THE IMPACT OF RECENT CHANGES IN CALIFORNIA DRINKING-DRIVING LAWS ON FATAL ACCIDENT LEVELS DURING THE FIRST POSTINTERVENTION YEAR: AN INTERRUPTED TIME SERIES ANALYSIS

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In 1982, a set of strict new countermeasures against drinking-driving went into effect in California. Interrupted time series analysis is used to investigate the effect that these countermeasures have had on fatal accident levels during the first postintervention year. The results do not indicate that a deterrent impact occurred among those accidents. Supplementary analyses of injury accidents during the first postintervention year and of fatality accidents during the first nine months of 1983 add important qualifications to this basic finding.

I. INTRODUCTION

On January 1, 1982, a set of new laws governing drinking-driving offenses in the state of California went into effect. This study evaluates the deterrent impact that those laws have had on the statewide incidence of alcohol-involved, fatal traffic accidents during 1982.

The new legislation changed California's existing drinking-driving laws in several ways. First, the legal definition of driving under the influence of alcohol was changed from a *presumptive* standard in which the determination of impairment must rest on more than test evidence alone to a *per se* standard in which a test result showing a blood alcohol content of at least .10 grams per 100 milliliters (BAC $\geq$ .10 percent) is, in itself, sufficient and nonrebuttable evidence of a violation. Second, a schedule of stiffer penalties for DUI

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offenders was established. The new penalties included mandatory minimum sentencing alternatives for all offenders and mandatory jail sentences for all repeat offenders. Third, restrictions were placed on plea bargaining so that it became harder to reduce DUI charges to lesser offenses. The purpose of these changes was to deter driving after drinking by "getting tough" with drunk drivers and thereby to reduce the accident rate.[1]

A growing body of international research has investigated attempts to deter drunk driving through the establishment of such legal countermeasures (see Jones and Joscelyn, 1978; Cameron, 1979; Ross, 1981; and Reed, 1982 for reviews of this literature). The countermeasure packages that have been studied have involved a variety of legal changes, including (in most instances) the adoption of a *per se* standard of alcohol impairment, as well as such innovations as implied consent laws (Carr *et al.*, 1975; Hurst, 1978), automatic license suspension (Ross, 1973), random roadside breath testing (Ross *et al.*, 1982), and the doubling of already severe prison sentences (Ross, 1975).

The general finding of these studies is that deterrent effects are at best temporary. Where traffic accident levels have declined following the enactment of new countermeasures (Ross, 1973; Carr *et al.*, 1975; Ross *et al.*, 1982), deterrent effects have been disappointingly short-lived, usually lasting for only a few months and never lasting for more than a year. In other cases, the evidence of deterrent effects has been either weak or nonexistent (Noordzij, 1979; Hurst, 1978; Ross, 1975). It remains, then, an open question whether any particular set of countermeasures can be expected to produce a lasting deterrent effect.

This paper asks whether fatal accident levels[2] were reduced following the enactment of the California countermeasure package. This question is investigated through

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1 An important point in the literature is that deterrent effects can only be expected when substantial publicity accompanies the establishment of countermeasure packages (Ross, 1981). Stories about the countermeasures were featured prominently by the press, and the issue was aggressively promoted by Mothers Against Drunk Drivers for several months before and after enactment. Most observers in the state would probably agree that the publicity surrounding the California countermeasures was extensive; however, explicit measures of the extent of publicity or public awareness are not offered here.

2 Although this paper concentrates its analysis on fatal accident levels, some discussion of injury accident levels appears in the "Supplementary Analysis" section below.

the use of interrupted time series analysis (Box and Jenkins, 1976; Box and Tiao, 1965; McCleary and Hay, 1980), by now a well recognized tool for investigating such issues (Ross, 1981). We begin by constructing ARIMA models that describe the trends in California fatal accident data. Then we ask whether fatal accident levels were significantly reduced after January 1, 1982. Finding reductions, we ask whether these persisted over the course of 1982 or instead lasted for only a few months. Finally, we analyze various categories of fatal accidents to see whether the deterrent impact was an alcohol-specific one; that is, whether relatively greater reductions in accident levels characterize those kinds of accidents that are more likely to be alcohol-related.

II. INTERRUPTED TIME SERIES ANALYSIS

Generally speaking, the purpose of time series analysis is to identify a mathematical equation that describes the behavior of a series of time-ordered measurements. This equation captures long-term trends that are at work in the data, so that instances of departure from such trends can be recognized and studied. The equation is called an ARIMA model. ARIMA is an acronym that stands for AutoRegressive, Integrated, Moving Average. These three terms refer to processes whereby past observations in the series and random shocks that affect the series can be combined to form an explanation of the current observation.

One useful feature of this technique is that it does not assume that the observations are independent of each other (i.e., that they are not autocorrelated). Instead, ARIMA modeling searches for patterns of autocorrelation in the data and builds them into the ARIMA model. Since traffic data are likely to contain patterns of autocorrelation, techniques that assume independence, such as ordinary least squared regression, would be inappropriate tools for analysis.

Once an ARIMA model that accurately describes the data has been determined, intervention terms can be inserted into the equation to simulate the effects of interventions, or interruptions, in the series of observations. From the sizes of the parameters in these intervention terms, an intervention can be judged as statistically significant relative to the previous amount of variability in the data.

