Sam Houston State University
Department of Economics and International Business
Working Paper Series

# Dead on Arrival: Zero Tolerance Laws Don't Work

Darren Grant

SHSU Economics & Intl. Business Working Paper No. SHSU_ECO_WP07-08
October 2007

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Why do college students drink so stupidly? Because drinking intelligently is against the law.

–Jack Hitt, New York Times Magazine, October 1999

## I. INTRODUCTION

For two decades U.S. drunk driving legislation has become increasingly strict as the states, responding to financial incentives provided by Congress, have reduced allowable blood alcohol content (BAC) levels. In 2005 the last state lowered its illegal per se BAC limit for adults to 0.08 percent, down from 0.10. During the 1990s even lower limits of 0.01 or 0.02 were instituted for drivers under 21 (here, termed "youth"), as shown in Figure 1. This legislation, which adopts a "zero tolerance" policy toward underage drinking and driving, is the subject of this paper. Previous work on zero tolerance laws has thoroughly examined their effects on a variety of drinking-related behaviors, but as yet their effects on traffic fatalities–the most fundamental question of all–have not received a full treatment. Existing U.S. fatality analyses have been limited either in scope, comprising a small part of a larger empirical study, or sophistication, utilizing relatively crude statistical methods.

A full treatment is needed for two reasons. The first is to thoroughly ground the analysis in economic theory, which has not been done before. On the surface, a zero tolerance law would seem to be a simple, powerful, sure way to reduce drunk driving fatalities. But economists, used to thinking on the margin, should suspect otherwise. This legislation generates strong marginal disincentives against taking the first drink, but much smaller marginal disincentives thereafter (depending on the effect of BAC on the probability of arrest, etc.). These marginal incentives are not uniformly stronger than those imposed by higher BAC limits. As a result, zero tolerance laws can have unintended, or perverse, effects of uncertain magnitude that counteract the purpose of the law. Ultimately, zero tolerance legislation has ambiguous effects on fatalities and economic efficiency. We demonstrate this in a simple model and derive new predictions that are testable and unambiguous.

Our second objective is to provide comprehensive evidence of zero tolerance laws' effects on traffic fatalities, applying a variety of panel techniques to data from the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration (NHTSA). The limited scope of earlier studies leaves their results, generally supportive of these laws' efficacy, potentially sensitive to specification or subject to omitted variables bias, problems that have occurred elsewhere in the traffic safety literature. In our analysis we pay particular attention to these issues, which turn out to be fundamental, using a variety of direct and indirect methods to control for unobservable factors and ascertain the robustness of our results. This is complemented by a distributional analysis, new to this literature, that quantifies the intended and unintended consequences of zero tolerance legislation and tests the only robust hypotheses generated by theory.

These analyses indicate that the intended and unintended consequences of zero tolerance legislation are minimal. Zero tolerance laws have no material effect on fatalities, because they have no material effect on driver behavior.

## II. THE THEORY OF SINGLE BAC THRESHOLDS

### Setup

A simple model can illustrate the individual and population effects of a reduction in permissible BAC levels, of which zero tolerance laws are the most extreme example. Let C denote the amount of alcohol consumed and $V_n(C)$ denote the "value" of that consumption to the consumer:

the amount individual *n* is willing to pay for permission to purchase C units of alcohol at the market price and consume them. Any harmful internal effects of alcohol consumption are captured in V; the expected harm to others resulting from drunk driving is denoted by D. We assume V increases at a decreasing rate up to its maximum, as is standard, while studies by Zador, Krawchuk, and Voas (2000) and Blomberg et al. (2005) indicate $dD/dC > 0$, $d^2D/dC^2 > 0$. We also assume the function V (but not C, of course) is invariant to changes in policy, in effect assuming that individual drinking preferences are not sensitive to the general state of inebriation on the roads. (The FARS data, described below, indicate collisions between two drinking drivers are quite rare, so this assumption is justified.) Absent legal penalties, consumer *n* purchases $S_n = \text{argmax} V_n$ units of alcohol.

The government sets penalties for drunk driving such that the monetary value of the expected legal penalty for driving after consuming C units of alcohol is H(C). While the penalty structure can be arbitrary, we first consider a single drunk driving threshold $T_1$: $H(C) = 0, C \leq T_1$; $H(C) = L, C > T_1$. This defines the legal, rather than the illegal, threshold, for ease of exposition. Figure 2 illustrates all penalty structures considered in this paper. This structure does not take into account the effects of BAC on the chances of arrest or conviction, violations occurring below the per se limit, or errors in assessing BAC by consumers or law enforcement, greatly simplifying the presentation without altering our central conclusions. We ignore driver switching for the same reasons.

## Effects of a Threshold Reduction

When there is a single BAC threshold, the penalty structure is a step function. The consumer will then choose one of two critical points: the point of maximum satisfaction $S_n$ or the legal threshold $T_1$. The latter is chosen if $V_n(T_1) > V_n(S_n) - L$, or, equivalently, if $V_n(S_n) - V_n(T_1) < L$. This second expression takes a cost-benefit form: the cost of satisfying the law, on the left, is the monetized utility loss incurred by limiting alcohol consumption, while the benefit of doing so is avoiding the expected penalty L, on the right. If the threshold falls from $T_1$ to $T_2$, the benefit of adhering to the law is unchanged, but the cost of doing so, in reduced consumption, has increased. Some consumers will incur this additional cost and further reduce their drinking, but others will not, and will revert to $S_n$. Reductions in BAC thresholds cause some drivers to drink less and other drivers to drink more.

The effect of BAC limits on a given driver's behavior depends, then, on his tastes for alcohol, specifically 1) his desired level of consumption and 2) his willingness to reduce that consumption to satisfy the law. For any given threshold $T_1$, some drivers, for whom $S_n < T_1$, will trivially satisfy the law; others, for whom $S_n > T_1$ but $V_n(S_n) - V_n(T_1) < L$, will limit consumption to adhere to the law; while still others, for whom $V_n(S_n) - V_n(T_1) > L$, will ignore the law. The effect of BAC limits in a population of drivers depends on the distribution of these tastes for alcohol. To illustrate we quantify these across this population using S and a second parameter, P, such that $V_n(C) = \min(P_n*C, P_n*S_n)$.[1] Then S represents the first "taste term" listed above and P, the monetized utility loss from sacrificing one unit of alcohol consumption, the second. Each driver occupies a point in (S,P) space, and his response to a single BAC threshold is characterized as shown in Figure 3. Region X contains drivers who trivially satisfy the threshold (without changing their alcohol consumption), region Y those who reduce their drinking to satisfy the threshold, and region Z who ignore the threshold.

When the BAC threshold is reduced from $T_1$ to $T_2$, the behavioral responses again depend on these two parameters, and can be classified by the sets A1-A3, B1-B2, and D in Figure 4. First consider drivers who trivially satisfied the original BAC threshold. Some of these also trivially satisfy the new, lower one (A1), while others, for whom $V_n(S_n) - V_n(T_2) < L$, reduce their consumption to $T_2$ (A2), and the remainder leave it unchanged (B1). As a result, there will be more drivers with $C \leq T_2$ and fewer with $T_2 < C \leq T_1$. Next, consider drivers who had lowered consumption in order to satisfy the original threshold. Some of these will further reduce their consumption to meet the new threshold (A3), while others, for whom $V_n(T_1) > V_n(S_n) - L > V_n(T_2)$, will cease to accommodate the law and will consume $S_n > T_1$ (B2). There will again be more drivers with $C \leq T_2$ and fewer with $T_2 < C \leq T_1$; there will also be more with $C \geq T_1$. Finally, consumers who violated the original threshold will continue to violate the new one (D).

The aggregate effect of a threshold reduction on fatalities, the sum of these intended and perverse effects, is theoretically ambiguous.[2] But theory does make three clear predictions about the change in the distribution of drivers' BAC: the fraction of drivers with $C \leq T_2$ should increase, that with $T_2 < C \leq T_1$ should decrease, and that with $C > T_1$ should increase.

In fact, were the data available, the fraction of drivers in each of the six sets listed above could be discerned from the CDFs of driver BAC before and after the threshold reduction, as illustrated in the solid and dashed lines at the bottom of Figure 4. Drivers who reduce their drinking to satisfy the original threshold generate a kink in the original CDF at $T_1$. The kink moves to $T_2$ after the threshold reduction; the two CDFs intersect at $T_1$. The fraction of drivers in each set could be calculated from this point of intersection and the values of the CDFs at $T_1$ and $T_2$, and the intended and perverse effects of the law quantified.

