

Long Run and Short Run Effects of Blood Alcohol Limits: Evidence  
from the United States

Craig A. Depken, II  
Department of Economics  
University of Texas at Arlington  
Arlington, Texas 76019-0479  
Office: (817) 272-3290  
Fax: (817) 272-3145  
E-mail: [depken@uta.edu](mailto:depken@uta.edu)

# Long Run and Short Run Effects of Blood Alcohol Limits: Evidence from the United States

## *Abstract*

This paper investigates the impact of lowered BAC limits on alcohol related traffic fatalities during the 1990s. While this is not the first study of this policy change, the innovation here is to consider the short-run and long-run impacts of lowered BAC limits. Unlike previous studies that find significant reductions in traffic fatalities after legal limits are reduced, implicitly assumed to be short-run, this paper shows that once controlling explicitly for enforcement efforts and the severity of penalties the short-run impacts of lowered BAC limits were insignificant during the 1990s. However, the long-run impacts were significant. This study is pertinent because national legislation was passed in 2000 requiring all states to have a legal limit of 0.08 BAC by October 2003. At the time, proponents of the legislation claimed an estimated 600 lives would be saved nation-wide because of the new legal limits, which this study shows was most likely overstated.

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# 1. Introduction

In October 2000, President Bill Clinton signed a transportation appropriations bill containing language requiring all 50 states and the District of Columbia to reduce *per se* Blood Alcohol Content (BAC) limits to 0.08 grams per deciliter of blood by 2003 or risk the withholding of federal highway funds. The bill was heralded by anti-drunk driving advocates who claimed the law would save between 500 and 600 lives per year.

Policy advocates claim that lowered BAC limits, combined with public announcement programs, increased enforcement, and harsher penalties promote less drunk driving thereby reducing the number of accidents caused by impaired drivers. These claims have been supported by numerous, but not all, empirical studies investigating this policy change. Therefore, the true impact of lowered BAC limits remains open to debate.

This study analyzes the impacts of reduced BAC limits, enforcement efforts, and minimum license sanctions (suspension or revocation) on alcohol related fatalities, one of the targets of anti-drunk driving activists. Using a panel comprised of the fifty states from 1991 through 2000, the empirical methodology facilitates a detailed investigation of the impact of BAC law changes in two important dimensions: *between* states that lowered limits voluntarily and those that did not, interpreted as long-run effects, and *within* states that voluntarily reduce BAC limits, interpreted as short-run effects.

The empirical evidence shows that the severity of first offender penalties has a dramatic impact on alcohol related traffic fatalities. Further analysis indicates, counter to many previous studies, that lower BAC limits did not have a statistically significant within-state impact on alcohol related traffic fatalities during the 1990s. However, between-state impacts of lowered BAC limits were statistically significant. These results are consistent with the short-run impacts of lowered BAC limits being small relative to the long-run impacts. The results suggest that anti-drunk driving advocates may achieve more success by focusing on improved enforcement and increased penalties.

The remainder of this paper is structured as follows. The next section offers a brief history of the anti-drunk driving movement, the previous legislation leading up to that which President Clinton signed in 2000 and the previous literature focusing on the effects of lowered BAC limits. Section 3

describes the data employed, the econometric methodology and the empirical results. The final section offers concluding remarks and suggestions for future research.

## 2. The History and Literature of Lowered BAC Limits

The anti-drunk driving movement is a relatively recent phenomenon. The most reliable data on advocacy efforts to reduce drunk driving come from Mothers Against Drunk Driving (MADD), founded in 1980. The social concern over drunk driving must be inferred for the period prior to this group's founding. Drinking and driving has been a social concern since at least the Second World War, as reflected by public service announcements about drunk driving in the immediate post-war period. However, the *New York Times* Master Index indicates that articles about drunk driving appeared occasionally as far back as the 1920's.

While Illinois set a *per se* limit of 0.15 BAC in 1958, indicating that drunk driving was of some concern well before the 1980s, MADD was created to combat the "lenient laws and weak judicial response to the crime of drunk driving" (MADD, 2002). Therefore, for operational purposes the history of the anti-drunk driving movement is taken from the founding of MADD in 1980.<sup>1</sup>

MADD's first major federal legislative success was the *1984 21 National Minimum Drinking Age Law*, which required states to pass such age limits by 1988 or risk losing federal highway funds. This risk was taken (perhaps infamously) by the state of Louisiana which finally raised its minimum drinking age to 21 in 1996, when the law was deemed constitutional by the Louisiana Supreme Court in March of that year. The move was cited as "saving" the state between \$14 million and \$18 million per year in federal highway funds.

The second major national legislation was an amendment to the *Anti-Drug Abuse Act of 1988*, called the *Drunk Driving Prevention Act of 1988*. This legislation offered financial rewards (rather than the threat of financial losses) for states to adopt minimum drinking age laws, open container laws, and administrative license revocation (ALR) programs, which allow judges to suspend or revoke a suspect's license before a conviction of drunk driving is obtained.<sup>2</sup> In the same year, Congress passed language embodied in the *Transportation Equity Act of 1988*, providing financial incentives for states

to reduce their BAC limits to 0.08, and penalizing states that did not pass open container laws and minimum penalties for repeat offenders.

This national legislative agenda culminated (to date) with the language included in the *2000 Department of Transportation Appropriations Act*, which added the threat of withholding federal highway funds if BAC limits have not been reduced to 0.08 BAC by October 2003. States stand to lose federal highway funds at a rate of 2% in 2004, 4% in 2005, and so forth, with the rate of withholding capped at 8% starting in 2007. States that lower BAC limits between 2003 and 2007 can appeal for a (partial) refund of lost tax revenues, whereas states that lower limits afterwards cannot.

National laws are only the most visible and notable “victories” of anti-drunk driving advocates. MADD claims that between 1981 and 2001, more than 2,300 anti-drunk driving laws have been passed in the United States, most at the state level. While this number sounds impressive, many of the laws have seemingly little impact on the larger proportion of the population. For instance, in the state of Illinois, effective January 1, 1998, school bus drivers lose their school bus permits if caught with any trace of alcohol in their system. In Ohio, it is illegal to drive a snowmobile or a riding lawn mower while intoxicated. Notwithstanding the positive probability that someone may cause injury to himself or others while operating such vehicles while intoxicated, the impact of these laws is arguably less than those that focus on operating a traditional automobile.

Despite the limited scope of some laws passed since 1981, it is clear that MADD takes an active role in promoting such legislation and uses the increased number of drunk-driving laws in its rhetoric and advertising. However, the question remains whether MADD is concerned about drunk driving alone or if it has a larger goal in mind. MADD’s own documentation shows that the organization considers the most important target of it’s efforts to be reducing alcohol related traffic fatalities.

MADD initiated the so-called “20 by 2000” campaign in 1990, aimed at reducing alcohol related fatalities nationwide by 20% by the year 2000; they claimed success in 1997. MADD’s goal now is to reduce alcohol related fatalities to less than 11,000 by 2005; an integral part of this program being the language embodied in the 2000 Transportation Appropriations bill. The goals of MADD provide the basis for the empirical approach undertaken in this study.

The literature investigating reduced BAC limits is extensive and appears predominantly in three disciplines: transportation and traffic safety, health and health policy, and economics (including public choice). These studies use various statistical approaches to test the impact of reduced BAC limits on alcohol related traffic fatalities. One approach is difference-in-means tests within states before and after the lower limits are imposed (for example, Foss et al., 1998), which indicates that states experience between a 7 to 10% reduction in alcohol related fatalities after reducing BAC limits (see Shults et al., 2001). The impacts of other variables that might influence alcohol related fatalities are subsumed in the difference-in-means test and may be inappropriately attributed to lower BAC limits.

