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Mandatory Seat Belt Laws in Eight States: A Time-Series Evaluation

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We examined state-specific and aggregate effects of U.S. legislation requiring the use of seat belts among front-seat motor vehicle occupants. Effects of compulsory seat belt use on the number of occupants fatally injured in traffic crashes were examined in the first eight states adopting such laws. Monthly data on crash fatalities between January 1976 and June 1986 were analyzed using Box-Tiao intervention analysis time-series methods. Because the new laws apply only to front-seat occupants, front-seat occupant fatalities were compared with: (1) rear-seat fatalities; (2) nonoccupant fatalities (motorcyclists, pedalcyclists, pedestrians); and (3) fatalities among front-seat occupants in neighboring states without compulsory seat belt use. Exposure to risk of crash involvement was controlled by analyzing fatality rates per vehicle mile traveled. Results revealed a statistically significant decline of 8.7% in the rate of front-seat fatalities in the first eight states with seat belt laws. The fatality rate declined 9.9% in states with primary enforcement laws and 6.8% in states with secondary enforcement only. Rates of rear-seat and nonoccupant fatalities did not change when the belt laws were implemented.

Use of automobile safety belts reduces the probability of death in a motor vehicle crash by 30% to 50% (Evans, 1986; O'Day & Flora, 1982). To increase belt use, laws requiring their use were first implemented in Australia in 1971 and spread to a number of European countries, Canadian provinces, and other jurisdictions in the subsequent decade. In the mid-1980s, selected states in the United States implemented compulsory belt use laws. The objective of this study was to evaluate the effects of belt laws on motor vehicle fatality rates in the first eight U.S. states implementing such laws.

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Numerous studies have found increased belt use and reduced rates of traffic fatalities following implementation of compulsory belt use laws. Although effects varied, rates of seat belt use have typically doubled or tripled immediately after belt laws took effect, both in the United States (Table 1) and in other countries (Table 2). After immediate dramatic increases in belt use at the time belt laws first took effect, many jurisdictions experienced some decay in use over the subsequent months or years (Campbell, Stewart, & Campbell, 1986). Estimated fatality reductions following implementation of compulsory belt use vary widely from country to country (from 0 to 80%; Table 3). Within the United States, preliminary estimates of the effect of belt laws on fatalities cluster much more narrowly in the range of 1% to 20% (Table 4). Many of these studies, especially the earlier ones, used nonrandom samples, inadequate control groups, and unreported analytic methods.

METHOD

Research Design

We evaluated eight U.S. states that implemented mandatory seat belt use laws prior to October 1985, using monthly data on traffic fatalities from January 1976 through June 1986. A longitudinal or time-series design was used to ensure that observed changes in fatalities were not due to long-term cycles or trends or were not a result of a regression-to-the-mean effect. In the absence of random assignment, time-series designs with comparison groups have the highest possible levels of internal validity (Cook & Campbell, 1979).

To further strengthen causal inferences concerning the relationship between compulsory seat belt laws and traffic fatalities, we examined two types of control groups that one would not expect to be affected by the new laws. First, we paired each "experimental" state that recently implemented a seat belt law with a neighboring "control" state that did not implement a belt law during the period under study. States analyzed include: New York with a belt law versus Pennsylvania without, New Jersey versus Maryland, Michigan versus Ohio, Illinois versus Indiana, Texas versus Georgia, Nebraska versus Kansas, Missouri versus Tennessee, and North Carolina versus Virginia.[1] Second, within the experimental states we examined two categories of traffic fatalities not directly affected by the new laws – rear-seat occupants and nonoccupants (including pedestrians, motorcyclists, and pedalcyclists).

Data Collection

All fatality data were based on the Fatal Accident Reporting System (FARS) maintained by the U.S. National Highway Traffic Safety Administration (NHTSA). Monthly counts of the number of fatalities were calculated separately within each state for front-seat occupants, rear-seat occupants, and nonoccupants. Occupant fatalities included only those traveling in passenger cars, vans, light trucks, and utility vehicles. Medium and heavy trucks, buses, and a variety of special vehicles were excluded because some are exempted from provisions of the seat belt laws and others were covered by preexisting regulations requiring seat belt use. All analyses were limited to persons age 10 and over because compulsory restraint use laws for young children were implemented several years before the adult seat belt laws took effect. Although most child restraint laws are limited to those under age 4, spill-over effects on older children have been reported (Wagenaar & Webster, 1986). The length of the resulting time series varied from 107 baseline months in New York to 117 baseline months in North Carolina, and from 9 postlaw months in North Carolina to 19 postlaw months in New York.

Exposure to risk of crash involvement was controlled by dividing all of the fatality frequency time-series by the number of vehicle miles traveled (VMT) within each of the states under study. The resulting rates of fatalities per VMT were used in all subsequent

[1]Two of the comparison states, Ohio and Tennessee, implemented compulsory belt use laws in the spring of 1986. Analyses involving these states were limited to the period in which no belt law was in effect.