A description for the general case of an ARIMA model for a series that contains both regular and seasonal variations can be given as:

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$$Y_t = \frac{(1-\Theta_1 B^s - \ldots \Theta_Q B^{sQ})\,(1-\theta_1 B - \ldots \theta q B^q)\, a_t + \alpha}{(1-\Phi_1 B^s - \ldots \Phi_P B^{sP})\,(1-\phi_1 B - \ldots \phi_P B^P)\,(1-B^s)^D (1-B)^d} \quad (1)$$

where:

Y_t	=	the value, at time t, of the variable whose behavior is being modeled
p	=	the order of the autoregressive process
d	=	the degree of non-seasonal differencing
q	=	the order of the moving average process
P	=	the order of the seasonal autoregressive process
D	=	the degree of seasonal differencing
Q	=	the order of the seasonal moving average process
s	=	the seasonal span
Φ_1 to Φ_P	=	the seasonal autoregressive parameters
ϕ_1 to ϕ_p	=	the regular autoregressive parameters
Θ_1 to Θ_Q	=	the seasonal moving average parameters
θ_1 to θ_q	=	the regular moving average parameters
a_t	=	a random (white noise) component
α	=	a constant
B	=	the backshift operator such that $B(Y_t)=Y_{t-1}$

The essence of this equation is that the behavior over time of the variable under consideration is modeled in terms of three separate components. An integrated component captures long-term trends that affect the level of the variable, while autoregressive and moving average components explain current observations in terms of previous observations and random shocks that enter the system and affect the series level. Any particular series may involve a specific combination of these three processes. The model also has seasonal terms in addition to its regular (in this case meaning monthly) terms. This allows for the investigation of phenomena that exhibit regular seasonal fluctuations.

Once an appropriate model has been selected, it can be used as a benchmark for assessing the impact of an intervention (such as the enactment of new drinking-driving laws) on the observed behavior. The simplest model of an intervention effect is generally called an abrupt-permanent intervention. This term can be somewhat misleading since "permanent" in this context means that the effect operates throughout that section of the postintervention period which is submitted to analysis. No claims about the duration of the effect beyond this period and into the indefinite future can legitimately be offered. Thus, it may be more appropriate to speak of an "abrupt-persistent" intervention than of an "abrupt-permanent" one.

An abrupt-persistent intervention can be modeled by adding an additional term ωI_t to Equation (1) and re-estimating the parameters. This would produce:

$$Y_t = \frac{(1-\Theta_1 B^s - \ldots \Theta_Q B^{sQ})(1-\theta_1 B - \ldots \theta_q B^q)a_t + \alpha}{(1-\Theta_1 B^s - \ldots \Phi_P B^{sP})(1-\phi_1 B - \ldots \phi_p B^p)(1-B^s)^D(1-B)^d} + \omega I_t \quad (2)$$

The variable I_t is a dummy variable that takes on a value of 0 before the intervention and a value of 1 after the intervention. In other words, the effect of the intervention is conceived of as a simple step function. The parameter ω indicates the magnitude of the change in the average level of the series that occurs following the intervention. If ω is statistically significant, the analyst can conclude that there has been an observable intervention effect in the data series.

The more complex case of an abrupt but temporary intervention can be modeled by adding the following term to the ARIMA model:

$$\frac{\omega}{1-\delta B} P_t \quad (3)$$

This model is appropriate if the series level abruptly changes at the time of intervention, but the impact immediately begins to decay, so that the series level eventually returns back to its previous level. In Expression (3), P_t is a pulse function that takes on a value of 1 in the first postintervention observation and a value of 0 elsewhere. In this intervention model (and assuming that the observations are recorded monthly), the parameter ω indicates the magnitude of the impact during the first month; during the second month, the impact would be $\omega\delta$; during the third month, it would be $\omega\delta^2$; and so forth. Since:

$$-1 < \delta < 1 \quad (4)$$

the magnitude of the impact dies out over time. The parameter δ indicates the rate at which the initial impact ω wears away.

III. DATA

Data on California traffic accidents were compiled by the Management Information Section of the California Highway Patrol (CHP) from accident reports prepared by county and local police organizations as well as by the CHP's own officers. This file is thought to be a complete record of all serious traffic accidents throughout the state.[3] Monthly data covering the period between May of 1977 and December of 1982 were taken from the CHP's records and included in this analysis.[4] This means that 56 months of preintervention data and 12 months of postintervention data were analyzed.

The second hypothesis under investigation here requires that alcohol-involved accidents be distinguished from accidents that are not alcohol-involved. Official determinations of alcohol involvement were made in the data. According to the CHP's classification system, an accident was defined as alcohol-involved if it was: "Any motor vehicle traffic accident where a driver, pedestrian or bicyclist had been drinking" (California Highway Patrol, 1982: 79). A party to the accident was counted as "had been drinking" if the officer on the scene checked any of the following categories on the accident report form: "Had Been Drinking—Under Influence," "Had Been Drinking—Not Under Influence," "Had Been Drinking—Impairment Unknown."

Unfortunately, the quality of the alcohol involvement data is suspect. Waller (1971) concluded that California police officers substantially underreported the extent of alcohol involvement among fatally injured traffic victims. This was because the determination of alcohol involvement often rested on the judgments of the officers at the scene instead of on objective chemical tests. In light of this, we will pursue a two-pronged strategy in order to distinguish between alcohol-involved and non-alcohol-involved accidents.

The first prong is to assume that, despite its problems, the CHP's classification system is an improvement over a random guess. The proportion of alcohol-involved accidents should be greater in the set of cases where alcohol involvement was noted than in the set where it was not noted. If an alcohol-specific

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[3] The CHP defines a fatal accident as "A motor vehicle traffic accident resulting in the death of one or more persons within thirty days of the accident" (California Highway Patrol, 1982: 80). While many minor fender-benders go unreported, and hence fail to find their way into the CHP data, one can be reasonably confident that fatal traffic accidents are rarely undetected by police agencies.

