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General Deterrence of Drunk Driving: Evaluation of Recent American Policies

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A testable hypothesis of deterrence theory is that efforts to increase the expected cost of criminal activity by increasing the threat of punishment should, other things being equal, reduce the crime rate. In this paper, we examine whether the incidence of drinking and driving is responsive to escalation of the punitive threat. The recent national campaign against drunk driving provides a natural experiment in which to test the predictions of deterrence theory. Using state level data over the 1975–1986 period, we report no conclusive evidence that any specific form of punitive legislation is having a measurable effect on motor vehicle fatalities. We report suggestive evidence that multiple laws designed to increase the certainty of punishment (e.g., sobriety checkpoints and preliminary breath tests) have a synergistic deterrent effect. The most striking finding is that mandatory seat belt use laws and beer taxes may be more effective at reducing drunk driving fatalities than policies aimed at general deterrence.

KEY WORDS: Drunk driving; traffic safety; general deterrence.

1. INTRODUCTION

Economists have contributed to the study of crime by viewing potential criminals as rational decision-makers in the face of uncertainty.[(1)] The incidence of illegal behavior is presumed to be responsive to the marginal benefits and costs of such behavior—at least those that are perceived by the would-be criminals. A testable implication of this model is that efforts to increase the expected cost of criminal activity by magnifying the threat of punishment should, other things being equal, reduce the crime rate.[(2)] The punitive threat can be magnified by increasing the probability of punishment ("certainty"), by increasing the size of penalties ("severity"), and by increasing the swiftness of punishment ("celerity").[(3)] Empirial studies suggest that crimes as diverse as theft, violence, and draft evasion have been deterred by adjustments in the punitive threat.[(4–5)]

In this paper, we examine whether the incidence of drinking and driving is responsive to escalation of the punitive threat. The dominant view in the international literature appears to be that it is difficult to detect significant and sustained reductions in drunk driving following the enactment of punitive policies.[(6–8)] The recent national campaign against drinking and driving in the United States provides a fruitful context for reexamining this question.

The societal impact of drinking and driving is readily observed in traffic fatality statistics. Earlier in this decade it was estimated that alcohol may have been involved in 50–55% of fatal motor vehicle crashes.[(9)] Public health professionals consider drunk driving to be the number-one killer of Americans between the ages of 15 and 24. Estimates of the direct and indirect costs of drunk driving to the nation approach $25 billion per year.[(10)] In light of these societal consequences and re-

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cent policy initiatives, it is important to assess whether drunk driving can be effectively curtailed through punitive measures.

2. THE AMERICAN CAMPAIGN AGAINST DRUNK DRIVING, 1982–1986

Although drunk driving has long been recognized as a serious problem, the years since 1980 witnessed an unprecedented growth of grassroots activism and legislation designed to address the drunk driving problem. The best known grassroots lobbying group, Mothers Against Drunk Driving (MADD), formed its first chapters in 1981, and membership increased dramatically in the years 1982–1985.[4] Mass media coverage of drunk driving incidents and legislation mushroomed.[5] The establishment of the President's Commission on Drunk Driving in 1982 placed the issue on the national agenda, and the Alcohol Traffic Safety Act of 1983[6] established financial incentives for states to bring their traffic safety programs into compliance with federal recommendations for tough drunk driving laws. Several states, such as California and Maine, had enacted some drunk driving countermeasures prior to creation of these federal incentives. However, the grants intensified the pressure to enact new legislation in states that had not already done so. In the years between 1981 and 1986, 729 state laws pertaining to drunk driving were enacted.[(11)]

Recent evidence suggests that the national campaign against drunk drivers generated tangible results. The percent of drivers who die in traffic accidents with blood-alcohol concentrations (BAC) of 0.10% or above declined from 50% in 1980 to 43% in 1984.[(12)] Although total traffic fatalities were also declining over this period, the number of intoxicated drivers declined by about twice as much as the total number of drivers killed in crashes. Roadside breath-testing data from 18 police jurisdictions showed a 51% decline in the proportion of drivers who tested at or above 0.10% BAC in 1985 compared to 1973.[(13)] Neville *et al.*[(14)] found that the national campaign against drunk driving from 1982–1986 was responsible for a 16–20% reduction in alcohol-related traffic fatalities.

The types of anti-drunk driving activities adopted in the 1980s are quite varied. Cook[(15)] lists four categories into which most measures can be classified:

1. "Policies to reduce drinking by high-risk groups or the general public," such as minimum legal drinking age laws, treatment programs for alcoholics, and excise taxes on alcohol.
2. "Policies to reduce access to alcohol by people who are driving or will be driving within a few hours," such as dram shop rules and open container laws.
3. "Policies to persuade or help people to separate drinking and driving," such as education about the dangers of drunk driving and designated driver programs.
4. "Policies to deter people from driving while drunk," such as legislation aimed at increasing the certainty, severity, and/or speed of punishment.

