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DMV
DEPARTMENT OF MOTOR VEHICLES

The General Deterrent Impact of California's 0.08% Blood Alcohol concentration Limit and Administrative Per Se License Suspension Laws

Volume 1
of

AN EVALUATION OF THE EFFECTIVENESS OF CALIFORNIA'S
0.08% BLOOD ALCOHOL CONCENTRATION LIMIT AND
ADMINISTRATIVE PER SE LICENSE SUSPENSION LAWS

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This project evaluated the effects of two new driving-under-the-influence (DUI) laws implemented in California. The first law, effective January 1, 1990, reduced California's illegal per se limit to 0.08% blood alcohol concentration (BAC) and the second, effective July 1, 1990, imposed an administrative per se (APS) pre-conviction license suspension on DUI offenders. Intervention time series analysis was used to evaluate the deterrent impact of these laws on the general population of DUI offenders as measured by the effects on alcohol-related traffic accidents.

In spite of the new laws being imposed in an environment of already strict sanctions for DUI, relatively high use of post-conviction license suspension actions, and a long-term downward trend in alcohol-involved accidents, the results of this evaluation showed a significant general deterrent effect associated with the implementation of the APS law, with somewhat less support for such an effect associated with the 0.08% BAC per se limit law. Larger proportions of the observed accident reductions were associated with the timing of the APS law than with the lowering of the illegal per se limit. Some, but not all, of these reductions were statistically significant, even after accounting for additional socioeconomic factors shown to contribute to the consistent downward trend observed in both the alcohol- and nonalcohol-related accident series. There was suggestive evidence that a media campaign initiated approximately one year after implementing the APS law may have enhanced that law's effectiveness.

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PREFACE

This report is the first volume of a two volume report entitled *An Evaluation of the Effectiveness of California's 0.08% Blood Alcohol Concentration Limit and Administrative Per Se License Suspension Laws.* This project is a part of the California Traffic Safety Program and was funded by the National Highway Traffic Safety Administration through a grant administered by the California Office of Traffic Safety (Grant # AL9101). The present report is being issued as a departmental publication rather than an official State of California policy monograph. The opinions, findings, and conclusions expressed in this report are therefore those of the author and not necessarily those of the State of California or the National Highway Traffic Safety Administration.

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This study was conducted under the general direction of Raymond C. Peck, DMV Research Chief, and the supervision of Clifford J. Helander, Research Manager. The author wishes to thank them and Dr. Mary K. Janke, Research Scientist III, for their thorough reviews of, and contributions to, the report drafts. Thanks also go to Robert A. Hagge, Research Manager, for his guidance in conducting the time series analyses and to Leonard A. Marowitz, Research Analyst II, for his assistance with some of the statistical methodology.

Finally, thanks go to Douglas Luong, Office Technician, for assistance in typing portions of the report and to Debbie McKenzie, Staff Services Analyst, who in addition to typing portions of the report, completed the numerous tables and figures in the report and oversaw its publication.

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EXECUTIVE SUMMARY

Background

- Although California driving-under-the-influence (DUI) law was strengthened during the 1980s, drunk driving has continued to present a major problem as reflected by over 2,500 fatalities and 63,000 injuries in alcohol-related accidents in the state during 1989. Additionally, more recent figures show that California continues to have less than optimal DUI conviction rates, ranging by county from only 57.8% to 83.3%. DUI cases are also often marked by long delays between arrest and conviction.

- License suspension has proven to be an effective sanction in California (and elsewhere) in reducing total accidents subsequent to conviction.

- Studies from other states have shown preconviction administrative license action laws to have a significant impact in reducing alcohol-involved accidents. These studies have shown that administrative license action laws have been more effective as an overall deterrent than court imposed measures such as jail sentences or fines. Consequently, in an effort to effect a greater general deterrent against drunk driving, on July 1, 1990, California became the 28th state to implement an administrative per se (APS) license suspension law. Six months prior to enactment of APS, California also reduced the illegal per se blood alcohol concentration (BAC) threshold from 0.10% to 0.08%.

The APS Law

- The APS law requires the Department of Motor Vehicles (DMV) to suspend the driving privilege of drunk drivers arrested with a BAC of 0.08% or greater, or who refuse or fail to complete a chemical test upon arrest, in a more timely, civil action, independent of the criminal DUI charge. Depending on whether the arrest was for a first or repeat offense, the APS suspension/revocation terms vary from four months to one year, or from one year to three years upon refusing a chemical test. Due process is allowed by the issuance of a 30-day temporary license intended to provide the driver with sufficient time to challenge the suspension through DMV administrative review.

The 0.08% BAC Per Se Limit Law

- Research has shown that some driving related skills are impaired at BACs as low as 0.03% - 0.05%, and that accident responsibility increases with BAC level. On January 1, 1990, just six months prior to implementation of the APS law, California became only the fourth state (behind Oregon, Utah, and Maine) to lower the illegal per se BAC limit to 0.08%. Under per se laws, drivers are defined to be impaired and to be driving illegally if they exceed the prescribed level irrespective of the degree of observed behavioral impairment.