We do not observe the BAC of all drivers, however, just those involved in fatal accidents, and the chances of being in an accident rise rapidly with BAC. As long as total fatalities fall or do not rise much after a threshold reduction, however, the two CDFs will cross at a single point and will exhibit kinks near the appropriate thresholds. The intended and perverse effects of the threshold reduction are still discernable, and the three predictions listed above still hold. If, instead, a large increase in total fatalities swamps the growth in fatalities involving drivers with $C \leq T_2$, then the fraction of fatalities attributable to those drivers could fall. But the other two predictions remain valid.

The assumed penalty structure is overly simplistic in that the probability of being arrested for drunk driving, and hence the expected penalty H(C), is increasing in C. The qualitative predictions of the model, however, are unchanged. If the probability of arrest for drunk driving was proportional to one's BAC level ($H(C) = kC$, $C > T$, with $kT = L$), for example, the graphs in Figures 3 and 4 retain their shape, but are shifted vertically by the constant of proportionality: the analysis of population effects of a threshold reduction would be identical. In fact, ambiguous effects on fatalities are predicted for *any* change to *any* penalty structure that increases penalties below some BAC level but not does increase them above that level [$H_2(C) > H_1(C)$, $K_1 \leq C \leq K_2 < T$; $H_2(C) = H_1(C)$, $C \geq T$], because the marginal penalty for consuming an additional unit of alcohol is increased at some BAC levels and decreased at others. Some drivers respond to the former, and drink less, but others respond to the latter, and drink more. One cannot eliminate perverse incentives simply by finessing the penalty structure.

Policy Optimality

A reduction in BAC thresholds, with L held constant, cannot be justified on the simple basis that it is "tougher," and would unambiguously lead to a reduction in drunk driving. Can such a change be theoretically justified on the basis of economic efficiency instead?

Efficiency would be achieved by a penalty structure resembling a Pigovian tax, forcing drivers to internalize the external costs of drunk driving. Each driver would then choose the consumption level for which marginal private benefits equaled marginal social costs. Line G1 in Figure 3 illustrates the marginal penalty, dH/dC, for the efficient penalty structure, H(C) = D(C). This penalty structure encourages all consumers to the right of G1 to reduce their drinking to satisfy the law, but leaves consumers to the left of G1 unaffected. All drinking is eliminated for which dD/dC > dV/dC = P. The single threshold, in contrast, reduces drinking among a vastly different, "almost orthogonal" set of consumers–those in region Y. (This near-orthogonality obtains because this penalty structure is not convex, as D is.) This cannot be efficient: consumption levels are too low for those consumers in region Y above $P_m$ and too high for all other consumers located underneath G1.

A single threshold need not even improve aggregate economic efficiency. The reduction in drinking for consumers in region Y below $P_m$ increases efficiency, but the reduction in drinking for most of those consumers in region Y above $P_m$ reduces it. The net effect is ambiguous. Furthermore, lower thresholds need not be more efficient than higher thresholds. As the threshold falls, so does $P_m$; the region in which efficiency increases gets smaller, while the region in which it decreases gets larger. The net effect, which depends on the number of consumers in each region, is ambiguous.

These findings result from the shape of the penalty structure, not the magnitude of the penalty itself. Line G2 represents the marginal penalties for a weaker, second-best penalty structure that does not reduce fatalities to the optimal level but does maximize consumer surplus for the fatality level that is attained. The previous analysis carries through unchanged.

Kenkel (1993) points out that, while penalties for drunk driving are inefficiently low, the greater probability of arrest at high BAC levels makes the penalty structure more efficient by making the expected penalty (the product of the probability of arrest and the penalty if convicted) increasing for all $C > T$, more closely resembling a Pigovian tax. While true, however, this does not lead to full efficiency over the 0.02-0.10 BAC range influenced by zero tolerance laws, as Grant's (2007) careful examination of recent data shows that, as BAC rises, the increase in crash probabilities exceeds the increase in arrest probabilities. As a result, the conclusions of the previous analysis continue to hold.[3]

A theoretical case for the efficacy or efficiency of zero tolerance legislation cannot be made. Any appraisal must stand on its effects on traffic safety, which we assess next.

## III. DATA

Our empirical analyses relate the aggregate number of fatalities in each state in each year, or the distribution of BAC in those fatalities, to an indicator for the presence of a zero tolerance law and controls. The dependent variables are taken from the 1988-2000 FARS, which records each U.S. fatality that occurs on a public highway. This time period spans the range of years over which all but one of the zero tolerance laws satisfying the Congressional mandate were adopted (see below and Figure 1), yet, with the smallest of exceptions (a few months in South Dakota and Wyoming), post-dated the 1980s increase in the minimum legal drinking age to 21. For each fatality the state, time of day, and age of the driver in each vehicle involved in the accident is recorded. As discussed in Subramanian and Utter (1998), driver BAC is measured directly in about half of the observations and is imputed using NHTSA's imputation technique for the others, mostly nondrinkers; only about

5% of the observations involve drinking drivers with unmeasured BAC. Because most alcohol-related traffic fatalities involving youth drivers occur at night (Dee and Evans, 2001), our analysis will ultimately focus on fatalities in nighttime accidents involving youth drivers, of which there are roughly 30,000 in the data. The distributional analysis uses this microdata directly, while the aggregate fatality analysis, or "levels analysis," agglomerates fatalities in each state for each year. For this there are a total of 663 observations (13 years * 51 states, including the District of Columbia).

Figure 5 presents the CDF of driver BAC for all fatalities of youth drivers in single vehicle, nighttime accidents in the two years prior to the year a zero tolerance law was adopted in each state (top left) and the two years following the year a zero tolerance law was adopted in each state (top right). Surprisingly, no kinks are visible near the adult or youth thresholds in either CDF.

The two CDFs overlap so closely they cannot be easily distinguished; instead, their difference at each BAC level is presented in the bottom left graph of the figure. This corresponds to the difference between the dashed and solid CDFs in Figure 4, and is first positive, then negative, as expected. The bottom right graph in Figure 5 adopts the inverse perspective, presenting the difference in BAC at each percentile, the format used for the quantile regression results presented below. Again the results conform to theory, with BAC falling at low percentiles and increasing at high percentiles. But in both graphs these differences are small.

Our key independent variable is a dummy for the presence of a zero tolerance law in that state in that year. Initially this variable recognizes only those laws that pass Congressional muster by proscribing BAC levels of 0.02 or higher for all drivers under 21. As Figure 1 illustrates, however, some states passed earlier legislation that covered only some youth drivers or set more modest BAC limits. Later we will investigate the effect of these laws separately. The other independent variables are a full set of state and year fixed effects, population, the number of highway miles driven, the unemployment rate (all standard) and dummies for the presence of 0.08 adult BAC laws, seat belt laws, and administrative license revocation laws (ALR)–arguably the most important relevant legislation widely implemented over the period zero tolerance laws were adopted.[4]

State fixed effects account for all relevant state-specific, time-invariant factors, such as geography, while year effects capture time-varying factors common to all states, along with the average effect of all unmeasured factors that vary over time, such as vehicle weight. Both are standard in the modern traffic safety literature. The remaining controls vary within states over time. These are far from exhaustive. This is partly by necessity: one cannot hope to measure all statewide legal, weather, highway quality, and attitudinal factors that affect traffic safety over time. It is also partly by design. The traffic safety literature reveals the difficulties of controlling for all relevant factors directly. Aside from population, unemployment, and vehicle miles, all included here, other studies' controls are typically insignificant. Thus, our empirical approach is to begin with a simple specification, and then employ numerous direct and indirect methods to gauge robustness and the extent of omitted variables bias, including a control group methodology that involves estimating the model for groups to which the law does not apply. This reveals bias of the size and magnitude needed to generate false evidence of zero tolerance laws' effectiveness. With this specification, too, we can replicate the findings of previous studies and explain why they differ from ours.

## IV. EMPIRICAL METHODS

Our strategy for estimating the effects of zero tolerance laws on fatalities–the levels analysis–relies on non-synchronous changes in these laws at the state level. This motivates a panel count data model, in particular the following negative binomial count data regression specification:

$$\textbf{(1)} \qquad F_{s,t} \sim NegBin(\lambda_{s,t}, \theta)$$
$$where \quad \ln(\lambda_{s,t}) = \beta X_{s,t} + \gamma ZT_{s,t}$$

where F represents the number of fatalities in state s during year t, X contains control variables including state and year fixed effects, ZT is a dummy that equals one for states with a zero tolerance law in that year, and ß and $\gamma$ are coefficients. The negative binomial model is an extension of the Poisson count data model that allows for overdispersion (the variance exceeds the mean): $\lambda_{s,t}$ is the expected value of $F_{s,t}$, while $\theta$ is the overdispersion parameter (which is uniformly significant here). The coefficient $\gamma$ captures the change in fatalities due to the zero tolerance law, in percentage terms.

