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Changes in Alcohol-Involved Fatal Crashes Associated With Tougher State Alcohol Legislation

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16. Abstract

From 1982 to 1988, many states implemented statutes, such as administrative suspension of driver licenses and mandatory jail terms and/or fines for persons convicted of drunk driving, intended to reduce the incidence of alcohol-related motor vehicle crashes. Two recent studies of sanction effectiveness related to changes in alcohol-related fatal crashes arrived at differing conclusions. These differences in findings appear due, in part, to the methodologies employed to assess sanction effectiveness, and the authors' determination as to what constituted a severe sanction in a particular state. To identify potentially effective sanctions and resolve inconsistencies in existing findings, NHTSA contracted with Sigmastat Inc. to assess the effectiveness of selected sanctions.

Box-Tiao time series/intervention analysis was used to measure the significance of changes in crash rates coincident with the implementation of particular sanctions. Single-vehicle nighttime (8 p.m. to 4 a.m.) driver involvements in fatal crashes per 100 fatal crash driver involvements were used as a surrogate for alcohol-related fatal crashes. Estimated changes were normalized by dividing the change in each state by the baseline rate and multiplying by 100 to produce a percentage change for each legislative sanction in each state. Six of the 17 states (35 percent) implementing mandatory administrative license suspension, either alone or in combination with other sanctions, experienced statistically significant reductions. Two of the seven states (29 percent) implementing mandatory license suspension, most often in association with an illegal per se law, experienced significant reductions. Six of the 26 states (23 percent) implementing illegal per se, alone or in combination with various sanctions, demonstrated significant reductions. Only one of the 13 states (8 percent) implementing a mandatory jail/community service sanction, typically in combination with an illegal per se statute, was found to experience a significant reduction.

These results suggest that some form of licensing sanction appears to be relatively more promising as a deterrent to drunk driving, compared with other sanctions. Differences in effectiveness among the states in this analysis suggest that many other factors associated with the implementation of sanctions, such as enforcement, public perception, etc., are important to their success.

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Section I
Introduction

A. Purpose

This report presents the results of an analysis of changes in DWI legislation conducted by Sigmastat, Inc. in accordance with Task 4 of NHTSA contract DTNH22-88-C-07045. The task specified an analysis to be conducted with the goal of identifying changes in crashes associated with changes in DWI legislation.

The study was not intended to evaluate the administrative aspects of the particular DWI legislation under investigation. That is, the analysis of crash data was intended to investigate changes associated with the implementation of specific DWI legislation, without regard to the effectiveness of implementation (specifically, changes in the number of convicted DWIs sentenced to jail/community service, the number of driver licenses revoked or suspended, the number of driver licenses administratively suspended, or conviction rates associated with the passage of illegal per se laws, etc.).

The collection of more detailed administrative measures of effectiveness was beyond the scope of this investigation. This is not to deny the importance of such information in conducting a more complete study of the cause-and-effect relationship between legislative activity and changes in crashes. However, in the interest of identifying changes in the large number of states implementing various alcohol legislation, the analysis was restricted to identifying changes *associated* with the legislation.

This study should be viewed as a first step in identifying and understanding the relationship between the various legislation studied and changes in alcohol-involved crashes. The changes observed in the various states should be viewed as indicative of candidate states for further study, to examine why some states' legislation was associated with statistically significant reductions in crashes while other states' implementations of similar legislation were associated with lesser or no reductions.

B. Background

Serious large-scale efforts to control the drunk-driving problem in the United States date back to the early 1970's, when the U. S. Department of Transportation launched its

Alcohol Safety Action Program (ASAP), with $88 million of federal funds and over $40 million of state-matching support. The program consisted of projects in 35 experimental communities across the country with the goal of demonstrating that a *systems approach*, encompassing all facets of the legislative, judicial, enforcement and rehabilitation areas, could provide a sufficiently large deterrent to drinking and driving. As a result of project activities, statistically significant reductions in fatal crash alcohol involvement were demonstrated in 12 of the 35 communities.[1]

Unfortunately, ASAP was probably before its time. In spite of demonstrating effectiveness, local programs generally were discontinued when the federal funding was exhausted. This was due in part to the large expenditures required to provide overtime police manpower, increased staff to process cases through the judiciary, and a general lack of public support for these types of drunk-driving programs.

With the coming of the 1980's, and in response to the persistent drunk-driving problem, a new wave of grass-roots activism appeared. Groups such as Mothers Against Drunk Driving (MADD) supported tougher sanctions to be levied against convicted drunk drivers. Increased media coverage brought the drunk-driving problem to the public's attention.

In light of the public's apparent desire to attack the drunk-driving problem, many states were motivated to pass new tougher legislation covering the sanctioning of convicted drunk drivers, strengthening the enforcement of existing laws, and initiating public information campaigns.

Several studies have noted a decline in alcohol-involved crashes nationally during the 1980's. Fell and Klein[2] estimated that the percentage of all drivers in fatal crashes with blood alcohol concentration (BAC) at or above 0.10 percent decreased from 30 percent in 1982 to 28 percent in 1984 (preliminary figures for 1988 show a continuing decline to 25 percent of all drivers at or above 0.10 percent BAC).[3]

Based on states with the most complete BAC data reported to the U. S. Department of Transportation, the percentage of fatally-injured passenger vehicle drivers with high BACs declined from a high of 51 percent in 1980 to a low of 40 percent in 1986.[4]

A roadside breath alcohol survey in 1986 found that the proportion of drivers with high BACs had declined by more than a third since the last national survey in 1973.[4]

A February 1988 study by the Insurance Institute for Highway Safety (IIHS)[5] estimated that in 1985 about 1,560 fewer drivers were involved in fatal crashes due to the implementation of illegal per se laws, administrative license suspension or revocation prior to conviction, and laws that mandate jail or community service for first convictions of driving under the influence. The authors estimated that if all 48 of the contiguous states adopted laws similar to those studied, and if these new laws had effects comparable to those reported, another 2,600 fatal driver involvements could be prevented each year.

On the other hand, a study prepared for the AAA Foundation for Traffic Safety by the Mid-America Research Institute, Inc.[6] studied the impact of severe mandatory penalties for drunk driving (jail, community service and loss of license). The author con-

cluded that states which have enacted severe DWI sanctions to deter first-time DWI offenders have not shown greater drunk-driving reductions than states which had no significant changes in their laws from 1980 to 1985.

A November 1988 report by Neville, Evans, and Graham[7] documented a study of the impact of eight selected drunk-driving countermeasures on alcohol-related fatalities during the period 1975-1986. The results indicated that two interventions substantially reduce alcohol-related fatalities: the use of preliminary breath tests (PBT) and sobriety checkpoints. These two interventions were estimated to have decreased fatalities by 7 percent and 12 percent, respectively; the interaction of these interventions together were estimated to have decreased single-vehicle nighttime occupant fatalities by 35 percent.

In addition, the authors concluded that:

> *"Using our most alcohol-sensitive indicator (single-vehicle occupant fatalities at night), preliminary breath test laws might have saved 394 lives per year and the combination of preliminary breath test laws and sobriety checkpoints might have saved 3,455 lives per year (in the U.S. during the years 1982-1986). The other deterrence-based countermeasures do not selectively impact on alcohol-related fatalities ..."*

In light of the divergent conclusions of these studies, it was of interest to examine the data from a slightly different perspective. The current study analyzes the fatal crash experience of individual states rather than a cross-sectional type of national analysis conducted in the IIHS study, cross-sectional analysis of trends conducted in the Mid-America Research Institute study, and the pooled analysis in the study by Neville et al.

Section II
Research Design

A. Scope of Analysis

1. Legislation

The intent of this analysis was to investigate changes in alcohol-involved crashes associated with the implementation of legislation described as *tougher* in terms of the sanctions imposed on convicted DWI offenders. The study focused on four legislative policies:

Administrative Per Se License Suspension - This policy refers to laws under which anyone arrested for an alcohol-involved driving violation can be immediately subject to license suspension/revocation. This licensing action occurs independently of the outcome of criminal charges, and in many states, is the responsibility of the department of motor vehicles or other such licensing agencies. The term *per se* is used to describe these laws because the arrest alone is sufficient to trigger their application.

Illegal Per Se - This policy refers to the definition of operating a vehicle at or above a specified BAC as a crime, regardless of the individual's ability to function at or above the specified BAC. Thus, the mere presence of a sufficiently high BAC exhibited by a person operating a motor vehicle is *per se* evidence of intoxication, and is a crime. There is no defense to a per se charge other than possible objections to the test or arrest procedures.

