



# Deterring Drunk Driving Fatalities: An Economics of Crime Perspective

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## I. Introduction

Econometric studies of public policies that might deter driving-under-the-influence (DUI) offenses generally adopt, either explicitly or implicitly, the basic framework provided in Becker's (1968) expected utility model of criminal behavior. The literature has generally not provided consistent evidence of any particular source of direct deterrence due to the probability and/or severity of punishment for DUI, however. This may reflect the fact that only indirect DUI deterrence through alcohol control policies such as beer taxes and drinking age is consistently considered in these studies. Forced to select among many policy variables to keep regressions manageable, researchers have virtually always controlled for beer taxes and drinking-age laws in reduced-form models, but the selection of other variables (laws establishing sanctions for DUI, law enforcement efforts and laws that facilitate this effort, the potential for civil action against drivers or against bar owners who sell to drunk drivers, determinants of liquor price and availability, attitudes toward drinking, etc.) has been much less consistent. Furthermore, because there is no direct measure of state-level DUI offenses, some proxy must be used as a dependent variable (generally, some measure of traffic fatalities is employed) in studies using state-level data, and the proxy or proxies chosen can also vary from study to study (e.g., total fatalities, night-time fatalities, single-occupant fatalities, and fatalities involving young drivers). Thus, one study may find some specific laws or policies to be effective, but those same variables may have insignificant coefficients in another study while other variables seem significant, implying that no robust results can be found.

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Indeed, some DUI studies suggest that direct deterrence is ineffective because variables measuring or proxying the probability of conviction and severity of punishment are not significantly correlated with proxies for drunk driving [e.g., see Evans et al. (1991) and Wilkinson (1987)].

The wide variety of specifications and conflicting results in DUI studies implies that a systematic examination of direct DUI deterrence in the context of Becker's (1968) economic theory of crime is warranted. This implication is worked out below, and in the process, many of the conflicting implications of previous research are addressed. First, the data used in this study may provide a better measure of the degree to which alcohol is related to an accident than general fatality measures employed in almost all previous studies. Data are used that account for the blood alcohol content (BAC) of drivers involved in fatal accidents. Only Chaloupka et al. (1993), Mast (1996), and Mast et al. (1998) have employed a fatality measure directly related to the BAC level of drivers, and the sample of Chaloupka et al. (1993) was quite limited. Second, this study controls for *per capita* consumption of alcohol. Most DUI research has focused on beer taxes as a proxy for beer price and, implicitly, beer consumption. Wilkinson (1987) and Mast et al. (1998) are the only studies using state-level data to directly estimate the effect of alcohol consumption on vehicle mortality. Third, fixed-effects models are estimated, which may result in more reliable estimates when using panel data. Although some researchers have controlled for state-specific factors, many others have not.<sup>1</sup> Fourth, and most significantly, variables that should influence the probabilities of arrest and conviction and the severity of punishment for DUI are systematically examined, both independently and in combination, in greater detail than in any previous study.<sup>2</sup>

The variables that tend to have the strongest deterrent effects in econometric studies of crime—those reflecting the probability of detection (e.g., the probability of arrest)<sup>3</sup>—generally cannot be measured for DUI, of course, because no state-level measure of DUI offenses exists (the probability of arrest is generally measured as the ratio

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<sup>1</sup>Because studies using pooled cross-section data contain multiple observations for each state, it is likely that the error terms for each state are correlated over time. Including state fixed effects alleviates any omitted variable bias due to unmeasured time-invariant state factors. In this regard, Ruhm (1996) and Mast et al. (1998) found large differences between models estimated with and without state dummy variables, and Sloan et al. (1994a, 1994b) and Evans et al. (1991) also used fixed-effects models. Saffer and Grossman [(1987a), p. 369] estimated a model with state fixed effects, however, concluding that their results "suggest a model with state dummies is overdetermined and plagued by multicollinearity." Chaloupka et al. [(1993), p. 172] noted that they tried a fixed-effects model, but "collinearity made it impossible to obtain meaningful results." This would be the case if one or more of the laws did not change much over time. Fixed-effects models bias coefficients toward zero, of course, making it more difficult to find "meaningful results" in the form of significant coefficients. However, if a Hausman (1978) test indicates that the fixed-effects model is preferred over the OLS model, and if some coefficients change sign and/or become significant when controls for fixed effects are added, then the fixed-effects version of the model must be preferred. Indeed, changes in the sign of coefficients, particularly if the coefficients are significant in each case or if the coefficient becomes significant in the fixed-effects specification, suggests that the OLS estimate is unreliable. Therefore, even if a coefficient is significant in an OLS equation, it is not a "meaningful result" when fixed effects should have been controlled for. In the following analysis, Hausman tests support the use of fixed-effects, and some coefficients do change sign and significance, so only the fixed-effects version is presented even though relatively few of the deterrence variables seem to be significant by themselves.

<sup>2</sup>A possible exception to this claim is Wilkinson (1987), but that study did not consider fixed effects, and the specification of some deterrence variables may be problematic, as noted below.

<sup>3</sup>For literature reviews reaching this conclusion, see Elliot (1977), and Rasmussen and Benson (1994). Critics have questioned the efficacy of this literature because of many inherent data limitations [see, for example, Brier and Fienberg (1980)], but the deterrence hypothesis has been confirmed in studies not suffering from the spurious correlation problems that plague much of the literature [Myers (1980); Craig (1987); Benson et al. (1992, 1998)]. Recent innovations in the study of deterrence using fixed-effects models also support the conclusion that the

of reported crimes to arrests for those crimes, or by actual clearance rates). Nonetheless, data measuring a number of factors that should determine the probability of detection are available, as explained below. In this context, the fact that individual variables that are expected to influence the probability (or severity) of punishment do not have significant coefficients in reduced form models (or that findings of significance in one study are not reproduced in another) do not necessarily imply that direct deterrence is ineffective. Even if individual laws that establish punishment or facilitate making DUI arrests do not seem to be significant deterrents, the overall effect of a state's DUI policy as reflected by its package of DUI laws and law enforcement efforts may have a very significant impact on drinking and driving behavior. This possibility is suggested by two recent studies. Chaloupka and Wechsler (1996) used an index developed by Mothers Against Drunk Driving (MADD) to reflect the restrictiveness of each state's drunk-driving laws targeting youths and young adults as their deterrence variable, and concluded that strong state policies "significantly reduce all measures of drinking in both specifications for the underage and older college student samples." Although the effect of such laws on females was less significant, they concluded that "increasing the probability of arrest, easing the standards for arrest and conviction, and raising the penalties upon conviction for youth and young adult driving under the influence will reduce both drinking and binge drinking" [Chaloupka and Wechsler (1996), p. 121]. Similarly, Evans et al. [(1991), p. 279] reported "no conclusive evidence that any specific form of punitive legislation is having a measurable effect" on DUI behavior as measured by various traffic fatality variables, but they found that states with both laws allowing sobriety checkpoints and preliminary-breath-test laws had 24% (22%) fewer single-vehicle occupant night-time fatalities (single-vehicle occupant fatalities), again suggesting that a combination of deterrence variables matters rather than a single law or law enforcement effort. This issue is a primary focus of the following presentation.

The paper is organized as follows. First, a brief review of the DUI deterrence literature is provided in Section II, focusing on the evidence regarding direct deterrence through the probability and/or severity of punishment. Then in Section III, a model of driver involvement in alcohol-related traffic fatalities is introduced, and the data used to test this model are discussed. Empirical results are presented in Section IV. These results imply that policies that focus on direct deterrence can be effective. In particular, as a group, factors that increase the probability of being stopped and/or arrested for DUI significantly affect drinking and driving behavior. Concluding comments appear in Section V.

