Each ARIMA model was composed of the following additive components:

- a control component—an independent variable consisting of one of the daytime series (i.e., 6 to 2 p.m., 10 to 11 a.m., or multiple vehicle daytime accidents) or the non-HBD series scaled by a coefficient designated β;

- two intervention components (legislation implementation dates)—binary variables (with values of either 0 or 1 depending on whether a given observation was in the prelaw or postlaw period), scaled by the proportion of the initial change in the level of the series following intervention (designated ω), divided by the rate at which the series' asymptotic level was reached (designated δ) after intervention. As such, these parameters may be used to estimate the rate with which the prelaw and postlaw portions of the series converge;

- a noise component—a multiplicative combination of terms characterizing the interdependence of observations in the series. The terms in this component comprised so-called autoregressive (ϕ) and/or moving average (θ) factors, and could contain a trend (or constant) representing the average difference between adjacent series observations. Together these factors described the seasonal and regular patterns in each alcohol (HBD, nighttime, SVNM, or 2 to 2:59 a.m.) series unaccounted for by the control or intervention components;

- an error component—an independent variable representing random error in the series unaccounted for by the other components of the model;

- a covariate component—when applicable, additional explanatory variables (designated β) consisting of one or more of the four independent variables representing latent background trends left unaccounted for by the control coefficient, lagged with the dependent series in such a manner as to maximize their cross-correlational relationship.

To identify each covariate's optimal structure relevant to the dependent variables, the covariates were individually filtered through tentative models applied to each prewhitened dependent series. Prewhitening refers to the process of controlling trend in the series prior to allowing the series to enter the analysis. This process produced a cross-correlation function which identifies the between-series correlation using an approximation of the familiar Pearson product-moment correlation coefficient between two time series separated by $\pm k$ observations. When k equals zero, that is when there is no time lag between the two series, the formulae are identical. By convention (see McCleary & Hay, 1982), lag relationships were said to be significant if their resulting cross-correlation estimates were greater in absolute value than two times their standard errors. All of the covariates were considered for each of these analyses. Then to establish the optimal combination of covariates for reducing error in the dependent series, the covariates with significant cross-correlations were entered in tentative bivariate time series models with the dependent series using the lags identified by the cross-correlation functions, that is, in either direct month-to-month correspondence, or after shifting the covariate series back no more than one year. This latter constraint was imposed because a causal connection between the covariate and dependent series becomes less supportable over longer periods of time. When more than one significant lag relationship was identified by the cross-correlations within the one-year time constraint, the bivariate analyses were performed shifting the covariate back to each significant lag indicated. These bivariate analyses, used a two-tailed test ($p \leq .10$) as the criteria for considering a covariate statistically significant. Covariates were then entered in the final full model only if they were shown in these preliminary bivariate models to collectively improve the predictive value of the final model. If a covariate did not significantly reduce error in the bivariate model, it was not entered in the final full model. At this point, in addition to the caution sited above, a second general caution is now offered regarding these analyses. While there was no compelling reason to restrict the time lags to a particular relationship, there is a possibility that by allowing the data to be used in establishing the appropriate relationship between the covariates and the dependent series, we are capitalizing on chance variation in the data. This possible limitation is somewhat analogous to that found in conducting statistical regression analysis. As such, the reader should be cautious in ascribing too much meaning to the particular lag relationships identified in these cross-correlations. It should be noted however, that in separate analyses we verified that the results of the analyses were fairly robust to modifications made to the time lags between the covariate and the dependent series. That is, we found that shifting the lag between covariate and dependent series backward or forward one or two months did not substantially change the outcome of the resulting time series analyses. Interested readers may refer to McCleary and Hay (1982) and McLeod (1983) for more information about the theory and mathematics of using covariates as applied here, and to Hagge and Romanowicz (1995) for a further example as applied in traffic safety research.

The ultimate focus of the present evaluation is on the intervention-component parameters of this final model structure. Their estimated direction and size reflect the effect, if any, which may be attributed to the DUI legislation. Resulting t-values associated with each estimated intervention component were assessed using a one-tailed test of the probability ($p \leq .10$) that the resulting values were not due to error. A one-tailed test was considered the most appropriate in this application since it was thought that a significant accident increase could not reasonably be attributed to the intervention of the two laws considered. This is supported by the vast majority of past research which has found substantial accident reductions associated with these types of laws. The inclusion of the control series, and additional covariate series when appropriate, helps to prevent attributing significance to the DUI legislation which should, more accurately, be attributed to some independent but coincident event.

In this evaluation, both pre- and postintervention observations were used in the structural model building process underlying each analysis, since the intervention impact was not expected to overwhelm the other features of the series (McCleary & Hay, 1982). In this approach, the intervention and noise components are assumed independent. A modeled intervention component is considered adequate only when the cross-correlation of the model residuals reflects this independence, as represented by a random or "white noise" process. In each analysis, the final model selected was one for which the residuals were best represented by this white noise process, the residual mean squared error was low, the Ljung-Box Q statistic[2] was not significant, and a simple and reasonable structure was preserved.

Three common forms of intervention effects were initially considered as equally viable possibilities for each series in this study. They are presented below in the order in which they were considered.

1. Abrupt/temporary effects - expected if potential offenders became immediately aware of the implications of the new legislation (perhaps as a result of media coverage) but their sense of threat began to diminish, again putting themselves at risk by eventually returning to their preintervention rate of driving while impaired. This return to preintervention drunk driving levels (commonly found among studies of public response to DUI legislation) may result from the driver's subsequent experience that the likelihood of his or her arrest for drunk driving had not increased.

2. Gradual/permanent effects - expected if the onset of awareness regarding the enhanced legal threat was gradual after the legislation became operative (perhaps conveyed by "word of mouth" or exposure to protracted media coverage), and the deterrent effects persisted over time.

3. Abrupt/permanent effects - expected if potential offenders were immediately deterred by the implications of the new DUI legislation and the deterrent effects persisted over time.

Given that there was no a priori basis on which to select one of these intervention types over the others, a three-stage evaluation procedure recommended by McCleary and Hay (1982) was adopted. In this procedure abrupt-temporary effects are first tested and ruled out prior to attempting to fit a permanent effect, beginning with a gradual-permanent effect and proceeding to an abrupt-permanent effect if the former fails to be statistically significant ($p \leq .10$, using a one-tailed test) or violates the constraints described by McDowall et. al. (1980) as the "bounds of system stability." This constraint requires that the δ parameter estimated by the intervention model must be greater than zero but less than unity to be meaningfully interpretable. A large negative δ parameter would represent an oscillating unstable pattern which changes in magnitude from one month to the next. Given that a δ parameter equal to unity represents a pattern of no recovery or convergence, a δ parameter greater than unity is not meaningfully interpretable. Consequently, neither a large negative δ parameter nor one greater than unity could be reasonably attributed to the intervention of the laws assessed here. In the event that none of the three hypothesized models produces a

[2] The Ljung-Box Q statistic represents the degree to which the residuals from the tentative model are distributed as white noise. A white noise process is one which is randomly distributed and hence, the series observations are uncorrelated with one another.

significant intervention effect estimate, or fails to achieve system stability, the null hypothesis (of no intervention effect) cannot be rejected.

Arrest data

The data series of DUI arrests and total arrests were provided by the California Department of Justice. Both series consisted of both misdemeanor and felony arrests for both juvenile and adult populations. The analysis of these data was similar to that of accidents except that none of the additional covariates were assessed. Consistent with the accident analyses, all three forms of intervention were considered in accordance with the blind modeling approach described above.

Media campaign

As previously noted, a special media campaign to promote the APS law began in 1991, with its greatest effort focused between June and December 1991. The four counties of Los Angeles, Sacramento, San Diego, and San Francisco were particularly targeted by this campaign for media saturation. Consequently, separate interrupted time series analyses were performed on each of the three HBD series (as described above) for the combined accidents occurring in the four targeted counties to investigate the possibility that a renewed general deterrent effect would be revealed immediately following this focused campaign and ensuing media coverage of the law. Thus, in addition to the intervention parameters associated with the timing of the two laws, a third intervention point was introduced into each of the HBD accident series at June 1991, the point representing the reported height of the media campaign. HBD accidents in California's other 54 counties were combined by severity level to form control series which were used in these analyses.