Mandatory Jail (or Community Service in lieu thereof) - All states have provisions for sentencing convicted drunk drivers to jail or community service. The laws selected for study in this research require a mandatory jail sentence of at least 24 consecutive hours for first-time convicted drunk drivers. However, in lieu of this, the court in some states may sentence the convicted drunk driver to perform community service as a substitute. Exceptions to the mandatory nature of the sanction are noted.

Mandatory License Suspension - This policy represents the mandatory suspension/revocation of the driver license, by the court, for convicted drunk drivers, and is independent of any administrative license suspension mandated by state law. In some

states, a *hardship* license may be granted by the court, allowing the convicted offender to drive to and from work and/or any court-mandated treatment facilities.

The relative importance of this legislation was based on the fact that a large number of states have implementated each of these separately or in combination with one another, and that recent research studies[5,6,7] have specifically investigated the effects of this legislation on fatal crashes. These studies reached somewhat divergent conclusions regarding the effectiveness of this legislation, and it was of interest to investigate these data from a different perspective.

Initial investigations focused on identifying which legislation was present in which states, and on what date such legislation was implemented. This information was not available from a single document or database, and had to be abstracted from a number of sources.

The major sources of information included:

- Digest of Traffic Safety Alcohol Legislation[8],
- Annual Review of New Alcohol Legislation[9], and
- Previous research reports (IIHS and AAA)[5,6].

The information abstracted from these reports appears in Exhibit II-1, presenting, for each state, the presence of specific legislation and the dates of implementation. (Reference 10 lists a summary of legislation that was compiled and printed after the current effort.)

As can be seen, a large number of states implemented various combinations of the four policies under study (administrative per se, illegal per se, and mandatory jail/community service, mandatory license suspension) during the period 1975 to 1987. Many states simultaneously implemented combinations of these policies in waves of legislative activity, probably due to the increased public pressure to contain the drunk-driving problem. Thus, one is limited in the ability to distinguish between/among policies that were implemented simultaneously within each state.

Some legislative policies were implemented too early in the 1975-1987 time period to allow investigation. For example, Exhibit II-1 shows that Arizona and Arkansas implemented mandatory jail in January 1975, and Florida implemented Illegal Per Se in January 1975; no analysis of before vs. after change was conducted for these early policy implementations since they were in effect during the entire study period.

To more clearly present the various combinations of legislation implemented simultaneously, the Exhibit II-2 arranges the states within a matrix, according to their particular legislative combinations.

For example, the table shows that nine states (IL, ME, MN, MO, NC, OK, OR, WV, WY) implemented administrative per se alone, while five states (CO, IN, LA, MS, NM) implemented administrative per se at the same time as illegal per se. One state (UT) implemented administrative per se and mandatory license suspension together, one state (IA) implemented administrative per se together with mandatory jail and illegal per se, and one state (NV) implemented administrative per se, mandatory jail, illegal per se and mandatory license suspension all

State	Admin Per Se Law	Illegal Per Se Law	Mand. Jail (1st Off.)	Mand. License Susp.	21 MLDA Effective Date
AL		09-80		Yes	10-85
AK	07-83			Yes	11-84
AZ		08-82	01-75*	07-82	11-85
AR		04-83	01-75(8)		XX-35
CA		03-82			XX-33
CO	07-83	07-83(4)			07-87
CT		10-85	10-82*(9)	Yes	09-85
DE	03-83	03-83		02-82	01-84
DC				XX-54	10-86
FL		01-75			07-85
GA		09-83(5)			09-86
HI				05-83	10-86
ID		03-84			04-87
IL	01-86	01-82			XX-80
IN	09-83	09-83		Yes	XX-34
IA	07-82	07-82(6)	07-82(10)		10-86
KS		07-85	07-82*	07-76	07-85
KY				Yes	XX-38
LA	01-84	01-84	01-83*		03-87
ME	01-84	10-81	10-81	Yes	07-85
MD					07-82
MA				Yes	06-85
MI		04-83			XX-78
MN	09-78(1)	01-75			09-86
MS	07-83	07-83		Yes	10-86
MO	10-83	10-75			XX-45
MT		10-83	10-83(11)		04-87
NE		01-75		07-82	01-85
NV	07-83	07-83	07-83*	07-83	XX-35
NH		09-83		Yes	06-85
NJ		04-83		XX-37	01-83
NM	07-84	07-84			XX-34
NY		01-75			12-85
NC	10-83	01-75			09-86
ND	07-83	07-83		07-81	XX-36
OH		04-83	04-83(12)		07-87
OK	07-83	07-82		Yes	11-85
OR	07-84	07-75(7)	03-85*		XX-33
PA		01-83		01-83	XX-35
RI		07-83		Yes	07-84
SC			06-83*		09-86
SD		01-75			04-88
TN			07-82		08-84
TX		01-84			10-86
UT	08-83	01-75(7)	03-82*	07-83	XX-35
VT		01-75		Yes	07-86
VA		07-85			07-85
WA		09-79	09-79	Yes	XX-34
WV	09-81(2)			01-75	07-86
WI		05-82		05-82	09-86
WY	04-84(3)				07-88

Notes:

* Community service may be imposed in lieu of jail.

(1) Strengthened in 1982.

(2) Corrected in 1983 and 1986.

(3) Improved in March 1986.

(4) BAC $\geq$ 0.15.

(5) BAC $\geq$ 0.12.

(6) BAC $\geq$ 0.13.

(7) BAC $\geq$ 0.08.

(8) Policy ended 04-83.

(9) Policy ended 10-83.

(10) 48 hours minimum; but may be suspended.

(11) 48 hours minimum; may only be suspended for defendent's physical/mental well being.

(12) 3 days minimum; may be suspended for attendance at driver intervention program for 3 consecutive days.

Yes denotes that the policy was implemented, but date of implementation was not known.

Exhibit II-1 **Summary of Selected Alcohol Legislation and Policies**

Admin	Admin Licen	Admin Illeg	Admin Jail Illeg	Admin Jail Licen Illeg	Illeg	Jail Illeg	Licen Illeg	Jail	Licen
							AZ		
					AR				
					CA				
		CO							
					CT			CT	
					GA				
									HI
					ID				
IL					IL				
		IN							
			IA						
					KS			KS	
		LA						LA	
ME						ME			
					MI				
MN									
		MS							
MO									
						MT			
									NE
				NV					
					NJ				
		NM							
NC									
						OH			
OK					OK				
OR								OR	
							PA		
								SC	
								TN	
					TX				
	UT							UT	
					VA				
						WA			
WV									
							WI		
WY									

Exhibit II-2 **States Analyzed by Legislative Combination**

at the same time. Other combinations are also presented.

The presence of so many legislative combinations necessitated analyzing states individually. While any single analysis of states with concommitantly implementated legislation is limited in terms of estimating individual legislative policy effectiveness, it was hoped that a review of the results, ag-

gregated across many states, could shed light on the potential effectiveness of each individual legislative policy.

2. Data

An in-depth investigation into the effectiveness of tougher anti-drunk driving legislation would require the collection and analysis of several components of detailed information, typically categorized as administrative evaluation and impact evaluation.

Administrative evaluation addresses the activity associated with achieving program objectives, identifying performance measures (e.g., the numbers of DWI convictions, administrative license suspensions, convicted DWIs sent to jail/community service, etc.) to track program progress compared with some baseline time frame. Other data are also important: measuring changes in the target audience's knowledge of the program, attitudes toward program objectives, and self-reported behavior changes due to program activities.

For example, the effectiveness of a mandatory jail/community service law for first offender DWIs should be related to a number of factors, including: the driving public's knowledge of the mandatory sentence, the public's attitude toward the act of drinking and driving, the frequency with which the mandatory sentence is applied, etc. Thus, it is critical to document changes in program activity in order to understand why some programs are effective in reducing crashes while others are not. Statistical tests merely demonstrate whether a statistically significant change has occurred during the period of program activities; the administrative evaluation provides the data used to establish causality.

The impact evaluation component involves the collection and analysis of both intermediate and ultimate performance measures. For example, an intermediate measure of program effectiveness might include changes in self-reported drunk driving behavior attributable to the tougher sanctions. The ultimate goal of this legislation is the reduction of alcohol-related crashes, especially fatal crashes. Since alcohol is much more prevalent in fatal crashes than in crashes of lesser severity, a measure of fatal crash involvement is the metric of choice.