## II. Evidence of Direct DUI Deterrence

A number of studies have explored the relative effectiveness of policies intended to reduce DUI, typically by considering their effect on some measure of vehicle mortality rates. Raising the legal drinking age from 18 to 21 years old is generally shown to significantly reduce automobile deaths, but this offers little guidance for public policy because all states have moved to the age 21 standard. Most studies have found that higher beer taxes reduce vehicle mortality rates, and, until recently, the strongest conclusion from the DUI literature seemed to be that increasing beer taxes is the most

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probability of apprehension is a stronger deterrent than the severity of punishment [Cornwell and Trumbull (1994); Levitt (1998); Benson et al. (1998)].

effective way to reduce motor vehicle fatalities, but this conclusion is being challenged.<sup>4</sup> Despite the focus on alcohol taxes and the widely held conclusion that non-tax deterrence efforts are relatively ineffective, there actually is quite a lot of evidence that some state laws establishing sanctions and/or facilitating arrests or prosecution may significantly reduce DUI.<sup>5</sup> The actual laws considered as potential sources of deterrence have varied considerably from study to study, however. As an illustration, a list of deterrence variables contained in Ruhm (1996), Chaloupka et al. (1993), Evans et al. (1991), and Saffer and Grossman (1987a, 1987b) is presented in Table 1, where a plus sign indicates the variables included in the regression model. Findings concerning the effect of individual anti-drunk driving policies on fatality rates are also varied.

Ruhm's (1996) results provided no support for the hypothesis that preliminary-breath-test laws, laws allowing for the automatic suspension of a driver's license at the time of a DUI arrest (administrative *per se* laws), laws mandating that driver's license holders agree to alcohol and drug tests on request or their licenses are suspended (implied-consent laws), or mandatory jail terms significantly deter traffic fatalities, for instance, but Sloan et al. (1995) suggested that minimum fines and jail terms deter binge drinking, which can lead to drunk driving (these laws and sanctions are described in more detail in Section III below where data for this study are discussed). Similarly, in Mullahay and Sindelar (1994) minimum fines and license suspensions had significant negative impacts on self-reported drunk driving, and Sloan and Githens (1994) estimated that mandatory jail terms reduced drinking and driving. Chaloupka et al. (1993) concluded that mandatory administrative license suspensions, preliminary-breath-test laws, and relatively high minimum fines, no-plea-bargaining laws, and minimum license suspensions for DUI convictions reduced fatalities, although mandatory jail sentences, community service laws, and open-container laws had no deterrent effect. Kenkel (1993, 1996) found that mandatory jail terms, administrative license sanctions, preliminary breath tests, laws allowing sobriety check points, and no-plea-bargaining laws deter self-reported drunk driving.

Given the differences in data (e.g., microsurvey data versus state-level aggregates),

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<sup>4</sup>Indeed, the DUI literature has focused on the indirect effect of general alcohol taxation to "deter" drunk driving, rather than the use of more direct law enforcement efforts and punishment that are the focus of the economics of crime literature that has developed from Becker's path-breaking work. Committing a DUI offense requires consumption of alcohol, of course, which is generally assumed to be a normal good for which an increase in price diminishes consumption. Raising the price is a "deterrent" to the consumption of all goods, so econometric studies have attempted to include some measure of, or proxy for, alcohol price as a right-hand side variable. Often, this proxy variable has been the excise tax on beer, perhaps because it is a direct policy instrument. This focus on taxation rather than direct deterrence through law enforcement and penalties may be unfortunate, however. Virtually all econometric studies were consistently concluding that the most effective deterrent to drunk driving, particularly among youth, is to raise the price of beer by imposing higher taxes on beer [Chaloupka et al. (1993); Evans et al. (1991); Saffer and Grossman (1987a, 1987b); Laixuthai and Chaloupka (1993); Phelps (1988)], but recent research using new and/or updated sources of data and/or controlling for other factors (e.g., alcohol sentiment, state fixed-effects, non-tax determinants of alcohol price, and availability) cast considerable doubt on the robustness of these results [e.g., see Laixuthai and Chaloupka (1993); Kenkel (1996); Mast et al. (1998); Chaloupka and Wechsler (1996)]. Yet, perhaps because of this focus on taxation, relatively little has been learned about the potential efficacy of direct deterrence through law enforcement.

<sup>5</sup>Several recent studies have also considered the potential role of civil law as a DUI deterrent. Ruhm (1996) found that dram-shop laws making liquor establishments that sold the drinks to a drunk driver liable for damages caused by the driver had a significant negative impact on total and night-time fatalities, for instance, but the impact of this law was quite sensitive to model specification. Robust evidence was provided by Sloan et al. (1994a, 1994b) and Mast et al. (1998) that dram-shop liability reduces vehicle mortality, however, although Sloan et al. (1995) found no evidence that dram-shop laws reduced self-reported drunk driving.

TABLE 1. Sampling of deterrence variables in past studies

<i>Deterrence Variables</i>	<i>Ruhm*</i>	<i>Chaloupka et al. †</i>	<i>Evans et al. ‡</i>	<i>Saffer and Grossman §</i>
<i>Administrative per se laws</i>	+	+	+	
<i>Beer legal drinking age</i>	+	+		+
<i>Community service in lieu of jail for 1st conviction</i>		+		
<i>Dram-shop laws</i>	+	+		
<i>Fine for 1st conviction</i>		+		
<i>Illegal per se laws</i>		+	+	
<i>Implied-consent laws</i>	+	+		
<i>Jail for 1st conviction</i>	+	+	+	
<i>No-plea-bargain laws</i>		+	+	
<i>Open-container laws</i>		+	+	
<i>Preliminary-breath-test laws</i>	+	+	+	
<i>Real beer tax</i>	+	+	+	+
<i>Sobriety checkpoints</i>			+	
<i>Suspension for 1st conviction</i>		+		

\*Ruhm (1996) for 1982–1988.

†Chaloupka et al. (1993) for 1982–1988.

‡Evans et al. (1991) for 1975–1986.

§Saffer and Grossman (1987a, 1987b) for 1975–1981.

sample sizes, and periods, as well as in the variety of fatality measures to proxy drunk driving and deterrence variables and in the estimation methods employed in the studies cited above, one should be cautious when comparing results. Furthermore, measurement of law enforcement effort and/or determinants of that effort are conspicuously missing from many studies of drunk-driving deterrence, making questionable any conclusions regarding drunk driving deterrence. There are a few potentially important exceptions, however. Wilkinson (1987) included numerous variables intended to measure differences in law enforcement across states during his 1976 to 1980 study period. Probability of arrest was measured by the number of DUI arrests per vehicle mile. This may not be a good measure, however. After all, this proxy for probability of arrest would only equal DUI arrests per vehicle mile if the rate of drunk driving per vehicle mile is the same in all states. The probability of conviction given arrest was calculated from disposition data for about 10% of the population obtained from the Federal Bureau of Uniform Crime Reporting System. Data were only available for 1976 to 1977. It was assumed that jurisdictions not reporting data did not systematically differ from reporting jurisdictions. For reporting jurisdictions, the percentages of arrestees found guilty or pleading guilty to DUI, found guilty of a lesser offense, or acquitted were calculated. The calculated percentages were regressed against the following variables: district attorney staff *per capita*, law enforcement employment *per capita*, registered vehicles, vehicle miles traveled, average speed and standard deviation, percentage of young drivers, alcohol consumption, and number of on- and off-premise alcohol outlets. Results were used to compute probabilities of DUI conviction, conviction on a lesser charge, and acquittal for other states and years. Despite this relatively extensive examination, however, Wilkinson [(1987), p. 330] found that “drunken driving deterrence policies appear to have no marginal effect on the demand for alcohol or the level of fatalities.”