Many of the recent legislative initiatives enacted to reduce drunk driving fall under the rubric of Cook's fourth category. This heavy emphasis on deterring the drunk driver was reflected in the President's Commission on Drunk Driving, which recommended that "in order to ensure that our laws discourage the largest possible number of potential drunk drivers, States and localities should take a general deterrence approach to the problem."[(10)] The large number and variety of deterrence-based policies that have been implemented provide numerous tests of the predictions of deterrence theory.

The specific drunk driving policies we consider in this study are described in Table I. These policies were selected on the basis of their hypothesized importance, data availability, and their incorporation into state statutes. Our list includes six deterrence-oriented policies: administrative per se laws (ADMIN), anti plea bargaining statutes (ANTI), mandatory penalties for first offenders (FIRST), illegal per se statutes (ILL), preliminary breath tests (PBT), and sobriety checkpoints (SOBER). One policy, open container laws (OPEN), is designed to separate drinking and driving.

The swiftness of the legislative campaign is evident in Table II, which provides counts of states with particular interventions. The number of states with either an ILL, PBT, or SOBER law tripled in the period 1981–1986. The number of states with ADMIN increased from 4–16 and states with ANTI increased from 2–16 in the same period.

While each of the six deterrence-oriented policies is designed to increase the expected cost of driving drunk, their potential points of impact within the criminal jus-

[4] The yearly number of MADD chapters chartered grew from 5 in 1981 to 98 in 1982, 257 in 1983, 322 in 1984, 386 in 1985, and 395 in 1986. (Private correspondence with MADD.)

[5] The total number of articles devoted to drunk driving in four sources (*Los Angeles Times, New York Times, Washington Post*, and *The Readers Digest*) grew from 38 and 52 in 1979 and 1980, respectively, to 349 in 1983 and 311 in 1984.

[6] Alcohol Traffic Safety Act Pub. L. No. 97-364 (1982).

Table I. Description of Anti-Drunk Driving Policies[a]

Policy (variable name)	Definition
Administrative per se law (ADMIN)	Laws that permit the state licensing agency to suspend a driver's license via administrative action (independent of any court action related to a DUI charge), if a driver's blood alcohol concentration (BAC) level is in excess of a specified level
Anti-plea bargaining law (ANTI)	State statute prohibiting a prosecutor and defense attorney from agreeing to reduce a "driving under the influence (DUI)" charge to a lesser offense in exchange for the defendent pleading guilty to a lesser charge
Mandatory penalty for first offense (FIRST)	Law establishing a mandatory jail sentence or community service for the first conviction of drunk-driver
Illegal per se law (ILL)	Laws that make it a criminal offense to operate a motor vehicle with a specified amount of alcohol in the blood. The BAC level we examine in this study is 0.10%
Open container law (OPEN)	Laws that prohibit open containers of alcohol in the passenger cab of a motor vehicle
Preliminary breath tests (PBT)	A roadside test involving a portable breath-alcohol tester to measure a driver's intoxication level. Results of the test may be used to establish probable cause for arrest
Sobriety checkpoints (SOBER)	Police practice of checking drivers for signs of driving under the influence of alcohol. Although many states may incorporate such assessments as part of routine safety checks, in this study we consider only those states that have passed legislation authorizing police to use sobriety checkpoints

[a]The interventions considered in this study were selected on the basis of the following considerations: (1) hypothesized importance (as suggested by deterrence theory, the President's Commission on Drunk Driving, and personal communication with individuals involved in government programs related to drunk driving at the state and national level); and (2) availability of data on the interventions at the state level. The types of interventions which have been introduced in some states but not included in our study include: (a) second offense mandatory penalty; (b) designated driver programs; (c) victim restitution programs; (d) continuing education for police, prosecutors, and judges; (e) dram shop statutes; and (f) minimum legal drinking age laws.

tice system vary (See Fig. 1). Policies designed to increase the certainty of punishment can do so by increasing the probability of detection, increasing the probability of conviction given detection, increasing the probability of punishment given conviction, or some combination of the above. Roadside sobriety checkpoints and preliminary breath tests are designed to increase the probability of detection. Certainty of punishment is enhanced by administrative per se laws (which allow arresting officers to suspend driving privileges without court action), and by illegal per se laws (which provide officers with a liberalized definition of the criminal activity). The interventions that increase the expected severity of punishment are anti-plea bargaining statutes, mandatory penalties for first offenders, and administrative per se laws. It should be noted that administrative per se laws are also designed to increase the speed of punishment through swift license suspension.

3. DETERRING THE DRUNK DRIVER: PREVIOUS EMPIRICAL WORK

Before the recent national drunk driving campaign in the United States, Ross[6–8] reviewed studies of drunk driving interventions in Scandinavia, Great Britain, New Zealand, Australia, Canada, The Netherlands, France, and selected cities in the United States. He concluded that policies that promise increased certainty of punishment cause only a temporary reduction in alcohol-related fatalities, while policies aimed at increased severity (without an increase in certainty) are ineffective. Ross also concluded that little is known about the effects of the speed of punishment. Votey,[16–18] using methods that differ from Ross's, found that Norwegian and Swedish anti-drunk driving policies do have a substantial and lasting impact on alcohol-related fatalities. In contrast to the large body of international literature accumulated before 1982, there have been only a handful of attempts to evaluate recent policies in the United States.