An alternative approach tests the policy's impact by matching one state with a lower BAC limit with a "comparable" state that does not, e.g., California, which has a limit of 0.08 BAC, with Texas, which does not (see, for example, Hingson, et al., 1996). This methodology often finds a significant impact of lowered BAC limits on alcohol related traffic fatalities and total traffic fatalities. While somewhat similar to the within estimator common to panel data techniques, this methodology has been criticized by other authors (see, for example, Apsler, et al., 1999).

Voas, Tippetts and Fell (2000) investigate the impact of lower BAC limits for all fifty states and the District of Columbia using quarterly data from 1982 through 1999 in a pooled OLS framework and concludes that the benefits of lower BAC limits is greatest when combined with other legal sanctions on driver behavior.<sup>3</sup> They relate the *percentage* of all traffic fatalities that were alcohol related to variables describing socioeconomics, traffic characteristics, a dummy variable for whether a state had a lower BAC limit, a dummy variable that indicates whether a state has an Administrative License Revocation (ALR) program, and linear and quadratic time trends, and find the impact of a lower BAC level is negative and statistically significant. They use their estimate to determine that 590 lives per year would have been saved if all states had a lower BAC limit of 0.08 in 1997, thereby providing support for the estimate generated by Hingson, et al. (1996).

Dee (2001) relates total traffic fatalities in the lower forty-eight states (excluding the District of Columbia) from 1982 through 1998 to a number of state specific traffic laws including 0.08 BAC limits, mandatory seat-belt laws, ALR programs, 65 and 70 mph speed limits, and whether there is minimum

jail time for a first time DUI offense. Employing a fixed effects estimator, he finds that the impact of lower BAC limits is negative and statistically significant. He further estimates that if all states had lowered BAC limits, up to 1,200 lives would have been saved nationwide in 1998.

Additional multivariate analyses focus on various aspects of the drunk driving and policies intended to dissuade the behavior. Mast, Benson and Rasumussen (1999) suggest that higher beer taxes reduce alcohol related traffic fatalities. Chaloupka, Saffer and Grossman (1993) show that state legislations including minimum drinking ages reduce alcohol fatalities. Benson, Rasmussen and Mast (1999) investigate the relationship between alcohol related traffic fatalities and the deterrence effects of several policies intended to reduce alcohol availability and to increase the costs of drunk driving. Using data from the 48 contiguous states from 1984 to 1992, they find that individual policies rarely have a significant effect on alcohol related fatalities, but that deterrence variables are jointly significant in explaining all alcohol related traffic fatalities and those that included at least one person with a BAC greater than 0.10. Their investigation is the template for the empirical work in this study.

### **3. The Impact of Drunk Driving Legislation**

The National Highway Traffic Safety Administration (NHTSA) offers the following list of positives from lowered BAC limits: “Virtually all drivers are substantially impaired at 0.08 BAC ... the risk of being involved in a crash increases substantially at 0.08 BAC ... lowering the BAC limit is a proven countermeasure which will reduce alcohol related traffic fatalities ... 0.08 is a reasonable level to set the limit ... the public supports levels below 0.08 BAC ... [and] most other industrialized nations have set BAC limits at 0.08 or lower” (NHTSA, 2001). While many of these “benefits” may provide good reasons for legislatures to support lower BAC limits, none offers a substantial behavioral reason for the reduced limits.

The first benefit is supported by studies that show driving abilities, including steering, braking and lane changing, are reduced at levels above 0.08 BAC (see, for example, Borkenstein, et al., 1964). However, NHTSA does not provide a rationale for why individuals would change their behavior given a statutory change in BAC limits. If average driving abilities deteriorate substantially at 0.08 BAC or

higher, lower limits may make sense; however, states ignored this evidence by keeping 0.10 BAC limits until Oregon reduced its limit to 0.08 BAC in 1983. The remaining items in NHTSA's list of benefits have little direct economic interpretation.

Yet, there does exist an economic rationale for at least an indirect impact of lowered BAC limits on decision making. This is well understood in the economics of crime, the cornerstone of which is Becker's (1968) approach which suggests that illegal behavior is undertaken in a rational way: the expected benefits must outweigh the expected costs for an individual to engage in illegal behavior. The benefits to driving drunk include personal pride and control, saving the cost of a taxi or bus, and the opportunity costs of abandoning one's vehicle (for some period of time). The expected costs to drunk driving include the (subjective) probability of being caught and convicted of a DUI, the associated explicit and implicit costs (monetary and opportunity) of a DUI conviction, and the probability and costs (monetary, opportunity and psychic) of causing an accident while driving intoxicated. Any variable or policy that reduces (increases) the perceived benefits of driving drunk will discourage (encourage) such behavior. On the other hand, anything that increases (decreases) the perceived costs of driving drunk will dissuade (encourage) the behavior, *ceteris paribus*.

Therefore, BAC limits may reduce drunk driving if sufficient social pressure reduces the (perceived) benefits or increases the expected costs of driving drunk. Moreover, while reduced statutory limits may have no impact on the probability of a collision while driving intoxicated, they may increase the odds of being caught driving drunk, thereby increasing the expected costs of such behavior. Thus, lowered BAC limits may have an indirect effect on driving drunk through the increased odds of being caught driving intoxicated. It is expected that less drunk driving is revealed in reduced alcohol related traffic fatalities. However, the relationship between drunk driving and alcohol related fatalities is most likely not linear (see Brown, Jewell and Richer, 1996); some individuals drive drunk multiple times without causing a fatal accident, whereas others drive drunk once and cause a fatal accident.

If policies targeting drunk driving aim to reduce the frequency of impaired driving, how successful are these policies? Unfortunately, the actual frequency of impaired driving is not currently measurable. Moreover, data on DUI recidivism are not available for the vast majority of states, thereby making

it difficult to determine the impacts that enforcement and penalties have on the decision to drive drunk. However, the state of Wisconsin recently published the total number of DUI convictions over the period 1989 through 2000, including the number of individuals with multiple DUI convictions (Wisconsin Department of Transportation, 2000). These data are replicated in Table 1. As can be seen, the vast majority of those convicted of a DUI were one-time offenders. The proportion of those convicted for a second DUI offense over the given period was 14.7%, whereas three-time offenders were only 7% of those convicted a first time. While the state of Wisconsin did report one individual with 12 convictions and one individual with 13 convictions over the thirteen years reported, these two individuals were clearly outliers. The rate of recidivism in drunk driving (as reflected in multiple convictions) is relatively low.

*<Table 1 about here>*

It is unknown whether other states experience similar DUI recidivism as Wisconsin. However, if Wisconsin is representative of other states, first-time offenders seem to alter their behavior so to avoid the personal costs, both explicit and implicit, that accompany a second DUI conviction.<sup>4</sup> If DUI convictions are reduced through enforcement and punishment, it is reasonable to assume that alcohol related traffic fatalities are also reduced by increased enforcement and penalties. This intuition provides further support for including enforcement and penalties in an analysis of the impacts of reduced BAC limits.

