Per Se Drugged Driving Laws and Traffic Fatalities

D. Mark Anderson
Department of Agricultural Economics and Economics
Montana State University
dwight.anderson@montana.edu

Daniel I. Rees
Department of Economics
University of Colorado Denver
daniel.rees@ucdenver.edu

April 2013

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.

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.

Drugged driving is often characterized as a serious and growing threat to public safety (Leinwand 2004; Walsh and DuPont 2007; Westall 2010; Freeman and DyBuncio 2011; DuPont et al. 2012). Indeed, according to data from the 2010 National Survey on Drug Use and Health, 10.6 million Americans reported driving under the influence of an illicit drug in the past year; in comparison, 28.8 million Americans reported driving under the influence of alcohol (Substance Abuse and Mental Health Services Administration 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.

The Office of National Drug Control Policy (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 2012a). In an effort to achieve this goal, R. Gil Kerlikowske, the director of the ONDCP, has urged states to pass per se drugged driving laws (Kerlikowske 2012a, White House 2012b). The Governors Highway Safety Association (GHSA) and Mothers Against Drunk Driving (MADD) have also expressed strong support for per se drugged driving laws.[1] However, aside from anecdotal evidence that these laws make drugged driving easier to prosecute (Lacey et al. 2010, p. 5), 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. As predicted by the Beckerian model of crime (Becker 1968), we find evidence of a negative relationship between per se laws and traffic fatalities in the cross section. However, when we control 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, laws

[1] On its website, the GHSA argues that reducing drug-impaired driving should be a "national priority" and encourages states to "adopt drug per se (zero tolerance) drug impairment laws." In 2011 Kerlikowske and the President of MADD, Jan Withers, announced a new partnership to raise public awareness regarding the consequences of drugged driving. Kerlikowske was quoted as saying:

> Research shows that drugs have adverse effects on judgment, reaction time, and motor skills – all vital requirements for responsible driving…I can think of no greater organization with which to partner to save lives on our roadways than MADD. For decades, MADD has been a lynchpin in our Nation's efforts to make our roadways safer and I am proud to join them to help raise public awareness regarding the devastating consequences of drugged driving (White House 2012b).

prohibiting the operation of a motor vehicle with drugs (or drug metabolites) in the system have no discernible impact on traffic fatalities.

2. BACKGROUND

2.1. Substance use and driving

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).[2] Although a number of etiological studies have shown a positive association

[2] 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.

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 2013), but it is exceedingly difficult to account for the influence of other, difficult-to-observe factors potentially correlated with marijuana. Such factors could include, but are certainly not limited to, personality and an individual's attitude towards risk.[3]

If the relationship between marijuana use and the risk of collision is entirely spurious, per se laws could still be related to traffic fatalities through the use of other drugs. Nine percent of U.S. drivers killed in traffic accidents test positive for stimulants and 6 percent test positive for narcotics (Brady and Li 2013). Despite the fact that these drugs are used by a non-trivial fraction of drivers in the Unites States, 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.[4] 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).

---

[3] 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).

[4] 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.

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 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. Supporters claim these laws deter drugged driving and, as a consequence, improve roadway safety (DuPont 2004; Walsh and DuPont 2007; Dupont et al. 2012; Wood 2013).[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). Moreover, the passage of a per se law does not guarantee public awareness. Becker's theory of crime is based on the assumption that individuals rationally weigh the expected costs and benefits of committing an offense (Becker 1968). If very few residents of per se states actually recognize that the probability of being convicted for drugged driving has

[5] For instance, in a letter to *The Washington Post*, Walsh and DuPont (2007) encouraged states to pass per se laws to combat "the epidemic of drugged driving." They went on to describe "prosecutors who are restricted by antiquated laws…and parents whose innocent children have been injured or killed" because of government inaction. California State Senator Lou Correa, who recently introduced a zero-tolerance drugged driving bill, is quoted on his website (http://sd34.senate.ca.gov/) as saying:

> Driving under the influence of illegal drugs is dangerous and cannot be tolerated. Creating a zero tolerance drugged driving policy will equip law enforcement with the tools needed to keep our communities and roads safe…If you have drugs in your system you should not be on the road.

increased, then there is little reason to expect a change in behavior or an improvement in roadway safety.[6]

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.[7] 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 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

[6] More generally, Kleck et al. (2005, p. 627) argued that the "validity of rational decision-making theories does not require a perfect correspondence between contingencies and perceptions of them, but does require some correspondence if the theories are to have any explanatory or predictive power." Williams et al. (1980) found that perceptions of penalties reflect "public preferences as to appropriate sanctions for crimes and not necessarily actual knowledge of statutory penalties" (p. 105). MacCoun et al. (2009) found that beliefs about whether marijuana possession could lead to jail time were essentially unrelated to the decriminalization of marijuana. See Apel (2013) for a review of the research on the relationship between perceptions and criminal sanctions.