California was one of the first states to implement stricter drunk driving laws. On January 1, 1982, the state enacted an illegal per se law (BAC $\geq$ 0.10%), mandatory jail sentences for all repeat offenders, and restrictions on plea bargaining of drunk driving charges. Hilton[19] found that in the first year after adoption of the statute, the number of fatal traffic accidents decreased. However, the decline was not "alcohol specific" in that alcohol and nonalcohol-related accidents declined by the same amount. Hilton concluded that the 1982 accident reductions were not attributable to the specific statute. More recent evaluations of the California experience do not find strong evidence of a deterrent effect. Evaluations of new programs in Maine, Arizona, and Massachusetts also yielded inconclusive results.[20,21]

Saffer and Chaloumpka[22] used a panel of data for the 48 contiguous state for the period 1980–1985 to analyze the effectiveness of state laws permitting the use of

Table II. Number of States with Specific Forms of Anti-Drunk Driving Legislation, 1975–1986[a]

Year	ADMIN	ANTI	FIRST	ILL	OPEN	PBT	SOBER
75	1	0	3	12	10	0	1
76	1	0	3	13	10	8	1
77	1	0	3	13	10	8	1
78	3	0	4	13	10	8	1
79	3	0	6	14	10	8	1
80	3	2	7	16	10	8	2
81	4	2	8	17	13	11	3
82	6	5	15	21	14	15	3
83	13	12	24	37	15	22	5
84	16	15	27	38	17	23	6
85	17	16	27	39	17	23	6
86	18	16	27	42	17	24	6

[a]*Sources*: Survey of the 50 Governor's Highway Safety Representatives, cross-checked with law library research on statutes.

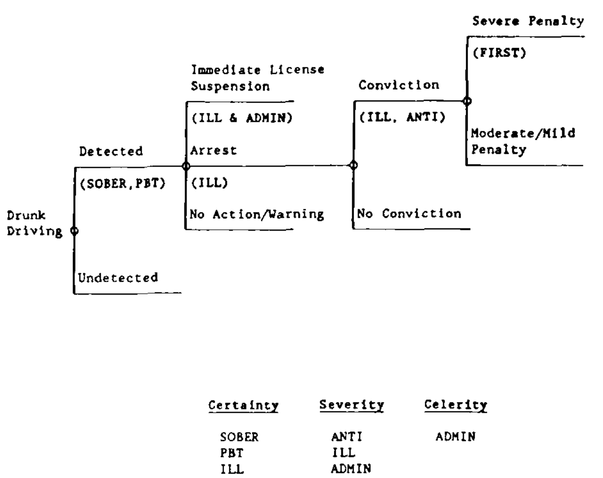

Fig. 1. Likely points of impact within the criminal justice system of various drunk-driving policies.

preliminary breath tests. They concluded that allowing preliminary breath tests in all states would reduce traffic fatalities by more than 2000 per year. Their econometric models do not include any other deterrence-oriented measures, and their results are sensitive to the inclusion of year dummy variables.

Zador *et al.*[23] used fatal crash data from the 48 contiguous states from 1978–1985 to evaluate three types of drunk driving laws: administrative per se, mandatory penalties for first offenders, and illegal per se. The results based on their preferred statistical method of state-pair comparisons suggest that each of the three types of laws provide a modest reduction of 6–9% in alcohol-related fatalities. Alternative analyses based on econometric methods suggested that the three interventions were somewhat effective. The effectiveness estimates for administrative per se laws were particularly robust to alternate analytic methods. The authors acknowledge that the estimated effects may be reflecting the change in behavior and public attitudes generated by heightened mass media coverage that accompanies such laws.

Edelman[24] used a 2-year cross-section of fatal crash data from the 50 states and the District of Columbia to estimate models of urban and rural fatality rates. Three types of explanatory variables are included: (1) laws aimed at curbing drunk driving (of which 13 are included); (2) economic factors such as income and gas prices; and (3) cultural variables, such as the percent of "dry" counties and the prevalence of religions that forbid the use of alcohol. Edelman's results suggest that drunk driving laws are more effective on urban road than rural roads. However, given the cross-sectional nature of this study, it is difficult to determine whether the variations in alcohol-related fatalities can be attributed to the specific policies, or to unmeasured state-specific effects.