[4] The choice of May 1977 as the starting point for the analysis was based on an unexplained peculiarity in the data. Between the end of 1976 and early 1977, the reported levels of all categories of traffic accidents rose sharply. There is no indication that changes in CHP reporting procedures occurred at this time, nor are other explanations involving driving habits, the legal environment, or the economy readily at hand to account for this phenomenon. A discontinuity of this sort can interfere with the identification of an adequate ARIMA model, unless it is conceptually understood and can be built into the model. Therefore, the data for the period prior to this unexplained increase were excluded from the analysis.

deterrent effect exists, a greater reduction should be found among the accidents that were classified as alcohol-involved than among those that were not so classified.

The second prong of this strategy makes use of existing knowledge about the kinds of accidents that are more likely to involve alcohol. For example, it is known that alcohol involvement is more prevalent among nighttime fatal accidents than among daytime ones (Filkins *et al.*, 1970; Waller *et al.*, 1969). Thus, if an alcohol-specific deterrent effect exists, one would expect to find a greater relative reduction in the level of nighttime fatal accidents than in the level of daytime ones. Also, alcohol involvement is known to be more prevalent among weekend fatal accidents than among weekday fatal accidents (Filkins *et al.*, 1970; Waller *et al.*, 1969). Therefore, an alcohol-specific deterrent effect should produce greater reductions among weekend accidents than among weekday ones.

In the CHP data, an accident was classified as a nighttime accident if it occurred between the hours of 6:00 p.m. and 6:00 a.m. Otherwise, it was classified as a daytime accident. An accident was classified as a weekend accident if it occurred between 6:00 p.m. Friday and 6:00 a.m. Monday. Otherwise, it was classified as a weekday accident.

The principal hypothesis under investigation here is that California's 1982 drinking-driving laws exerted an alcohol-specific deterrent effect on the levels of fatal traffic accidents throughout the state. If this hypothesis is true, one would expect to find that greater reductions occurred among those accidents that were classified as alcohol-involved than among those that were not classified as alcohol-involved. One would also expect reductions in fatalities to be greater for nighttime and weekend accidents than for their daytime and weekday counterparts.

IV. ANALYSIS OF THE FATAL ACCIDENT SERIES

One can begin the analysis of the data for total fatal accidents by examining Figure 1, which is a plot of monthly accident totals over time. Note the seasonal fluctuation. Accident levels tend to peak in the summer months of June through August of each year and reach annual lows every January and February. Also, the average level of accidents (as indicated by the 12-month moving average) tends to rise until 1980 and to decline thereafter. During the first few months after the intervention, there is a substantial drop in fatality

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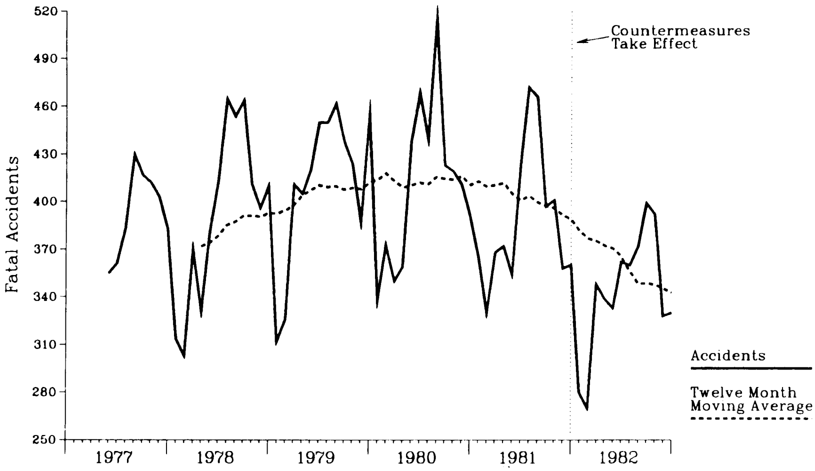
levels, a drop that goes well beyond both the typical seasonal reductions and the long-term decline in average levels. In mid and later 1982, accident levels appear to rise from their depressed levels, but it is difficult to determine from the plot alone whether they reached to levels that would have been comparable to those of preceding years. A more precise discussion of these issues can be based on the results of an interrupted time series analysis.

Figure 1. Total Fatal Accidents per Month



The ARIMA model identification technique produced a three parameter, autoregressive model as the best representation of the total fatal accident series.[5] In other words, the general equation that appears as Equation (1) above was reduced to a simpler version, which nevertheless provides an acceptable representation of the data. This model can be represented mathematically as:

$$Y_t = \frac{\alpha + a_t}{(1 - \phi_1 B - \phi_4 B^4 - \phi_{12} B^{12})} \tag{5}$$

[5] The data for this series, and for the other series to be discussed below, were analyzed according to the time series analysis procedure described by McCleary and Hay (1980) and with the aid of the BMDP2T computer program (Liu, 1981). Three departures from the analysis strategy outlined by McCleary and Hay were made: 1) The ARIMA models were identified on the basis of the preintervention series only (May 1977 through December 1981). 2) The Ljung-Box Q statistic (Ljung and Box, 1978) that is computed by the BMDP2T program was used for model diagnosis rather than the less conservative Q statistic described by McCleary and Hay. 3) The ARIMA models that were selected were more complex than those suggested by McCleary and Hay; however, the models that were selected did meet McCleary and Hay's general criteria of parsimony and statistical adequacy.