Demonstrating causality is, therefore, a three-stage process: (1) establishing that a positive change has occurred during the period of program activities, (2) demonstrating that sufficient program activity has been generated to effect positive changes in the target population's knowledge, attitudes and behaviors, and (3) eliminating competing hypotheses regarding the effects of other non-program factors such as socio-economic, legislative, technological, etc.

Such an in-depth study, conducted in a large number of states, requires enormous financial and personnel resources, the cost of which were well beyond the study constraints. The objective of this study was to investigate the crash experience in a large number of states, and to document changes in alcohol-related crashes coincident with the implementation of the four major legislative policies. The existence of these legislative policies are to be associated with changes in alcohol-related crashes during the respective time periods of implementation.

In order to cover a large number of states, documenting changes in alcohol-related crashes is at least a first step an overall research effort. The results of this investigation

provide an overview to the potential effectiveness of the four legislative policies, and identifies states in which the various policies can be associated with reductions in crashes. From these findings, one can identify candidate states for more in-depth investigation of program activities, with the goal of examining why the same legislation, implemented in two states, can be associated with a reduction in crashes in one state, and no change in others.

The best measure of alcohol involvement in fatal crashes would be the actual BACs exhibited by drivers involved, these data being maintained on the Fatal Accident Reporting System (FARS). Unfortunately, BAC data are not available for more than half of all drivers, and as one goes back in time, BAC reporting is even less complete. Exhibit II-3 presents the proportion of known BAC test results on FARS for 1982 through 1988.

Year	Number of Drivers	Known Test Results	Proportion Known
1982	56,029	18,489	0.33
1983	54,656	18,789	0.34
1984	57,512	21,985	0.38
1985	57,883	24,068	0.42
1986	60,335	26,478	0.44
1987	61,442	27,362	0.45
1988*	62,237	27,442	0.44

Note: * indicates preliminary estimate
(FARS 1988 subm 202, 19 May 89)

Exhibit II-3 **Proportion of Known BAC Test Results on FARS**

A surrogate measure of alcohol involvment is required in order to avoid the pitfalls associated with using only those cases with known BAC. These pitfalls include the well-recognized bias in the selection of drivers for BAC testing (the tendency to test those drivers who appear to be intoxicated) and the changes in known BAC distributions resulting from the historically increasing trend in the proportion of drivers tested.

The measure selected for analysis was the rate of single-vehicle nighttime (8 p.m. to 4 a.m.) fatal crash driver involvements per 100 fatal crash driver involvements. Single-vehicle nighttime fatal crashes have been widely used as a surrogate measure for the prevalence of alcohol in fatal crashes. The measure is more objective than the incomplete BAC data in that it is not subject to the selection bias inherent in actual BAC data. In addition, single-vehicle nighttime fatal crashes historically have been highly associated with driver alcohol involvement, and drivers involved in this category of crash continue to exhibit a very high rate of alcohol involvement, as presented in Exhibit II-4.

Year	Number of Drivers with Test Results	Proportion with BAC >= 0.10
1982	4,866	0.77
1983	4,827	0.77
1984	5,354	0.75
1985	5,390	0.73
1986	6,314	0.72
1987	6,330	0.70
1988*	6,334	0.71

Note: * indicates preliminary estimate
(FARS 1988 subm 202, 19 May 89)

Exhibit II-4 **Known BAC Test Results for Drivers Involved in Single-Vehicle Nighttime Fatal Crashes**

As can be seen in Exhibit II-4, over 70 percent of the drivers in single-vehicle nighttime fatal crashes with known BAC test

results consistently exhibit BAC at or above 0.10 percent.

A potential source of confounding in the analysis is the presence of other factors that could affect the overall level of crashes, such as economic, climatic, or technological. One method of attempting to account for this is the use of comparison series, such as daytime fatal crashes, to investigate changes that occurred concommitant with the legislative implementation.

In this study, the measure of change that was used was the rate of single-vehicle nighttime fatal crash driver involvements per 100 fatal crash driver involvements. Using this measure, the overall level of crash involvement was explicitly represented in the models by forming the resultant ratio. Thus, factors that would be expected to affect the overall level of crashes could be accounted for in the analysis. Observed changes in the *rate* of single-vehicle nighttime fatal crashes were hypothesized to represent changes in the rate of driver alcohol involvement in fatal crashes, and not be confounded with changes due to other exogenous factors.

One area of potential confounding not addressed by the above ratio, involves the implementation of 21 year old drinking age laws in a large number of states. A change in the minimum legal drinking age would be expected to have a differential effect on single-vehicle nighttime fatal crashes, and needs to be accounted for explicitly.

In many states these laws were enacted as part of a legislative package that included other laws such as mandatory jail or administrative per se. Since the effects of two simultaneous legislative changes cannot be statistically separated, it was important to account for the raised drinking age, while still providing as clear a picture as possible of the four legislative policies to be studied.

To address this issue, the analysis was divided into two strata: drivers age 21 years and older, and drivers less than 21 years of age. This stratification provided the ability to estimate changes in crashes for drivers 21 + years old without the confounding influence of the raised minimum legal drinking age laws. Drivers age 21 + years form the largest portion of the drunk-driving problem, and should be the main focus of the study effort. Analyses were conducted for each driver age group independently.

B. Methodology

The statistical method employed was Box-Tiao time series/intervention analysis[11]. The method involves the use of linear difference equations producing an analysis that is analogous to linear regression modeling, with the advantage of permitting a more generalized error structure. The application of linear regression models requires the assumption of normally and independently distributed error; accident-related data generally exhibit serial and/or seasonal correlation, the presence of which negates the assumption of independently distributed error terms. The presence of serial/seasonal correlation in the error terms results in biased estimates of the variance of computed estimates, such as the regression-type coefficients used in this study, yielding unreliable t-statistics for testing hypotheses. The time series approach permits the modeling of the more generalized error structure, and permits the introduction and estimation of intervention, or *dummy*, variables that represent the absence/presence of a condition.

The method has been applied in a large number of major evaluation efforts, including the U. S. Department of Transportation's Alcohol Safety Action Projects[1], the introduction of the 55 mph NMSL[12], and changes in the minimum legal drinking age[13,14].

Because of the possible presence of more complex error structure, no estimate of model fit, analogous to the R-squared statistic for linear regression, is available. In linear regression, the square of the multiple correlation (R-squared) represents the proportion of variance explained by the introduction of predictor variables. Since the more complex error structure requires the use of parameters for fitting models, these parameters contribute to the reduction of unexplained variation, confounding the intended interpretation of the R-squared statistic.

The measure of change selected for this effort was the rate of single-vehicle nighttime fatal crash driver involvements per 100 fatal crash driver involvements. The objectivity of this measure over time permitted the use of a long time period for study. FARS data for the period January 1975 through December 1987 was used, providing thirteen years of data, or 156 monthly data points. This period of time is more than sufficient to take full advantage of the use of the time series/intervention analysis technique.

Intervention variables were defined as taking the value one for months during which the particular legislation was in effect, and the value zero otherwise. Each legislative policy was represented by a separate intervention variable, except when two or more legislative policies were implemented simultaneously. In this case, the intervention variable represented the combination of policies implemented (e.g., illegal per se and mandatory jail, etc.). Exhibit II-2, on page 7, presents the various combinations of legislation implemented simultaneously for each state.

In designing a test of hypothesis, one proposes a null hypothesis that is to be rejected based on the evidence, and selects an alternate hypothesis based on the desired research question.

The issue in this effort is whether states implementing various legislative policies are associated with better alcohol-involved fatal crash experiences (i.e., lower rates of single-vehicle nighttime driver involvements) during the period of implementation compared with the appropriate baseline period. The key word here is *lower*. A standard two-tailed hypothesis test would indicate whether the test statistic was different from zero, either greater than or less than. The null hypothesis in this study is that the rate during the (for example) jail policy time period is not less than the rate otherwise. This indicates the need for a one-tailed test, where the alternate hypothesis is that the rate during the (for example) jail policy time frame is less than the rate during other time frames.

It is not the belief that these policies could not result in greater rates of alcohol-involved fatal crashes that motivates one to select a one-tailed hypothesis test. It is the desire to determine whether one policy is better than another and not whether it is *different* (greater than or less than), since if the presence of the policy is equal to or *worse* than its absence, one would conclude that the policy was ineffective.