We estimate this model on fatalities in the "target group," the population of interest, and, to check for omitted variables bias, fatalities in "control groups" exempt from the law but subject to the same underlying factors that influence fatalities in the target group, such as weather, road quality, or safety attitudes. When no bias-causing controls are omitted from X, $\hat{\gamma}$ should be insignificant in the control group and (multiplied by 100) will be an unbiased estimate of the percentage change in fatalities attributable to the law in the target group. In contrast, if $\hat{\gamma} < 0$ is comparable in the target and control groups, both estimates may be caused by omitted factors, and our conclusions are weakened accordingly. This estimation strategy, adopted in recent studies of traffic safety legislation such as Dee and Evans (2001) and Grant and Rutner (2004), has additional power here because of the existence of several reasonable control groups, described below.

We also examine the effects of zero tolerance laws on the distribution of fatalities by driver

BAC. One can do this using least squares regressions of the fraction of statewide, annual fatalities falling in different ranges of driver BAC–$[T_2, T_1)$, and $[T_1, \infty)$, in the terminology of the theory–on the zero tolerance dummy and controls. If the law works, fewer fatalities should involve the "mild drinkers" whose BAC falls between the original threshold $T_1$ (0.08 or 0.10 generally) pertaining to adult drivers and the new youth threshold $T_2$ (0.01 or 0.02) instituted by zero tolerance laws.

Alternatively, one can use quantile regression for the same purpose. This essentially estimates the inverse of the difference between the two CDFs in Figure 4 (though only for drivers involved in fatal accidents). As in Koenker (2005), this regression is specified as follows:

$$\begin{aligned} &\hat{\beta}(\tau),\hat{\delta}(\tau) = argmax \sum_{i=1}^{N} \rho_\tau(BAC_i - X_i\beta - \delta_1 TARGET_i - \delta_2 ZT_i - \delta_3 TARGET_i * ZT_i) \\ \textbf{(2)} \qquad &where \quad \rho_\tau(z) = \tau z, \quad z \geq 0, \\ &\qquad\qquad \rho_\tau(z) = (\tau - 1)z, \quad z < 0 \end{aligned}$$

where i indexes individual fatalities (not state-year aggregates), BAC is the driver's blood alcohol concentration, ZT and X are the zero tolerance law dummy and the controls, as before, and TARGET is a dummy that equals one if the observation is from the target group and zero if it is from a control group. This approach allows one to incorporate the control group directly into the analysis, which is the easiest way to utilize a control group methodology with quantile regression. In this model, which is estimated on fatalities in both the target and control groups combined, $\delta_2 ZT$ "absorbs" omitted variable bias from all unmeasured influences on driver BAC that affect both groups jointly and coincide with the implementation of a zero tolerance law in that state. The causal effects of the zero tolerance law on the target group appear in the interaction term $\delta_3$.

Quantiles, indexed by $\tau$, can take any value on the unit interval, so the curve $\delta_3(\tau)$, $\tau \in (0,1)$ describes the effect of zero tolerance laws on the distribution of driver BAC in fatal accidents. A zero tolerance law changes driver BAC at percentile $100*\tau_0$ by $\delta_3(\tau_0)$. One can infer the magnitudes of the intended and perverse effects of zero tolerance laws from these estimates.

## V. RESULTS

Replication

We begin by replicating the results of previous fatality analyses.[5] The first such studies, by Voas et al. (1998), Blomberg (1992), and Hingson et al. (1989, 1994), used a simple "pre-post" methodology that compares the change in youth fatalities in states adopting zero tolerance laws to that in a control group, such as youth in states without such laws. These studies find these laws reduce nighttime youth traffic fatalities by about 20%. This estimate is implausibly large. Zero tolerance laws operate by reducing the number of youth drivers whose BAC falls between the contemporaneous adult illegal threshold of 0.08 or 0.10 and the new youth threshold of 0.01 or 0.02. Figure 5 shows that these mild drinkers represent at most 15% of youth driver fatalities, and that their ranks are hardly thinned in the years after the law is instituted. These early studies' findings probably reflect omitted variables bias, as no confounding factors are explicitly controlled for.

This deficiency was remedied by a second wave of studies, by Dee (2001), Dee and Evans (2001), Eisenberg (2003), and Voas et al. (2003), that used multiple regression to examine fatalities in all states over a period of several years. All but the last of these utilize state and year fixed effects, along with several statewide time-varying controls. In our data, these fixed effects are highly significant and their inclusion substantially diminishes the zero tolerance coefficient. The conclusions of the three studies utilizing fixed effects, while somewhat variable, are mildly favorable, finding fatality reductions on the order of 5% for youth and no effect for adults. The negative coefficient estimate for youth suggests the laws work; the zero estimate for adults, to whom the law does not apply, suggests the specification is sound.

In our analysis of similar data, with similar controls, we find the same thing. Panel A of Table 1 presents the results: the coefficient estimate on the zero tolerance dummy in a regression that includes the controls listed above, which have been suppressed for brevity. Each cell of the table represents a separate regression. Four dependent variables are considered: all fatalities in all accidents involving youth drivers; all fatalities in vehicles driven by youth drivers; those fatalities in single-vehicle accidents alone; and driver fatalities in single-vehicle accidents. As it turns out, the results are similar in all cases –a 5% reduction in fatalities for youth, but not adults. (They would also be similar if the dependent variable was per mile fatalities, as the coefficient on the logarithm of miles rarely differs significantly from one.) Results like these have been the basis for the conclusion, to date, that zero tolerance laws work.

Level Effects with Control Groups, Broken Down by Time of Day

Panel A, like previous studies, analyzes fatalities at all times of day. But Dee and Evans (2001) show that fewer than 15% of daytime traffic fatalities among teens involve alcohol, while more than half of nighttime fatalities do. A more discerning approach, then, following Dee and Evans, is to focus on youth nighttime fatalities as the target group, and treat youth daytime fatalities as a quasi-control group with which to check the soundness of the estimates. (Chaloupka, Grossman, and Saffer, 2002, note this approach requires daytime drinkers to be no more sensitive to the law than their nighttime counterparts.) We now adopt this approach and separate nighttime (9:00 pm-4:59 am) fatalities from those occurring during the day (8:00 am - 5:59 pm). A sizeable negative coefficient should be found only for youth, not adults, and then only at night.

Estimates for this target group, at the top of Panel B, suggest zero tolerance laws reduce nighttime fatalities involving youth drivers by four to seven percent. While statistical significance is mild, ranging from 5-15%, the consistency of the results is reaffirming. But similar estimates also occur during the day, in the top row of Panel C, when they are not anticipated. Similar estimates are also found in age-related control groups, drivers too old to be covered by the law: "young adults" (drivers aged 21-25) and "adults" (drivers aged 21-90). Nighttime fatality estimates for the former are found in the second row of Panel B, and those for the latter in the third row. These imply fatality reductions of four to seven percent, as with the target group, youth nighttime fatalities, in row one.

The fact that zero tolerance laws are estimated to cut fatalities by similar amounts in the target and control groups implies that these estimates are attributable not to the law itself, but to other coincident factors. These may relate to drinking generally, generating the negative nighttime coefficients for adults, or to youth driving, generating the negative daytime coefficients for youth. Unfortunately, attempts to further discern these factors proved indeterminate.[6] The only time zero tolerance laws *aren't* negatively associated with fatalities is for adults during the day (Panel C). This divergence in estimates, between youth daytime fatalities and adult daytime fatalities, is ultimately the sole foundation of the favorable conclusions of earlier panel studies of zero tolerance laws.

We conducted several robustness checks with this basic model. The first broadened our controls by incorporating three additional dummies, for 70+ mph speed limits, graduated licensing laws, and mandatory imprisonment requirements for first time violators, the only additional controls that were significant and affected youth and adult fatalities disproportionately in either of the two panel data studies most strongly supporting zero tolerance laws, Dee (2001) and Eisenberg (2003).