In summary, the primary objective of this study is to evaluate the presence and nature of any general deterrent effect of the two 1990 DUI laws on California's alcohol-related accidents and arrests, and secondarily, to assess any measurable impact of the subsequent media campaign designed to promote the APS law.

RESULTS

Process Measures

Summary reports on Administrative Per Se Process Measures (presented in the Appendix) document the APS license suspension/revocation totals to date. These reports show that in the first five years of the law, over one million APS actions were taken (excluding actions later set aside). Table 1 presents the total actions taken by year and offender status.

Table 1

Administrative Per Se (APS) Actions Taken by Year by Offender Status[a]

Offender status	BAC test	Year				
		1990/91	1991/92	1992/93	1993/94[b]	1994/95
Total APS Offenders		**275,786**	**249,823**	**218,943**	**197,191**	**171,502**
No prior DUI convictions	Completed	179,757	162,015	142,753	125,620	107,838
	Refused	11,101	10,068	8,999	7,546	6,525
Prior DUI status convictions	Completed	74,404	68,136	59,355	53,025	42,373
	Refused	10,524	9,604	7,836	6,806	5,253

[a]Figures exclude actions later set aside.

[b]In January 1994 California implemented a .01% BAC per se limit for drivers under age 21 carrying an administrative license suspension for violators. In 1993/94 there were 4,194 such suspensions, and in 1994/95 there were 9,511 such suspensions, which are included in the total offender counts.

These figures reflect a drop in suspensions/revocations of 9.4% from the first to the second year, 12.4% from the second to the third year, 9.9% from the third to the fourth year, and 13.0% from the fourth to the fifth year. This drop is generally consistent with decreases in overall DUI arrest rates as reported by the California Department of Justice (DOJ, 1992).

During the first year of APS, only 4.4% of eligible first offenders opted to participate in an alcohol treatment program—which qualified them for a restricted license to drive to and from the program—and only 3.6% of such offenders opted to participate in such programs during the second year. In the third year, participants rose to 3.8% of eligible first offenders and to 4.5% in the fourth year. On January 1, 1995, midway through the fifth year of the law, new legislation (SB 1758-Kopp) expanded the restriction to allow driving to and from and during the course of employment, with an increased restriction length of six months. Consequently, in 1994/1995, 8.6% of eligible first offenders opted to participate in an alcohol treatment program and receive a restricted license.

Table 2 summarizes annual departmental administrative hearing activity regarding APS. It shows that for each of the years that the law has been in effect, the great majority of offenders do not request a hearing, and that when hearings are requested the suspension action is usually upheld. These data also show a trend toward increases in the rate of hearing requests and decreases in the proportion of sustained actions.

by the control series. Consequently, the time series analyses which follow included only those covariates with predictive potential in the analysis, beyond what was explained by the control series.

Table 3

Potential Covariates and their Lag-Relationship (Number of Months Lagged) to the Dependent Series

Accident series	Covariates[a]		
	Licensed drivers	Personal income	Gasoline sales
Had-Been-Drinking:			
Fatal/Injury			
Fatal/Severe Injury	-5, -6		
Fatal	-5		
Nighttime:			
Fatal/Injury			
Fatal/Severe Injury	-5		
Fatal	-5	-9	
2:00-3:00 a.m. (Bar Closing Hour):			
Fatal/Injury		-6	0
Fatal/Severe Injury			
Fatal	-8	0	
Single Vehicle Nighttime Male:			
Fatal/Injury		-9	
Fatal/Severe Injury			
Fatal			

[a]Unemployment is not tabled since it was not significantly cross-correlated with any of the dependent measures.

Intervention time series analyses of alcohol-related accidents

Time series analyses were performed on monthly counts of the accident categories of interest—those likely to be alcohol-involved. These included HBD accidents, nighttime accidents, SVNM accidents and 2 to 3 a.m. bar-closing hour accidents. All of the series extend through 1993, providing 48 months of postintervention data following implementation of the 0.08% law and 42 months of data following implementation of the APS law. This post period is sufficient to identify salient long term impact patterns associated with the timing of the laws. All noise parameters in the time series models presented here were within the bounds of invertability[4] (McCleary & Hay, 1982) and the residuals for each model were best represented by a white noise process.

HBD Fatal and Injury Accidents

Series characteristics. Monthly fatal and injury (FI) accidents involving HBD and non-HBD drivers are plotted in Figure 2. For the time span represented in Figure 2, 1985 to 1994, the average monthly accident frequencies for HBD FI and non-HBD FI accidents were 3,121.75 and 15,981.83 accidents, respectively. Scaling differences of the vertical axes for the two plots reflect these different accident volumes, with non-HBD accidents being somewhat over five times greater in volume than HBD accidents.

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[4] When a series is within the bounds of invertability, it is statistically stationary in both level and variance, meaning it neither drifts nor trends.

Initially, the most visually predominant characteristic of the HBD FI series, represented in Figure 2.1, is its steady downward trend beginning in 1987. Also somewhat visually evident is a reduction in variance beginning midway through the series which, as will be discussed shortly, ultimately lead to a log-transformation of the series as a stabilizing measure.

The non-HBD FI accident control series shown in Figure 2.2 initially exhibits a pattern of steady increases between 1985 and 1987 followed by a fairly stable (horizontal) pattern of accidents through mid-1990, when the series begins a downward trend which persists through the remaining months of the series. As was evident in the HBD FI accident series, a reduction in variance is apparent in the series.

The reductions in variance in both the HBD FI and non-HBD FI data, beginning in mid-series, probably resulted from the downward trend observed in both series. McCleary and Hay (1982) state that "many social processes have naturally defined 'floors' which constrain the stochastic behavior of the process." They explain that as the process, or series, approaches its "floor" (in this case zero accidents), the series variance is necessarily constrained. Consequently, when the variance of such a process changes in mid-series, the series variance must first be rescaled to make the series stationary with regard to variance. A rescaling of the data to their natural logarithms produces such stationarity. As such, the HBD FI accident series and non-HBD FI accident series were each transformed using the natural log scale prior to conducting the time series analyses.

Figure 2.3 presents the proportion of total FI accidents which were considered HBD accidents. This plot shows that across the entire study period, there was a large, steady decline in the proportion of total accidents categorized as HBD.

Time series analysis. Table 4 presents model statistics and their associated diagnostics for HBD FI accidents. In addition to the logarithmic transformation of the series, to adjust for regular monthly trend in the data it was necessary to adjust the series' level by differencing them at lag 1. Once the HBD FI accident series and the non-HBD FI accident series were both made stationary in the larger sense, the previously described three-stage time series modeling strategy was applied. In addition to meeting the requirements of noise stability, models presented were judged to be the most parsimonious given the requisite of also providing the best "fit" or prediction of the dependent series. This, of course, is also true of all the final models accepted for each dependent series evaluated in this study. Thus model acceptance here, and for all models developed throughout the evaluation, was predicated on having both a nonsignificant Ljung-Box Q statistic and a relatively low residual mean square (RMS) error term. The RMS was used to measure unexplained variance or "error" left after the predictive time series model has been applied to the dependent accident series. None of the four potential covariate series were included in the final time series models because they were not significantly cross-correlated with the dependent variable; hence, their inclusion would not have significantly improved the predictive ability of the transfer function to detect an intervention effect.