The antidrunk driving policy which has been most extensively evaluated in the United States is the minimum legal drinking age. With few exceptions, studies consistently confirm that this particular intervention reduces the number of fatal crashes among teenagers.[25,26]

Our empirical investigation builds on what we see as the major methodological lessons of recent papers. First, the use of both temporal and cross-sectional comparisons offers more information than does either alone. Second, a specific type of law (say, mandatory penalties for first offenders) should be evaluated in a model that accounts for the effects of other laws enacted during the same period (e.g., sobriety checkpoints). Third, the model should allow for the possibility that the effects of specific laws are synergistic with each other. For example, the effects of simultaneously escalating certainty and severity of punishment may be multiplicative rather than additive. Finally, since the specific state-level interventions occur in a period of changing national norms toward drunk driving, the model must be designed to disentangle nationwide effects from the effects of specific interventions enacted at the state level.

4. SPECIFICATION OF THE FATALITY EQUATION

The best indication of alcohol involvement in a fatal crash is the blood-alcohol concentration of a fatally injured driver. However, only 15 states have been consistently testing drivers killed in crashes during the 1980–1985 period, and even in these states anywhere from 10–20% of killed drivers have unknown BAC. For the purpose of this paper, the question then is how best to proxy the extent of drunk driving in the population.

One possible measure of drunk driving is a fatality category that is known to be disproportionately alcohol-related, such as single-vehicle occupant fatalities. This was the outcome measure used by Williams *et al.*,[27] and Wagenaar[25] in evaluations of drinking age legislation. Such alcohol-sensitive measures are of limited use from a policy perspective because they only provide a lower bound estimate of the lifesaving benefit of a particular policy. A more inclusive measure of traffic safety may more accurately reflect the total lifesaving benefit from a particular policy. This is the strategy employed by Cook and Tauchen[28] in their analysis of minimum drinking age legislation. However, since an inclusive measure has a smaller alcohol-related component, it may be impossible to distinguish small changes in drunk driving from the year-to-year fluctuations in the aggregate data.

For this study we utilize both inclusive and alcohol-sensitive measures. Specifically, we present results for two "alcohol-sensitive" measures (single vehicle occupant fatalities, single vehicle occupant fatalities that occur at night), one inclusive measure (total fatalities), and one alcohol-insensitive measure (multivehicle occupant fatalities that occur during the day). The extent of alcohol involvement for all four fatality categories is displayed in Table III. We expect that a policy that significantly decreases drunk driving will have a larger impact on the alcohol-sensitive dependent variables than on either the inclusive category or the alcohol-insensitive variable.[7]

Table III. NHTSA Estimates of Percent of Dead Drivers with BAC ≥ .10

Category	% with BAC ≥ 0.10
Single vehicle, occupant fatalities that occur at night	68%
Single vehicle, occupant fatalities	55%
Total fatalities	34%
Multivehicle, occupant fatalities	23%

Since enactment of the Alcohol Traffic Safety Act of 1982, *all* states have adopted some form of legislation designed to reduce alcohol-related traffic fatalities. The elements of each legislative package and their timing vary across states. We assume that the mix of laws adopted and the timing of each proposal is independent of prior fluctuations in the state-level fatality rates, which allows us to consider the particular intervention as exogenous with respect to the fatality rate. Previous studies have found community activism, media coverage, and federal financial incentives were responsible for the proliferation of legislative activity.[11] Consequently, each state can be treated as a unique trial in a natural experiment, permitting an investigation of the pre- and postintervention fatality experience for the states. The proper model in this instance is an analysis of covariance procedure.

The equation we choose to estimate is of the form

$$F_{it} = X'_{it}\gamma + Z'_{it}\beta + u_i + \delta_t + \epsilon_{it} \quad (1)$$

where F_{it} is the natural log of fatality counts for state i in year t, Z_{it} is a $k_1 \times 1$ vector that measures the anti-drunk driving interventions, X_{it} is a $k_2 \times 1$ vector of state characteristics that explains variations in a states' mean fatality level, and ϵ_{it} is a zero mean random error. The variable u_i is assumed to be a state-specific effect that varies across states but is constant over time, which captures the effect of such factors as road conditions, weather patterns, or the demographic composition of the driving population. The variable δ_t captures year effects that are common to all states, but vary over time, such as the 1979 energy crisis, or the heightened public awareness of the hazards of drunk driving that occurred throughout the nation in the 1980s. The state and year

[7] There is the possibility, however, that some types of drunk driving are more deterrable than others. For example, if daytime drunk driving is more deterrable than nighttime drunk driving, our expectations might be incorrect.

effects are designed to measure fixed effects not captured by the *X* and *Z* vectors. The parameters of interest are the elements of the β vector that indicate the effectiveness of the particular interventions. This type of pooling model has become increasingly popular in the evaluation of selected traffic safety programs, such as the minimum legal drinking age,[28] beer taxes,[29] mandatory seat belt use laws,[30] and mandatory motorcycle helmet use laws.[31]