TABLE 1
EFFECTS OF U.S. SEAT BELT LAWS ON RESTRAINT USE

Jurisdiction	Effective Month	Month Observed	Use Rate	Source
New York	12/84	10/84	16%	Rood, Kraichy, & Carubia (1985)
		4/85	57%	
		9/85	46%	
		1/85	69%	Williams, Wells, & Lund (1986)
		4/85	60%	
		2/86	44%	
		4/85	63%	Pace, Thailer, & Kwiatkowski (1986)
		4&7/86	37%	
New Jersey	3/85		18%	Brick et al. (cited in Williams et al., 1986)
		7/85	40%	
		11/84	16%	Williams et al. (1986)
		3/85	51%	
		7/85	44%	
		4/86	38%	
Illinois	7/85	4/85	16%	Mortimer (1986)
		7/85	40%	
		12/85	35%[a]	
		1/86	29%	
		3/86	32%	
		6/86	34%	
Michigan	7/85	12/84	18%	Wagenaar, Molnar, & Businski (1987)
		4/85	25%	
		7/85	61%	
		12/85	44%	
		4/86	44%	
		7/86	47%	
		12/86	44%	
Texas	9/85	1-6/85	15%	Hatfield, Hinshaw, Bunch, & Bremmer (1985)
		1-6/86	66%	Bunch, Hatfield, Hinshaw, & Womack (1986)
		3/86	75%	Dept. of Highways (cited in Campbell et al., 1986)
Nebraska	9/85	11/85	26%	Office of Highway Safety (cited in Campbell et al., 1986)
		11/85	44%	
		2/86	38%	
		2/87	29%	Insurance Institute for Highway Safety (IIHS)(1987b)
Missouri	10/85	7/85	12%	Missouri Safety Center (cited in Campbell et al.,1986)
		10/85	19%	
North Carolina	10/85	9/85	25%	Campbell, Stewart, & Campbell (1986)
		11/85	44%	
		1/86	42%	
		3/86	45%	
		5/86	48%	
		2/87	78%	IIHS (1987a)

[a]Drivers only.

TABLE 2
EFFECTS OF NON-U.S. SEAT BELT LAWS ON RESTRAINT USE

Jurisdiction	Effective Month	Period Observed	Use Rate	Comments[a]	Source
Australia					
Victoria	12/70	5/71	32%-48%	Front-seat occupants	Vulcan (1977)
		2/72	47%-60%		
		2/73	52%-65%		
		2/74	67%-79%		
		2/75	73%-79%		
		2/76	73%-88%		
		prelaw	18%	Observation date not cited	Joubert (1979)
		postlaw	64%	Rural	
		postlaw	75%	Urban	
Melbourne	12/70	11/82	95%	Drivers	Manders (1984)
		3/84	96%	Drivers	
Adelaide, South Australia	12/71	10/71	23%	Occupants with belts available	Johinke (1977)
		10/72	78%		
		10/75	66%		
		mid 76	84%		
		10/64	8%	All seating positions	Crinion, Foldvary, Lane (1975)
		10/70	14%		
		10/71	23%		
		10/72	78%		
		11/82	91%	Drivers	Road Traffic Board (1983)
		11/82	85%	Front-seat passengers	
		11/82	61%	Rear-seat passengers	
Queensland	1/72	12/72	90%		Seeney (1977)
New South Wales-Sydney Metro	10/71	4/71	30%	Drivers	Schnerring (1983)
		10/71	60%	Drivers	
		11/71	76%	Drivers	
		12/72	89%	Drivers	
		12/73	91%	Drivers	
		7/75	91%	Drivers	
		7/77	91%	Drivers	
		7/79	89%	Drivers	
		7/81	84%	Drivers	
Austria	7/76	prelaw	5%-10%	Urban	Marburger (1986)
		prelaw	20%-25%	Rural	
		postlaw	10%-15%	Urban	
		postlaw	40%	Urban	
		9/84	81%	Urban	
		9/84	82%	Rural	
		8/85	81%	Urban	
		8/85	82%	Rural	
Belgium	6/75	prelaw	17%		Fisher (1980)
		postlaw	87%		
		11/84	70%	Rural	Marburger (1986)
		11/84	60%	Urban	

TABLE 2
(CONTINUED)

Jurisdiction	Effective Month	Period Observed	Use Rate	Comments[a]	Source
Canada					
British Columbia	10/77	prelaw	20%-24%		Rockerbie (personal communication, 2/16/83)
		postlaw	50%		
		4/83	55%		British Columbia Research (1983)
		6/83	67%		
		11/83	67%		Arora (1985)
New Brunswick	9/83	11/82	4%	Drivers	Arora (1985)
		11/83	66%	Drivers	
		8/84	73%		New Brunswick Dept. of Transportation (1984)
Newfoundland	7/82	11/81	9%		Arora (1982)
		11/82	68%		
		11/83	76%	Urban	Arora (1985)
		11/83	61%	Rural	
		7/84	74%		Murray (1984)
Manitoba	1/84	11/83	11%		Arora (1985)
		11/84	62%		
		6/84	79%	Drivers	DataCom (1984)
Ontario	1/76	12/75	21%		Snow (1979)
		2/76	77%		
		6/76	50%		
		5/77	50%	Increased enforcement	Pierce (1979)
		5/78	65%	mid '77	
		9-12/80	49%		Matthews (1982)
		11/82	49%		Arora (1985)
		11/83	60%		
		5/84	70%		Jonah & Lawson (1986)
Montreal	8/76	5-6/76	15%	Drivers	Stulginskas & Pless (1983)
		5-6/77	33%	Drivers	
		5-6/78	45%	Drivers	
		5-6/81	56%	Drivers	
Quebec	8/76	11/82	68%	Drivers	Arora (1985)
		11/83	60%	Drivers	
		6/83	60%	Urban drivers	Regie de l'assurance Automobile du Quebec (cited in Jonah & Lawson, 1986)
			74%	Freeway drivers	

TABLE 2
(CONTINUED)