The economic theory of crime also predicts that the severity of penalties alters behavior. It would be best to have a measure of all potential penalties for DUI convictions, at various levels of recidivism, yet such data are not available for the majority of the states during the time period investigated. However, California does advertise its minimum penalties for the first four DUI convictions, both with and without probation (Campbell and Demetrick, 2002). In California, convictions without probation carry the following minimum penalties: For a first offense, minimum jail time is 96 hours, 48 of which must be served continuously, minimum fine is \$390, and license is suspended for six months. For a second offense, minimum jail time is 90 days, minimum fine is \$390, and license is suspended for two years. For a third offense, minimum jail time is 120 days, minimum fine is \$390 and license is revoked

for three years. For a fourth offense, minimum jail time is 16 months, minimum fine is \$390 and license is revoked for four years.

As these examples show, while dollar fines may alter behavior, jail time may have a greater impact on the decision to drive drunk, especially after the first offense. Moreover, while the minimum dollar fine does not change across the different levels of recidivism in California, jail time and license sanctions do co-vary in a positive fashion. As data describing all forms of sanctions are unavailable (although desirable), minimum license sanctions serve as a useful proxy for the severity of penalties imposed at different levels of recidivism.

#### 4. A Panel Data Investigation of Lowered BAC Limits

The data studied here comprise a panel describing the fifty states and the District of Columbia from 1991 through 2000. Following the existing literature, the endogenous variable investigated is the number of alcohol related traffic fatalities. The exogenous variables include state socioeconomic characteristics, variables describing traffic conditions in each state, and variables thought to influence drunk driving: having lower BAC limits, a measure of enforcement and a measure of penalties.

Following Voas et al. (2000), the sample of total alcohol related traffic fatalities is divided into two sub-samples: those that included anyone with a BAC between 0.01 and 0.10, i.e., those who were “less” drunk, and those where at least one individual had a BAC greater than 0.10. These subsamples facilitate investigation in how legal limits, enforcement, and punishment have different (greater?) impacts on those who are driving drunk (with a  $BAC \geq 0.10$ ).

Figure 1 depicts the total number of traffic fatalities, including any individual with a BAC greater than 0.01 and anyone with a BAC greater than 0.10, for the years 1984 through 2000.<sup>5</sup> It is evident that the total number of traffic fatalities has remained relatively stable around the sample mean of 42,755 per year. Moreover, the number of alcohol related fatalities is stable around a mean of 19,985 per year, although alcohol related fatalities as a percentage of total traffic fatalities have been declining over the sample period, as shown in Figure 2.<sup>6</sup>

*<Figures 1 and 2 about here>*

The explanatory variables used in this study describe state socioeconomic and traffic conditions, enforcement levels and severity of penalties. The socioeconomic variables include state per-capita incomes (*PERINC*) and state unemployment rates (*UNEMP*). It is anticipated that states with higher incomes tend to have fewer alcohol related fatalities whereas states with higher unemployment rates tend to have more alcohol related fatalities.

To control for state specific traffic characteristics, millions of vehicle miles driven (*MILES*) and traffic density (*DENSITY*) are included. The number of miles driven is common in previous studies and has been found to correlate strongly (and positively) with the total number of alcohol related fatalities. However, total miles driven does not completely describe the conditions facing drivers. Therefore, traffic density, measured as the ratio between the average miles driven per registered vehicle and the total miles of paved road in the state, is also included. This measure provides a valuable state-specific descriptor of traffic flows.

The impact of traffic density on alcohol related fatalities is ambiguous. First, more dense traffic creates more possibilities for collisions involving drunk drivers, and may therefore increase alcohol related fatalities. However greater traffic density may reduce the speed that (drunk) drivers can attain, thereby reducing the probability that a collision will be severe enough to prove fatal. Moreover, several engineering studies have shown that drunk drivers become more dangerous when monotony increases, such as on dark country roads. Therefore, greater traffic density may actually increase the alertness of the impaired driver, vis-a-vis monotonous driving, thereby reducing the risk of a fatal accident.

The remaining variables match the policy choices Becker's theory predicts influence illegal behavior. A dummy variable indicating whether the state voluntarily imposed lower BAC limits of 0.08 (*BAC08*) is included. To control for enforcement efforts, data gathered from the FBI Uniform Crime Reports (UCR) are utilized. Since 1991, the UCR's have specifically included the number of DUI arrests by state agencies that report crime enforcement data. While a potential measure of enforcement, the data reported do have potential problems. First, not every agency in each state reports any or all arrest data. Moreover, those agencies that do report data do not necessarily include the state's entire population in their combined jurisdictions. To control for these discrepancies, DUI arrests are normalized by the

total population included in the combined jurisdiction of the state agencies reporting; *ENFORCE* is the resultant variable. If DUI arrests per capita is a sufficient measure of enforcement, and having a lower BAC limit does not alter behavior on its own, then one would expect the parameter estimate for *BAC08* to be insignificant while that for *ENFORCE* would be significant and negative.

Since 1990, the National Highway Traffic Safety Administration (NHTSA) has published the statutory minimum license sanctions (either suspensions or revocations) measured in days for first, second and third time offenders (*1STPEN*, *2NDPEN*, *3RDPEN*). If these penalties were static over the time of the sample, their inclusion would duplicate any state fixed-effects. However, several times during the sample period states changed their minimum penalties.<sup>7</sup> Indeed, some states increased certain penalties while simultaneously decreasing others, then reversed themselves the next year.<sup>8</sup> The changes in minimum penalties provide enough variation so that the *between* and *within* effects of changing minimum penalties can be estimated.

Finally, a time trend starting in 1991 and a dummy variable that takes a value of one in 1991 and zero otherwise are included to account for any time-related trends in the data. The *YR91* dummy variable is included to accommodate the measurement “error” that seems apparent in the data for that year (see Figure 1).<sup>9</sup>

Table 2 reports the variables included in the empirical analysis, their definitions and descriptive statistics. The descriptive statistics indicate that over the sample period each state averaged 347 alcohol related fatalities, with approximately 250 involving at least one person with a BAC greater than 0.10. In the sample, Vermont had the fewest alcohol related traffic fatalities (with  $BAC \geq 0.01$ ), while California had the most. However, Texas had the most alcohol related fatalities in accidents where one individual had a greater than 0.10 BAC.

<Table 2 about here>

Average traffic density was 0.42, indicating that the average miles driven per vehicle was approximately half of the paved miles in the state. However, most drivers do not drive half of a state’s paved roads in a given year. Rather, drivers tend to drive on the same roads, for example to and from work on a regular basis, and therefore density should be carefully interpreted. Nevertheless, Texas has the

lowest traffic density in the sample, while the District of Columbia has the greatest.

Twenty one percent of the observations in the sample correspond to states that voluntarily imposed lower BAC limits. Enforcement averaged six DUI arrests per 1000 people, with Delaware reporting the fewest DUI arrests and Minnesota reporting the most. The average first offender minimum license sanction was 35 days, the average minimum for the second offense was 335 days and that for the third offense was 801 days. The states with ten year license revocation for three time offenders are Alaska and New Jersey.

Table 3 reports correlations between select variables and facilitates simple generalizations that conform with economic intuition. For instance, the correlation between alcohol related fatalities and density indicates that states with greater traffic density have fewer alcohol related fatalities. Likewise, the correlation between alcohol related fatalities and minimum penalties is negative. Those states with lower BAC limits tend to have higher minimum penalties, perhaps reflecting state specific legislative or public interest concerns. Finally, enforcement has a negative correlation with alcohol related fatalities and with minimum penalties. In general, the correlations are as intuition would predict, yet they do not provide solid statistical evidence that lowered BAC limits actually reduce alcohol related fatalities.

