are "working well" (Lacey et al. 2010, p. 5). However, because urine or blood samples must be obtained in order to determine the presence of a controlled substance in the system, and because probable cause is typically required in order to obtain toxicological evidence, it has been argued that per se laws are not a "panacea" (Walsh et al. 2004, p. 251). Whether their adoption leads to fewer accidents and traffic fatalities is an open question.

Although no previous study has examined the relationship between per se laws and traffic fatalities, the relationship between BAC laws and traffic fatalities has received considerable attention from economists.[6] Using FARS data for the period 1982-1998 and a difference-in-differences approach, Dee (2001) found that the 0.08 BAC limit was associated with a 7 percent reduction in traffic fatalities. Eisenberg (2003), who used FARS data for the period 1982-2000 and an empirical approach similar to that used by Dee (2001), found that the 0.08 BAC limit was associated with an 11 percent reduction in traffic fatalities. In contrast, Freeman (2007), who used FARS data for the period 1980-2004, found little evidence that the BAC 0.08 limit was

[6] Jones (2005) found that the number of blood samples collected by police from Swedish motorists suspected of driving under the influence of drugs went up dramatically after a zero tolerance drugged driving law was introduced in 1999. Jones (2005, p. 321) concluded:

> Sweden's new zero-concentration limit for scheduled drugs in the blood of drivers has stimulated police efforts to apprehend and prosecute DUID offenders…However, the problem of drug-impaired driving is far from solved. Those people who drive after taking illicit drugs are mostly criminal elements in society who lack a valid driving permit and whose police records show many previous convictions for drunk and/or drugged driving as well as other deviant behavior. Indeed, recidivism is close to 50–60% in these individuals so the zero-limit law has certainly not reduced DUID or functioned as a deterrent.

effective. Freeman (2007, p. 302) noted that over 30 states passed BAC 0.08 laws in the early 2000s, but "alcohol-related traffic fatalities, as a percent of the total, were constant." He concluded that BAC 0.08 laws "have no measurable effects on traffic fatality rates" (Freeman 2007, p. 306).[7]

The evidence with regard to zero tolerance (ZT) drunk driving laws and traffic fatalities is also mixed. Several studies have found that ZT drunk driving laws, which make it illegal for individuals under the age of 21 to operate a motor vehicle with detectable levels of alcohol in their blood, are negatively related to traffic fatalities (Dee and Evans 2001; Eisenberg 2003; Voas et al. 2003).[8] However, Grant (2010) found that the estimated relationship between ZT drunk driving laws and daytime traffic fatalities was as strong as the relationship between ZT drunk driving laws and nighttime traffic fatalities. Because a substantial proportion of fatal crashes at night involve alcohol (Dee 1999), this pattern of results raises the possibility of omitted variable bias.

In the empirical analysis below, we are careful to distinguish between traffic fatalities that occurred at night and those that occurred during the day. In addition, we distinguish between traffic fatalities that occurred during the week and those that occurred on Friday night through Monday morning. The percentage of drivers who test positive for marijuana and other controlled substances is highest at night and on weekends (Compton and Berning 2009). Presumably, if per se laws reduce drugged driving, then their impact should be most pronounced during these times.

[7] See also Young and Bielinska-Kwapisz (2006) who found that adopting a BAC 0.08 law was associated with an increase in traffic fatalities. French et al. (2009) found little evidence of a relationship between BAC 0.08 laws and motorcycle fatalities.

[8] See also Carpenter (2004) and Liang and Huang (2008) who examined the relationship between ZT drunk driving laws and alcohol consumption. Carpenter (2007) found that ZT drunk driving laws reduced property and nuisance crimes among 18- through 20-year-olds.

3. ESTIMATION

As noted in the introduction, information on traffic fatalities comes from the Fatality Analysis Reporting System (FARS), which is produced by the National Highway Traffic Safety Administration. The FARS data represent a census of all fatal injuries resulting from motor vehicle accidents in the United States. Information on the details of each accident and whether alcohol was involved comes from a variety of sources including police reports, driver licensing files, vehicle registration files, state highway department data, emergency medical services records, medical examiner reports, toxicology reports and death certificates.[9]

We begin the empirical analysis by estimating the following equation for the period 1990-2010:

(1) $$\ln(\textit{Traffic Fatalities}_{st}) = \beta_0 + \beta_1 \textit{Per se law}_{st} + v_s + w_t + \varepsilon_{st},$$

where $\textit{Traffic Fatalities}_{st}$ is equal to the number of traffic fatalities per 100,000 population of state *s* in year *t*.[10] The variable $\textit{Per se law}_{st}$ is an indicator for whether a per se law was in effect. The coefficient of interest, β_1, represents the effect of these laws on traffic fatalities. State fixed effects, represented by the vector v_s, capture the influence of time-invariant factors at the state level. Year fixed effects, represented by the vector w_t, capture the influence of nationwide shocks to traffic fatalities.

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[9] Additional information on how the FARS data are collected is available at: http://www.nhtsa.gov/FARS.

[10] Population data come from the National Cancer Institute and are available at: http://seer.cancer.gov/popdata/index.html. Appendix Table 1 presents means, standard deviations, and definitions of the dependent variables used in the analysis.

