Does Setting Limits Save Lives? The Case of 0.08 BAC Laws

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Abstract

Nineteen states have established laws that make it illegal per se to drive with a blood alcohol concentration (BAC) of 0.08. The controversy over extending this stricter definition throughout the nation has focused largely on whether the state laws have been effective at saving lives. Prior evidence on this question has been mixed as well as criticized on several methodological grounds. This study presents novel, panel-based evaluations of 0.08 BAC laws, which address the potential methodological limitations of previous studies. The results of this study indicate that 0.08 BAC laws have been effective in reducing the number of traffic fatalities, particularly among younger adults. These estimates suggest that the nationwide adoption of 0.08 BAC laws would generate substantial gains, reducing the annual count of traffic fatalities by at least 1200. © 2001 by the Association of Public Policy Analysis and Management.

INTRODUCTION

Over the last 25 years, almost every state has adopted a law that makes it illegal per se to drive with certain blood alcohol concentrations (BAC).[1] Most states initially established this limit at a BAC of 0.10 or higher. However, by the end of 1998, 14 states had established an illegal per se limit at a BAC of 0.08 (Table 1).[2] The continued expansion of this stricter standard has been strongly advocated by law enforcement groups, insurance industry advocates, and traffic safety organizations like Mothers Against Drunk Driving (MADD) who claim that these regulations can save lives by reducing the prevalence of drunk driving. However, these claims have also been contested aggressively by the alcohol and restaurant industries, which argue that this regulation merely punishes responsible social drinkers who pose no threat to others. Over the last several years, much of this debate has focused on possible actions by

[1] Only Massachusetts and South Carolina have no established BAC at which it is illegal per se to drive. BAC is measured as the weight of alcohol in a certain volume of blood and can be determined though the analysis of blood, urine, breath and saliva.
[2] Since then, five other states (Hawaii, Washington, Texas, Kentucky, and Rhode Island) have also adopted 0.08 BAC laws. Several other industrialized nations also define drunk-driving at a BAC of 0.08 or lower.

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the federal government to compel all states to adopt this stricter BAC standard. In particular, in March of 1998, the Senate approved by a vote of 62 to 32 a transportation appropriations bill that would withhold federal highway funds from states that do not adopt an illegal per se limit of 0.08. The Clinton administration also endorsed this legislation. However, there was less support for this measure among Republican leaders in the House. After a period of intense lobbying, the Senate's initial decision was reversed: the final legislation did not withhold highway funds from states without a 0.08 BAC standard. And there was no threat of a presidential veto in response to this change (Pianin, 1998).[3] Commentators attributed the demise of this drunk-driving legislation in part to aggressive lobbying by the alcohol and restaurant industries as well as to a propensity among many legislators to allow states to make these decisions for themselves (Weisman, 1998). However, unclear statistical evidence on the effects of previous state-level 0.08 BAC laws was also cited as an important part of the public debate. The final legislation acknowledged this concern by directing the General Accounting Office (GAO) to evaluate the existing studies of the efficacy of state 0.08 BAC laws. In their subsequent report, the GAO (1999) cited several methodological concerns with this research in concluding that available evidence had not clearly established that the state-level 0.08 BAC laws actually reduced alcohol-related traffic fatalities.[4]

These concerns about the uncertain effects of state-level experiences with 0.08 BAC laws are likely to surface again: in its most recent transportation appropriations bill, the Senate has again approved the withholding of highway funds from states that do not have a 0.08 BAC standard. This study addresses these issues by presenting new empirical evidence on how state-level 0.08 BAC laws influenced traffic fatalities. These evaluations are based on data and empirical specifications that address the potential methodological limitations of the prior research reviewed in GAO (1999). For example, these evaluations are based on a relatively long (1982–1998) panel of annual state-level data on traffic fatality rates, instead of data on alcohol involvement in fatal crashes.[5] Furthermore, the specifications adopted here improve upon much of the previous literature partly by introducing a broader set of controls for potentially confounding and omitted determinants of traffic safety. These include explicit regressors that control for several of the key traffic-related policies that were also being introduced within states over this period (for example, other drunk-driving policies, seat-belt laws, speed limits). New state regulations that allow licensing authorities to revoke the driver's license of allegedly drunk drivers before any court action (administrative license revocations) are of particular concern in this context. More specifically, several of the states that introduced 0.08 BAC laws introduced administrative license revocations almost simultaneously (Table 1). Some studies have been criticized for failing to control for the possibly confounding influence of this contemporaneous drunk-driving policy (GAO, 1999).

[3] However, as part of the compromise, the legislation allocated $500 million for incentive grants to states that adopted the 0.08 BAC standard.

[4] However, GAO (1999) noted that there were "strong indications" that the interaction of these laws with other drunk-driving measures may be effective. The GAO study also suggested that direct medical evidence of driver impairment at such BAC levels should be considered.

[5] One criticism of some previous studies has been that they have had too little data after the adoption of 0.08 BAC laws. Another potential shortcoming in prior studies of 0.08 BAC laws has been the focus on rates of alcohol involvement in fatal crashes. Since alcohol involvement in fatal crashes is not always determined, much of the available data are actually imputed. In contrast, the prevalence of traffic fatalities, which are arguably the true outcome of interest, is essentially observed in every state and year without error.

Table 1. Effective dates of 0.08 BAC laws and administrative license revocations, 1982–1998.

	Effective Dates	
State	Illegal Per Se at 0.08 BAC	Administrative License Revocation
Alabama	August 1995	August 1996
California	January 1990	July 1990
Florida	January 1994	October 1990
Idaho	July 1997	July 1994
Illinois	July 1997	January 1986
Kansas	July 1993	July 1988
Maine	August 1988	January 1986
New Hampshire	January 1994	July 1992
New Mexico	January 1994	July 1984
North Carolina	October 1993	October 1983
Oregon	October 1983	July 1984
Utah	August 1983	August 1983
Vermont	July 1991	December 1989
Virginia	July 1994	January 1995

Note: Hawaii, Washington, Texas, Kentucky, and Rhode Island also adopted 0.08 BAC laws in 1995, 1999, 1999, 2000, and 2000, respectively.