More recently, Sloan et al. (1995, 1994a, 1994b) and Mast et al. (1998) controlled for police employment rates in their studies of drinking and driving, and Sloan et al. (1995) suggested that police employment may reduce binge drinking that leads to the results of drunk driving. The findings of Sloan et al. (1994a, 1994b) also implied that police employment might reduce state vehicle fatality rates, but these results were sensitive to the inclusion of time dummies in their models, and Mast et al. (1998) did not find a significant relationship between police employment and alcohol-related fatalities when controls for state fixed-effects were added to their model (the coefficient was significant without these controls, however). Police expenditures were also examined in Brown et al. (1996), and as a substitute for police employment in Sloan et al. (1994a). Brown et al. (1996) found no significant relationship between fatalities per mile and police expenditures, but Sloan et al. (1994a) estimated that police expenditures had a negative effect on vehicle mortality. As noted above, Kenkel (1993, 1996) included a dummy variable for states that had laws authorizing police to use sobriety checkpoints, as did Evans et al. (1991), expecting such laws to increase the probability of DUI arrests. Binary variables cannot account for interstate differences in enforcement intensity, however, and Chaloupka et al. (1993) rejected including sobriety checkpoints in their study for this reason, as well as the fact that checkpoints are used to some degree in all states whether such a law exists or not [Evans et al. (1991) acknowledged that many states use sobriety checkpoints as part of routine safety checks]. Nonetheless, Kenkel (1993, 1996) concluded that laws authorizing sobriety checkpoints had a negative impact on heavy drinking leading to drunk driving, although the magnitude and significance of the estimates varied with gender and age, whereas Evans et al. (1991) found that these laws were not significant deterrents (although, as noted above, the presence of these laws in conjunction with preliminary-breath test laws did seem to reduce fatalities). Finally, in contrast to previous studies, Mast et al. (1998) found that open-container laws, which are expected to increase the probability of being stopped and/or arrested, reduced the number of drivers in alcohol-related fatalities using a dependent variable that was a direct measure of drunk driver involvement in fatal accidents (as opposed to some aggregate fatality rate).

As noted above, Chaloupka and Wechsler's (1996) deterrence results may be as strong as any. The MADD index reflecting the restrictiveness of each state's drunk driving laws targeting youths and young adults was significantly and negatively related to all of their measures of drinking in their underage and older college student samples. This index considered factors that increase the probability of arrest, ease standards for arrest and conviction, and raise penalties given conviction, so whether some subset of these factors are more important than others cannot be determined. Instead, these findings imply that the overall effect of a state's DUI laws and law enforcement efforts can have a significant deterrence impact. To find out whether particular aspects of deterrence policy are more influential than others, a model of DUI deterrence will be constructed and tested.

### **III. Modeling Driver Involvement in Traffic Fatalities**

Intensity of alcohol-related traffic deaths in a state can be measured by the driver involvement rate,  $R$ , which equals the number of drivers involved in alcohol-related fatalities each year, divided by the total number of drivers in the state.  $R$  is modeled as a function of drunk driving offenses,  $D$ , and a vector  $T$  of measures of traffic, vehicle safety and driver safety:

$$R = f(D, T). \quad (1)$$

Drunk driving and the amount of traffic should have a positive effect on  $R$ , whereas vehicle safety and driver safety should have negative influences on  $R$ . Drunk driving is modeled as a function of alcohol consumption,  $Q$ , the expected punishment from drunk driving,  $E$ , a vector  $N$  of variables controlling for laws that may impact the likelihood of drinking and driving behavior but are not direct DUI deterrents, and a vector measuring general social attitudes toward alcohol use,  $SA$ :

$$D = g(Q, E, N, SA). \quad (2)$$

$Q$  should have positive effects on  $D$ , whereas  $N$  and  $E$  should be negatively related to  $D$ .  $SA$  variables can be positively or negatively related to  $D$ , depending on the attitudes that they capture. Expected punishment is positively related to the probability of being stopped,  $P(S)$ , the probability of being arrested given being stopped,  $P(A|S)$ , the probability of being convicted given arrest,  $P(C|A)$ , the expected severity of punishment given conviction  $E(SV|C)$ , and, conceivably, the expected severity of punishment even if not convicted,  $E(SV|NC)$ , inasmuch as some sanctions can be imposed automatically on arrest, as explained below. Therefore, expected punishment is:

$$E = f[P(S), P(A|S), P(C|A), E(SV|C), E(SV|NC)]. \quad (3)$$

Because there is no state-level direct measure of a drunk driving offense rate, the driver involvement equation must be estimated in reduced form by substituting (2) and (3) into (1):

$$R = f(Q, P(S), P(A|S), P(C|A), E(SV|C), E(SV|NC), N, SA, T). \quad (4)$$

It is hypothesized that deterrence works; that is, as  $P(S)$ ,  $P(A|S)$ ,  $P(C|A)$ ,  $E(SV|C)$ , and/or  $E(SV|NC)$  rise, the driver involvement rate should fall, and *vice versa*.  $Q$  should be positively related to  $R$ , as should the amount of traffic, but car and driver safety should be negatively related to  $R$ .

#### *Dependent Variable (R)*

Most empirical studies of drunk driving deterrence have used proxy variables such as fatality rates, night-time fatalities, and fatalities in accidents involving youth as dependent variables because these proxies are expected to be correlated with drunk driving.<sup>6</sup>

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<sup>6</sup>For instance, several studies [e.g., Saffer and Grossman (1987a, 1987b); McCormac (1982); Cook and Tauchen (1984)] focused on youthful drivers because automobile accidents are a leading cause of death of persons under 35 years of age, and alcohol has been involved in over half of these fatal accidents. Studies using only young drivers will not give accurate estimates of possible lives that can be saved from various policies, however, as older drivers are killed in alcohol-involved accidents as well. The National Highway Traffic Safety Administration (NHTSA) reported that in 1992, an estimated 73.8% of night-time single-vehicle driver fatalities were alcohol related, so this is also an attractive proxy variable [e.g., Ruhm (1996)]. In contrast, 12.1% of fatally injured drivers in multiple-vehicle daytime crashes had BACs of 0.01 or greater in 1992. Measures that correspond highly with alcohol involvement, such as night-time single-vehicle occupant fatalities, will not result in accurate estimates of total possible lives saved from certain policies, however, because alcohol-related deaths occur to some degree for multivehicle and daytime fatalities as well. Evans et al. (1991) noted that such measures only result in lower bound estimates of the total lifesaving potential of different policies. Using total fatalities could conceptually give the best estimate of total lifesaving potential of anti-drunk driving measures because alcohol is involved to some degree in all categories of fatal motor vehicle accidents, but, because total fatalities have a smaller rate of alcohol involvement, "it may be impossible to distinguish small changes in drunk driving

However, these proxies create varying degrees of statistical problems because of a low signal-to-noise ratio [see Evans et al. (1991), Mast (1996), and Mast et al. (1998) for discussion of alternative measures]. A better measure would explicitly account for the extent to which alcohol is actually involved in fatal accidents as determined by the BAC of drivers. Such a measure actually exists for some states. In particular, 15 states consistently tested fatally injured drivers' BAC from 1980 to 1985, and since then such testing has increased. By 1990, however, BAC test results were still missing for 27% of dead drivers nationally, as well as for 75% of surviving drivers involved in traffic fatalities. To overcome this deficiency in the data, the National Highway Traffic Safety Administration (NHTSA) introduced a methodology using discriminant analysis to estimate BAC values for drivers and nonmotorists for whom test results were lacking [Klein (1986)].<sup>7</sup> Therefore, two measures of driver involvement in alcohol-related traffic fatalities are employed here using data provided by the NHTSA on the BAC of drivers in fatal accidents, including estimated values when BAC is missing for an accident.<sup>8</sup> Because the NHTSA defines accidents as alcohol-related if a driver involved had BAC of 0.01 or above, this measure is considered. However, only drivers with a BAC equal to 0.10 or higher are considered legally drunk in most states, so a second set of estimates employ this measure. Driver involvement rates per 1000 drivers are computed by dividing the number of drivers involved in each BAC category by the total numbers of drivers in each state:  $R = 1000 * (\text{drivers involved}) / (\text{total drivers})$ .<sup>9</sup> The dependent variable is a logistic transformation of  $R = \text{Lgt}(R) = \ln[R / (1 - R)]$  because each driver involvement rate lies within the unit interval, and, as suggested by Maddala (1983), weighted least squares is employed with weights  $[\text{pop} * R(1 - R)]$ .<sup>5</sup>