Research Design and Data Development

- Intervention time series analyses were used for assessing the long term impact of the two DUI laws implemented in 1990. An intervention time series analysis involves repeated measurement of a variable (e.g., alcohol-involved accidents) both before and after the occurrence of an intervention (in this case the DUI laws). If the laws were effective in deterring impaired driving, one expects to find an alteration in the level and pattern of the time series during the post-law period.

- From a technical standpoint, the close temporal contiguity of the two interventions (implemented only 6 months apart) creates a sensitivity problem in detecting intervention effects uniquely attributable to either of the two laws. That is, effects which appear to be associated with the APS law might be more accurately attributable to lingering effects of the 0.08% BAC law. Conversely, the earlier effects associated with the implementation of the 0.08% law might actually be reflecting anticipatory effects of the upcoming APS law. As such, it is best to view the interventions as being interrelated rather than as independent operators with unique effects.

- Separate intervention time series analyses were performed for each of four alcohol-involved accident or surrogate measures, each paired with an appropriate low- or nonalcohol accident (control) series. The inclusion of the control series helps prevent attributing significance to the DUI legislation that should, more accurately, be attributed to some independent, but coincident, latent event. The four major dependent variable accident categories, and their control series were:

    1. A police designated category of accidents for which the reporting officer indicated that one or more of the involved drivers "had been drinking" (HBD), fitting non-HBD accidents as the control;

    2. nighttime accidents, fitting daytime accidents as the control;

    3. single-vehicle nighttime accidents involving male drivers, fitting multiple vehicle daytime accidents as the control;

    4. accidents occurring between 2 and 3 a.m. (the first hour subsequent to California's 2 a.m. mandatory bar-closing time), fitting daytime accidents occurring between 10 and 11 a.m. (the one hour, across all days of the week, for which there were proportionately fewer HBD incidents than at any other time of day) as the control.

- For each accident variable described above, three subsets of accident severity having varying degrees of statistical power or association with alcohol involvement were assessed. They were:

    1. fatal and total-injury accidents combined;

    2. fatal and severe-injury accidents combined;

    3. fatal accidents alone.

- To account for further uncontrolled variation in the dependent series, additional explanatory variables were considered for inclusion in each accident series analysis, and were included if they significantly furthered error reduction in the model. These series consisted of: California licensed drivers, monthly highway sales of gasoline, California personal income, and California monthly unemployment rates.

Results

Process Measures

- In the first five years of the APS law, over 1 million license suspension/revocation actions were taken (after excluding actions later set aside). Administrative hearings were requested by between 7% and 10% of those who received an APS order and the majority of those hearings (between 72% and 88%) resulted in the action being upheld.

- Compared with the pre-APS time lag of *5.5* months between a DUI arrest and license action (post-conviction), post-APS average time lags were eventually reduced to the current average of about 33 days between arrest and suspension or revocation, representing a substantial increase in the "swiftness" of punishment for DUI offenders.

- During the first four years of the law, only about 4% of eligible first offenders, on average, opted to apply for an alcohol treatment program license restriction, which allowed driving to and from program activities only. Midway through the fifth year of the law, on January 1,1995, new legislation expanded the restriction to also allow driving to, 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.

Long Term Impact on Statewide Alcohol-Related Accidents

- The accident, arrest, and covariate series in this report showed broad, general changes beginning around, or even before, 1990 which resulted in a directional shift in many of the series presented. Among accidents, this is evidenced as a downward trend in *all types* of accidents portrayed, irrespective of their likely alcohol-involvement status.

- Significant reductions were most evident among fatal and severe-injury accident measures. There were few significant findings among fatal, and fatal and injury, accident measures. While this may be attributable to lack of statistical power (for fatal) or alcohol-involvement (for fatal and injury), the number of nonsignificant results in these analyses serves to underscore the somewhat marginal nature of many of the effects. Given the precipitous, unprecedented decline in alcohol-related accident measures over the past decade, however, it is perhaps not surprising that the small but significant effects of these laws would tend to be obscured by the strong downward trend over the past 10-15 years.

Effects Associated with the 0.08% Per Se BAC Limit

- While there were reductions in some of the alcohol surrogate accident categories following implementation of the 0.08% law, that legislation could not be linked to any significant decreases in the direct alcohol-involved accident measures of HBD accidents or in fatal accidents alone. Where impact occurred in association with the implementation of the 0.08% law, it was consistently in the form of either abrupt or gradual, permanent decreases as follows:

    - An immediate 7.2% reduction in nighttime fatal and severe-injury accidents, reflecting a one-year estimated total decrease of approximately 500 such accidents following the new laws.

    - An immediate 10.2% reduction in 2 to 3 a.m. fatal and total-injury accidents and an immediate reduction of 16.5% in 2 to 3 a.m. fatal and severe-injury accidents. These reductions represent one-year total estimated decreases of approximately 518 fewer fatal and total-injury accidents and 130 fewer fatal and severe-injury accidents.

    - Among fatal and total-injury single-vehicle nighttime accidents involving male drivers, there was marginal evidence of a small initial decrease of approximately 10 accidents per month, estimated to gradually increase to a maximum of 9.4% fewer accidents per month compared to the years preceding the new laws.