Furthermore, several traffic safety studies have recognized that other important and unobserved determinants of traffic safety may vary substantially from one geographic area to another as well as over time periods (for example, state-specific or year-specific cultural sentiment toward drunk driving). As in these studies (for example, Benson, Rasmussen, and Mast, 1999; Cook and Tauchen, 1984; Dee, 1999; Evans and Graham, 1988; Evans, Neville, and Graham, 1991; Mast, Benson, and Rasmussen, 1999; Ruhm, 1996; Young and Likens, 2000), the results presented here control for the influence of these unobserved and potentially confounding omitted variables through the use of state and year fixed effects. This study also presents some counterfactual evaluations that validate this study's key inferences by exploiting the patterns in the timing of alcohol involvement in fatal traffic accidents. It is well known that traffic fatalities that occur on weekends and at night are substantially more likely to involve drunk driving than those that occur during the day or on weekdays. This pattern presents a compelling opportunity to evaluate the reliability of the inferences presented here. More specifically, if the fixed effects specifications were generating reliable inferences, life-saving benefits of introducing drunk-driving policies like 0.08 BAC laws would be expected to be relatively concentrated in observed reductions of weekend and nighttime traffic fatalities. If, in contrast, these models suggest that such policies are more effective in reducing daytime and weekday traffic fatalities, it would point to the possible existence of confounding specification errors.

The results of these evaluations clearly indicate that the adoption of state-level 0.08 BAC laws generated large and statistically significant reductions in the prevalence of traffic fatalities. Furthermore, these results suggest that the law-driven reductions in traffic fatalities were particularly large among teenagers and young adults. However, it is important to note that 0.08 BAC laws were almost never in effect without administrative license revocations (Table 1). This implies that the "direct" effects of

0.08 standards cannot be effectively distinguished from its potentially interactive effects with administrative license revocations. However, from a policy perspective, this caveat is not particularly constraining given that most states already have administrative license revocations in place. In particular, even under the conservative assumption that 0.08 BAC laws would only save lives when combined with administrative license revocations already in effect, the results presented here suggest that the nationwide adoption of this policy would reduce traffic fatalities by roughly 1200 annually.

EVALUATING 0.08 BAC LAWS

According to the National Highway Traffic Safety Administration (NHTSA), an average 170-pound man would reach a BAC of 0.08 by consuming his fifth 12-ounce beer (4.5 percent alcohol by volume) within a two-hour period (GAO, 1999). An average 120-pound woman would have a BAC of 0.08 after consuming three beers over the same period.[6] Varied evidence suggests that driving at such levels of intoxication is associated with increased traffic fatality risk (Levitt and Porter, 1999; Zador, 1991; Zador, Krawchuk, and Voas, 2000). For example, Zador, Krawchuk, and Voas (2000) found that the fatality risk for drivers with blood alcohol concentrations between 0.08 and 0.10 was at least six times higher than for sober drivers and that the increased risk was particularly high among young males. Similarly, NHTSA claims that impairment of visual function, reaction time, steering, and emergency responsiveness is substantial among drivers with a 0.08 BAC (GAO, 1999). However, alcohol industry associations have disputed this evidence and suggested that a nationwide 0.08 BAC law would only punish "responsible social drinking."[7]

More direct evidence on the potential efficacy of 0.08 BAC laws has been based on reduced-form evaluations of the available state-level experiences with such regulations. More specifically, seven studies have evaluated how the adoption of 0.08 BAC laws may have influenced the proportion of alcohol involvement in fatal crashes and the number of alcohol-related fatalities (GAO, 1999). Two studies focused on the effects of California's 0.08 BAC law, which was adopted at the beginning of 1990 (NHTSA, 1991; OTS, 1995). The first of these studies reported a 12 percent decline in alcohol-related fatalities after the adoption of the 0.08 BAC law. However, the GAO criticized this study in part because the post-law period was so short and because just six months into this post-law period, California introduced administrative license revocations for drunk drivers (Table 1). The second California study, which was based on four years of data, reported mixed results regarding the 0.08 BAC law. NHTSA (1994) examined data from five states that adopted 0.08 BAC laws (California, Maine, Oregon, Utah, and Vermont). This study considered how six measures of alcohol involvement (driver involvement in fatal crashes by certain BAC levels, nighttime involvement) changed after the adoption of 0.08 BAC laws in these five states. They reported significant decreases in nine of the 30 measures. Hingson, Heeren, and Winter (1996) also evaluated the changed rates of alcohol involvement in fatal crashes for these five states. However, this study compared the changes in these states with those in nearby comparison states to provide potential controls for the shared but unobserved time-

[6] Such calculations vary because the absorption of alcohol into the bloodstream depends on a number of individual characteristics, such as age.

