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Effects of Mandatory Seatbelt Use Laws on Highway Fatalities in 1985

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April 1987
Final Report

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16. Abstract

Each year between 40,000 and 50,000 people die in highway accidents. Mandatory seatbelt use laws (MULs) promise a substantial reduction in both highway fatalities and the severity of non-fatal accidents. A pooled cross-section time-series model is constructed to estimate the effectiveness of MULs in the nine states and District of Columbia where they were implemented in 1985. It is estimated that because of MULs 258 lives were saved in 1985.

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PREFACE

Mandatory seatbelt use laws (MULs) promise a reduction in deaths of passenger car occupants. Using data from the Fatal Accident Reporting System (FARS), fatality implications are assessed in the nine states and District of Columbia where MULs were implemented in 1985.

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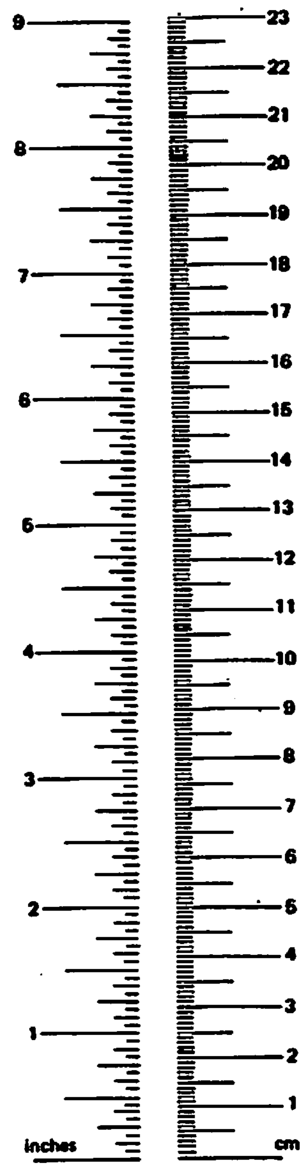
METRIC CONVERSION FACTORS

Approximate Conversions to Metric Measures

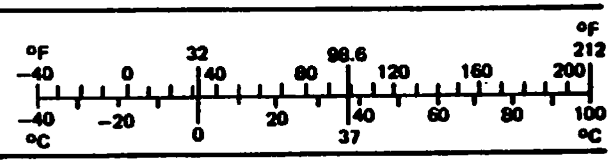
Symbol	When You Know	Multiply by	To Find	Symbol
		LENGTH		
in	inches	*2.5	centimeters	cm
ft	feet	30	centimeters	cm
yd	yards	0.9	meters	m
mi	miles	1.6	kilometers	km
		AREA		
in^2	square inches	6.5	square centimeters	cm^2
ft^2	square feet	0.09	square meters	m^2
yd^2	square yards	0.8	square meters	m^2
mi^2	square miles	2.6	square kilometers	km^2
	acres	0.4	hectares	ha
		MASS (weight)		
oz	ounces	28	grams	g
lb	pounds	0.45	kilograms	kg
	short tons (2000 lb)	0.9	tonnes	t
		VOLUME		
tsp	teaspoons	5	milliliters	ml
Tbsp	tablespoons	15	milliliters	ml
fl oz	fluid ounces	30	milliliters	ml
c	cups	0.24	liters	l
pt	pints	0.47	liters	l
qt	quarts	0.95	liters	l
gal	gallons	3.8	liters	l
ft^3	cubic feet	0.03	cubic meters	m^3
yd^3	cubic yards	0.76	cubic meters	m^3
		TEMPERATURE (exact)		
°F	Fahrenheit temperature	5/9 (after subtracting 32)	Celsius temperature	°C

*1 in. = 2.54 cm (exactly). For other exact conversions and more detail tables see NBS Misc. Publ. 286. Units of Weight and Measures. Price $2.25 SD Catalog No. C13 10 286.



Approximate Conversions from Metric Measures

Symbol	When You Know	Multiply by	To Find	Symbol
		LENGTH		
mm	millimeters	0.04	inches	in
cm	centimeters	0.4	inches	in
m	meters	3.3	feet	ft
m	meters	1.1	yards	yd
km	kilometers	0.6	miles	mi
		AREA		
cm^2	square centimeters	0.16	square inches	in^2
m^2	square meters	1.2	square yards	yd^2
km^2	square kilometers	0.4	square miles	mi^2
ha	hectares (10,000 m^2)	2.5	acres	
		MASS (weight)		
g	grams	0.035	ounces	oz
kg	kilograms	2.2	pounds	lb
t	tonnes (1000 kg)	1.1	short tons	
		VOLUME		
ml	milliliters	0.03	fluid ounces	fl oz
l	liters	2.1	pints	pt
l	liters	1.06	quarts	qt
l	liters	0.26	gallons	gal
m^3	cubic meters	35	cubic feet	ft^3
m^3	cubic meters	1.3	cubic yards	yd^3
		TEMPERATURE (exact)		
°C	Celsius temperature	9/5 (then add 32)	Fahrenheit temperature	°F



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EXECUTIVE SUMMARY

Mandatory seatbelt use laws (MULs) promise a substantial reduction in highway fatalities; voluntary usage is low, in the 5 to 20 percent range, and seatbelts are very effective in reducing fatalities if they are used. The potential life savings of universal seatbelt usage is estimated at about 40 percent of the 26,138 front seat occupant fatalities that occurred in 1985.

In 1985, nine states and the District of Columbia implemented MULs. The objective of this study is to assess the effect of these laws in reducing highway fatalities. The difficulty is to estimate what would have happened in these jurisdictions if there had not been MULs.

To estimate what would have happened in the jurisdictions without MULs, we have developed two sets of statistical models of fatalities covered by MULs to capture the historical relationship between this fatality series and other related series. In the first set of models, we estimate the front seat occupant fatalities per capita for each of the fifty states and the District of Columbia from other variables and fatality series which account for the variation in this fatality rate, including whether or not the state had a MUL during the period. Using this first set of models, we found that the average reduction in law covered fatalities is 6.7 percent. However, there is also an indication that during the first quarter after introduction a MUL appears to have a much stronger effect (about 12 percent) than during subsequent quarters where the effect is not statistically different from zero. This set of models also suggests that the effect may be stronger in states where the MUL has primary enforcement (police can stop violators of the MUL and issue citations) than those states which adopt MULs that permit only secondary enforcement (police can issue citations for MUL violations only after stopping motorists for other violations). Without evidence from more states, we do not feel confident that the higher effectiveness in primary enforcement states is caused by the MUL. It may simply be an anomaly in the set of states with primary enforcement.

In the second set of models, the law covered fatalities of the six largest individual states which passed MULs are estimated from other series. These models revealed wide differences among the states in the size of the MUL effect. New York, Michigan, Texas and probably North Carolina have relatively large effects -- in the ten to twenty percent range. New Jersey and Illinois have effects which cannot be distinguished statistically from zero. The models were also used to test for the presence of the strong first quarter effect that was found in the national model. The effect was found in Michigan and, to a lesser extent, Illinois, but not in New Jersey or New York. Texas and North Carolina had only one quarter experience with the MUL so they could not be used to test for this effect.