The accuracy of predicted variables is always of concern, of course. Validation tests suggest that the predicted values of driver involvement with alcohol in traffic fatalities are quite accurate, however. Klein (1986) performed validation tests using cases with known BAC to test results from the 1984 and 1985 Fatal Accident Reporting System (FARS). These tests predicted the percent of drivers involved in fatal accidents in 1985 with known BAC = 0.01 to 0.09 to be 0.12, for instance, whereas the actual value was 0.13. Similarly, both the actual and predicted percentages of drivers in 1985 with BACs of 0.10 or higher was 0.45. Other validation tests resulted in similar degrees of accuracy

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from the year-to-year fluctuations in the aggregate data" [Evans et al. (1991), p. 283]. Wilkinson (1987) used the total fatality rate for the population 15 years old and above in his study of drunk driving deterrence, Evans et al. (1991) derived estimates using "alcohol-sensitive," "inclusive," and "alcohol-insensitive" measures of traffic fatalities to determine the sensitivity of their results to different dependent variables, and several other studies also have used multiple measures [e.g., Ruhm (1996); Chaloupka et al. (1993); Mast et al. (1998)].

<sup>7</sup>Klein [(1986), p. i] explains that this analysis involves the formation of linear combinations of variables correlated with alcohol involvement in drivers and nonoccupants (e.g., police-reported alcohol involvement, accident hour, age, vehicle role, injury severity, weekday/weekend, use of occupant restraint, driver license status, number of entries on driver record, sex, location of nonoccupant in relation to roadway, and whether or not the driver could legally drink given the minimum drinking age in the accident state), to estimate posterior BAC distributions on the attributes of various persons, vehicles, and accidents.

<sup>8</sup>These data are also used in Mast (1996) and Mast et al. (1998) to study the impact of alcohol taxes on alcohol consumption and, through consumption, on DUI fatalities (models were estimated using data for all 50 states, and the results were almost identical). Mast et al. (1996) also used a total vehicle fatality rate per 1000 population 16 years of age and older, and their general findings were not dramatically different with the two dependent variables. The coefficients using driver-involvement rates are presumably better estimates of policy effects on DUI-related fatalities, however.

<sup>9</sup>Driver-involvement rates were multiplied by 1000 to reduce scaling problems when computing weights (1000 was the largest multiple of 10 that the driver-involvement rates could be multiplied by while keeping rates within the unit interval so that the logistic transformation could be used).

[Klein (1986)]. A direct test of the validity of the results reported below would be to estimate the same regressions using only those data that are actually observed rather than the combination of observed and estimated data. Unfortunately, the data provided by the NHTSA do not allow us to separate observed and estimated variables. Therefore, as an additional check, an alternative dependent variable is also considered: the total vehicle fatality rate per 1000 population [the total number of vehicle deaths in each state divided by the number of people in the population 16 years old and above:  $F = (\text{fatalities}) / (\text{population} > 15 \text{ years and } 364 \text{ days old})$ ]. This total fatality rate in each state also lies within the unit interval, so the equation is estimated using the minimum chi-square method, with the dependent variable being a logistic transformation of  $F = \ln[F/(1 - F)]$ , and weighted least squares.<sup>10</sup> It would be surprising if the resulting estimates suggest that all of the same variables are significant because this measure includes a large number of fatalities that are not alcohol related (and the literature typically finds that different variables seem significant with different fatality measures, as suggested above), but if the same groups of variables (e.g., those determining the probability of arrest or the severity of punishment) are significant, then the general conclusions drawn from the driver-involvement-rate equations will be supported.

A panel of annual data is employed for the 48 contiguous states for 1984 to 1992. Rates of alcohol involvement in traffic fatalities fluctuated from year to year from 1984 to 1988. Starting in 1989, however, these rates declined for 3 of 4 years for all age groups and for both BAC categories. Overall, driver involvement rates declined between 1984 and 1992 by about 30%.

Now, let us turn to the data employed to represent the independent variables in equation (4). Descriptive statistics for these data are provided in Table 2.

#### *Alcohol Consumption (Q)*

The number of gallons of ethanol shipped to a state divided by the number of people in the population 18 years old and above is employed as measures of state alcohol consumption. *Ethanol per capita* is actually computed from data on shipments of beer, liquor, and wine, using 0.046 alcohol content for beer, 0.40 for liquor, and 0.11 for wine.<sup>11</sup> The driver involvement rate should be positively related to alcohol consumption.<sup>12</sup>

<sup>10</sup>Another specification with the night-time fatality rate as a dependent variable was also considered, but the results were generally consistent with those reported below for the total fatality equation, so they are not discussed here.

<sup>11</sup>These values were provided by the Distilled Spirits Council of the United States. Quantity data were taken from the *Brewers Almanac* [U.S. Brewers Association (1984–1992)], and population data were provided by the Bureau of Census.

<sup>12</sup>Note that many factors that determine the demand and supply of alcohol influence alcohol consumption, including a number of policy variables directed at alcohol prices (e.g., alcohol taxes) and availability (e.g., mandated exclusive territories for beer distributors). Therefore, these factors and policies have indirect impacts on DUI behavior. Some determinants of the variable employed to represent consumption, such as taxes in border states and the level of tourism, mean that this variable is not simply a measure of consumption by potential drivers in the state, of course, so the coefficient on this variable must be interpreted with caution. The focus of this paper is on direct deterrence, however, so policies that influence the variable, such as the state's alcohol taxes, the relative level of taxes in neighboring states, forced deposit laws, mandated exclusive territories, and cash laws, are not considered. Mast et al. (1998) develop a recursive model of DUI behavior with an alcohol consumption equation and a fatality equation much like the one employed here (but without the full array of direct deterrence variables) to explore the relationship between DUI behavior and these indirect factors. A reduced-form equation could replace  $Q$  with its determinants, of course, but there are so many factors that might reasonably be hypothesized to affect traffic fatalities (indirect factors determining the supply and demand of alcohol in a state, including taxes and laws intended to affect alcohol availability

TABLE 2. Descriptive statistics

<i>Variables</i>	<i>Mean</i>	<i>Standard Deviation</i>	<i>Minimum</i>	<i>Maximum</i>
<i>Administrative per se laws</i>	20.116	46.182	0	365
<i>Anticonsumption laws</i>	0.6713	0.47029	0	1
<i>Seat-belt laws</i>	0.49207	0.48434	0	1
<i>Catholics</i>	0.19041	0.13458	0.01893	0.6394
<i>Police per capita</i>	193.83	39.996	125	321
<i>Dram-shop laws</i>	0.64815	0.4781	0	1
<i>Dry-county population</i>	0.024493	0.07853	0	0.4636
<i>Ethanol per capita</i>	2.6479	0.61637	1.506	5.324
<i>Fines for 1st conviction</i>	92.146	155.95	0	1018
<i>Fines for 2nd conviction</i>	203.3	332.75	0	1448
<i>Illegal per se laws</i>	0.88194	0.32305	0	1
<i>Implied-consent laws</i>	140.16	129.94	0	365
<i>Per capita disposable income</i>	16038	1900	11310	21460
<i>Jail for 1st conviction</i>	0.40394	0.7333	0	3
<i>Jail for 2nd conviction</i>	11.699	27.667	0	180
<i>Legal drinking age</i>	0.090475	0.22763	0	1
<i>Males 16–44 per capita</i>	0.29794	0.014844	0.2601	0.3349
<i>Mormans</i>	0.03088	0.10527	0.001253	0.7149
<i>No-plea-bargaining laws</i>	0.15972	0.36677	0	1
<i>Open-container laws</i>	0.39583	0.4896	0	1
<i>Preliminary-breath-test law</i>	0.49074	0.50049	0	1
<i>Metropolitan population</i>	64.946	21.947	18.9	100
<i>Other Protestants</i>	0.23531	0.099832	0.02902	0.4798
<i>Southern Baptists</i>	0.076681	0.10434	0.0008695	0.341
<i>Suspension for 1st conviction</i>	36.396	63.623	0	365
<i>Suspension for 2nd conviction</i>	362.14	329.65	0	1825
<i>Unemployment rate</i>	6.2896	1.9097	2.2	15
<i>Vehicle miles per driver</i>	12,548	2,732.8	8,653	18,920