Jurisdiction	Effective Month	Period Observed	Use Rate	Comments[a]	Source
Canada (cont.)					
Saskatchewan	7/77	prelaw	26%	Drivers	Simpson & Warren (1981)
		postlaw	78%	Drivers	
		5/77	24%		Shiels (1978)
		7/77	65%		
		10/77	73%		
		5/78	60%		
		7/77	52%	Drivers	Bergen, Watson, Rivett, & Shiels (1979)
		10/77	70%	Drivers	
		5/78	55%	Drivers	
		5/79	70%	Drivers	
		11/80	56%	Drivers	Arora (1982, 1985)
		11/81	49%	Drivers	
		11/82	48%	Drivers	
		11/83	54%	Drivers	
Denmark	1/76	prelaw	19%		Marburger (1986)
		postlaw	74%		
England	2/83	1/83	43%		Ashton, Mackay, & Camm (1983)
		5/83	95%		
		2/83	90%		Mackay (1984a, 1984b)
Finland	7/75	prelaw	15%-20%		Oranen (1977)
		6/75	30%	Highway	
		6/75	9%	Urban	
		8/75	68%	Highway	
		8/75	53%	Urban	
		7/76	64%	Highway	
		8/76	37%	Urban	
		prelaw	8%	Urban	Berard-Anderson (cited in Fisher, 1980)
		prelaw	31%	Rural	
		postlaw	38%	Urban	
		postlaw	66%	Rural	
		8-9/78	71%	Highways	Oranen & Koivurova (1981)
		8-9/78	41%	Urban	
		4/82	87%	Urban	Marburger (1986)
		4/82	86%	Rural	
		9/83	82%	Urban	
		9/83	92%	Rural	
France	7/73	prelaw	20%-25%		Fisher (1980)
		7/73	80%		
		10/73	50%		

TABLE 2
(CONTINUED)

Jurisdiction	Effective Month	Period Observed	Use Rate	Comments[a]	Source
France (cont.)	7/73	1972	20%		Chodkiewicz & Dubarry (1977)
		1973	26%		
		1974	67%		
		1975	80%		
		7/73	80%	Law applied to rural only	Gerondeau (1979
		11/73	50%		
		early '74	80%		
		1979	70%-75%		
		1974	54%	Law applied to rural only	Gerondeau (1981)
		1975	76%		
		1976	79%		
		1977	72%		
		1978	67%		
		1979	69%	Law expanded to all roads	
		1980	79%	in 1979	
		1982	95%	Highways	Hartemann et al. (1984)
		1982	75%	Other nonurban	
Ireland	2/79	Fall '78	19%	Drivers on national roads	Hearne (1981)
		Fall '78	9%	Drivers on other roads	
		Summer '79	46%	Drivers on national roads	
		Summer '79	38%	Drivers on national roads	
Israel	7/75	prelaw	6%		Hakkert, Zaidel, & Sarelle (1981)
		8/75	77%		
		1976	83%		
		1977	70%		
Netherlands	6/75	1974	11%	Urban	Fisher (1980)
		1974	24%	Rural	
		7/76	58%	Urban	
		7/76	75%	Rural	
		1983	46%	Urban	Vaaje (1986)
		1983	65%	Rural	
New Zealand	6/72	5/72	40%		Toomath (1977)
		6/72	87%		
		1974	83%		
		1975	89%		
Norway	9/75	prelaw	15%	Urban	Fisher (1980)
		prelaw	37%	Rural	
		1976	28%	Urban	
		1976	59%	Rural	
		1977	30%	Urban	
		1977	63%	Rural	
		3/80	74%	Urban	Oranen & Koivurova (1981)
		3/80	90%	Rural	

TABLE 2
(CONTINUED)

Jurisdiction	Effective Month	Period Observed	Use Rate	Comments[a]	Source
Puerto Rico	1/74	7/73	5%		Fisher (1980)
		5/74	24%		
		9/74	7%		
		1/76	34%		
		5/77	14%		
South Africa	12/77	11/77	18%		Fernie (1980)
		3/78	62%		
		9/79	70%		
Sweden	1/75	1974	35%		Bohlin (1973)
		1975	84%		
		1974	36%		Tingvall (cited in Fisher, 1980)
		1978	79%	Urban	
		1978	87%	Rural	
		1983	80%		Norin, Carlsson, & Korner (1984)
Switzerland	1/76	prelaw	35%		Fisher (1980)
		2/76	95%	Expressway drivers	
		2/76	92%	Rural drivers	
		2/76	89%	Urban drivers	
		9/78	64%	Expressway drivers	
		9/78	46%	Rural drivers	
		9/78	33%	Urban drivers	
	7/81[b]	1982	77%	Expressways	Andreasson (1983)
		1982	76%	Rural	
		1982	62%	Urban	
West Germany	1/76	8/75	28%		Federal Institute of Streets (cited in Fisher, 1980)
		11/75	32%		
		1/76	50%		
		3/77	46%		
		9/77	48%		
		9/78	58%		
		9/84	92%	Fines began 8/85	Marburger (1986)
		3/86	94%		

[a]Drivers and front-seat passengers unless otherwise noted. [b]Switzerland's 1976 law declared invalid by the Supreme Court in 9/77 and reinstated by the government on 7/1/81.

analyses. State-specific VMT figures by month were obtained from the U.S. Federal Highway Administration and are based on traffic counter and motor fuel sales data. Pre- and postlaw means and standard deviations for each major time-series analyzed are shown in Table 5.