The numerical estimates of the parameter values, as supplied by the BMDP2T program that was used, appear in the top section of Table 1. These indicate that the first and twelfth order autoregressive parameters had positive values while the fourth order parameter had a negative value. This model was judged to be an adequate representation of the data on the basis of the modeling criteria developed by Box and Jenkins (1976), as described by McCleary and Hay (1980).[6]

Table 1. Parameter Values and Model Diagnosis Statistics for the Preintervention Series

Series	Parameter Value	Standard Error	t Statistic	Ljung-Box Q	R^2
(1) Total fatal accidents	ϕ_1 = .355	.100	3.57	Q_{26} = 21	.651
	ϕ_4 = −.312	.043	−7.19		
	ϕ_{12} = .500	.099	5.07		
	Θ_0 = 391.3	7.616	51.38		
(2) Alcohol-involved fatal accidents	ϕ_1 = .394	.114	3.44	Q_{26} = 12	.610
	ϕ_4 = −.248	.103	−2.42		
	ϕ_{12} = .358	.120	2.97		
	Θ_0 = 195.7	4.979	39.31		
(3) Non-alcohol-involved fatal accidents	ϕ_1 = .498	.115	4.34	Q_{27} = 31	.480
	ϕ_{12} = .346	.113	3.06		
	Θ_0 = 185.7	13.50	13.75		
(4) Nighttime fatal accidents	ϕ_1 = .258	.092	2.80	Q_{26} = 25	.687
	ϕ_4 = −.323	.035	−9.18		
	ϕ_{12} = .579	.089	6.47		
	Θ_0 = 239.5	4.918	48.71		
(5) Daytime fatal accidents	ϕ_1 = .403	.126	3.20	Q_{27} = 27	.361
	ϕ_{12} = .230	.125	1.84*		
	Θ_0 = 153.2	5.440	28.16		
(6) Weekend fatal accidents	ϕ_4 = −.365	.108	−3.39	Q_{27} = 31	.529
	ϕ_{12} = .554	.118	4.70		
	Θ_0 = 185.7	3.876	47.90		
(7) Weekday fatal accidents	ϕ_4 = −.263	.121	−2.17	Q_{26} = 33	.577
	ϕ_{12} = .491	.125	3.92		
	Θ_0 = 209.2	3.613	57.89		

*Not significant at the .05 level. The twelfth order parameter for the daytime accident series was not significant in the modeling of the preintervention data. However, this parameter re-emerged as a significant one when the full series models, with intervention effects, were estimated (see Table 2). Therefore, this twelfth order term is retained in Table 1 for the purpose of consistency.

[6] These modeling criteria accomplish a number of purposes. First, they ensure that all of the terms included in the model are statistically significant. They also ensure that the model is complex enough to adequately represent all of the relationships that exist in the data. Finally, they require that the model explain a substantial proportion of the variance that exists in the data. For this model, the criteria produced the following results: 1) Each model parameter was significant at the .05 level. The t statistics for each parameter in Equation (5) are displayed in Table 1. 2) An examination of the autocorrelation and partial autocorrelation functions for the model residuals did not reveal significant spikes at any lags. 3) At 26 degrees of freedom, the Ljung-Box Q statistic calculated for the series residuals equaled 21. Since this

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Two of the three autoregressive parameters in this model lend themselves easily to meaningful conceptual interpretations. The first order parameter indicates a positive month-to-month correlation. This is to be expected whenever such factors as weather or price fluctuations (for either gas or drink) have the effect of raising or lowering accident levels over a period of adjacent months. The positive parameter at the twelfth order indicates the seasonal pattern that was noted above. Because of this seasonality, observations for each month tended to be positively correlated with observations for the same month of the preceding year.

The negative correlation at the fourth lag was unexpected, but it appeared repeatedly in the series that were analyzed. The reason for this fourth order effect is unclear, but one plausible explanation involves the distribution of weekends among the months. Fatal traffic accidents tend to be concentrated on the weekends. Also, our calendar is arranged so that the numbers of weekends are unevenly distributed among the months. In particular, if there are relatively many weekend days in any one month, there seems to be a relatively low number of such days in the month four months hence (and vice versa). Given this, one would expect that any phenomenon that is concentrated on the weekends would be negatively autocorrelated at a fourth, monthly lag. This explanation is corroborated by the fact that the significant fourth order effect disappears when daytime fatal accidents and non-alcohol-involved fatal accidents are modeled.

After this analysis of the preintervention data had been performed, the abrupt-persistent intervention component was added to the model, and the parameters were then re-estimated using the full series of both pre- and postintervention observations. This produced the set of parameter estimates that appear in the top section of Table 2.

These estimates indicate that there were 51.7 fewer fatal accidents per month in California in 1982 than there would

value was not significant at the .05 level, the autocorrelations among the model residuals were not determined to be significantly different from those produced by white noise. 4) The R^2 value for the model was .651, indicating the amount of variance explained by the model. The same procedure was used for diagnosing the other models that will be discussed in the remainder of this paper. While we will not discuss the adequacy of each additional model, Tables 1 and 2 present much of the data needed for assessing the adequacy of each.

Table 2. Parameter Values and Model Diagnosis Statistics for Full Series, Including Intervention Effects