Next, we add a set of controls to the estimating equation, represented by the vector $\boldsymbol{X_{st}}$:

(2) $$\ln(\textit{Traffic Fatalities}_{st}) = \beta_0 + \beta_1 \textit{Per se law}_{st} + \boldsymbol{X_{st}\beta_2} + v_s + w_t + \varepsilon_{st}.$$

Previous studies provide evidence that graduated driver licensing regulations and stricter seatbelt laws lead to fewer traffic fatalities (Cohen and Einav 2003; Dee et al. 2005; Freeman 2007; Carpenter and Stehr 2008). Other studies have examined the effects of speed limits (Ledolter and Chan 1996; Farmer et al. 1999; Greenstone 2002; Dee and Sela 2003), administrative license revocation laws (Freeman 2007), BAC laws (Dee 2001; Eisenberg 2003; Young and Bielinska-Kwapisz 2006; Freeman 2007), zero tolerance drunk driving laws (Voas et al. 2003; Carpenter 2004; Liang and Huang 2008; Grant 2010), beer taxes (Chaloupka et al. 1991; Ruhm 1996; Dee 1999; Young and Likens 2000; Young and Bielinska-Kwapisz 2006), the legalization of medical marijuana (Anderson et al. forthcoming), marijuana decriminalization (Chaloupka and Laixuthai 1997), and cellphone/texting bans (Kolko 2009; Abouk and Adams forthcoming). In addition to these state-level policies that could potentially be correlated with per se laws and traffic fatalities, we include the mean age of the driver population in state *s* and year *t*, the unemployment rate, real per capita income, and vehicle miles driven per licensed driver in the vector X_{st}.[11]

[11] Appendix Table 2 presents means, standard deviations, and definitions of the independent variables used in the analysis. Information on graduated driver licensing laws and seatbelt requirements is available from Cohen and Einav (2003), Dee et al. (2005), and the Insurance Institute for Highway Safety (iihs.org). Information on administrative license revocation laws and BAC limits is available from Freeman (2007). Data on beer taxes are from the *Brewers Almanac*, an annual publication produced by the Beer Institute. Data on whether texting while driving was banned and whether using a handheld cellphone while driving was banned are from www.handsfreeinfo.com. Mean age in state *s* and year *t* was calculated using U.S. Census data, and information on vehicle miles driven per licensed driver is from *Highway Statistics*, an annual publication produced by the U.S. Department of Transportation. The unemployment and income data are from the Bureau of Labor Statistics and the

Finally, we add state-specific linear time trends to our model, represented by $\Theta_s \cdot t$:

$$(3) \qquad \ln(\textit{Traffic Fatalities}_{st}) = \beta_0 + \beta_1 \textit{Per se law}_{st} + X_{st}\beta_2 + v_s + w_t + \Theta_s \cdot t + \varepsilon_{st}.$$

State-specific linear time trends control for factors at the state level that evolve at a constant rate over time (e.g., sentiment towards drugged driving). All models are estimated using ordinary least squares and observations are weighted using the population in state *s* at time *t*. Standard errors are corrected for clustering at the state level (Bertrand et al. 2004).[12]

Because previous studies have shown that drugged driving rates are highest at night and on weekends, we estimate (3), our preferred specification, replacing $\textit{Traffic Fatalities}_{st}$ with the following alternative dependent variables: $\textit{Fatalities Weekdays}_{st}$, $\textit{Fatalities Weekends}_{st}$, $\textit{Fatalities Daytime}_{st}$, and $\textit{Fatalities Nighttime}_{st}$.[13] Because there is evidence that drugged driving is especially prevalent among males, teenagers, and young adults (National Household Survey on Drug Abuse 2002; Reinberg 2010), we estimate (3) replacing $\textit{Traffic Fatalities}_{st}$ with: *Fatalities*

Bureau of Economic Analysis, respectively. Data on decriminalization laws are from Model (1993) and Scott (2010).

[12] Controlling for state fixed effects, year fixed effects, and state-specific linear time trends is standard in the literature on traffic fatalities. See, for instance, Dee et al. (2005), Miron and Tetelbaum (2009), and Dills (2010).

[13] Following Dee (2001), $\textit{Fatalities Weekdays}_{st}$ is defined as the traffic fatality rate between 6 A.M. on Mondays to 5:59 P.M. on Fridays per 100,000 population in state *s* and year *t*; $\textit{Fatalities Weekends}_{st}$ is equal to the traffic fatality rate between 6 P.M. on Fridays and 5:59 A.M. on Mondays per 100,000 population in state *s* and year *t*; $\textit{Fatalities Daytime}_{st}$ is equal to the traffic fatality rate between 6 A.M. and 5:59 P.M per 100,000 population in state *s* and year *t*; $\textit{Fatalities Nighttime}_{st}$ is equal to the traffic fatality rate between 6 P.M. and 5:59 A.M per 100,000 population in state *s* and year *t*.

Males$_{st}$, *Fatalities Females*$_{st}$, and a series of fatality rates corresponding to specific age groups (i.e., 15 through 19 years of ages, 20 through 29 years of age, 30 through 39 years of age, etc.).[14]

4. RESULTS

4.1. The relationship between per se laws and traffic fatalities

Figure 1 presents traffic fatality trends for states that adopted a per se law during the period 1990-2010. The vertical lines represent the years in which these laws came into effect. Figure 1 also shows the average trend for states that did not adopt a per se law. Although Figure 1 provides little evidence that per se laws reduce traffic fatalities, omitted factors, could have masked their effects. For instance, traffic fatalities were falling in most states prior to 2008, but the economic downturn appears to have accelerated this trend.[15]

Estimates of the relationship between per se laws and traffic fatalities are presented in Table 2. The baseline estimate, in column (1), is negative and large, but not statistically significant. If taken at face value, it would suggest that the adoption of a per se law leads to an 11.3 percent ($e^{-0.120} - 1 = -0.113$) decrease in the traffic fatality rate. However, this estimate does not account for factors potentially correlated with per se laws and traffic fatalities.