[7] Public rhetoric on how many drinks it actually takes to reach a 0.08 BAC (and, by implication, what may constitute responsible social drinking) has often been based on misleadingly varied choices of weight, gender, and drink type for a representative person (Gawande, 1998).

series determinants of alcohol involvement.[8] Nonetheless, both of these studies have been reasonably criticized for failing to control for other important time-varying determinants. In particular, as Hingson, Heeren, and Winter (1996) recognized, three of the five states in these studies also adopted administrative license revocations within only 10 months of their 0.08 BAC law (Table 1). The study by Hingson, Heeren, and Winter (1996) has also been criticized for the potentially problematic nature of the comparison states.[9]

NHTSA released three other studies on 0.08 BAC laws in April of 1999. One of these studies (Foss, Stewart, and Reinfurt, 1998) focused on North Carolina and concluded that the 0.08 BAC law had no clear effect and that reductions in alcohol-related traffic fatalities appeared to be part of a long-term trend that began before the adoption of a 0.08 BAC law. Apsler et al. (1999) presented time-series evaluations for the 11 states that had a 0.08 BAC law by the end of 1994. They found that 0.08 BAC laws significantly reduced alcohol involvement in only two to five of these 11 states. Voas, Tippetts, and Fell (2000) conducted an evaluation of 0.08 BAC laws by estimating regression models based on quarterly data from all 50 states and the District of Columbia from 1982 through 1997. These regression models included controls for other determinants of alcohol involvement such as administrative license revocations, vehicle miles traveled, urbanicity, shared trends, and fixed state-level variables for whether each state had adopted certain traffic safety policies at any time over this period.[10] They found that 0.08 BAC laws reduced the involvement of drinking drivers relative to sober drivers. GAO (1999) criticizes this study in part for excluding young drivers, noting that many young drivers have been prosecuted under the 0.08 BAC law in California.

In reviewing the mixed evidence from these seven state-level studies, GAO (1999) concluded that they fell short of clearly establishing the efficacy of 0.08 BAC laws. This study presents novel evaluations of the effect of 0.08 BAC laws, which address the potential methodological limitations of these previous studies. One class of innovations in this study simply involves the nature of the data being analyzed. The evaluations presented here are based on annual state-level panel data on traffic fatality rates from 1982 to 1998. These data are sufficiently recent to provide observations well after most states enacted 0.08 BAC laws (Table 1). Furthermore, these data reflect total fatalities including those among young adults who were excluded from some prior studies (for example, NHTSA, 1994; Voas, Tippetts, and Fell, 2000).[11] And some of the evaluations presented here focus specifically on traffic fatality rates among younger adults. However, another potentially important distinction in the data set under study here is simply that the key outcomes are traffic fatality rates. Most studies have instead examined how 0.08 BAC laws influenced rates of alcohol involvement in fatal crashes. The rate of alcohol involvement in crashes is undoubtedly a policy-

[8] This approach is analogous to a basic "difference-in-differences" estimator, since it compares changes in the "treatment" state to contemporaneous changes in the "control" states. The preferred regression specifications adopted here, which include state and year fixed effects, provide a more general and flexible variation on this basic identification strategy.

[9] For example, California was paired with Texas. GAO (1999) suggests that, in this context, it is better to compare "treatment" states to several states or the rest of the nation. The two-way fixed effects specifications employed here effectively adopt this approach and allow the introduction of other potentially relevant controls that vary within states over time.

[10] However, these specifications included trend variables instead of year fixed effects. They also omitted unrestrictive state fixed effects, including instead time-invariant dummy variables for states that had certain traffic safety policies any time over the study period. Their specifications also excluded variables representing other potentially important policies that varied within states over this period (speed limits and other drunk-driving policies).

[11] GAO (1999) noted that in 1997, more under-21 California drivers were convicted under the state's 0.08 BAC law than under the "zero tolerance" law.

relevant outcome as it is strongly associated with fatality risk. However, tests for alcohol involvement are not actually conducted and recorded for all fatal crashes. Therefore, NHTSA has simply imputed much of the available data on alcohol involvement. In contrast, the actual number of traffic fatalities, which are ultimately the outcome of interest, is essentially observed without error in each state and year.[12] Similarly, other key attributes of fatal crashes known to be associated with alcohol use (time of accident, age of victims) are also recorded for nearly all accidents and allow construction of alcohol-sensitive measures of traffic fatalities.

A second class of innovations in this study involves the research design employed to identify the effects of 0.08 BAC laws. The most recent studies on 0.08 BAC laws have employed multiple regression techniques to purge the potentially confounding influence of other observed and unobserved determinants of traffic safety (Apsler et al., 1999; Voas, Tippetts, and Fell, 2000). However, the number of controls included in previous studies may be too limited. The period over which 0.08 BAC laws were adopted was characterized by considerable within-state variation in other important policies related to traffic safety (other drunk-driving measures, seat-belt laws, speed limits, etc.). Furthermore, other less tangible attributes that influence traffic safety may also vary substantially from one geographic area to another and over periods of time (for example, state-specific or period-specific cultural sentiment toward drunk driving). By definition, such unobserved determinants are inherently difficult to measure. However, omitting controls for these determinants could easily bias statistical inferences regarding traffic safety measures as well as attenuate the precision of those inferences. Several empirical studies of traffic safety have controlled for such omitted variable biases by introducing state and year fixed effects, which unambiguously purge the influence of unobserved state-specific determinants as well as shared, year-specific determinants.[13] This study presents multiple regression results based on such two-way fixed effects models. As noted earlier, the method adopted by Hingson, Heeren, and Winter (1996) is conceptually consistent with this approach because it relies on comparing the within-state changes in 0.08 BAC states with the contemporaneous changes in states that did not adopt 0.08 BAC laws. However, the two-way fixed effects models presented here generalize this approach in at least two important ways. One is that the effective "comparison" states are less selective because they are drawn from the entire nation. The second is that it allows for other important robustness checks because other variables reflecting important state policy changes over this period can easily be included as controls.

Nonetheless, even the use of fixed effects and an expanded set of control variables does not obviate all reasonable concerns about the possibly confounding influence of omitted variables or other specification errors. As an additional check on these results, this study presents evidence from counterfactual estimations that attempt to exploit the patterns in the timing of alcohol-related accidents. It is well established that the rates of alcohol involvement in fatal crashes are substantially larger during weekends and at nighttime.[14] For example, NHTSA (1999) reports that, in 1988, 49 percent of the drivers killed during weekends were in accidents involving someone who was

[12] Since 1975, NHTSA has obtained data on all traffic-related fatalities through its Fatal Accident Reporting System (FARS). The economic literature on traffic safety has focused almost exclusively on fatalities as the key dependent variable (for example, Chaloupka, Saffer, and Grossman, 1993; Cook and Tauchen, 1984; Dee, 1999; Evans and Graham, 1988; Evans, Neville, and Graham, 1991; Mast, Benson, and Rasmussen, 1999; Ruhm, 1996; Young and Likens, 2000).