The benefit of using a one-tailed hypothesis test is that it is a more powerful test of the research hypothesis; that is, the test will

reject the null hypotheses more often if the alternate hypothesis is true. Since this research effort attempts to determine the potential effectiveness of various legislative policies to combat drunk driving, it is important to identify as many potentially effective policies as practicable for further study. Thus, a one-tailed test with alpha = 0.05 was selected. This resulted in a rejection region (using Student's t-distribution) of $t \leq -1.645$ for the test of the intervention coefficients estimated using time series analysis.

Section III
Results

A. Individual State Estimates

The analysis provided estimates of change for two groups of drivers (under 21 years of age, and 21 years and older) within each state. Exhibit III-1 presents the results of the analysis for both driver age groups.

Table sub-entries are t-test values; arrows indicate the simultaneity of legislative policy implementation, and point to the result which represents all of the legislation implemented at the same time, also indicated by boldface entries. For example, for the state of Arizona, the arrow under the illegal per se column indicates that the estimate under the mandatory license suspension column represents the combined estimate for both legislative policies; note that illegal per se was implemented in August 1982 and mandatory license suspension was implemented in July 1982, too close in time for separate estimation. Statistically significant estimates (alpha of 0.05 percent, one-tailed test) are indicated by t-test values less than or equal to -1.645.

- For drivers age 21 and older, eleven of the 37 states analyzed exhibited statistically significant reductions in the rate of driver fatal crash alcohol involvement during the time frame when tougher sanctions for DWI were in effect.

- For drivers under the age of 21 years, six of the 30 states analyzed exhibited statistically significant reductions in the rate of driver fatal crash alcohol involvement during the time frame when tougher sanctions for DWI were in effect (7 states had sample sizes too small for meaningful analysis).

Of the six states exhibiting statistically significant reductions for drivers under the age of 21 years, three (MS, NJ, TN) were associated with the implementation of 21 year old minimum legal drinking age laws, two (NC, WV) were associated with the implementation of administrative per se laws, and one (MI) was associated with the implementation of illegal per se laws.

Most drivers under the age of 21 years currently are covered by MLDA laws, and generally failed to exhibit reductions in the rate of fatal crash driver involvement associated with the four legislative policies under study. This may be due to several fac-

State	Admin Per Se Law	Illegal Per Se Law	Mand. Jail (1st Off.)	Mand. License Susp.	21 MLDA Effective Date
AZ		**08-82**	*01-75**	**07-82**	11-85
(21+)		----->		-1.83	
(<21)		----->		-0.87	0.45
AR		04-83	*01-75* (8)		XX-35
(21+)		-1.37			
(<21)		0.60			
CA		03-82			XX-33
(21+)		-5.20			
(<21)		-1.43			
CO	**07-83**	**07-83**	(4)		07-87
(21+)	-1.70	<-----			
(<21)	-1.20	<-----			0.49
CT		10-85	10-82*	(9)Yes	09-85
(21+)		0.87	-0.66		
(<21)		----->	-0.87		0.26
GA		09-83	(5)		09-86
(21+)		-1.34			
(<21)		0.13			-1.28
HI				05-83	10-86
(21+)				1.37	
(<21)	(Sample size too small)				
ID		03-84			04-87
(21+)		0.25			
(<21)	(Sample size too small)				
IL	01-86	01-82			XX-80
(21+)	-1.91	1.98			
(<21)	0.65				1.17
IN	**09-83**	**09-83**		Yes	XX-34
(21+)	-1.33	<-----			
(<21)	0.17	<-----			
IA	**07-82**	**07-82**?	(6)**07-82**	(10)	10-86
(21+)	----->	0.39	<-----		
(<21)	----->	-0.79	<-----		-0.62
KS		07-85	07-82*	*07-76*	07-85
(21+)		1.01	0.04		
(<21)			0.47		-0.48
LA	**01-84**	**01-84**	01-83*		03-87
(21+)	0.12	<-----	0.11		
(<21)	0.79	<-----	0.05		-1.20
ME	01-84	**10-81**	**10-81**	Yes	07-85
(21+)	-2.45	----->	1.23		
(<21)	(Sample size too small)				

State	Admin Per Se Law	Illegal Per Se Law	Mand. Jail (1st Off.)	Mand. License Susp.	21 MLDA Effective Date
MI		04-83			XX-78
(21+)		-1.74			
(<21)		-1.84			0.97
MN	09-78	(1)*01-75*		09-86	
(21+)	(0.32,-0.65)				
(<21)	(0.97,-0.71)				-0.42
MS	**07-83**	**07-83**		Yes	10-86
(21+)	0.05	<-----			
(<21)	1.76	<-----			-2.71
MO	10-83	*10-75*			XX-45
(21+)	-0.27				
(<21)	-1.07				
MT		**10-83**	**10-83**(11)		04-87
(21+)		----->	-0.47		
(<21)	(Sample size too small)				
NE		*01-75*		07-82	01-85
(21+)				-0.54	
(<21)				0.51	-0.55
NV	**07-83**	**07-83**	**07-83***	**07-83**	XX-35
(21+)	----->	----->	-0.52	<-----	
(<21)	(Sample size too small)				
NJ		04-83		*XX-37*	01-83
(21+)		-2.01			
(<21)		0.67			-2.49
NM	**07-84**	**07-84**			XX-34
(21+)	-2.18	<-----			
(<21)	-1.06	<-----			
NC	10-83	*01-75*			09-86
(21+)	-2.32				
(<21)	-4.82				-1.42
OH		**04-83**	**04-83**(12)		07-87
(21+)		----->	0.15		
(<21)		----->	0.54		-0.02
OK	07-83	07-82		Yes	11-85
(21+)	0.10	-0.67			
(<21)	0.77	-0.20			-0.02
OR	07-84	*07-75*(7)	03-85*		XX-33
(21+)	0.06		-2.39		
(<21)	-1.05		0.02		
PA		**01-83**		**01-83**	XX-35
(21+)		----->		0.23	
(<21)		----->		-0.31	

Exhibit III-1 **Individual State Estimates For Each Legislative Policy (Part 1)**

State	Admin Per Se Law	Illegal Per Se Law	Mand. Jail (1st Off.)	Mand. License Susp.	21 MLDA Effective Date
SC			06-83*		09-86
(21+)			0.72		
(<21)			-1.63		0.94
TN			07-82		08-84
(21+)			0.16		
(<21)			1.39		-2.64
TX		01-84			10-86
(21+)		1.34			
(<21)		0.34			-0.21
UT	**08-83**	*01-75* (7)	03-82*	**07-83**	XX-35
(21+)	----->		0.89	-1.80	
(<21)	(Sample size too small)				
VA		07-85			07-85
(21+)		0.31			
(<21)					-0.19
WA		**09-79**	**09-79**	Yes	XX-34
(21+)		----->	0.36		
(<21)		----->	-1.23		
WV	09-81 (2)			*01-75*	07-86
(21+)	1.89				
	0.16				
	-0.92				
(<21)	1.95				-0.43
	-2.51				
	0.98				
WI		**05-82**		**05-82**	09-86
(21+)		----->		0.16	
(<21)		----->		-0.99	-0.08
WY	04-84 (3)				07-88
(21+)	0.68				
(<21)	(Sample size too small)				

Notes:
* Community service may be imposed in lieu of jail.
(1) Strengthened in 1982.
(2) Corrected in 1983 and 1986.
(3) Improved in March 1986.
(4) BAC $\geq$ 0.15.
(5) BAC $\geq$ 0.12.
(6) BAC $\geq$ 0.13.
(7) BAC $\geq$ 0.08.
(8) Policy ended 04-83.
(9) Policy ended 10-83.
(10) 48 hours minimum; but may be suspended.
(11) 48 hours minimum; may only be suspended for defendent's physical/mental well being.
(12) 3 days minimum; may be suspended for attendance at driver intervention program for 3 consecutive days.

Exhibit III-1 **Individual State Estimates For Each Legislative Policy (Part 2)**

tors including the fact that drivers under 21 years old are disenfranchised from public drinking by MLDA laws, and they may not be suitably deterred by the mandatory sanctions due to the difference in lifestyle between teenagers and older adults, producing a lesser reliance on driving. Since older drivers comprise the greatest portion of drivers involved in alcohol-related fatal crashes, and a large body of research[15] has been completed on the effects of MLDA laws, the remainder of the discussion will focus on the analytical results for drivers age 21 years and older.