Series	Parameter Value	Standard Error	t Statistic	Ljung-Box Q	R^2
(1) Total fatal accidents	$\phi_1 = .378$	.088	4.32	$Q_{25} = 22$	.711
	$\phi_4 = -.326$	.039	−8.28		
	$\phi_{12} = .471$	.087	5.44		
	$\Theta_0 = 392.3$	7.109	55.18		
	$\omega = -51.66$	10.994	−4.70		
(2) Alcohol-involved fatal accidents	$\phi_1 = .402$	.102	3.94	$Q_{25} = 17$	.630
	$\phi_4 = -.250$	.091	−2.74		
	$\phi_{12} = .347$	.100	3.47		
	$\Theta_0 = 196.0$	4.651	42.13		
	$\omega = -23.55$	7.890	−2.99		
(3) Non-alcohol-involved fatal accidents	$\phi_1 = .478$	.106	4.52	$Q_{25} = 29$	.570
	$\phi_{12} = .343$	.106	3.24		
	$\Theta_0 = 188.0$	11.986	15.69		
	$\omega = -21.65$	13.585	−1.59		
(4) Nighttime fatal accidents	$\phi_1 = .303$	.088	3.46	$Q_{25} = 32$	.712
	$\phi_4 = -.343$	.035	−9.69		
	$\phi_{12} = .525$	.086	6.13		
	$\Theta_0 = 240.2$	4.680	51.32		
	$\omega = -31.32$	7.162	−4.37		
(5) Daytime fatal accidents	$\phi_1 = .353$	.1152	3.07	$Q_{26} = 26$	.459
	$\phi_{12} = .253$	.1155	2.19		
	$\Theta_0 = 153.4$	4.974	30.84		
	$\omega = -20.14$	7.728	−2.61		
(6) Weekend fatal accidents	$\phi_4 = -.292$	.101	−2.88	$Q_{25} = 34$	.550
	$\phi_{12} = .559$	.104	5.38		
	$\Theta_0 = 184.8$	4.120	44.86		
	$\omega = -23.96$	6.880	−3.48		
(7) Weekday fatal accidents	$\phi_4 = -.227$	.114	−1.98	$Q_{26} = 34$	.577
	$\phi_{12} = .460$	.112	4.11		
	$\Theta_0 = 209.4$	3.672	57.03		
	$\omega = -26.48$	5.897	−4.49		

have been had preintervention trends continued unchanged. This represents a 12.9 percent reduction in fatal accident levels relative to the preintervention mean.[7] As the t value of −4.70

[7] Because 1981 was a recession year, some have speculated that accident reductions in that year were simply a result of the fact that California motorists did less driving. This, however, was not the case. According to Highway Patrol estimates, the number of vehicle miles driven in the state actually increased from 160.8 billion miles in 1981 to 170.0 billion miles in 1982 (California Highway Patrol, 1982).

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indicates, this is a statistically significant reduction at the .001 level.

These data indicate that there was a reduction in fatal accident levels following the institution of the new California countermeasures. This reduction is important given both the seriousness and the intractability of the drunk driving problem. The next step is to determine whether the change persisted at more or less the same level throughout the period under study. We do this by evaluating models that contain the more complex intervention term described by Expression (3). Table 3 presents the results.

Table 3. Intervention Parameters for an Abrupt-Temporary Intervention Model of the Total Fatal Accident Series

	Parameter Value	Standard Error	t-Ratio (against zero)
ω	−81.8	24.7	−3.31
δ	.907	.060	15.04

The magnitude of the δ parameter is the key to distinguishing between persistent and temporary impacts. When δ is close to unity, a persistent model of impact is indicated; otherwise, a temporary model supplies the more accurate description of the data. In this case, the value of δ was high, and unity fell within a 95 percent confidence interval constructed around it. For this reason, the reduction in fatal accident levels was judged to be a persistent rather than a temporary phenomenon during the first postintervention year.

This durability is impressive given the international experience. In both the United Kingdom and France, effects eroded during the first year (Ross, 1973; Ross *et al.*, 1982). In Canada and the Netherlands, it is difficult to determine erosion rates because data from those countries were presented on an annual rather than on a monthly basis (Carr *et al.*, 1975; Noordzij, 1979; Van Ooijen, 1979). In neither case, however, did the effects seem to last longer than one year, thus making the California effects all the more striking.

V. COMPARISONS OF DETERRENT EFFECTS FOUND AMONG ALCOHOL-RELATED AND NON-ALCOHOL-RELATED SERIES

Was the deterrent effect alcohol-specific? In order to answer this question, one must compare the deterrent impacts that occurred in the alcohol-related categories of accidents against the impacts that occurred in the non-alcohol-related categories of accidents. Plots of the data series used for making these comparisons appear in Figures 2 through 7.

