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by

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I. Introduction

The past twenty-five years have witnessed a sea change in attitudes and legislation regarding driving under the influence of drugs and alcohol. Although the first laws against drunk driving were passed as long ago as 1910 by New York and by California in 1911, statutes in most states prior to about 1980 simply prohibited "driving while intoxicated," without providing specific guidelines to define "intoxicated." As a result, much discretion was involved in the arrest and prosecution of drinking drivers, and punishment was often light, involving a relatively small fine even for chronic offenders.

Since 1980, however, under pressure from advocacy groups such as Mothers Against Drunk Driving (MADD), insurance companies, and arguably most important, federal threats of loss of highway funds for non-compliance, all states have passed tougher drunk driving statutes incorporating explicit measures of presumed alcohol impairment. In 1980, for example, only 15 states had legislation establishing a permissible Blood Alcohol Content (BAC), with 0.10 grams/deciliter (g/d) then defined as the presumed level of intoxication, and no state provided for the automatic suspension of driving privileges (termed "Administrative License Revocation" or ALR) in the event of a drunk driving arrest (not conviction). By 2005 all states had a BAC limit, and at a lower threshold of 0.08 g/d, and only 9 states do not impose ALR. MADD (2005) lists 39 pieces of recommended state legislation for the prevention of drunk driving, ranging from ALR to Zero Tolerance for drinking by minor drivers; by 2005, the median state had passed 27 pieces, and all had passed at least 17.

Arguably, this flurry of legislation has had a measurable effect on traffic fatalities, the most destructive consequence of drunk driving. From 1982, the first year for which reliable measures of victims' BAC have been collected, to 2003 the number of alcohol-related traffic fatalities has fallen from 26,173 to17,013, even as the number of miles traveled has increased by 81 percent. More significant for supporters of alcohol control legislation, given that advances in automotive safety engineering over time have led to declines in fatalities from all causes, the rate of alcohol involvement in fatal crashes fell from 60 to 40 percent during the same period, indicating progress in getting drunk drivers off the streets.

[Figure 1 about here]

The good news over the past twenty five years is tempered, however, by more modest progress in the past decade or so in reducing total fatalities, and almost no progress in further reducing the rate of alcohol involvement in fatal crashes. Figure 1 displays the trends in total fatalities per million miles traveled (solid line, left axis) for the years 1975-2003 and the percentage of fatalities that are alcohol-related (dotted line, right axis) for the years 1982-2003. As shown in the chart, total fatality rates were constant until 1980, fell sharply during the 1980-82 recession, stabilized, then fell sharply again during the period 1987-92. After 1992, fatality rates continued to decline, but at a much slower pace than in the previous decade, with the number of total traffic deaths fluctuating around 40,000 annually for the past decade.

Alcohol-related fatalities have held steady at 40 percent of total fatalities since 1997, despite the continued strengthening of alcohol-control legislation, including the successful push in all states to reduce the BAC limit to 0.08 (again, under threat of loss of federal highway funds) and to impose zero tolerance restrictions for alcohol consumption by underage drivers. Other measures of alcohol-impaired driving, including surveys of self-reported drinking and driving by the Centers for Disease Control (CDC), cited in Quinlan, et al. (2005) and by the National Highway Traffic Safety Administration (NHTSA, 2003) report actual increases from 1997 to 2002 in the frequency of driving under the influence and in the number of drinks consumed when driving after drinking. These troubling trends call for a re-examination of the effectiveness of the many types of drunk-driving laws passed over the years: can we determine which laws have had meaningful and long-lasting effects?

Figure 2 displays the annual frequency of the adoption by the states of two important types of alcohol-control legislation, BAC laws (dark bar) and ALR laws (light bar), together with the annual percent change in total traffic fatalities per miles traveled (dark line, no markers) and alcohol related fatalities (light line, with markers) since 1980.

[Figure 2 about here]

Growth rates in both total and in alcohol-related fatalities per miles traveled declined throughout the remainder of the 1980s and into the 1990-1991 recession. As the economy recovered in 1992 onward, traffic fatality growth rates became less negative, even as a second wave of states adopted BAC and ALR laws. Since 1998, growth rates in both total and alcohol-related fatalities have more or less stabilized at around -2 percent per annum; hence the constant rate of alcohol involvement.

About half the states passed new laws in the early 1980s establishing BAC limits (almost all 0.10) and ALR laws. The vast majority of BAC laws passed after 1992 were at the more stringent 0.08 limit, as most states already had 0.10 laws on the books. Seventeen states passed BAC 08 laws since 2000, primarily in response to federal legislation again threatening the loss of highway funding for non-compliance. Proponents of BAC 08 laws, including MADD and the National Highway Traffic Safety Administration argue that drivers with BAC 08 are "significantly impaired," that the risk of a crash "rises...rapidly once a driver reaches or exceeds BAC 08," and that the BAC 08 limit "has the potential for saving hundreds of lives and reducing thousands of serious injuries each year."(NHTSA, 2003)

The imposition of BAC 08 laws via an effective federal mandate has not been without controversy, however. There are those who believe that states and local jurisdictions are better placed to decide driving laws. There are some who argue that the lower limit unfairly penalizes moderate drinkers who are unlikely to cause crashes while doing nothing to deter heavily intoxicated drivers who pose the more serious risk to themselves and to others. And there are those who may agree that a lower standard may indeed save some lives, but that the additional costs of enforcement, and the criminal penalties imposed on the marginal offender, exceed the benefits accruing from the law.[1]

The present study does not address issues of impairment, but only whether or not BAC 08 laws have been effective in reducing traffic fatalities relative to other measures, principally ALR penalties. The problem is in isolating the effects of one measure on fatality rates versus all the others that have been enacted over the period, while simultaneously controlling for safety improvements. Like previous studies such as Dee (2001) and Eisenberg (2003), the present study addresses this problem by using state-level traffic fatalities as the dependent variable in a pooled cross-section time-series analysis. The advantage of using pooled, state-level data is the ability

[1]In 2003, the latest available data total 17,013 alcohol-related fatalities; drivers accounted for 9,357 of those killed. Drivers with 0.07 and 0.08 BACs were about 10 percent of the total drivers killed, or 5.5 percent of total alcohol-related fatalities.

to control, through two-way fixed effects, unobservable differences across states and across time in a way that analyses using aggregate national data or individual state-level data cannot.

This research differs from its predecessors in some fundamental ways, however. One is the availability of more observations, an advantage of any later study, but one that is especially critical given the relatively short time series available, and the few states (5 only) with BAC 08 laws as late as 1993. The second is in the treatment of the time-series properties of the fatality series. The present study finds strong evidence of serial correlation in the residuals of the fixed effects regressions. We use a correction to standard tests of significance proposed by Bertrand, et al. (2004) to demonstrate that previous findings of a significant effect of BAC 08 legislation on traffic fatalities may have been overstated, even as the effect of ALR legislation remains significant.

Third, both driving and drinking behaviors are habitual and persistent, and the response to exogenous changes, such as new traffic laws, may be attenuated. Most of the previous research, however, treats the enactment of a new restriction as having instantaneous and permanent impact, which may contribute to the problem of serial correlation noted above.[2] To test this possibility, we also estimate a dynamic model, which produces results that are consistent with the hypothesis that adjustment to drunk driving legislation is gradual, not instantaneous.