*< Table 3 about here >*

The initial step in the empirical analysis is to confirm the findings of previous studies, most notably Voas et al. (2000). Table 4 reports robust log-log OLS estimates for the entire sample of alcohol related fatalities, those with  $0.01 \leq BAC < 0.10$  and those in which at least one person had a  $BAC \geq 0.10$ . The variables included in these replication models match those from Voas et al. (2000), except for *DENSITY*, and generally confirm their results. Using their technique, the pooled OLS results predict that 981 lives would have been saved in 2000 if all states had lowered BAC limits.

*< Table 4 about here >*

This estimate is consistent with other estimates in the literature and those of policy advocates when President Clinton endorsed the legislation requiring all states to lower BAC limits. While the prediction can be questioned on intuitive and statistical grounds (as is done below), it is perhaps valuable to show that the number was obtained “legitimately.” Yet, as the prediction was used as justification to enact

nation-wide legislation, a careful analysis of this prediction is warranted if only to assist in future policy debates.

The pooled OLS approach used in previous studies assumes the marginal impact of a lower BAC limit is the same whether one looks at two states with identical explanatory variables where one has the lower BAC limit and the other does not, or if one state is compared to itself before and after the lower limits are imposed.<sup>10</sup> If this assumption is invalid, Pooled OLS can cause misleading inference (see Baltagi and Griffin, 1984, and Benson, Rasumussen and Mast, 1999). Panel data methodology is well suited to investigating the impacts of policy changes such as BAC limits, and for statistical tests to determine whether the pooled OLS methodology (such as those reported in Table 4) is appropriate (see Woolridge, 2002, for more discussion). Specifically, panel data estimation can distinguish between the impact of a policy *within* states and *between* states. The former have traditionally been associated with short-run impacts of policy changes, while the latter are associated with long-run impacts (Houthakker, 1965). In the current context, if the *between* impacts of lowered limits dominate the *within* impacts, the previously reported estimated lives saved by all states having lowered limits may be overstated.

Panel data estimation is applied first to the models reported in Table 4. For the sake of brevity these results are not reported here but are available from the author upon request. The continuous variables are generally statistically significant however the BAC08 variable is, in general, statistically insignificant. The Hausman specification test indicates that the random effects model is statistically inferior to the fixed effects model.

However, the models presented in Table 4 do not adequately control for the policy variables thought to influence behavior, as discussed in Section 3. Therefore, the models are expanded to include measurements of enforcement efforts and the minimum license sanctions for first, second and third time offenders. These models are reported in Table 5 and confirm that the estimated lives saved provided in previous studies may be misleading.

Table 5 reports three specifications for three different samples: total alcohol related traffic fatalities, those that included an individual with more than 0.01 but less than 0.10 BAC and those that involved at least one individual with greater than 0.10 BAC.<sup>11</sup> The three specifications include pooled OLS, the

Between estimator (OLS on differences in overall means) and the Within estimator (OLS on differences from state means, with state specific intercepts).<sup>12</sup> In general, the Pooled OLS results are consistent with previous studies. However, there are significant differences between previous findings and the Between and Within estimates reported here.

*< Table 5 about here >*

The variables are discussed in turn, addressing the three subsamples simultaneously. Total miles driven is positively related to alcohol related fatalities in all three subsamples for the Pooled OLS and Between specifications. Total miles driven is also positively related to alcohol related fatalities in the Within specification but statistically significant only for the sample with higher BAC levels. On the other hand, traffic density is consistently negatively related to alcohol related fatalities, and is statistically significant in the Pooled OLS for the total sample and the subsample of higher BAC levels, but is only statistically significant in the Between specification for those fatalities with lower BAC levels.

The socioeconomic variables are consistent with other studies. Per capita income is negatively related to alcohol related fatalities in all three samples for the Pooled OLS and the Between specifications. However, income is statistically insignificant in the Within specifications and consistently takes the “wrong” sign. Greater unemployment has a positive and statistically significant relationship with alcohol related fatalities in all three samples, consistent with other studies.

Skipping the discussion of the lowered BAC limits for the moment, enforcement is negatively related to alcohol related fatalities with the highest BAC levels. Perhaps enforcement is not an influence on those who do not drive legally drunk, or perhaps enforcement is not measured appropriately.

The penalty levels yield consistent results only for first-offender license sanctions (in contrast to the findings of Benson, Rasumussen and Benson, 1999). The greater the penalty for first time offenders, the fewer alcohol related fatalities at all levels of blood alcohol content. This provides support for Becker’s hypothesis that the severity of penalties alters decision making through changes in the expected costs of driving drunk, thereby reducing alcohol related fatalities, and is counter to the findings of Benson, Rasmussen, and Mast (1999). The remaining penalties do not have the same influences as the first time

offender penalties. The severity of second offender penalties takes the “wrong” sign but is only weakly significant in the Pooled OLS and Within specifications for fatalities involving lower blood alcohol contents. The severity of third-offender penalties is statistically insignificant in all three samples and all specifications. It might be that the majority of those who habitually drive drunk are not sufficiently dissuaded by the severity of repeat offender penalties (see Table 1).

The time trend indicates that the absolute level of alcohol related fatalities was not declining in the 1990s, contrary to the findings in other studies, but consistent with Figures 1 and 2. However, this analysis includes several variables not included in other studies, thereby making direct comparisons problematic. Moreover, the limited time span of the sample may provide the difference between the results obtained here and those found in other studies. If the majority of the decline in alcohol related fatalities occurred between 1982 and 1991 (as proposed by Voas et al., 2000), then the insignificance of the time trend would not be surprising. Indeed, a specification without the penalty and enforcement variables, which increases the sample period to cover 1983 through 2000 (not reported here), indicates a negative and statistically significant time trend. The  $YR91$  dummy variable is statistically significant, consistent with Figure 1.

For the Pooled OLS specifications, lowered BAC limits have a negative relationship with total alcohol related fatalities and those fatalities with at least one person with a higher BAC level (a finding consistent with Smith, 1986). The lack of a significant relationship between lower BAC level fatalities and lowered BAC limits is not surprising if the majority of these fatalities had BAC levels below 0.08. If this were the case, lower BAC limits would not be expected to alter behavior as the limits did not bind.

A statistical test between the appropriateness of Pooled OLS or the Within specifications is an F-test on whether the state-specific intercepts are jointly equal to zero, which would support the Pooled OLS (single intercept) specification. The null hypothesis that the Pooled OLS is appropriate is soundly rejected for all three of the subsamples. However, it is not immediately clear whether the fixed effects (Within) model is superior to the random effects model (a weighted average of the Within and Between estimates). A Hausman test of whether the state-specific fixed effects are correlated with the error term,

which would provide inconsistent estimates and favor the random effects model, indicates that the fixed effects model is statistically superior for the two subsamples but not the total sample.

For the entire sample of alcohol related fatalities, the Between estimates conform the most with the Pooled OLS results. This is consistent with the majority of the variation of the pooled sample being cross-sectional rather than time-series in nature. The Within estimator is thus interpreted as short-run impacts whereas the Between estimator is interpreted as long-run impacts (Baltagi and Griffin, 1984). The Within estimates indicate that lower BAC limits do not have a statistically significant impact on *within-state* changes in alcohol related fatalities. This result can have two interpretations. One is that the short-run impacts of the policy changes are relatively minor, whereas the long-run impacts are more pronounced. On the other hand, the short time-period included in the panel (high N and low T) may mask the impact of the policy change. Nonetheless, it seems the negative impact of lowered BAC limits in Pooled OLS results is primarily caused by differences *between* states that voluntarily imposed the lower limits and those that did not. These results indicate that previous estimates of lives saved by forcing all states to enact lowered BAC limits may have been overstated, primarily because the short-run impacts of the lowered limits seem to be insignificant.