Effects Associated with the APS Law

- The timing of the APS legislation was associated with significant permanent reductions in each alcohol surrogate accident category for at least one, and sometimes more, of the accident severity levels assessed. As was the case with the 0.08% law, the largest proportional decrease was among accidents occurring between 2 and 3 a.m. Again, where legislation-specific impact occurred, it was consistently in the form of either abrupt/permanent or gradual/permanent decreases, as follows:

    - Immediate 9.4% to 13.4% reductions in HBD fatal and severe injury accidents, reflecting one-year estimated total decreases of between approximately 640 to 900 fewer such accidents than in the years preceding the new laws.

    - An estimated immediate 12.7% reduction in HBD fatal accidents, representing approximately 250 fewer fatal accidents, in the first year following the new laws.

    - Among nighttime fatal and severe-injury accidents, an estimated gradual 11.6% reduction, representing a one-year decrease of approximately 800 fewer such accidents than in the years preceding the new laws. This effect decreased to

10.7% or 740 fewer accidents when adjusted for historical trends in the number of licensed drivers.

- Among accidents occurring between 2 and 3 a.m., there was a small initial decrease of approximately 8 fatal and injury accidents per month, estimated to gradually increase to a maximum of 15.5% fewer accidents than in the years preceding the new laws. Among fatal accidents of this type, there was marginal evidence of an immediate 12.5% decrease, amounting to approximately 30 fewer fatal accidents between 2 and 3 a.m. in the first year following implementation of the new laws.

- Among fatal and total-injury single-vehicle nighttime accidents involving male drivers, there was an initial decrease in accidents of less than 1%, gradually increasing to a maximum of 10.1%, fewer accidents per month than in the years preceding the new laws.

<u>Intervention Effects of the Laws on DUI Arrests</u>

- While DUI arrests did not significantly decline following implementation of the 0.08% BAC law, they showed a statistically significant decline following the APS legislation, representing a 4.0% decrease from the preintervention mean.

<u>Impact of the APS Media Campaign on Regional HBD Accidents</u>

- Fatal and injury HBD accidents in four counties (Los Angeles, San Diego, San Francisco, and Sacramento) which were targeted for focused media campaigning declined significantly relative to the rest of the state following the campaign.

Conclusions

- This study demonstrated evidence of a significant general deterrent effect associated with the implementation of an administrative per se license suspension law in California and somewhat less support for such an effect associated with California's 0.08% BAC per se limit law. Larger proportions of the observed accident reductions were associated with the timing of the APS law than with the lowering of the illegal per se limit. Some, but not all, of these reductions were statistically significant, even after accounting for additional socioeconomic factors shown to contribute to the consistent downward trend observed in both the alcohol- and nonalcohol-related accident series. There was suggestive evidence that the media campaign slightly enhanced the effectiveness of the APS law.

- The magnitude and significance of the general deterrent effects of California's APS law are fairly consistent with those shown by evaluations in other states. This is in spite of: (1) the imposition of APS in California at a time when DUI-related accidents were already exhibiting a long term downward trend, and, (2) the fact that California has historically had a strong DUI enforcement /sanction program as measured by DUI arrest rates and the use of post-conviction license suspensions and mandatory alcohol treatment programs.

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INTRODUCTION

Relatively successful attempts were made during the 1980s to reduce the incidence of driving under the influence (DUI) in California through a series of laws which increased the likelihood and severity of punishment for a DUI offense (Helander, 1986; Rogers & Schoenig, 1994; Tashima & Helander, 1992). In spite of these advances, however, drunk driving continued to present a major social, legal, public health, and safety problem as reflected by over 2,500 fatalities and over 63,000 injuries in alcohol-related accidents in the state during 1989.

Although California DUI law has been strengthened in recent years in terms of punishment severity, research (Helander, 1986; Tashima & Helander, 1992, 1994, 1995) has consistently revealed conviction rates and time lags between DUI arrest and conviction which attenuate the potential for DUI punishment being perceived as either "certain" or "swift." Tashima and Helander (1995) documented conviction rates among California's larger counties ranging from only 57.8% to 83.3% for 1992 (the most current year for which actual rates are available). Their data revealed that California's largest county, Los Angeles, which accounted for over 25% of all DUI arrests in the state, had a DUI conviction rate of only 67.8%, and that the statewide average DUI conviction rate for that year was only slightly higher, at 70.0%. In the same report, Tashima and Helander showed a statewide average time lag of 2.6 months from DUI arrest to conviction and another 2.9 months from conviction to update of the Department of Motor Vehicles (DMV) driver record database. These time lags between arrest, conviction, and potential postconviction license actions cannot help but diminish any deterrent effect of DUI law.