The variables we include in the *X* vector are the log of the number of vehicle miles of travel (VMTL), the percentage of young drivers (YOUNG), the percentage unemployment rate in each state (UE), and a dummy variable equalling 1 if a state has a mandatory seat belt use law (BELT). The variables VMTL and YOUNG control for the degree of exposure to collisions and their expected signs are both positive. The unemployment rate is included because recent research has demonstrated that the business cycle has a procyclic effect on fatalities, beyond the obvious effect of the business cycle on vehicle miles of travel.[32] We include BELT in the *X* vector since recent evidence indicates that mandatory seat belt use laws have significantly reduced occupant fatalities.[30]

The antidrunk driving laws included in the *Z* vector are listed and defined in Table I. To capture the effects of the particular antidrunk driving laws, we characterize each type of laws as a dummy variable, equalling one if the legislation is in effect in the state for the year, and zero otherwise.[8] Policies that reduce drunk driving fatalities should have a negative coefficient, and the coefficient's magnitude should be larger in the equations where the dependent variable has a greater degree of alcohol involvement. Data on the effective dates of laws in each state were obtained by a mail survey of State Governor's Highway Safety Representatives. The dates were cross-checked with a review of state statutes.[9]

We also include in the *Z* vector the real tax on a case of beer (BTAX). Recent evidence indicates that alcohol-related fatalities, especially among teenagers, are sensitive to changes in price.[29] Since we do not have access to state-level price of alcohol data, we use the sum of the state and federal taxes on a case of beer as a price surrogate.[10]

One variable we did not include in the *Z* vector is a variable capturing the minimum legal drinking age. Because teen drivers affected by a minimum drinking age account for a small portion of all drunk driving fatalities, the effect of a change in the drinking age on fatalities (the signal) is likely to be undetectable from the year-to-year fluctuations in a state's fatality counts (the noise). We verified this assumption by running a seperate model with a drinking age variable and found little effect of the drinking age on fatalities. We also found that including the drinking age measure did not materially alter the values of the other variables.

All data were collected at the state level for the 12-year period 1975–1986. Variable definitions and sample characteristics are included in Table IV. Equation (1)

Table IV. Variable Definitions and Descriptive Statistics, 50 States, 1975–1986

Variable	Definition	Mean (SD)
TF	Total fatalities, TFL = log(TF)	932.9 (931.2)
SVOF	Single-vehicle, occupant fatalities, SVOFL = log(SVOF)	378.2 (355.5)
SVOFN	Single-vehicle, occupant fatalities that occur at night, SVOFNL = log(SVOFN)	201.8 (194.8)
MVOF	Multivehicle, occupant fatalities	408.7 (413.7)
UNEMPLOY	Unemployment rate (%)	7.24 (2.24)
VMT	Vehicle miles of travel ($\times 10^6$), VMTL = log(VMT)	31.47 (31.77)
YOUNG	Proportion of drivers under 24 years of age	0.21 (0.03)
BTAX	Sum of state and federal excise taxes on a case of beer, in constant 1987 dollars	1.53 (0.66)
BELT	Dummy variable (DV) = 1 if state has a mandatory seat belt use law, 0 otherwise	0.03 (0.16)
ADMIN	DV = 1 if state has administrative per se law, 0 otherwise	0.13 (0.33)
ANTI	DV = 1 if state has anti-plea bargaining statute, 0 otherwise	0.01 (0.29)
FIRST	DV = 1 if state has mandatory jail term or community service for first offense, 0 otherwise	0.24 (0.41)
ILL	DV = 1 if state has illegal per se law, 0 otherwise	0.42 (0.48)
OPEN	DV = 1 if state has open container law, 0 otherwise	0.23 (0.42)
PBT	DV = 1 if state allows preliminary breath tests, 0 otherwise	0.25 (0.43)
SOBER	DV = 1 if state has legislation authorizing sobriety checkpoints, 0 otherwise	0.06 (0.23)
N	Number of observations	600

[8] Dummy variables for legislation are based on effective date of the law, and not the date of enactment. If a law took effect in the middle of the year, the value of the legal variable equals the fraction of the months in the year that the law was in effect.

[9] We received written survey responses from all but three state governments. For these three states, we obtained the dates by telephone.

[10] We obtained state-level excise tax data from The Beer Institute.

was estimated for the two alcohol-related variables (SVOFNL and SVOFL), the inclusive measure of fatalities (TFL), and the alcohol-insensitive measure (MVOFL). Given that there is a wide variance in the number of fatalities across states, the additive error ϵ_{it} may not be identically distributed across states. Since the exact form of the heteroskedasticity is unknown, we do not rely on a generalized least-squares estimator, but rather, we calculate heteroskedastic-consistent standard errors as described by White.(33) To facilitate the construction of the White standard errors, state and year means were subtracted from, and a sample mean was added to, all dependent and independent variables, which obviates the need to estimate separate parameters for u_i and δ_t.(34)

5. RESULTS

The results for Eq. (1) are presented in Table V. In all four regressions, the exposure variables (UNEM and VMTL) have the expected signs and are significantly different from zero (except for UNEM in the first column). The coefficient for YOUNG in the total fatality equation suggests an elastic response to a change in youthful drivers: a 1% increase in young drivers will have a greater than 1% increase in fatalities. The coefficient for the variable BELT in the total fatality equation indicates that belt use laws reduce total traffic fatalities by about 6%. Evans and Graham(30) found that mandatory belt use laws reduced front-seat car occupant fatalities by about 8%, and since front-seat car occupant fatalities represent about one half of all traffic fatalities, the two estimates are roughly consistent. Belt use laws also appear to reduce alcohol-related fatalities, as the coefficients for the two alcohol-sensitive equations suggest. For the most sensitive alcohol variable (SVOFN), belt use laws appear to reduce fatalities by almost 12%.