Finally, this research employs event study methodology as a test of endogeneity bias. In an event study, a period prior to the "event" – in this case, a new alcohol-control law -- is used to estimate a baseline model of the variable of interest. The model is used to make predictions over

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[2]Eisenberg (2003) is an exception, but his treatment does not address a fundamental problem, to be discussed below.

the event window, a period surrounding the event.[3] The pattern of the prediction errors can provide both an alternative test of the effectiveness of various alcohol-control laws and a test of whether the laws occasion a change in driving behavior or are a result of driving behavior. For example, an unanticipated law that effectively reduced fewer traffic deaths would have no discernable pattern in the prediction errors in years immediately before and during the law's passage, and negative prediction errors in the years subsequent. On the other hand, large positive prediction errors in the years prior to a law's passage may indicate that the law is in response to behavior in the dependent variable, thereby violating the independence assumption between regressors and the error term.

The results of the combined analyses of pooled regressions, correcting for serial correlation, and event analyses indicate that the marginal effect of strengthening BAC laws from 0.10 to 0.08 has had little to no effect on traffic fatalities, whether measured in total or restricted to alcohol-related. On the other hand, consistent and significant reductions in fatalities follow Administrative License Revocation statutes. Because ALRs almost always use a BAC limit as a criterion, however, the results are properly interpreted as a partial effect conditioned on the existence of a BAC law.

The paper is organized as follows: Section II provides a brief review of the literature, Section III describes the data and methodology, Section IV the pooled regression results, Section V the event study, and Section VI concludes.

[3]In this paper, the estimation period stops three years prior to the enactment of the law, and the event window is the event year +/- two years.

II. Previous Research on Alcohol-Control Legislation

There is a voluminous literature on alcohol-control legislation. This selective review emphasizes studies focusing on, or at least with substantial coverage of, BAC 08 and ALR laws. Much of the relevant literature appears subsequent to the flurry of federal and state legislation passed in the early 1980s.[4]

Zador, et al. (1989) reviews the effects of three types of control legislation: BAC laws, ALR laws, and mandatory first-offense jail sentence laws, all enacted by states between 1975-1985. The Zador analysis compares changes in fatalities in states with legislation (the treatment group) to contiguous states without (the control group). Although all three types of legislation were found to contribute to reductions in fatalities, the effects were small, and only ALR laws were consistently significant across types of crashes and drivers.

The National Highway Traffic Safety Administration (NHTSA, 1994) noted improvement in several measures of alcohol-related fatalities following enactment of BAC 08 laws in five states (California, Maine, Oregon, Utah, and Vermont; all had pre-existing BAC 10 laws). In an independent study of the same states, Hingson, Heeren, and Winter (1996) also found significant effects of BAC 08 laws. Both studies have been criticized, however, on several grounds, including failure to adequately control for the passage of other alcohol-control legislation during the sample, not recognizing the role of safety improvements in reducing fatalities, and arbitrary selection of comparison states

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[4]The literature on alcohol control uses very similar methodology and econometric specification, differing only in the variable of interest, such as minimum drinking age, sobriety checkpoint, BAC laws, etc. Any properly specified model of control legislation will contain all or most of the same regressors.

In a meta-analysis, the U.S. General Accounting Office (GAO, 1999) evaluated seven government-sponsored studies of BAC 08 laws (five by NHTSA, a strong proponent of BAC 08 laws), concluding that the "...studies fall short of finding conclusive evidence that 0.08 BAC laws by themselves have been responsible for reductions in traffic crashes" (GAO, p.23). The GAO did find some evidence of BAC 08 law effectiveness when combined with other laws and with efforts at public education, but any effects depended on a number of other factors that were not controlled within the studies they examined, including levels of law enforcement, other laws in effect, and public attitudes.

Voas, et al. (2000) evaluate the implementation, in 1997, of BAC 08 in Illinois, a state with a pre-existing ALR law. None of Illinois's bordering states had passed a BAC 08 law by 1997, so these were thought to be good controls. Voas, et al. find that the ratio of drinking to non-drinking drivers fell in the 18 months following the enactment of Illinois's BAC 08 law, and a followup study by NHTSA (2001) found a continued reduction in the subsequent year (for a total of 30 months of data). Looking at the data from a somewhat different perspective however, the drop in the percentage of Illinois fatalities that were alcohol-related, from 46 percent in 1996 to 43 percent in 1997, can be compared to a percentage that had already been as low as 44 percent in 1994. Furthermore, the percentage has remained at or above 44 percent since 1998. Thus it is not clear whether the BAC 08 law had a permanent effect on alcohol-related fatalities, or whether the measured effects were only short-term, due perhaps to other factors like publicity surrounding the new law or greater enforcement efforts during the year or so after enactment.

Dee (2001) estimates pooled time-series of state-level traffic fatalities to address some of the methodological shortcomings of previous studies. State and year fixed effects control for heterogeneity in traffic fatalities across states and time. The time effects control for improved automotive safety, changing national attitudes, legislation toward drunk-driving, and other common time-varying but unobserved factors. The state effects control for cross-state differences in unobservable factors such as enforcement levels, traffic conditions, liquor laws, and so on. Using a technique commonly termed "differences-in-differences," wherein indicator variables are used to discriminate states with BAC 08 laws (the "experimental" group) from those that do not (the "control" group), Dee finds that the presence of a BAC 08 law reduces total traffic fatalities by 7.2 percent in his preferred specification, controlling for the effects of other traffic laws and for the state-specific business cycle.[5]

Using a similar differences-in-differences approach, Eisenberg (2003) tests directly and finds evidence confirming the hypothesis that the marginal effect of BAC 08 over BAC 10 laws is statistically significant and amounts to about an additional 2 percent reduction in fatalities. Eisenberg also tests for timing effects of BAC 08 laws, and finds that there is some evidence of delayed response (up to six years) to lower BAC limits. The lengthy delayed response is heavily dependent on just seven states that set the lower limits in the 1983-1993 period, however, and may be capturing part of the general downward trend in fatalities otherwise unexplained by the time effects during those years. Unlike Dee (2001), Eisenberg finds little support that ALR laws reduce fatalities in most specifications, but does find that graduated license laws have been effective in reducing fatalities in underage drivers.

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[5]The construction of the indicator variable for the BAC 08 law in Dee (2001) causes the interpretation of the coefficient to be the effect of any BAC law (because the BAC 10 law, included as a control variable, has its own indicator variable, which is set to zero when the BAC 08 law takes effect), instead of the marginal effect of BAC 08 over BAC 10, a point noted by Eisenberg (2003).

In general, studies that focus on single states and/or groups of states tend to find only mixed support for the position that BAC 08 laws have an effect on traffic fatalities beyond that of existing BAC 10 and ALR sanctions. Studies using pooled time-series cross-sections of state-level data tend to show stronger results for BAC 08 laws, but in no case in previous work are the time-series properties of the residuals examined very closely.

As Bertrand, Duflo, and Mullainathan (2002) have shown, however, standard errors in differences-in-differences models can be biased by the serial correlation that is endemic to most economic time series, reinforced by the certain serial correlation in the treatment (dummy) variable. In what follows, we re-examine the state-level data on traffic fatalities and alcohol laws using the differences-in-differences methodology, but with an eye toward the potential effects of serial correlation on the results. We also examine more closely the issue of the endogeneity of the laws themselves using event analysis to verify the timing of the legislation relative to changes in fatality rates.