According to deterrence theory (as presented in Ross, 1982, 1992), the behavior of the "target population of all potential drunk drivers" may be affected through the function of "general" deterrence. In theory, general deterrence is achieved when potential offenders suppress proscribed behavior upon perceiving it as likely to result in swift, certain, and severe punishment. This perceived threat of punishment can operate to deter potential drinking drivers from driving drunk even without having experienced the punishment first-hand. Administrative per se (APS) laws, which provide for the relatively swift, certain, and severe punishment of "on the spot" license suspension by law enforcement at the time of arrest, offer hopes of producing just such a general deterrent effect. General deterrence may be measured by reductions in alcohol-related driving incidents such as alcohol-involved accidents or, with considerably less confidence, DUI arrests. DUI arrests are less indicative of general deterrence because they are susceptible to changes in the allocation of law enforcement resources. Traffic accidents, particularly fatal accidents, are a more reliable indicator of a general deterrent effect because they occur relatively independently of changes in law enforcement levels.

Specific, or "special," deterrence refers to the impact of the punishment experience on sensitizing convicted offenders to the threat of further punishment for committing a similar crime in the future (Ross, 1992). Such deterrence, like rehabilitation, is measured by diminished recidivism. Increases in punishment severity, while possibly producing a specific deterrent effect, can be expected to have little general deterrent impact since the punitive effects are not experienced by offenders who are not apprehended (Ross, 1992). As stated by Ross (1982), increases in the severity of punishment alone cannot be expected to have a general deterrent effect unless accompanied by increases in the actual or perceived certainty and swiftness of punishment.

An extremely low probability of apprehension for a DUI offense, estimated to be approximately 1 out of 118 drunk driving incidents in California (Helander, 1992), combined with less than a 100% conviction rate for those who are apprehended, results in a low probability or "certainty" of punishment for the offense of DUI. APS laws were developed out of the need to address these problems.

On July 1,1990, California became the 28th state to implement an APS law. Senate Bill (SB) 1623-Lockyer allowed arrested drunk drivers with a blood alcohol concentration (BAC) of 0.10% or greater (changed very shortly thereafter to 0.08% or greater with passage of SB 1150-Lockyer) to be suspended in a separate, more timely, civil action independent of the adjudication of, and sanctions for, the criminal DUI charge. Implementation of the APS license suspension law (California Vehicle Code §13353 and §13353.2) has increased the overall number of license suspensions in California by orders of magnitude, addressing both the deficiencies in speed and in probability of punishment under California's earlier DUI laws.

Just six months prior to implementation of the APS law, in separate legislation (SB 408-Leonard), California reduced its "illegal per se" BAC level from 0.10% to 0.08%. It was expected that APS actions, and conceivably the closely timed reduction in the illegal per se level, would substantially increase the perceived certainty of punishment for a DUI offense, and therefore have a considerable traffic safety impact.

Effectiveness of License Suspension

Studies have consistently shown that post-conviction driver license suspension and revocation actions are among the most effective countermeasures in reducing fatal, injury, and total accidents (Sadler & Perrine, 1984; Tashima & Marelich, 1989; Tashima & Peck, 1986). Despite the fact that many suspended drivers continue to drive, there is consistent evidence provided by their subsequent driving histories that they either do so more carefully or drive less often (Ross & Gonzales, 1988; Sadler, Perrine, & Peck, 1991; Williams, Hagen, & McConnell, 1984).

When imposed in addition to jail and/or fine, such license actions following a DUI conviction have been shown to have a greater traffic safety impact among DUI offenders than does imposition of jail and/or fine alone (Tashima, Marowitz, DeYoung, & Helander, 1993). Furthermore, these specific deterrent effects last well beyond the actual suspension term lengths (Hagen, 1978). License suspension, even when imposed by the court and even after the typical lengthy delay between arrest and conviction, has proven to be an effective sanction in reducing total accidents (Sadler et al., 1991).

Addressing the effectiveness of court-ordered licensing actions, Williams et al. (1984) stated:

> "To date, the most effective method of impacting a multiple DUI offender's subsequent driving record has been the use of mandatory

> licensing action (suspension or revocation). It would appear the risk level of first offenders merits such a licensing action. However, from a practical standpoint, mandatory license controls for first offenders would be expected to grossly impair the process of adjudicating the DUI offense. The number of dismissals, requests for trials by jury, and the incidence of plea bargaining abuse would be expected to escalate."

Administrative license actions circumvent many of the problems associated with post-conviction imposition of license actions, as cited by Williams et al., because the action is summarily imposed in a civil track separate from the criminal adjudication of the case.

Administrative license action laws serve to effect immediate and certain sanctions against the drunk driver, adding a measure of certitude, immediacy, and rigor not previously found in the DUI countermeasure system, but crucial to the success of deterrence. Swifter and more certain imposition of administrative license actions would, as Ross (1982) claimed, likely augment existing DUI laws to produce a greater general deterrent effect. Since they require demonstrating only a "preponderance of evidence" common to civil actions, as opposed to "proof beyond a reasonable doubt" as in criminal prosecutions, APS laws are more consistently imposed, providing greater certainty of punishment. Ross (1992) suggests that administrative license actions serve to incapacitate offenders by removing their driving privilege as effectively as does criminal incarceration as it is currently imposed.