The results for the beer tax variable indicate that traffic fatalities are sensitive to changes in the price of alcohol. As one would expect, the magnitude of the parameter estimates are smaller for total fatalities and the alcohol-insensitive measure (MVOFL) than for the two alcohol-sensitive variables (SVOFN and SVOF). Our estimate in the total fatalities equation appears to be consistent with previous work. Using similar techniques with different data, Saffer and Grossman(29) find the elasticity of fatalities with respect to the beer tax[11] to be −0.17 for 18- to 20- and 21- to 24-year-olds, while our estimate implies an elasticity of −0.08 for TF and −0.12 for SVOFN.

The results for the OPEN and ADMIN variables are somewhat puzzling. Open container and administrative per se laws are each associated with a statistically significant drop in fatalities of 7–8% for MVOFL.[12] However, the results from the equations with alcohol-sensitive variables indicate a much smaller and statistically insignificant effect. It may be the case that open container laws can only be effectively enforced during the day when violators are most easily detected. Also, the administrative per se law may have a greater effect on daytime drivers because these drivers tend to be commuters, and the cost to the potential drunk driver of the administrative per se law may be greatest for this population. However, given the lack of statistical significance for administrative per se in the other equations, we suspect that this result is spurious.

The most striking feature of the results in Table V

Table V. Fixed-Effect Estimates of Fatality Equations: Coefficient Estimates and Heteroskedastic-Consistent Standard Errors

Independent variable	Dependent variables			
	SVOFNL	SVOFL	TFL	MVOFL
VMTL	0.529	0.500	0.661	0.817
	(0.126)	(0.100)	(0.083)	(0.113)
UNEMPLOY	2.7E-4	−0.010	−0.017	−0.022
	(4.8E-3)	(0.004)	(0.003)	(0.004)
YOUNG	1.279	0.843	1.012	1.350
	(0.567)	(0.352)	(0.268)	(0.403)
BTAXR	−0.129	−0.092	−0.075	−0.052
	(0.036)	(0.036)	(0.023)	(0.028)
BELT	−0.119	−0.116	−0.059	−0.070
	(0.051)	(0.028)	(0.027)	(0.032)
OPEN	−0.042	−0.054	−0.071	−0.078
	(0.032)	(0.024)	(0.019)	(0.026)
ADMIN	−0.014	−0.008	−0.017	−0.074
	(0.038)	(0.025)	(0.020)	(0.027)
ANTI	0.005	0.024	0.022	0.024
	(0.030)	(0.021)	(0.017)	(0.023)
FIRST	0.066	0.059	0.042	0.014
	(0.026)	(0.019)	(0.015)	(0.020)
ILL	0.017	0.005	0.004	−0.007
	(0.024)	(0.019)	(0.016)	(0.020)
PBT	−0.008	0.004	0.016	0.014
	(0.030)	(0.022)	(0.018)	(0.025)
SOBER	−0.080	−0.070	−0.045	−0.030
	(0.056)	(0.037)	(0.030)	(0.039)
R^2	0.979	0.988	0.992	0.987

[11] Saffer and Grossman(29) actually calculate the elasticity of death rates (fatalities per population) with respect to beer tax to be −0.17. However, if we assume that the beer tax only alters the numerator and not the denominator in the death rate equation, then the fatality elasticity will also equal −0.17.

[12] Edelman(24) found similar results for open container laws.

is that the deterrence-oriented measures (except SOBER) do not appear to be associated with large declines in fatality counts. The coefficient estimates are either of the wrong sign, or are statistically *insignificant*. This pattern suggests that, by themselves, the specific interventions do not exert a powerful deterrent effect on drunk driving. Sobriety checkpoints (which happen to be the least popular legislative strategy) do appear to exert a significant deterrent effect. The coefficient estimates for SOBER are negative, of marginal statistical significance, and the absolute size of the effect is larger in the alcohol-sensitive regressions.