The zero tolerance coefficients for all groups were virtually identical to those in Table 1. Several specification changes also had no effect on coefficient estimates: the inclusion of state time trends, altering the young adult control group to include only drivers aged 26-30, or replacing the negative binomial with the simpler, more restrictive Poisson model. Nor would estimates materially change by accounting for residual autocorrelation, which was small (< 0.10).

Finally, extended specifications were used to break down the laws' effects more finely. The first looks for internal consistency. Our initial specification only recognizes laws that pass Congressional muster by restricting the illegal BAC to 0.01 or 0.02 for all drivers under 21, here called "full laws." But Figure 1 shows some states had earlier passed "partial laws" that pertained only to some youth drivers, allowed higher BAC levels, or both. (For example, in 1991 Georgia adopted an illegal BAC of 0.06 for youth under 18.) The specification in the upper half of Table 2 includes coefficients for full laws, partial laws, and an "interaction" of the two, which estimates the effect of a full law in states that already had partial laws. If zero tolerance laws operate as their advocates intend, the full law coefficient should dominate the partial law coefficient, while the interaction term should be positive, because the introduction of a full law will have less impact in states where a partial law is already in effect. Instead, the reverse is true: coefficients on the partial law dominate those on the full law, while the interaction term is negative. Again similar coefficients are observed in the control group (for two of the three variables). The simplest explanation for these awkward results is the existence of omitted variables that impart a sizeable bias to key coefficients.

Then, in the lower half of Table 2, we estimate the effect of zero tolerance laws separately for states that do and do not have administrative license revocation. Eisenberg (2003) finds a significant effect of zero tolerance laws only in states with ALR, while Dee and Evans (2001) do not find any difference between states with and without ALR. Here the estimated effect in the control group is similar to that in the target group, whether or not the state has ALR.

Ultimately, none of these regressions obtain a zero tolerance coefficient that is significant in the expected direction among youth without a matching coefficient in a control group. Even those coefficients that are not significant–and most are not–are matched by those in the control groups. There is little evidence that zero tolerance laws reduce fatal accidents involving youth drivers.

Distributional Effects

The effect of zero tolerance laws on aggregate fatalities is theoretically ambiguous and empirically small. But theory does make strong predictions about the influence of these laws on the distribution of BAC among drivers involved in fatal accidents. The distributional analysis tests these predictions and quantifies the intended and perverse effects of the law. It is possible, after all, that small aggregate effects are obtained because the intended and perverse consequences of zero tolerance laws are offsetting. These estimations also serve as a final robustness check on our previous findings, because the distributional analysis implicitly controls for all factors that affect fatalities independent of driver BAC, even if they vary across states over time.

We first use quantile regression to estimate the effect of zero tolerance laws on the distribution of BAC of youth drivers killed in single-vehicle nighttime accidents in our data. The results are presented in Figure 6, which graphs $\delta_3(\tau)$ for $\tau \in (0.4,1)$, all quantiles with a positive BAC in the aggregate. Negative values imply a given quantile is reached at a lower BAC, positive values at a higher BAC, and theory predicts negative coefficient estimates until the crossing point in Figure 4 and then positive values afterwards. The estimates are consistent with this prediction, but are never significant. They are also extremely small in magnitude, indicating that zero tolerance laws hardly change the distribution of driver BAC, as observed in the simple comparison in Figure 5.

The other way to examine distributional effects is to estimate the influence of zero tolerance laws on the fraction of fatalities falling in two categories, defined by the (approximate) adult and youth thresholds during the time zero tolerance laws were imposed: those with $0.02 \leq$ BAC $\leq 0.09$, whose share should decrease, and those with BAC $\geq 0.10$, whose share should increase. The results are presented in Table 3. Each cell of the table presents the zero tolerance coefficient, its standard error, and the mean of the dependent variable (so relative effects can be inferred). Again each cell represents a different regression, with controls suppressed for brevity. The coefficients for youth, in the first and fourth rows, take the expected signs, but are small and never significant, and are matched by the corresponding coefficients in the control groups more often than not. These results reinforce those from the quantile regression.

To look for problems arising from imputed BAC, this analysis was replicated using the unimputed data, in which the distribution of fatalities with measured driver BAC and the number of fatalities with unmeasured BAC were compared across the target and control groups. We also estimated several alternative quantile regressions to check for robustness: omitting the control group, utilizing all fatalities in single vehicle accidents, and limiting the sample to states with high BAC reporting. These analyses all confirm the findings presented here. There is no evidence that zero tolerance laws materially change the distribution of BAC of drivers involved in fatal accidents. These laws have little effect on fatalities because they have little effect on driver behavior.

## VI. CONCLUSION

The only evidence that consistently supports zero tolerance laws' effectiveness ultimately comes from two sources: pre-post studies that do not utilize controls and yield implausibly large effects, and significant negative panel regression coefficients for youth daytime fatalities that are not matched among adults, who are exempt from the law. This evidence is far outweighed by that to the contrary: similar coefficient estimates for daytime and nighttime fatalities among youth, though far more of the latter are alcohol-related; significant negative coefficients on adult fatalities at night, matching those for youth; confounding findings concerning full and partial laws; and, especially, the absence of any effect in the distributional analysis.

Perhaps it is not surprising that zero tolerance laws are ineffective. After all, the strongest zero tolerance law ever passed–Prohibition–had a relatively small effect on alcohol consumption in the long term, according to Dills, Jacobson, and Miron (2005). Still, one wonders whether these laws' weak effects are a function of their design. Zero tolerance laws encourage mild drinkers to reduce their drinking, without providing further incentives for heavy drinkers to reduce their drinking. Indeed, some individuals should increase alcohol consumption following adoption of a zero tolerance law. Mild drinkers are less dangerous drivers than heavy drinkers are, and they generate a small fraction of traffic fatalities. Zero tolerance laws thus attempt to further reduce drinking among "lower priority" mild drinkers, while slightly weakening drinking disincentives for heavier drinkers, who are far more dangerous and cause far more traffic fatalities. This is of questionable merit.

This contention is buttressed by other recent evidence. A comprehensive study by Freeman (2007) finds the other major reduction in alcohol thresholds, to 0.08 for adults, has also been ineffective, and Grant's (2007) new structural analysis of the drunk driving penalty structure supports both Freeman's conclusion and that in this paper. The only way to strengthen drunk driving penalties unambiguously, guarantee a decrease in drunk driving, and improve economic efficiency is to ensure that the marginal legal penalty for consuming a unit of alcohol does not decrease at any BAC level under the new law. This, in turn, cannot happen unless the penalty at or beyond the original BAC threshold is raised ($H_2(C) > H_1(C)$ for $C > K^* \geq T_1$), as some states have recently done by adopting aggravated drunk driving laws that administer additional penalties for drivers with BACs of 0.15 or more. Collectively, these studies suggest the existing penalty structure can be improved upon, and economic analysis can serve helpfully in doing so. For one such effort, see Grant (2007).