[7] 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.

concluded that BAC 0.08 laws "have no measurable effects on traffic fatality rates" (Freeman 2007, p. 306).[8]

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).[9] In contrast, 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. Grant (2010, p. 769) concluded by noting that Prohibition, "the strongest zero tolerance law ever passed", had little effect on alcohol consumption.[10]

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

---

[8] 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.

[9] 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.

[10] Specifically, Grant (2010, p. 769) wrote:

> Perhaps it is not surprising that zero tolerance laws are ineffective. After all, the strongest zero tolerance law ever passed—Prohibition—had a relatively small effect on alcohol consumption in the long term, according to Dills, Jacobson, and Miron (2005).

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.

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.[11]

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

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

where $\textit{Traffic Fatalities}_{st}$ is equal to the number of traffic fatalities per 100,000 population of state s in year t.[12] 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

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

[12] 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.

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.

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

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

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 $\boldsymbol{X_{st}}$.[13]

[13] 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

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

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

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).[14]

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 $\textit{Fatalities Nighttime}_{st}$.[15] In addition, we estimate (3) replacing $\textit{Traffic Fatalities}_{st}$ with: $\textit{Fatalities Males}_{st}$, $\textit{Fatalities Females}_{st}$, and a series of fatality rates corresponding to specific age groups (i.e., 15 through 19 years of ages, 20 through 29 years of

---

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

[14] 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).

[15] 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*.

age, 30 through 39 years of age, etc.).[16] Driving under the influence of illicit drugs is especially prevalent among teenagers and young adults (Lacey et al. 2007; O'Malley and Johnston 2007; Substance Abuse and Mental Health Services Administration 2011), suggesting the relationship between adopting a per se standard and traffic fatalities should be strongest when we focus on these groups. The ONDCP has emphasized the problem of drugged driving among teenagers and young adults (White House 2012a, 2012b, 2012c) and has issued a toolkit for parents containing activities “designed to raise awareness about drugged driving” (White House 2012d, p. 2).

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.[17]

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

---

[16] *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.

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

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 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.[18]

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 $\textit{Per se law}_{st}$ 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.

[18] 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. Estimating the relationship between per se laws and traffic fatalities at the monthly level produced estimates that were similar to those presented in Table 2.

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.[19] 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 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.[20] 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.

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

[20] 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 (2012) commented on the work of Yörük and Yörük (2011).

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, 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 illustrates 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 teenagers and young adults is of particular interest given the ONDCP's focus on these groups (White House 2012a, 2012b, 2012c, 2012d).

Among 15- through 19-year-olds, the estimate of β_1 is negative, but is not statistically significant at conventional levels.[21] 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

[21] 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.

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 drive 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 (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 2 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 $Traffic\ Fatalities_{st}$.[22] Second, we use the traffic fatality rate per vehicle miles traveled.[23] Lastly, we

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

consider a logistic transformation often used by researchers working in this area (e.g. Ruhm 1996; Young and Likens 2000; Dills 2010).[24] Regardless of the dependent variable used, there is little evidence to support the hypothesis that per se laws reduce traffic fatalities.

4.3. Interstate Heterogeneity

Eleven of the states that have enacted per se laws also have a Drug Recognition Expert (DRE) program.[25] 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).[26] 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).[27]

---

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

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

[25] 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).

[26] 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).

[27] 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).

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.[28] 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 rate, but this estimate is not statistically significant at conventional levels.

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.[29] 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.[30] 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.[31]

---

[28] 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.

[29] 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).

[30] 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).

[31] We also examined the relationship between per se laws and traffic fatalities distinguishing between zero tolerance states and states that set nonzero thresholds for controlled substances. Controlling for state-specific linear time trends, the adoption of a zero tolerance per se law was associated with a 2 percent decrease in the traffic fatality rate, but this estimate was not statistically significant at conventional levels. These results were omitted for the sake of brevity, but are available from the authors upon request.

5. CONCLUSION

On November 6, 2012 Washington became the 16^{th} 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).[32] 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 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 2012a, 2012c), 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 as an effective strategy for making our roadways safer (Walsh and DuPont 2007; DuPont et al. 2012; White House 2012a, 2012c), we find no evidence that they reduce traffic fatalities. 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

[32] 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).

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 at least 4 possible explanations for these findings. First, simply passing a per se law does not guarantee public awareness. Although no previous study has explored the degree to which the public tracks changes in drugged driving laws, MacCoun et al. (2009) found that beliefs about whether marijuana possession could lead to jail time were only weakly related to the decriminalization of marijuana.