The results for sobriety checkpoints suggest that increases in the perceived certainty of detection may successfully deter drunk driving. We find no evidence that the severity or speed of punishment for driving drunk has had an effect. This result does not necessarily cast doubt on the deterrence hypothesis. Cook notes that if the probability of detection and/or arrest is sufficiently low, an increase in the severity of punishment may have no marginal deterrent effect.[35]

To further explore the possible effects of heightened severity of punishment, we tested for a synergistic effect between the laws designed to increase certainty of punishment and the laws designed to increase severity of punishment. To perform the test, we created two dummy variables: SEVERE, which equals 1 if a state passed a law that increases the severity of punishment, and CERTAIN, which equals 1 if a state passed a law designed to increase the certainty of punishment. For the four dependent variables, we include the previously defined X vector, plus SEVERE, CERTAIN, and its cross-product SEVERE × CERTAIN. If a synergistic effect is present, we expect that the coefficient on the cross-product term to be negative. We found no evidence of such a synergistic effect.[13]

We did find, however, some evidence of a synergistic effect among the laws designed to increase certainty of punishment. Table VI reports our tests for synergistic effects between SOBER and PBT and SOBER and ILL. By including the product SOBER × PBT in the four regressions from Table V, we estimate that states with both SOBER and PBT experienced a 24% decline in SVOFN, whereas the effects for SOBER or PBT alone were statistically insignificant. This effect was less pronounced yet evident in the SVOF regression. SOBER and ILL together were also associated with a 22% reduction in SVOFN and SVOF.

Since only a small number of states have enacted

Table VI. Fixed-Effect Estimates of Fatality Equations with Intervention Interactions: Coefficient Estimates and Heteroskedastic-Consistent Standard Errors

Independent variable	Dependent Variables			
	SVOFNL	SVOFL	TFL	MVOFL
PBT	0.004	0.009	0.017	0.011
	(0.028)	(0.022)	(0.018)	(0.025)
SOBER	0.023	−0.026	−0.032	−0.056
	(0.066)	(0.041)	(0.037)	(0.046)
Both SOBER and PBT	−0.249	−0.108	−0.032	−0.066
	(0.095)	(0.070)	(0.058)	(0.072)
ILL	0.039	0.019	0.014	−0.003
	(0.025)	(0.010)	(0.015)	(0.020)
SOBER	0.076	0.037	0.026	−0.001
	(0.046)	(0.033)	(0.027)	(0.035)
Both ILL	−0.226	−0.158	−0.105	−0.043
	(0.054)	(0.040)	(0.031)	(0.043)

these combinations of laws, these results should be interpreted with caution. It is nonetheless possible that SOBER is not the critical variable in generating deterrence; rather, a combination of laws aimed at escalating certainty of punishment may be necessary to produce a detectable deterrent effect.

Although most of the specific deterrence-oriented laws do not generate detectable effects on fatalities, the coefficients on the calendar year dummies suggest a downward trend in fatality counts during the 1982–1986 period. The calendar year dummies in all regressions are negative and significant for years 1982–1986, while they are positive and significant for 1975–1981. The decline appears to be especially large in magnitude for the alcohol-sensitive fatality counts, SVOFN and SVOF, where the values for 1982–1986 range from −0.10 to −0.23. A possible explanation for this pattern is that deterrence is operating through a change in national norms about drinking and driving, rather than through driver response to specific punitive laws.

7. CONCLUSION

Snortum[36] and Howland[11] have independently identified a paradox in the evaluations of recent American policy toward drinking and driving. On a national basis, trends in alcohol-related fatality counts suggest that drinking and driving is on the decline. Our earlier time-series analysis revealed that the national campaign against drinking and driving is associated with up to a 20% decline in alcohol-related fatalities in the mid-1980s.

[13] We do not report these results here. Results are available from the authors upon request.

Results from recent surveys suggest a similar trend.[36] Despite these encouraging signs, the recent literature has yet to identify a specific law (except minimum drinking-age legislation) that is unequivocally responsible for a significant decline in drinking and driving. In this paper, which is the most comprehensive evaluation of deterrence-oriented policies to date, we find *no* evidence that any specific type of punitive legislation—with the possible exception of sobriety checkpoints—is a major contributor to the success of the national campaign.

Our initial reaction to the inconclusive findings was to examine the degree of collinearity among the specific interventions. If the new laws are highly correlated with each other, the model may not be able to detect significant legislative effects. Although all states have adopted some form of anti-drunk driving policy, the pattern of enactment across states is varied. For example, the highest bivariate correlation among pairs of laws was 0.41 for PBT and ILL. All other bivariate correlations were smaller. To explore this point further, we ran separate regressions of SVOFNL on the same X_{it} vector as in Table IV and only one intervention variable at a time. In each of these regressions, the coefficients on the intervention variables were not substantially different from the results in Table IV.[14] We are, therefore, unconvinced that the inconclusive results are attributable to collinearity among the interventions.