*Traffic, Safety, and Related Economic Variables (T and/or N)*

*Vehicle miles traveled per driver*,<sup>13</sup> and the fraction of days during the year the state had a mandatory *seat belt law*<sup>14</sup> are employed as measures of traffic density and of driver and car safety. Coefficients on the vehicle miles traveled per driver and seat belt law variables should be positive and negative respectively. Males 16 to 44 years old as a fraction of the population (*males 16–44 per capita*) is included to capture the driving age population most likely to drink and drive. It should be positively related to *R*.

Although income is likely to be positively related to car ownership and driving, it

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and prices, and related laws and taxes in neighboring states, direct deterrence factors, such as laws intended to affect driving, and DUI and law enforcement efforts, traffic conditions, socioeconomic characteristics, including attitudes toward drinking) that a reduced-form model attempting to control for them all would be both unmanageable and uninterpretable (substantially, collinearity between policy variables is likely after all). Therefore, *Q* should be interpreted as a control for all of the factors that influence shipments to a state rather than simply as a measure of consumption.

<sup>13</sup>Data on vehicle miles per state are from *Highway Statistics* [U.S. Department of Transportation (1984–1992)]. Data on drivers were provided by the Federal Highway Administration.

<sup>14</sup>The NHTSA provided this information.

probably is also positively correlated with car safety and with the opportunity cost of drunk driving (e.g., the possibility of being apprehended, injured, or killed). Therefore, real cost-of-living (COL) adjusted *per capita disposable income*<sup>15</sup> is used as an explanatory variable, but no *a priori* signs can be predicted for its coefficient. As with income, the impact of the *unemployment rate* on *R* is not certain.<sup>16</sup> Unemployed people presumably have more leisure time to spend drinking and driving, but they also have less income, so if drinking and driving are both normal goods, they could drink and drive less.

The percentage of the state population residing in metropolitan areas (*metropolitan population*) is also included as a control variable, but no *a priori* prediction is made for its sign either.<sup>17</sup> Metropolitan areas may have greater alcohol availability, but people in urban areas may also drive less because of access to buses, subways, and taxis, as well as to bicycles and walking due to the relatively short distances traveled.

#### *Alcohol Control (N)*

The Federal Alcohol Traffic Safety Programs of 1983 provided incentives for states to adopt and enforce more stringent drunk driving laws. Between 1981 and 1986, the states passed more than 729 laws aimed at deterring drunk driving [Evans et al. (1991), p. 280]. Similarly, the Federal Uniform Drinking Age Act of 1984 provided incentives for states to raise their legal drinking age; states that did not comply with a uniform legal drinking age of 21 could be denied a portion of their federal highway funds. Thus, states that allowed younger drinkers to legally consume before 1984 tended to respond during the early part of our data period by raising the legal drinking age to 21.<sup>18</sup> By the end of 1988, all states had legal drinking ages of 21. Holding *per capita* alcohol consumption constant, the *legal drinking age* may have an independent effect on *R* for various reasons. Because no state-level data on age-specific alcohol consumption are available, this variable might pick up aspects of the age distribution of alcohol consumption. Also, holding alcohol consumption constant for those affected by the minimum drinking age, the minimum drinking age might change aspects of drinking behavior such as location and intensity. Asch and Levy (1990) provide evidence that higher legal drinking ages increase fatality rates for drivers above the legal age by decreasing their drinking experience at younger ages. The *a priori* sign of the coefficient of legal drinking age on drivers ages 18 to 20 years old is negative, but for other age

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<sup>15</sup>Data on *per capita* disposable income are taken from *Statistical Abstract of the United States* [U.S. Bureau of the Census (1983–1995)]. Consumer price indices (CPIs) are from *The Economic Report of the President* (various years). Cost-of-living (COLs) indices are taken from McMahon and Chang (1991), and the COL index for 1990 was used to adjust data for 1991 to 1992. Dumond et al. (forthcoming) provide evidence that the COL indexes based on ACCRA price data overestimate differences in wages across metropolitan areas due to COL by about 60% due to differences in demand across these areas. Therefore, per capita income was adjusted by 40% of COL.

<sup>16</sup>Data were taken from the *Statistical Abstract of the United States* [U.S. Bureau of the Census (1983–1995)]. The poverty rate for each state was also included in some preliminary regressions; its effect was never significant and did not affect other coefficients or significance levels.

<sup>17</sup>These data are taken from the *Statistical Abstract of the United States* [U.S. Bureau of the Census (1983–1995)]. During the sample period of 1984 to 1992, changes took place in the Census Bureau's names for metropolitan areas (Standard Metropolitan Statistical Areas, Metropolitan Statistical Areas, and finally Metropolitan Areas), but no changes occurred in their definition of a metropolitan area. Population density was also considered in some equations as a substitute for or in addition to the metropolitan population variable, but with no effect on the results.

<sup>18</sup>Data on the legal age for drinking beer with 3.2% or greater alcohol content were provided by Tim R. Sass and David S. Saurman who had collected it for Sass and Saurman (1993). Population data are from the Bureau of the Census.

categories the *a priori* sign is not known so for the population as a whole this sign cannot be predicted.

*Dram-shop laws* allow persons injured by intoxicated parties to take legal action against the person or establishment serving the alcohol. Liability is made possible by legislation or case law. These laws increase the probability that a person serving alcohol to a drunk driver will be punished, so it is not a direct deterrent of DUI. The control effect results from lower expected levels of excessive drinking by people who might drive. As of January 1, 1992, 42 states had dram-shop liability.<sup>19</sup> A dummy variable is included to represent these states.

#### *Attitudes Toward Drinking (SA)*

To capture attitudes toward drinking, the fraction of the population living in counties dry for beer (*dry-county population*) is included.<sup>20</sup> Although residents of dry counties might drink less than people in wet counties, when they do drink they would be expected to drive more. For this reason, the coefficient on this variable has no *a priori* predicted sign. Religious participation is also considered by including the fraction of the population who are *Catholics, Mormons, Southern Baptists*, and members of Protestants denominations other than Mormon or Southern Baptist (*Other Protestants*).<sup>21</sup> Although Catholics do not prohibit alcohol use, Mormons and Southern Baptists explicitly forbid its consumption, and the beliefs of other Protestants about alcohol vary. Thus, it might seem reasonable to assume *a priori* that the signs for the coefficients for the Mormon and Southern Baptist variables will be negative, but the evidence of Ornstein and Hanssens (1985) suggests that both groups (as well as Protestant and Catholic church membership) apparently are associated with increasing beer consumption and decreasing wine and liquor consumption. The net impact of this substitution on drinking and driving behavior may be positive or negative, so no *a priori* predictions are made for the signs of these coefficients.