Table 4

California "Had-Been-Drinking" Fatal/Injury Accident Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Non-"Had-Been-Drinking" Fatal/Injury Accidents as Control Series

Intervention model	Model component	Parameter	Lag	Estimate	t-value	L-B Q[a] (lag 25)	df	RMS[b]
Abrupt/temporary	.08 intervention	ω	0	-.0870	-1.95	33	97	.003
		δ	1	-.1143	-.23			
	APS intervention	ω	0	-.0126	-.37			
		δ	1	1.042	21.21			
	Control	β	0	1.004	8.85			
	Noise	θ	1	.7001	9.65			
		θ	7	.3468	3.37			
		θ	12	-.3396	-3.36			
		constant	0	-.0048	-2.66			
Gradual/permanent	.08 intervention	ω	0	-.0313	-.67	26	98	.003
		δ	1	-.5382	-.44			
	APS intervention	ω	0	-.0488	-1.08			
		δ	1	-.8016	-2.05			
	Control	β	0	.9673	8.45			
	Noise	θ	1	.6716	8.67			
		θ	7	.3484	3.38			
		θ	12	-.3927	-3.86			
		constant	0	-.0054	-3.28			
Abrupt/permanent	.08 intervention	ω	0	.0041	.11	34	100	.003
	APS intervention	ω	0	-.0046	-.13			
	Control	β	0	.9126	8.00			
	Noise	θ	1	.6854	9.20			
		θ	7	.3234	3.28			
		θ	12	-.4442	-4.72			
		constant	0	-.0053	-3.16			

Note. To adjust for monthly trend in the data, it was necessary to difference both the HBD and non-HBD series at lag 1. To adjust for mid series changes in variance, both the HBD and non-HBD series were log transformed prior to the analysis.

[a]Ljung-Box Q statistic

[b]Residual mean square

Intervention effects of the 0.08% and APS laws. Table 4 presents the model statistics for each time series analysis in the order performed to comply with the three-stage "blind" analysis procedure recommended by McCleary and Hay (1982). As outlined in the Method section, the "blind" analysis procedure is recommended for use when, as in this evaluation, selection of the specific form of the intervention is not guided by a particular a priori hypothesis.

Table 4 indicates that either the ω parameter estimates failed to reach statistical significance or in the single case of the abrupt/permanent model for the 0.08% law intervention for which the ω parameter estimate was significant, the δ parameter estimate was nonsignificant and negative. Recall that in order to be accepted, both the ω and the δ parameter estimates must be statistically significant and the δ parameter estimate must have a positive value. Given these accident series, a negative value suggests an oscillating effect which could not be reasonably argued to have been caused by the introduction of the two new laws. Consequently, for HBD FI accidents, the null hypotheses failed to be rejected for all of the intervention effects tested. Collectively, therefore, these assessments of HBD FI accidents, using non-HBD FI accidents as a control, failed to reveal a statistically significant change in accidents associated with either the timing of the APS law or the earlier 0.08% law.

HBD Fatal and Severe-Injury Accidents

Series characteristics. Figure 3.1 presents a plot of monthly HBD fatal and severe-injury (HBD FS) accidents. Figure 3.2 presents the comparable figures for fatal and severe-injury accidents involving drivers who were not identified as having been drinking (non-HBD FS), and Figure 3.3 presents a plot of the proportion of total fatal and severe-injury accidents that were considered HBD. (Recall that these levels of accident severity were combined and included to provide greater statistical power than the use of fatal accidents alone, and are considered somewhat more specifically alcohol-related than are fatal and total injury incidents.) Again the scaling of the vertical axes are different between plots as a result of the greater number of non-HBD FS accidents relative to HBD FS accidents.

Both the HBD FS and non-HBD FS plots in Figure 3.1 and 3.2, respectively, reveal similar patterns of seasonal fluctuations and a pattern of accident increases through mid-1987 followed by steady persistent declines beginning in 1990. These patterns of regular and seasonal trend are somewhat more pronounced among the control series accidents than they are among the HBD FS accidents, which is to be expected based on the "flooring" phenomenon described above.

Figure 3.3 reveals very gradual persistent decreases in the proportion of total fatal and severe-injury accidents considered HBD from late 1986 until late 1990, when a sharp downward trend began which persists for the remainder of the series.

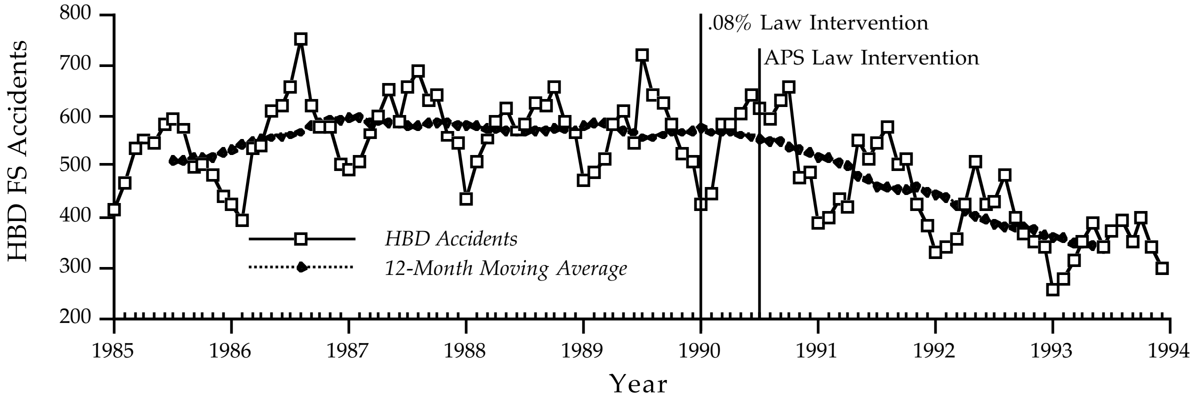


Figure 3.1. California "had-been-drinking" (HBD) fatal and severe-injury (FS) accidents by month, 1985-1994.

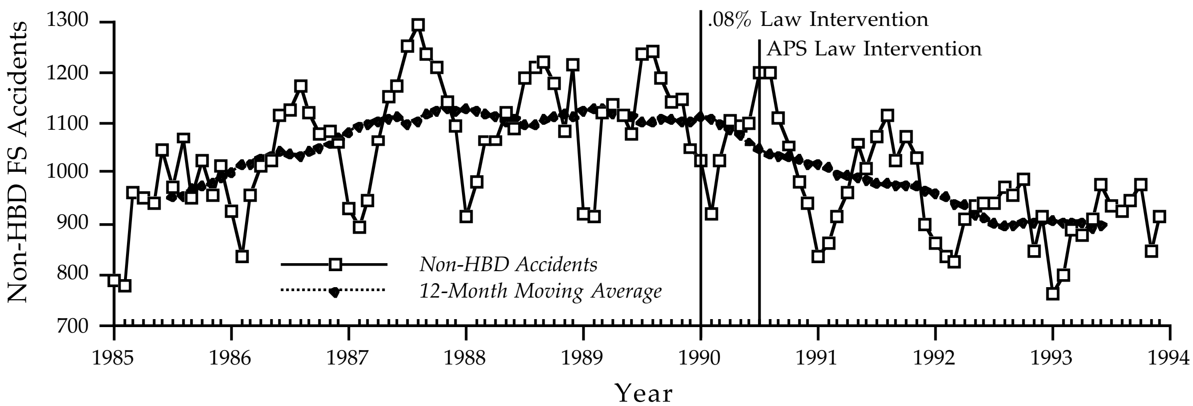


Figure 3.2. California non-"had-been-drinking" (Non-HBD) fatal and severe-injury (FS) accidents by month, 1985-1994.