Second, even if residents of per se states recognize that the expected cost of driving under the influence of a controlled substance has increased, they may not respond by changing their behavior. A number of studies have, in fact, failed to find evidence of a relationship between dramatic, well-publicized changes in criminal sanctions and substance use (MacCoun and Reuter 2001; Dills et al. 2004; Hughes and Stevens 2010).[33]

Third, although more than 10 percent of U.S. drivers killed in traffic accidents test positive for cannabinols (Brady and Li 2013), evidence with regard to the causal relationship between marijuana use and roadway safety is decidedly mixed. Laboratory studies have shown that THC slows reaction times and impairs hand-eye coordination, but simulator and driving-course studies suggest that marijuana users compensate for these effects by slowing down and

[33] Moreover, there is little evidence that prominent drug prevention programs such as Project D.A.R.E impact behavior (Ennett et al 1994; West and O'Neal 2004). Anderson (2010) examined methamphetamine use among high school students after the introduction of the Montana Meth Project (MMP), an advertising campaign designed to curb demand. Despite anecdotal evidence to the contrary, Anderson (2010) found little evidence that the campaign appreciably impacted the behavior of Montana high school students.

increasing the distance between themselves and the car in front of them (Kelly et al. 2004; Sewell et al. 2009).

Finally, it is possible that our results simply reflect poor policy design. Light and moderate users of substances such as marijuana, who can be difficult to convict if the prosecution is forced to demonstrate impairment, are unambiguously discouraged from driving when a zero-tolerance standard is adopted (Grant 2010). In contrast, per se laws may not change the incentives facing heavy users to the same degree: even in the absence of a zero-tolerance standard, heavy users are presumably at substantial risk of being convicted of operating a motor vehicle while impaired. This design flaw is potentially important because there is evidence that heavy use poses a greater threat to road safety than moderate and light use (Ramaekers et al. 2000; Hartman and Huestis 2013).

The National Survey on Drug Use and Health (NSDUH), the best source of information on substance use among adults living in the United States, asks respondents whether they drove under the influence of an illicit drug in the past year. This information could, in theory, be used to explore whether moderate or heavy users respond to the adoption of a per se standard.[34] 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.

[34] It should be noted, however, that the NSDUH does not typically provide individual-level data with state identifiers to researchers. Our attempts at obtaining these data were politely rejected.

REFERENCES

Abouk, Rahi and Scott Adams. Forthcoming. "Texting Bans and Fatal Accidents on Roadways: Do they Work? Or do Drivers Just React to Announcements of Bans? *American Economic Journal: Applied Economics.*

Anderson, D. Mark. 2010. "Does Information Matter? The Effect of the Meth Project on Meth Use among Youths." *Journal of Health Economics* 29: 732-742.

Anderson, D. Mark, Benjamin Hansen and Daniel I. Rees. Forthcoming. "Medical Marijuana Laws, Traffic Fatalities, and Alcohol Consumption." *Journal of Law and Economics*.

Apel, Robert. 2013. "Sanctions, Perceptions, and Crime: Implications for Criminal Deterrence *Journal of Quantitative Criminology* 29: 67-101

Asbridge, Mark, Jill Hayden and Jennifer Cartwright. 2012. "Acute Cannabis Consumption and Motor Vehicle Collision Risk: Systematic Review of Observational Studies and Meta-Analysis." *British Medical Journal* 344: e536.

Becker, Gary. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76: 169-217.

Bertrand, Marianne, Esther Duflo and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119: 249-276.

Brady, Joanne E. and Guohua Li. 2013. "Prevalence of Alcohol and Other Drugs in Fatally Injured Drivers." *Addiction* 108: 104-114.

Burian, Scott, Anthony Liguori and John Robinson. 2002. "Effects of Alcohol on Risk-Taking During Simulated Driving." *Human Psychopharmacology* 17: 141-150.

Carpenter, Christopher. 2004. "How do Zero Tolerance Drunk Driving Laws Work?" *Journal of Health Economics* 23: 61-83.

Carpenter, Christopher. 2007. "Heavy Alcohol Use and Crime: Evidence from Underage Drunk-Driving Laws." *Journal of Law and Economics* 50: 539-557.

Carpenter, Christopher and Mark Stehr. 2008. "The Effects of Mandatory Seatbelt Laws on Seatbelt Use, Motor Vehicle Fatalities, and Crash-Related Injuries among Youths." *Journal of Health Economics* 27: 642-662.

Chaloupka, Frank and Adit Laixuthai. 1997. "Do Youths Substitute Alcohol and Marijuana? Some Econometric Evidence." *Eastern Economic Journal* 23: 253-275.

Chaloupka, Frank, Henry Saffer and Michael Grossman. 1991. "Alcohol Control Policies and

Motor Vehicle Fatalities." NBER Working Paper No. 3831.

Cohen, Alma and Liran Einav. 2003. "The Effects of Mandatory Seat Belt Laws on Driving Behavior and Traffic Fatalities." *Review of Economics and Statistics* 85: 828-843.