III. Data and Methodology[6]

The dependent variable in the empirical analyses to follow is either the rate of total or of alcohol-related traffic fatalities per 100,000 population at the state level over the years 1980-2003

[6]Data on alcohol-related traffic legislation for the years 1982-1999 was kindly provided by Thomas Dee. Earlier data on legislation was taken from Zador, et al. (1989) and later data on legislation and all fatality data from the National Center for Statistics and Analysis at the NHTSA website at http://www-nrd.nhtsa.dot.gov/departments/nrd-30/ncsa/. State unemployment rates are from Dee and the Bureau of Labor Statistics; age data are from the Bureau of the Census.

for total fatalities and 1982-2003 for alcohol-related fatalities for the 48 contiguous states.[7] Other measures were considered, and have been used in the previous literature, including fatalities per miles driven or per registered drivers. We follow the more recent literature, including Dee (2000) and Eisenberg (2003), in using the rates per total population.

The independent variables include the variables of main interest, indicator variables for BAC and ALR laws, and several control variables, including seat belt and speed limit laws, business cycle variables, mileage traveled, and in some cases, demographic characteristics. The included controls, in particular those indicating the existence or absence of a specific alcohol-related statute, are by no means exhaustive, but as most of the variation in the model is explained by the state and time specific effects, and as the baseline results for the variables of main interest are comparable to previous research, the included controls were limited in the interest of parsimony. Descriptive statistics for all variables are listed in Table 1.

[Table 1 about here]

As shown in Table 1, both total and alcohol-related traffic fatality rates have fallen over time, with alcohol-related rates declining about twice as fast. The dispersion of alcohol-related rates as measured either by the range or by the standard deviation has also diminished much faster than total rates, perhaps reflecting the greater uniformity of alcohol-control laws across states, as discussed below.

The unemployment rate measures business cycle activity and has been shown by Ruhm (1996) and others to demonstrate procyclicality in traffic deaths. Vehicle Miles Traveled (VMT)

[7]Only since 1982 has a consistent methodology been established for counting alcohol-related traffic deaths, and even now there are wide variations across states in the proportion of drivers tested, alive or dead (Yi, et al., 1999).

accounts for traffic intensity, given the population level, and is positively related to fatality rates. The percent of the population 14-24 controls for the demographic segment with less driving experience and more risky behavior, and is positively correlated with fatality rates.

The indicator variables are coded 0-1 for states that have not or have enacted the described traffic control law, respectively.[8] Only 18 states had enacted BAC laws in 1982, all with maximum limit of 0.10. By the end of 2003, all states had BAC laws, with 45 at the lower limit of 0.08.[9] No states had ALR laws in 1982; by 2003, 39 states had enacted these laws. Seat belt laws were non-existent in 1982; by 2003, 13 states had laws allowing penalties for not wearing a seat belt only (primary enforcement), and 34 more allowing penalties for not wearing a seat belt when stopped for some other violation (secondary enforcement). Finally, all states followed the federally-mandated 55 miles per hour maximum speed limit in 1982; by 2003, 18 states had maximum limits at 70 miles per hour or above.[10]

The initial methodology to be employed is a two-way fixed effects specification of the pooled time-series cross-section regressions of state fatality rates on the indicator and continuous variables of the form:

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[8]For states enacting the law within the year, the variable is coded for the fraction of the time the law was in force.

[9]By August 2005, all states will have 0.08 limits.

[10]Other changes in state legislation of note over the time period are the institution of a uniform minimum drinking age, 21, across all states, and the enactment of zero tolerance laws for underage drinking while driving for all states. These actions have been shown to have effects on traffic fatalities among young drivers (see Kaestner [2000] and Carpenter [2004]) but were not found to be significant in the empirical analyses here or to cause material changes in the coefficients of the variables of interest.

$$y_{it} = \mu_i + \tau_t + \gamma' L_{it} + \varphi' X_{it} + \varepsilon_{it} \quad \textbf{(1)}$$

where y_{it} is the annual fatality rate (either total or alcohol-related) for state *i* in year *t*, $t = 1, \ldots,$ T, u_i is the state fixed effect, τ_t is the year time effect, L_{it} is a vector of indicator variables with values of 1 for the years in which the laws were in effect and 0 otherwise, and X_{it} is a vector of control variables. The coefficients of L_{it} are often described as "difference-in-difference" estimators; that is, the coefficient estimates the difference in the mean of the dependent variable of the "treatment" group before and after the passage of the law, less the same quantity for the "control" group.

There are potential problems with Ordinary Least Squares (OLS) estimates of equation (1). First, the error term, ε_{it}, is usually assumed to be i.i.d., but this assumption is often violated in practice. We show that autocorrelation in the residuals probably biased the significance tests of similar regressions in previous research. To address this issue, we employ "random inference" tests, a Monte-Carlo type approach to calculating the empirical distributions of the coefficients of the indicator variables. Bertrand, Duflo, and Mullainathan (2003) find that random inference performed the best among several alternatives in correcting problems of over-rejection in differences-in-differences estimators where autocorrelation is present.

The basic technique of random inference is straightforward. Suppose that law L_j is to be tested. Equation (1) is estimated so as is to obtain the coefficient $\hat{\gamma}_j$. Then, to construct the empirical distribution of γ_j , equation (1) is re-estimated with "pseudo-laws" generated randomly, using the same dates as in the original sample, but assigned randomly across the states.

In the full sample, for instance, there are 18 states that passed BAC 08 laws during the years 1980-2000, and 30 that did not. In the first draw, a year is chosen from the 18 + 30 possibilities (duplicate years among the 18 are included; the 30 are assigned zeroes). In the second draw, a state is chosen and assigned the "law" from the first draw (either one of the true law-years or zero). The procedure is repeated for all 48 states, and the estimate of the pseudo-γ_j is made and stored. The entire process is repeated 10,000 times to generate the empirical distribution, and from the distribution, the empirical p-values.

As an alternative to the static fixed-effects model, we also estimate a dynamic, lagged dependent variable (LDV) model. A model of this form can be justified as an application of a Koyck-type geometric lag in the impact of the new law on traffic fatalities. In this scenario, it takes time for the full effect of a new law to take place. As in the static model, random inference tests are conducted to construct empirical *p*-values.

Another potential problem in the differences-in-differences approach is the assumption of random assignment of "treatment" and "control" states. Laws are not passed randomly, however, but in response to a perceived need for legislative remedy to address a problem. If states passing alcohol-control legislation were those with the more serious alcohol-related traffic problems, estimates of the effects of the subsequent legislation could be biased, especially in small samples. We therefore examine the behavior of traffic fatalities around the times that the laws were passed using event studies.

Event studies are often used to measure the effect of an economic or legal "event" on the value of a firm. A firm's "normal" return is assumed to be a linear function of the return to the market portfolio:

$$R_{i,t} = a_i + b_i R_{m,t} + \nu_{i,t} \tag{2}$$

where $R_{i,t}$ and $R_{m,t}$ are returns to the firm and to the market, respectively, in period *t*. The parameters a_i, b_i are estimated via OLS during an estimation period prior to the *event window*, the latter specified as the period of interest before and after the actual event. The effect of the event is estimated by the abnormal return during the event window, defined to be the actual return less the return predicted by estimates from (2), or $\hat{\nu}_{i,\tau}$, where τ designates the event window.