When we include state and year fixed effects, the estimate of β_1 remains negative but becomes much smaller in absolute magnitude: the adoption of a per se law is associated with a (statistically insignificant) 1.5 percent decrease in the traffic fatality rate. When we include the covariates discussed in the previous section, the estimate of β_1 becomes positive: the adoption of

[14] *Fatalities Males*$_{st}$ is equal to the traffic fatality rate per 100,000 males in state *s* and year *t*. *Fatalities Females* is equal to the traffic fatality rate per 100,000 females in state *s* and year *t*. The fatality rates by age group are rates per the relevant state-by-age populations.

[15] Cotti and Tefft (2011) provide evidence with regard to the effect of the "Great Recession" of 2008-2009 on traffic fatalities.

a per se law is associated with a (statistically insignificant) 1.6 percent increase in the traffic fatality rate. When we include state-specific linear time trends, the adoption of a per se law is associated with a (statistically insignificant) 0.8 percent increase in the traffic fatality rate.[16]

It is possible that per se laws become more effective over time as the necessary apparatus for enforcement is put into place. To explore this issue, we replace the variable *Per se law*$_{st}$ with an indicator for the year in which the law changed and five lags. The estimates are reported in Table 3.

With or without state-specific time trends, there is a small, statistically insignificant reduction in traffic fatalities the year in which the law changed and the first full year after implementation. The remaining lags are positive, but none are statistically distinguishable from zero. After 5 full years, and controlling for state-specific linear time trends, the adoption of a per se law is associated with a (statistically insignificant) 5.3 percent increase in traffic fatalities. Using the 90 percent confidence interval around this estimate, we can reject the hypothesis that traffic fatalities fell by more than 0.5 percent.

In the final column of Table 3, we include three leads of *Per se law*$_{st}$. Adding leads to the model provides a simple check for whether the treatment and control states differed systematically prior to the adoption of per se laws. While the coefficients of the leads are uniformly positive, none are statistically significant, suggesting the common trends assumption holds.[17] The lags are, with one exception, positive and statistically insignificant. After 5 full years, and controlling for state-specific linear time trends, the adoption of a per se law is

[16] Using the 90 percent confidence interval around this estimate suggests that, at most, the adoption of a per se law reduces the traffic fatality rate by 3.2 percent.

[17] We can reject the hypothesis that the leads are jointly significant.

associated with an (statistically insignificant) 8.0 percent increase in traffic fatalities. Using the 90 percent confidence interval around this estimate, we can reject the hypothesis that traffic fatalities fell by more than 0.2 percent.

Although statistically insignificant, the estimates in the third column of Table 3 provide some evidence that, after 4 or 5 years, the adoption of per se laws could actually lead to an increase in traffic fatalities. One possible explanation for this result is that, because they reduce the relative cost of drunk driving, per se laws may lead to more alcohol-related accidents.[18] To test this hypothesis, we estimated the relationship between per se laws and traffic fatalities resulting from accidents where at least one driver had a positive blood alcohol concentration. We found no evidence to suggest that per se laws increase alcohol-related traffic fatalities. Results were similar when estimating the relationship between per se laws and traffic fatalities resulting from accidents where at least one driver had a blood alcohol concentration greater than or equal to 0.10. These results are available from the authors upon request.

Table 4 presents estimates of the relationship between per se laws and traffic fatalities by the day of the week and the time of day. Because drivers are more likely to test positive for illicit drugs on nights and on weekends (Compton and Berning 2009), it is important to distinguish between weekday and weekend traffic fatalities and between daytime and nighttime traffic fatalities. The estimates in Table 4 suggest that the adoption of a per se law is associated with small increases in the traffic fatality rate on weekdays, weekends, and during the daytime. However, none of these estimates are statistically distinguishable from zero. On the other hand,

[18] There is a substantial literature on the relationship between the use of marijuana and alcohol. A number of studies have found evidence suggesting that marijuana and alcohol are substitutes (Chaloupka and Laixuthai 1997; Saffer and Chaloupka 1999; DiNardo and Lemieux 2001; Crost and Guerrero 2012; Anderson et al. forthcoming). Others have found evidence of complementarity between marijuana and alcohol (Pacula 1998; Farrelly et al. 1999; Williams et al. 2004; Yörük and Yörük 2011). DeSimone and Farrelly (2003) found evidence of complementarity between marijuana and cocaine. Crost and Rees (forthcoming) commented on the work of Yörük and Yörük (2011).

the adoption of a per se law is negatively associated with the nighttime traffic fatality rate. While the direction of this effect is consistent with the argument that, if per se laws reduce drugged driving, then their impact should be pronounced at night, it is nowhere near statistically significant.

Up to this point, we have not distinguished between drivers based on age or gender, raising the possibility that the effects of per se laws on demographic subgroups have gone undetected. Table 5 presents estimates of the relationship between per se laws and traffic fatalities by age group. The potential for per se laws to affect the behavior of youths is of particular interest given recent attempts by the ONDCP to curb teenage drugged driving (White House 2012c). In 2008, one in 10 high school seniors reported having recently driven a vehicle after smoking marijuana (White House 2012c).

Among 15- through 19-year-olds, the estimate of β_1 is negative, but is not statistically significant at conventional levels.[19] Of the remaining estimates, 4 out of 5 are positive and only one is statistically significant. The adoption of a per se law is associated with a 6.8 percent increase in the traffic fatality rate of individuals over the age of 60, and this estimate is statistically significant at the 0.10 level.

There is evidence that males are more likely to dive under the influence of a controlled substance than females (National Household Survey on Drug Abuse 2002). However, the adoption of a per se law is not associated with a statistically significant reduction in traffic fatalities among males (Table 6). In fact, it is associated with a (statistically insignificant) 0.3 percent *increase* in male traffic fatalities. The adoption of a per se law is also associated with a

[19] When our focus is restricted to traffic fatalities among drivers ages 15-19, we code North Carolina and South Dakota as if they were "treated." Both states have per se drugged driving laws that apply only to individuals under the age of 21. North Carolina changed its law on December 1, 2006; South Dakota changed its law on July 1, 1998.