Compton, Richard and Amy Berning. 2009. "Results of the 2007 National Roadside Survey of Alcohol and Drug Use by Drivers." Washington, D.C.: National Highway Traffic Safety Administration.

Cotti, Chad and Nathan Tefft. 2011. "Decomposing the Relationship Between Macroeconomic Conditions and Fatal Car Crashes During the Great Recession: Alcohol- and Non-Alcohol-Related Accidents." Bates College, Working Paper.

Crost, Benjamin and Santiago Guerrero. 2012. "The Effect of Alcohol Availability on Marijuana Use: Evidence from the Minimum Legal Drinking Age." *Journal of Health Economics* 31: 112-121.

Crost, Benjamin and Daniel I. Rees. 2012. "The Minimum Legal Drinking Age and Marijuana Use: New Estimates from the NLSY97." *Journal of Health Economics* 31: 112-121.

Dee, Thomas. 1999. "State Alcohol Policies, Teen Drinking and Traffic Fatalities." *Journal of Public Economics* 72: 289-315.

Dee, Thomas. 2001. "Does Setting Limits Save Lives? The Case of 0.08 BAC Laws." *Journal of Policy Analysis and Management* 20: 111-128.

Dee, Thomas and William Evans. 2001. "Teens and Traffic Safety." In J. Gruber (Ed.), *An Economic Analysis of Risky Behavior Among Youths*, The University of Chicago Press, Chicago, IL.

Dee, Thomas, David Grabowski and Michael Morrisey. 2005. "Graduated Driver Licensing and Teen Traffic Fatalities." *Journal of Health Economics* 24: 571-589.

Dee, Thomas and Rebecca Sela. 2003. "The Fatality Effects of Highway Speed Limits by Gender and Age." *Economics Letters* 79: 401-408.

DeSimone, Jeffrey and Matthew Farrelly. 2003. "Price and Enforcement Effects on Cocaine and Marijuana Demand." *Economic Inquiry* 41: 98-115.

Dills, Angela. 2010. "Social Host Liability for Minors and Underage Drunk-Driving Accidents." *Journal of Health Economics* 29: 241-249.

Dills, Angela, Mireille Jacobson and Jeffrey Miron. 2005. "The Effect of Alcohol Prohibition on Alcohol Consumption: Evidence from Drunkenness Arrests." *Economics Letters* 86: 2 79–284.

DiNardo, John and Thomas Lemieux. 2001. “Alcohol, Marijuana, and American Youth: The Unintended Consequences of Government Regulation.” *Journal of Health Economics* 20: 991-1010.

Dupont, Robert L. 2004. "Drugs and Driving." *USA Today*. October 28.

DuPont, Robert, Robert Voas, Michael Walsh, Corinne Shea, Stephen Talpins and Mark Neil. 2012. “The Need for Drugged Driving *Per Se* Laws: A Commentary.” *Traffic Injury Prevention.*

Eisenberg, Daniel. 2003. “Evaluating the Effectiveness of Policies Related to Drunk Driving.” *Journal of Policy Analysis and Management* 22: 249-274.

Elliot, Steve. 2012. “Washington Becomes 2nd State in One Day to Legalize Marijuana.” *Toke of the Town*. November 6. Available at: http://www.tokeofthetown.com/2012/11/washington_becomes_2nd_state_in_one_day_to_legaliz.php.

Ennett, Susan T., Nancy S. Tobler, Christopher Ringwalt and Robert L. Flewelling. 1994. “How Effective is Drug Abuse Resistance Education? A Meta-Analysis of Project DARE Outcome Evaluations.” *American Journal of Public Health* 84: 1394–1401.

Farmer, Charles, Richard Retting and Adrian Lund. 1999. “Changes in Motor Vehicle Occupant Fatalities After Repeal of the National Maximum Speed Limit.” *Accident Analysis and Prevention* 31: 537-543.

Farrelly, Matthew, Jeremy Bray, Gary Zarkin, Brett Wendling and Rosalie Pacula. 1999. “The Effects of Prices and Policies on the Demand for Marijuana: Evidence from the National Household Surveys on Drug Abuse.” NBER Working Paper No. 6940.

Freeman, Donald. 2007. “Drunk Driving Legislation and Traffic Fatalities: New Evidence on BAC 08 Laws.” *Contemporary Economic Policy* 25: 293-308.

Freeman, David and Monica DyBuncio. 2011. “Drugged Driving Report Shows High Toll Among Young.” *CBS News*. October 13. Available at: http://www.cbsnews.com/8301-504763_162-20119985-10391704.html

French, Michael, Gulcin Gumus and Jenny Homer. 2009. “Public Policies and Motorcycle Safety.” *Journal of Health Economics* 28: 831-838.

Grant, Darren. 2010. “Dead on Arrival: Zero Tolerance Laws Don’t Work.” *Economic Inquiry* 48: 756-770.