For the purposes of examining the effects of alcohol control laws on traffic fatalities, the returns to the firm and to the market portfolio in (2) are replaced by state and national traffic fatalities, respectively, and augmented by the vectors of indicator and control vectors as in (1). The abnormal returns are the prediction errors for a window of two years preceding and two years following passage of the law in the particular state. The prediction errors for all the "treated" states are then averaged to yield the mean prediction error:

$$\bar{\nu}_\tau = \frac{1}{N}\sum_{i=1}^{N} \hat{\nu}_{i,\tau} \tag{3}$$

which has variance:

$$\sigma^2_{\bar{\nu}_\tau} = \frac{1}{N^2}\sum_{i=1}^{N} \{\sigma^2_{\nu_i} + \tfrac{1}{L_i}[1 + \frac{(R_{m,\tau}-\hat{\mu}_m)^2}{\sigma^2_m}]\} \tag{4}$$

where $\sigma^2_{\nu_i}$ is the estimated variance of the residuals from individual state regressions (2), L_i is the length of the estimation window for state *i*, and $\hat{\mu}_m$, σ^2_m are the estimated mean and variance of national traffic fatalities; see MacKinlay [1997].

The mean prediction errors are then accumulated over the event window $\left[\tau_1, \tau_2\right]$ to yield the cumulative prediction error $c_{\tau_1,\tau_2} = \sum_{\tau_1}^{\tau_2} \bar{\nu}_\tau$, which under assumptions of normally distributed $\nu_{i,t}$, has mean zero and variance equal to the simple sum of the variances (4) over the event window.[11]

By examining the <u>pattern</u> of the cumulative prediction error, we can, for example, infer whether traffic fatalities were unusually high prior to the passage of a particular law, suggesting that the law was to some degree a response to existing conditions. On the other hand, if there is no apparent pattern prior to passage, but large negative prediction errors after passage, we may

[11]The variance formula in (4) makes strong assumptions about the independence of the abnormal returns both intertemporally and across states, assumptions that are likely to be violated in practice [Salinger, 1992]. As will be shown below, however, this bias is not likely to affect the interpretation of the results

infer that the law was successful in reducing traffic fatalities. By using the estimated variance of the mean prediction error, we can infer whether any patterns detected in the errors are statistically significant.

In addition to the issues noted above, a complicating factor in isolating the effects of any particular piece of legislation on traffic fatalities is the rapid passage of many laws in succession in many of the states. One report, for example, noted that California amended its alcohol-related traffic laws 55 times between the years 1980 and 1986 (Drivers Research Institute, 2005). The failure to produce a significant estimate for a particular law may not mean the law was ineffective, only that its effects could not be separated from the effects of other legislation.

In following section, we apply alternative specifications of pooled cross-section time series estimates with corrected standard errors and event studies of different alcohol control laws to identify those laws, if any, that have had a significant effect on traffic fatalities.

IV. Empirical Results

Two-way fixed effects regressions

Table 2 presents the results of alternative specifications of equation (1). Columns headed "Dee" replicate approximately the results of Dee (2001) and Eisenberg (2003) and serve as benchmarks for the extensions and modifications in this paper. The left side of Table 2 presents the static model, the standard in the literature; the right side presents results with the lagged dependent variable added. Two sets of *p*-values are reported for the coefficients of BAC 08 and ALR, one in normal face calculated via robust standard errors generated from the usual White-type correction of the variance-covariance matrix; and one in **bold** face calculated via empirical distributions generated using random inference.

[Table 2 about here]

Model (A) approximates the Dee (2001) analysis (Table 3, column 3 from that paper), and restricts the sample to the Dee time period.[12] As in that paper, BAC 08, BAC 10, and ALR laws are all significant at the usual levels using *p*-values derived from robust standard errors and the usual *t* distribution. The construction of the indicator variable in the present paper is different from Dee's, however; on a comparable basis, the coefficient of BAC 08 in this paper would be -0.085, similar to the -0.072 estimated in Dee's model.[13] Seat belt laws are found to reduce fatalities; maximum speed limits 70 mph or higher are found to increase fatalities, and traffic fatalities are found to be procyclical. Again, these are all consistent with previous literature.

In model (B), the sample is extended back to 1980 and forward to 2003. Extending back to 1980 (possible for total fatalities, but not for alcohol-related fatalities) captures the years immediately prior to the first "big push" in alcohol control legislation: in 1980, only 15 of 48 states had BAC 10 laws, and none had ALR laws; by 1983, 34 of 48 states had BAC 10 laws and 14 had ALR laws. Extending the sample forward to 2003 has the obvious advantage of more observations, especially important in capturing the results of a another flurry of legislation in the

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[12]Variables included in Dee (2001) but not included here are dram shop statutes, mandatory jail time for first offense, Zero Tolerance for minors, and real personal income. None of the coefficients for these variables were significant, either in Dee (2001) or here (and their inclusion had no material effect on the coefficients of the variables in Table 2), so they were excluded in the interest of parsimony.

[13]In Dee (2001), indicator variables for the BAC variables are mutually exclusive. In this paper, the BAC 10 indicator remains coded "1" when BAC 08 is enacted, so that the coefficient for BAC 08 can be interpreted as the marginal effect of BAC 08 over BAC 10. In this way, the significance of the marginal effect can be tested directly. This interpretation seems more natural, as BAC 10 was in effect prior to BAC 08 in all states but one (Oregon).

early to mid-1990s. Separately, a new control is added: the percentage of the population ages 14-24. Drivers in this age group have accident involvement rates twice as high as the remainder of the population; in 2003 they account for 24 percent of the deaths and 14 percent of the population.

The extended sample and the inclusion of the age variable causes the BAC 10 coefficient to disappear in model (B) (excluding the population 14-24 variable results in a BAC 10 coefficient of -0.012, standard *p*-value of 0.351, so the extended sample alone is sufficient). In addition, secondary seat belt laws are now positively related to traffic deaths. BAC 08 and ALR laws are still found to reduce traffic deaths using standard tests of significance, and as expected the population variable is associated with increases in fatalities.

Not reported in previous literature is a test for serial correlation in the residuals. The test used here is the pooled Durbin-Watson, following Bhargava, et al. (1982).[14] As shown in the table, the null of no serial correlation is soundly rejected for all the static models. The average and standard deviation of first-order autoregression coefficients for the state-level residuals is given in the bottom row of the table, and as shown below, is very close to the coefficient of the lagged dependent variable in the dynamic models presented in the right-hand side of the table.[15]

Bertrand, et al. (2003) demonstrate that serial correlation in differences-in-differences estimates has potentially serious consequences for the standard errors of the coefficients, and the

[14]Similar results were found using an *LM*-type test with the lagged residuals.

[15]The mean autocorrelation coefficient is estimated as $\hat{\rho} = \frac{1}{N}\sum_{i=1}^{N}\sum_{t=2}^{T}\frac{\varepsilon_{it}\,\varepsilon_{it-1}}{\varepsilon_{it}^{2}}$, and the standard deviation estimated in the usual way, as the mean squared difference between the coefficient for the individual states and the mean coefficient.

evidence here confirms their case. The *p*-value of the BAC 08 coefficient jumps from 0.062 in Model (A) using robust standard errors to 0.336 using the empirical distribution generated by random inference. The *p*-value for the BAC 08 coefficient in Model (B) is also underestimated using standard methods, this time by a factor of almost nine.[16] The coefficient for ALR, on the other hand, retains a small and statistically significant *p*-value under either method and for all models.[17]

The underlying data is such that the BAC 08 coefficient is likely biased toward a negative estimate. Figure 3 illustrates the empirical distribution generated via random inference for the BAC 08 coefficient in Model (B) (dashed line), and the usual student *t*. The empirical distribution has much greater dispersion than the *t* distribution, and is centered at -0.0113 instead of zero. Because almost all of the BAC 08 laws were passed in the latter half of the sample, the negative bias may reflect general declines in fatalities not fully captured by the time effects.