(statistically insignificant) 2.6 percent increase in traffic fatalities among females. Moreover, estimates of the relationship between per se laws and traffic fatalities by age and gender (e.g., 15- through 19-year-old males and 15- through 19-year-old females) were qualitatively similar to those reported in Tables 5 and 6. These results are available from the authors upon request.

4.2. Robustness checks

In Table 7, we subject the findings discussed above to a series of sensitivity checks. For reference, the first column of Table 7 presents our preferred estimate from Table 4 that controls for the vector of covariates, state fixed effects, year fixed effects, and state-specific linear time trends. In the second column, we restrict the control states to those that bordered states that adopted a per se law between 1990 and 2010. The estimated relationship between per se laws and traffic fatalities is negative, small in magnitude, and nowhere near statistically significant.

In the remaining columns, we consider three alternative dependent variables. First, we use the traffic fatality rate per 100,000 licensed drivers in state *s* and year *t* instead of $Traffic\ Fatalities_{st}$.[20] Second, we use the traffic fatality rate per vehicle miles traveled.[21] Lastly, we consider a logistic transformation often used by researchers working in this area (e.g. Ruhm 1996; Young and Likens 2000; Dills 2010).[22] Regardless of the dependent variable used, there is little evidence to support the hypothesis that per se laws reduce traffic fatalities.

[20] Eisenberg (2003) used a similarly-defined dependent variable.

[21] Abouk and Adams (forthcoming) examined the effect of texting bans on the traffic fatality rate per vehicle miles traveled as a robustness check.

[22] The log-odds ratio of traffic fatalities takes into account the discrete nature of a traffic fatality at the individual level (Ruhm 1996).

4.3. Interstate Heterogeneity

Eleven of the states that have enacted per se laws also have a Drug Recognition Expert (DRE) program.[23] DRE programs are designed to train officers to recognize drug impairment in drivers and to guide analyses of biological specimens when the presence of drugs other than alcohol is expected (Lacey et al. 2010). These extensive training and certification programs are also designed to teach officers about symptoms of impairment that could be used to determine the type of drug a driver has been using (Lacey et al. 2010).[24] If drug intoxication is suspected, a blood or urine sample is submitted to a laboratory for confirmation (National Council on Alcoholism and Drug Dependence 2012). In a recent review of per se laws in the United States, DRE programs were characterized as a potentially important complement to per se legislation (Lacey et al. 2010).[25]

The top panel in Table 8 presents estimates of the relationship between per se laws and traffic fatalities distinguishing between per se states that have an active DRE program and states that do not.[26] In the specification with state-specific linear time trends, the adoption of a per se law is associated with a 2.1 percent decrease in the traffic fatality, but this estimate is not statistically significant at conventional levels.

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[23] The following states have a per se law and an active DRE program: Arizona, Delaware, Georgia, Indiana, Iowa, Minnesota, Nevada, Pennsylvania, Rhode Island, Utah, and Wisconsin (Lacey et al. 2010).

[24] From a practical standpoint, DRE officers may be called in for their expertise either before or after an arrest is made (Lacey et al. 2010).

[25] Some prosecutors have argued that DRE programs and officers make it more likely to obtain a guilty plea when a driver is arrested for suspicion of drugged driving (Lacey et al. 2010). However, even in states with large DRE programs, many cases go through the evidential and adjudicative process based only on testimony from the initial arresting officer (Lacey et al. 2010).

[26] While Rhode Island has a DRE program, only a handful of DRE officers have been employed at any given time. For example, there were 7 active DRE officers in Rhode Island at the beginning of 2007, but none at the end of the year (Lacey et al. 2010). We experimented with including Rhode Island among the states without a DRE program. This had little effect on the results presented in Table 8.

Per se laws also vary with regard to sanctions. The middle panel of Table 8 presents estimates of the relationship between per se laws and traffic fatalities distinguishing between per se states that require mandatory imprisonment for a first offense and those that do not.[27] The bottom panel of Table 8 presents estimates of the relationship between per se laws and traffic fatalities distinguishing between per se states that require a mandatory period of license revocation for a first offense and those that do not.[28] When state-specific linear time trends are included, per se laws with stricter sanctions for a first offense are positively associated with traffic fatalities; however, neither estimate is statistically distinguishable from zero.

5. CONCLUSION

On November 6, 2012 Washington became the 16th state to pass a per se drugged driving law. Specifically, Initiative 502 legalized the possession of up to one ounce of marijuana for recreational use, but came with the provision that a "limit of five nanograms per milliliter (5 ng/ml) of active THC in the bloodstream will be considered per se evidence of guilt of DUI" (Elliot 2012).[29] This provision was clearly intended to "allay fears that legalizing pot would lead to more impaired drivers on the roads" (Spitzer 2012), and per se drugged driving laws may, in the future, be viewed by voters and policymakers as a necessary complement to legalizing

[27] The following states have a per se law that requires mandatory imprisonment for a first offense: Arizona, Georgia, Iowa, Minnesota, Nevada, Ohio, and Utah (Lacey et al. 2010). The mandatory imprisonment lengths vary from a minimum of 24 hours (Arizona and Georgia) to a maximum of three days (Ohio).

[28] The following states have a per se law that requires a mandatory period of license revocation for a first offense: Arizona, Delaware, Illinois, Indiana, Iowa, Minnesota, Nevada, Ohio, Rhode Island, Utah, Virginia, and Wisconsin (Lacey et al. 2010). The mandatory periods of license revocation vary from a minimum of 30 days (Indiana and Rhode Island) to a maximum of 1 year (Delaware and Virginia).