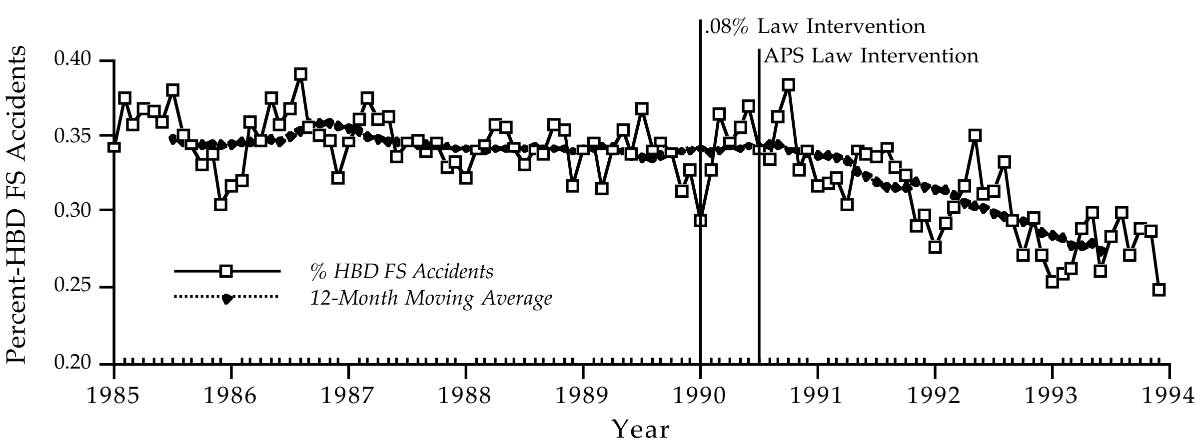


Figure 3.3. California "had-been-drinking" (HBD) fatal and severe-injury (FS) accidents as a proportion of total fatal and severe-injury accidents by month, 1985-1994.

<u>Time series analysis</u>. Table 5 presents model statistics and their associated diagnostics for HBD FS accidents. All models presented were judged to be the most parsimonious while providing the best "fit" or prediction of the dependent series.

In the earlier discussion of the preliminary analyses for considering the predictive merit of each covariate, Table 3 showed that the series of licensed drivers was the only covariate which had significant cross-correlations with the HBD FS accident series, thus warranting its inclusion in the time series models. The initial pattern of cross-correlations between the covariate and the HBD FS accident series indicated that maximal prediction would be obtained by using the number of licensed drivers both five and six months prior to the HBD FS accidents in any given month. When the covariate was assessed, lagging the covariate series both five and six months back simultaneously in the bivariate assessments with the dependent variable, the covariate became nonsignificant when lagged five months back and was removed from further assessment. Consequently, the series of licensed drivers was ultimately only included as a covariate lagged back six months. As will be the case in each table of time series model statistics throughout this report, the number of months that a given covariate was shifted backward in the final time series models is indicated in Table 5 by a negative number under the column heading "lag"; in this case licensed drivers is denoted with a "-6."

The control scaling coefficient β was positive and statistically significant for all tests, including those which incorporated the covariates, confirming the value of including the non-HBD FS accident series in the ARIMA models as a means of significantly reducing otherwise unexplained variation in the treatment series.

<u>Intervention effects of the 0.08% law</u>. When applied to the 0.08% law analysis, each of the three possible intervention effect hypotheses was rejected. The table shows that for each form of intervention model, either one or both of the 0.08% law parameter estimates were nonsignificant, or were unacceptable because they either resulted in a large negative δ or in a δ parameter greater than unity. As previously stated, the estimated intervention effect pattern predicted by a large negative δ parameter was not considered a reasonable outcome of these laws. More specifically, an oscillating pattern implied by such an effect could not be reasonably argued to have been caused by implementation of the 0.08% law. As explained in the Method section, a δ parameter greater than unity is outside of the required bounds of system stability and also suggests that the particular impact assessment model being considered is unstable.

Table 5

California "Had-Been-Drinking" Fatal/Severe-Injury Accident Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Non-"Had-Been-Drinking" Fatal/Severe-Injury Accidents as Control Series

Intervention model	Model component	Parameter	Lag	Estimate	*t*-value	L-B *Q*[a] (lag 25)	*df*	*RMS*[b]
Abrupt/temporary	.08 intervention	ω	0	-20.91	-2.01	29	100	1707
		δ	1	1.045	102.75			
	APS intervention	ω	0	-18.41	-1.03			
		δ	1	-.8408	-4.51			
	Control	β	0	.5315	62.69			
	Noise	θ	1	-.3634	-3.75			
		θ	3	-.2891	-2.90			
Abrupt/temporary with covariate	.08 intervention	ω	0	-13.89	-1.04	17	92	1521
		δ	1	1.053	57.23			
	APS intervention	ω	0	-40.34	-.90			
		δ	1	.4169	.49			
	Control	β	0	.5235	34.91			
	Licensed drivers	β	-6	183.8	2.22			
	Noise	θ	3	-.3731	-3.80			
		θ	12	-.3237	-3.36			
		ϕ	1	.4629	4.83			
Gradual/permanent	.08 intervention	ω	0	-18.80	-1.09	19	95	1566
		δ	1	-.9307	-11.32			
	APS intervention	ω	0	-2.852	-1.97			
		δ	1	1.008	42.03			
	Control	β	0	.5299	77.52			
	Noise	θ	3	-.2583	-2.59			
		ϕ	1	.3586	3.69			
		ϕ	4	-.2817	-2.89			
Gradual/permanent with covariate	.08 intervention	ω	0	-5.513	-.24	19	92	1502
		δ	1	-.9204	-1.40			
	APS intervention	ω	0	-3.061	-1.36			
		δ	1	1.006	29.91			
	Control	β	0	.5254	40.67			
	Licensed drivers	β	-6	218.2	3.21			
	Noise	θ	3	-.3328	-3.38			
		θ	12	-.3070	-3.20			
		ϕ	1	.4226	4.38			
Abrupt/permanent	.08 intervention	ω	0	-6.473	-.21	17	97	1760
	APS intervention	ω	0	-75.35	-2.41			
	Control	β	0	.5295	45.42			
	Noise	θ	3	-.3120	-3.02			
		ϕ	1	.5409	5.73			
		ϕ	4	-.1920	-1.83			
Abrupt/permanent with covariate	.08 intervention	ω	0	-12.24	-.41	14	94	1588
	APS intervention	ω	0	-53.10	-1.70			
	Control	β	0	.5158	27.35			
	Licensed drivers	β	-6	199.2	3.19			
	Noise	θ	3	-.3896	-3.99			
		θ	12	-.3604	-3.77			
		ϕ	1	.5566	6.09			

Note. To adjust for nonstationarity, the licensed drivers covariate series was independently differenced at lag 1 prior to analysis. The lag value -6 for the licensed drivers series indicates that it was shifted backward six months for maximal adjustment in the analyses. Shading indicates a statistically significant (p<.10; one-tailed test) and acceptable intervention effect.

[a]Ljung-Box Q statistic

[b]Residual mean square

Intervention effects of the APS law. Table 5 shows that the initial abrupt/temporary and gradual/permanent effect models also resulted in nonsignificant parameter estimates or parameter estimates outside of the bounds of system stability for the APS law intervention. Again this was manifest by either a large negative δ or in a δ parameter greater than unity. However, when the third-stage abrupt/permanent effect hypothesis was modeled, all model components were significant (t = -2.41, p = .016), and the null hypothesis of no intervention effect was rejected. Including the series of licensed drivers as an additional explanatory variable consistently reduced the error variance (as indicated by reductions in the RMS error measure) for each of the three hypothesized forms of intervention. The APS law intervention effect remained significant after including the additional variable (t = -1.70, p = .09), although the estimated monthly decline in accidents decreased from 75.4 to 53.1 fewer accidents per month. This latter figure equates to a reduction of 9.4% from the preintervention mean.

As described in the Method section, such a reduction after including the covariate series suggests that the control series did not adequately control for the shared variation with the dependent accident series, the covariate series itself was affected by the interventions or by a third exogenous variable affecting both dependent and covariate series, or the covariate itself exerted a causal effect on the dependent series independent of the effects of the laws.