Statistical Methods

We used the time-series intervention analysis methods of Box and Tiao (1975) and Box and Jenkins (1976). On a conceptual level, the analytic strategy involves explaining as much as possible of the variance in fatality

TABLE 3
EFFECTS OF NON-U.S. SEAT BELT LAWS ON FATALITIES

Jurisdiction	Effective Month	Post-law Months	Fatality Change	Investigators
Australia				
Victoria	1/71	9	-15%**	Foldvary & Lane (1974)
		48	-37%	Trinca & Dooley (1977)
		10	-15%	Andreassend (1976)
		12	-15%	Joubert (1979)
		84	***	McDermott & Hough (1979)
		144	-60%	Trinca (1984)
Queensland	1/72		-14%	Johinke (1977)
			-46%	Bhattacharyya & Layton (1979)
South Australia	11/71	12	-8%	Crinion et al. (1975)
Australia (overall)			-20%	Fisher (1980)
Canada				
Ontario	1/76	6	-13%	Snow (1979)
		72	-26%**	Jonah & Lawson (1984)
		72	-12%	Hedlund (1986)
Saskatchewan	7/77	12	-20%	Shiels (1978)
		52	-37%* -35%**	Jonah & Lawson (1984)[a]
		52	-29%	Hedlund (1986)
British Columbia	10/77	51	-30%***	Jonah & Lawson (1984)
		60	-52%	Hedlund (1986)
Quebec	8/76	65	-17%	Jonah & Lawson (1984)
		65	-18%	Hedlund (1986)
Denmark	1/76	12	-1%	Nordic Traffic Safety Council (1984)
		12	-13%	Hedlund (1986)
England	2/83	11	-25%	Mackay (1984b)
		3	-80%****	Pye & Waters (1984)
		23	-18%[b] -25%[c]	Durbin & Harvey (1985)

TABLE 3
(CONTINUED)

Jurisdiction	Effective Month	Post-law Months	Fatality Change	Investigators
France	7/73	23	-21%	Chodkiewicz & Dubarry (1977)
		114	-50%	Hartemann et al. (1984)
Ireland	2/79	11	-0.7%	Hearne (1981)
Israel	7/75	30	-42%[b] -44%[c]	Hakkert et al. (1981)
		30	-41%	Hedlund (1986)
New Zealand	6/72	24	+3%[d]	Toomath (1977)
		24	-43%	Hedlund (1986)
Norway	9/79		-21%	McCarthy, Taylor, Sandford, & Lange (1984)
		12	-10%	Nordic Traffic Safety Council (1984)
	9/75	12	-29%	Hedlund (1986)
South Africa	12/77		-8%	McCarthy et al. (1984)
Sweden	1/75		-14%	McCarthy et al. (1984)
		12	-12%	Bohlin (1973)
		12	-12%	Norin et al. (1984)
Switzerland	1/76	12	-12%	Fisher (1980)
	7/81[e]	12	-15%	Hedlund (1986)
West Germany	1/76	6[f]	-25%	Hedlund (1986)

[a]Crash data for the period 7/1/77 to 12/31/82 were analyzed; actual fatality rates were significantly different from predicted rates only in 1980 ($p < .10$) and 1981 ($p < .05$) [b]Drivers only. [c]Front-seat passengers. [d]In contrast, nonoccupant fatalities increased almost 40% during this period. [e]Switzerland's 1976 law declared invalid by the Supreme Court in 9/77 and reinstated by the government on 7/1/81. [f]This study compared the pre- and post-fine period 1-6/84 to 1-6/85.
*$p < .10$. **$p < .05$. ***$p < .01$. ****$p < .001$.

rates on the basis of the past history of those rates, before attributing any of the variance to an exogenous variable, such as implementation of a seat belt law. This approach of intervention analysis was particularly appropriate for the current study, because the objective was to identify significant shifts in fatality rates associated with seat belt laws, independent of observed regularities in the history of each series. Ordinary least-squares regression and other commonly used statistical procedures were not appropriate for the present study because they assume independent observations. However, a series of observations over time, such as the fatality rate series analyzed here, are highly auto-

TABLE 4
EFFECTS OF U.S. SEAT BELT LAWS ON FATALITITES

Jurisdiction	Effective Month	Post-law Months	Fatality Change	Investigators
New York	12/84	9	-9%**	Lund, Pollner, & Williams (1986)
		9	-15%+	Hedlund (1986)
		6	-20%+	Latimer & Lave (1987)
		3	-27%	Pace et al. (1986)
		13	-5%***	Lund, Zador, & Pollner (1986)
		13	-8%*	Campbell et al. (1986)
		19	-7%	Hoxie & Skinner (1987)
New Jersey	3/85	10	-4%	Lund, Zador, & Pollner (1986)
		10	-6%*	Campbell et al. (1986)
		16	-2%	Hoxie & Skinner (1987)
Michigan	7/85	12	-10%	Wagenaar, Maybee, & Sullivan (1987)
		6	-4%	Lund, Zador, & Pollner (1986)
		6	-16%*	Campbell et al. (1986)
		12	-14%*	Hoxie & Skinner (1987)
Illinois	7/85	9	-3%+	Mortimer (1986)
		6	-7%	Lund, Zador, & Pollner (1986)
	7/86	6	-9%	Campbell et al. (1986)
		12	-1%	Hoxie & Skinner (1987)
Texas	9/85	4	-18%***	Campbell et al. (1986)
		10	-18%***	Hoxie & Skinner (1987)
Nebraska	9/85	4	-11%	Campbell et al. (1986)
Missouri	9/85	3	+5%	Campbell et al. (1986)
		9	+18%***	Hoxie & Skinner (1987)
North Carolina	10/85	3	-0.4%	Campbell et al. (1986)
		9	-5%	Hoxie & Skinner (1987)
All[a]	b	b	-7%+	Partyka (1987)
			-10%	Campbell et al. (1986)
			-6%[c]	Hoxie & Skinner (1987)

[a]NY, NJ, IL, MI, TX, NB, MO, NC. [b]Different for each state, depending on date law enacted. [c]Total excludes NB.
*p < .10. **p < .05. ***p < .01. +Significance level not reported.