#### *DUI Deterrence (E)*

All deterrent variables are predicted to have a negative effect on  $R$ .<sup>22</sup>

*Probability of being stopped (P(S)).* *Open-container laws* make it illegal to have an open container of alcohol in an automobile's passenger compartment. This law could increase the probability of being stopped,  $P(S)$ , while drinking and driving if a driver is viewed by police while drinking; it could also increase the probability of arrest once stopped,  $P(A|S)$ , for drivers who are not legally intoxicated but have open containers in their cars (such laws could also increase the probability of conviction after an arrest,

<sup>19</sup>Information regarding dram-shop laws comes from the *Digest of State Alcohol-Highway Safety Related Legislation* [U.S. Department of Transportation (1985–1993)].

<sup>20</sup>These data are from the *Brewers' Almanac* [U.S. Brewers Association (1984–1992)].

<sup>21</sup>Data for 1984 to 1989 and 1991 to 1992 were interpolated/extrapolated from data for 1980 and 1990. Data on church membership for 1980 taken from Quinn et al. (1980), and 1990 data are from Bradley et al. (1990). Data on state population were provided by the U.S. Bureau of the Census (1984–1992).

<sup>22</sup>Note that sobriety checkpoint laws were not considered as a deterrent variable for reasons suggested by Chaloupka et al. (1993) and discussed above. Information regarding all drunk driving laws examined below comes from the *Digest of State Alcohol-Highway Safety Related Legislation* [U.S. Department of Transportation (1985–1993)]. These variables were only coded as being in effect in each state if the laws seemed to be binding. A detailed explanation on the coding of each drunk driving law is available on request. Data on police employment is from *Public Employment* (1986–1992).

$P(C|A)$ , and the severity of punishment if drivers are convicted of DUI and open-container violations and as a result receive harsher sentences). By January 1, 1992, 26 states had open-container laws. A dummy variable controls for the presence of such laws. *Anticonsumption laws* ban consumption of alcoholic beverages in automobiles. These laws could deter drunk driving in a manner similar to open-container laws, so a dummy variable is also used to represent the existence of such laws. By January 1, 1992, 37 states had anticonsumption laws. Police manpower is represented by the number of sworn police officers per 100,000 population (*police per capita*) for each state. More police patrolling should increase the probability of a drunk driver being observed and stopped.

*Probability of arrest given that an offender is stopped ( $P(A|S)$ ) and/or the probability of being convicted given arrest ( $P(C|A)$ ).* *Preliminary-breath-test laws* allow police to administer breath tests without medical supervision. The results are used as probable cause for DUI arrest. This law could increase the probability of being arrested once stopped,  $P(A|S)$ , as well as the probability of being convicted given arrest,  $P(C|A)$ . As of January 1, 1992, 26 states had preliminary-breath-test laws, and are represented with a dummy variable in the regression analysis. *Implied consent laws* presume driver's license holders agree to alcohol and drug tests on request, or their licenses are suspended. This law could also increase  $P(A|S)$  and  $P(C|A)$ . As of January 1, 1992, 43 states had implied-consent laws for first offenses. The suspensions last from 30 days to a year. The minimum license suspension pursuant to implied consent laws for drivers who refuse breath tests is the variable that is used below to control for such laws. *Illegal per se laws* make it a crime to drive with a BAC at or above some predetermined level. Under these laws, prosecutors do not have to show that the driver was actually impaired to get DUI convictions. This law also could increase  $P(A|S)$  and  $P(C|A)$ . As of January 1, 1992, 36 (10) states had illegal per se laws with BAC levels of .10 (.08). A dummy variable represents states with such laws.<sup>23</sup>

*Expected severity of punishment ( $E(SV|C)$  and  $E(SV|NC)$ ).* *Administrative per se laws* allow for the automatic suspension of a driver's license at the time of a DUI arrest, thus increasing the certainty of punishment for DUI arrest whether the person is convicted or not. To measure the expected punishment when a person is arrested but not convicted, the minimum license suspension pursuant to administrative per se laws is included (23 states had mandatory administrative license suspensions as of January 1, 1992 with license sanctions lasting from 10 to 180 days). *No-plea-bargaining laws* require persons arrested for DUI to be tried for DUI, unless there is clearly insufficient evidence for conviction. As of January 1, 1992, 10 states had no-plea-bargaining laws. Presumably, this increases the likelihood of conviction for DUI rather than for some lesser offense through a plea bargain.

Laws establishing minimum penalties for first offenses include fines, license suspensions, and jail sentences. As of January 1, 1992, 23 states had mandatory fines of \$100 to \$500 dollars after a first DUI conviction (*fine for 1st conviction*—the minimum fines are employed in the regressions, and they are adjusted for COL differences), 27 states suspended licenses for 15 days to a year (*suspension for 1st conviction*—the minimum mandated license suspension) for a first offense, and 16 states required a jail sentence

<sup>23</sup>Differences in illegal *per se* BAC levels were also considered by a model including dummies for states with illegal *per se* BAC levels above, below, and equal to 0.10, but this did not change the results.

of 1 to 3 days (*jail for 1st conviction*—the minimum jail term required).<sup>24</sup> Several states also have laws establishing relatively severe minimum penalties for conviction on a second DUI offense, including fines (*fine for 2nd conviction*), license suspensions (*suspension for 2nd conviction*), and jail sentences (*jail for 2nd conviction*). In 1992, 22 states had mandatory fines for second DUI convictions, ranging from \$300 to \$1000, and 44 states had minimum jail terms of from 2 to 180 days and license suspensions of 15 to 1095 days.

#### IV. Empirical Results

Table 3 presents the results of four regressions. The first two regressions use the driver involvement rate discussed above with a BAC of 0.01 or greater while the last two consider BAC equal to or greater than 0.10. The second and fourth equations include the minimum penalties for conviction on a second offense, whereas the first and third do not. All equations are specified with a set of year dummies for 1985 to 1992 to capture any year-specific factors that affect all states equally (such as national advertising campaigns) and a set of state dummies to capture any time-invariant, unmeasured, state-specific factors, even though the coefficients on these variables are not reported (intercepts are also not reported). Results from models without fixed effects are not presented [although they are available from the authors; see Mast et al. (1998) for details on fixed-effect results in a related study focusing on beer taxes], but fixed-effects estimates are superior in overall explanatory power, and, more significantly, coefficients change signs and significance when fixed effects are added to a basic ordinary least squares (OLS) model [e.g., see Ruhm (1996) and Mast et al. (1998)]. Furthermore, a Hausman (1978) test supports use of fixed-effects rather than an OLS specification.

The results reported in Table 3 suggest that alcohol consumption (*ethanol per capita*) is significant and positively related to the driver involvement rate, and most of the control variables generally have the expected signs.<sup>25</sup> Vehicle miles traveled per driver and the proportion of the population that is male between the ages of 16 and 24 are significant with the expected signs. Ruhm's (1995, 1996) contention that drunk driving is a normal good is also supported, as *per capita* disposable income is significant and positive while the unemployment rate is significant and negative. *Dry-county population*, *Catholics*, and *Mormons* are not significant explanatory variables, whereas *Southern Baptists* and *Other Protestants* (other than Southern Baptists and Mormons) have a large positive coefficient, consistent with findings in Ornstein and Hanssens (1985) and Mast et al. (1998).

In terms of alcohol control and deterrence variables, a higher legal drinking age, dram-shop laws, and open-container laws seem to be more effective than the other legislatively mandated policies analyzed here. Police *per capita* has the correct sign but does not meet standard significance level tests. Comparison of these results with other studies should only be done with considerable caution because of the wide variety of dependent variables, independent variables, and specifications that have been em-

<sup>24</sup>CPIs taken from *The Economic Report of the President* (various years), and COL indices taken from McMahon and Chang (1991), with the COL index for 1990 was used to adjust data for 1991 to 1992. It may be that the certainty of punishment is more important to deterring drunk driving than the severity of punishment. To account for this, some equations were estimated with mandated jail, fines, and license suspensions specified as dummy variables. Results were similar to those obtained when these variables were specified as continuous and, thus, are not reported.