A second possible type of collinearity involves the state-level interventions and the year dummy variables. Since most of the anti-drunk driving laws were adopted after 1982, one might speculate that the 1982–1986 year dummies are capturing the effects of the specific interventions. We do not find this to be a compelling argument. First, we note that many laws existed before 1982, which significantly reduces the correlation between the laws and a linear combination of the post-1982 year dummies. This is borne out by the fact that the post-1982 year dummies are significantly different from zero. Also, we estimated Eq. (1) without the fixed year effects. In these regressions, the results are similar to those in Table V, where SOBER, OPEN, and now PBT were shown to have statistically significant negative effects on alcohol-related fatalities. In any event, we believe it is crucial to retain the year dummies in order to disentangle changes in national norms from the effects of specific legislative interventions at the state level. Last, if significant collinearities exist between year dummies and specific interventions, we would expect that the correlation would be greatest between the belt use dummy variable and the year effects since over 30 states passed a belt use law in the 2-year period 1985–1986. However, the coefficient on the BELT variable, and the 1985–1986 year dummies are statistically significant.

In contrast to our results, Zador *et al.*[23] reported substantial evidence that illegal per se, administrative per se, and first-offense penalties were associated with declines in fatal crashes. Although a thorough analysis of the two studies is beyond the scope of this paper, we note here some significant differences in the data and methods used in the study.

First, there are a number of differences in how the state laws are coded in the two studies. We believe the differences reflect the complexity of the legislation and the simplifications that are necessary to describe legislation with dichotomous values. To test the sensitivity of our results to the dates of coding of the regulation variables, we reanalyzed our fatality data using the state laws as reported by Zador *et al.* The results of the sensitivity test do not materially change the findings that we have reported here.

Second, our analysis includes more interventions and control variables than were used by Zador *et al.* For purposes of sensitivity analysis, we reanalyzed our fatality data using only the three interventions studied by Zador *et al.* Once again, the results do not materially change the findings that we have reported here.

Finally, and most importantly, there are significant differences in the statistical methods used in the two studies. Zador *et al.* give primary emphasis to results based on the "state-pair" method. Although we acknowledge that this is a defensible analytic method, we share the reservations about the method that have been expressed by Garber.[26]

The appendix to the Zador *et al.* study includes some results based on alternate methods that are similar to the econometric methods that we have employed. Their econometric results continue to suggest intervention effectiveness, especially in the case of administrative per se laws, although there are some suggestions of sensitivity acknowledged by the authors.

The primary reason that our econometric results differ from Zador *et al.* is that we have employed a fixed-effects specification, which imposes fewer assumptions on the data-generating process than the state-pairs method or the random effects models used by Zador *et al.* Under the random-effects specification, the state effects must be orthogonal to the other exogenous variables in the equation. If the random-effects model is true and the

[14] The estimated coefficients (and standard errors) for the intervention variables when they were entered separately into the SVOFNL equation are: OPEN −0.036 (0.033); ADMIN −0.023 (0.030); ANTI 0.037 (0.029); FIRST 0.069 (0.025); ILL 0.017 (0.025); PBT −0.011 (0.027); SOBER −0.078 (0.044).

fixed-effects model is employed, the coefficient estimates will be unbiased, but the standard errors will be inconsistent. However, if the fixed-effects model is the appropriate specification and a random-effects model is used, the parameter estimates will be both biased and the standard errors inconsistent. Using a Hausman test, we reject the orthogonality assumption, thereby suggesting that the random-effects model is inappropriate. Interestingly, when we reanalyzed our data using a random-effects specification and the same right-hand-side variables as the model in the appendix to Zador *et al.*, the parameter estimates for the deterrence interventions were similar to the results in Zador *et al.*

We offer several possible explanations for our "negative" findings that merit some attention in future research. First, it is possible that more punitive laws per se are not a significant factor in the process of general deterrence. Further research should examine whether, apart from any legislative changes, the criminal justice system has intensified the enforcement of existing drunk driving laws and increased the punishment of offenders. Alternatively, the success of the national campaign may be related to an escalation in the punitive threat by police and judges that is not highly correlated at the state level with the new legislation.

Second, the success of the national campaign may be related to changing social norms and attitudes toward drinking and driving. Although new punitive legislation may contribute to or reinforce changing mores, the legislation may be primarily a reflection or byproduct of the changing social norms. According to this hypothesis, the grassroots activism of the 1980s was effective not because it stimulated deterrence through punitive legislation but because it caused the American public to reexamine its basic attitudes toward drinking and driving.

Finally, one cannot dismiss the possibility that the new deterrence-oriented legislation has in fact played a significant role in recent years. Our modeling suggests that some laws—especially those that raise the certainty of punishment—have a synergistic impact on the incidence of drinking and driving. A key to the success of future empirical work on these questions is creation of reliable, continuous measures of the punitive threat. It is possible that our characterization of the punitive laws via dummy variables is too crude an approximation to identify the deterrence process.

Regardless of how our results on deterrence are interpreted, one cannot avoid the conclusion that punitive laws are not necessarily the only or the best way to prevent drunk driving fatalities. Seat belt use laws and beer taxes appear to be far more effective than drunk driving laws. We leave it to others to assess the ethical and policy implications of these findings.

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