In addition to their potential for providing effective general deterrence, APS laws have been shown to be cost-beneficial (Lacey, Jones, & Stewart, 1991) and are generally supported by law enforcement, the courts and other personnel responsible for administering the suspensions (Stewart, Gruenewald, & Roth, 1989; Vingilis, Blefgen, Lei, Sykora, & Mann, 1988), thereby providing greater assurance that they will be maximally applied. Consequently, one would expect measurable increases in the general deterrent effect of DUI law subsequent to the enactment of an APS law.

APS Laws in Other States

There are numerous reports lauding such success in other states. The first states to introduce administrative license action laws, beginning with Minnesota in 1976, tended to be those with the most acute DUI problems, as measured by a high incidence of alcohol-related traffic accidents (Feimer, 1987). Perhaps in part because of the severity of the problem, related weaknesses in the overall DUI control infrastructure in many of these states, and variations in study designs, most assessments have found APS laws to be fairly effective from a general deterrence perspective. In contrast, California's APS law was introduced years later than those in most of these other states. Furthermore, as mentioned above, numerous other countermeasures intended to strengthen the DUI laws were enacted in California during the period of time that these other states were introducing administrative license suspension laws. Among these countermeasures were mandatory postconviction license actions which resulted in approximately 73% of convicted DUI offenders having their driver licenses restricted, suspended, or revoked in California during 1989—one year prior to the APS license suspension law.

More specifically, with varying degree of control for extraneous influences, many studies from other states have provided empirical evidence that administrative license actions contribute to reducing alcohol-related accidents beyond that expected from the prevalent downward trend in such accidents that has persisted over the past decade in this country (and, in fact, worldwide according to the Transportation Research Board, 1994). Significant accident reductions were found to be associated with implementation of preconviction administrative license actions in Minnesota (Cleary & Rodgers, 1986; Ross, 1991), New Mexico, and Delaware (Ross, 1991), and also in Wisconsin, even in the presence of judicial revocations already being imposed (Blomberg, Preusser, & Ulmer, 1987). Subsequent to North Dakota's administrative license withdrawal law, significant decreases in traffic fatalities were obtained with the largest decreases occurring during "peak" alcohol periods, leading to stronger evidence that the effects were associated with reductions in alcohol involvements (McDonald et al., 1987). Ross (1987) attributed decreases in alcohol-involved accidents in New Mexico to that state's administrative suspension law despite negligible efforts to publicize the deterrent threat. Such an effect may have been possible because New Mexico has one of the highest alcohol fatality rates in the nation.

Declines in alcohol-related accidents were also found subsequent to a law imposed in Oregon requiring preconviction administrative license actions. However, that same law also introduced post-conviction mandatory jail terms and alcohol-program completion, making the ensuing decreases less specifically attributable to the administrative remediation (Jones, 1989). After accounting for declines associated with economic trends, Muller (1989) found a 9% drop in total fatal accidents in Oklahoma over a two year period subsequent to two laws introducing an illegal per se limit and administrative license withdrawal. Lacey, Stewart, Marchetti, and Jones (1990) found a 12% decrease in alcohol-involved and nighttime crashes in Nevada after implementation of an information campaign publicizing the administrative license suspension law.

There are considerable differences in the length and scope of the administrative actions from state to state and internationally. However, even where only short suspension terms are imposed, the laws have generally been found to be associated with alcohol-related accident reductions. Such was the case in North Carolina where only a brief 10-day suspension term is imposed (Lacey, Stewart, & Rodgman, 1984) and in Ontario, Canada where offenders are suspended for only 12-hours (Vingilis et al., 1988).

Specific deterrence effects, measured as significant decreases in DUI recidivism rates, were obtained among offenders in Louisiana and North Dakota, and in decreased recidivism for other traffic offenses among offenders in Mississippi, subsequent to administrative license suspension laws (Stewart et al., 1989).

Perhaps the strongest support for the overall deterrent impact of administrative license action laws comes from studies which showed such laws in various states to be more effective than other court adjudicated measures imposed (Klein, 1989; Wagenaar, Zobeck, Williams, & Hingson, 1995; Zador, Lund, Fields, & Weinberg, 1988). In a multi-state comparison, Klein found a 7% median drop in single vehicle nighttime crashes (serving as a proxy for alcohol-involvement) subsequent to administrative license action laws, compared to a 5% median drop associated with mandatory court suspension/revocation laws. These decreases persisted even after controlling for the general effects of unemployment.

Zador et al. (1988) compared laws establishing illegal per se limits, administrative license actions, and mandatory jail in the 48 contiguous states and found a 9% reduction in nighttime fatal accidents in states with administrative license action laws, as compared to states with no such law. Except by pairing states as a control for other trends affecting accidents, this study did not empirically control for underlying economic trends during the period of study. However, as noted by Klein (1989), the 9% reduction cited here may be somewhat inflated because, in an effort to avoid the influence of helmet laws, Zador and his associates excluded accidents which had involved motorcycles, thereby excluding a group historically resistant to DUI (or other) legislative efforts.

Wagenaar et al. (1995) recently conducted a meta-analysis of 125 empirical general deterrence evaluations, comparing 12 common DUI policies throughout the United States, that showed administrative license suspension and illegal per se policies to be most consistently associated with reduced injuries and fatalities.