Figure 2. Alcohol-Involved Fatal Accidents per Month

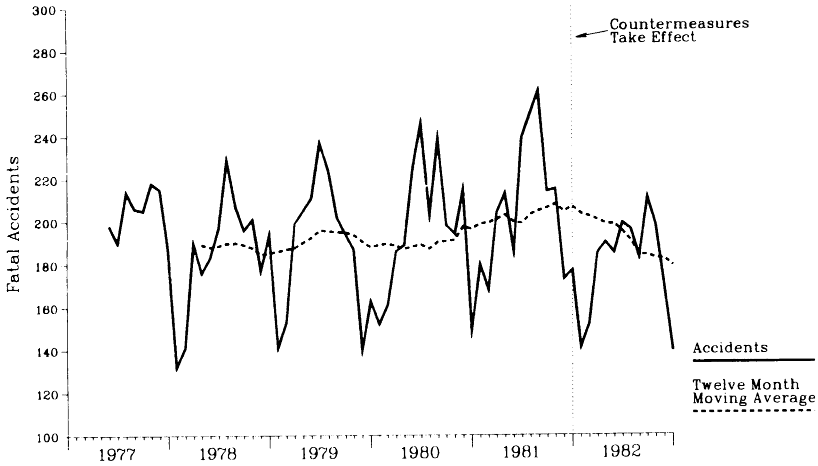

Figure 3. Non-Alcohol-Involved Fatal Accidents per Month

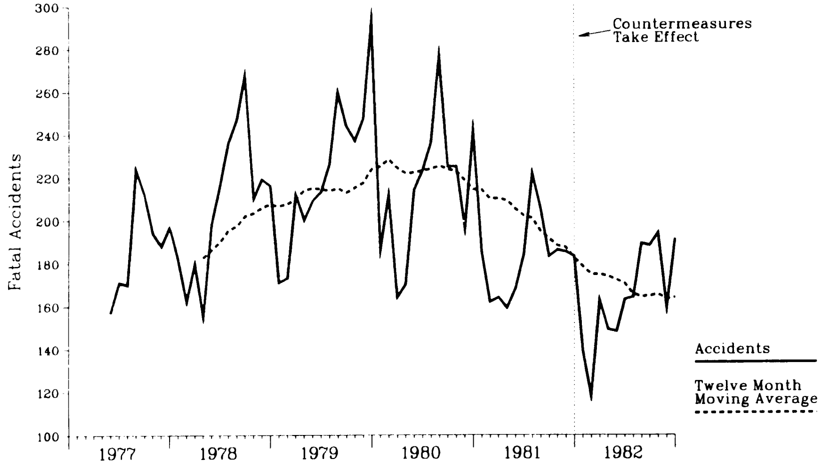

Figure 4. Nighttime Fatal Accidents per Month

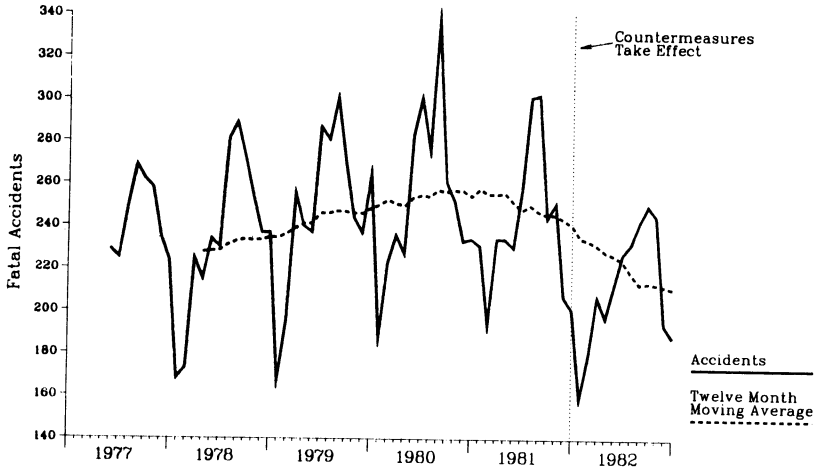

Figure 5. Daytime Fatal Accidents per Month

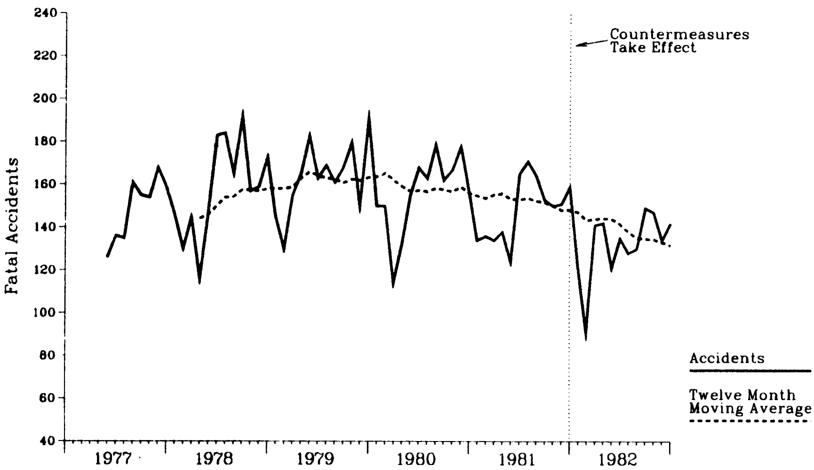

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Figure 6. Weekend Fatal Accidents per Month

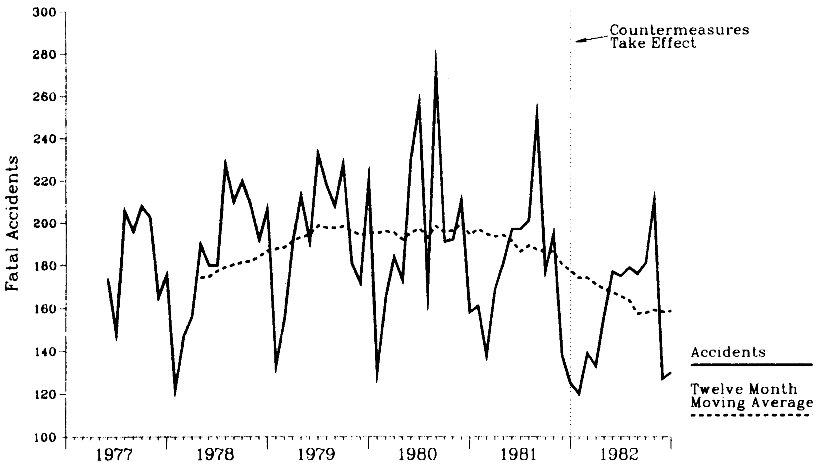
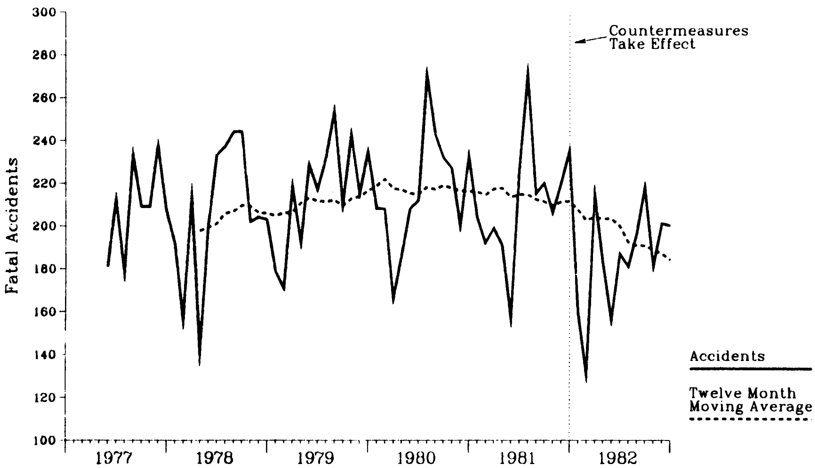