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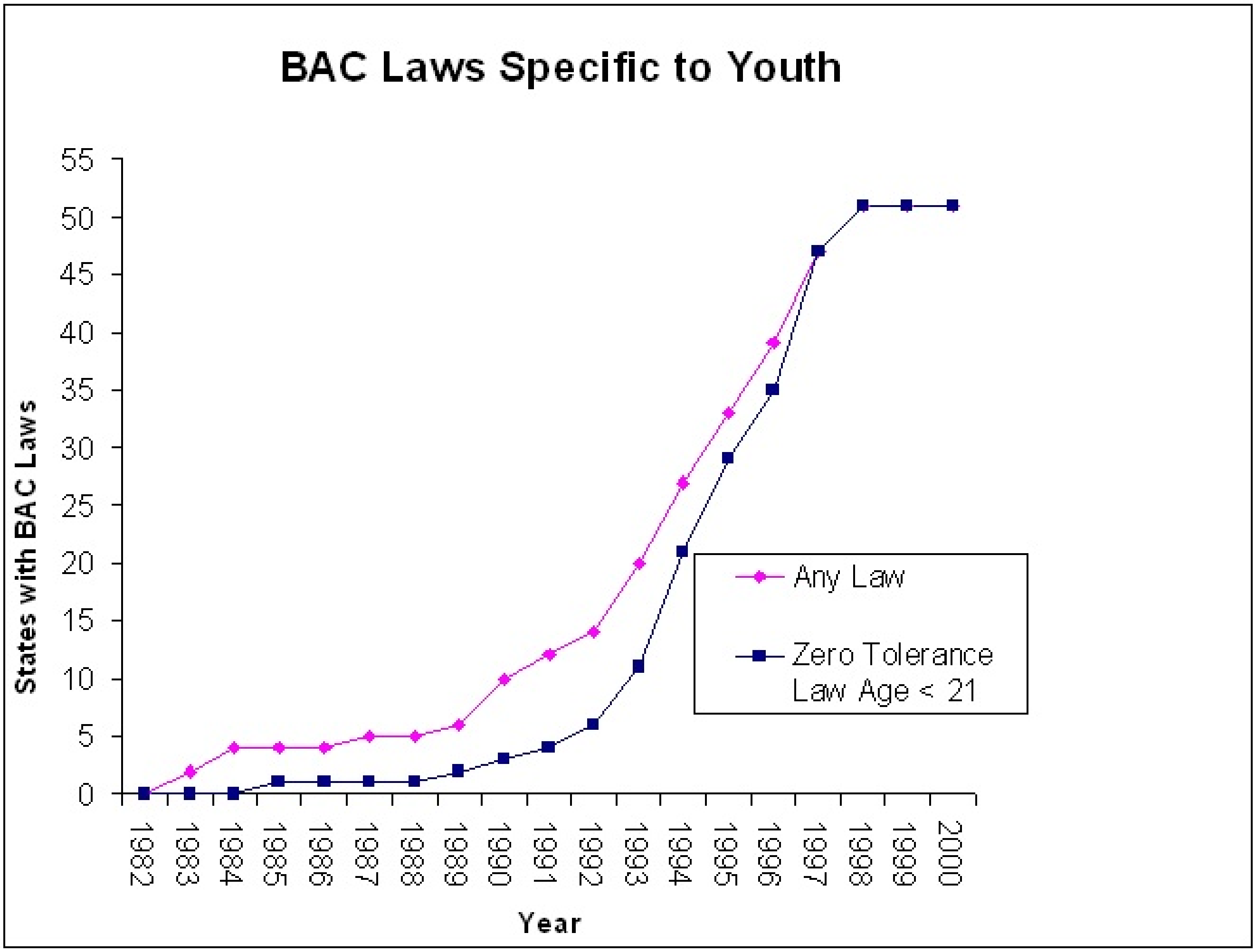
FIGURE 1
The Incidence of Zero Tolerance Laws in the United States

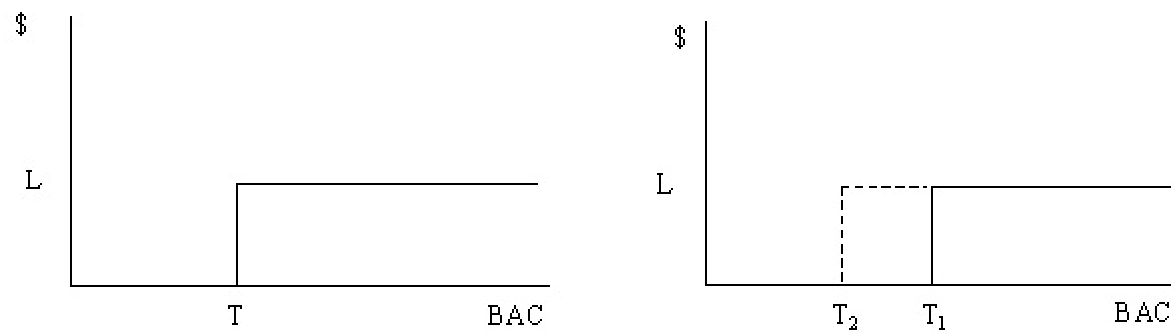
Note: The "any law" line includes any state with a law that specifies a reduced BAC threshold for



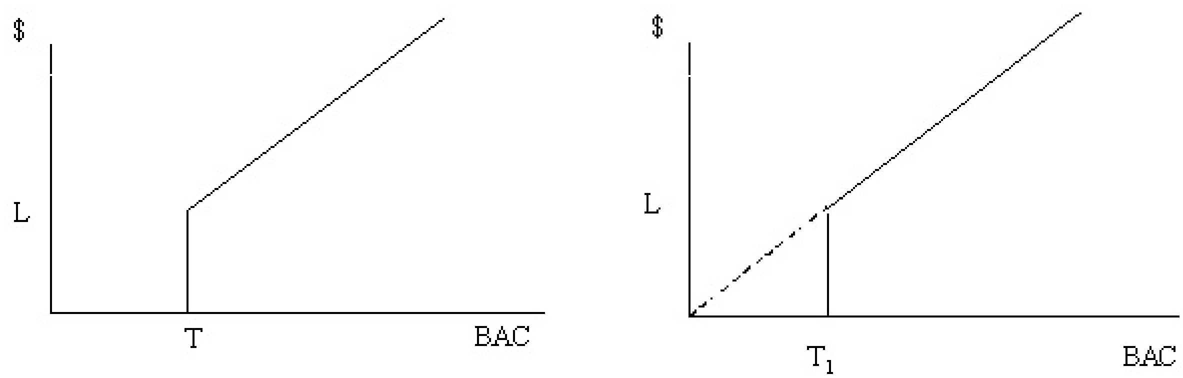

some or all drivers under the age of 21 in that year. The "zero tolerance law age < 21" line includes only those laws with a per se illegal BAC no greater than 0.02 for all drivers under the age of 21. These laws are sometimes called "full laws" in the text. The difference between the two lines reflects "partial laws" that pertained only to some youth drivers, allowed higher BAC levels, or both.

FIGURE 2
Illustrations of Alternative Penalty Structures

1. A single threshold T ($H(C) = 0$, $C \leq T$; $H(C) = L$, $C > T$), and threshold reduction from $T_1$ to $T_2$.

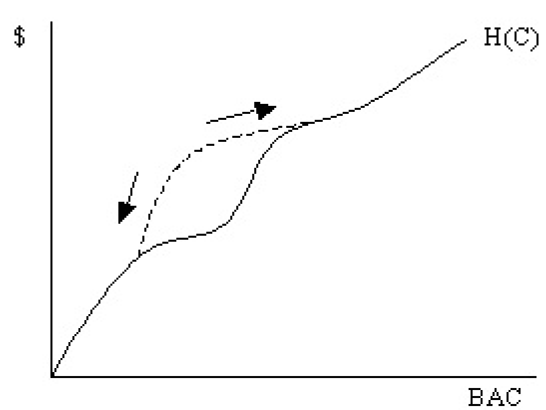



2. The probability of arrest is proportional to BAC, with original threshold at T ($H(C) = 0$, $C \leq T$; $H(C) = kC$, $C > T$, with $kT = L$) and threshold reduction from $T_1$ to 0.




3. Arbitrary change in the penalty structure that increases penalties below some BAC level but not above that level ($H_2(C) > H_1(C)$, $K_1 \leq C \leq K_2 < T$; $H_2(C) = H_1(C)$, $C \geq T$).