In both sets of models, we may underestimate the size of the MUL effect slightly because seatbelt usage often rises somewhat in anticipation of MUL implementation and states near MUL states may have higher usage because of "spill over" of publicity and/or drivers. Since both the before law fatalities and fatalities in non-MUL states are used to control for many of the influences on MUL states, and these fatalities may be decreased slightly by MULs, the resulting estimate of the effect of MULs may be underestimated slightly.

From these analyses we conclude that MULs have saved about 258 lives or 6.7 percent of law covered occupant fatalities in 1985. If this percent had applied to all states, 1,769 lives would have been saved in 1985. In 1986, MULs may save a smaller proportion of law covered occupant fatalities because a smaller percent of law covered occupants will be experiencing the large first quarter effect of the MULs. Even the 6.7 percent of law covered fatalities which were saved in 1985 is a small fraction of the 40 percent potential which could be realized if everyone wore their seatbelts.

1. INTRODUCTION

Every year between 40,000 and 50,000 people die in highway accidents. Seatbelts offer the promise of substantially reducing both these highway fatalities and the severity of injury in non-fatal accidents. Studies of the effectiveness of seatbelts indicate that belt users experience 40 to 50 percent fewer fatalities than non-users. (See National Highway Traffic Safety Administration (1)). Seatbelts have been installed in all automobiles sold in the U S. since the 1966 model year, yet voluntary usage is still very limited. Voluntary belt usage varies between 5 and 20 percent. (See Insurance Institute for Highway Safety (2)).

Mandatory seatbelt use laws (MULs) promise an increase in seatbelt usage and a consequent reduction in highway fatalities. Other studies have attempted to measure the increase in seatbelt usage (See Marburger (3) and Lawson (4) and Hedlund (5)). The objective of this study is to assess the effect of MULs on highway fatalities in 1985 when nine states and the District of Columbia put MULs into effect. The difficulty in estimating the effect of these laws is that there are many other influences on highway fatalities which should be controlled for in order to accurately estimate the MUL effect.

1.1 BACKGROUND DATA

Table 1-1 shows when each of the nine states and the District of Columbia implemented MULs in 1985. The table also shows the total MUL covered occupant fatalities and the MUL covered occupant fatalities during the time when the MUL was in effect. Overall, about half of the covered occupant fatalities in states which enforced MULs occurred during the part of the year when the laws were in effect. For comparison, covered occupant fatalities in the same time period of 1984 are also included in the table. 1985 covered occupant fatalities are lower than 1984 by 259 or about 6.7 percent. Notice that there are substantial differences in the effect of MULs among states when the effect is calculated in this way. If 1985 with no MULs had replicated 1984, then the 6.7 percent reduction is an accurate estimate of the effect of the MULs.

[1]Covered fatalities are those fatalities resulting from accidents w would be or have been affected by MULs.

TABLE 1-1. FATALITIES IN STATES WITH MULs IN 1985

State	Date Implemented	Total Fatalities in 1985	Fatalities During Period of MUL Enforcement 1985	Fatalities During Period of MUL Enforcement 1984	Changes 1984-85	Percent Change
New York	12/1/84	976	976	1,087	-111	-10.2
New Jersey	3/1/85	546	461	440	+21	+4.7
Illinois	7/1/85	853	459	488	-29	-5.9
Michigan	7/1/85	932	490	532	-42	-7.9
Nebraska	9/6/85	159	66	81	-15	-18.5
Texas	9/1/85	2,190	707	811	-104	-12.8
Missouri	9/28/85	659	197	154	+43	+27.9
North Carolina	10/1/85	978	229	252	-23	-9.1
District of Columbia	12/12/85	25	3	3	0	---
Hawaii	12/16/85	67	4	3	+1	+33
TOTALS		7,385	3,592	3,851	-259	-6.7

Source: Fatal Accident Reporting System.

What would have happened to covered occupant fatalities in the states and District of Columbia without MULs? Table 1-2 presents data on covered occupant fatalities in the jurisdictions which enforced MULs in 1985 during the period that they were in effect, and provides historical perspective on these fatality counts. Covered occupant fatalities have been declining since 1978, though there was an increase in 1984. Would covered occupant fatalities have increased in 1985 as they did in 1984, or would they have continued the longer term downward trend? The second column in Table 1-2 shows the series of fatalities not affected by MULs for the same jurisdictions and periods as the covered occupant fatalities in column 1. This series has also had a declining trend since 1980 except for 1984. This series of fatalities not covered by MULs decreased by less than covered occupant fatalities in 1985, perhaps this is because of the effects of MULs.

Another way to judge what would have happened to covered occupant fatalities in 1985 if there had not been any MULs is to look at front seat occupant fatalities in states that have MULs and compare them to front seat occupant fatalities in states which did not have MULs. Table 1-3 presents this comparison. Full years are compared since the states with MULs did not implement them at the same time. Again the pattern in the two columns is quite similar. In 1985 fatalities in states with MULs declined by 3.8 percent while they declined by only 1.2 percent in states without MULs.

These two means of judging what would have happened in 1985 without MULs indicate that 1985 covered occupant fatalities would probably have been somewhat lower than 1984, 2.8 percent lower, based on fatalities not covered by MULs in states where and when MULs were in effect; and 1.2 percent lower based on front seat occupants in states which did not enforce MULs in 1985. What we need is a more systematic method for estimating what would have happened without MULs.

In the remainder of this report, we discuss models of covered occupant fatalities which provide a systematic method for determining what would have happened to these fatalities if there had not been any MULs. The models permit us to use the full historical perspective on covered occupants and their relationship to other series in estimating what would have happened without the MULs. We develop a national estimate of the effect of MULs, and investigate how the timing of implementation and the type of enforcement affect the size of this effect. Because we observe wide differences in the effect among the states which have implemented MULs, we also investigate state specific models to verify and check the results of this national model. Both approaches are used to estimate the lives saved from MULs in 1985.

TABLE 1-2. COVERED AND NON-COVERED FATALITIES IN STATES AND MONTHS WITH MULs

Year	Covered Fatalities	Percent Change From Prior Year	Non-Covered Fatalities	Percent Change From Prior Year
1985	3,592	-6.7	2,824	-2.8
1984	3,851	+3.9	2,905	+4.9
1983	3,707	-6.9	2,770	-7.0
1982	3,980	+13.3	2,978	-4.8
1981	4,589	-2.4	3,129	-5.5
1980	4,704	-1,8	3,310	+3.9
1979	4,793	-2.4	3,189	-2.4
1978	4,911	+9.8	3,266	+1.8
1977	4,472	+1.3	3,207	+7.6
1976	4,413	-0.7	2,981	-1.4
1975	4,444		3,204	

Source: Fatal Accident Reporting System.