TABLE 5
MONTHLY FATALITY RATE MEANS AND STANDARD DEVIATIONS
BEFORE AND AFTER MANDATORY SEAT BELT LAWS

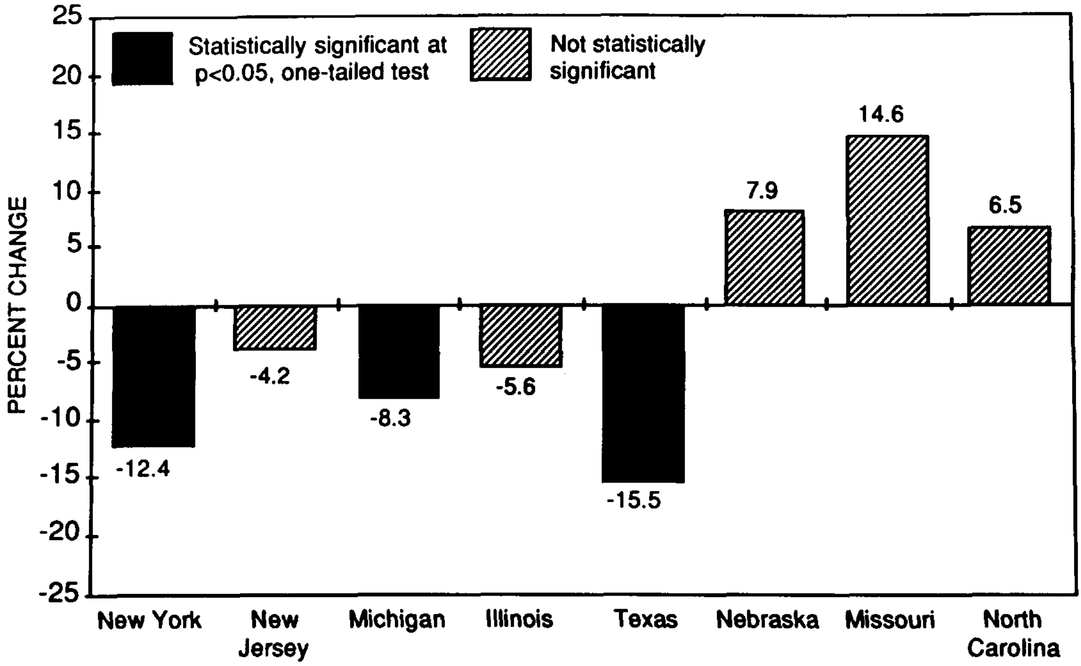
Comparison	Prelaw Mean	Prelaw SD	Postlaw Mean	Postlaw SD
Rate of front-seat fatalities per VMT				
New York	158.14	32.79	113.87	22.39
New Jersey	116.63	21.30	99.63	14.40
Michigan	165.84	36.06	132.76	26.90
Illinois	168.42	38.61	123.25	17.07
Texas	213.45	34.39	139.70	9.01
Nebraska	182.26	61.46	155.61	38.09
Missouri	201.77	45.75	189.31	26.08
North Carolina	211.44	37.45	201.84	22.76
Relative rate of front-seat fatalities per VMT				
New York vs. Pennsylvania	.95	.14	.75	.11
New Jersey vs. Maryland	.89	.24	.78	.19
Michigan vs. Ohio	1.07	.21	.97	.19
Illinois vs. Indiana	1.10	.35	.84	.14
Texas vs. Georgia	1.14	.23	.88	.18
Nebraska vs. Kansas	1.01	.44	.84	.24
Missouri vs. Tennessee	1.17	.32	.89	.21
North Carolina vs. Virginia	1.40	.33	1.46	.22
Aggregate relative rate of front-seat fatalities per VMT				
Eight belt-law vs. eight comparison states	1.07	.09	.89	.04
Aggregate rate of rear-seat fatalities per VMT				
Eight belt law states	12.48	3.18	9.54	1.24
Aggregate rate of nonoccupant fatalities per VMT				
Eight belt law states	66.92	18.21	41.90	3.92
Aggregate rate of front-seat fatalities per VMT by Enforcement provision				
Primary enforcement	191.14	29.48	128.76	8.86
Secondary enforcement	153.59	25.39	121.19	13.98

correlated, violating the assumption of independence and leading to biased standard error estimates using conventional methods.

Baseline Auto-regressive Integrated Moving Average (ARIMA) models were iteratively developed for each time series, repeatedly going through cycles of specifying a model, estimating it, and evaluating its adequacy in terms of accounting for all significant autocorrelation patterns in the series. All of the time series were natural-logarithm transformed prior to parameter estimation to reduce heteroscedasticity. All of the final models met the multiple criteria for model adequacy identified by Box and Jenkins (1976), including significant noise model parameters, low correlations among parameters, and insignificant residual autocorrelations.

Transfer functions were added to the noise models to test for effects of seat belt laws. Given the short postlaw period for which data were available, simple shift transfer function models were used to represent potential effects of the belt laws. Additional transfer functions were added to the

FIGURE 1
PERCENT CHANGE IN RATE OF FATALITIES PER VMT
ASSOCIATED WITH SEAT BELT LAWS:
FRONT-SEAT OCCUPANTS AGE 10 AND OVER



models for selected time series. The substantial decline in the fatality rate in 1982 in most of the states was controlled by including a simple shift transfer function. The 1982 decline was due to a variety of factors, including a major economic recession, campaigns to reduce alcohol-impaired driving, and changes in the age structure of the population (Hedlund, Arnold, Cerelli, Partyka, & Hoxie, 1984). Our objective was not to fully elucidate the causal structure underlying those fatality reductions, but rather to statistically control for those reductions when estimating the effects of recent compulsory seat belt laws.