4. A simple change in the law such that the marginal legal penalty for each unit of alcohol consumption never decreases.



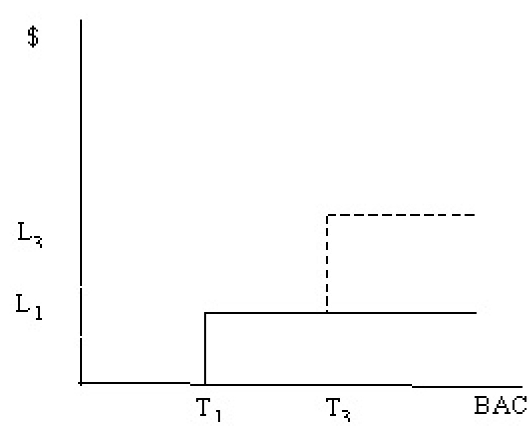



FIGURE 3
Population Effects of Single BAC Threshold

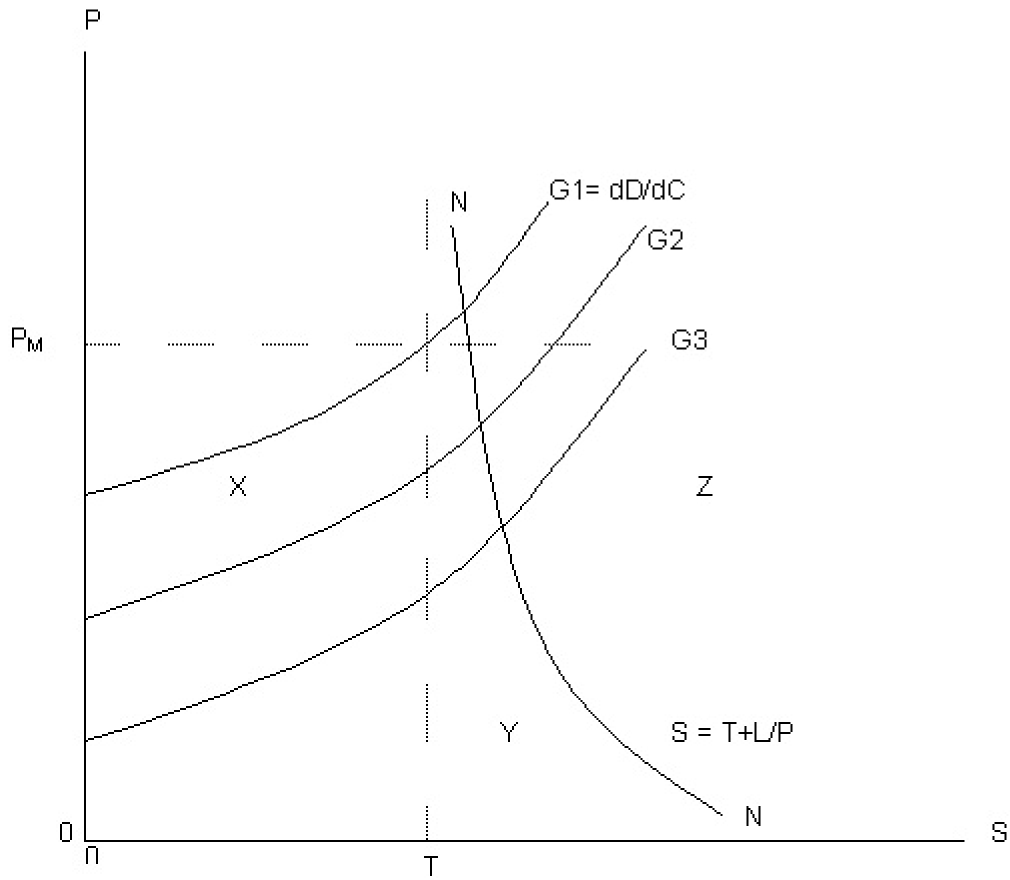
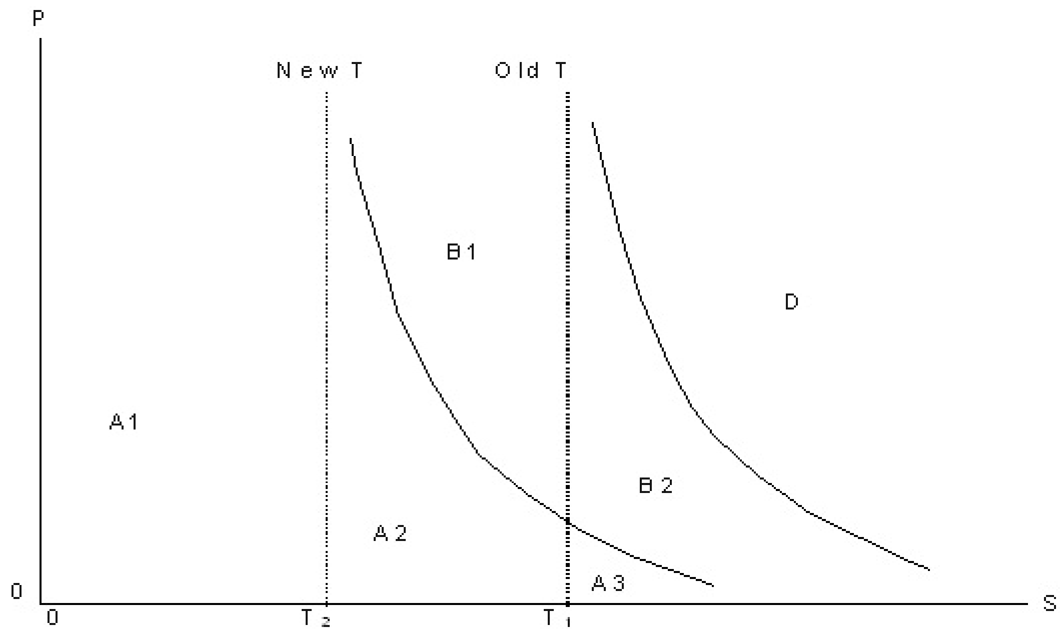


FIGURE 4
Population Effects of Threshold Reduction




CDF of Driver BAC

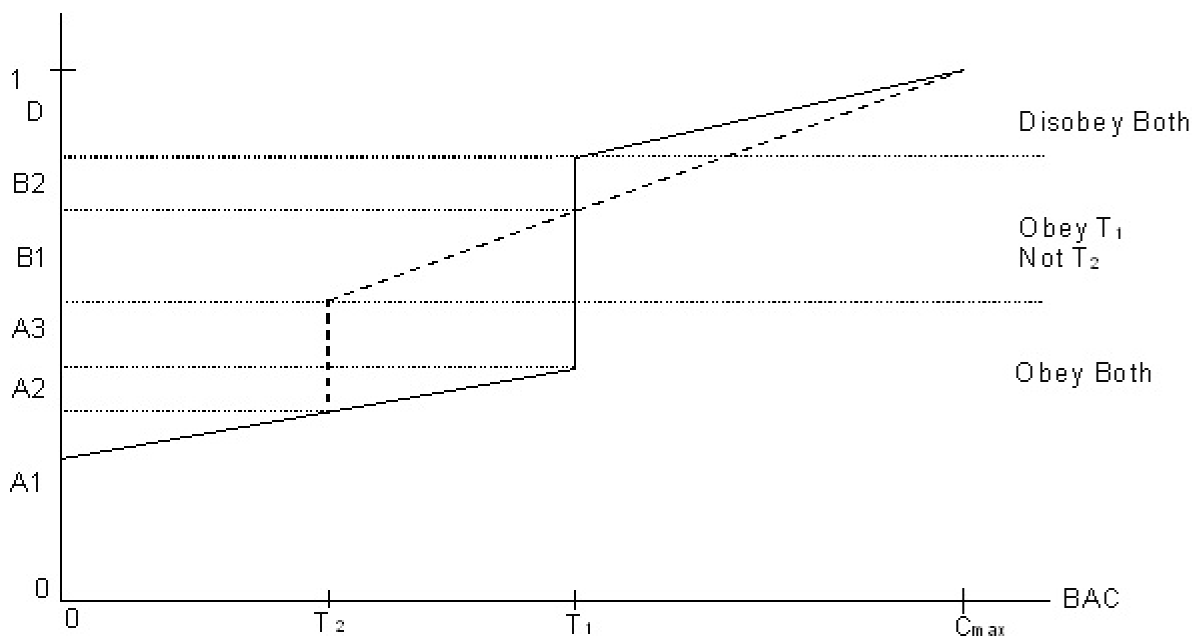
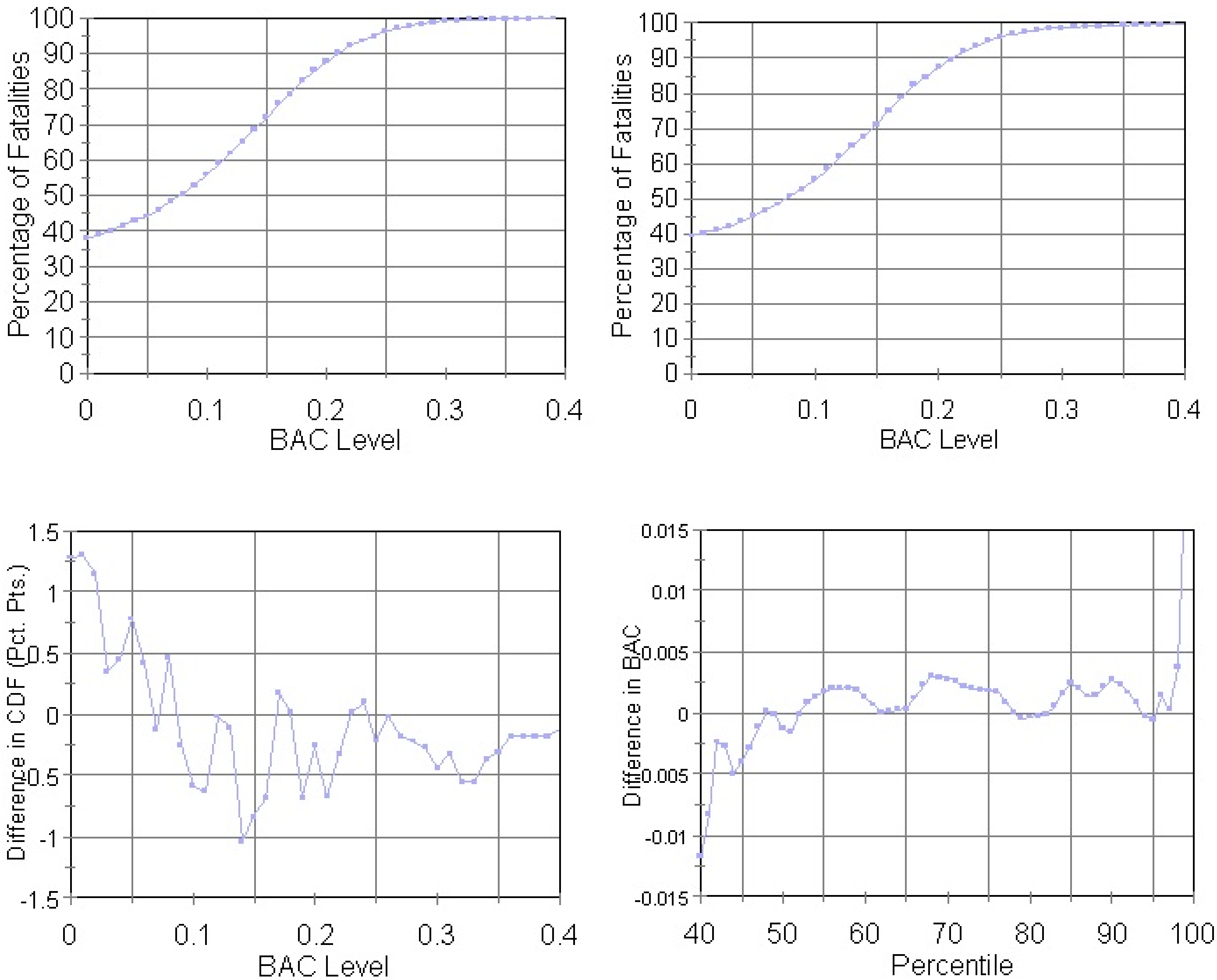