The California APS License Suspension Law

To describe its provisions briefly, the California APS law requires DMV to suspend or revoke the driving privilege of persons who are arrested for driving with a BAC of 0.08% or more or who refuse or fail to complete a chemical test upon arrest. As noted, this administrative action requires no DUI conviction and is independent of any criminal penalties imposed in court upon conviction of the DUI offense. The offender's driver license is seized by the arresting officer upon making the arrest. Due process of law is allowed by the issuance of a 30-day (reduced from 45 days beginning July 1, 1993 by Assembly Bill 3580-Farr) temporary license intended to provide the driver with sufficient time to challenge the suspension through DMV administrative review.

Under California's APS law, when a driver submits to and "fails" a BAC test and has no prior DUI convictions (within seven years), a 4-month license suspension is imposed. Following 30 days of "hard" suspension (and providing they first demonstrate proof of insurance, show proof of enrollment in an alcohol treatment program, and pay all penalty fees), the law provides for such drivers to obtain either a 60-day restricted license to drive to and from an alcohol treatment program, or (as of January 1, 1995, after the time period assessed in this evaluation) a 5-month restricted license which also allows driving to, from, and during the course of employment. A 1-year suspension is imposed on drivers having one or more prior DUI convictions within seven years, with no provision for a restricted license. If an arrested DUI offender refuses a BAC test, the term of license action imposed is one year for a first offense, two years for a second offense, or three years for a third or subsequent offense (within seven years). There are no provisions for license restriction following a BAC test refusal.

To prevent undue hardship, a commercial driver (having no prior DUI convictions) arrested in a noncommercial vehicle is allowed to drive to, from, and during the course of employment for a 4-month period following a 30-day "hard" suspension.

The 0.08% BAC Per Se Limit Law

Before the advent of chemical tests for alcohol, drunk driving was defined presumptively in terms of evidence of intoxication observable to a law enforcement officer. As BAC testing gained acceptance as an evidentiary tool, relatively high presumptive standards were established beyond which virtually any driver would clearly be considered to be impaired. Furthermore, as evidence on the relationship between lower BAC levels and driving impairment accumulated, many states lowered their presumptive BAC limits and some states began enacting per se thresholds. In 1982, California enacted a 0.10% BAC per se limit law while still maintaining a presumptive limit albeit somewhat reduced from its former 0.15% to 0.10% (Assembly Bill 7-Hart).

In a time series evaluation of the 1982 California legislation, Rogers and Schoenig (1994) found that the introduction of the 0.10% per se level and other legislative reforms introduced at that same time resulted in a significant temporary reduction in fatal and injury alcohol related accident rates, with somewhat more pronounced effects among injury accidents.

Zador et al. (1988) have concluded that per se laws, in addition to APS laws and mandatory jail or community service sentences, have been highly instrumental in producing reductions in fatal accidents nationwide.

A meta-analysis of studies evaluating the effects of alcohol on driving related tasks, by Moskowitz and Robinson (1988), found that most driving related tasks were impaired with BACs as low as 0.05%, and the majority of such skills were impaired before reaching 0.08%. The reader is referred to Cleary (1994) for a concise, comprehensive summary of the findings of that meta-analysis and other policy considerations concerning a reduced per se BAC limit.

Recent studies measuring impairment at different BAC levels have demonstrated that accident responsibility increases as BAC level increases (Terhune, 1982; Terhune et al., 1992). In a study of the relationship between alcohol concentration levels and accident risk, Zador (1991) found single vehicle crash drivers with BACs as low as 0.05% to 0.09% to have a fatality risk some nine times greater than non-drinking drivers. Given this kind of evidence of impairment at even low levels of BAC, on January 1, 1990, just six months before imposing the APS law, California became only the fourth state behind Oregon, Utah, and Maine to lower the illegal per se limit to 0.08% BAC.

In an early study of the impact of California's 0.08% law, commissioned by National Highway Traffic Safety Administration (NHTSA), Research and Evaluation Associates (1991) reported a 12% decline in the number of alcohol-related fatalities following the law's implementation, with no such drop in nonalcohol accidents. Similary, the study also found no decline in either alcohol-related or nonalcohol-related accidents following implementation of the APS law, just six months later. In a separate assessment of had-been-drinking (HBD) accidents in the state, with the exception of *increases* in two regional study sites, they found no effect of either law. The authors suggested that the increases in the HBD series may have resulted from an increased tendency by officers to charge a driver with an alcohol offense following passage of the new laws. However, no data were provided to support that conjecture.

In a subsequent assessment of the effectiveness of a reduced BAC level, NHTSA (1994) compared the percent change in post-0.08% law fatal accidents to the pre-0.08% accident rate across six interrelated alcohol surrogate categories in the five states which currently have such a limit. Of the 30 possible comparisons, 9 yielded statistically significant decreases. Sixteen of the remaining 21 comparisons resulted in nonsignificant decreases and the remaining 5 yielded nonsignificant increases in alcohol-related fatal accidents. In California, only one category proved to be significant—"intoxicated (BAC > .10)" with only a 4% decrease compared to 11% and 31% drops in the same category in Oregon and Vermont, respectively.