[Figure 3 about here]

Model (C) presents results for equation (1) with alcohol-related fatalities as the dependent variable. The results are qualitatively quite similar to those of Model (B), including the bias in the *p*-values. The coefficient for BAC 08 is not significant even under standard tests, and its *p*-values under the empirical distribution are again much larger than standard. ALR laws, however, stand up under either test. The imprecision of measuring alcohol-related fatalities, especially in the earlier years of the sample when much of the initial legislation was passed, probably

[16]While large, a factor of nine is not unprecedented; Bertrand, et al. (2003) found the likelihood of false rejection of the null to be overestimated by a factor of 13 in one case.

[17]A separate random inference procedure is required for each coefficient, one reason why only the values for the principal regressors of interest are shown.

contributes a great deal of measurement error in Model (C). Still, the consistency of the results helps to confirm our initial findings for total fatalities.

The right hand side of Table 2 reproduces the three models with the simple addition of a lagged dependent variable (LDV). It is well-known that coefficients of LDV models with fixed effects are biased (Hsiao, 2003, among many others), with the bias especially affecting the coefficient of the LDV, but the bias would be unlikely to alter the main results in the present case. Models (D)-(F) produce roughly similar results, and will be discussed as a group, with particular focus on Model (E).

The coefficient of the LDV is about 0.50 in all cases, quite similar to the autocorrelation coefficient in the non-LDV case. The pooled Durbin-Watson and the estimate of $\hat{\rho}$ indicate that the LDV mitigates the issue of autocorrelation in the model. The BAC coefficients are uniformly negative but insignificant at standard levels, further confirming the effect of autocorrelation on these estimates.[18] ALR laws, however, remain robust to the inclusion of the LDV, and though *p*-values are larger using random inference, they remain small enough to reject the null of no association at the usual levels of significance.

The impact of ALR laws is estimated in Model E to be a 2.6 percent reduction in fatalities in the initial year, and a long-term reduction of 5.8 percent, with a half-time to equilibrium of only about 1.2 years. It seems quite plausible that the effect of new legislation should be realized over a period of years, for at least two reasons. One, information about the new penalty must be disseminated to the public, and depending on a variety of factors, including levels of income,

[18]If the indicator variable is changed to "1" for any BAC law, it is significant only in the shorter "Dee" sample.

education, and awareness of public events, this may take some time. Second, if there is a stock of chronic offenders who are relatively resistant to all but the most punitive measures, it will take some time for the police to apprehend them. Only after the "word gets out" and/or a significant number of chronic offenders have been removed from the road will the laws intended effects take hold.

In summary, the results of the pooled fixed effects models as corrected for serial correlation indicate that ALR laws are effective in reducing traffic fatalities, and that BAC 08 laws are not, when each is controlled for the other. In the next sub-section, an alternative technique, event analysis, is used to determine whether this result is subject to endogeneity bias.

Event studies of alcohol-control laws

An event study allows us to analyze the timing and the magnitude of the effect of the "event" of passing a new alcohol-control law. We visually examine the prediction errors from a variation of equation (1) to determine if there are any abnormal movements prior to or after passage of the law, and whether these movements exceed error bounds (by convention, two standard deviations).[19]

The equation used to estimate the model is:

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[19]Eisenberg (2003) examines the timing of BAC 08 laws using dummy variables for two year periods before and after the effective dates of the laws. It is easily shown (see Greene [2000, pp. 308-310]) that the estimated coefficients of dummy variables for <u>individual</u> observations are the same as prediction errors for those observations (and in fact, the prediction errors in the present analysis are estimated this way), so Eisenberg's method has something in common with event studies. With the "dummying out" of so many observations in Eisenberg's regressions, however, (all but 2 years of data in the case of California), it is not clear what is actually being measured.

$$y_{it} = \mu_i + \alpha_i y_t + \beta_i' X_{it} + \gamma_i' L_{it} + \nu_{it}, \quad t = 1,2,\ldots \tau - 3 \tag{3}$$

where, as before, y_{it} is a measure of traffic fatalities at the state level, y_t is the national fatality rate, X_{it} is a vector of continuous control variables, L_{it} are indicator variables for alcohol-control legislation, and τ is the year of the passage of new legislation; the event window is thus $\tau - 2$ to $\tau + 2$. To allow sufficient degrees of freedom for estimating (3), only states enacting laws by 1990 or later were used, and the control variables were limited to BAC laws, ALR laws, state unemployment and population ages 14-24.[20]

There are 14 states with BAC 08 laws passed in 1990 or later, and 16 states passing ALR laws in 1990 or later.[21] Seven states appear on both lists, but only California has both a BAC 08 and an ALR law effective in the same year. The results of the event studies for BAC 08 and ALR laws are illustrated in Figure 4. The solid lines in the graphs are the cumulative mean prediction errors, and the dashed lines are two standard deviation error bounds. Because the prediction errors were often quite large relative to the magnitude of the dependent variable, we

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[20]Because of the shorter time series, and because of likely measurement error, especially in the earlier years of the sample (error bands exceeding 40 percent, for example), results of alcohol-related fatalities event studies were not considered reliable.

[21]For BAC 08 laws, the states and years of effect are: Alabama, 1995; California, 1990; Florida, 1994; Idaho, 1997; Illinois, 1997; Kansas, 1993; Kentucky, 2000; New Hampshire, 1994; New Mexico, 1994; North Carolina, 1993; Texas, 1999; Vermont, 1991; Virginia, 1994; Washington, 1999. For ALR laws, the states and years of effect are: Alabama, 1996, Arkansas, 1996; California, 1990; Connecticut, 1990; Florida, 1990; Georgia, 1993; Maryland, 1990; Massachusetts, 1994; Nebraska, 1993; New Hampshire, 1992; Ohio, 1993; South Carolina, 1998; Texas, 1995; Virginia, 1995; Washington, 1994.

also include on the chart the net +/- count of the prediction errors for each time period along the zero axis as a robustness check against influential outliers.

[Figure 4 about here]

The cumulative average prediction errors in the BAC 08 analysis are slightly negative prior to the effective date, and slightly positive in the years after, but well within the error bounds throughout. The net +/- errors confirm this pattern, with net negative prediction errors prior to the effective year, and no effect thereafter. For the 14 states in the sample, the BAC 08 laws had no systematic effect on the prediction errors of a model based on information prior to the effective date of the law. In fact, the prediction errors move in the direction opposite what we would expect to see if the law worked as hoped, but the size of the move could easily be explained by chance.

By contrast, the pattern of the ALR prediction errors is consistent with what one would expect to see if the ALR laws were effective in causing fewer traffic deaths. Cumulative average prediction errors are slightly negative prior to the laws' effective dates, then turn more sharply negative in the effective date and thereafter, and come quite close to the two standard deviation band in two years. The net +/- count is consistent with the cumulative errors, indicating that prediction errors for most states were negative immediately prior to and for two years after the effective date.[22] Assuming normality, the *p*-value of a *z*-test that the cumulative ALR prediction error is zero in the second year after effect is 0.074.[23]

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[22]The "-8" in the second year after the effective date means that 4 states had positive prediction errors and 12 states had negative errors.