FIGURE 5

Two Distributions of BAC for Youth Driver Fatalities in Single Car, Nighttime Accidents, and the Difference between These Distributions

Top Left: CDF of driver BAC in the two calendar years before the year a zero tolerance law was implemented in each state. Top Right: CDF of driver BAC in the two calendar years after the year




a zero tolerance law was implemented in each state. Bottom Left: The difference between the two CDFs at each BAC level. Bottom Right: The difference in BAC at each percentile, interpolated when necessary from the data.

FIGURE 6
Quantile Regression Results (Driver Fatalities in Single Vehicle, Nighttime Accidents)

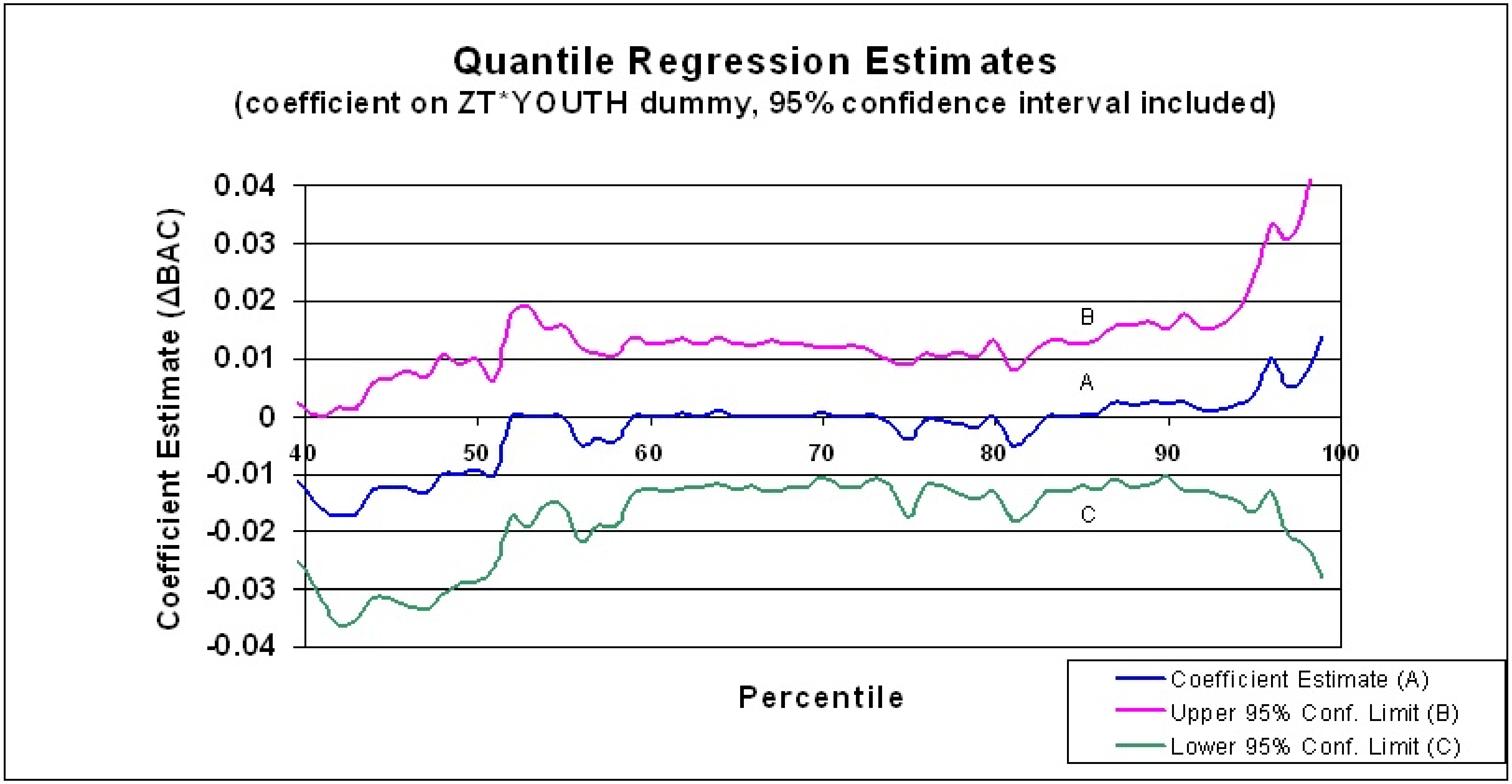


TABLE 1
Aggregate Fatality Analysis (coefficient estimates, with standard errors in parentheses)

| *TIME OF DAY*<br>Drivers' Age Range | All Fatalities in All Accidents Involving Specified Drivers | All Fatalities in Vehicles Driven by Specified Drivers | All Fatalities in Single Vehicle Accidents | Fatalities of Drivers in Single Vehicle Accidents |
|---|---|---|---|---|
| *Panel A: DAY AND NIGHT FATALITIES* | | | | |
| Drivers Aged 15-20 (Youth) | -0.047*<br>(0.018) | -0.044*<br>(0.020) | -0.047*<br>(0.024) | -0.040**<br>(0.027) |
| Drivers Aged 21-90 (Adults) | 0.008<br>(0.012) | 0.004<br>(0.012) | 0.008<br>(0.015) | 0.006<br>(0.016) |
| *Panel B: NIGHTTIME FATALITIES* | | | | |
| Drivers Aged 15-20 | -0.050**<br>(0.028) | -0.047<br>(0.030) | -0.065*<br>(0.033) | -0.070**<br>(0.037) |
| Drivers Aged 21-25 (Young Adults) | -0.047**<br>(0.028) | -0.034<br>(0.030) | -0.059**<br>(0.036) | -0.071**<br>(0.039) |
| Drivers Aged 21-90 | -0.034*<br>(0.017) | -0.039*<br>(0.018) | -0.053*<br>(0.021) | -0.056*<br>(0.022) |
| *Panel C: DAYTIME FATALITIES* | | | | |
| Drivers Aged 15-20 | -0.053*<br>(0.024) | -0.052**<br>(0.029) | -0.061<br>(0.040) | -0.007<br>(0.050) |
| Drivers Aged 21-25 | 0.021<br>(0.033) | 0.024<br>(0.033) | -0.040<br>(0.049) | -0.021<br>(0.058) |
| Drivers Aged 21-90 | 0.022<br>(0.014) | 0.022<br>(0.015) | 0.037**<br>(0.020) | 0.031<br>(0.022) |

Note: Each cell presents the coefficient on the zero tolerance dummy variable in a negative binomial regression also containing a full set of state and year dummy variables, the log of vehicle miles traveled, the log of population, unemployment, dummies for primary and secondary seat belt laws, a dummy for an administrative license revocation law, and a dummy for a 0.08 adult BAC limit, all measured by state by year. Sample size: 663 (51 states * 13 years). Significance is indicated at the 5% (*) and 10% (**) levels. Panel C includes fatalities at all times of day, not just the time periods defined as day (8:00 am-5:59 pm) and night (9:00-4:59 am) in the text.