[29] Opponents of the DUI provision claim the THC limit is not consistent with impairment and will "ensnare innocent individuals," especially those using marijuana for medicinal purposes (Sensible Washington 2012). While Colorado also legalized the use of marijuana for recreational purposes, its law did not contain a DUI provision (Wyatt and Johnson 2012).

marijuana for recreational or medicinal use. While the Obama Administration and the Office of National Drug Control Policy have encouraged all states to adopt per se drugged driving laws (White House 2012d), little is known about their effectiveness.

Our study draws on data from the National Highway Traffic Safety Administration's Fatality Analysis Reporting System for the period 1990-2010 to examine the relationship between per se drugged driving laws and traffic fatalities. Despite the fact that these laws have been touted by politicians and academics as an effective strategy for making our roadways safer (DuPont et al. 2012; White House 2012d), we find no evidence that they reduce traffic fatalities.[30] This basic result holds for a range of subsamples across the driving population and is robust to alternative model specifications. For instance, we find no evidence that per se drugged driving laws affect traffic fatalities by age or by gender. Nor do we find evidence that per se laws reduce traffic fatalities at night or on the weekend, times when the incidence of drugged driving is highest (Compton and Berning 2009). When we focus on laws that are accompanied by a Drug Recognition Expert program or laws that impose stricter sanctions on drivers who test positive, the estimated relationship between per se drugged driving laws and traffic fatalities is still small and statistically indistinguishable from zero.

There are a number of potential explanations for these findings. For instance, it is possible that per se laws increase the costs of driving under the influence of a controlled substance, but the behavioral response is essentially inelastic. It is also possible that our results simply reflect poor policy design. While zero tolerance laws clearly discourage consumption on the extensive margin, they provide minimal disincentive on the intensive margin (Grant 2010).

[30] Because of the relatively long time period under study, the substantial policy variation observed, and the rigorous empirical methods employed, our research does not suffer from some of the critiques used to discredit previous studies on the relationship between alcohol-related policies and traffic fatalities (Grant 2011).

This design flaw is important because heavy users are significantly more dangerous behind the wheel (Kelly et al. 2004; Sewell et al. 2009). Lastly, the simple presence of a law does not guarantee that the public is "aware and cognizant of the change in statutory penalties and hence incorporates this new information into their behavior" (MacCoun et al. 2009, p. 348).

Admittedly, the above arguments are speculative. Given our data, we cannot determine why per se drugged driving laws do not work, and leave this issue to future researchers. However, our results clearly indicate that, as currently implemented, laws that make it illegal to drive with detectable levels of a controlled substance in the system have little to no effect on traffic fatalities.

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Table 1. Per Se Drugged Driving Laws, 1990-2010

	Effective date
Arizona	June 28, 1990
Delaware	July 10, 2007
Georgia	July 1, 2001
Illinois	August 15, 1997
Indiana	July 1, 2001
Iowa	July 1, 1998
Michigan	September 30, 2003
Minnesota	August 1, 2006
Nevada	September 23, 2003
Ohio	August 17, 2006
Pennsylvania	February 1, 2004
Rhode Island	July 1, 2006
Utah	May 2, 1994
Virginia	July 1, 2005
Wisconsin	December 19, 2003

Notes: On November 6, 2012 Washington voters approved Initiative 502, which came into effect on December 1, 2012. It specifies a nonzero threshold for tetrahydrocannabinol, but not for other controlled substances. Information on per se drugged driving laws is available from Lacey et al. (2010).

Table 2. Per Se Drugged Driving Laws and Traffic Fatalities

	(1)	(2)	(3)	(4)
Per se law	-0.120	-0.015	0.016	0.008
	(0.103)	(0.029)	(0.027)	(0.024)
N	1071	1071	1071	1071
R^2	0.014	0.957	0.968	0.979
Year FEs	No	Yes	Yes	Yes
State FEs	No	Yes	Yes	Yes
State covariates	No	No	Yes	Yes
State-specific trends	No	No	No	Yes

*, statistically significant at 10% level; **, at 5% level; ***, at 1% level.

Notes: Each column represents the results from a separate regression. The dependent variable is equal to the natural log of total traffic fatalities per 100,000 population and the covariates are listed in Appendix Table 2. Regressions are weighted using state populations. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 3. The Timing of Per Se Drugged Driving Laws and Traffic Fatalities

	(1)	(2)	(3)
3 years before per se law	…	…	0.035
			(0.024)
2 years before per se law	…	…	0.023
			(0.026)
1 year before per se law	…	…	0.012
			(0.022)
Year of law change	-0.009	-0.017	-0.001
	(0.023)	(0.017)	(0.027)
1 year after per se law	-0.004	-0.008	0.009
	(0.035)	(0.023)	(0.030)
2 years after per se law	0.024	0.020	0.039
	(0.034)	(0.031)	(0.039)
3 years after per se law	0.015	0.013	0.033
	(0.031)	(0.023)	(0.036)
4 years after per se law	0.038	0.044	0.065
	(0.035)	(0.027)	(0.039)
5+ years after per se law	0.013	0.052	0.077
	(0.030)	(0.034)	(0.048)
p-value: joint significance of lags	0.078*	0.103	0.113
N	1071	1071	1071
R^2	0.968	0.979	0.979
Year FEs	Yes	Yes	Yes
State FEs	Yes	Yes	Yes
State covariates	Yes	Yes	Yes
State-specific trends	No	Yes	Yes

*, statistically significant at 10% level; **, at 5% level; ***, at 1% level.