Greenstone, Michael. 2002. “A Reexamination of Resource Allocation Responses to the 65-mph Speed Limit.” *Economic Inquiry* 40: 271-278.

Hartman, Rebecca and Marilyn Huestis. 2013. "Cannabis Effects on Driving Skills." *Clinical Chemistry* 59: 478-92.

Hughes, Caitlin Elizabeth and Alex Stevens. 2010. "What Can we Learn from the Portuguese Decriminalization of Illicit Drugs?" *British Journal of Criminology* 50: 999-1022.

Jones, Alan. 2005. "Driving Under the Influence of Drugs in Sweden with Zero Concentration Limits in Blood for Controlled Substances." *Traffic Injury Prevention* 6: 317-322.

Kelly, Erin, Shane Drake and Joanne Ross. 2004. "A Review of Drug Use and Driving: Epidemiology, Impairment, Risk Factors and Risk Perceptions." *Drug and Alcohol Review* 23: 319-344.

Kerlikowske, R. Gil. 2012a. "Guest Commentary: Driving High is a Threat to Public." *The Denver Post*. March 4. Available at: http://www.denverpost.com/opinion/ci_20083104.

Kleck, Gary, Brion Sever, Spencer Li and Marc Gertz. 2005. "The Missing Link in General Deterrence Research." *Criminology* 43: 623-660.

Kolko, Jed. 2009. "The Effects of Mobile Phones and Hands-Free Laws on Traffic Fatalities." *B.E. Journal of Economic Analysis and Policy (Contributions)* 9: 1-26.

Lacey, John H., Tara Kelley-Baker, Debra Furr-Holden, Robert B. Voas, Eduardo Romano, Anthony Ramirez, Katharine Brainard, Christine Moore, Pedro Torres and Amy Berning. 2009. "2007 National Roadside Survey of Alcohol and Drug Use by Drivers: Drug Results." Washington, D.C.: National Highway Traffic Safety Administration.

Lacey, John, Katharine Brainard and Samantha Snitow. 2010. "Drug Per Se Laws: A Review of Their Use in States." Washington, D.C.: National Highway Traffic Safety Administration.

Ledolter, Johannes and K.S. Chan. 1996. "Evaluating the Impact of the 65 mph Maximum Speed Limit on Iowa Rural Interstates." *American Statistician* 50: 79-85.

Liang, Lan and Jidong Huang. 2008. "Go Out or Stay In? The Effects of Zero Tolerance Laws on Alcohol Use and Drinking and Driving Patterns among College Students." *Health Economics* 17: 1261-1275.

Leinwand, Donna. 2004. "Growing Danger: Drugged Driving." *USA Today*. October 21. Available at: http://usatoday30.usatoday.com/news/nation/2004-10-21-cover-drugged-driving_x.htm.

MacCoun, Robert, Rosalie Pacula, Jamie Chriqui, Katherine Harris and Peter Reuter. 2009. “Do Citizens Know Whether Their State Has Decriminalized Marijuana? Assessing the Perceptual Component of Deterrence Theory.” *Review of Law and Economics* 5: 347-371.

MacCoun, Robert and Peter Reuter. 2001. “Evaluating Alternative Cannabis Regimes.” *British Journal of Psychiatry*. 178: 123-128.

MacDonald, Scott, Robert Mann, Mary Chipman, Basia Pakula, Pat Erickson, Andrew Hathaway and Peter MacIntyre. 2008. “Driving Behavior Under the Influence of Cannabis or Cocaine.” *Traffic Injury Prevention* 9: 190-194.

Marczinski, Cecile, Emily Harrison and Mark Fillmore. 2008. “Effects of Alcohol on Simulated Driving and Perceived Driving Impairment in Binge Drinkers.” *Alcoholism: Clinical and Experimental Research* 32: 1329-1337.

Miron, Jeffrey and Elina Tetelbaum. 2009. “Does the Minimum Legal Drinking Age Save Lives?” *Economic Inquiry* 47: 317-336.

Miranda, Nannette. 2013. “Drugged-Driving Bill Introduced by Sen. Correa Makes Any Detectable Amount Illegal.” *ABC News.* February 19. Available at: http://abclocal.go.com/kabc/story?section=news/state&id=8999069.

Model, Karyn. 1993. “The Effect of Marijuana Decriminalization on Hospital Emergency Room Drug Episodes: 1975-1978.” *Journal of the American Statistical Association* 88: 737-747.

National Council on Alcoholism and Drug Dependence. 2012. “Drugged Driving.” Available at: http://www.ncadd.org/index.php/learn-about-drugs/drugged-driving.

O'Malley, Patrick and Lloyd Johnston. 2007. “Drugs and driving by American high school seniors, 2001-2006.” *Journal of Studies on Alcohol and Drugs* 68: 834-842.