Figure 7. Weekday Fatal Accidents per Month



Following the same procedure described above, ARIMA models for the six remaining fatal accident series were identified. All six solutions appeared to be related to the model that was identified for total fatal accidents because each could be constructed from some combination of autoregressive parameters of the first, fourth, and twelfth order. Departures from a common pattern occurred when the generalized, three parameter model—Equation (5)—produced estimates of parameter values that were not statistically significant. In these cases, the insignificant parameters were dropped from the model and the remaining parameters were then re-estimated. This procedure produced two parameter models for the series for non-alcohol-involved fatal accidents, daytime fatal accidents, weekend fatal accidents, and weekday fatal accidents. Table 1 indicates the number and order of the parameters used for modeling each series.

In most cases, a model that combined the autoregressive terms additively produced the most adequate representation of the data. However, for the weekday fatal accident series, an adequate fit could only be obtained by combining the parameters multiplicatively. Thus, the model for the weekday fatal accident series departs from the others and is expressed as:

$$Y_t = \frac{\alpha + a_t}{(1-\phi_4 B^4)\,(1-\phi_{12} B^{12})} \qquad (6)$$

Comparisons between the deterrent impacts found in these series were created by examining the impact of abrupt-permanent interventions. The intervention component was added to each series and the parameters were then re-estimated using the full series of both pre- and postintervention observations. The results appear in Table 2. Table 4 presents, for each series, figures for the intervention parameter (ω), its significance, the preintervention mean, and the intervention parameter divided by the preintervention mean. Comparisons between the alcohol-related series and the non-alcohol-related series are made by comparing this last figure for corresponding series.

In the comparison between alcohol-involved accidents and non-alcohol-involved accidents, the difference in the percentage reduction is in the anticipated direction, but it is too small (12.1 percent vs. 10.6 percent) to support the conclusion that an alcohol-specific deterrence occurred. The comparisons between nighttime and daytime and between weekend and weekday accidents also show only small differences. In the former case, the direction of the effect is the reverse of what was expected.

In none of these comparisons does as much as two percentage points separate the reduction in accident levels between the alcohol-concentrated and the non-alcohol-concentrated categories of accidents. This means that the

Table 4. Intervention Parameters and Related Statistics

Series	ω	t Statistic	Preintervention Mean	ω/Mean
(1) Total fatal traffic accidents	−51.66	4.70	399.8	−.1292
(2) Alcohol-involved fatal traffic accidents	−23.55	2.99	195.5	−.1205
(3) Non-alcohol-involved fatal traffic accidents	−21.65	1.59	204.3	−.1060
(4) Nighttime fatal traffic accidents	−31.32	4.37	244.4	−.1282
(5) Daytime fatal traffic accidents	−20.14	2.61	155.4	−.1296
(6) Weekend fatal traffic accidents	−23.96	3.48	187.9	−.1275
(7) Weekday fatal traffic accidents	−26.48	4.49	211.9	−.1250

deterrent impact was evenly evident whether one looks at alcohol-related or non-alcohol-related categories of accidents. Hence, the data presented here do not indicate an alcohol-specific effect.[8]

VI. SUPPLEMENTARY ANALYSIS

This analysis has concentrated on first-year effects among fatal accident levels. This focus was chosen because the countermeasure literature indicates both that deterrent effects are expected to be stronger among fatal accidents than among injury ones (Borkenstein *et al.*, 1964; Hurst, 1974; Farris *et al.*, 1975; Perrine, 1975) and that deterrent effects are expected to occur in the first few months following an intervention if at all (Ross, 1981; Cameron, 1979). However, additional data both on serious personal injury accidents and on fatal accidents during the second postintervention year are now available. These data are not fit to ARIMA models, but, as we shall see, important findings emerge from simple before and after comparisons based on them.

[8] A competing interpretation can be offered. It could be the case that reduction effects are not uniform between accident categories. For example, perhaps daytime drunk drivers were deterred more heavily than nighttime ones. Since the latter make up a larger percentage of all nighttime accidents than do the former of all daytime accidents, a non-uniform effect of this type could produce an equal lowering of daytime and nighttime accident rates. It must be recognized, however, that this explanation has been offered *post hoc* and that it ultimately rests on notions of non-uniform effects that are both unobserved and unprecedented. Because of this, the interpretation given in the text is to be preferred.

Table 5. Percentage Decrease in Serious Personal Injury Accidents, 1981 to 1982

Accident Category	Percentage Decrease 1981 to 1982
(1) Total injury traffic accidents	7.9
(2) Alcohol-involved injury traffic accidents	11.2
(3) Non-alcohol-involved injury traffic accidents	6.5
(4) Nighttime injury traffic accidents	11.8
(5) Daytime injury traffic accidents	4.7
(6) Weekend injury traffic accidents	9.5
(7) Weekday injury traffic accidents	6.9
(8) Single-vehicle injury traffic accidents	9.1
(9) Multiple-vehicle injury traffic accidents	7.4

Table 5 shows the variations in the percentage decrease of serious personal injury accidents between the pre- and postintervention years by alcohol relatedness.[9] These results give a rather different picture from that suggested by our analysis of the fatality data. Accident levels in the alcohol-related categories were consistently reduced by greater amounts than accident levels in the corresponding non-alcohol-related categories. This evidence supports the claim that for serious personal injury accidents, the intended deterrent effects of the countermeasures were achieved.