TABLE 1-3. COVERED OCCUPANT FATALITIES

Year	Covered Fatalities in States With MULs	Percent Change From Prior Year	Covered Fatalities in States Without MULs	Percent Change From Prior Year	MUL States As a Percent of Total Front Seat Fatalities
1985	7,385	-3.8	18,753	-1.2	28.3
1984	7,676	+4.5	18,990	+3.3	28.8
1983	7,344	-5.1	18,379	-0.0	28.6
1982	7,739	-13.2	18,376	-11.2	29.6
1981	8,921	-2.1	20,696	-2.9	30.1
1980	9,109	+0.5	21,313	-0.8	29.9
1979	9,068	-0.8	21,444	+0.8	29.7
1978	9,140	+6.9	21,316	+8.4	30.0
1977	8,549	+2.3	19,663	+5.2	30.3
1976	8,360	+0.2	18,683	+3.8	30.9
1975	8,345		17,997		31.7

Source: Fatal Accident Reporting System.

2. NATIONAL MODELS

2.1 PURPOSE

The purpose of this section is to develop an estimate of the effect of MULs on highway fatalities in the U.S. The central question in determining the effect of MULs is, What would fatalities have been in 1985 without MUL intervention? To answer this question, it is necessary to develop a model of the fatality series affected by MUL. The model tests for a MUL effect by following the MUL affected fatality series and then determining how this series deviates from its expected levels after MUL intervention.

The plan of study was to develop a model based on fatality series that change with MUL affected fatalities because of common causal factors. The model must hold across time and across states; and measure the deviation after MUL implementation. The analytic structure which fits this plan is a pooled cross-section, time-series model.

This section begins by describing the model used to measure the effect MULs have had on fatalities in the nine states and District of Columbia where they were implemented in 1985. Variable descriptions and estimating techniques are discussed first, then the results of estimating a basic model and testing variations on data from NHTSA's Fatal Accident Reporting System (FARS) are presented. Finally, based on the sensitivity of fatalities to the introduction of MUL as determined by these models, fatality reductions are estimated under various scenarios.

2.2 MODEL DESCRIPTION

2.2.1 Variable Definitions

The dependent variable in our model is the fatalities affected by MULs in each state divided by the population of that state. However, since all MULs do not affect precisely the same group of occupants, some compromises were necessary. For instance, New York requires that persons in the back seat under the age of ten and over the age of three be restrained by a federally approved device. Most other states require only front seat occupants to be restrained. States differ in their definition of vehicles considered to be used for passenger purposes. In the national models, we use front seat occupant fatalities in vehicles required by federal law to be equipped with specified restraining devices as the standard definition of MUL affected fatalities. This standardized fatality set allows for states which did not pass a MUL to be included in the pooled cross-section, time-series model as a control for the activity in MUL states.

A model based on quarterly data is constructed. Quarterly data is the best compromise between the precision of the specification of the MUL intervention, which favors shorter time periods, and the random error associated with smaller state MUL affected fatality counts, which favors larger time periods.

The MUL affected fatalities for each state are converted into a mortality rate by dividing by state population. Since population by state is available only on an annual basis, quarterly values were interpolated by adding one-fourth of the difference between years to the beginning year population for each quarter. By using a rate, statistical comparison can be made more easily among states of varying size. If the errors in statistical estimation are proportional to the size of the state, the problem of heteroscedasticity is also probably reduced by using the fatality rate. Finally, there is less chance of being misled into thinking that a useful model has been developed when that model only captures factors associated with the size of the state.

The fatality rate formed becomes the dependent variable in an econometric model used to test if MUL intervention was effective and to what extent. The rate is designated as FSFATRATE in Table 2-1 which shows the equation form and other variables used in the model. The model is linear in the logarithm of the continuous variables. This permits the coefficients to be interpreted as percent changes resulting from a hundred percent change in the independent variables.

Data observations of FSFATRATE are generated from FARS for each state (s), for each year (y), and for each quarter (q). The range of observations available for the model is from 1975-1985. So, 44 quarterly observations are available for each state and the District of Columbia or 2,244 observations all together. Definitions and explanations of the other variables in the model follow.

TABLE 2-1. NATIONAL MODEL

$$\mathrm{LN\ (FSFATRATE)}_{syq} = B0 + B * \mathrm{QUARTER}_q + B * \mathrm{LN\ (OTHERRATE)}_{syq} + B * \mathrm{MULD}_{syq}$$
$$+ B * \mathrm{YEAR}_y + B * \mathrm{STATE}_s + B * \mathrm{STATE}_s * \mathrm{TREND}_y$$
$$+ B * \mathrm{PRIMARY}_{syq} + B * \mathrm{FIRSTQUARTER}_{syq} + B * \mathrm{MULDSQ}_{syq}$$
$$+ B * \mathrm{STATE}_s * \mathrm{SEGMENT}_y$$

s = 1..51: 50 states and District of Columbia

y = 1975...1985: 1975 through 1985

q = 1..4: 4 quarters

Bs: coefficients, one for each variable

Where:

FSFATRATE=Front seat fatalities in passenger vehicles divided by population.

QUARTER = '1' if specific quarter
'-1' if fourth quarter
'0' otherwise

OTHERRATE = All fatalities not included in FSFATRATE divided by population.

MULD '1' if state had MUL for full quarter
'66' if two months of quarter
'.33' if one month of quarter*
'0' otherwise

*Prorated for partial months.

YEAR = '1' if specific year
'-1' if year 1985
'0' otherwise

STATE = '1' if state is the one under consideration
'0' otherwise

TREND = Linear trend by year, '1' if 1975,...'11' if 1985

PRIMARY = '1' if state has primary MUL enforcement
'0' otherwise

FIRSTQUARTER = '1' for first quarter of MUL enforcement

MULDSQ = '1' for all quarters after the first quarter of MUL enforcement

SEGMENT = '1' if y equals 1983, 1984, 1985
'0' otherwise

B_o represents the constant of the regression. The B_s are the coefficients of the independent variables.

To account for the fact that FSFATRATE has a seasonal pattern, three quarterly dummy variables ($QUARTER_q$) are included. The seasonal pattern of FSFATRATE is similar to that of other highway fatalities, high in the summer months and relatively low in January through March. As specified in this model, we assume that the quarterly pattern is the same for all states. Tests of other specifications of the model which did not make this assumption indicated that there is little gain in relaxing the assumption and giving each state an individual quarterly dummy. It seems that even though the variation in temperature and type of precipitation is different among states, traditional factors like school sessions, vacations, and length of daylight are more important in determining seasonality.

One of the main sources of control in the model is another fatality rate: the per capita fatality rate for all other fatalities ($OTHERRATE_{syq}$) (those fatalities not included in the dependent variable). These are pedestrian, motorcycle, and commercial truck fatalities, fatalities of those passengers in the back seat of MUL covered vehicles, and a few other "non-front seat" fatalities as well. This per capita fatality series (OTHERRATE) should track FSFATRATE well. Factors affecting it should also affect FSFATRATE. If a strong historical relationship exists between the two series, then in 1985, in the nine states and District of Columbia a deviation from this quantifiable historical relationship should appear if MULs have been successful in reducing law covered fatalities. Obviously, the stronger the relationship existing between the two series, the better the measurement of the MUL effect.

MULD is the dummy variable which measures the MUL effect. Table 2-2 gives its values for each state implementing a MUL for each quarter in 1985. Basically, the value '0' is given if a MUL has not been implemented and '1' if the law has been in effect for a full quarter. Because laws were not always implemented at the beginning of a quarter, it is necessary to code '.33' for each month of enforcement during the quarter. Further, prorating was necessary for Hawaii and Missouri because of enforcement for part of a month. The expected sign for the MULD coefficient is minus indicating that laws have saved lives.