Notes: Each column represents the results from a separate regression. The dependent variable is equal to the natural log of total traffic fatalities per 100,000 population and the covariates are listed in Appendix Table 2. In columns (1) and (2), the omitted category is 1+ years before a per se law was adopted. In column (3), the omitted category is 3+ years before a per se law was adopted. Regressions are weighted using state populations. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 4. Per Se Drugged Driving Laws and Traffic Fatalities by Day and Time

	Fatalities Weekdays	*Fatalities Weekend*	*Fatalities Daytime*	*Fatalities Nighttime*
Per se law	0.004	0.014	0.020	-0.004
	(0.026)	(0.031)	(0.028)	(0.027)
N	1071	1071	1071	1071
R^2	0.970	0.961	0.967	0.966
Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
State covariates	Yes	Yes	Yes	Yes
State-specific trends	Yes	Yes	Yes	Yes

*, statistically significant at 10% level; **, at 5% level; ***, at 1% level.

Notes: Each column represents the results from a separate regression. The dependent variable is equal to the natural log of total traffic fatalities per 100,000 population and the covariates are listed in Appendix Table 2. Regressions are weighted using state populations. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 5. Per Se Drugged Driving Laws and Traffic Fatalities by Age

	Fatalities 15-19	*Fatalities 20-29*	*Fatalities 30-39*	*Fatalities 40-49*	*Fatalities 50-59*	*Fatalities 60+*
Per se law	-0.035	0.029	0.017	0.001	-0.019	0.066*
	(0.043)	(0.027)	(0.032)	(0.031)	(0.031)	(0.036)
N	1071	1071	1071	1071	1071	1071
R^2	0.915	0.939	0.943	0.939	0.876	0.921
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
State covariates	Yes	Yes	Yes	Yes	Yes	Yes
State-specific trends	Yes	Yes	Yes	Yes	Yes	Yes

*, statistically significant at 10% level; **, at 5% level; ***, at 1% level.

Notes: Each column represents the results from a separate regression. The dependent variable is equal to the natural log of total traffic fatalities per 100,000 population and the covariates are listed in Appendix Table 2. Regressions are weighted using the relevant state-by-age populations. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 6. Per Se Drugged Driving Laws and Traffic Fatalities by Gender

	Fatalities Males	*Fatalities Females*
Per se law	0.003	0.026
	(0.025)	(0.029)
N	1071	1071
R^2	0.973	0.960
Year FEs	Yes	Yes
State FEs	Yes	Yes
State covariates	Yes	Yes
State-specific trends	Yes	Yes

*, statistically significant at 10% level; **, at 5% level; ***, at 1% level.

Notes: Each column represents the results from a separate regression. The dependent variable is equal to the natural log of total traffic fatalities per 100,000 population and the covariates are listed in Appendix Table 2. Regressions are weighted using the relevant state-by-sex populations. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 7. Robustness Checks

			Alternative dependent variable transformations		
	Preferred estimate from Table 4	Bordering states only as controls	Fatalities per licensed driver population	Fatalities per vehicle miles traveled	Logistic model $ln\left(\frac{Traffic\ fatalities_{st}}{1 - Traffic\ fatalities_{st}}\right)$
Per se law	0.008	-0.0002	0.001	-0.004	0.010
	(0.024)	(0.024)	(0.021)	(0.022)	(0.060)
N	1071	798	1071	1071	1071
R^2	0.979	0.981	0.975	0.961	0.950
Year FEs	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
State covariates	Yes	Yes	Yes	Yes	Yes
State-specific trends	Yes	Yes	Yes	Yes	Yes

*, statistically significant at 10% level; **, at 5% level; ***, at 1% level.

Notes: Each column represents the results from a separate regression. In the first two columns, the dependent variable is equal to the natural log of total traffic fatalities per 100,000 population and these regressions are weighted using state populations. In the last three columns, the dependent variable is equal to the indicated measure. The regression in the third column is weighted using state licensed driver populations. The regression in the fourth column is weighted using state vehicle miles traveled. The regression in the fifth column is weighted based on the variance of the log-odds ratio dependent variable (Ruhm 1996). The covariates are listed in Appendix Table 2. Standard errors, corrected for clustering at the state level, are in parentheses.