To determine whether the reduction in the effect found here could possibly be attributed to a significant intervention effect on the covariate series, a univariate intervention time series analysis was performed using the licensed drivers covariate series as the dependent series. Similar analyses were performed for each of the other covariate series as well. This assessment of the interventions on the licensed drivers covariate series revealed a significant decrease (t = -4.58, p<.001) associated with the timing of the 0.08% law. No significant decreases were found associated with the timing of the APS law on this or any of the other covariate series and none of the other covariates revealed a significant decrease associated with the timing of the 0.08% law. While the significant decrease in licensed drivers was most likely caused by something other than the introduction of the 0.08% law, the fact that the series does show a significant decrease coinciding with the 0.08% intervention point suggests that the covariate series may be contributing to the diminished effect of the APS intervention.

The time series analyses performed for HBD FS accidents revealed high correlations between the parameter estimates of the 0.08% law intervention and that of the APS law. Similarly, high to moderate correlations were also obtained in several of the analyses of the other accident categories as well. To obtain an indication of the effect that this lack of independence might have on the sensitivity of the main analyses to detect individual effects of the two interventions (only six months apart), a series of supplemental exploratory time series analyses were performed in which the two interventions were assessed separately. Such an analysis was performed for each accident variable which had revealed at least moderate cross-correlations (r≤.4) in the analyses which had simultaneously included both interventions. In each of these supplemental analyses, when the original assessments incorporating both interventions simultaneously had revealed significance associated with the timing of one or both of the new laws, the new analyses resulted in significance associated with both interventions. Conversely, when the results of the original analysis had failed to detect any significant intervention effect, but showed high correlations between the intervention parameter estimates, these supplemental analyses revealed comparable nonsignificance and relatively unchanged effect magnitudes. This pattern of results suggests that had one of the interventions been assessed without consideration for the other, too much of the variance would have been falsely attributed to the one intervention examined. In effect, by including both, each intervention may be operating much like a covariate to the other. Since none of these supplemental analyses jeopardized the integrity of the current intervention-inclusion strategy, and in fact provided some evidence that it may be a superior strategy, no further results of the supplemental analyses will be presented here but may be furnished upon request made to the author.

HBD Fatal Accidents

Series characteristics. Figure 4.1 presents a plot of monthly HBD fatal accidents and Figure 4.2 presents the comparable figures for fatal accidents involving drivers who were not identified as having been drinking (non-HBD). Figure 4.3 presents a plot of the proportion of total fatal accidents that were considered HBD. (Recall that HBD fatal accidents represent the single most specifically alcohol-related category of accidents in this assessment.) Again the scaling of the vertical axes are different between plots as a result of the greater number of non-HBD fatal accidents relative to HBD fatal accidents.

Both the HBD and non-HBD plots in Figures 4.1 and 4.2, respectively, reveal similar patterns of large seasonal fluctuations and variability throughout the series. HBD fatals in Figure 4.1 show a slight increase in the early part of the series followed by a steady decline thereafter. Non-HBD fatals in Figure 4.2 show a pattern of accident increases marked by increasing variability through 1988 followed by steady declines through mid-1992, when the pattern reverses and non-HBD fatals again show an increase throughout the remainder of the series.

The kind of accelerated drop coinciding with the implementation dates of the new drunk driving laws that was found among the percent of fatal and severe-injury accidents considered HBD is not found in this series. To the extent that severe injuries serve as an alcohol surrogate measure, this leads to the speculation that, at that time, there was a disproportionately greater drop in alcohol injuries among fatal and severe-injury accidents than there was among alcohol related fatalities.

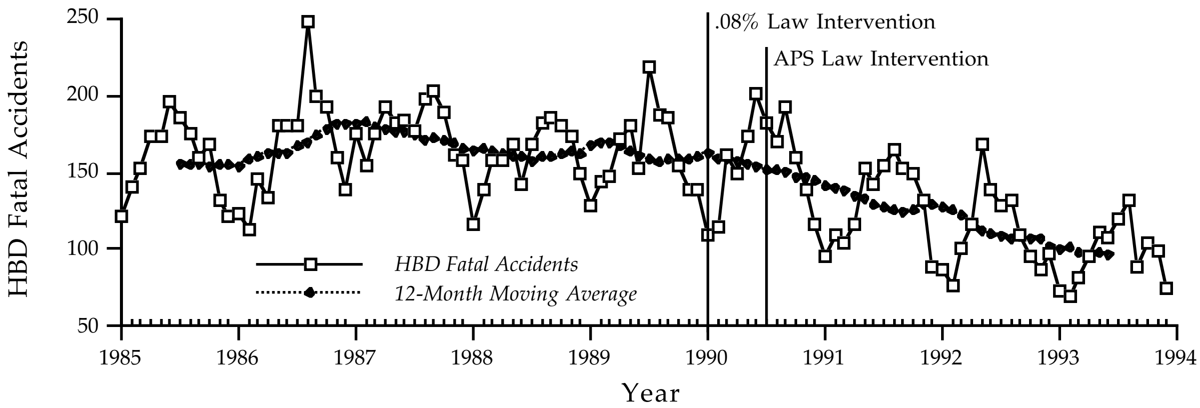


Figure 4.1. California "had-been-drinking" (HBD) fatal accidents by month, 1985-1994.

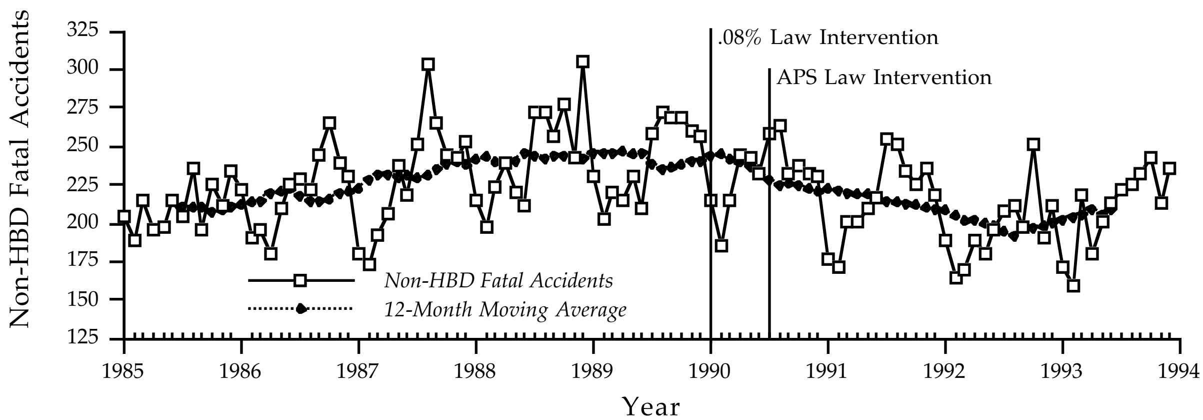


Figure 4.2. California non-"had-been-drinking" (Non-HBD) fatal accidents by month, 1985-1994.

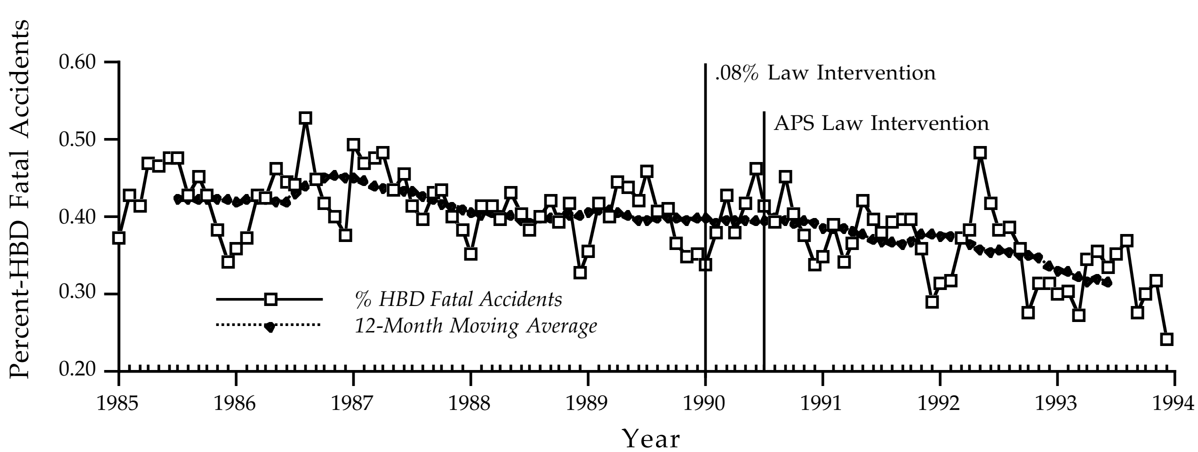


Figure 4.3. California "had-been-drinking" (HBD) fatal accidents as a proportion of total fatal accidents by month, 1985-1994.