TABLE 2-2. INTERVENTION MULD VARIABLE CODING

		1984	1985 Quarter			
	Date	4	1	2	3	4
District of Columbia	12/12	0	0	0	0	.20*
Hawaii	12/16	0	0	0	0	.15*
Illinois	7/1	0	0	0	1	1
Michigan	7/1	0	0	0	1	1
Missouri	9/28	0	0	0	.02*	1
Nebraska	9/6	0	0	0	.27*	1
New Jersey	3/1	0	.33	1	1	1
New York	12/1/84	.33	1	1	1	1
North Carolina	10/1	0	0	0	0	1
Texas	9/1	0	0	0	.33	1

1 = full quarter
.33 = 1 month
.66 = 2 months

*Prorated for partial months.

MULs affect seatbelt use in the month(s) before the law is enforced as drivers anticipate their new responsiblities and become aware of the benefits of belt use. MULs also effect belt use in states near the enacting MUL state even though these states do not have MULs themselves. Drivers in the MUL state do not unbuckle when they cross the border, and the publicity on seatbelts in the MUL state spills over the border as well. Both of these "spill overs" affect the fatality rates against which we measure the effect of MULs and tend to slightly reduce our estimate of the effect of MULs. The effects of the spill over are slight because the spillovers are limited in terms of duration in the case of anticipation and are limited in geographic scope in the case of the effect of the MUL on adjacent states. Low fatality counts for a state in one or two months during the 1975-1985 period, over which the model is estimated, will have little effect on the estimated coefficients. Similarly, low fatality counts for few states during the period when MULs are enforced, will have only a very slight effect on the estimated coefficients.

One variable related to MUL which would eliminate those slight biases would be seatbelt usage. Because we need data on belt usage between 1975 and 1985 which represents usage for complete states, we cannot use the limited seatbelt survey data collected before and after MULs are introduced. FARS reports seatbelt use, but this measure of seatbelt usage was not useful for two reasons. First, most of the reported variation in seatbelt usage occurred between the before (MUL) and after periods. Only minor variation occurred during the before or after periods. Because of this pattern, the coefficient will reflect the discrete before to after change, just like the MULD variable. The second reason is that we suspect usage reported in FARS is overreported and the passage of MULs further increases the overreporting. Data for FARS is reported by police officers who arrive at the accident some time after the occurrence. Occupants and even victims will probably have unfastened seatbelts before the officer arrives, so the officer must rely on the accounts of usage reported by those involved in the accident. Since MULs make it a violation to be unrestrained, usage in FARS is likely to have a substantial bias towards reporting that seatbelts were used.

The next three variables in the model are dummy variables that give the equation a covariance model structure. Each variable is put into the model because not all factors which explain the dependent variable are known or quantifiable. YEAR allows for each of the 11 years to have a different effect in determining FSFATRATE. It captures the overall national trends. If any trend, or high or low periods, exists in FSFATRATE, then the YEAR dummy variables will be significant in identifying it. Periods like 1977-1980 should be higher than average, and periods like 1982-1983 lower. The coding of YEAR is like that of QUARTER. There are ten variables (y-1).

Recognizing that the fatality rates, FSFATRATE, are different among states, STATEs provides an individual adjustment for each rate--a benchmark. This series of 50 dummy variables calibrates the differential rates arising from local and regional peculiarities. Urbanized states have low basic rates partly because proportionately more driving is at a lower speed and because some of the population in urban areas does little driving. Rural states have a large per capita use of automobiles. These states should have on average higher FSFATRATEs. So, the STATE dummy variables account for the average difference between each state's FSFATRATE and the national average.

The basic FSFATRATE adjustment that STATE provides is further adjusted by combining these dummy variables with an interacting TRENDy term. TREND is a linear trend progressing from '1' for each quarter of 1975 up to '11' for each quarter of 1985. This specification accounts for trends in each state's FSFATRATE which differ from the overall national trend. It is reasonable to assume that some states would have changes caused by population and economic growth as well as changes in safety programs. Because the MUL's are introduced in the last few quarters of our data, it is particularly important to control for any trends which affect the states which implemented the MULs so we can avoid attributing those trends to the MUL effect.

A linear trend may place too much of a restriction on this phenomenon and actually impose a structure which is misleading. A more flexible structure would be to allow each year in each state to have a different value with a set of variables like YEAR. This specification would result in too many variables for the regression equation to evaluate. A more parsimonious specification is tried with the variable SEGMENT which represents a dummy variable coded '1' if the year is 1983-1985, and '0' otherwise. This period was selected among the many possible because it was to be determined if MULD could detect differences once other possible influences have been accounted for in the regression equation. By differentiating the latter years, STATE*SEGMENT will allow the MULD variable to measure how different the MUL intervention was from the 1983-1985 period for that state. Periods of less than the three year 1983-1985 period were not tested because they would have attributed too much of the MUL effect to this interaction term.

The three remaining variables in the model are used to test specific characteristics of the MUL intervention. The PRIMARY variable is used to address the question of whether or not the type of enforcement is significant in determining how effective the law is in terms of fatality reductions. Primary enforcement allows police to stop and issue citations to motorist if they are in violation of the seat belt law. New York, Texas, North Carolina, and Hawaii are primary enforcement states. Secondary enforcement prevents officers from stopping motorists for violations of the MUL alone, and allows citations for a MUL violation only when another type of violation has also occurred which permits the office to stop the motorist. PRIMARY is coded '1' for the four primary states during the period of enforcement and '0' otherwise.

The final two variables in the model test the persistence of the MUL effect. They examine whether the effect remains the same over time, grows, or declines. The result obviously has important implications for the estimated "steady state" effect of the MULs and may also provide states with information helpful in adjusting enforcement policy. FIRSTQUARTER is a dummy variable coded '1' for the first three months a state enforces a new MUL and '0' otherwise. MULDSQ is a dummy variable coded '1' for all quarters a state has a MUL except the first and '0' otherwise.

2.2.2 Estimating Technique

The equation described above was estimated using weighted least squares (WLS). All observations are weighted by the number of fatalities potentially affected by MULs in the state in the quarter in the year. WLS was used to decrease the possibility of heteroscedasticity and thus provide a more efficient estimation of the coefficients. The smaller fatality states have a larger variance in the number of fatalities than the larger states. By weighting the estimation more toward the larger fatality states, an estimate will be produced which takes the reliability of state data into account.

2.3 RESULTS: THE MUL EFFECT

Table 2-3 presents the results of fitting a number of variations of the model described above. Each different model is represented in a column. If a variable is not included in an equation, 'NO' is given in the variable row. The coefficients and t-statistics of the dummy variables, QUARTER, YEAR, STATE, and STATE*TREND, are not given to conserve space, but the inclusion of these variables is indicated in an equation by 'YES'. Column 1 presents the WLS estimation of the basic model describing the effect on average in each of the nine states and District of Columbia from implementing a MUL during 1985.