[13] Ruhm (1996) addressed this issue directly and finds that the omission of such controls can lead to confounded inferences about alcohol-related traffic safety policies.

[14] NHTSA (1999) defines the weekend as the period from 6:00 PM on Friday to 5:59 AM on Monday and defines nighttime as the period from 6:00 PM to 5:59 AM. These definitions are also adopted here.

intoxicated (that is, a BAC of at least 0.10). In contrast, only 29 percent of the drivers killed during weekdays in 1988 were in accidents involving someone who was intoxicated. Similarly, the rate of alcohol involvement for driver fatalities in 1988 was 56 percent at night and 16 percent during the day.[15] Because of these patterns, several empirical studies of alcohol policies (including this one) have focused on these alcohol-sensitive weekend or nighttime outcomes. However, the much lower rates of alcohol involvement in weekday and daytime fatalities also present a compelling opportunity. More specifically, if the conventional regression models were generating reliable inferences about the effects of 0.08 BAC laws, one would reasonably expect these effects to be smaller in similarly specified models of weekday or daytime traffic fatalities. However, if 0.08 BAC laws appeared to have relatively large and statistically significant effects in such models, it would suggest a confounding specification error. This study presents such counterfactual evidence by comparing the results from similarly specified models of weekend, weekday, nighttime, and daytime traffic fatality rates. These ad hoc comparisons are particularly useful in this context because they provide a compelling way of validating the inferences from these models without simply introducing additional controls that exhaust the already limited sample variation in 0.08 BAC laws and traffic fatalities. However, the power of these simple comparisons as a specification test should not be overstated. Comparisons of evaluation results for models of daytime, nighttime, weekend, and weekday traffic fatalities may yield a plausible heterogeneity even in the presence of some specification error. Furthermore, drunk-driving measures such as 0.08 BAC laws may actually have no detectable effects on daytime and weekday traffic fatalities if efforts at enforcement are substantially lower during these periods. Alternatively, 0.08 BAC standards may have larger effects on weekday and daytime fatalities if those at risk for driving drunk during these periods are more responsive to illegal per se laws. Nonetheless, the patterns of response heterogeneity across these types of traffic fatalities can provide a useful, additional commentary on this study's main results.

DATA AND SPECIFICATIONS

These traffic safety evaluations are based on a panel of annual state-level data from 1982 to 1998. The data on traffic fatalities were drawn from the Fatal Accident Reporting System (FARS), which is administered by the U.S. Department of Transportation and contains fairly detailed data on every traffic accident involving a fatality in the United States. Combining FARS and state population data, total, weekend, weekday, nighttime, and daytime traffic fatality rates were generated by state and year (Table 2). The traffic fatality rates defined by accident day or hour exhaust all observed traffic fatalities except for the small number for which the hour or day was not recorded. As in much of the literature, Alaska, Hawaii, and District of Columbia were excluded from this analysis, which implies a final data set with information over 17 years from 48 states ($n = 816$). The key independent variable in these evaluations is an indicator equal to one for states in years when they have an effective 0.08 BAC law. For states and years, where laws become effective at some point during the year, fractional values are employed. Another binary indicator identifies states in years when it was illegal per se to drive with a BAC of 0.10 or more. A third key regressor identifies whether the state licensing authority is allowed

[15] These patterns of alcohol involvement are typical even though they are partly based on imputed data and are only for 1988 drivers. According to the author's calculations with the 1982–1998 FARS data on all traffic fatalities, the patterns of police-reported rates of alcohol involvement are quite similar.

Table 2. Variable means, state panel data, 1982–1996.

Variable	**Mean (standard deviation)**
Traffic fatality rate per 100,000	19.1 (5.8)
Weekend (Friday, 6:00 PM to Monday, 5:59 AM)	8.4 (2.8)
Weekday (Monday, 6:00 AM to Friday, 5:59 PM)	10.6 (3.2)
Nighttime (6:00 PM to 5:59 AM)	10.1 (3.4)
Daytime (6:00 AM to 5:59 PM)	8.8 (2.8)
Illegal per se at 0.08 BAC	0.12 (0.32)
Illegal per se at 0.10 or higher BAC	0.75 (0.42)
Administrative license revocation	0.51 (0.49)
Dram shop statute or case law	0.77 (0.42)
Mandatory jail time for first DUI offense	0.29 (0.45)
Zero tolerance law	0.31 (0.45)
Mandatory seat belt law—primary enforcement	0.20 (0.40)
Mandatory seat belt law—secondary enforcement	0.43 (0.50)
65 MPH speed limit	0.52 (0.50)
70+ MPH speed limit	0.09 (0.29)
Vehicle miles traveled (100,000)	43,723 (44,344)
State unemployment rate	0.06 (0.02)
Real state personal income per capita (100,000)	0.13 (0.02)
Number of observations	816

Note: Alaska, Hawaii, and the District of Columbia are omitted.

to suspend a driver's license before any court action related to a charge of drunk driving. As noted, this may be a particularly relevant control variable because several of the states that adopted 0.08 BAC laws almost simultaneously adopted administrative license revocations (Table 1).