<sup>25</sup>These findings are consistent with Mast et al. (1998), but beer, wine, and liquor consumption were separated and beer alone proved to be significantly related to the driver involvement rate.

TABLE 3. Driver involvement equations

Independent Variables	Equation (1) (BAC $\geq 0.01$ )		Equation (2) (BAC $\geq 0.01$ )		Equation (3) (BAC $\geq 0.10$ )		Equation (4) (BAC $\geq 0.10$ )	
	Coefficient	t statistic	Coefficient	t statistic	Coefficient	t statistic	Coefficient	t statistic
Legal drinking age	0.081267**	2.038	0.0901867**	2.208	0.0927677**	2.434	0.1046*	2.694
Dram-shop laws	-0.073817**	-2.144	-0.07977**	-2.31	-0.05765***	-1.75	-0.065517**	-2.01
Open-container laws	-0.09791**	-2.487	-0.10299**	-2.51	-0.06379**	-1.75	-0.0773**	-1.99
Anticonsumption laws	-1.53e-02	-0.371	-1.99e-02	-0.48	-1.50e-02	-0.38	-2.33e-02	-0.59
Police <i>per capita</i>	-0.00163	-1.591	-0.00154	-1.50	-0.00145	-1.48	-1.33e-03	-1.37
Preliminary breath-test laws	-0.01064	-0.337	-0.00343	-0.10	-0.00236	-0.07	-0.007012	0.233
Illegal <i>per se</i> laws	0.005215	0.07	-0.01612	-0.21	0.004652	0.067	-0.02541	-0.36
Implied-consent laws	-0.00019	-1.009	-0.00015	-0.77	0.00015	0.81	-8.32e-05	-0.45
No-plea-bargaining laws	0.015093	0.273	0.008445	0.153	0.017026	0.323	0.009213	0.176
Administrative <i>per se</i> laws	-0.00019	-0.672	-0.00015	-0.51	-0.00009	-0.33	-2.63e-05	-0.09
Jail for 1st conviction	-0.043	-1.586	-0.04167	-1.53	-0.03811	-1.47	-0.0355	-1.37
Jail for 2nd conviction	-0.00013	-0.964	0.000617	0.264	-0.00013	-1.01	-0.00032	-0.14
Fines for 1st conviction	0.000528	0.847	0.000551	0.871	0.000382	0.644	0.000478	0.8
Suspension for 1st conviction	-0.03076	-1.191	-0.03042	-1.17	-0.04055	-1.64	-0.04028	-1.64
Suspension for 2nd conviction	6.61e-05*	6.059	6.52e-05*	5.962	6.46e-05*	6.231	6.38e-05*	6.174
Seat-belt laws	0.37555*	3.342	0.38952*	3.338	0.35379*	3.28	0.36924*	3.315
Ethanol <i>per capita</i>	-0.1028***	-1.893	-0.00874	-1.60	-0.00785	-1.53	-0.0063	-1.22
Metropolitan population	7.7704***	1.883	6.6694	1.593	5.5265	1.404	3.881	0.977
Males 16-44 per capita	0.000042**	2.027	0.00004**	1.961	0.000043**	2.192	4.24e-05**	2.14
<i>Per capita</i> disposable income	-0.02075**	-2.536	-0.0198**	-2.38	-0.02133*	-2.71	-0.02012**	-2.53
Unemployment rate	0.04424	0.317	0.014147	0.101	-0.03809	-0.28	-0.07373	-0.55
Dry-county population	0.1369	0.106	0.41875	0.32	0.31302	0.252	0.75809	0.603
Catholics	-5.7197	-1.241	-5.6694	-1.23	-5.8295	-1.32	-5.7203	-1.31
Mormons	9.8165*	3.599	9.0812*	3.306	9.1349*	3.506	8.3131*	3.18
Southern Baptists	2.6*	2.681	2.8491*	2.893	2.606*	2.816	2.8442*	3.045
Other Protestants								
Adjusted R <sup>2</sup>		0.905		0.906		0.909		0.911
F statistic		52.4		50.7		54.8		53.9

Note: Dependent variable =  $1n[R/(1 - R)]$ . N = 432. Intercepts, year, and state dummy variables are not reported.

\*Significant at the 0.01 level.

\*\*Significant at the 0.05 level.

\*\*\*Significant at the 0.10 level (in two-tailed tests).

ployed in the literature. Nonetheless, it may be appropriate to note that both legal drinking ages and dram-shop laws have often been found to be significantly related to various measures of fatalities, whereas open-container laws tend to be insignificant when they are considered (there are exceptions, however), and the results regarding measures of policing tend to be mixed. Therefore, although these results suggest that deterrence might be effective, at least some of them may not provide strong support for focusing on a particular policy instrument. However, that is precisely the point made above. It may be the overall package of policy instruments that are important, rather than single policy variables. After all, as indicated above, findings in Evans et al. (1991) and Chaloupka and Wechsler (1996) suggest that groups of deterrence variables or some aggregate measure of enforcement intensity can have an important impact with DUI, perhaps even when an individual law or activity does not have a significant marginal impact. To consider this hypothesis a number of group effects were tested.<sup>26</sup>

Table 4 provides the F tests in consideration of various group effects. As anticipated from an economics of crime perspective, the impact on all of the regressions of removing all alcohol control and deterrence variables is highly significant. Consideration of more narrowly focused subsets of deterrence variables reveals that the alcohol control variables by themselves (legal drinking age and dram-shop laws) make very significant contributions to the explanatory power of the model. The removal of factors influencing policing (the probability of being stopped and/or arrested) also has a significant impact in all four equations, however. Among these variables, those grouped as influencing the probability of being stopped (open-container laws, anticonsumption laws, and police *per capita*) seem to be the most important, whereas illegal *per se* laws, preliminary-breath-test laws, and implied-consent laws by themselves do not have a significant group effect. Indeed, once an alleged DUI offender has been stopped it seems that the aggregate impact of additional actions may have relatively little deterrent impact, although all punishment variables as a group do have a significant impact in the fourth equation. Nonetheless, it seems that the probability of punishment has more deterrent effect than the severity of punishment for DUI, just as it does for crime in general, an important implication given recent findings that the supposed deterrent impact of alcohol taxes is not robust across time periods or survey samples.

To obtain some idea of the potential magnitude of deterrence effects, policy simulations were conducted for the grouped alcohol control variables (*legal drinking age* and

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<sup>26</sup>An alternative approach would be to examine the impact of the probabilities of arrest and conviction directly as in Wilkinson (1987). However, data problems prevent effective analysis of this type. Recall, for instance, that Wilkinson's probability of conviction given arrest ( $P(C|A)$ ) was calculated from disposition data for about 10% of the population obtained from the FBI's Uniform Crime Reporting System, and that his data were only available for years 1976 to 1977. It was assumed that jurisdictions not reporting data did not systematically differ from reporting jurisdictions. In addition, his probability of arrest variable (DUI arrests per vehicle miles traveled) is problematic for reasons indicated earlier. DUI arrests divided by the number of drivers involved in alcohol-related fatalities seem to be a much more appropriate indicator of the probability of a DUI arrest, and not surprisingly in light of resulting spurious correlation problems, it proves to be negative and highly significant in a regression similar to those presented here. Alternative specifications of proxies for the probability of a DUI arrest were also examined. For instance, DUI arrests divided by total traffic fatalities also was significant and negative in such regressions, but total fatalities were highly correlated with the driver involvement rate, so the spurious correlation problem remains. DUI arrests divided by ethanol *per capita* did not prove to be significant. Equations explaining DUI divided by the driver involvement ratio suggest that open-container laws, unemployment rates, and *per capita* disposable income are significant explanatory variables, along with indicators of the allocation of police manpower [Benson et al. (forthcoming)]. Thus, the equations presented here can be seen as reduced-form results that avoid the spurious correlation problems that arise when the probability of arrest is more directly examined.