The short-run impacts of the lowered BAC limits may be diluted if individuals adjust their behavior before the law is in force, which might occur if sufficient media coverage aims at the impact the new limits would have on the perceived costs of drunk driving. However, additional models in which up to three lead values of *BAC08* were included yield consistently insignificant parameter estimates on these additional variables.

While other studies show that within-state impacts of lowered BAC limits are statistically significant, these studies include the 1980s, and sometimes the 1970s, in the sample period. If the impact of lowered BAC limits was more dramatic during the 1980s, the lack of statistical significance during the 1990s may not be surprising. While advocates do not specifically admit to this possibility, the increased focus by advocacy groups on penalties and enforcement efforts may reflect a reduced impact of changing legal limits.

The between-state impacts of lowered BAC limits are statistically significant, consistent with sig-

nificant long-run impacts of lowered limits on alcohol related traffic fatalities. These long-run impacts may reflect significant long-run changes in behavior caused by lowered BAC limits. On the other hand, perhaps states with fewer alcohol related fatalities were more prone to voluntarily lower BAC limits, thereby creating a spurious result that has no relation to the enactment of lowered limits. Consistent with the theory of public choice, legislatures may not pass reduced BAC limits if their constituency does not demand such legislation; perhaps because the state has a relatively high per capita alcohol consumption (see below).

The lack of significance of the parameter estimates on *ENFORCE* in Table 5 suggests that DUI arrests per capita is not correlated with alcohol related fatalities, a somewhat surprising result; although measurement error may cause a Type II error. Therefore, the number of agencies in each state that reported any crime statistics to the FBI (*AGENCIES*) is used as a proxy for enforcement efforts, not specifically as an instrumental variable, and the models presented in Table 5 are re-estimated. These results are reported in Table 6.

<Table 6 about here>

Nothing qualitatively changes with the vast majority of the parameter estimates except those on *BAC08* and *AGENCIES*. Now, lowered BAC limits is negatively related to total alcohol related fatalities and those with higher BAC levels for the Pooled OLS and Between estimators. However, the lower BAC limits remain statistically insignificant in the Within estimation. Enforcement is now negatively correlated with alcohol related fatalities in the Pooled OLS and Between specifications for the total sample and those fatalities with higher BAC levels. However, the variable remains insignificant in the sample with lower BAC levels.

Predictions based on pooled OLS results might considerably overstate the expected gains from forcing all states to lower BAC limits, thereby bringing into question the actual net social benefits (as opposed to net political benefits) that will accrue from the national legislation. Indeed, as local enforcement efforts are not compensated by the national treasury, the involuntary reduction in BAC limits may be considered an unfunded mandate passed down to the states. If state and local law enforcement personnel are not directly compensated for the increased probability of catching individuals

driving legally drunk, it is possible that law enforcement may actually become less strict, thereby making the impact of involuntarily lowered BAC limits ambiguous.

If the Within estimates measure short-run impacts of policy changes, then reduced BAC limits seem to have had no significant short-run impact on alcohol related fatalities during the 1990s. On the other hand, the statutory changes do seem to have a long-run impact on alcohol related fatalities. Therefore, the short-run prediction of the number of lives saved if all states had lowered BAC limits was most likely overstated. As an alternative to lowered BAC limits, the results indicate that the minimum license sanction for first-time offenders has the largest short-run impact on alcohol related fatalities. Recent policy initiatives by MADD and other activist groups to pressure legislatures to increase the minimum penalties for first-time, and especially for repeat, offenders seems to support this result.

## 5. Conclusions

This study investigates the impact of lowered blood alcohol content limits on alcohol related fatalities in the fifty states and the District of Columbia from 1991 through 2000. The study has practical value for a number of reasons. First, it extends the existing literature on drunk driving policy by investigating how law enforcement and severity of penalties combine with changes in legal BAC limits to impact drunk driving fatalities. In this sense, the study provides an application of Becker's (1968) theory of illegal behavior.

Further, this study has implications for policy advocates. In 2000, national legislation passed requiring all states to lower BAC limits to 0.08 by 2003 or lose a portion of their share of federal highway funds. When enacted, advocates heralded the law as a victory, claiming that approximately 600 lives a year would be saved by the new laws. The present study casts doubt on this number for various reasons and suggests that anti-drunk driving advocates may wish to focus their efforts on alternative policy levers.

Previous studies of BAC limits use pooled OLS which assumes the effects *between* states that lower limits and those that do not are identical to the effects *within* states that enact the lower limits (voluntarily before 2000). In this study, the larger impact of lowered BAC limits lies in the *between* effects,

whereas the *within* effects are insignificant. Therefore, the ability to predict the impact of involuntarily passing lowered limits is difficult. If, following convention, the within-state impacts measure short-run impacts of a policy, whereas between-state impacts measure long-run impacts, it seems that during the 1990s changing BAC limits had little short-run impact on alcohol related fatalities.

A simple quadratic model indicates that all 50 states and the District of Columbia might have voluntarily passed lowered BAC limits by 2011.<sup>13</sup> To further understand the impact of lowered BAC limits, it might prove fruitful to investigate *why* some states chose to voluntarily impose lower standards, and, perhaps more importantly, why some states did not (similar to Brown, Jewell and Richer, 1996). Initial, albeit naive, empirical investigation suggests that alcohol related traffic fatalities were not a significant (either statistically or economically) factor in a state voluntarily passing a lower limit.<sup>14</sup> However, greater per-capita alcohol consumption significantly reduced the chances that a state would voluntarily reduce its BAC limit.<sup>15</sup>

What has not been questioned in the literature is the economic impact of forcing states to change their laws involuntarily. The national legislation requiring reduced BAC limits provides another natural experiment, similar to maximum speed limits, seat-belt laws and underage drinking laws, conducive to empirical analysis. Further study of involuntary legislative changes, including the impact on target variables and actual versus predicted social gains will prove interesting.

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## Notes

<sup>1</sup>The early 1980's coincided with the creation of other anti-drunk driving groups such as SADD, Students Against Drunk Driving, (see McCarthy and Harvey, 1988).

<sup>2</sup>The act was amended in 1991 to increase the financial rewards to states that implemented these programs.

<sup>3</sup>Voas, Tippetts and Fell (2000) seems to be a revised version of a study by Voas and Tippetts (1999), prepared for the Department of Transportation.

<sup>4</sup>The total monetary costs of a DUI conviction are not available from most states. However, the state of Illinois estimates the average monetary cost for a DUI conviction to be \$11,200. These costs are comprised of: \$4,500 in increased insurance premiums (over three years), \$3,000 in legal fees (including lawyer fees), \$3,350 in court fees, \$2,000 in lost wages (due to court appearances and jail time), \$250 in rehabilitation programs (required by state law), and \$120 in license reinstatement fees (*Illinois DUI Fact Book*, 2001).

<sup>5</sup>The pre-1991 data are not included in the subsequent analysis because of limited availability for many variables included in this study, especially enforcement and penalties.

<sup>6</sup>The data reported for 1990 and 1991 are measured differently by NHTSA.