Time series analysis. Table 6 presents model statistics and their associated diagnostics for HBD fatal accidents. As mentioned above, all minimal parameter requirements were satisfied by the models presented.

Table 3 (above) showed that, as with the previous analysis of HBD FS accidents, number of licensed drivers had significant cross-correlations with HBD fatal accidents. However, among fatals the predictive ability was maximized with a lag going back only five months. The bivariate assessment of the covariate with the dependent variable suggested that including the series of licensed drivers would provide further predictive potential than the inclusion of the control series alone.

Again the control scaling coefficient β was positive and statistically significant for all tests, including those which incorporated the covariate.

Intervention effects of the 0.08% law. Table 6 presents the time series model statistics for each analysis of HBD fatal accidents in accord with the three-stage "blind" analysis procedure. With one exception, all of the intervention parameter estimates for both the abrupt/temporary and gradual/permanent effect hypotheses, and for both law interventions, were unacceptable because they either resulted in a large negative δ or in a δ parameter greater than unity, in either event indicating an effect which could not be reasonably argued as resulting from the implementation of a new law. The single exception was the abrupt/temporary 0.08% law intervention parameter estimates which were stable but clearly nonsignificant. With respect to the 0.08% law intervention, the remaining abrupt/permanent effect hypothesis was also rejected since the ω estimate value was nonsignificant.

Intervention effects of the APS law. Among HBD fatal accidents the APS law model components were marginally significant in only the third-stage abrupt/permanent effect hypothesis, and only prior to including the licensed drivers covariate. This model estimated a reduction of 20.83 accidents per month from the pre-intervention period ($t = -1.31$, $p = .19$), representing a 12.7% decrease from the series pre-intervention mean of 164 accidents per month. Once the covariate was added to the model, the effect parameter dropped to 17.5 fewer accidents per month, amounting to a 10.7% reduction, although the estimated intervention parameter was no longer statistically significant. Again it is presumed that the covariate reduced the magnitude of the effect by alternatively explaining some portion of the variance which had been attributed to the intervention in the absence of another explanatory variable.

Table 6

California "Had-Been-Drinking" Fatal Accident Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Non-"Had-Been-Drinking" Fatal Accidents as Control Series

Intervention model	Model component	Parameter	Lag	Estimate	*t*-value	L-B Q[a] (lag 25)	*df*	*RMS*[b]
Abrupt/temporary	.08 intervention	ω	0	-20.60	-.94	32	100	512.14
		δ	1	.4886	.54			
	APS intervention	ω	0	-8.177	-1.47			
		δ	1	1.054	65.29			
	Control	β	0	.7018	33.02			
	Noise	θ	1	-.4278	-4.61			
		θ	11	-.2578	-2.69			
Abrupt/temporary with covariate	.08 intervention	ω	0	-5.314	-1.16	21	95	492.86
		δ	1	1.055	62.76			
	APS intervention	ω	0	8.166	1.52			
		δ	1	-.9453	-20.77			
	Control	β	0	.6915	29.92			
	Licensed drivers	β	-5	-58.75	-1.33			
	Noise	θ	1	-.4884	-5.35			
		θ	11	-.3193	-3.37			
Gradual/permanent	.08 intervention	ω	0	6.234	1.06	27	101	495.82
		δ	1	-.9890	-27.91			
	APS intervention	ω	0	-.9224	-1.39			
		δ	1	1.025	33.86			
	Control	β	0	.6900	31.58			
	Noise	θ	1	-.5011	-5.78			
		θ	11	-.3211	-3.43			
Gradual/permanent with covariate	.08 intervention	ω	0	-.4653	-.84	15	94	469.39
		δ	1	1.041	28.70			
	APS intervention	ω	0	13.93	1.51			
		δ	1	-.9563	-24.07			
	Control	β	0	.6610	20.74			
	Licensed drivers	β	-5	-59.44	-1.41			
	Noise	θ	1	-.6268	-6.69			
		θ	2	-.3249	-2.91			
		θ	11	-.3261	-3.40			
Abrupt/permanent	.08 intervention	ω	0	-1.687	-.11	18	102	498.97
	APS intervention	ω	0	-20.83	-1.31			
	Control	β	0	.6607	19.94			
	Noise	θ	1	-.5840	-6.30			
		θ	2	-.3992	-3.81			
		θ	11	-.2774	-2.77			
Abrupt/permanent with covariate	.08 intervention	ω	0	-4.506	-.27	20	96	512.76
	APS intervention	ω	0	-17.54	-1.03			
	Control	β	0	.6604	19.59			
	Licensed drivers	β	-5	-30.33	-.66			
	Noise	θ	1	-.5731	-5.82			
		θ	2	-.3853	-3.66			
		θ	11	-.2633	-2.59			

Note. To adjust for nonstationarity, the licensed drivers covariate series was independently differenced at lag 1 prior to analysis. The lag value -5 for the licensed drivers series indicates that it was shifted backward five months for maximal adjustment in the analyses. Shading indicates a statistically significant ($p<.10$; one-tailed test) and acceptable intervention effect.

[a]Ljung-Box Q statistic

[b]Residual mean square

In the supplemental analyses referred to earlier, in which each intervention was separately modeled subsequent to obtaining high correlations between the transfer function estimates in the combined analysis, the HBD fatal series residuals presented marginal evidence of an annual seasonal trend when 0.08% law intervention was included (but not even suggestively evident when the APS intervention parameters were entered). As an added precaution, a seasonal difference was introduced in the supplemental analyses, for both the HBD and non-HBD fatal accidents. The differencing produced an acceptable model but one in which the β coefficient for the control series became nonsignificant. This provides some indication that the relationship between the control and dependent series was largely accounted for by shared seasonal patterns. In effect, differencing the series (i.e., removing the seasonality) removed the variance that would have been controlled by the control series. With the single exception of reducing the usefulness of including the control series, the reanalysis resulted in substantially similar intervention parameter estimates obtained when both interventions were simultaneously assessed. Taken together the results of these two sets of analyses suggest that in the former analysis, the control series adequately accounted for the slight (if any) seasonal trending in the dependent series and the time series model, as presented without differencing, was acceptable.

Nighttime Fatal and Injury Accidents

Series characteristics. Plots of aggregated monthly nighttime and daytime FI accidents are shown in Figure 5.1 and 5.2 respectively. Scaling differences of the vertical axes for the two plots reflect the series slightly different accident volumes.

The nighttime FI series shows a strong seasonal pattern of cyclical fluctuations 12 months apart. Within this cyclical pattern, and across the entire series, the fewest accidents consistently occur in January and February and the greatest number of accidents occur in the summer months of June through September, when there are more hours of daylight and driving exposure is at its peak.

In addition to further strong visual evidence of the cyclical pattern, Figure 5.3 indicates that the proportion of total FI accidents occurring during nighttime also showed a pattern of slow steady decline throughout the series.