The basic model in the first column fits the data very well. The high adjusted R-squared indicates a high proportion of the variation in the state, quarterly, front-seat fatalities per capita is explained by the model. The OTHERRATE variable is highly significant and has the anticipated sign. The coefficient of MULD, the intervention dummy, indicates that the nine states had, on average, a 6.7 percent reduction in MUL covered fatalities during the period of enforcement in 1985. The indicated reduction is statistically significant at the five percent level for a two-tailed test.

The next three columns in Table 2-3, labeled "Stepwise Elimination" show the results of eliminating three important groups of dummy variables from the model. The three variable groups are: STATE*TREND, STATE, and YEAR. Column 2 shows that eliminating STATE*TREND has little impact on the overall explanatory power of the model, as measured by the adjusted R-squared, but the change in the coefficient of MULD indicates that the state level trend term is an important control in measuring the MUL effect.

Next, the STATE dummies are eliminated. The explanatory power of the model as indicated by the adjusted R-squared is halved as shown in Column 3. The effect of MUL is now estimated to be 28.8 percent which again indicates the importance of these state level controls in developing our estimate of the effect of MULs. It is concluded that without a variable to establish an individual base fatality rate for each state, the FSFATRATE cannot be adequately modeled because important variables are omitted.

Finally, in the stepwise elimination, the YEAR dummy variables are removed without much change from the prior step in the overall fit or the coefficient of the MULD variable. These dummy variables have only small explanatory power and point to the importance of modeling trends in the individual states.

TABLE 2-3. NATIONAL MODEL RESULTS

	Basic Model	Stepwise Elimination			Dummies for Trend	Truncating Estimating Range		First Quarter	Primary Enforcement
CONSTANT	-2.67 (-41.48)	-2.66 (-41.56)	-1.03 (-13.53)	-0.93 (-13.52)	-2.68 (-42.14)	-2.75 (-35.26)	-2.76 (-30.78)	-2.67 (-41.51)	-2.68 (-41.69)
QUARTER	YES	YES	YES	YES	YES	YES	YES	YES	YES
OTHERRATE	0.22 (13.91)	0.21 (13.51)	0.62 (33.90)	0.64 (36.43)	0.21 (13.72)	0.20 (10.36)	0.20 (8.89)	0.22 (13.89)	0.22 (13.78)
MULD	-0.067 (-2.19)	-0.113 (-3.76)	-0.288 (-4.98)	-0.297 (-5.48)	-0.069 (-2.17)	-0.059 (-1.89)	-0.040 (-1.25)	NO	-0.018 (-0.47)
YEAR	YES	YES	YES	NO	YES	YES	YES	YES	YES
STATE	YES	YES	NO	NO	YES	YES	YES	YES	YES
STATE*TREND	YES	NO	NO	NO	NO	YES	YES	YES	YES
FIRSTQUARTER	NO	NO	NO	NO	NO	NO	NO	-0.116 (-2.613)	NO
MULDSQ	NO	NO	NO	NO	NO	NO	NO	(-.024) (-0.57)	NO
PRIMARY	NO	NO	NO	NO	NO	NO	NO	NO	-0.161 (-3.48)
STATE*SEGMENT	NO	NO	NO	NO	YES	NO	NO	NO	NO
RANGE	1975-85	1975-85	1975-85	1975-85	1975-85	1979-85	1981-85	1975-85	1975-85
ADJR.2	0.87	0.85	0.43	0.42	0.86	0.87	0.88	0.87	0.87
F-VALUE	130.64	199.00	113.57	331.49	124.85	90.34	67.40	129.61	130.29

STATE*TREND imposes a strong structure on the state trends, forcing a linearly increasing or decreasing trend on each state. Ideally, each year in each state would be allowed to increase or decrease. Such a specification is not statistically practical, and also would threaten to capture much of the MUL effect in the 1985 term. One parsimonious specification that was tried is that the TREND part of STATE*TREND is replaced by SEGMENT which is coded '1' for the years 1983-1985 and '0' otherwise. In this way, the latter part of the estimating range is differentiated from the other years. Doing so protects other factors which might be confused with the intervention effect of MUL from biasing the estimation. This specification of the model emphasizes the 1983-1985 period in estimating the effect of the MULs. The results of estimating this model are presented in column 5. Replacing TREND with SEGMENT does not change the fit of the model or the estimated coefficient of the MULD variable by an appreciable amount. Statistically, the models in Columns 1 and 5 are very similar. This similarity is an indication that the STATE*TREND term, while not theoretically ideal, is adequate to capture the variations in FSFATRATE.

Finally, one other test is made of the stability of the basic model. The estimation range is truncated. First, four years are removed and the basic model is estimated from 1979-1985. The MULD still has the expected sign, but the effect has dropped from 6.7 percent to 5.9 percent and the statistical significance drops into the 90 percent level. Truncating six years from the estimating range results in still a lower estimate of the effectiveness of MUL. The states, on average, experienced a four percent reduction in FSFATRATE, but the result is not statistically significant. The coefficient of OTHERRATE, on the other hand, remains stable in the basic model when the range is truncated and is statistically significant as well. This behavior gives us confidence in the size and stability of the effect of OTHERRATE.

Two important aspects of MUL effectiveness can be tested using the basic model. The first is whether the MUL effect is constant or whether the MULs have a high initial effect followed by a low continuing effect or the reverse. Only New York had enforcement for the full year. New Jersey had enforcement for ten months of 1985 while the other states and District of Columbia had enforcement for six months or less. The variables FIRSTQUARTER and MULDSQ are included in the basic model to test if there are stages in effectiveness. FIRSTQUARTER is coded '1' for the first three months of MUL of implementation. MULDSQ is coded '0' for this first quarter and '1' thereafter. The results of this test are shown in Column 8. The FIRSTQUARTER variable is statistically significant at the 95 percent level. The MULDSQ coefficient is not statistically different from zero. From this we conclude that much of the 6.7 percent average effect resulted from an initial, high impact of about 12 percent in the first quarter of enforcement. After the first quarter there is a sharp decrease in effectiveness to about two percent. Thus, the average effect found by the basic model in 1985 could have varied widely depending on the timing of the states implementation of the MULs during 1985. If most of the states had implemented the laws at the beginning of the year, then these results suggest that the effect would have been smaller than the 6.7 percent we found because more time would have been spent at the lower effectiveness levels after the first quarter. This finding has obvious implications for the estimates of the "steady state" effects of MULs on highway fatalities. These will be discussed in the next section.

The effect of a second characteristic of MULs which we can test with the model is whether the type of enforcement, primary versus secondary, has any bearing on the level of effectiveness. PRIMARY is coded '1' for New York, Texas, North Carolina, and Hawaii; and '0' otherwise. As the result, the last column in Table 2-3 shows that states with primary enforcement seem to experience a much more powerful effect. The effect in states with secondary enforcement is not significantly different from zero. However, with such a small sample, it is hard to say what the primary enforcement coding is exactly picking up. It may be that those states with primary enforcement have some other attribute which is important (like a political will to enforce the seat belt law) or the result might have occurred because New York and Texas happen to have had declines in FSFATRATE and primary enforcement. We feel that data from more states with primary enforcement is needed before we can positively attribute this effect solely to the type of enforcement.