For drivers age 21 years and older, of the eleven states that exhibited statistically significant reductions in driver fatal crash alcohol involvement:

- 3 (IL, ME, NC) were associated with administrative per se license suspension alone,
- 2 (CO, NM) were associated with administrative per se and illegal per se simultaneously,
- 1 (UT) was associated with administrative per se and mandatory license suspension simultaneously,
- 3 (CA, MI, NJ) were associated with illegal per se alone,
- 1 (AZ) was associated with mandatory license suspension and illegal per se simultaneously, and
- 1 (OR) was associated with mandatory jail/community service alone.

These summary results are presented in Exhibit III-2.

B. Summary Legislative Estimates

To estimate changes in driver fatal crash alcohol involvement associated with the four legislative policies, the individual state estimates were pooled to provide summaries of the legislative experiences.

To facilitate the comparison of results among the individual states, the estimated changes were normalized by dividing the change in each state by the baseline rate and multiplying by 100, producing a percentage change for each policy (or combination thereof) for each state. The baseline rate was computed as the mean of the series after accounting for the changes associated with each states' respective legislative policy implementations, and not necessarily the pre-policy time frame.

	Admin	Illeg	Jail	Licen
Admin	**IL *** **ME *** MN MO **NC *** OK OR WV WY			
Illeg	**CO *** IN LA MS **NM ***	AR **CA *** CT GA ID IL KS **MI *** **NJ *** OK TX VA	ME MT OH WA	**AZ *** PA WI
Jail			CT KS LA **OR *** SC TN UT	
Licen	**UT ***			HI NE
Jail/Illeg	IA			
Jail/ Illeg/Lic	NV			

Note: Asterisk/boldface indicates statistical significance of t-test of intervention coefficient (alpha = 0.05, one-tailed test)

Exhibit III-2 **Results of Analysis by Legislative Combination (Drivers 21+)**

Exhibit III-3 presents the percentage changes for each state for each combination of legislative policies.

- For the 17 states implementing administrative per se laws either alone or in combination generally with illegal per se

laws, 6 states (35 percent of the states in the group) exhibited statistically significant reductions in the rate of driver fatal crash alcohol involvement (IL, ME, NC, CO, NM, UT). For the group as a whole, the estimated changes range from a decrease of 30 percent (Maine) to an increase of 7 percent (Wyoming) with a median 6 percent decrease.

- For the 26 states implementing illegal per se alone or in combination, 6 states (23 percent) exhibited statistically significant reductions in the rate of driver fatal crash alcohol involvement (AZ, CA, CO, MI, NJ, NM). For the group as a whole, the estimated changes range from a decrease of 11 percent (California, Michigan, New Jersey) to an increase of 14 percent (Maine, implemented at the same time as

	Admin	Illeg	Jail	Licen		Admin	Illeg	Jail	Licen
Admin	**IL *** **ME *** MN MO **NC *** OK OR WV WY				**Admin**	**-11%*** **-30%*** - 7% - 1% **-11%*** 1% 1% 2% 7%			
Illeg	 **CO *** IN LA MS **NM ***	AR **CA *** CT GA ID IL KS **MI *** **NJ *** OK TX VA	 ME MT OH WA	 **AZ *** PA WI	**Illeg**	 **- 6%*** - 7% 1% 1% **- 9%***	- 9% **-11%*** 7% - 6% 3% 8% 10% **-11%*** **-11%*** - 6% 3% 1%	 14% - 3% 1% 2%	 **- 8%*** 1% 1%
Jail			CT KS LA **OR *** SC TN UT		**Jail**			- 4% 0% 1% **-30%*** 3% 1% 11%	
Licen	**UT ***			HI NE	**Licen**	**-23%***			15% - 5%
Jail/Illeg	IA				**Jail/Illeg**	4%			
Jail/ Illeg/Lic	NV				**Jail/ Illeg/Lic**	- 6%			

Note: Asterisk/boldface indicates statistical significance of t-test of intervention coefficient (alpha = 0.05, one-tailed test)

Exhibit III-3 **Percentage Changes for Each State by Legislative Combination (Drivers 21+)**

mandatory jail) with a median 1 percent increase.

- For the 13 states implementing mandatory jail/community service alone or in combination generally with illegal per se laws (this includes all entries in the **jail** column plus IA and NV from the administrative per se column), one state (8 percent) exhibited a statistically significant reduction in the rate of driver fatal crash alcohol involvement. The estimated changes for all 12 states range from a decrease of 30 percent (Oregon) to an increase of 14 percent (Maine, implemented at the same time as illegal per se) with a median 1 percent increase.

- For the 7 states implementing mandatory license suspension alone or in combination generally with illegal per se laws (this includes all entries in the **license** column plus UT and NV), two states (29 percent) exhibited statistically significant reductions in the rate of driver fatal crash alcohol involvement. For the group as a whole, the estimated changes range from a decrease of 23 percent (UT, implemented at the same time as administrative per se) to an increase of 15 percent (Hawaii) with a median 5 percent decrease.

	Admin	Illeg	Jail	Licen
Admin				
prop.sign.	0.33			
med.chg.	-1%			
# states	9			
Illeg				
prop.sign.	0.40	0.25	0.00	0.33
med.chg.	-6%	-2%	1%	1%
# states	5	12	4	3
Jail				
prop.sign.			0.14	
med.chg.			1%	
# states			7	
Licen				
prop.sign.	1.00			0.00
med.chg.	-23%			5%
# states	1			2
Jail/Illeg				
prop.sign.	0.00			
med.chg.	4%			
# states	1			
Jail/Ill/Lic				
prop.sign.	0.00			
med.chg.	-6%			
# states	1			
Policy Totals				
prop.sign.	0.35	0.23	0.08	0.29
med.chg.	-6%	1%	1%	-5%
# states	17	26	13	7

Exhibit III-4 **Summary of Analysis by Legislative Combination (Drivers 21+)**

Exhibit III-4 presents the proportion of statistically significant results, the median change and the number of states for each cell of the matrix and for the marginal policy totals (which include some overlap).

The summary information suggests that some form of license sanction (either administrative per se, mandatory license suspension/revocation, or both in combination) holds the most promise for deterring drunk driving, with 32 percent of the licensing-sanction (administrative per se and mandatory license suspension/revocation) states exhibiting statistically significant reductions in driver fatal crash alcohol involvement during the period of implementation. While in force, administrative per se laws were associated with a median change of a seven percent decrease. Mandatory license suspension/revocation laws were associated with a median change of a five percent decline.

On the other hand, of the states implementing mandatory jail/community service policies, only 8 percent exhibited statistically significant reductions, with the group as a whole posting a median change of a one percent increase during the period of implementation. These results do not support the case for jail as a deterrent to drunk driving.

For illegal per se laws, 23 percent of the implementing states were associated with statistically significant reductions in driver fatal crash alcohol involvement, with a median change for the group of a one percent increase.

C. Effect of Economic Factors

One of the major threats to the validity of the hypothesized association of changes in fatal crash driver involvement with the legislation under study would be the effects of exogenous factors not accounted for in the analytical models. In other words, observed changes in driver involvements associated with alcohol legislation might be confounded with changes due to, for example, changes in economic conditions. Previous studies have documented the relationship of total fatalities with changes in the economy, as represented by the civilian unemployment rate. Decreases in the unemployment rate are generally associated with increases in total fatalities. In addition, it has been proposed that economic factors also affect the distribution of the types of crashes that occur (e.g., single- vs. multi-vehicle crashes).

In the current study, the measure selected was the ratio of single-vehicle nighttime fatal crash driver involvements to all driver involvements. By forming this ratio it was hoped that factors that would affect all crashes would be accounted for. However, after initial analyses were conducted, it was proposed that the effect of economic factors be investigated.

To investigate this possibility, unemployment data for each state, for the period 1978-1987 were obtained from the U. S. Department of Commerce's Bureau of Labor Statistics. Consistent data were not available for the period before 1978.

Since the hypothesized effect of economic factors threatened the validity of the conclusions, the first step of this analysis focused on the eleven states (AZ, CA, CO, IL, ME, MI, NJ, NM, NC, OR, and UT) that exhibited statistically significant reductions associated with their particular legislation. If the inclusion of the unemployment rate in the models changed the results, then further investigations would be warranted.