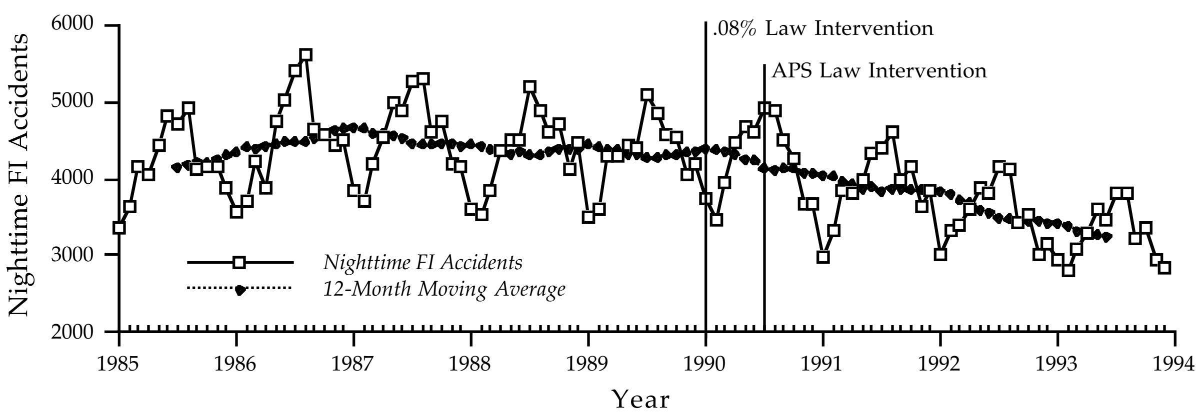


Figure 5.1. California nighttime fatal and injury (FI) accidents by month, 1985-1994.

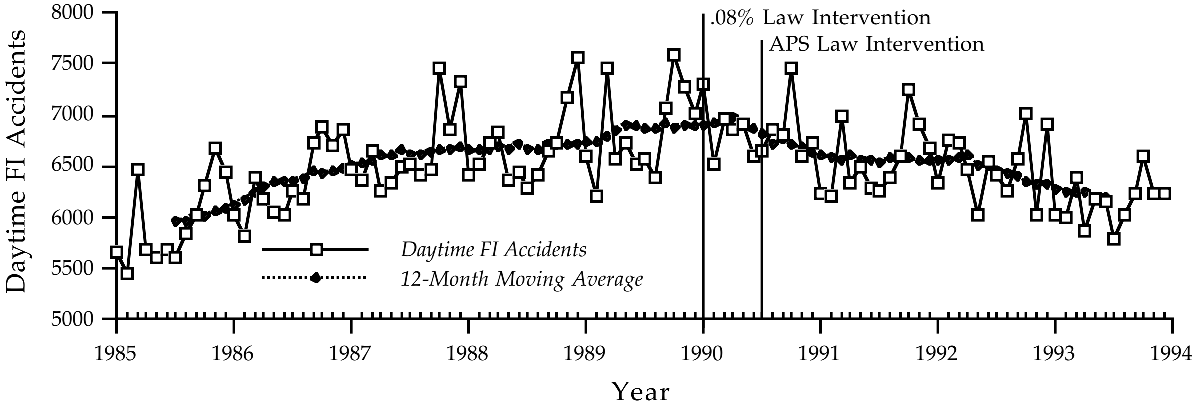


Figure 5.2. California daytime fatal and injury (FI) accidents by month, 1985-1994.

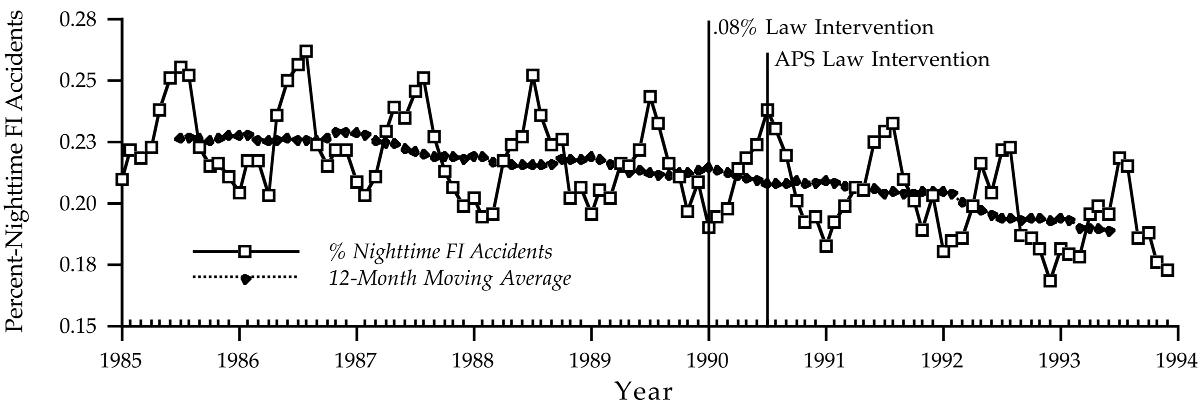


Figure 5.3. California nighttime fatal and injury (FI) accidents as a proportion of total fatal and injury accidents by month, 1985-1994.

Time series analysis. None of the four potential covariate series were included in the final time series models because none were found to be significantly cross-correlated with the dependent variable, hence, their inclusion would not have significantly improved the predictive ability of the model to detect an intervention effect. Table 7 presents model diagnostics and statistics for the intervention effects detected by the three-stage modeling strategy. Again, the models presented were judged to be the most parsimonious and to provide the best fit as determined by the model diagnostics. The table shows that the control scaling coefficient β was positive and statistically significant for all tests, again confirming the value of its inclusion in significantly reducing unexplained variation in the treatment series.

Table 7

California Nighttime Fatal/Injury Accident Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Daytime Fatal/Injury Accidents as Control Series

Intervention model	Model component	Parameter	Lag	Estimate	*t*-value	L-B Q^a (lag 25)	*df*	RMS^b
Abrupt/temporary	0.08 intervention	ω	0	-100.4	-1.31	19	86	33008
		δ	1	-.9520	-15.21			
	APS intervention	ω	0	189.4	1.50			
		δ	1	-.7690	-3.18			
	Control	β	0	.2871	3.82			
	Noise	θ	1	.6487	7.91			
		θ	7	.2949	2.94			
		θ	12	.8659	23.87			
Gradual/permanent	0.08 intervention	ω	0	-135.4	-1.02	22	87	33382
		δ	1	-.9523	-11.13			
	APS intervention	ω	0	137.7	.83			
		δ	1	-.8224	-1.99			
	Control	β	0	.2701	3.59			
	Noise	θ	1	.6325	7.72			
		θ	7	.2829	2.85			
		θ	12	.8703	25.30			
Abrupt/permanent	0.08 intervention	ω	0	-29.24	-.21	23	89	32935
	APS intervention	ω	0	-78.46	-.58			
	Control	β	0	.2559	3.52			
	Noise	θ	1	.6530	8.25			
		θ	7	.2427	2.51			
		θ	12	.8693	25.40			

Note. To adjust for monthly trend and to stablize annual trend in the data, it was necessary to difference both the nighttime and daytime series at lags 1 and 12.

[a]Ljung-Box *Q* statistic

[b]Residual mean square

Intervention effects of the 0.08% and APS laws. Table 7 indicates that nonstationarity of the series required that both the nighttime and daytime accident series be differenced to adjust for both significant seasonal (12 months apart) and regular (month-to-month) downward trends. The null hypotheses failed to be rejected for all of the intervention effects tested. Only the abrupt/temporary model of the three stage hypothesis testing process resulted in significant changes in the series of nighttime FI accidents subsequent to the implementation of the two new laws. While the model suggests a reduction in accidents associated with the timing of the implementation of the 0.08% law, it suggests a temporary increase in accidents associated with the timing of the APS law. However, in this and in the gradual/permanent effect model, the δ parameter estimates resulted in large negative δ values representing an oscillating pattern of recovery which could not be considered a reasonable outcome of these laws. The ω parameters shown in

Table 7 indicate that the accident reductions estimated in the subsequent stages of the hypothesis testing model were nonsignificant for both interventions. Consequently, the nighttime FI accident series, using daytime FI accidents as a control, failed to reveal a statistically significant change in accidents that could reasonably be attributed to either the 0.08% law or the subsequent APS law, six months later.