Another drunk-driving control variable is a binary indicator for whether the state has "dram shop" case law or statutes. More specifically, the dram shop variable represents whether, in a particular state and year, those injured by drunk drivers are allowed to bring suit against alcohol servers. Other alcohol-related policy variables identify whether the state mandates jail time for first-time drunk drivers and whether it is illegal per se to drive with a positive BAC if the driver is not of legal drinking age ("zero tolerance" laws).[16] Controls for three traffic-related laws unrelated to alcohol use are also included (Table 2). Laws unrelated to alcohol use include two binary indicators for mandatory seat-belt laws. Seat-belt laws with primary enforcement allow the police to stop a motorist for not wearing a seat belt. Secondary enforcement implies that a violation can only be assessed if the driver were pulled over for some other reason. Seat belts are known to reduce traffic fatality risk substantially. Research suggests that the enforcement level for mandatory seat-belt laws did substantively influence the magnitude of the increases in belt usage (Dee, 1998). Two other binary indicators identify states that have increased their highest speed limit to 65 miles per hour or to 70 or more miles per hour.[17] Three other controls (vehicle miles traveled, the state unemployment rate, and real state personal income per capita) reflect road usage and the state macroeconomic environment and are typically associated with road congestion, alcohol use, and, by implication, safety on the roads (Evans and Graham, 1988).

The canonical two-way fixed effect specification for traffic fatality models takes the following basic form:

$$Y_{st} = \mathbf{X}_{st}\beta + \gamma Z_{st} + w_s + v_t + \epsilon_{st}$$

where Y_{st} is the dependent variable, X_{st} contains the control variables, Z_{st} is the indicator for 0.08 BAC laws. The terms, w_s and v_t, are state-specific and year-specific fixed effects and ϵ_{st} is a mean-zero random error. The results reported here are based on a semi-log model in which Y_{st} is the natural log of the fatality rate per 100,000 in the population in a given state and year.[18] The probable existence of heteroskedasticity is treated in an unrestrictive manner through the use of a White correction. Given that this correction is only valid asymptotically and some of these models have relatively few degrees of freedom, a finite sample correction that increases the standard errors is applied (Davidson and MacKinnon, 1993). The most basic specifications reported here include only the two indicators for illegal per se laws and state and year fixed effects. This model is roughly consistent with the methods employed by Hingson, Heeren, and Winter (1996) because the identification of policy responses effectively relies on within-state comparisons but excludes a control for administrative license revocations. The two other specifications evaluate the robustness of the results from this sparse model by first introducing the binary indicator for administrative license revocations and, then, the remaining control variables. Several additional robustness checks are discussed along with the evaluation results.

[16] DeJong and Hingson (1998), Hingson (1996), and Zador et al. (1989) survey research regarding these drunk-driving policies. In general, these surveys suggest that most policies of general and specific deterrence have been effective. However, some of the surveyed evidence may not be robust to specifications that include state and year fixed effects (for example, Ruhm, 1996; Young and Likens, 2000). I have also replicated this study's results in models that include the state minimum legal drinking age (MLDA). However, that policy variable is excluded in the results reported here because the state increases in MLDA began well before the study period and ended by 1988.

[17] Evidence on the effects of higher speed limits is mixed. Lave and Elias (1994) suggest that the movement in the late 1980s to 65 MPH speed limits actually reduced overall fatalities by redirecting traffic away from more dangerous secondary roads and influencing patterns of enforcement. However, this conclusion has been challenged in recent studies (for example, Farmer, Retting, and Lund, 1999) which also considered the effects of more recent movements to speed limits above 65 MPH. The evaluations presented here provide further evidence on this question.

[18] The results from linear probability models and logistic models for grouped data are quite similar.

RESULTS

Total Traffic Fatalities

The key estimation results from the fixed effect models for total traffic fatality rates are reported in Table 3. The first column presents the results for the sparsest specification, which includes only the state and year fixed effects and two variables representing illegal per se laws. These statistically significant estimates suggest that 0.08 BAC laws reduced total traffic fatality rates by 16.5 percent and that less stringent BAC standards (0.10 or higher) reduced fatality rates by 10.1 percent. Interestingly, the magnitude of this basic within-state estimate roughly parallels Hingson, Heeren, and Winter's (1996) finding that 0.08 BAC laws reduced alcohol involvement in fatal crashes by 16 percent. However, the next two models examine the robustness of such within-state comparisons by introducing explicit regressors that control for the influence of other potentially important and confounding determinants of traffic safety. The results of these models clearly indicate that the omission of such controls can

Table 3. Least-squares estimates, semi-log models for total traffic fatality rates.

Independent Variables	**(1)**	**(2)**	**(3)**
Illegal per se at 0.08 BAC	–0.165‡ (0.030)	–0.121‡ (0.030)	–0.072‡ (0.028)
Illegal per se at 0.10 or higher BAC	–0.101‡ (0.023)	–0.089‡ (0.023)	–0.053‡ (0.020)
Administrative license revocation	—	–0.081‡ (0.014)	–0.061‡ (0.013)
Dram shop statute or case law	—	—	–0.016 (0.018)
Mandatory jail time for first DUI offense	—	—	0.007 (0.014)
Zero tolerance law	—	—	–0.002 (0.013)
Mandatory seat belt law—primary enforcement	—	—	–0.034† (0.017)
Mandatory seat belt law—secondary enforcement	—	—	–0.011 (0.012)
65 MPH speed limit	—	—	–0.023 (0.015)
70+ MPH speed limit	—	—	0.057† (0.024)
ln (vehicle miles traveled)	—	—	0.159† (0.068)
State unemployment rate	—	—	–3.7‡ (0.3)
Real state personal income per capita	—	—	–0.537 (0.907)
R^2	0.9155	0.9195	0.9395

Note: Heteroskedastic-consistent standard errors are reported in parentheses. All models include state and year fixed effects. * Statistically significant with a p-value <0.10; † Statistically significant with a p-value <0.05; ‡ Statistically significant with a p-value <0.01

lead to misleading inferences about the efficacy of 0.08 BAC laws. For example, introducing only the one variable representing administrative license revocations reduced the estimated effect of 0.08 BAC laws by 27 percent (that is, [12.1–16.5]/16.5). Introducing the remaining set of control variables reduced the estimated effect of 0.08 BAC laws even further.[19] However, this preferred specification still suggests that 0.08 BAC laws were highly effective at saving lives, generating a statistically significant reduction of 7.2 percent in traffic fatality rates.