Of the eleven states analyzed, the coefficient for the unemployement rate was statistically significant (at the alpha = 0.05 level, two-tailed test) in four cases (AZ, CA, NM, and NC). In each case, the magnitude and statistical significance of the estimate associated with the alcohol legislation was relatively unchanged.

For Arizona, the model without the unemployment rate produced an estimated monthly reduction of 1.58 compared with a reduction of 2.20 for the model with the unemployment rate. For California the results were a reduction of 2.82 (without unemployment rate) compared with 2.95 (with); for New Mexico the estimates were a reduction of 2.33 compared with 2.68, respectively; for North Carolina the estimates were a reduction of 2.38 compared with 2.90, respectively.

In each case, the estimated reduction associated with the alcohol legislation increased slightly in the models that included the unemployment rate. In every model, the coefficient of the unemployment rate was positive, indicating that decreases in the unemployment rate tended to be associated with decreases in the rate of single-vehicle nighttime driver involvements. This result is consistent with the results presented in the paper by Neville et al[7], wherein the unemployment rate was found to play a significant role (negative relationship) in the models of total fatalities, but not for the models of single-vehicle nighttime fatalities. Decreases in unemployment, such as those experienced during the mid- to late-1980's would be associated with increases in total fatalities.

The ratio used in the current analyses would be expected to decrease as a result of the expected increase in the denominator (total driver involvements) resulting from decreases in the rate of unemployment, while the numerator (single-vehicle nighttime driver involvements) would be unaffected. However, the models which included the unemployment rate produced results that were relatively unchanged compared with earlier models (actually, the estimated reductions associated with the alcohol legislation increased slightly when the unemployment rate was included).

Thus, while the inclusion of the unemployment rate was statistically significant in four of the eleven models, in each case the resulting estimates of change associated with the alcohol legislation were unaffected, as were the conclusions regarding the association of observed changes with the particular legislation. Based on these results, the previous conclusions regarding the alcohol legislation were unchanged, and no further analyses of the effect of economic factors were conducted.

D. Reconciliation With Other Studies

In February 1988, the Insurance Institute for Highway Safety issued a report titled *Fatal Crash Involvement and Laws Against Alcohol-Impaired Driving*[5]. The study focused on illegal per se, administrative per se license suspension/revocation prior to conviction for driving under the influence, and mandatory jail or community service for first convictions of driving under the influence.

The authors used weighted multivariate regression of the log-odds ratio to estimate changes in driver involvements associated with various alcohol legislation, using geographically contiguous states to control for the effects of other factors.

The study was based on all drivers age 21 or older who were in fatal crashes during the years 1978-1985 in the 48 contiguous states. Drivers age 20 and younger were excluded from this study to avoid confounding the results with the documented effects of raising the minimum legal drinking age. (This was also done in the current study through stratification by age groups.) In addition, motorcycle crashes were excluded, as were drivers in crashes with four or more vehicles.

The authors concluded that about 1,560 fewer drivers were involved in fatal crashes because of these three types of drinking-driving laws. It was estimated that another 2,600 fatal driver involvements could be prevented if all 48 of the contiguous states adopted similar laws and they had comparable effectiveness.

The authors go on to state that:

"During hours when typically at least half of all fatally injured drivers have a BAC over 0.10 percent, administrative suspension/revocation is estimated to reduce the involvement of drivers in fatal crashes by about 9 percent; during the same hours, first offense mandatory jail/community service laws are estimated to have reduced driver involvement by about 6 percent. The effect of per se laws was estimated to be a 6 percent reduction during hours when fatal crashes typically are less likely to involve alcohol."

For the current study, the estimated median change of a 6 percent reduction associated with administrative per se license suspension is comparable to the 9 percent estimated reduction found in the IIHS study. However, the estimate for illegal per se (a 6 percent reduction during lower-alcohol hours) is not based on a comparable time period; during the high-alcohol hours, the measure comparable to that used in the current study, the estimated 2 percent increase associated with illegal per se is very close to the current study results (a median 1 percent increase). The current study found a median change of a 1 percent increase for mandatory jail/community service, compared with an estimated reduction in the IIHS study of 6 percent. The estimated change associated with mandatory jail/community service cannot be reconciled.

It should be pointed out that the author's choice to exclude drivers of motorcycles (in order to avoid confounding with changes in motorcycle helmet usage and laws) would increase the likelihood of finding a reduction since these drivers historically have been resistant to efforts to reduce drinking and driving. Thus, the larger reduction found in the IIHS study for administrative per se, compared with the current study, might be expected; however, this would not explain the large difference found for mandatory jail/community service. In the current study, only the State of Oregon was found to have a statistically significant reduction associated with mandatory jail/community service.

Since the comparison states are represented explicity in the model formulation, the validity of the estimated changes rests squarely on the appropriateness of using all adjacent states as comparisons.

According to the authors there are 135 state pairs with common boundaries, producing a set of equations, comparing all states against each of their adjacent states, that can be estimated using weighted multivariate regression. The authors propose that:

"Estimating the [coefficients] from these equations provides a natural and powerful extension to the traditional case/comparison method that is often used for evaluating the effects of law changes."

There appears to be no basis to accept this proposition. Most of the 48 contiguous states have implemented some form of tougher alcohol legislation; it is not clear that the use of **all** possible adjacent states as comparison states overcomes the confounding effect of the presence of alcohol legislation with the desired intent of using the comparison states to account for the effects of factors **other than** alcohol legislation. The authors apparently recognized this fact in an earlier statement:

"...or the case of several states changing two or more laws, [the model] cannot be used directly either because not all law change states can be paired with adjacent states without law changes or because there are two or more coefficients per equation."

It must be remembered that states appear in the equations over and over again as both *treatment* states and *comparison* states, and it is not clear how the presence of tougher alcohol legislation is accounted for when using the states as comparisons, especially if comparison states have implemented the same legislation as the treatment states for which they were to act as control. The effect of this confounding on the resulting estimates is not immediately obvious.

In May 1988, the AAA Foundation for Traffic Safety released a study titled *The Impact of Severe Penalties on Drinking and Driving*[6]. The study focused on severe mandatory penalties for drunk driving: jail, community service and loss of license. States selected for study were those that tested at least 60 percent of the fatally injured drivers, and at least 100 annually. Seven states which introduced severe mandatory penalties during this period were compared with seven states which had no change in legislation.

Four sets of quarterly data for the six years 1980-1985 were used. The four sets were the percentage of fatally injured drivers tested for BAC whose BAC exceeded 0.00, 0.07, 0.09, and 0.19.

The analysis involved eliminating the general downtrend found for the percentage of drivers exceeding each BAC level and accounting for the effect of increased testing on reported BAC levels. In the next step, the actual values were compared with the predicted values resulting from the model by forming the ratio (actual / predicted).

The author hypothesized that:

> *"If the severe sanctions had no effect one would expect [these ratios] to show only random fluctuations around 1. If the severe sanctions had an effect, one would expect [these ratios] to decline over time in the states with such sanctions, and to increase in those without them."*

Based on this hypothesis, for each state and BAC level, a linear time trend was fitted to the ratios, and the resulting trend coefficients were arranged by magnitude. If the sanctions had the desired effects, it was hypothesized that the states with such sanctions would cluster in the lower half of the tables (with negative values representing downward trends over time).

The study found that during the period 1980-1985, drunk driving declined in the fourteen states studied; the decline was greater at the higher BAC levels than at the lower ones. However, there was no indication that the decline was greater in states which had introduced more severe sanctions (jail, community service, loss of license) during this time period.

The results of the AAA study are in contrast to both the IIHS and the current study. The AAA study found no evidence of relative effectiveness associated with the presence of jail, community service or loss of license. The current study also found no evidence to support the deterrent effect of mandatory jail/community service (except for the State of Oregon), but found evidence to support the deterrent effect of loss of license. Illegal per se laws were not explicitly investigated in the AAA study.

Several inconsistencies in the definition of states implementing severe sanctions exist between the AAA study and both the IIHS and current study. Tables 3 through 5 from the AAA report present the results of the analysis; these tables have been summarized in Exhibit III-5.

Oregon was not considered in the severe-penalty group due to the possibility of first-offender diversion from the mandatory jail sentence. Whether Oregon was a severe-penalty state would depend upon how often convicted DWIs actually served time in jail or community service. Oregon did consistently better than the general trend, a result consistent with the current study (30 percent reduction).