TABLE 2

Extended Nighttime Fatality Analysis (coefficient estimates, standard errors in parentheses)

| SPECIFICATION<br>*Drivers' Age Range*<br>Law Variable/Coefficient | All Night Fatalities in Vehicles Driven by Specified Drivers | Fatalities of Drivers in Single Vehicle Night Accidents |
|---|---|---|
| FULL AND PARTIAL LAWS | | |
| *Drivers Aged 15-20* | | |
| "Full Law" Coefficient | -0.042<br>(0.032) | -0.040<br>(0.043) |
| "Partial Law" Coefficient | -0.053<br>(0.052) | -0.058<br>(0.053) |
| Interaction Term | -0.033<br>(0.038) | -0.115*<br>(0.043) |
| *Drivers Aged 21-90* | | |
| "Full Law" Coefficient | -0.028<br>(0.019) | -0.043**<br>(0.023) |
| "Partial Law" Coefficient | 0.002<br>(0.032) | -0.013<br>(0.038) |
| Interaction Term | -0.044*<br>(0.023) | -0.052**<br>(0.027) |
| ALR INTERACTION | | |
| *Drivers Aged 15-20* | | |
| ALR Coefficient | -0.080*<br>(0.034) | -0.015<br>(0.038) |
| Zero Tolerance Coefficient | -0.062<br>(0.040) | -0.021<br>(0.049) |
| Interaction Term | 0.021<br>(0.037) | -0.070<br>(0.045) |
| *Drivers Aged 21-90* | | |
| ALR Coefficient | -0.045*<br>(0.020) | -0.060*<br>(0.024) |
| Zero Tolerance Coefficient | -0.059*<br>(0.024) | -0.074*<br>(0.029) |
| Interaction Term | 0.028<br>(0.022) | 0.025<br>(0.027) |

Note: Each cell presents selected coefficients from a negative binomial regression also containing a full set of state and year dummies, the log of vehicle miles traveled, the log of population, unemployment, dummies for primary and secondary seat belt laws and a 0.08 adult BAC limit. Sample size: 663 (51 states * 13 years). Significance indicated at the 5% (*) and 10% (**) levels.

TABLE 3
Fatality Distribution Analysis (coefficient estimates, with standard errors in parentheses and the mean of the dependent variable in brackets).

DEPENDENT VARIABLE

| ACCIDENT TYPE<br>Drivers' Age Range | Fraction of Fatalities in which $0.02 \leq$ Driver's BAC $\leq 0.09$ | Fraction of Fatalities in which Driver's BAC $\geq 0.10$ |
|---|---|---|
| ALL NIGHTTIME FATALITIES IN VEHICLES DRIVEN BY:<br>Drivers Aged 15-20 | -0.009<br>(0.011)<br>[0.146] | 0.001<br>(0.014)<br>[0.375] |
| Drivers Aged 21-25 | -0.002<br>(0.011)<br>[0.141] | -0.023<br>(0.014)<br>[0.603] |
| Drivers Aged 21-90 | 0.000<br>(0.004)<br>[0.104] | -0.012**<br>(0.007)<br>[0.582] |
| DRIVER FATALITIES IN NIGHTTIME SINGLE-VEHICLE ACCIDENTS<br>Drivers Aged 15-20 | -0.009<br>(0.014)<br>[0.137] | 0.006<br>(0.022)<br>[0.485] |
| Drivers Aged 21-25 | -0.025*<br>(0.013)<br>[0.122] | 0.022<br>(0.018)<br>[0.714] |
| Drivers Aged 21-90 | -0.006<br>(0.005)<br>[0.088] | 0.002<br>(0.009)<br>[0.716] |

Note: Each cell presents the coefficient on the zero tolerance dummy variable in a weighted least squares regression (population as weights) also containing a full set of state and year dummy variables, the log of vehicle miles traveled, the log of population, unemployment, dummies for primary and secondary seat belt laws, a dummy for an administrative license revocation law, and a dummy for a 0.08 adult BAC limit. Sample size: 663 (51 states * 13 years). Significance is indicated at the 5% (*) and 10% (**) levels.

*. Craig Depken, Kenneth Copeland of NHTSA, and Erik Strickland of Mothers Against Drunk Driving (MADD) provided data; Raynoo Issarachevawat, Huong Pham, John Hansen, Pritesh Patel, and Mishuk Chowdhury provided helpful research support; and seminar participants at Nicholls State University, the University of North Texas Health Science Center, the University of Montana, Sam Houston State University, UT-Arlington, and the Southern Economic Association Conference made useful comments. These contributions are appreciated. The design and presentation of this study were helpfully influenced by Cox (2006), Friedman and Sjostrom (1993), Grogger (2004), Miron (1998), and, especially, Rosen and Rosenfield (1997).

*Grant*: Assistant Professor, Sam Houston State University, Box 2118, Huntsville, TX 77341-2118. Phone 1-936-294-4324, Fax 1-936-294-3488, E-Mail dgrant@shsu.edu.

1. One need not parameterize V to do this, because the behavioral response to a threshold reduction depends on just two things: the reduction in V needed to satisfy $T_1$, and that needed to satisfy $T_2$. The population response to a threshold reduction is given by the joint distribution of these two quantities, which can be illustrated nonparametrically, without making any additional assumptions about V. While this is the most general and most facile way to illustrate the population effects of threshold reductions and other simple penalty structures, the parametric approach used in the text yields the same conclusions and is preferable for discussing efficiency.

2. As is the relation of threshold reductions to the penalty magnitude L. An increase in L moves both hyperbolas to the right in Figure 4, increasing areas A2, A3, and B2, thus magnifying both the intended and perverse effects of the policy. Therefore, threshold reductions need not be more effective when expected penalties for drunk driving are larger.

3. Treat G3 as the marginal penalty structure dH/dC under a zero tolerance law. It lies below G1 because the probability of arrest grows more slowly with C than the probability of an accident. All

drivers in Figure 3 to the right of G3 reduce their consumption so that they "lie on" G3; that is, driver *n* chooses the BAC for which dH/dC equals $P_n$ (unless $P_n$ is low enough, and then C = 0). Compared to a threshold at T, this penalty structure efficiently lowers alcohol consumption for some drivers, for example, those in region X lying below G3. But it inefficiently raises consumption for others, for example, those in region Y between $P_M$ and G3. Again the net effect is ambiguous.

4. The data come from the Bureau of Labor Statistics, *Highway Statistics*, *Traffic Safety Facts*, the *Digest of Alcohol Highway-Safety Related Legislation*, and Wagenaar, O'Malley, and LaFond (2001). Seat belt laws can be primary or secondary; only the former allows a vehicle to be stopped solely for a seat belt violation. ALR allows the state to suspend or revoke an individual's license immediately upon testing positive for drunk driving or refusing to be tested. Population and miles are logged because the "link function" in the generalized linear regression specification below models fatalities in logs.

5. Several other studies examine these laws' effect on drinking or other related outcomes: drinking after driving, suicide, and venereal disease. For the most part, these studies are evenly divided between those finding no effect and those finding a significant negative effect. And some of these negative effects are not, in fact, predicted by theory, because they concern heavy drinking or outcomes associated with heavy drinking, which this paper has shown should rise, not fall, with a zero tolerance law. Both these claims apply, in particular, for the two studies examining drinking and drunk driving, Wagenaar, O'Malley, and LaFond (2001) and Carpenter (2004).

6. Every few years MADD grades several dimensions of state drunk driving control efforts, including political leadership, laws and other sanctions, law enforcement, public awareness, and youth-directed efforts. Improvements in these grades tended to be positively associated with the adoption of zero tolerance laws, but the associations were imprecise, insignificant, and weaker when 0.08 BAC laws

and ALR were controlled for.  One cannot draw firm conclusions from these results.