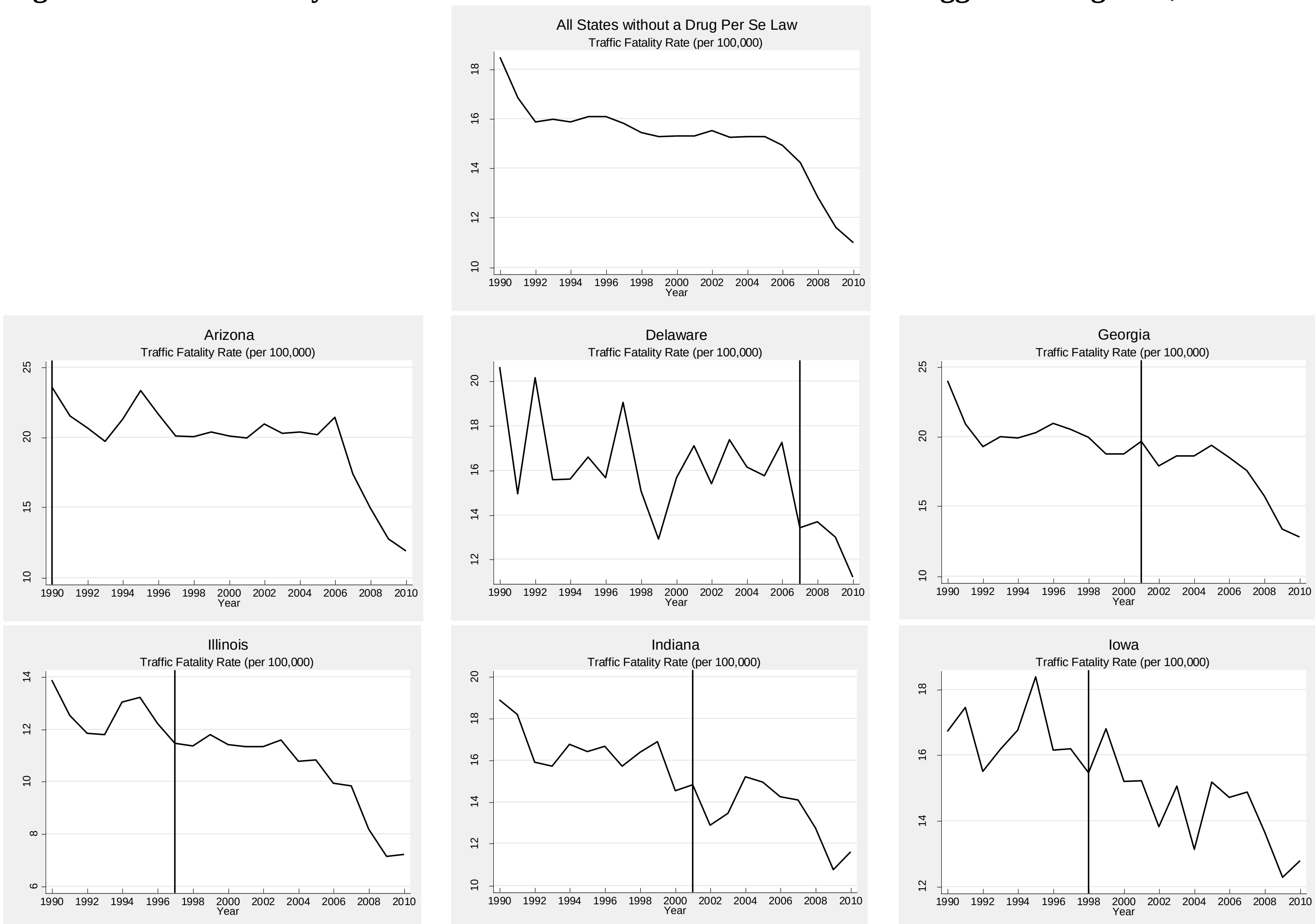
Table 8. Drug Recognition Expert Programs, Mandatory Imprisonment, and Mandatory License Revocation

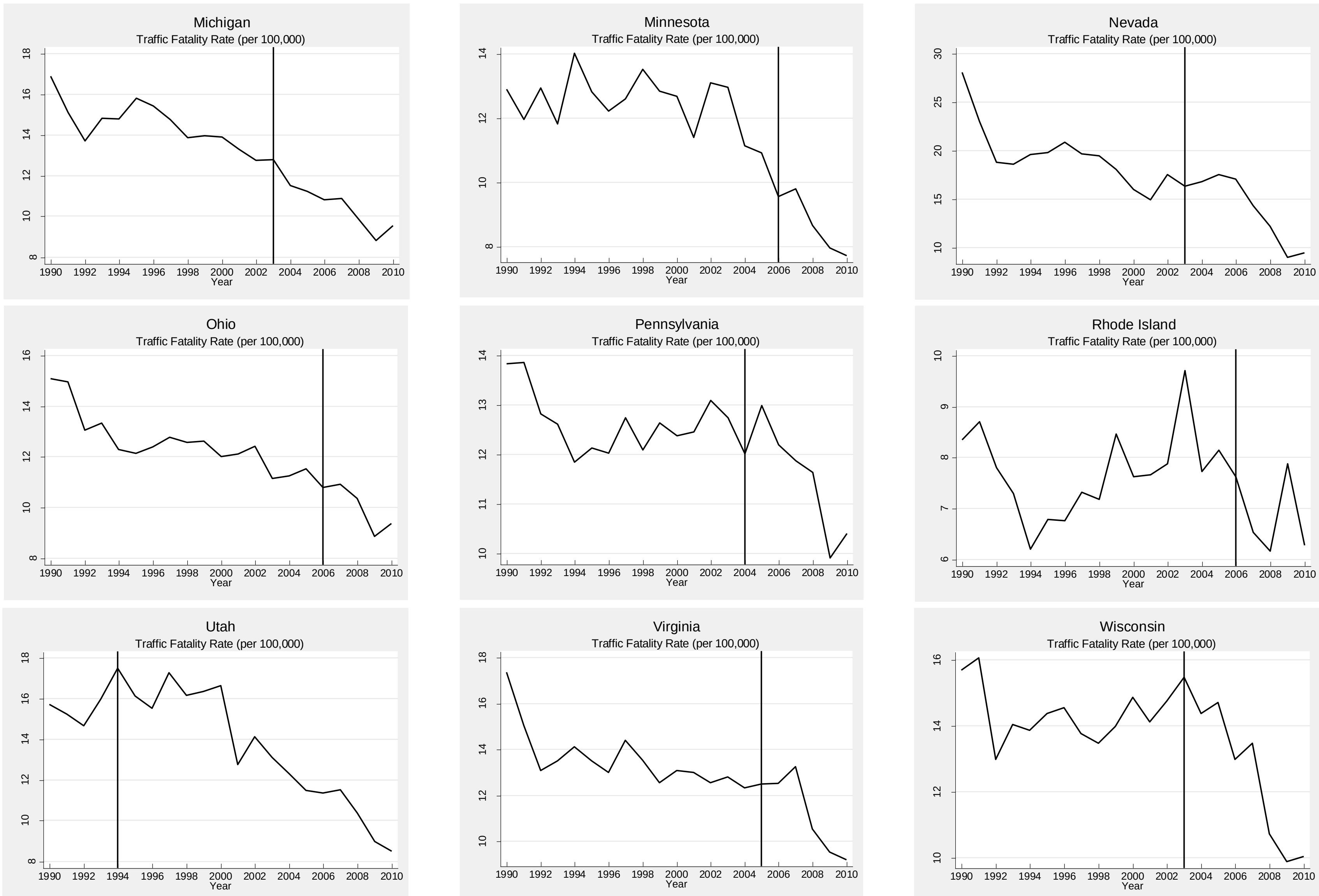
	Traffic Fatalities	*Traffic Fatalities*	*Traffic Fatalities*
DRE Activity			
Per se law with active DRE program	0.011 (0.034)	0.028 (0.035)	-0.021 (0.022)
Per se law without active DRE program	-0.046 (0.038)	-0.001 (0.031)	0.039 (0.034)
N	1071	1071	1071
R^2	0.957	0.968	0.979
Mandatory Imprisonment for 1st Offense			
Per se law with mandatory imprisonment	-0.022 (0.029)	-0.003 (0.036)	0.044 (0.038)
Per se le without mandatory imprisonment	-0.011 (0.040)	0.026 (0.033)	-0.015 (0.026)
N	1071	1071	1071
R^2	0.957	0.968	0.0979
Mandatory License Revocation for 1st Offense			
Per se law with mandatory license revocation	-0.021 (0.024)	0.007 (0.026)	0.024 (0.029)
Per se law without mandatory license revocation	-0.005 (0.065)	0.030 (0.050)	-0.030 (0.021)
N	1071	1071	1071
R^2	0.957	0.968	0.979
Year FEs	Yes	Yes	Yes
State FEs	Yes	Yes	Yes
State covariates	No	Yes	Yes
State-specific trends	No	No	Yes