<sup>7</sup>For example, in 1997 Utah increased the minimum first-offender license suspension to 180 days from 90 days.

<sup>8</sup>In 1997, Utah doubled the minimum license sanctions for first, second and third offenses. However, in 2000, the state reduced the minimum penalties to what they had been in 1996. The author has been unable to uncover any printed documentation discussing the reasons for the reversal on minimum sanctions.

<sup>9</sup>As Greene (2002) shows, measurement error in the dependent variable does not distort the desirable

statistical properties of parameter estimates.

<sup>10</sup>This prediction may be misleading. The impact of voluntarily lowered BAC limits on alcohol related fatalities might differ from involuntarily lowered limits. The predictions of lives saved, used to justify “forcing” states to reduce their BAC limits, were generated using econometric results from the era of voluntarily lowered BAC limits.

<sup>11</sup>The two subsamples differentiated by BAC levels are mutually exclusive.

<sup>12</sup>The Pooled OLS estimator is a weighted average of the Between and Within estimators (see, for example, Greene, 2002).

<sup>13</sup>This “prediction” was obtained by regressing the percentage of states with lowered BAC limits against an intercept and linear and quadratic time trends, taking 1980 as the initial period. The prediction of 2011 was obtained using the parameter estimates from this regression.

<sup>14</sup>A proportional hazard model indicated that no set of explanatory variables, including region of the country and propensity to elect Democrat governors and state legislatures, were statistically related to the voluntary passage of lower BAC limits.

<sup>15</sup>This result was obtained using a simple logit model (the dependent variable taking the value of one when a state passed lower BAC limits).

Table 1: Drunk Driving Recidivism in Wisconsin (1989-2000)

# DUI Convictions	Individuals Convicted	Min. Penalty
1	234,826	None
2	34,435 (14.7% of 1 DUI Population)	60 days
3	16,708 (7% of 1 DUI Population)	90 days
4	5,250 (2.2% of 1 DUI Population)	N/A
5	1,666	N/A
6	557	N/A
7+	315 (1 with 12, 1 with 13 offenses)	N/A

Source: *2000 Wisconsin Alcohol Traffic Facts*, p. 27.

Table 2: Variable Definitions and Descriptive Statistics

Variable	Definition	Mean	Std. Dev.	Min.	Max.
TOTFAT	Total Traffic Fatalities	814.69	793.83	50.00	4685.00
ALCFAT	Total Alcohol Related Fatalities	347.17	359.06	20.48	2397.00
ALCFAT1	Alcohol Related Fatalities with $0.01 \leq BAC < 0.10$	97.51	142.68	2.60	1596.00
ALCFAT2	Alcohol Related Fatalities with $BAC \geq 0.10$	249.66	263.19	15.00	1582.44
MILES	Vehicle Miles Driven (millions)	48985.47	50424.93	3448.00	306649.00
DENSITY	Traffic Density	0.42	1.15	0.03	13.28
PERINC	Per Capita Income	23339.37	4711.51	10333.00	40870.00
UNEMP	Unemployment Rate	5.32	1.65	1.20	11.40
BAC08	BAC Limit = 0.08 (1=Yes)	0.22	0.41	0.00	1.00
ENFORCE	DUI per 1000 Capita Covered	6.83	7.36	0.00 <sup>a</sup>	7.33
AGENCIES	Agencies Reporting to FBI	209.92	194.22	1.00	901.00
1STPEN	Min. 1st Offense License Sanction (days)	35.33	61.75	0.00	365.00
2NDPEN	Min. 2nd Offenses License Sanction (days)	335.42	243.56	0.00	1825.00
3RDPEN	Min. 3rd Offense License Sanction (days)	801.43	796.61	0.00	3650.00
TIME	Time Trend (1991=0)	4.43	2.88	0.00	9.00

<sup>a</sup> The reported per-capita arrest rate in Delaware is  $2.63 \times 10^{-4}$

Data sources: *TOTFAT*, *ALCFAT*, *ALCFAT1*, *ALCFAT2*, *MILES*, *BAC08*, *1STPEN*, *2NDPEN*, and *3RDPEN* from NHTSA; *DENSITY* self generated from data available from NHTSA and FHA; *PERINC*, *UNEMP* from BLS; *ENFORCE*, *AGENCIES* from the FBI.

Table 3: Correlations Between Select Variables  
(Number of Observations: 473)

	$BAC < 0.10$	$BAC \geq 0.10$	MILES	DENSITY	BAC08	ENFORCE	AGENCIES	1STPEN	2NDPEN
$BAC < 0.10$	1.000								
$BAC \geq 0.10$	0.523	1.000							
MILES	0.617	0.891	1.000						
DENSITY	-0.151	-0.208	-0.211	1.000					
BAC08	0.044	0.138	0.181	-0.014	1.000				
ENFORCE	-0.051	-0.063	-0.091	0.084	0.006	1.000			
AGENCIES	0.538	0.735	0.773	-0.181	0.047	-0.103	1.000		
1STPEN	-0.112	-0.111	-0.089	0.231	0.014	-0.069	0.073	1.000	
2NDPEN	-0.142	-0.213	-0.239	0.037	0.131	-0.021	-0.104	0.332	1.000
3RDPEN	-0.014	-0.043	-0.013	-0.018	0.015	-0.004	0.068	0.195	0.503

Table 4: Replication Models (Ordinary Least Squares)

Dependent Variable: Log of Alcohol Related Fatalities

Variable	$BAC \geq 0.01$		$0.01 \leq BAC < 0.10$		$BAC \geq 0.10$	
	Parameter	Std. Error	Parameter	Std. Error	Parameter	Std. Error
Intercept	0.109	1.322	-3.561 <sup>a</sup>	1.261	0.343	1.417
lnMILES	0.849 <sup>a</sup>	0.023	0.941 <sup>a</sup>	0.030	0.823 <sup>a</sup>	0.025
lnDENSITY	-0.128 <sup>a</sup>	0.026	-0.043	0.031	-0.150 <sup>a</sup>	0.029
lnPERINC	-0.441 <sup>a</sup>	0.141	-0.299	0.135	-0.470 <sup>a</sup>	0.151
lnUNEMP	0.409 <sup>a</sup>	0.052	0.303 <sup>a</sup>	0.065	0.444 <sup>a</sup>	0.064
BAC08	-0.079 <sup>a</sup>	0.034	-0.044	0.039	-0.088 <sup>a</sup>	0.037
TIME	0.011	0.008	0.014	0.010	0.010	0.009
YR91	0.346 <sup>a</sup>	0.040	1.613 <sup>a</sup>	0.518	-0.604 <sup>a</sup>	0.051
N	473		473		473	
F-stat	699.870 <sup>a</sup>		574.275 <sup>a</sup>		639.963 <sup>a</sup>	
$R^2$	0.912		0.894		0.904	

Robust standard errors reported.

<sup>a</sup> significant at the 0.05 level in a two-tailed test.

<sup>b</sup> significant at the 0.10 level in a two-tailed test.

These models replicate those in Voas, et al. (2000).