Pacula, Rosalie. 1998. “Does Increasing the Beer Tax Reduce Marijuana Consumption?” *Journal of Health Economics* 17: 557-585.

Ramaekers, J., H. Robbe and J. O'hanlon. 2000. “Marijuana, Alcohol and Actual Driving Performance.” *Human Psychopharmacology* 15: 551-558.

Robbe, Hindrick and James O’Hanlon. 1993. “Marijuana and Actual Driving Performance.” Washington, D.C.: U.S. Department of Transportation, National Highway Traffic Safety Administration.

Ronen, Adi, Pnina Gershon, Hana Drobiner, Alex Rabinovich, Rachel Bar-Hamburger, Raphael Mechoulam, Yair Cassuto and David Shinar. 2008. “Effects of THC on Driving Performance, Physiological State and Subjective Feelings Relative to Alcohol.” *Accident Analysis and Prevention* 40: 926-934.

Ruhm, Christopher.1996. "Alcohol Policies and Highway Vehicle Fatalities." *Journal of Health Economics* 15: 435-454.

Saffer, Henry and Frank Chaloupka. 1999. "The Demand for Illicit Drugs." *Economic Inquiry* 37: 401-411.

Scott, Emilee Mooney. 2010. "Marijuana Decriminalization." Office of Legislative Research Report. Available at: http://www.cga.ct.gov/2010/rpt/2010-R-0204.htm.

Sensible Washington. 2012. "Moving Forward." *Sensible Washington*, November 6. Available at: http://sensiblewashington.org/blog/.

Sewell, R. Andrew, James Poling and Mehmet Sofuoglu. 2009. "The Effect of Cannabis Compared with Alcohol on Driving" *The American Journal on Addictions*. 18: 185–193.

Spitzer, Gabriel. 2012. "Will Legal Marijuana Make our Roads More Dangerous?" *KPLU News*, October 8. Available at: http://www.kplu.org/post/will-legal-marijuana-make-our-roads-more-dangerous.

Substance Abuse and Mental Health Services Administration. 2011. "Results from the 2010 National Survey on Drug Use and Health: Summary of National Findings." Rockville, MD: Center for Behavioral Health Statistics and Quality.

Voas, Robert, A. Scott Tippetts and James Fell. 2003. "Assessing the Effectiveness of Minimum Legal Drinking Age and Zero Tolerance Laws in the United States." *Accident Analysis and Prevention* 35: 579-587.

Walsh, Michael, Johan Gier, Asbjorg Christopherson and Alain Verstraete. 2004. "Drugs and Driving." *Traffic Injury Prevention* 5: 241-253.

Walsh, Michael J. and Robert L. DuPont. 2007. "The Drugged Driving Epidemic." *Washington Post*. June 17. Available at: http://www.washingtonpost.com/wp-dyn/content/article/2007/06/15/AR2007061501895.html.

West, Steven L. and Keri K. O'Neal. 2004. "Project D.A.R.E. Outcome Effectiveness Revisited." *American Journal of Public Health* 94: 1027–1029.

Westall, Sylvia. 2010. "U.S. Says 'Drugged Driving' Growing Threat" *Reuters*, March 9. Available at: http://www.reuters.com/article/2010/03/09/us-usa-drugs-idUSTRE6284W520100309.

White House. 2012a. “Drugged Driving.” Office of National Drug Control Policy. Available at: http://www.whitehouse.gov/ondcp/drugged-driving.

White House. 2012b. “White House Drug Policy Director and Mothers Against Drug Driving Unite to Combat Drugged Driving; Call on Parents to Act to Reduce Significant Public Safety Threat.” Available at: http://www.whitehouse.gov/sites/default/files/ondcp/white_house_drug_policy_director_and_mothers_against_drunk_driving_unite_to_combat_drugged_driving.pdf.

White House. 2012c. “Working to Reduce Drugged Driving and Protect Public Health and Safety.” Office of National Drug Control Policy. Available at: http://www.whitehouse.gov/sites/default/files/page/files/drugged_driving_fact_sheet_4-6-12.pdf.

White House. 2012d. “Teen Drugged Driving: Parent, Coalition and Community Group Activity Guide.” Office of National Drug Control Policy. Available at: http://www.whitehouse.gov/sites/default/files/ondcp/issues-content/drugged_driving_toolkit.pdf.

Williams, Kirk, Jack Gibbs and Maynard Erickson. 1980. “Public Knowledge of Statutory Penalties: The Extent and Basis of Accurate Perception.” *Pacific Sociological Review* 23: 105-128.

Williams, Jenny, Rosalie Pacula, Frank Chaloupka and Henry Wechsler. 2004. “Alcohol and Marijuana Use among College Students: Economic Complements or Substitutes?” *Health Economics* 13: 825-843.