Because the models are intrinsically nonlinear, the Gauss-Marquardt backcasting algorithm implemented in the software package BMDP2T was used to estimate the parameters (Dixon et al., 1983). All parameter estimates in the logarithm metric were converted to an estimated percent change in the series after the seat belt law, from levels expected given baseline patterns, using $(e^{\omega}-1)100$. Final statistical models for aggregate series are shown in the Appendix.

RESULTS

Significant declines in the rate of front-seat occupant deaths per VMT occurred in three of the eight states with mandatory seat belt laws (Figure 1). The fatality rate declined 8.3% in Michigan, 12.4% in New York, and 15.5% in Texas. Intervention parameter estimates were in the expected direction (though not significant) in New Jersey and Illinois. The fatality rate increased in Missouri, Nebraska, and North Carolina, but only in Missouri was the estimated increase larger than two standard errors.[2] While these analyses control for long-term trends and cycles within each state, and control for changes in exposure via rates per

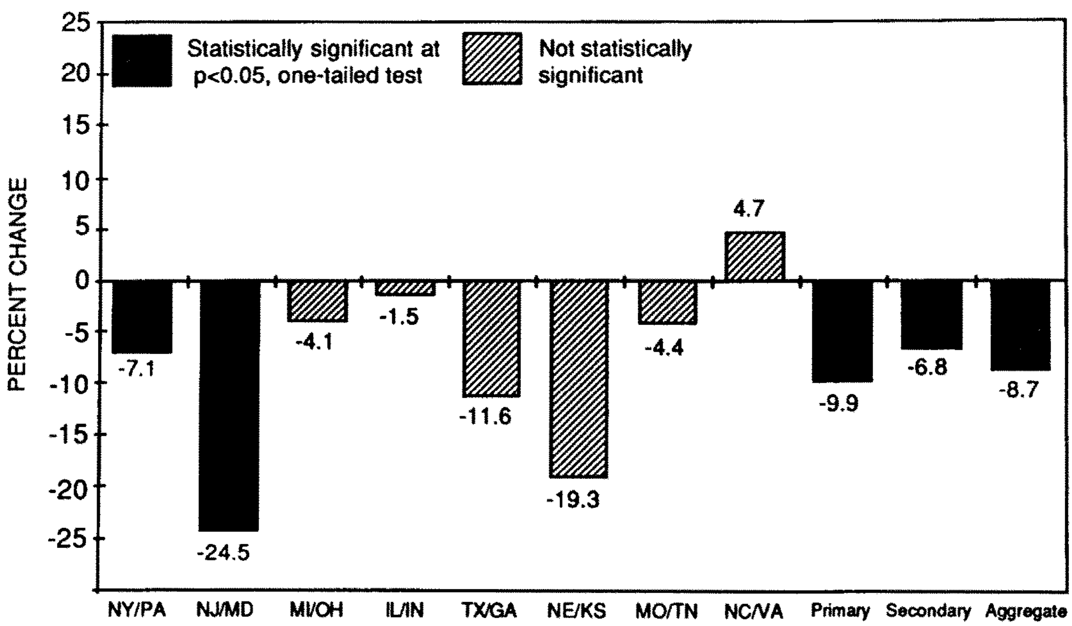
[2]Technically not statistically significant because we hypothesized a fatality reduction following implementation of belt laws and thus used one-tailed tests.

VMT, these state-specific changes in fatalities may still simply reflect broader regional or national changes due to other factors. To ensure that observed fatality changes were associated with the seat belt laws and not other factors, we analyzed the rate of fatalities per VMT in a state with a new belt law *relative to* the rate of fatalities per VMT in a neighboring state without a belt law during the period studied. In other words, the fatality rate in the target state was divided by the rate in the comparison state.

Analyses of the relative rates again indicated significant declines in fatalities associated with seat belt laws in three of the eight states: New York, 7.1%; New Jersey, 24.5%; and Nebraska, 19.3% (Figure 2). However, as noted in the previous paragraph, two of these three, (New Jersey and Nebraska) showed no significant decrease when examining the state alone, without taking into account the experience in comparison states. In addition to significant reductions in the relative rates of fatalities in three states, time-series modeling produced estimates in the expected direction (although not significant) in an additional four states.

Clearly, the small number of postlaw data points available (9 to 19 months), and the substantial baseline variability in fatality rates over time, results in moderately large standard errors and what appear to be inconsistent results across states. To reduce this background variation, we combined the eight belt-law states, and estimated the aggregate effect of the belt laws in these eight states. The state-specific time series were aligned on the month each state's belt law took effect, and the number of fatalities and amount of vehicle mileage traveled were summed. The eight comparison states were similarly summed. The result was a time series in which each month no longer represented a specific month in time, but rather represented the ordinal month from the point at which belt laws were implemented. Dividing the fatality rate per VMT for the belt-law states as a group by the fatality rate per VMT for the comparison states as a

FIGURE 2
PERCENT CHANGE IN RATE OF FATALITIES PER VMT
AMONG FRONT-SEAT OCCUPANTS AGE 10 AND OVER:
BELT-LAW STATES RELATIVE TO COMPARISON STATES