Table 5: Panel Estimation  
 Dependent Variable: Log of Alcohol Related Fatalities  
 (Continued on Next Page)

Variable	$BAC \geq 0.01$			$0.01 \leq BAC < 0.10$			$BAC \geq 0.10$		
	OLS	Between	Within	OLS	Between	Within	OLS	Between	Within
Intercept	-0.182 (1.284)	2.502 (2.627)		-3.839 <sup>a</sup> (1.258)	-1.339 (2.980)		0.024 (1.381)	2.267 (2.785)	
lnMILES	0.852 <sup>a</sup> (0.021)	0.869 <sup>a</sup> (0.055)	0.415 (0.301)	0.948 <sup>a</sup> (0.029)	0.988 <sup>a</sup> (0.063)	0.347 (0.381)	0.823 <sup>a</sup> (0.023)	0.828 <sup>a</sup> (0.059)	0.508 <sup>b</sup> (0.298)
lnDENSITY	-0.104 <sup>a</sup> (0.024)	-0.839 (0.066)	-0.049 (0.043)	-0.015 (0.031)	0.043 (0.075)	-0.048 (0.058)	-0.129 <sup>a</sup> (0.026)	-0.124 <sup>b</sup> (0.070)	-0.051 (0.044)
lnPERINC	-0.407 <sup>a</sup> (0.135)	-0.682 <sup>a</sup> (0.262)	0.127 (0.078)	-0.263 <sup>a</sup> (0.133)	-0.523 <sup>b</sup> (0.297)	0.199 (0.153)	-0.437 <sup>a</sup> (0.144)	-0.704 <sup>a</sup> (0.278)	0.099 (0.071)
lnUNEMP	0.424 <sup>a</sup> (0.052)	0.596 <sup>a</sup> (0.160)	0.007 (0.058)	0.318 <sup>a</sup> (0.066)	0.400 <sup>a</sup> (0.181)	0.027 (0.106)	0.461 <sup>a</sup> (0.062)	0.682 <sup>a</sup> (0.169)	-0.025 (0.060)
BAC08	-0.088 <sup>a</sup> (0.033)	-0.135 (0.089)	0.022 (0.037)	-0.052 (0.039)	-0.113 (0.101)	0.068 (0.058)	-0.096 <sup>a</sup> (0.036)	-0.133 (0.094)	0.004 (0.036)
lnENFORCE	0.012 (0.015)	0.041 (0.058)	-0.012 (0.010)	0.022 (0.017)	0.092 (0.066)	-0.010 (0.020)	0.002 (0.017)	0.020 (0.062)	-0.023 <sup>a</sup> (0.010)
ln1STPEN	-0.041 <sup>a</sup> (0.007)	-0.041 <sup>a</sup> (0.018)	-0.035 <sup>a</sup> (0.012)	-0.041 <sup>a</sup> (0.009)	-0.037 <sup>a</sup> (0.020)	-0.044 <sup>a</sup> (0.014)	-0.044 <sup>a</sup> (0.008)	-0.047 <sup>a</sup> (0.019)	-0.025 <sup>a</sup> (0.013)
ln2NDPEN	0.034 <sup>a</sup> (0.017)	0.049 (0.048)	0.040 (0.023)	0.040 <sup>a</sup> (0.020)	0.048 (0.054)	0.071 <sup>a</sup> (0.030)	0.033 <sup>b</sup> (0.018)	0.053 (0.051)	0.026 (0.025)
ln3RDPEN	-0.015 (0.014)	-0.032 (0.041)	-0.024 (0.021)	-0.023 (0.017)	-0.039 (0.046)	-0.046 <sup>b</sup> (0.028)	-0.012 (0.015)	-0.033 (0.043)	-0.012 (0.023)

Robust standard errors reported in parentheses.

<sup>a</sup> significant at the 0.05 level in a two-tailed test.

<sup>b</sup> significant at the 0.10 level in a two-tailed test.

Table 5 (Continued): Panel Estimation  
 Dependent Variable: Log of Alcohol Related Fatalities

Variable	$BAC \geq 0.01$			$0.01 \leq BAC < 0.10$			$BAC \geq 0.10$		
	OLS	Between	Within	OLS	Between	Within	OLS	Between	Within
TIME	0.013 (0.008)	-0.004 <sup>a</sup> (0.092)	-0.027 <sup>a</sup> (0.012)	0.016 <sup>b</sup> (0.009)	0.070 (0.105)	-0.010 (0.016)	0.012 (0.008)	-0.021 (0.098)	-0.035 <sup>a</sup> (0.012)
YR91	0.358 <sup>a</sup> (0.039)	-0.508 <sup>a</sup> (0.766)	0.323 <sup>a</sup> (0.036)	1.628 <sup>a</sup> (0.051)	0.726 (0.869)	1.596 <sup>a</sup> (0.052)	-0.594 <sup>a</sup> (0.048)	-1.773 <sup>a</sup> (0.812)	-0.634 <sup>a</sup> (0.038)
N	473	473	473	473	473	473	473	473	473
R <sup>2</sup>	0.918	0.961	0.966	0.900	0.957	0.943	0.912	0.956	0.965
OLS vs. Within			10.760 <sup>a</sup>			5.826 <sup>a</sup>			11.889 <sup>a</sup>
Hausman (Random Effects vs. Within)			5.667			21.958 <sup>a</sup>			16.841 <sup>a</sup>

Robust standard errors reported in parentheses.

<sup>a</sup> significant at the 0.05 level in a two-tailed test.

<sup>b</sup> significant at the 0.10 level in a two-tailed test.

Table 6: Panel Estimation (Enforcement Measured by Proxy)  
 Dependent Variable: Log of Alcohol Related Fatalities  
 (Continued on Next Page)

Variable	$BAC \geq 0.01$			$0.01 \leq BAC < 0.10$			$BAC \geq 0.10$		
	OLS	Between	Within	OLS	Between	Within	OLS	Between	Within
Intercept	-0.271 (1.270)	2.782 (2.434)		-3.962 <sup>a</sup> (1.264)	-1.085 (2.911)		-0.033 (0.025)	2.987 (2.533)	
lnMILES	0.891 <sup>a</sup> (0.023)	0.926 <sup>a</sup> (0.054)	0.417 (0.301)	0.985 <sup>a</sup> (0.032)	1.018 <sup>a</sup> (0.065)	0.346 (0.381)	0.869 <sup>a</sup> (0.025)	0.903 <sup>a</sup> (0.056)	0.513 <sup>b</sup> (0.300)
lnDENSITY	-0.126 <sup>a</sup> (0.023)	-0.130 <sup>a</sup> (0.053)	-0.048 (0.043)	-0.042 (0.031)	-0.311 (0.063)	-0.050 (0.058)	-0.148 <sup>a</sup> (0.026)	-0.161 <sup>a</sup> (0.055)	-0.048 (0.043)
lnPERINC	-0.420 <sup>a</sup> (0.133)	-0.738 <sup>a</sup> (0.243)	0.127 <sup>b</sup> (0.079)	-0.277 <sup>a</sup> (0.131)	-0.575 <sup>b</sup> (0.291)	0.196 (0.153)	-0.450 <sup>a</sup> (0.141)	-0.766 <sup>a</sup> (0.253)	0.099 (0.072)
lnUNEMP	0.419 <sup>a</sup> (0.050)	0.564 <sup>a</sup> (0.147)	0.008 (0.057)	0.317 <sup>a</sup> (0.062)	0.392 <sup>a</sup> (0.176)	0.024 (0.107)	0.451 <sup>a</sup> (0.060)	0.635 <sup>a</sup> (0.153)	-0.038 (0.064)
BAC08	-0.099 <sup>a</sup> (0.034)	-0.150 <sup>b</sup> (0.082)	0.021 (0.038)	-0.062 (0.040)	-0.116 (0.098)	0.057 (0.058)	-0.111 <sup>a</sup> (0.036)	-0.154 <sup>b</sup> (0.085)	0.004 (0.037)
lnAGENCIES	-0.060 <sup>a</sup> (0.013)	-0.100 <sup>a</sup> (0.037)	-0.0008 (0.008)	-0.060 <sup>a</sup> (0.015)	-0.089 <sup>b</sup> (0.044)	-0.023 <sup>b</sup> (0.014)	0.066 <sup>a</sup> (0.014)	-0.113 <sup>a</sup> (0.039)	0.007 (0.010)
ln1STPEN	-0.042 <sup>a</sup> (0.007)	-0.041 <sup>a</sup> (0.016)	-0.036 <sup>a</sup> (0.012)	-0.041 <sup>a</sup> (0.009)	-0.036 <sup>b</sup> (0.020)	-0.046 <sup>a</sup> (0.014)	-0.045 <sup>a</sup> (0.007)	-0.047 <sup>a</sup> (0.017)	-0.027 <sup>a</sup> (0.013)
ln2NDPEN	0.034 <sup>b</sup> (0.017)	0.043 (0.044)	0.040 <sup>b</sup> (0.023)	0.040 <sup>b</sup> (0.020)	0.039 (0.053)	0.073 <sup>a</sup> (0.030)	0.033 <sup>b</sup> (0.018)	0.048 (0.046)	0.024 (0.025)
ln3RDPEN	-0.010 (0.014)	-0.031 (0.072)	-0.024 (0.021)	-0.017 (0.017)	-0.015 (0.044)	-0.048 <sup>b</sup> (0.028)	-0.008 (0.015)	-0.018 (0.039)	-0.011 (0.023)