At best, the preceding two studies of the deterrent impact of .08% BAC laws must be regarded as equivocal, and neither study addressed the impact of .08% laws in the context of preconviction administrative license suspension authority. The present study employed a more comprehensive and sophisticated statistical analysis in attempting to assess the two laws. As explained later, the proximity at which the two laws were enacted in California presented some major methodological obstacles to disentangling their separate effects.

Media Campaign

After its introduction in July 1990, the APS license suspension law was given relatively little media attention compared to that which accompanied the 0.08% law six months earlier (Bloch, 1990; Research and Evaluation Associates, 1991). Relative to APS, the lowering of the BAC limit to 0.08% carried a higher degree of newsworthiness, because California was only the fourth state to have such a low BAC limit, and also because it potentially would impact every driver who drinks. It can reasonably be assumed that the goal of most drivers would be to avoid ever driving while being over an illegal limit in the first place. Consequently, for most drivers, their need to know that the per se BAC limit was being *reduced* would be greater than their need to know that upon apprehension, yet another severe (albeit swift) action would be taken. That is, APS may have been considered little more than yet another enhancement of already severe punishment for DUI. The media may have ignored this law because it was introduced so shortly after their recent focus on the 0.08% law, and it also would only affect apprehended drunk drivers.

Aware of the lack of media coverage about the APS law, the Office of Traffic Safety funded an organized public information and education campaign in the Spring of 1991 to focus media and public attention on the APS law. The campaign was launched at the start of the summer driving season just prior to Memorial Day 1991 and was most heavily promoted during the summer to Christmas period in 1991. The campaign consisted largely of newspaper articles, radio and television stories, and public-service messages "warning motorists about the newest weapon in the continuing war against drunk driving." Other efforts were also made to inform and involve in the campaign a wide range of statewide organizations including major employers, law enforcement, Mothers Against Drunk Driving (MADD) chapters, and county alcohol and drug program administrators (D. Saavedra, personal communication, November 8, 1993).

Although the campaign effort was distributed statewide, somewhat greater attention was focused on four target regions including Sacramento, Los Angeles, San Diego, and San Francisco counties (D. Saavedra, personal communication, December 17,1993).

Time Series Evaluations

The present study was designed to describe the functioning of the California APS system and to answer a number of questions related to the law's operational efficiency and effectiveness as a general deterrent to drunk driving. Because it succeeded the introduction of the 0.08% law by only six months, this study necessarily evaluates the immediate and long-term traffic safety impact of the APS law in conjunction with the lowered per se BAC limit. To this end, this study provides a series of comprehensive intervention time series evaluations of four traffic accident categories considered most likely to involve alcohol. These four categories are the police-designated category of accidents for which the reporting officer indicated that one or more of the involved drivers "had been drinking," and three categories of nighttime accidents: nighttime accidents of any type, those nighttime accidents involving only a single vehicle driven by a male, and nighttime accidents occurring between 2 and 3 a.m. (which is the first hour subsequent to California's 2 a.m. mandatory bar-closing time). Within these categories, three levels of severity will be assessed, including fatal accidents, fatal and severe injury accidents combined, and fatal and total injury accidents combined. These levels of severity are defensible on the basis that, "it is generally accepted that alcohol is frequently present in non-fatal crashes, with alcohol presence increasing as crash severity increases" (Hedlund, 1994). Therefore, somewhat greater importance will be ascribed to the outcome resulting from the analyses of each category of fatal-only accidents.

In a separate time series analysis, DUI arrests are also assessed over the same time period. In this analysis total arrests are included as a control for changes which may have occurred in the level of overall enforcement activities and, indirectly, of crime in general. Although somewhat open to interpretation, this analysis represents the most direct measure of the immediate effect of the laws. The interpretation of the results is not straightforward because a significant finding suggests only that DUI arrests decreased relative to other arrests. It cannot address the question of cause since arrest reductions could equally reflect either a transfer of resources from DUI to other crimes or an actual decrease in the number of drunk drivers.

The analytical approach used made it possible to assess over a long time period both the type of onset (either abrupt or gradual) and the duration of any general deterrent effect of the two laws. Furthermore, this study incorporates the use of a nonalcohol-related covariate series in each analysis to produce a more valid assessment of law-mediated effects on the accident series by adjusting for concomitant trends in the control series. In addition to the nonalcohol control series, which is always included, other explanatory variables accounting for statewide economic or traffic exposure trends are included in only those analyses for which it could be established that they potentially explain variation in the accident series that would otherwise be considered error. There are four of these explanatory variables considered in the analysis of all four accident types: the interpolated number of drivers licensed in California, gasoline sales in the state, personal income, and unemployment figures.

These variables may disproportionately effect alcohol- and nonalcohol-involved accidents. This view was expressed by Hedlund et al. (1984), who demonstrated that the state of the economy is among the strongest factors influencing vehicle miles traveled. They argued that people tend to travel less, particularly on discretionary trips, when the economy worsens. The existence and impact of any changes in the volume of drivers, overall vehicle miles traveled, unemployment rates, and these other indices of economic standing are fully explored.