Oklahoma implemented administrative per se in July 1983. Either this was not considered to be "hard" loss of license, or this was overlooked. Oklahoma was not considered in the severe-penalty group; however, Oklahoma did better than the general trend in 2 of the 3 BAC group cases. In the current study, Oklahoma exhibited a 1 percent increase associated with administrative per se license suspention and a 6 percent reduction associated with illegal per se.

(Table 3) BAC >0.00	Chg.	(Table 4) BAC ≥0.10	Chg.	(Table 5) BAC ≥0.20	Chg.
CO *	0.9	CO *	0.9	NJ *	1.4
WA *	0.3	NM *	0.4	NM *	1.0
IL	0.2	WI	0.3	WI	0.7
TN *	0.2	WA *	0.3	OK	0.7
WI	0.2	VA	0.2	VA	0.6
NM *	0.1	IL	0.2	WA *	0.5
CA	0.1	TN *	0.1	TN *	0.4
UT *	0.0	CA	0.1	CO *	0.1
VA	0.0	NJ *	0.0	CA	0.1
NJ *	-0.1	OK	-0.1	IL	-0.1
OR	-0.1	UT *	-0.3	UT *	-0.5
OK	-0.4	OR	-0.4	MN	-0.7
MN	-0.4	MN	-0.5	OR	-1.1
NV *	-0.5	NV *	-0.6	NV *	-2.3

Notes:
* Indicates states with either:
(1) mandatory jail, with no possibility for suspension of sentence or diversion;
(2) community service, by itself or as an alternative to jail;or
(3) "hard" loss of license without the possibility of getting an occupational or hardship license.

Only sanctions applying to first offenders were considered in the study.

Exhibit III-5 **Annual Change in Percentage of Drivers Killed with Specified BAC Level; Difference Against General Trend (AAA Foundation for Traffic Safety Study)**

Minnesota originally implemented administrative per se in September 1978. Since the law was in effect during the entire study period (1980-1985) Minnesota was not considered in the severe-penalty (change) group. However, the law was strengthened in 1982, correcting oversights and loopholes in the original law[16]. Minnesota consistently did better than the general trend, consistent with the 7 percent reduction associated with administration per se in the current study.

This leaves California, Illinois, Virginia, and Wisconsin as non-severe-penalty states. In only one of the twelve possible state-by-BAC group combinations did any of these states outperform the general trend.

The use of the difference against the general trend as the evaluative measure can provide misleading results. For example, removing the trend from a series represented by a step change in crashes coincident with the passage of legislation produces an alternating pattern of residuals which, when regressed over time, results in an estimated zero trend. This would place a state with effective legislation in the middle of the table.

In November 1988, Neville, Evans, and Graham, released a report titled *Evaluating the Impact of Drunk-Driving Countermeasures on Alcohol-Related Fatalities*[7]. The report documented a study of the impact of eight selected drunk-driving counter-

measures on alcohol-related fatalities during the period 1975-1986. A fixed effects model of traffic fatality counts was estimated, using pooled data from the 50 states, explicitly controlling for confounding factors such as the business cycle, travel exposure, seasonality, age of driver and state effects.

The results indicated that two interventions substantially reduce alcohol-related fatalities: the use of preliminary breath tests (PBT) and sobriety checkpoints. These two interventions were estimated to have decreased fatalities by 7 percent and 12 percent, respectively; the interaction of these interventions together were estimated to have decreased single-vehicle nighttime occupant fatalities by 35 percent.

In addition, the authors concluded that:

> "*Using our most alcohol-sensitive indicator (single-vehicle occupant fatalities at night), preliminary breath test laws might have saved 394 lives per year and the combination of preliminary breath test laws and sobriety checkpoints might have saved 3,455 lives per year (in the U.S. during the years 1982-1986). The other deterrence-based countermeasures do not selectively impact on alcohol-related fatalities...*"

While the pooling of data allows the analyst to investigate the effects of the countermeasures averaged over the implementing states, it does not permit the estimation of effects for individual states. It is not surprising, given the current study results, that administrative per se license suspension was found to be nonsignificant in the study by Neville et al. In the current effort only 6 of the 17 states implementing administrative per se exhibited statistically significant reductions. By pooling the data, the effect of administrative per se is averaged over all implementing states.

Of the 24 states using PBTs, eight states had already implemented PBT use by the January 1975 start date of the study (this contributes no information to the results); another 11 states implemented PBT use at the same time as other drunk-driving legislation. For example, Colorado and Kentucky both implemented PBT at the same time as anti-plea bargaining and first-offender jail/community service; Iowa, Delaware and West Virginia all implemented PBT use at the same time as administrative per se license suspension; Nevada and Mississippi implemented PBT use at the same time as anti-plea bargaining, first-offender jail/community service and administrative per se.

Of the six states utilizing roadside sobriety checkpoints, Montana was already using checkpoints in 1975 (contributing no information); Georgia began the use of checkpoints at the same time as first-offender jail/community service; North Carolina's checkpoints began at the same time as their administrative per se policy.

With regard to the large reduction associated with the combined use of PBTs and checkpoints, only two states (North Carolina and Vermont) had both policies in effect during the 1975-1986 time frame. Thus, the estimated effect for this combination was based on the experience of two states, one of which (North Carolina) implemented administrative per se at the same time as checkpoints, and had PBTs in use during the entire study time period. In the current study, North Carolina experienced an 11 percent reduction associated with administrative per se (the current study did not assess the changes associated with PBT use and roadside

sobriety checkpoints due to an absence of data regarding the date of implementation).

The conclusions presented appear somewhat stronger than the supporting data would permit. The use of PBTs probably would reinforce the effects of administrative per se, allowing an on-the-spot BAC test which could result in an administrative suspension. By averaging out the effects of the legislation through pooling, it would appear as if one could untangle the changes associated with the coincident implementation of multiple legislation. This is based on the premise that each state is a reasonable replication of all others.

Other problems exist with the use of PBTs in states. PBTs may be procured and legislation implemented; however, it may be some time before sufficient training has been conducted, and it is not clear that this equipment is used with sufficient regularity to expect such a large effect.

While the pooling of data provides a convenient setting for investigating the effects of various alcohol legislation, more detailed data can be obtained through the analysis of individual state experiences. For example, from the pooled analysis, it is not possible to determine whether administrative per se was ever associated with statistically significant reductions. Individual state analyses would indicate which states had effectively implemented such policies, and which states should be investigated in greater detail.

The estimated life saving of 3,455 per year during 1982-1986 associated with the use of preliminary breath test laws and sobriety checkpoints appears to be related to the use of vehicle miles of travel (VMT) in the model. The problem with using VMT in these types of analyses is that the relationship cannot be modeled in a linear fashion. Once a coefficient is estimated, the model predicts increases in fatalities proportional to increases in VMT. This provides a larger than expected projected increase in fatalities against which the actual values are measured.

The relationship between fatalities and VMT, represented by the fatality rate, has been almost steadily decreasing over time in an evolutionary manner (as opposed to abrupt revolutionary change). This gradual decline in the fatality rate is at least partially due to increases in safety that are gradually introduced into the traffic system, such as improved vehicles and roads. The analysis did not consider these evolutionary changes in traffic safety which would reduce the ultimate estimated changes by attributing some of the observed differences between actual and predicted to increased safety measures independent of the alcohol legislation. This is not necessarily an easy task, however, the omission of the safety factor will invariably lead to inflated estimates of reductions in light of the predicted linear effect of increasing VMT.

Since the fatality rate has been declining over time, it would appear that the trend in VMT is not a good predictor of fatalities except in the grossest of uses.

The effectiveness of roadside sobriety checkpoints has not been widely studied. However, a 1985 report[17] of a concentrated roadside checkpoint program in Charlottesville, Virginia found the program effective in reducing drunk-driving crashes.

Under a grant from the Virginia Office of Highway Safety, the Charlottesville Police Department implemented a driver's license and sobriety checkpoint program from

December 30, 1983 to December 31, 1984. During this period, checkpoint operations were conducted each Friday and Saturday night, except during rain, resulting in 94 checkpoint operations. Almost 24,000 drivers were stopped and interviewed. According to the authors, this was the most concentrated use of checkpoints in any single area in the United States. The report contains a great deal of information on operations, demographics of drivers stopped, changes in knowledge and attitudes from random digit dialing surveys, court actions resulting from DWI arrests, and relative efficiency measures of patrol manhours for traditional DWI patrol methods vs. the checkpoint program.