Robust standard errors reported in parentheses.

<sup>a</sup> significant at the 0.05 level in a two-tailed test.

<sup>b</sup> significant at the 0.10 level in a two-tailed test.

Table 6 (Continued): Panel Estimation (Enforcement Measured by Proxy)  
 Dependent Variable: Log of Alcohol Related Fatalities

Variable	$BAC \geq 0.01$			$0.01 \leq BAC < 0.10$			$BAC \geq 0.10$		
	OLS	Between	Within	OLS	Between	Within	OLS	Between	Within
TIME	0.017 (0.008)	-0.031 (0.072)	-0.027 <sup>a</sup> (0.012)	0.014 (0.009)	0.0008 (0.087)	-0.010 (0.016)	0.011 (0.008)	-0.030 (0.075)	-0.036 <sup>a</sup> (0.012)
YR91	0.356 (0.037)	-0.905 (0.711)	0.325 (0.036)	1.623 <sup>a</sup> (0.050)	0.263 (0.851)	1.600 <sup>a</sup> (0.052)	-0.594 <sup>a</sup> (0.046)	-2.171 <sup>a</sup> (0.740)	-0.630 <sup>a</sup> (0.038)
$N$	473	473	473	473	473	473	473	473	473
$R^2$	0.921	0.966	0.966	0.902	0.959	0.943	0.916	0.962	0.965
OLS vs. Within			10.020 <sup>a</sup>			5.552 <sup>a</sup>			10.924 <sup>a</sup>
Hausman (Random Effects vs. Within)			5.446			21.009 <sup>a</sup>			190.960 <sup>a</sup>

Robust standard errors reported in parentheses.

<sup>a</sup> significant at the 0.05 level in a two-tailed test.

<sup>b</sup> significant at the 0.10 level in a two-tailed test.

Figure 1: Alcohol Related Fatalities in the United States (1984-2000)

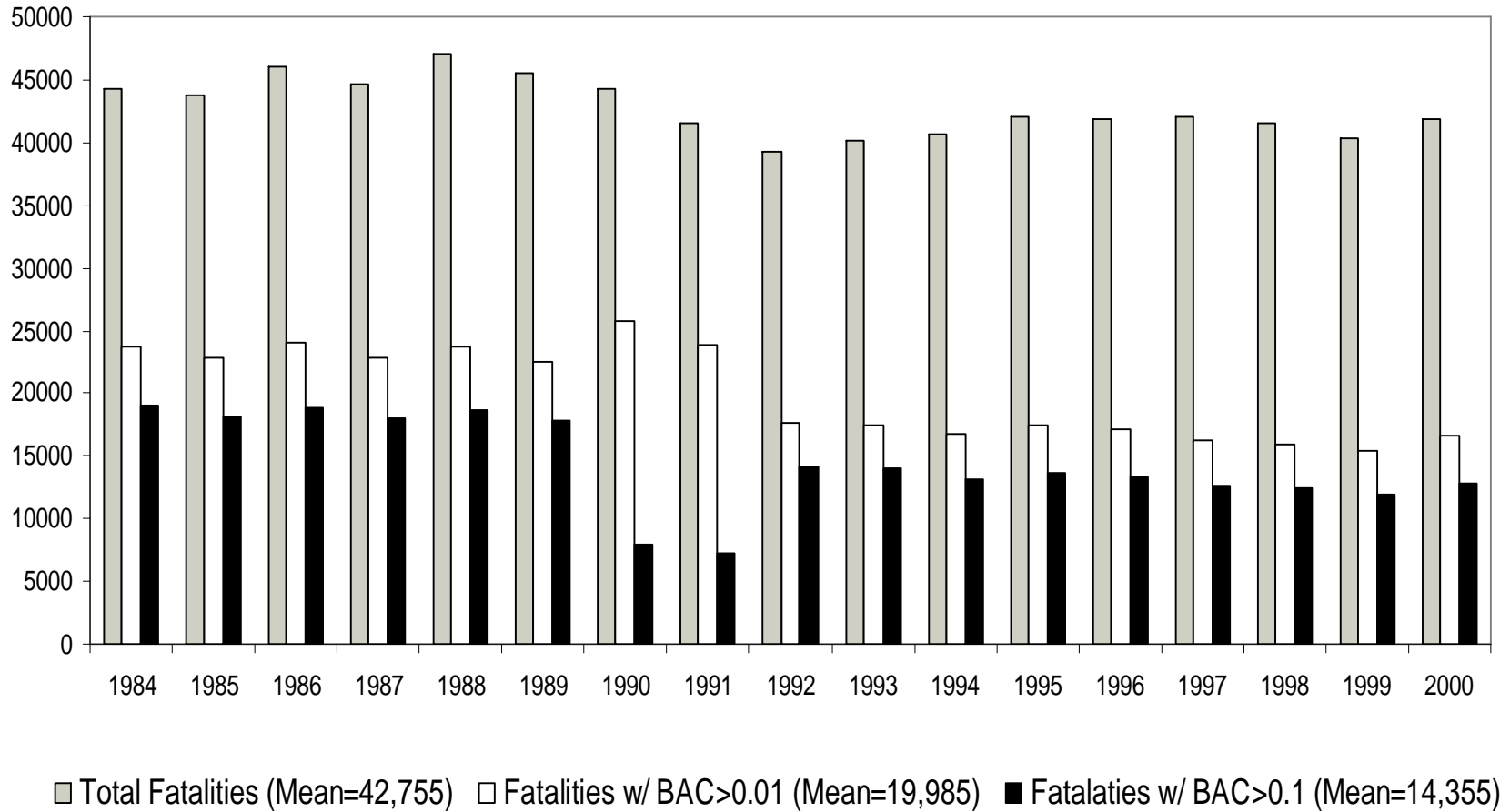


Figure 2: Alcohol Related Fatalities as a Percentage of Total Traffic Fatalities