TABLE 4. F tests for deterrence variables

Deterrence Variables Tested	Equation 1 (BAC > = 0.01)	Equation 2 (BAC > = 0.01)	Equation 3 (BAC > = 0.10)	Equation 4 (BAC > = 0.10)
All deterrence variables	F[13,351] = 3.21*	F[16,348] = 2.96*	F[13,351] = 2.65*	F[16,348] = 2.79*
Alcohol control ( <i>legal drinking age, dram-shop laws</i> )	F[2,351] = 4.72*	F[2,348] = 5.36*	F[2,351] = 4.81*	F[2,348] = 5.86*
Probability of arrest ( <i>police per capita, open-container laws, anticonsumption laws, illegal per se laws, preliminary breath-test laws, implied-consent laws</i> )	F[6,351] = 2.11***	F[3,648] = 2.08***	F[6,351] = 1.47	F[6,348] = 1.65
Probability of being stopped ( <i>police per capita, open-container laws, anticonsumption laws</i> )	F[3,351] = 3.18**	F[3,348] = 3.20**	F[3,351] = 2.15***	F[3,348] = 2.48***
Probability of arrest given being stopped ( <i>illegal per se laws, preliminary-breath-test laws, implied-consent laws</i> )	F[3,351] = .45	F[3,348] = .27	F[3,351] = .27	F[3,348] = .18
Expected punishment for 1st offense ( <i>administrative per se laws, no-plea-bargaining laws, fines, jail, suspensions</i> )	F[5,351] = .87		F[5,351] = .66	
Expected punishment for 1st and 2nd offenses ( <i>administrative per se laws, no-plea-bargaining laws, fines, jail, suspensions</i> )		F[8,348] = 1.21		F[8,348] = 1.62
Expected punishment given conviction for 1st offense ( <i>jail, fines, suspensions</i> )	F[3,351] = 1.40		F[3,351] = 1.17	
Expected punishment given conviction for 1st and 2nd offenses ( <i>jail, fines, suspensions</i> )		F[6,348] = 1.59		F[6,348] = 2.20*

\*Significant at the 0.01 level.  
 \*\*Significant at the 0.05 level.  
 \*\*\*Significant at the 0.10 level.

*dram-shop laws*), and probability-of-being-stopped variables (*open-container laws*, *anticonsumption laws*, and *police per capita*). For the alcohol control variables, the predicted driver involvement rates if all states had a drinking age of 21 (a decrease from a weighted average of 0.099 to 0) and dram-shop liability (increasing the weighted average from 0.595 to 1) for the entire sample period was compared to the predicted driver involvement rates at the weighted means of all the variables in the model. The predicted decreases in driver involvement rates ranged from 2.9% to 3.6% across the four models. For the probability-of-being-stopped variables, the predicted driver involvement rates if all states had open-container and anticonsumption laws and 10% greater police employment rates was compared to the predictions at the weighted average of all the variables. This involved an increase in the weighted average of open-container laws from 0.41 to 1, and in anticonsumption laws from .66 to 1, whereas the weighted average of police per capita was increased from about 200 to 220. The predicted driver involvement rates decreased by 6.4 to 8.1% for the four models. The decreases for the BAC levels greater than or equal to 0.01 drivers were larger than for the BAC greater than or equal to 0.10 drivers, a finding that is consistent with the literature suggesting that moderate drinkers are more price sensitive than heavy drinkers.

Because the dependent variable for the regressions reported in Table 3 and the group tests reported in Table 4 involves some observations that are estimated rather than directly observed, an alternative dependent variable was also considered, as noted above: the total number of vehicle deaths in each state divided by the number of people in the population 16 years old and above. These results are not reported in detail because the dependent variable is a much less direct proxy for DUI behavior, and because the estimates are, in general, quite consistent with those reported in Tables 3 and 4. In particular, the F tests for groups of variables are consistent with those reported in Table 4,<sup>27</sup> and several individual variables are significant, with the same signs as those reported for significant coefficients in one or more of the regressions in Table 3.<sup>28</sup>

## V. Conclusions

If one were to summarize in one sentence the results of the extensive econometric literature on deterrence that has developed since Becker (1968) published his economic theory of crime, that sentence might be something like this: The probability of punishment seems to be an important deterrent, whereas the evidence regarding the severity of formal punishment is much less conclusive [e.g., see Elliot (1977) and Rasmussen and Benson (1994) for literature reviews reaching this conclusion]. Precisely the same conclusions seem to apply to the crime of DUI. Factors that enhance the

<sup>27</sup>For the following groups, F tests are provided in parentheses with \* indicating significance at the 0.01 level, \*\* at the 0.05 level, and \*\*\* at the 0.10 level, as in Table 4: All deterrence variables (3.06\*), alcohol control (3.59\*\*), probability of arrest (1.87\*\*\*), probability of being stopped (1.36), probability of arrest given being stopped (1.63), expected punishment for 1st offense (0.70), expected punishment for 1st and 2nd offenses (2.91\*), expected punishment given conviction for 1st offense (0.97), and expected punishment given conviction for 1st and 2nd offenses (3.77\*).

<sup>28</sup>*Dram-shop laws*, *fines for 2nd conviction*, *vehicle miles per driver*, *seat-belt laws*, *ethanol per capita*, *metropolitan population*, *males 16–44 per capita*, *per capita disposable income*, and the *unemployment rate* are all significant with signs that correspond to those in Table 3. The only significant coefficients in this regression that are not significant in any of the regressions in Table 3 are *implied-consent laws* and *suspension for 2nd conviction*, whereas the *legal drinking age*, *open-container laws*, *Southern Baptists*, and *other Protestants* are not significantly different from zero, in contrast to the results in Table 3. The remaining coefficients are insignificant in Table 3 and in this regression.

probability that a driver who drinks will be stopped by the police seem to significantly reduce drunk driving-related fatalities. This result seems to be quite robust. On the other hand, findings regarding laws that mandate various minimum levels of punishment are much less robust, although they seem to be significant for some specifications. One possible explanation of this is that when the chance of detection and/or arrest is small enough, increases in the severity of punishment may have no marginal effect on deterrence [Cook (1980)]. In this light, finding any law enforcement deterrent effect for drunk driving might, in fact, be surprising. After all, it does not seem that any sort of consistent, long-term, systematic law enforcement effort has been made against drunk driving. Certainly, many laws have been passed, but if police do not actively pursue offenders, such laws may have no long-term consequences. Policing agencies do occasionally develop short-term programs that target drunk drivers, setting up temporary sobriety checkpoints, temporarily increasing patrols during holidays or weekends, and so on, of course, but it seems that, in general, a DUI arrest is probably relatively unlikely unless an accident actually occurs [Benson et al. (forthcoming)]. At least in some jurisdictions, police are more likely to escort or drive a drunk person home than to arrest him, for instance, unless an accident occurred.

The conclusions of this study are in contrast to the conventional wisdom that has developed from econometric studies focusing on alcohol taxes as a deterrent. The conclusions echo those of other recent studies [Kenkel (1996); Chaloupka and Wechsler (1996); Mast et al. (1998)], however: Specifically, it seems that direct law-enforcement efforts could be an effective way to significantly reduce drunk driving if policies were implemented to produce systematic, persistent, and consistent increases in the probability of being stopped and arrested for drunk driving (and perhaps in the severity of punishment). At the very least, such policies are deserving of much more attention than they have been getting.

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