Wyatt, Kristen and Gene Johnson. 2012. “With Marijuana Legal in Colorado and Washington, Police Worry About Stoned Driving.” *Huffington Post*, November 15. Available at: http://www.huffingtonpost.com/2012/11/15/with-pot-legal-police-wor_n_2136034.html.

Yörük, Bariş and Ceren Yörük. 2011. “The Impact of Minimum Legal Drinking Age Laws on Alcohol Consumption and Marijuana Use: Evidence from a Regression Discontinuity Design Using Exact Date of Birth.” *Journal of Health Economics* 30: 740-753.

Young, Douglas and Agnieszka Bielinska-Kwapisz. 2006. “Alcohol Prices, Consumption, and Traffic Fatalities.” *Southern Economic Journal* 72: 690-703.

Young, Douglas and Thomas Likens. 2000. “Alcohol Regulation and Auto Fatalities.” *International Review of Law and Economics* 20: 107-126.

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

	Preferred estimate from Table 2	Bordering states only as controls	Alternative dependent variable transformations: 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.

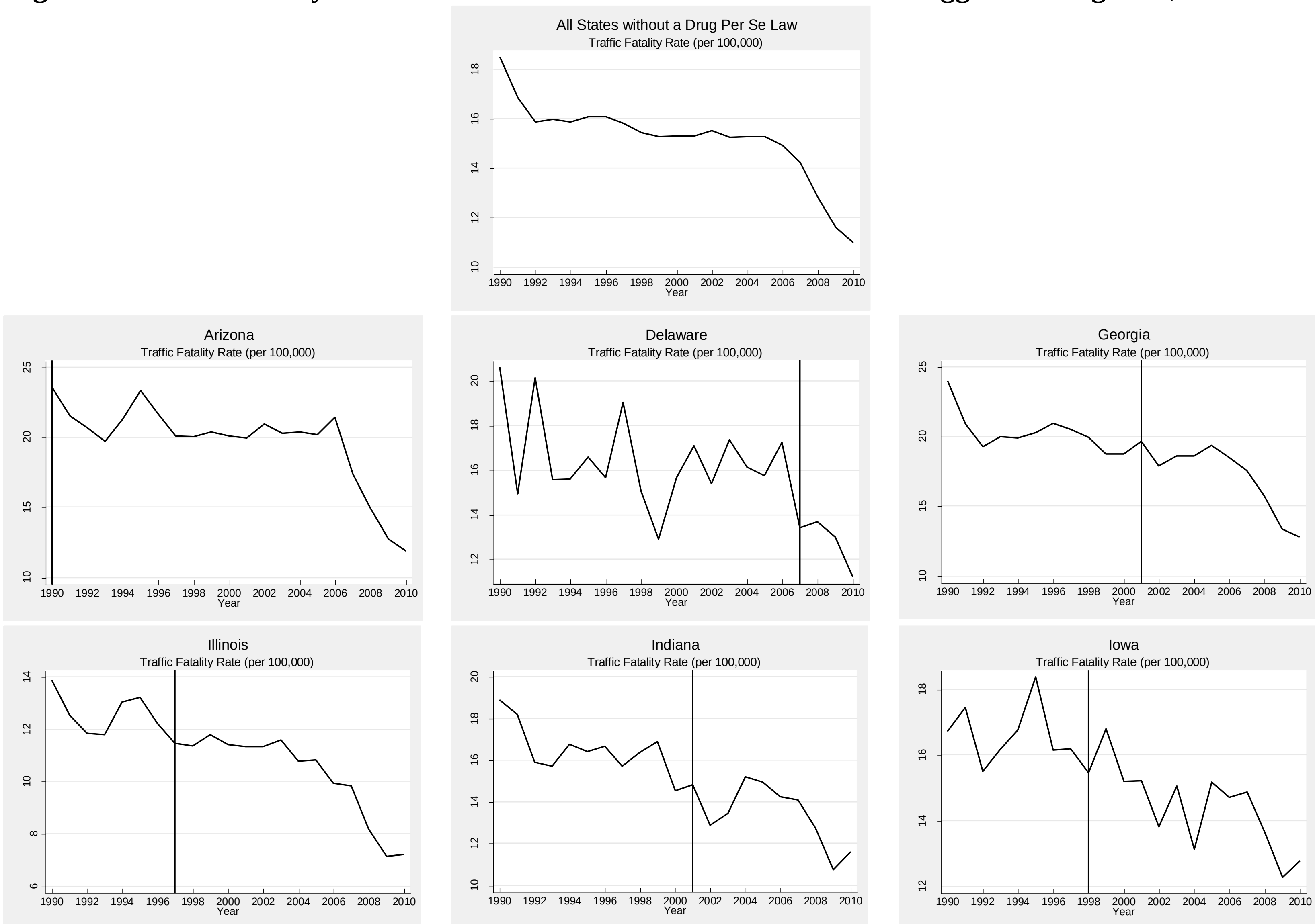
Table 8. Drug Recognition Expert Programs, Mandatory Imprisonment, and Mandatory License Revocation