Four specific robustness checks regarding the results of these models are worth noting. One involves this study's use of state fixed effects. Several recent traffic studies have emphasized that such fixed effects provide important controls for the unobserved and state-specific determinants of traffic safety that could confound policy evaluations (for example, cultural sentiment towards drunk driving). Furthermore, in each of the three models in Table 3, *F*-tests indicate that the state fixed effects are jointly significant determinants of total traffic fatality rates. However, some studies avoid the use of these controls, in part because it removes much of the available sample variation. In this context, this issue is largely a moot one. The results of Model 3, when state fixed effects are excluded, suggested that 0.08 BAC laws had a much larger effect, reducing traffic fatality rates by 13.4 percent (*t*-statistic = 3.56 in absolute value). Therefore, the results of the two-way fixed effects models reported here could simply be construed as identifying conservative lower bounds on the true effects of this BAC standard. A second robustness check involves alcohol taxes. Recent studies clearly indicate that the excise taxes on beer do not have robust effects on beer consumption or traffic fatalities (Dee, 1999; Mast, Benson, and Rasmussen, 1999; Young and Likens, 2000). However, this literature has not addressed the influence of liquor taxes on traffic fatalities, despite earlier evidence linking the within-state variation in liquor taxes with the prevalence of alcohol abuse and traffic fatalities (Cook, 1981; Cook and Tauchen, 1982). That omission does not appear to be meaningful in this context either. A model that includes the real value of the state and federal taxes on liquor indicated that 0.08 BAC laws reduced traffic fatality rates by 7.5 percent.[20] A third robustness check involves the regulations allowing administrative license revocations in Maryland and Massachusetts. In these two states, the administrative license revocations during this period were predicated on specific BAC levels (0.08 in Massachusetts, 0.10 in Maryland) even when it was not illegal per se to drive at specific BAC levels in these states. However, this distinction does not appear to be relevant to this study's results. A model that excludes all data from Maryland and Massachusetts indicates that 0.08 BAC laws reduced total fatality rates by a statistically significant 8 percent (*t*-statistic = 2.78 in absolute value). A fourth robustness check examined the possibility that other traffic safety determinants are omitted from the models in Table 3, confounding its results. More specifically, this check consisted of introducing state-specific, linear trend variables to control for the other unobserved time-series variation specific to each state. Models that include these controls suggest that 0.08 BAC laws reduced total traffic fatality rates by a weakly significant 4.5 percent (*t*-statistic = 1.72 in absolute value). Though this estimate is somewhat smaller, the relevance of this modest sensitivity should not be overdrawn given that including trend variables specific to each of the 48 states exhausts nearly all of the already limited within-state sample variation.

[19] An *F*-test indicates that the variables introduced in Model 3 are jointly significant.

[20] The absolute value of the *t*-statistic for this estimate is 1.94. These evaluations have somewhat less precision since the liquor tax is defined only for the 30 "license" states (n = 17 x 30 = 510). In the remaining "control" states, state authorities exercise monopoly control over the wholesale or retail sale of liquor and any statutory taxes may not be meaningful.

Table 4. Least-squares estimates, semi-log models for traffic fatality rates by accident day.

Independent Variables	**(1)**	**(2)**	**(3)**
	Dependent Variable: Weekend Fatality Rates		
Illegal per se at 0.08 BAC	–0.179‡	–0.134‡	–0.086†
	(0.038)	(0.039)	(0.037)
Illegal per se at 0.10 or higher BAC	–0.101‡	–0.087‡	–0.056†
	(0.028)	(0.028)	(0.025)
Administrative license revocation	—	–0.085‡	–0.064‡
		(0.016)	(0.015)
R^2	0.8919	0.8957	0.9140
	Dependent Variable: Weekday Fatality Rates		
Illegal per se at 0.08 BAC	–0.151‡	–0.110‡	–0.058*
	(0.032)	(0.033)	(0.032)
Illegal per se at 0.10 or higher BAC	–0.100‡	–0.087‡	–0.048†
	(0.025)	(0.026)	(0.023)
Administrative license revocation	—	–0.079‡	–0.057‡
		(0.016)	(0.016)
R^2	0.8797	0.8835	0.9056
State-year covariates?	no	no	yes

Note: Heteroskedastic-consistent standard errors are reported in parentheses. All models include state and year fixed effects. * Statistically significant with a p-value <0.10; † Statistically significant with a p-value <0.05; ‡ Statistically significant with a p-value <0.01.

The remaining estimates from Model 3 in Table 3 suggest the plausible influence of other attributes and policies related to traffic safety. For example, like several prior studies, these results underscore the cyclical nature of traffic fatalities as well as the significant effects of 0.10 BAC standards, mandatory seat-belt laws, and administrative license revocations (for example, Evans and Graham, 1988; Ruhm, 1996). In contrast, these results also suggest that dram-shop policies, mandatory jail terms, and zero tolerance laws were relatively ineffective. However, the apparent ineffectiveness of zero tolerance laws should be qualified because the youths affected by this policy constitute only a fraction of total traffic fatalities. In fact, the subsequent estimation results for fatalities among 18- to 20-year-olds (Table 6) indicate that these laws do appear to have been effective, reducing traffic fatality rates for this group by 6.2 percent (t-statistic = 1.93 in absolute value). As in Farmer, Retting, and Lund (1999), the results in Table 3 suggest that the recent increases in speed limits above 70 MPH significantly increased traffic fatality rates. However, these results also indicate that the earlier movement to 65 MPH speed limits did not have a detectable effect.