[23]No claim is made here for the accuracy of the standard deviation of the cumulative prediction errors. With such a small estimation window, the variance of the prediction error is

The event studies both confirm the findings of the modified fixed effect regressions, that ALR laws have had a significant effect of traffic deaths, and also provide evidence that reduced traffic deaths were a result of these laws and not a cause of them.

V. Conclusions

The foregoing analyses measures the outcomes of different forms of alcohol-control legislation on traffic fatalities. The results of the present study, arguing that ALR laws are effective in reducing traffic fatalities, but BAC 08 laws are not, are consistent with the earlier work of Zador, et al. (1989) and the GAO (1999) review, but differ from the more recent results of Dee (2000) and Eisenberg (2003), even though the methodology here is closer to the later work.

Because it is so rare, however, to have ALR as a penalty without some BAC standard (only 28 state-years out of 1152 in the sample), the analysis cannot realistically predict the efficacy of ALR laws apart from BAC limits.[24] Similarly, because only in one state was it the case that a BAC 08 law was not preceded by a BAC 10 law, BAC 08 represents only the tightening of an existing standard and thus represents a difference in degree and not in kind.

It should not be overlooked, moreover, that these laws are complements, not substitutes; BAC laws set a standard for impaired driving, and ALR laws establish a punishment for violating

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dominated by the estimation error of the coefficients, which also leads to serial correlation in the abnormal returns (MacKinlay, 1997). Added to the size problems are issues of cross-sectional dependence, which is very likely given the tendency for alcohol-control legislation to be passed in waves. Thus the error bands are intended to be merely suggestive.

[24]The converse is more substantial, however; there were BAC laws without ALR laws in 381 state years, almost one-third of the sample.

the standard. There are punishments besides ALR for impaired driving, of course, including fines and mandatory jail terms, with stiffer terms for repeat offenders; and there are other ways besides BAC laws to establish impairment, including the judgement of the arresting officer or forms of agility tests, but there must always be a standard and a punishment.

We can say is that there is compelling evidence that ALR laws are shown to be effective in reducing traffic deaths when used in conjunction with BAC standards, but we cannot know how effective the ALR laws would be if there were no BAC standards. But we can also say that results of this study indicate that efforts spent to reduce allowable BACs from 0.10 to 0.08, including the federal mandate by threatening to withhold highway funding, may have been better spent on the states that still do not impose ALR sanctions on DUI offenders.

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Table 1: Descriptive Statistics

Continuous Variable	Year	Mean	Standard Deviation	Maximum	Minimum
Total Fatalities per 100,000 population	1982:	2.09	0.67	4.23 (N. Mexico)	1.10 (Rhode Il)
	2003:	1.68	0.60	3.29 (Wyoming)	0.72 (Mass)
Alcohol-Related per 100,000 population	1982:	1.25	0.45	2.75 (N. Mexico)	0.64 (New York)
	2003:	0.68	0.27	1.39 (Montana)	0.20 (Utah)
Unemployment rate	1982:	9.3	2.3	15.5 (Michigan)	5.5 (S. Dakota)
	2003:	5.6	1.0	8.2 (N. Dakota)	3.6 (S. Dakota)
Vehicle Miles Traveled (billions)	1982:	33.0	32.3	170.3 (Calif.)	4.0 (Vermont)
	2003:	60.0	60.0	324.2 (Calif.)	7.4 (N. Dakota)
Population ages 14-24 (%)	1982:	18.1	0.8	19.5 (S. Carolina)	16.0 (Florida)
	2003:	14.2	1.0	19.0 (Utah)	12.5 (Florida)

Indicator Variable	Year	Number of States Adopting
BAC 08	1982:	0
	2003:	30
BAC 10	1982:	18
	2003:	18
Administrative License Revocation	1982:	0
	2003:	39
Seat Belt (Primary Enforcement)	1982:	0
	2003:	13
Seat Belt (Secondary Enforcement)	1982:	0
	2003:	34
Maximum Speed Limit 70 +	1982:	0
	2003:	18

Table 2: Pooled Time-Series Cross-Section Regressions of Traffic Fatalities per 10,000 Population on Alcohol Control Laws

	No Lagged Dependent Variable			With Lagged Dependent Variable		
Model Variable	(A) Dee (1982-98)	(B) Full Sample (1980-2003)	(C) Alcohol Only (1982-2003)	(D) Dee (1982-98)	(E) Full Sample (1980-2003)	(F) Alcohol Only (1982-2003)
Lagged Fatalities	–	–	--	0.456 ** (0.000)	0.553 ** (0.000)	0.524 ** (0.000)
BAC .08	-0.031 * (0.062) **(0.336)**	-0.022 * (0.079) **(0.701)**	-0.025 (0.160) **(0.562)**	-0.022 (0.123)	-0.007 (0.482)	-0.011 (0.439)
BAC .10	-0.054 ** (-0.002)	0.008 (0.952)	0.008 (0.683)	-0.008 (0.634)	-0.010 (0.374)	0.027 (0.120)
Adm. License Revocation	-0.074 ** (0.000) **(0.000)**	-0.061 ** (0.000) **(0.004)**	-0.071** (0.000) **(0.024)**	-0.034 ** (0.001) **(0.014)**	-0.026 ** (0.005) **(0.008)**	-0.028 ** (0.030) **(0.085)**
Seat Belt (Pr)	-0.053 ** (0.000)	-0.028 * (0.073)	-0.020 (0.297)	-0.022 * (0.092)	-0.016 (0.225)	-0.011 (0.517)
Seat Belt (Sc)	-0.021* (0.069)	0.282 ** (0.014)	0.066 ** (0.000)	-0.007 (0.511)	0.019 * (0.070)	0.036 ** (0.015)
70+ MPH	0.037 ** (0.018)	0.029 ** (0.014)	0.054 ** (0.003)	0.019 (0.333)	0.016 (0.113)	0.034 ** (0.028)
Vehicle Miles	0.013 (0.736)	0.058 (0.182)	-0.088 (0.158)	-0.023 (0.525)	-0.007 (0.817)	-0.109 ** (0.035)
Unemployed	-0.026 ** (0.000)	-0.022 ** (0.00)	-0.026 ** (0.000)	-0.015 ** (0.000)	-0.012 ** (0.000)	-0.014 ** (0.000)
Pop 14-24		0.019 ** (0.00)	0.021** (0.000)		0.009 ** (0.006)	0.007 (0.211)
Adjusted R^2	0.92	0.90	0.86	0.95	0.94	0.90
Pooled D.W.	1.00 **	0.83 **	0.92 **	2.07	2.17	2.07
$\hat{\rho}, (S_{\hat{\rho}})$		0.52 (0.23)			-0.04 (0.28)	

Notes to Table 2: p-values from robust standard errors in parentheses. p-values from empirical distributions generated via random inference in **bold**. **, * : significant at 0.05, 0.10 level, respectively.