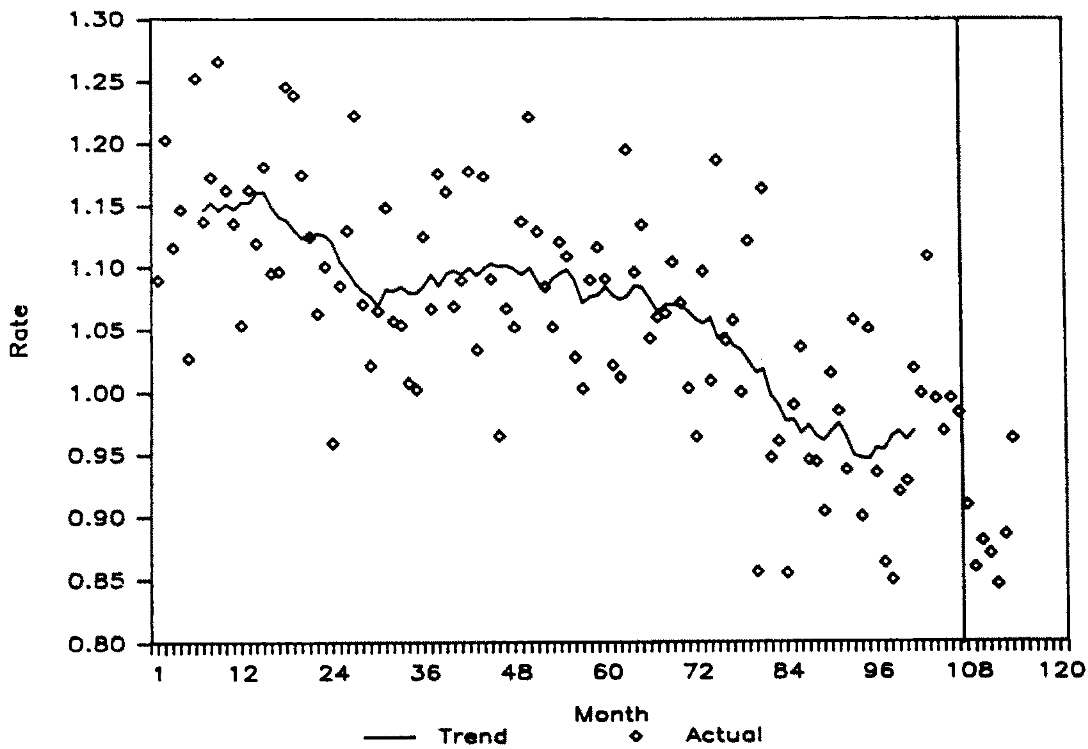
group resulted in an aggregate relative rate (Figure 3). Time-series modeling of the aggregate relative rate estimated a statistically significant 8.7% decline associated with belt laws in these states.

One obvious explanation for the differential effects of seat belt laws across states is the size of the change in belt use caused by the law. Most states experienced an increase in belt use from about 16% before to 45% a few months after the laws took effect (Table 1). Texas had a larger than average increase in belt use (from 15% to 66%) and Illinois a smaller than average increase (from 16% to 30%). Given the different survey methods used in each state, and the standard errors of our estimates of belt law effects on fatalities, we are not in a position to argue that cross-state differences in our fatality reduction estimates reflect differences in belt use rates across states.

Nevertheless, specific provisions of the law, such as primary versus secondary enforcement,[3] and the intensity with which it is enforced are expected to influence belt use rates. To take into account these major differences in the laws across states, we conducted time-series analyses of two groups of states—states with primary enforcement versus states with secondary enforcement

[3]Secondary enforcement means that police officers may only issue citations for failure to use belts if the motorist is first stopped for some other offense. That is, a motorist may not be stopped solely for failure to use seat belts.

FIGURE 3
AGGREGATE RELATIVE RATE OF FRONT-SEAT FATALITIES
AGE 10 AND OVER PER VMT
FOR EIGHT BELT-LAW VERSUS EIGHT COMPARISON STATES[a]



[a]Time series for each state were aligned on the month in which seat belt use became compulsory (month 108).

only. Because North Carolina and Missouri were not actively enforcing their laws during this period, they were excluded from these analyses. Results indicated a significant 9.9% fatality reduction in the primary enforcement states and a significant 6.7% fatality reduction in secondary enforcement states (Figure 2). As expected, states with primary enforcement experienced larger fatality reductions than states limited to secondary enforcement. However, it is worth noting that clear benefits also accrued from secondary enforcement belt laws, provided citations were actually issued to violators.

Finally, in addition to controlling for other plausible explanations for observed fatality declines by including comparison states, we conducted time-series analyses of rates of rear-seat occupant deaths and nonoccupant (motorcycle, pedalcycle, and pedestrian) deaths in the states with seat belt laws. All of the laws examined here are limited to front-seat occupants; as a result, rear-seat occupants along with nonoccupants serve as useful comparison groups. Analyses of aggregate fatality rates for the eight belt-law states revealed no change in fatality rates among either rear-seat occupants or nonoccupants. This result substantially increased our level of confidence in attributing observed declines in front-seat fatalities to the seat belt laws.

DISCUSSION

Our results confirm that laws requiring seat belt use can significantly reduce rates of motor vehicle fatalities. However, one cannot expect the fatality declines to be clearly demonstrable within single jurisdictions a short time after the laws are implemented. The nature of fatality trends over time, and the amount of unpredictable variation in number of deaths from month to month, means that a minimum of a 6% to 10% reduction over a 6- to 12-month period is required before the reduction can be reliably identified. Despite the lack of statistical significance for the estimated effects of seat belt laws in some jurisdictions, it is premature to conclude that laws in those states had *no* effect. As additional data become available, increasing the statistical power of analytic techniques used, some of the state-specific estimates obtained in the current study may become statistically significant. Results from our most powerful analyses, those involving aggregate effects across several states, clearly demonstrate significant fatality reductions. Moreover, use of comparison states and comparison groups not directly affected by the seat belt laws increases our confidence in interpreting the observed declines as caused by the mandatory seat belt use laws.