Counterfactual Results

The results from models of total traffic fatalities (Table 3) indicate that the effects of 0.08 BAC laws can be overstated by statistical techniques that omit controls for the contemporaneous variation in other traffic safety policies and determinants. Nonetheless, these results also indicate that state-level 0.08 BAC laws have been effective, reducing traffic fatality rates by roughly 7.2 percent. Though this result appears to be quite robust, it can be further validated by considering models that exploit the distinct timing of alcohol involvement in fatal accidents. As noted, rates of alcohol involvement in fatal traffic accidents are higher during weekends and at night. This implies that, if a particular specification were generating reliable inferences about drunk-driving policies like 0.08 BAC laws, the policy effects would be relatively concentrated in models of weekend and nighttime traffic fatalities. However, if the observed benefits of 0.08 BAC laws were sharply concentrated in similarly specified models of weekday or daytime fatalities, it would suggest the existence of a confounding specification error in the particular model.[21]

The results in Tables 4 and 5 offer direct evidence on this question by presenting the key results of the regression models for weekend, weekday, nighttime, and daytime fatalities. In general, these results suggest that the basic two-way fixed-effect models (Table 3) are generating reliable inferences. For example, the results in Table 4 uniformly indicate that 0.08 BAC laws led to larger percentage reductions in weekend

[21] The power of this "falsification test" should not be overstated. However, these counterfactuals would be clearly suggestive of specification error if implausibly large reductions in traffic fatalities were associated with 0.08 BAC laws in such models.

Table 5. Least-squares estimates, semi-log models for traffic fatality rates by accident time.

Independent Variables	**(1)**	**(2)**	**(3)**
	Dependent Variable: Nighttime Fatality Rates		
Illegal per se at 0.08 BAC	–0.155‡	–0.107‡	–0.065*
	(0.036)	(0.037)	(0.035)
Illegal per se at 0.10 or higher BAC	–0.097‡	–0.083‡	–0.054†
	(0.028)	(0.028)	(0.025)
Administrative license revocation	—	–0.090‡	–0.063‡
		(0.016)	(0.015)
R^2	0.8969	0.9012	0.9195
	Dependent Variable: Daytime Fatality Rates		
Illegal per se at 0.08 BAC	–0.159‡	–0.119‡	–0.062*
	(0.032)	(0.033)	(0.033)
Illegal per se at 0.10 or higher BAC	–0.088‡	–0.076‡	–0.034
	(0.024)	(0.024)	(0.023)
Administrative license revocation	—	–0.076‡	–0.061‡
		(0.016)	(0.016)
R^2	0.8916	0.8948	0.9132
State-year covariates?	no	no	yes

Note: Heteroskedastic-consistent standard errors are reported in parentheses. All models include state and year fixed effects. * Statistically significant with a *p*-value <0.10; † Statistically significant with a *p*-value <0.05; ‡ Statistically significant with a *p*-value <0.01

Table 6. Least-squares estimates, semi-log models for traffic fatality rates by age groups.

	Dependent Variable: Traffic Fatality Rate By Age Group								
	Age 18–20			Age 21–24			Age 25+		
Independent Variables	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Illegal per se at 0.08 BAC	–0.214‡ (0.054)	–0.170‡ (0.055)	–0.140‡ (0.055)	–0.202‡ (0.050)	–0.162‡ (0.053)	–0.097* (0.052)	–0.152‡ (0.030)	–0.113‡ (0.030)	–0.067† (0.029)
Illegal per se at 0.10 or higher BAC	–0.164‡ (0.039)	–0.151‡ (0.039)	–0.121‡ (0.040)	–0.163‡ (0.037)	–0.151‡ (0.038)	–0.111‡ (0.037)	–0.095‡ (0.023)	–0.084‡ (0.022)	–0.052† (0.021)
Administrative license revocation	—	–0.083‡ (0.024)	–0.041* (0.027)	—	–0.075‡ (0.024)	–0.049† (0.024)	—	–0.073‡ (0.014)	–0.052‡ (0.013)
R^2	0.7483	0.7513	0.7692	0.7652	0.7681	0.7879	0.9138	0.9168	0.9302
State-year covariates?	no	no	yes	no	no	yes	no	no	yes
Mean fatality rate (standard deviation)		42.9 (15.7)			36.1 (12.5)			19.6 (6.3)	

Note: Heteroskedastic-consistent standard errors are reported in parentheses. All models include state and year fixed effects and the state-year controls (see Model 3 in Table 3). However, only the model for 18- to 20-year-olds includes the zero tolerance law. * Statistically significant with a p-value <0.10; † Statistically significant with a p-value <0.05; ‡ Statistically significant with a p-value <0.01

traffic fatalities than in those that occur during weekdays. More specifically, the estimates from Model 3 in Table 4 indicate that 0.08 BAC laws generated a statistically significant reduction of 8.6 percent in weekend fatalities but only a weakly significant reduction of 5.8 percent in weekday fatalities. This is precisely the sort of response heterogeneity that would be expected if the key inferences from these models were reliable. Table 5 presents the key estimation results for models of nighttime and daytime fatality rates. Interestingly, the results of Models 1 and 2 suggest somewhat implausibly that 0.08 BAC laws led to a slightly larger percentage reductions in daytime traffic fatalities than in those that occur at night. However, the differences in these estimates are relatively small (that is, well within one standard error). Furthermore, in the preferred specification that introduces all the available control variables, the estimated effect of 0.08 BAC laws on nighttime fatalities (a 6.5 percent reduction) becomes somewhat larger than the estimated effect for daytime fatalities (a 6.2 percent reduction). When compared with the weekend/weekday models (Table 4), these counterfactual results (Table 5) are neither particularly crisp nor precisely estimated. However, the relative plausibility of Model 3's results provides some additional evidence on the importance of including controls for the other traffic safety determinants that varied within states over time.