2.4 RESULTS: CALCULATING LIVES SAVED

Table 2.4 presents estimates of the lives which were or could have been saved in 1985 with MULs. The figures are based on the various estimates of the effect of MULs discussed in the previous section. The first set of scenarios shows the implications of variations in the MUL effect. The best estimate of the average effect of the MULs as they actually occurred in 1985 is a 6.7 percent reduction. The 3,592 fatalities shown on the table under the heading "Fatalities with MULs" actually occurred in states and months when the MULs were in effect. The standard error on the best estimate is 3.0 percent. The implications of effects which are one standard error higher and lower are also shown on the table.

The next row in the table shows the fatality reduction which might be expected if the MUL effect were constant at 6.7 percent and all states in the U.S. enforced the laws before January 1, 1985. We do not believe that the MUL effect is constant, however. The model indicates that the first quarter effect is about 12 percent while subsequent quarters are about two percent. The implications of this variation in the effectiveness of MULs is shown in the next two rows. The first is calculated under the assumption that the MULs are enforced starting January 1, 1985. So, the full effect of the first quarter after introduction will be counted in 1985. Under this scenario the fatality reductions are only about 60 percent of those calculated under the assumption that the MUL effect is constant at 6.7 percent for the full year.

The final row in the table shows the "steady state" effect of the MULs. This is the effect which would be expected if the MULs had always been enforced. The high initial quarter is not counted in this estimate. Only the ongoing effect which was estimated at two percent is counted.

Obviously, all of the "All State" estimates of fatality reductions presented in Table 2-4 assume that the experience of the nine states and the District of Columbia which implemented MULs in 1985 can be extrapolated to other states which have not yet implemented MULs. As the models which include the PRIMARY variable showed, this is not a sound assumption. States have had very different experiences with the effects of MULs. In the next section, the analysis of these individual state experiences will be described and the implications for the estimated effects of MULs discussed.

TABLE 2-4. ESTIMATED LIVES SAVED FROM MULs IN 1985

Scenario	Estimated Fatalities Without MULs	Fatalities With MULs	Reduction
Average MUL Effect (9 states and D.C.)			
-3.7% (-1 S.D.)	3,730	3,592	138
-6.7% (best estimate)	3,850	3,592	258
-9.7% (+1 S.D.)	3,978	3,592	386
Average MUL Effect (all states)			
January 1985 implementation			
-6.7%	26,396*	24,627	1,769
Distinguishing First Quarter MUL Effect (12% 1st qtr., 2% others)			
All States			
implementation January 1, 1985 (12%)	26,396*	25,368**	1,082
All States			
implementation prior to Oct. 1, 1984 (2%)	26,396*	25,868	527

*Actual 1985 fatalities plus 258 which were avoided by MULs.

**Assumes 21 percent of front seat fatalities occurred in the first quarter of 1985.

3. STATE MODELS

3.1 PURPOSE

The purpose of this section is to develop estimates of MUL effects for each of the large states which introduced the laws prior to November 1985. By constructing individual state models for some of the larger states, it should be possible to verify the results of the national models regarding the first quarter effect; the state models should also be able to determine the importance of state-to-state variation in MUL effects. If the variation is small, the 1985 results could reasonably be extended to other states because it would not seem to depend on which states implemented the laws in 1985. A large disparity, however, would indicate that effectiveness in 1985 was a function of which particular states implemented the laws as well as a function of the timing of implementation and the type of enforcement. A large disparity would also make accurate forecasts of the effects of implementation in other states difficult.

3.2 MODEL DESCRIPTION

3.2.1 Definitions

State models are constructed for six states: New York, New Jersey, Illinois, Michigan, Texas, and North Carolina. The other states and District of Columbia do not have large enough MUL fatality counts to model because the large random component of those fatality counts would make it difficult to form meaningful historical relationships which could be used to estimate deviations caused by MUL intervention.

Fatalities of passenger vehicle occupants who are required by the state's MUL to wear their seatbelt, or law covered occupants are the dependent variables in the state models. This subset of fatalities is different for each state because the MULs are slightly different for each state.

The MUL covered fatality series, LCFAT, is the dependent variable in the state model shown in Table 3-1. It has not been converted to a mortality rate by dividing by state population. In order to get as close as possible in time to the intervention effect of MUL, the fatality series was constructed on a monthly basis. This division was possible in the state models because only those states with larger fatality counts were selected to be modeled. LCFAT is an absolute level of fatalities consisting of monthly (m) observations over six years (y), 72 observations in all.

TABLE 3-1. STATE MODEL

$$
\begin{aligned}
LN(LCFAT)_{ym} = \; & b_o + B * MONTHS_m \\
& + B * LN(USCONTROL)_{ym} \\
& + B * LN(N.OCCFAT)_{ym} \\
& + B * MULDS_{ym} \\
& + B * TRENDS_{ym} \\
& + B * FIRSTQUARTERS_{ym} \\
& + B * MULDSQ_{ym}
\end{aligned}
$$

y = 1980...1985: 1980 through 1985

m = 1...12: January through December

Bs: coefficients, one for each variable

STATE MODELS

LCFAT	=	Law Covered Fatalities
MONTHS	=	'1' If Current Mosnth, '0' Otherwise
USCONTROL	=	Aggregate fatalities in same vehicle type and seating positions as 'LCFAT' for states not implementing a MUL in 1985.
N.OCCFAT	=	Non-occupant fatalities and fatalities in vehicles not covered by MUL.
MULDS	=	'1' if MUL implemented, '0' otherwise
TRENDS	=	Linear trend by month (1...132)
FIRSTQUARTERS	=	'1' for first three months of implementation, '0' otherwise
MULDSQ	=	'1' for all months of implementation except the first three '0' otherwise

B_0 is the constant of the regression. The Bs are coefficients of the other variables. MONTH is a series of 11 dummy variables which adjust the model for the seasonal variation in the variables.

USCONTROL is the aggregate monthly total of law covered fatalities for the 41 states which did not implement a MUL in 1985. The law covered fatalities for this series are defined in exactly the same way as specified by the particular state law for the model being estimated. For instance, New York s MUL, unlike any of the other five state laws, includes back seat occupants in passenger vehicles. The USCONTROL for New York would include front and back seat occupants of passenger vehicles for the 41 states without MULs. The USCONTROL variable should capture those social, demographic, economic, and institutional forces that act upon all fatalities of this type regardless of the location.

To account for forces on law covered fatalities that are peculiar to a state, a variable measuring non-occupant fatalities, N.OCCFAT, is included in the regression equation. It is a series comprised of the state's motorcycle, pedestrian, bicycle, and several other minor classifications of fatalities. The only group of a state's fatalities not included is those persons in motor vehicles covered by MUL, but not required to be belted like other occupants. This group was purposely omitted because those persons might be influenced to use restraints even though not required by law. So, they would not serve as a good control group to measure the effect of the MUL against.

MULDS is the intervention dummy variable coded '1' if a state has a MUL in force during the month and '0 otherwise. As with the coefficient of the MUL variable in the national model, the coefficient of MULDS may slightly underestimate the true effect of the MUL because of spill over effects on months preceeding the MUL and on states near the MUL states.