In terms of the magnitude of the effects of compulsory belt use laws, one can expect a U.S. law that permits primary enforcement and is actually enforced at moderate levels to result in about a 10% reduction in traffic fatalities. A law that permits secondary enforcement only or is enforced at very low levels will have less effect. Although some advocates of compulsory seat belt use have indicated that substantially larger declines in traffic fatalities would result, a 10% decline in a leading cause of death for the entire population represents a resounding public policy success. How many major programs aimed at reducing disease and injury can document an immediate 10% decline in mortality due to that cause of death across an entire population of millions of people? Moreover, effective implementation of a compulsory seat belt use policy requires minimal expenditure of resources when compared to efforts to reduce mortality attributable to other leading causes of death (e.g., cardiovascular disease, cancer).

Despite the clear success of compulsory seat belt laws to date, much more remains to be done. As noted earlier, belt use in the U.S. typically peaks within a month or two of implementation of belt laws, partially decaying after that point. Special enforcement efforts not only can arrest that decline, but further increase belt use rates, at least temporarily (Williams, Preusser, Blumberg, & Lund, in press). Clearly, our results demonstrate that belt laws with primary rather than secondary enforcement provisions are needed. We believe that rigorous enforcement of a primary seat belt law in the U.S. can achieve and maintain belt use rates of approximately 60%, in contrast to less than 20% under the most favorable conditions without compulsory use (i.e., extensive education and public information programs).

Even if asymptotic belt use of 60% were achieved throughout the U.S., declines in traffic fatalities of more than 20% are extremely unlikely. This is because of the differential between belt users and nonusers; that is, those at highest risk of involvement in serious traffic crashes are least likely to use belts (Evans & Wasielewski, 1983). Therefore, other avenues of reducing traffic crash-induced injury and death that do not require action on the part of each individual driver (such as airbags) must be pursued simultaneously with efforts to implement and enforce mandatory belt use laws.

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APPENDIX

Autoregressive Integrated Moving Average/Transfer Function Model

$$(1 - \phi_1 B - \ldots \phi_p B^p)\,(1 - \Phi_1, B - \ldots \Phi_P B^{Ps})\,(1 - B)^d (1 - B^s)^D \,\mathrm{Ln} Y_t = \alpha + (1 - \theta_1 B - \ldots \theta_q B^q)\,(1 - \Theta_1 B^s - \ldots \Theta_Q B^{Qs}) u_t + \psi X_t + \omega I_t,$$

B = the backshift operator such that $B(z_t)$ equals z_{t-1}

ϕ_1 to ϕ_p = regular autoregressive parameters

q = order of the moving average process

θ_1 to θ_q = regular moving average parameters

Φ_1 to Φ_P = seasonal autoregressive parameters

d = order of nonseasonal differencing

D = order of seasonal differencing

s = seasonal span

LnY_t = natural logarithm transformation of the dependent time series

α = a constant

Θ_1 to Θ_Q = seasonal moving average parameters

u_t = random error component

ψ and ω = intervention parameters to be estimated

X_t = step function with the value 1 beginning at month t and 0 otherwise

I_t = step function with the value 1 beginning at month t and 0 otherwise

Final statistical models for selected variables are included here. Standard errors are shown in parentheses below each parameter estimate.

Aggregate Relative Rate of Front-seat Fatalities Age 10 and Over per VMT for Eight Belt-law versus Eight Comparison States

$$(1\text{-}B)\,\mathrm{Ln}Y_t = (1 - \underset{(.075)}{.857}B - \underset{(.079)}{.089}B^3)u_t - (1\text{-}B)\,\underset{(.028)}{.098}X_{79} - (1\text{-}B)\,\underset{(.034)}{.091}I_{108}$$

Adjusted R^2 = .51

Aggregate Rate of Rear-seat Fatalities Age 10 and Over per VMT for Eight Belt-law States

$$(1\text{-}B)\,\mathrm{Ln}Y_t = (1+ \underset{(.091)}{.259}B^{12})\,(1 - \underset{(.062)}{.665}B - \underset{(.072)}{.307}B^4)\,u_t - (1\text{-}B)\,\underset{(.069)}{.366}X_{60} - (1\text{-}B)\,\underset{(.093)}{.009}I_{108}$$

Adjusted R^2 = .67

Aggregate Rate of Nonoccupant Fatalities Age 10 and Over per VMT for Eight Belt-law States

$$(1\text{-}B)(1\text{-}B^{12})(1+ \underset{(.081)}{.551}B^{12})\,(1+ \underset{(.093)}{.347}B)\,\mathrm{Ln}Y_t = u_t - (1\text{-}B)(1\text{-}B^{12})\,\underset{(.085)}{.062}X_{62} + (1\text{-}B)(1\text{-}B^{12})\,\underset{(.103)}{.090}I_{108}$$

Adjusted R^2 = .87

Aggregate Rate of Front-seat Fatalities Age 10 and Over per VMT for Two Primary Enforcement Provision States

$$(1\text{-}B)(1\text{-}B^{12})\,\mathrm{Ln}Y_t = (1 - \underset{(.033)}{.873}B^{12})\,(1 - \underset{(.068)}{.713}B)\,u_t - (1\text{-}B)(1\text{-}B^{12})\,\underset{(.052)}{.104}I_{108}$$

Adjusted R^2 = .81

Aggregate Rate of Front-seat Fatalities Age 10 and Over per VMT for Four Secondary Enforcement Provision States

$$(1\text{-}B^{12})\,\mathrm{Ln}Y_t = (1+ \underset{(.097)}{.228}B)\,u_t - (1\text{-}B^{12})\,\underset{(.034)}{.144}X_{66} - (1\text{-}B^{12})\,\underset{(.039)}{.070}I_{108}$$

Adjusted R^2 = .69