By Age Groups

Overall the results of the previous sections indicate that 0.08 BAC laws were highly effective in reducing the prevalence of traffic fatalities. These estimates appear to be quite robust and exhibit a plausible heterogeneity, particularly with respect to the day of fatal traffic accidents. However, another approach that may address important omissions from previous studies involves examining the possibly heterogeneous effects of 0.08 BAC laws on traffic fatality rates among specific age groups. Given that the rates of alcohol involvement in traffic fatalities are quite higher for younger drivers (NHTSA, 1999), the existence of such heterogeneity is a clear possibility. It is addressed here by replicating these evaluations with data on traffic fatality rates among three groups: 18- to 20-year-olds, 21- to 24-year-olds, and those 25 or more years old. These traffic fatality rates are based on age-specific populations for each state and year. The mean values of these rates are reported in Table 6 along with the key results from these evaluations. These estimates uniformly indicate that 0.08 BAC laws generated statistically significant reductions in the traffic fatality rates of all three age cohorts. However, as in the earlier models, the omission of controls for other traffic safety determinants (Models 1 and 2) leads to an upward bias in the apparent efficacy of 0.08 BAC laws. Furthermore, for each of the three specifications, these estimated effects of the 0.08 BAC standard are monotonically smaller among the older age cohorts. For example, the preferred specification (Model 3) indicates that 0.08 BAC laws reduced traffic fatality rates by 14.0 percent among 18- to 20-year-olds and by 9.7 percent among 21- to 24-year-olds, but only by 6.7 percent among those 25 and older.

CONCLUSIONS

Over the last 20 years, an extensive array of legislative initiatives has attempted to reduce the prevalence of drunk driving. Although, by most accounts, these efforts have been successful, drunk driving continues to exact a heavy toll. In 1998, 38 percent of the 41,471 traffic fatalities in the United States were classified as alcohol-involved (NHTSA, 1999). Such disturbing facts motivate the continued legislative efforts to discourage risky drunk driving. The focus of the most recent activity has largely been on state laws that establish an explicit blood alcohol concentration (BAC) at which it is illegal per se to drive. In most states, this standard has been set at a BAC of 0.10. However, to date, 19 states have adopted a stricter BAC standard of 0.08. Further expansion of this stricter drunk-driving standard has been under consideration in most states as well as at the federal level. The nationwide adoption of 0.08 BAC laws has been strongly supported by traffic safety advocates who argue that these regulations save lives by reducing driving at unsafe BAC levels. However, these claims have also been aggressively contested by the alcohol and restaurant industries, which argue that these regulations merely punish responsible social drinking. In 1998, a federal proposal sought to withhold highway funds from states that do not adopt a 0.08 BAC standard. Congressional negotiators ultimately rejected that proposal after a period of intense lobbying that one official characterized as "deep emotions versus deep pockets" (Dao, 1998).

However, much of the controversy over extending the 0.08 BAC standard has also focused on arguably legitimate concerns about the mixed empirical evidence on the efficacy of the earliest state laws. In particular, evaluations of 0.08 BAC laws have been explicitly criticized on a variety of methodological grounds (GAO, 1999). This study presents novel evaluations of state-level 0.08 BAC laws that address the criticisms raised in the GAO report as well as several specification issues that are not. The regression models presented here examine these potential shortcomings through the analysis of a relatively long and recent panel data set on traffic fatalities and through the inclusion of additional controls for other contemporaneous and potentially confounding determinants of traffic safety. The results suggest that methodological criticisms, like those raised in GAO (1999), are indeed valid. In particular, these evaluations indicated that the failure to control for the influence of other traffic safety policies could lead to highly inflated estimates of the life-saving benefits of 0.08 BAC laws.

Nonetheless, the results of these evaluations also demonstrated that state-level 0.08 BAC laws have generated statistically significant reductions in traffic fatality rates. The preferred specification indicate that this stricter BAC standard reduced fatality rates by 7.2 percent. This evidence appears to be quite robust and was validated, in part, by the results of counterfactual estimations that exploited the timing of alcohol involvement in fatal traffic accidents. Interestingly, these results also indicate that these policy-induced reductions in traffic fatalities were particularly large among younger drivers. One relevant caveat to these results is that the direct effects of 0.08 BAC laws cannot be clearly distinguished from their potentially interactive effects with administrative license revocations because states that adopted the 0.08 BAC standard almost always had administrative license revocations in effect (Table 1). However, this qualification is not particularly constraining with respect to the policy relevance of these results in light of the fact that most states have already adopted administrative license revocations. For example, the U.S. Congress is currently reconsidering withholding highway funds from any state without a 0.08 BAC standard. This study's results suggest that federal actions that led to the nationwide expansion of 0.08 BAC laws would generate a considerable reduction in the number of annual traffic fatalities. More specifically, this study's results can be used to estimate the number of lives that would be saved annually by expanding the 0.08 BAC standard under the conservative assumption that this policy would only be effective in states that already have administrative license revocations. Twenty-three states (excluding Alaska) currently have administrative license revocations but have not yet adopted

0.08 BAC laws. In these states, during 1998, there were roughly 90.1 million people and the total traffic fatality rate averaged 18.7 per 100,000 in the population. A 7.2 percent reduction in traffic fatality rates in these states would imply roughly 1200 lives saved annually. The evaluation results presented in Table 6 are a reminder that these saved lives would be disproportionately young. In considering the policy implications of such simulations and the future of BAC standards in the United States, it should also be noted that other types of evidence point to the likely efficacy of 0.08 BAC laws. Medical evidence suggests that driver ability is significantly impaired at this BAC level. Studies based on actual crash data (Levitt and Porter 1999; Zador, Krawchuk, and Voas, 2000) also demonstrate a sharply increased risk associated with driving at relatively modest BAC levels, which may not be conventionally associated with drunk driving.

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