[9] Serious personal injury accidents are defined here as those that were classified by the officer on the scene as involving either "severe wound" or "other visible injury." Accidents involving only "complaint of pain" were excluded from this analysis. Both here and in Table 6 there is separate consideration of single-vehicle and multiple-vehicle accidents. Single-vehicle accidents, especially fatalities, are thought to be more likely than multiple-vehicle accidents to involve alcohol (see Cameron, 1977; Jones and Joscelyn, 1978).

The disparity between the injury data and the fatality data[10] means that the fatality-based findings above must be importantly qualified. Although reductions in fatal accident levels are not attributable to the California laws, reductions in injury accident levels are, and those laws can be judged a success on this basis.

Table 6. Changes in Fatal Accident Levels Between 1981 and 1982 and Between the First Nine Months of 1982 and the First Nine Months of 1983

Accident Category	Percentage Change 1981 to 1982 Full twelve months	Percentage Change Jan. - Sept. 1982 to Jan. - Sept. 1983
(1) Total fatal traffic accidents	−11.8	−0.33
(2) Alcohol-involved fatal traffic accidents	−13.4	−4.20
(3) Non-alcohol-involved fatal traffic accidents	−10.1	4.15
(4) Nighttime fatal traffic accidents	−12.3	−4.72
(5) Daytime fatal traffic accidents	−11.1	6.91
(6) Weekend fatal traffic accidents	−10.6	−1.32
(7) Weekday fatal traffic accidents	−12.9	0.55
(8) Single-vehicle fatal traffic accidents	−11.4	−5.10
(9) Multiple-vehicle fatal traffic accidents	−12.1	2.65

Table 6 reports data that have recently become available for the first nine months of the second postintervention year (1983). The first column of percentage change figures in the table shows again the mixed pattern of 1982 reductions that was discussed above. Reductions are not consistently greater in the alcohol-related categories. In the first nine months of the second postintervention year, however, markedly different results emerge. Fatal accident levels continue to decline for all alcohol-related categories, but they rise for all non-alcohol-related categories. This pattern is found in every comparison that appears in the table. These results clearly indicate that the relative incidence of drunk driving began to decline during the second postintervention year.

Again, the results of the supplementary analysis mean that an important qualification must accompany the original

[10] This disparity also appears in the data presented by Bloch (1983).

findings. The conclusions based on the time series analysis apply to the first postintervention year only. In the second year, the across-the-board pattern of reductions was replaced by an apparently different effect.

Because of the time lag between the passage of the laws and the onset of these second year effects, the attribution of causality becomes uncertain. One could claim that the countermeasures had a delayed effect that didn't take hold until a second year. However, this delayed effect explanation assumes a mode of deterrence that is without precedent in the countermeasure literature. Alternatively, one could argue that this later change was caused by something other than the deterrent effect of the countermeasures. Any of a number of forces (such as a general change in public attitudes regarding drunk driving) could have produced the new pattern. The choice between these competing explanations is by no means clear, and future debate on this topic is to be expected.[11]

VII. DISCUSSION

California's 1982 package of drinking-driving countermeasures signaled a vigorous new attack on the alcohol-traffic problem. As such, it has drawn national attention as a possible model for other states. Changes that have occurred in California accident levels since the enactment of these laws are, therefore, of great interest to those who seek to reduce the drunk driving problem. At the same time, they are of interest to students of deterrence, who repeatedly find that new efforts to control drunk driving have had only weak or temporary effects.

The principal aim of this study was to search for evidence of deterrent effects in the data on fatal accident levels during the first postintervention year. This focus was chosen because the countermeasure literature has consistently shown that the deterrent effects of drinking-driving countermeasures exist only in the short term and that they are more visible among fatal accidents than among personal injury ones.

In 1982, there was a statistically significant reduction in fatal accident levels. Some 51.7 fewer fatal accidents per month occurred in that year than would have been expected on the basis of previous accident levels. These reductions persisted throughout the year instead of declining as the year went on.

[11] Peck (1983) indicates that additional research into effects during the first two postintervention years is currently under way.

The coincident timing of the establishment of new countermeasures and the onset of reduction impacts makes it tempting to attribute the reduced accident levels to the workings of the laws. Such a conclusion would imply that the countermeasures successfully deterred drunk drivers by intensifying the legal consequences of driving under the influence. If this were true, however, accidents involving alcohol should have declined more sharply than other types of accidents.

The data presented here show that this did not happen during the first postintervention year. Accident levels for alcohol-related accidents and for non-alcohol-related accidents declined by about the same amount. Rather than being alcohol-specific, the reductions in fatal accident levels were across-the-board. This makes it difficult to attribute the 1982 accident reductions to the influence of the countermeasure package. Instead, it seems more likely that some unknown factor caused reductions in all types of fatal accidents.

Supplementary analyses demonstrate the limitations of these conclusions. Deterrent effects did occur among injury accidents, and these seem to be attributable to the deterrent impact of the law. Also, the relative incidence of alcohol-related fatal accidents did decline during the second postintervention year, although it is less clear, in the absence of a first-year effect, that this decline should be attributed to the deterrent impact of the law. In short, results that indicated deterrence were not found where they were most expected, but suggestions of deterrence did emerge elsewhere. The former indicates that the laws did not unambiguously achieve their most critical purpose; the latter indicates that they were, nevertheless, effective in other ways.

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