Time series analysis was used to analyze the crash data. Statistically significant reductions of approximately 13 percent were found in the number and proportion of police-reported *had-been-drinking* crashes, and in the percent of nighttime crashes that occurred in Charlottesvile compared with all of Virginia. Several other surrogate measures showed declines, but were not statistically significant.

This report presents a good case for the effectiveness of roadside sobriety checkpoints as a component in the enforcement of DWI laws. However, it should also be remembered that this was a very intense program, over a one-year time period, in a relatively isolated community (24,000 drivers stopped in a community with 40,000 inhabitants; at least one hour's drive from any other large urban area). The authors concluded that:

> "... *the checkpoint program reduced crashes related to drunken driving approximately 10 percent. However, the limited data available from the first year of operations do not permit a final conclusion, because while all the series analyzed demonstrated reductions in alcohol-related crashes, only three of the six were statistically significant. Continuation of the checkpoint program in Charlottesville should provide the data to resolve these issues.*"

E. Discussion

This analysis should be viewed as a first step in attempting to understand the causal nature of legislative policies aimed at deterring drunk driving. The conclusions drawn can be used as a starting point for identifying states for more in-depth study, possibly collecting data on implementation activities such as the numbers of licenses suspended (both administratively and as a mandatory sanction resulting from a DWI conviction), convicted DWIs sentenced to jail/community service, etc. From these data, it may be possible to identify factors which determine whether policies will provide sufficient deterrence to reduce drunk driving and the resulting crashes, contrasting performance measures for states exhibiting statistically significant reductions against those that did not.

The results suggest that some form of licensing sanction appears to be relatively more promising as a deterrent to drunk driving, compared with mandatory jail/community service or illegal per se laws. The fact that experiences with administrative per se laws are far more numerous than mandatory license suspension/revocation laws (17 states vs. 7 states, respectively; however, only five states implemented mandatory license suspension/revocation without administrative per se) should provide greater confidence in the results for administrative per se laws. Of the five states implementing only mandatory license suspension/revocation, one state (20 percent) exhibited statistically

significant reduction in driver fatal crash alcohol involvement; the median change for the five states was a 1 percent increase.

It has been proposed that the swiftness and certainty of sanctioning is a key element in achieving a sufficiently high level of deterrence to drunk driving to produce significant reductions in crashes. In this regard, administrative per se license suspension appears to be better suited than mandatory license suspension/revocation, since the administrative suspension generally begins at or within a short period of time after the violation, not requiring a conviction for DWI.

One state not studied in the current effort due to the small sample size of monthly fatal crashes was North Dakota. The State of North Dakota was the first to qualify for funding under Section 408, the Alcohol Traffic Safety Incentive Grant Program, of P. L. 97-364. To qualify for funding, the state implemented new alcohol legislation, part of which included administrative per se license suspension and illegal per se.

A report by North Dakota State University and the University of North Dakota[18] investigated a number of fatal and injury crash subsets to evaluate the effects of the new legislation. Using Box-Tiao time series/intervention analysis, the authors found statistically significant reductions in single-vehicle crash injuries during high drinking-driving times (8 p.m. to 8 a.m. on Friday and Saturday nights), compared with no change during the off peak time periods.

In North Dakota, a package of legislation, including administrative per se license suspension and illegal per se, and supplemented by a great deal of public information, was associated with significant reductions in alcohol involvement in injury crashes.

With regard to mandatory license suspension, a study by Dunlap and Associates, Inc.[19] of Winconsin's 1982 law mandating three to six month license suspensions for first-time convicted drinking drivers reported positive results following the passage of their law. In 1981, 45 percent of convicted Wisconsin drinking drivers lost their licenses. In 1982, mandatory loss of license legislation was implemented; between May 1982 and December 1985, 100 percent of convicted DWIs (reported to the Wisconsin Bureau of Driver Licensing) lost their licenses for at least 90 days.

The authors reported that:

> *"A time series analysis of statewide accident data for the years 1977 through 1985 (108 months) showed a significant reduction in 'alcohol' crashes beginning in 1982 and continuing through 1985. The average number of crashes decreased by approximately 25% following adoption of the new law."*

The above time series analysis investigated single vehicle nighttime weekend fatal and injury crashes involving male drivers, a much larger crash set compared with single-vehicle nighttime fatal crashes analyzed in the current study, which found a 1 percent increase.

A comparison also was made between recidivism rates for drivers convicted of DWI before license suspension was required for a first offense (May 1980-April 1981) compared with after (May 1982-April 1983). The results showed a substantial drop in recidivism rates for the first 12 months following conviction. The reduction in

recidivism was greatest during the first three months following conviction (63 percent reduction); recidivism was still about 20 percent lower up to one year following conviction.

The case for mandatory jail/community service as a deterrent is not supported by the current analyses. Only one of the 13 states studied showed a statistically significant reduction in alcohol-involved fatal crashes. However, the reduction observed in the State of Oregon (30 percent decline) is probably worth investigating, comparing the incidence of mandatory penalties in Oregon with other states implementing the mandatory jail/community service policy.

An evaluation of a voluntary jail sentencing policy in Minnesota was conducted by the Minnesota Department of Human Services, Chemical Dependency Division[20].

In January 1982 the judges of the Hennepin County, Minnesota adopted a policy whereby all first-time alcohol-related offenders would receive two-day jail sentences. In spite of the fact that jail sentences were voluntarily imposed by the municipal judges, roughly 82 percent of first-time DWI offenders were sentenced to serve two days in jail even two years after the policy was adopted.

The author concluded that:

> *"Coincident with the adoption of the policy there has been a statistically significant average monthly reduction of 35 nighttime injury accidents in Hennepin County. This represents a 20 percent reduction when compared to the pre-policy monthly average. Such is not the case in neighboring Ramsey County where a similar jail policy did not exist during the time period studied. Traffic fatalities have also declined in Hennepin County but not significantly more than in Ramsey County. In Hennepin County the adoption of the policy was accompanied by a marked increase in DWI arrests."*

The study analyzed nighttime injury accidents using Box-Tiao time series/intervention analysis.

One confounding factor is the implementation of more sweeping drunk-driving legislative changes (such as the toughening of an earlier administrative per se law) only three months after adoption of the two-day jail policy. The author recognized this fact in several places, and attempted to account for this through the use of Ramsey County, Minnesota as a comparison site (no jail policy). Since it is the differential effect observed in Hennepin County compared with Ramsey County, it is likely that a new analysis of the data, incorporating Ramsey County explicity in the analysis in ratio form, would find some positive relative effects due to the jail policy, but not as large as that reported in the study.

With regard to the use of community service as an alternative to mandatory jail, the Northwestern University Traffic Institute[21] evaluated a one year progam in Baton Rouge, Louisiana, in which virtually all DWI offenders were given community service. The authors concluded:

> *"In general, self-reported data and comparison of annual accident data with other Louisiana jurisdictions failed to indicate any significant change in driving behavior during the study period."*

A time series analysis of surrogate measures of alcohol-related accidents did find decreases in some measures (percent of

single vehicle fatal and injury accidents, and percent of drivers who had been drinking according to the police). However, it was not possible to separate the community service program effect from the fact that federal funding support for increased enforcement resulted in a 32 percent increase in DWI arrests. In the current study, the State of Louisiana exhibited a 1 percent increase associated with mandatory jail/community service. However, this particular community service program was restricted to Baton Rouge.

Questionnaires and interviews conducted by the project staff indicated that:

> "*Even after the one-year PI&E campaign, the three traditional sanctions of fines, license action, and jail were still identified as the most influential sanctions to deterring DWI behavior. ... Perhaps this can be best summarized by noting that even among drivers who identified community service as a sanction [25 percent] fewer than 50 percent indicated that it 'strongly influenced' them not to violate Louisiana's DWI law.*"

Illegal per se laws are a valuable tool for facilitating the conviction of DWI offenders by defining a legal BAC level as evidence of a crime *per se*. However, the deterrent effect of these laws is not clear, since the laws generally do not include additional penalties above what already is permitted by law. The alleged increased likelihood of a DWI conviction resulting from the implementation of illegal per se laws may be sufficiently threatening to potential drunk drivers, so as to produce a deterrent effect. However, it is probably worth investigating what other factors may have contributed to the reductions in alcohol-involved drunk driving, such as increases in DWI enforcement, public information, etc. This, of course, is true for all of the policies studied.

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