LCFAT may not have a proportional relationship with its controls, USCONTROL and N.OCCFAT, through time. The relationship could drift for a variety of reasons. Slower economic growth in a state, relative to the nation, would result in LCFAT becoming smaller relative to USCONTROL. The relationship between N.OCCFAT and LCFAT may not remain constant because there might be some change in N.OCCFAT as pedestrian or motorcycling habits change. To allow for these possibilities, a trend term is included in the model. TRENDS is an arithmetic progression, each month of the estimating range having a value of 1 greater than the previous month.

As with the national model, a test is made to determine if, given there is a MUL effect, the effect increases, decreases, or remains the same over the period of enforcement. FIRSTQUARTERS is coded '1' for the first three months of MUL enforcement in a state, and '0' otherwise. MULDSQ is coded '1' for all months after the first three when a MUL is in effect, and '0' otherwise.

The variables LCFAT, USCONTROL, and N.OCCFAT are transformed into natural logarithms. The transformation will make the distributions more normal and all the coefficients can then be interpreted as percent changes.

3.2.2 Estimating Technique

The model presented above was estimated for six states using ordinary least squares (OLS) first. If a test for first order autocorrelation indicated that the errors for successive months were correlated, a correction was made using generalized least squares (GLS).

3.3 RESULTS: STATE MUL EFFECTS

Table 3-2 presents the results of estimating the model described above, and some variations, for the six large states which implemented MULs in 1985. Four models are presented for each state:

- o Basic Model
- o Extended Model
- o Non-Trend
- o First Quarter Model

In general the "basic model" provides the best estimate of the average effect of the MUL in the state over the period it was in force. We prefer this estimate to the estimate produced by fitting the model to the longer 1975-1985 period because the shorter 1980-1985 period is more relevant to the MUL implementation in 1985. Unfortunately, the range over which the model was fit makes a difference. We would have more confidence

TABLE 3-2. STATE MODEL RESULTS

	CONSTANT	MONTHS	USCONTROL	N.OCCFAT	MULDS	TREND	FIRST QUARTER	ESTIMATE TECHNIQUE	RAGNE	ADJ.R^2	F-VALUE
NEW YORK											
Basic Model	-2.85 (-1.51)	YES	1.07 (4.15)	-0.03 (-0.25)	-0.129 (-2.01)	-0.002 (-1.43)	No	OLS	1980-85	0.68	11.04
Extending Range	-1.94 (-2.03)	YES	0.78 (5.55)	0.24 (2.95)	-0.116 (-2.14)	-0.001 (-1.78)	No	GLS	1975-85	0.65	16.66
Non-Trend	-4.83 (-3.75)	YES	1.31 (6.47)	0.01 (0.06)	-0.202 (-5.12)	No	No	OLS	1980-85	0.67	11.47
First Quarter	-2.82 (-1.48)	YES	1.07 (4.10)	-0.03 (-0.23)	-0.12* (-1.79)	-0.002 (-1.43)	0.15 (-1.64)	OLS	1980-75	0.67	10.19
NEW JERSEY											
Basic Model	0.20 (0.09)	YES	0.51 (1.79)	0.06 (0.59)	-0.02 (-0.25)	-0.002 (-1.64)	No	OLS	1980-85	0.39	4.04
Extending Range	-2.12 (-2.52)	YES	0.81 (6.56)	0.05 (0.66)	-0.098 (-1.67)	0.000 (0.08)	No	OLS	1975-85	0.49	9.21
Non-Trend	-2.04 (-1.23)	YES	0.78 (3.29)	0.09 (0.89)	-0.112 (-2.07)	No	No	OLS	1980-85	0.37	4.02
First Quarter	0.20 (0.10)	YES	0.51 (1.77)	0.06 (0.57)	-0.025* (-0.29)	-0.002 (-1.63)	-0.006 (-0.05)	OLS	1980-85	0.38	3.72

*Coefficient for MULDSQ.

TABLE 3-2. STATE MODEL RESULTS (Cont.)

	CONSTANT	MONTHS	USCONTROL	N.OCCFAT	MULDS	TREND	FIRST QUARTER	ESTIMATE TECHNIQUE	RAGNE	ADJ.R^2	F-VALUE
ILLINOIS											
Basic Model	0.06 (0.04)	YES	0.62 (2.83)	0.05 (0.66)	-0.064 (-0.94)	-0.003 (-3.10)	No	OLS	1980-85	0.54	6.48
Extending Range	-0.74 (-0.75)	YES	0.71 (5.11)	0.09 (1.44)	-0.076 (-1.06)	-0.003 (-6.61)	No	GLS	1975-85	0.64	16.75
Non-Trend	-2.92 (-2.11)	YES	0.98 (4.92)	0.09 (1.03)	-0.179 (-2.90)	No	No	OLS	1980-85	0.47	5.44
First Quarter	-0.01 (-0.01)	YES	0.63 (2.87)	0.05 (0.63)	0.021* (0.24)	-0.003 (-3.08)	-0.150 (-1.676)	OLS	1980-85	0.54	6.22
MICHIGAN											
Basic Model	-2.80 (-1.54)	YES	0.96 (3.88)	0.03 (0.45)	-0.170 (-2.19)	0.001 (0.91)	No	OLS	1980-85	0.47	5.25
Extending Range	-0.50 (-0.49)	YES	0.67 (4.60)	0.07 (1.23)	-0.097 (-1.29)	-0.002 (-4.27)	No	GLS	1975-85	0.63	16.21
Non-Trend	-1.80 (-1.24)	YES	0.84 (4.04)	0.03 (0.36)	-0.132 (-2.02)	No	No	OLS	1980-85	0.47	5.58
First Quarter	-2.84 (-1.55)	YES	0.97 (3.90)	0.03 (0.36)	-0.115* (-1.13)	0.001 (0.91)	-0.225 (-2.21)	OLS	1980-85	0.47	4.94

*Coefficient for MULDSQ.

TABLE 3-2. STATE MODEL RESULTS (Cont.)

	CONSTANT	MONTHS	USCONTROL	N.OCCFAT	MULDS	TREND	FIRST QUARTER	ESTIMATE TECHNIQUE	RAGNE	ADJ.R^2	F-VALUE
TEXAS											
Basic Model	2.91 (2.22)	YES	0.24 (1.36)	0.19 (2.41)	-0.191 (-2.94)	-0.001 (-1.18)	No	GLS	1980-85	0.58	5.92
Extending Range	0.35 (0.49)	YES	0.60 (5.79)	0.12 (2.28)	-0.278 (-4.45)	0.002 (5.17)	No	GLS	1975-85	0.63	17.05
Non-Trend	2.01 (1.82)	YES	0.34 (2.10)	0.22 (2.88)	-0.22 (-3.64)	No	No	GLS	1980-85	0.57	6.08
First Quarter	insufficient data										
NORTH CAROLINA											
Basic Model	-5.74 (-3.35)	YES	1.34 (5.64)	0.03 (0.40)	-0.153 (-1.59)	0.003 (3.21)	No	OLS	1980-85	0.47	5.20
Extending Range	-0.39 (-0.36)	YES	0.61 (4.05)	0.07 (1.13)	-0.091 (-0.88)	0.001 (1.64)	No	GLS	1975-85	0.42	6.38
Non-Trend	-2.63 (-1.73)	YES	0.94 (4.31)	0.06 (0.73)	-0.035 (-0.36)	No	No	OLS	1980-85	0.38	4.16
First Quarter	insufficient data										

confidence in the stability and usefulness of the models if the coefficients were not sensitive to changes in the range of data used to develop the model. The "extended range" model is included to show the sensitivity of the coefficients to this longer range of data.