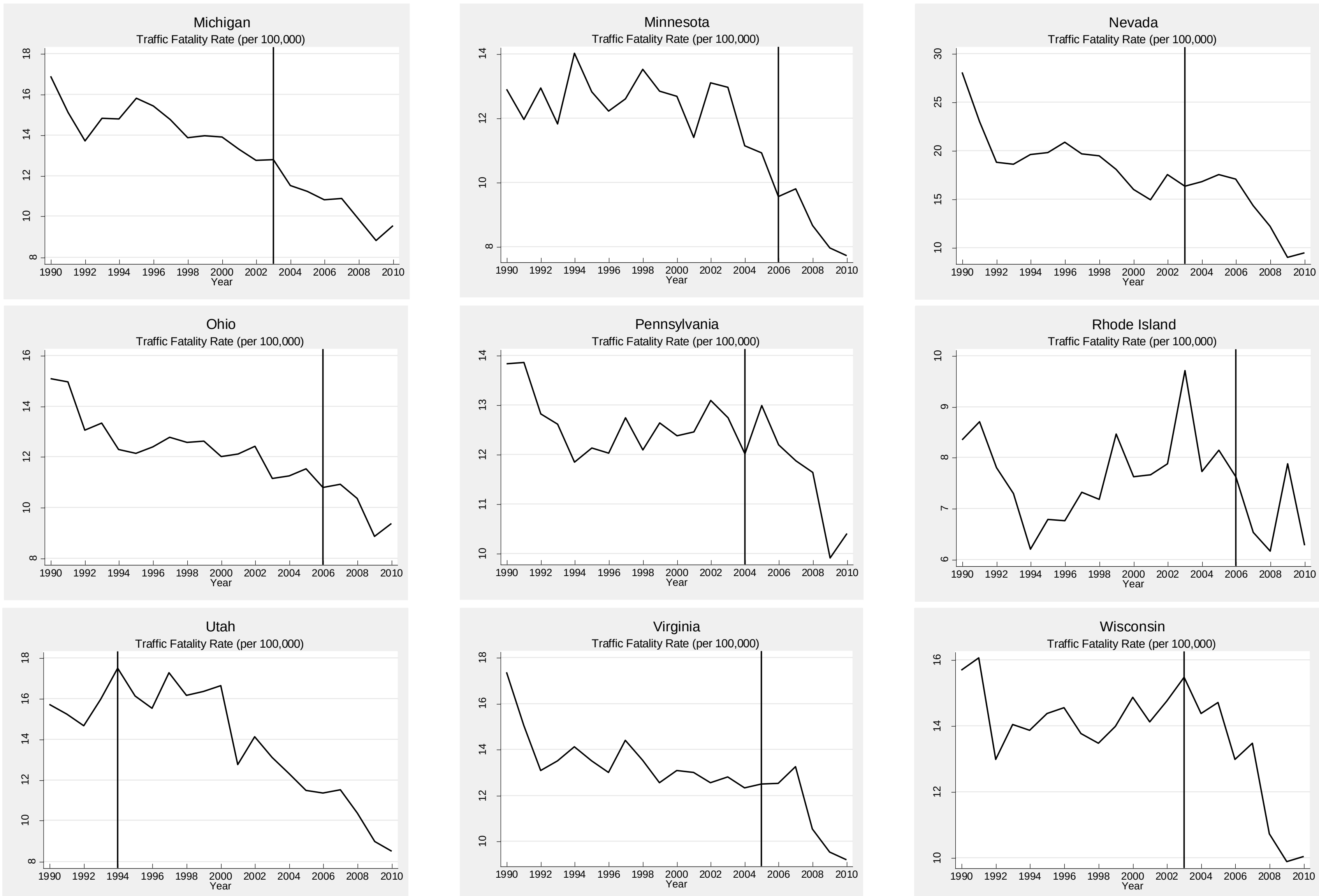
	Traffic Fatalities	*Traffic Fatalities*	*Traffic Fatalities*
DRE Activity			
Per se law with active DRE program	0.011	0.028	-0.021
	(0.034)	(0.035)	(0.022)
Per se law without active DRE program	-0.046	-0.001	0.039
	(0.038)	(0.031)	(0.034)
N	1071	1071	1071
R^2	0.957	0.968	0.979
Mandatory Imprisonment for 1st Offense			
Per se law with mandatory imprisonment	-0.022	-0.003	0.044
	(0.029)	(0.036)	(0.038)
Per se le without mandatory imprisonment	-0.011	0.026	-0.015
	(0.040)	(0.033)	(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	0.007	0.024
	(0.024)	(0.026)	(0.029)
Per se law without mandatory license revocation	-0.005	0.030	-0.030
	(0.065)	(0.050)	(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.