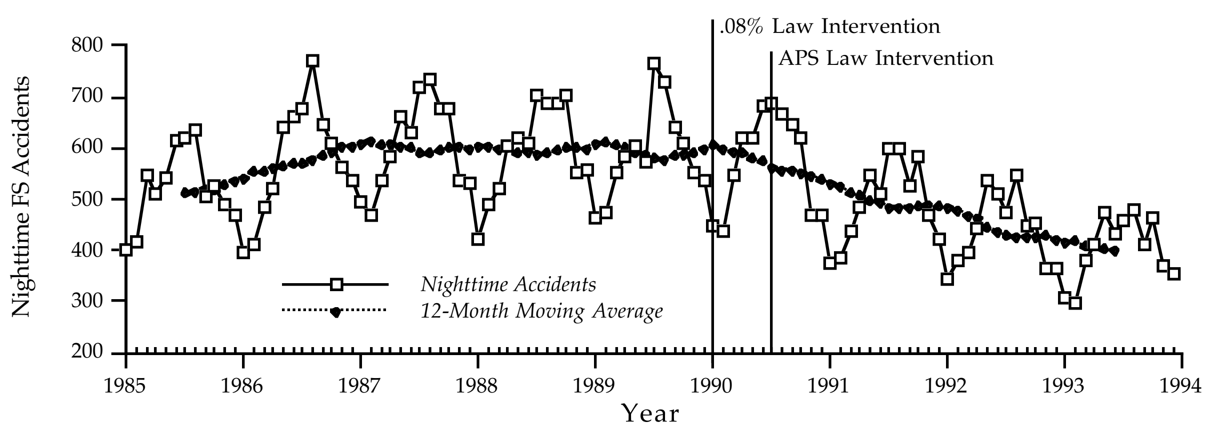
<u>Nighttime Fatal and Severe-Injury Accidents</u>

<u>Series characteristics</u>. Figure 6.1 presents a plot of aggregated monthly nighttime fatal and severe-injury (FS) accidents. Figure 6.2 presents figures for the daytime FS accident control series. Figure 6.3 presents a plot of the proportion of total fatal and severe-injury accidents occurring during nighttime hours (between 8 p.m. and 3:59 a.m.). Notice that the scaling of the vertical axes are again somewhat different between plots. This is a result of the greater number of nighttime FS accidents, relative to daytime FS accidents, with a range half that of those at night. As usual, the 12-month moving average and both points of intervention are indicated in each of the time series plots.

As with the series of nighttime FI accidents, nighttime FS accidents show a strong 12-month seasonal component. In Figure 6.1, it can again be seen that the fewest nighttime FS accidents occur in January and February and the highest during the summer months when driving exposure is at its greatest.

Visual inspection of the nighttime and daytime series in Figure 6 suggests that while the seasonal pattern is more pervasive in the nighttime series, both series show patterns of accident increases in the first few years of the series followed by steady decreases beginning midway through the series. This downward trend is somewhat more evident in the daytime accident series.

With the exception of the predominant 12-month cyclical fluctuations, Figure 6.3 reveals a fairly stable overall pattern in the proportion of FS accidents that occur at night. Close inspection shows that the series exhibits a slight downward trend beginning in 1990 which persists through the remainder of the series.



<u>Figure 6.1</u>. California nighttime fatal and severe-injury (FS) accidents by month, 1985-1994.

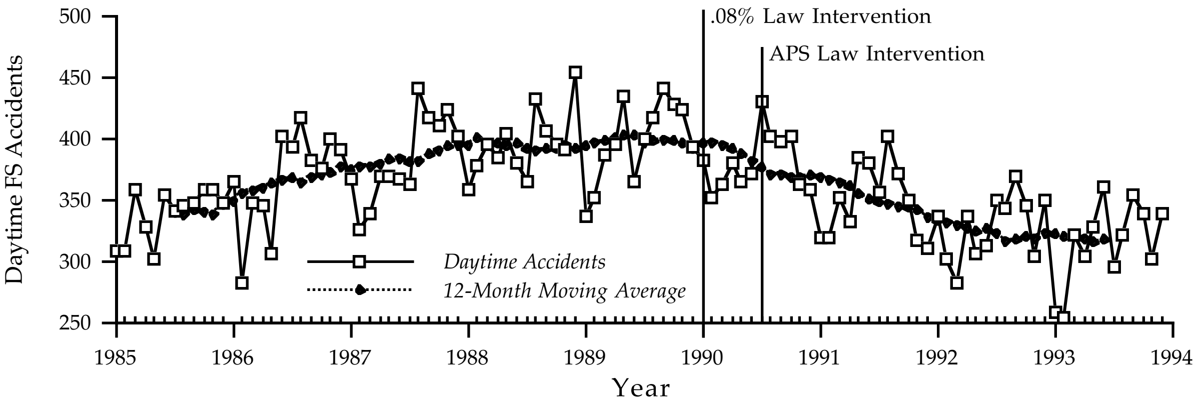


Figure 6.2. California daytime fatal and severe-injury (FS) accidents by month, 1985-1994.

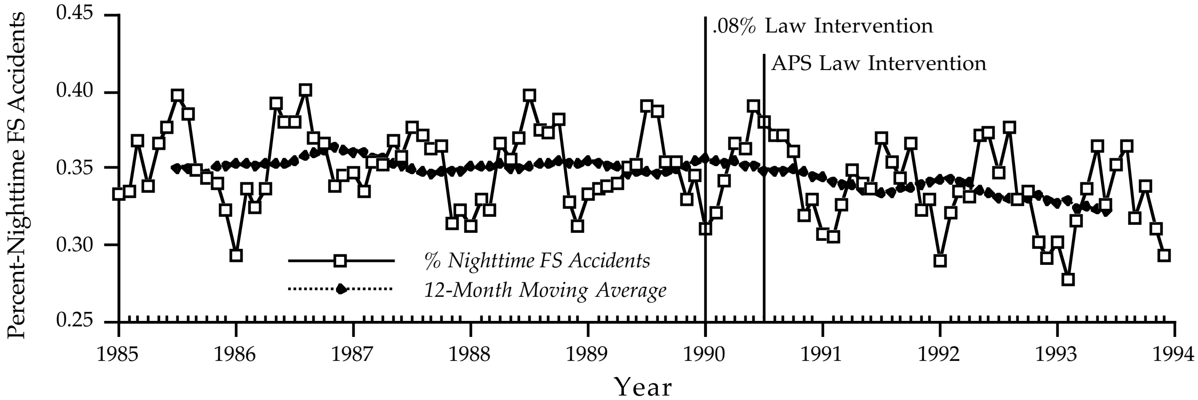


Figure 6.3. California nighttime fatal and severe-injury (FS) accidents as a proportion of total fatal and injury accidents by month, 1985-1994.

Time series analysis. Table 8 presents model diagnostics and statistics for the intervention effects detected by the three-stage modeling strategy. As always, the models presented were judged to be the most parsimonious and to provide the best fit as determined by the model diagnostics.

With one possible exception, the table shows that the control scaling coefficient β was positive and statistically significant for all tests, again confirming the value of its inclusion in significantly reducing unexplained variation in the treatment series. The possible exception is in the abrupt/permanent modeling strategy in which licensed drivers (lagged five months back) was included as a covariate. Here, it can be seen that the control scaling coefficient was only marginally significant.

Intervention effects of the 0.08% law. Table 8 shows that, as in each of the previous analyses, the intervention parameters in the abrupt/temporary effect model were either nonsignificant or were outside of the bounds of system stability and therefore could not be considered to have resulted from the legislation. Likewise, the intervention parameters in the gradual/permanent effect model pertaining to the 0.08%