The model "non-trend" shows the importance of the TREND variable in controlling for the changing relationship between LCFAT and the other variables in the model. In most cases, excluding the TREND variable makes a substantial difference in the estimated value of the MUL effect.

Finally, the model "first quarter" tests the hypothesis that the MUL is much more effective in reducing fatalities during the first quarter after it is introduced.

The results in the table show that there is a substantial variation in the MUL effect among states. The effect varies from -.02 in New Jersey where it cannot be statistically distinguished from zero, to -.19 in Texas where it is statistically significant at the 99 percent confidence level. Generally, the states cluster into two groups: those with MULs with effectiveness of more than 10 percent, and those where the effect is small and insignificant. New York, Texas, and Michigan fall into the first group; North Carolina is on the edge of this group. The other group consists of New Jersey, and Illinois. Based on these results it does not appear that there is a single MUL effect. Rather it depends on the specifics of the law, how it is enforced, and the receptiveness of the population covered by the law.

The other key question that can be addressed by these results is: What is the pattern of the MUL effect over time? The national model showed the first quarter effect to be 12 percent, while subsequent quarters have only about a two percent effect on law covered occupant fatalities. Tests of this pattern of effectiveness in the state models are ambiguous. Table 3-3 summarizes these results. Michigan and, to a lesser extent, Illinois results indicate a much stronger first quarter effect than is found in subsequent quarters. The New Jersey and New York results do not reveal much difference between the first and subsequent quarters. The other two states do not have enough data after MUL implementation to test this hypothesis. So, while some states have a clear pattern of MUL effect over time, (high first quarter effect followed by a very small continuing effect) others do not show this pattern.

TABLE 3-3. TESTS OF THE FIRST QUARTER EFFECT USING STATE MODELS

State	Average MUL Effect	First Quarter Effect	Subsequent Quarter Effect
New York	-.13*	-.15	-.12
New Jersey	-.02	-.01	-.03
Illinois	-.06	-.15	.02
Michigan	-.17*	-.23*	-.12

*Statistically significant at the 95 percent confidence level.

The individual state dummy variables accounted for about half of the models adjusted R-squared in the national model, indicating the importance and magnitude of the state-to-state variation in fatality rates. These state models reinforce this finding. Though the state models each have the same terms, the size of the coefficients and their significance vary widely.

3.4 RESULTS: ESTIMATES OF LIVES SAVED

Table 3-4 shows the results of applying the effects estimated in the state models to the law covered fatalities in each state. The table also shows the range in the estimates which would result if the estimated effect was actually one standard deviation higher or lower than the best estimate. The best estimate of the number of lives saved based on the state models is about 500. More than eighty percent of these lives are saved in three states: New York, Texas, and Michigan. Notice that the one standard deviation range of the estimated lives saved is quite broad, 250 to 839, indicating the uncertainty associated with these estimates.

TABLE 3-4. STATE MODEL ESTIMATES OF LIVES SAVED

	Date MUL Implemented	Fatalities While MUL Enforced	Fatalities Fractional Reduction	Estimated Reduction	RANGE ± Standard Error Fractional Reduction	RANGE ± Standard Error Estimated Reduction
New York	1/1/85	976	.13*	146	.07-.19	73-229
New Jersey	3/1/85	461	.02	9	0-.10	0-51
Illinois	7/1/85	459	.06	29	0-.13	0-69
Michigan	7/1/85	490	.17*	100	.09-.25	48-163
Texas	9/1/85	707	.19*	166	.13-.25	105-236
North Carolina	10/1/85	229	.15	40	.05-.25	12-76
All Other MUL States		274		?		?
TOTAL		3,596		490		238-824

*Statistically significant at the 95 percent confidence level.

4. CONCLUSIONS

Based on the limited experience with mandatory seatbelt use laws (MULs) in 1985, we estimate that 6.7 percent of the covered occupant fatalities which would have occurred in 1985 in states with MULs were avoided. This is precisely the same estimate that we obtained from the simple comparison of 1984 and 1985. However, we also found that the estimate is actually an average of many different effects. Some states had large effects in the ten to twenty percent range, notably New York, Texas, Michigan, and probably North Carolina. Others had effects which could not be distinguished statistically from zero. New Jersey, and Illinois fall into this group. During the first quarter after implementation, the effect appears to be stronger (12%) than in subsequent quarters when the effect is not statistically different from zero, but more experience with MULs is needed to have confidence in this finding. The type of enforcement also appears to influence the effectiveness of MULs. Primary enforcement states (New York, Texas, North Carolina and Hawaii) appear to have stronger effects (16%) than secondary enforcement states (2%), though other factors could be responsible for this difference among states.

Our best estimate of the number of lives saved in 1985 because of MULs is 258, though there is substantial uncertainty in this estimate. It could be as high as 800 or as low as 150.

How do these results, however tentative, compare to the promise that MULs originally offered? In 1985 there were 43,795 highway fatalities. 26,138 of these (60 percent) would be affected if all states had MULs. If everyone affected wore their seatbelt and the best estimates of seatbelt effectiveness are correct, then about 40 percent of these 26,138 fatalities[2] could be avoided. We estimate that in 1985 about 6.7 percent of the covered fatalities were avoided because of MULs. In 1986 the results may be even lower if the strong first quarter effect proves true because the large first quarter effects of many states were experienced in 1985 and these states will be contributing only small

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[2]This estimate is similar to those found in Hedlund (1) and is based on a 45% effectiveness; a before MUL usage of 15%,u_1; and after MUL usage of 100%,u_2; and the following formula: proportionate fatality reduction = $(e(u_2-u_1)/(1-3*u_1))$.

fatality savings in 1986. Even the 6.7 percent is a small fraction of the 40 percent potential. As Table 4-1 shows, this level of fatality reduction is consistent with seatbelt usage of about 30 percent. Though reported usage is somewhat higher than this level, seatbelt usage did not rise to the level needed for the effectiveness of MULs to approach the original promise.

TABLE 4-1. 1985 FATALITY REDUCTION: SENSITIVITY TO SEAT BELT USAGE

	Actual	Theoretical Sensitivity to Belt Usage	
Seat Belt Effectiveness	?	.45	.45
Belt Usage			
Before (u_1)	?	.15	.15
After (u_2)	?	1.00	.30
Proportionate			
Fatality Reduction (D_C)*	6.7%	41%	7%
Covered Fatalities	3,850	26,396**	26,396**
Lives Saved	258	10,827	1,847

*$D_C = [e\ (u_2 - u_1)/(1 - eu_1)] * 100$

**Actual covered occupant fatalities plus 258 avoided by MULs.

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