Figure 1:

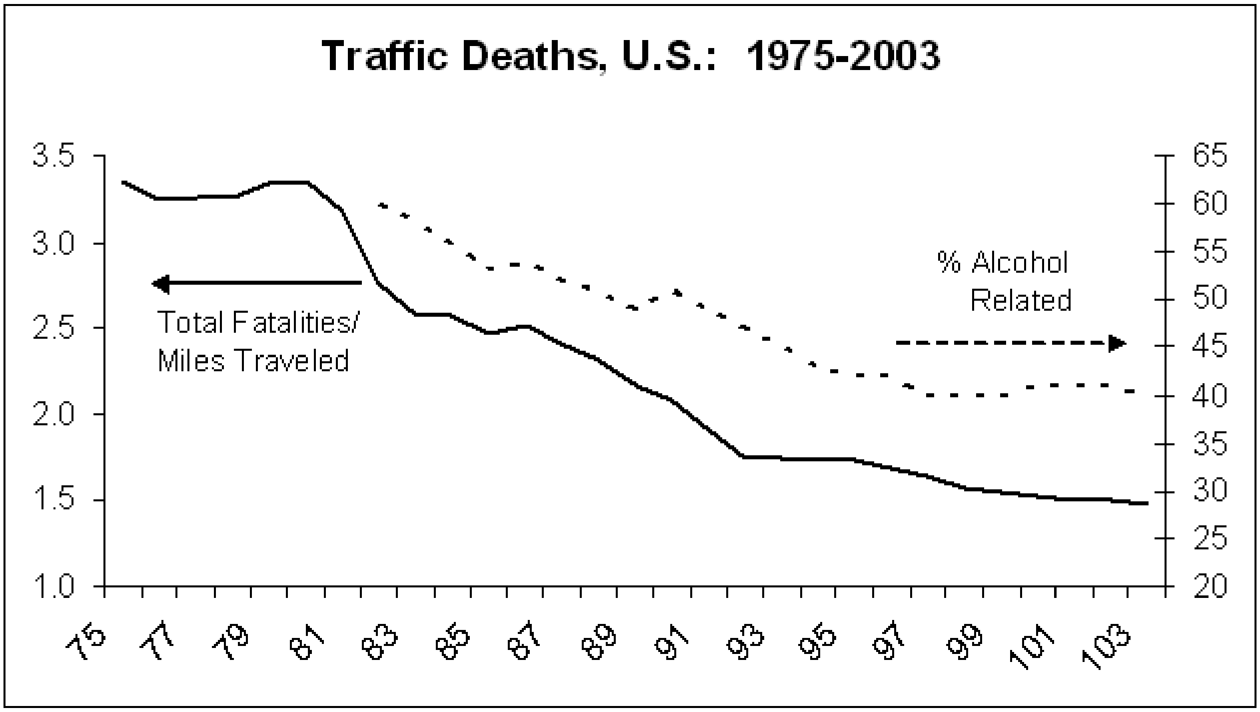


Figure 2:

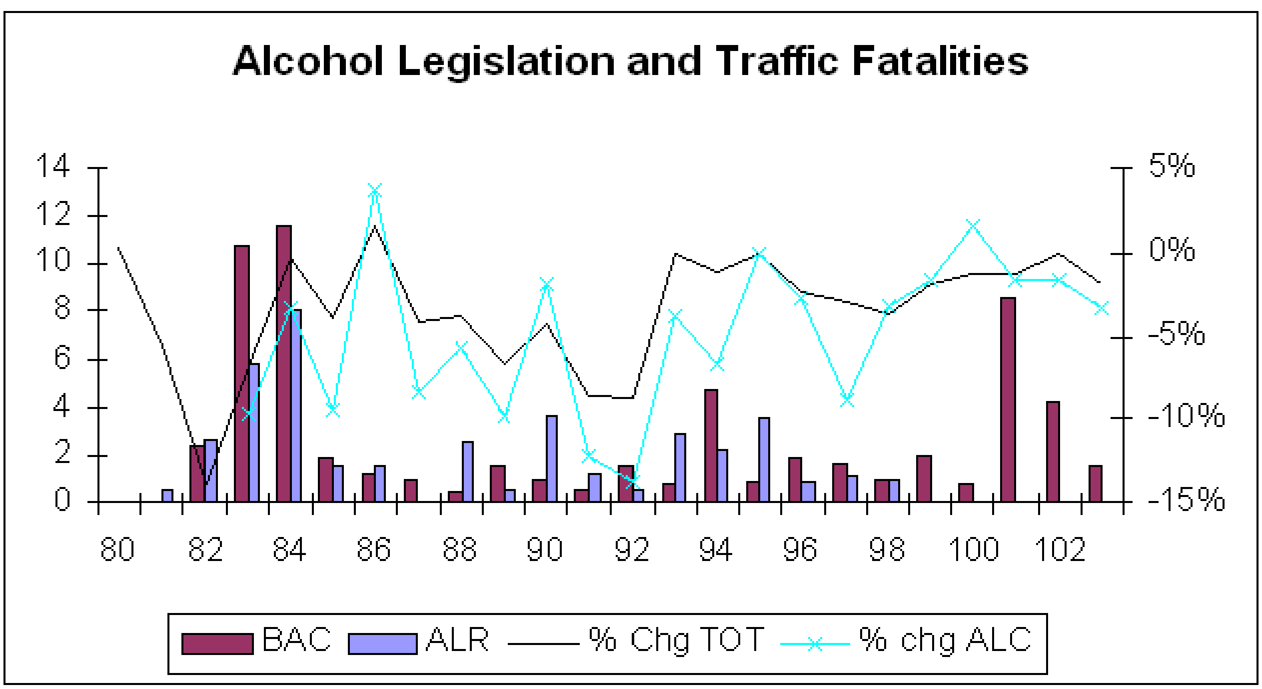


Figure 3:

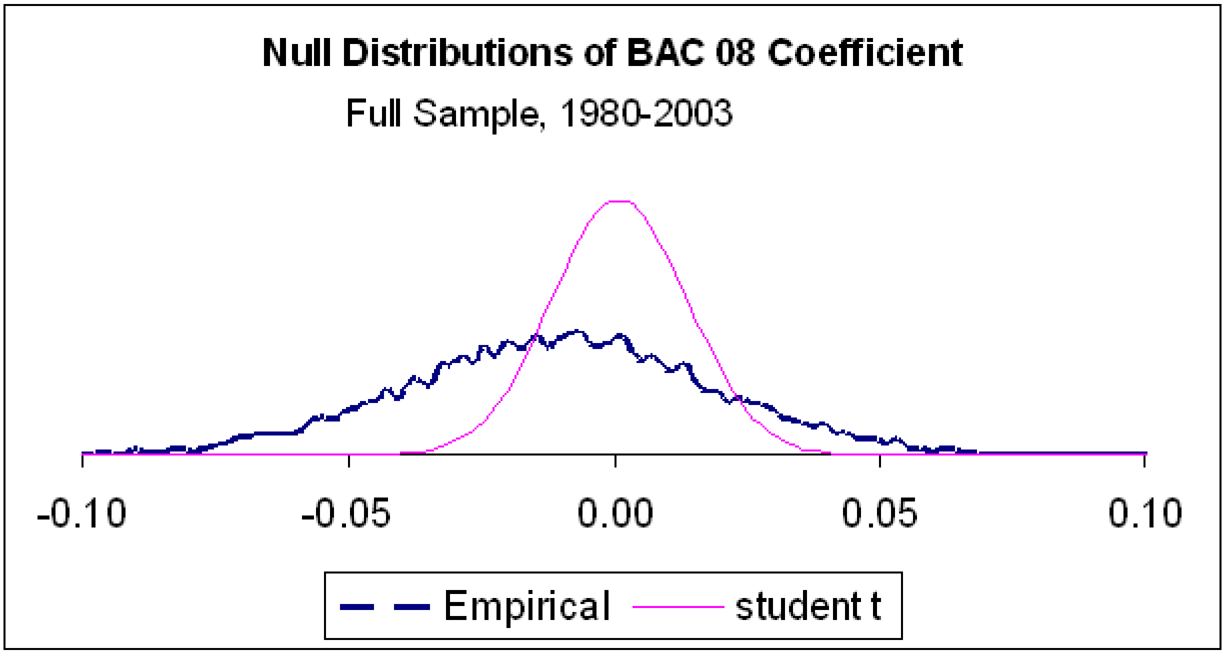
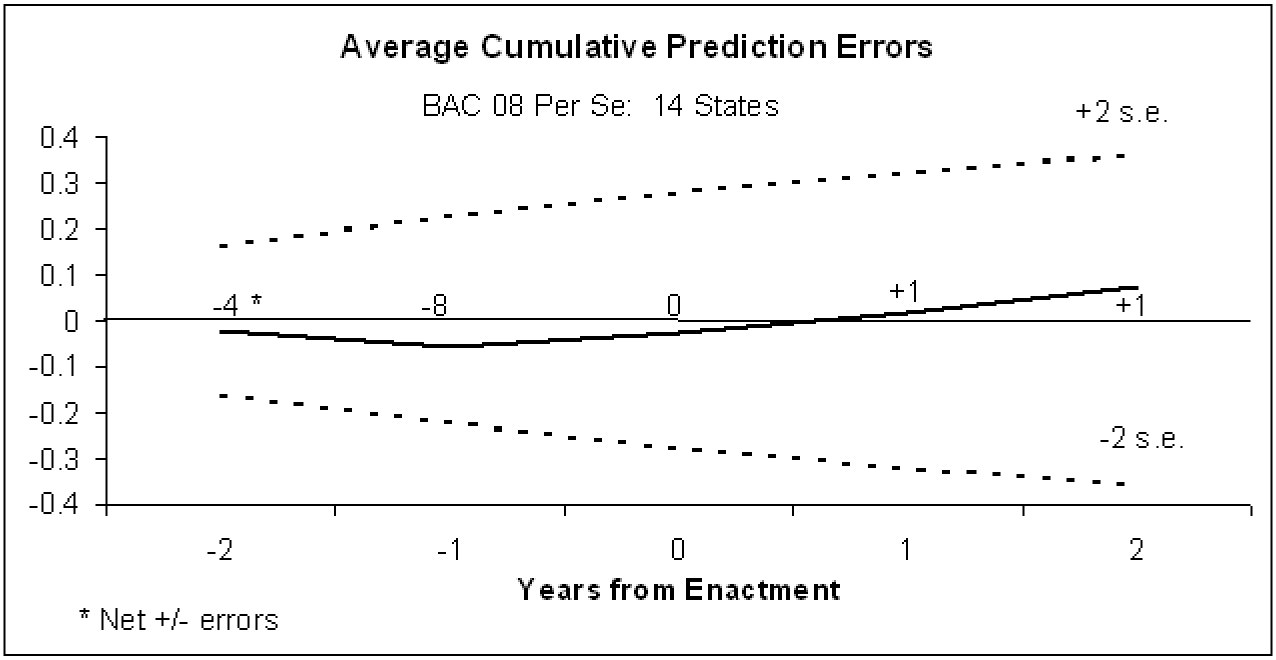
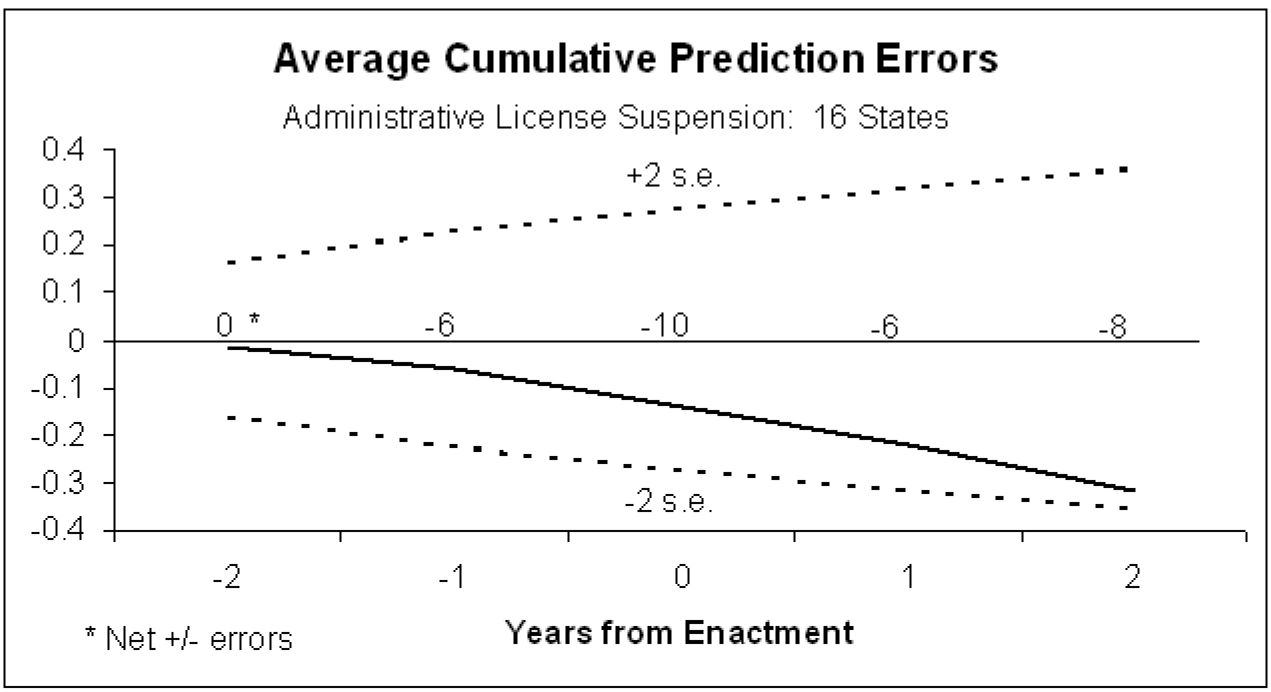

Null Distributions of BAC 08 Coefficient
Full Sample, 1980-2003
-0.10
-0.05
0.00
0.05
0.10
Empirical
student t

Figure 4:


Average Cumulative Prediction Errors
BAC 08 Per Se: 14 States
+2 s.e.
-2 s.e.
0.4
0.3
0.2
0.1
0
-0.1
-0.2
-0.3
-0.4
-4 *
-8
0
+1
+1
-2
-1
0
1
2
Years from Enactment
* Net +/- errors


Average Cumulative Prediction Errors
Administrative License Suspension: 16 States
+2 s.e.
-2 s.e.
0.4
0.3
0.2
0.1
0
-0.1
-0.2
-0.3
-0.4
0 *
-6
-10
-6
-8
-2
-1
0
1
2
* Net +/- errors
Years from Enactment