*, statistically significant at 10% level; **, at 5% level; ***, at 1% level.

Notes: Each column within each panel represents a separate regression. The dependent variable is equal to the natural log of total traffic fatalities per 100,000 population and the covariates are listed in Appendix Table 2. Regressions are weighted using state populations. Standard errors, corrected for clustering at the state level, are in parentheses.

Figure 1. Traffic Fatality Trends in States with and without a Per Se Drugged Driving Law, 1990-2010





Note: Vertical lines represent the year in which a per se drugged driving law came into effect.

Appendix Table 1. Descriptive Statistics for FARS Analysis (Dependent Variables)

Variable	Mean (SD)	Description
Traffic Fatalities	14.58 (5.05)	Number of fatalities per 100,000 population
Variable	Mean (SD)	Denominator
Fatalities Weekdays	8.32 (2.88)	per 100,000 population
Fatalities Weekend	6.22 (2.25)	per 100,000 population
Fatalities Daytime	7.04 (2.59)	per 100,000 population
Fatalities Nighttime	7.42 (2.60)	per 100,000 population
Fatalities 15-19 year-olds	24.55 (9.75)	per 100,000 15- through 19-year-olds
Fatalities 20-29 year-olds	23.59 (8.41)	per 100,000 20- through 29-year-olds
Fatalities 30-39 year-olds	15.45 (6.49)	per 100,000 30- through 39-year-olds
Fatalities 40-49 year-olds	14.00 (5.63)	per 100,000 40- through 49-year-olds
Fatalities 50-59 year-olds	13.22 (4.93)	per 100,000 50- through 59-year-olds
Fatalities 60+ year-olds	17.39 (5.28)	per 100,000 60-year-olds and above
Fatalities Males	20.48 (7.15)	per 100,000 males
Fatalities Females	9.04 (3.30)	per 100,000 females

Note: Weighted means based on the FARS state-level panel for 1990-2010.

Appendix Table 2. Descriptive Statistics for FARS Analysis (Independent Variables)

Variable	Mean (SD)	Description
Per se law[a]	0.142 (0.345)	= 1 if a state had a drug per se law in a given year, = 0 otherwise
Mean age	44.15 (1.40)	Mean age of the state driver population (16 years of age and older)
Unemployment	5.87 (1.87)	State unemployment rate
Income	10.27 (0.156)	Natural logarithm of state real income per capita (2000 dollars)
Miles driven	14.13 (2.05)	Vehicle miles driven per licensed driver (thousands of miles)
Medical marijuana[a]	0.130 (0.334)	= 1 if a state had a medical marijuana law in a given year, = 0 otherwise
Decriminalized[a]	0.330 (0.470)	= 1 if a state had a marijuana decriminalization law in a given year, = 0 otherwise
Graduated driver licensing[a]	0.522 (0.493)	= 1 if a state had a graduated driver licensing law with an intermediate phase in a given year, = 0 otherwise
Primary seatbelt[a]	0.461 (0.494)	= 1 if a state had a primary seatbelt law in a given year, = 0 otherwise
Secondary seatbelt[a]	0.518 (0.494)	= 1 if a state had a secondary seatbelt law in a given year, = 0 otherwise
BAC 0.08[a]	0.584 (0.485)	= 1 if a state had a 0.08 BAC law in a given year, = 0 otherwise
Administrative license revocation[a]	0.721 (0.445)	= 1 if a state had an administrative license revocation law in a given year, = 0 otherwise
Zero Tolerance[a]	0.763 (0.417)	= 1 if a state had a “Zero Tolerance” drunk driving law in a given year, = 0 otherwise
Beer tax	0.245 (0.207)	Real beer tax (2000 dollars)
Speed 70	0.463 (0.499)	= 1 if a state had a speed limit of 70 mph or greater in a given year, = 0 otherwise
Texting ban[a]	0.041 (0.185)	= 1 if a state had a cell phone texting ban in a given year, = 0 otherwise
Hands Free[a]	0.025 (0.150)	= 1 if a state had a “Hands Free” cell phone law in a given year, = 0 otherwise

[a] Takes on fractional values for the years in which laws changed.

Note: Weighted using state populations.