Since it was quite likely that a larger general deterrent effect would be revealed in those regions of the state that received greater media coverage of the new laws, monthly alcohol-related accident figures for the four major counties, which were targeted for more intensive campaigning, are combined to form categories of HBD fatal, HBD fatal and severe injury, and HBD fatal and injury accidents, and separate analyses of these combined regions are performed.

METHOD

Intervention Time Series Analyses

Intervention time series analyses were used for assessing the long term impact of the two DUI laws implemented in 1990. An intervention time series analysis involves repeated measurement of a variable both before and after the occurrence of an intervention (in this case the DUI laws) which is expected to interrupt the trend or pattern in that variable. Since traffic accidents usually exhibit patterns of correlation in the form of longitudinal trends and seasonal cycles, typical least-squares curve-fitting methods are not appropriate, and because they do not account for the expected autocorrelations which result from systematic changes in driving patterns (from one monthly observation to the next) or from seasonal changes (across a longer time interval) (McDowall, McCleary, Meidinger, & Hay, 1980). The method of time series modeling used in these analyses is therefore based on the Box and Jenkins (1970) autoregressive, integrated, moving average (ARIMA) technique as presented in application to behavioral data in McCleary and Hay (1982). Among others, Ross (1982; 1992) discussed several applications of intervention ARIMA modeling in his assessments of DUI legislative reforms. Cleary and Rodgers (1986) described ARIMA time series modeling as they applied it in assessing changes in the DUI laws of Minnesota. The technique of incorporating a "covariate" series as a control was described by Krishnamurti, Narayan, and Raj (1986) and was previously applied by Rogers and Schoenig (1994) to evaluate DUI legislative reforms in California.

Design

Aggregated statewide monthly alcohol-related accidents from January 1985 through December 1993 were analyzed through ARIMA time series models developed to detect intervention effects associated with the January 1, 1990 implementation date of the 0.08% illegal per se BAC limit, and the July 1,1990 effective date of the APS law (just six months later). This provided 60 months of data prior to, and 48 months after, the 0.08% law and 66 months of data prior to, and 42 months of data after, the APS law. From a technical standpoint, the close temporal contiguity of the two interventions (implemented only six months apart) creates a sensitivity problem in detecting intervention effects uniquely attributable to either of the two laws. That is, effects which appear to be associated with the APS law might be more accurately attributable to lingering effects of the 0.08% BAC law. Conversely, the earlier effects associated with the implementation of the 0.08% law might actually be reflecting anticipatory effects of the upcoming APS law. It is not possible to disentangle the two effects since there would be, at most, only six months of potentially "uncontaminated" post-data for use in evaluating the impact of the 0.08% law before the APS law was enacted. Furthermore, any residual or delayed effect of the 0.08% law would carry over into the subsequent APS law period potentially contaminating the APS post-period in its entirety. Finally, introduction of the APS law may have operated to sensitize people to the new lower 0.08% BAC level resulting in a synergistic effect of the two laws manifest in the post-APS period. As such, it is best to view the two interventions as being interrelated rather than as independent operators with unique effects.

Accident data

All of the accident data assessed in this study were obtained from the Statewide Integrated Traffic Records System (SWITRS) maintained by the California Highway Patrol (CHP). This system is an automated file containing counts of all traffic accidents reported by law enforcement organizations throughout the state.

Separate intervention time series analyses were performed for one direct alcohol-accident measure and three surrogate alcohol-accident measures, each paired with an appropriate low- or nonalcohol accident (control) series. In each case, the control series served to remove extraneous sources of variance from the alcohol accident series. The rationale for using a low- or nonalcohol accident series as a control in judging the impact of the DUI laws is relatively straightforward: any impact of the new laws would be expected to be confined to accidents involving alcohol; thus a more valid assessment of law-mediated effects on accidents is produced by adjusting for concomitant trends in the control series. The inclusion of the control series helps prevent attributing significance to the DUI legislation that should, more accurately, be attributed to some independent, but coincident, latent event. Since the control and dependent series were presumably independent, but subject to the same or relatively similar influences, the raw control series was used in the model directly without any preliminary adjustment for underlying monthly or seasonal patterns.

The four major dependent variable accident categories were:

- "Had-been-drinking" (HBD) accidents, fitting non-HBD accidents as the control - HBD accidents were those for which the reporting officer detected and reported any evidence of alcohol consumption in an involved driver. Non-HBD accidents were all other reported accidents, including those nighttime, bar-closing-hour, and single-vehicle nighttime male-driver accidents for which alcohol was not judged to have been a factor. To the extent that the reporting officer's judgment was accurate and the record complete, these non-HBD accidents should represent a defensible control series.

- Nighttime accidents, fitting daytime accidents as the control - Nighttime accidents comprised all those reported as occurring between 8 p.m. and 3:59 a.m. Reported accidents occurring between 6 a.m. and 1:59 p.m. were classified as daytime accidents. Drunk driving most commonly occurs during nighttime hours both in California (CHP, 1987, 1994) and in other states (Heeren, Smith, Morelock, & Hingson, 1985; Williams & Wells, 1993). According to CHP annual statistics obtained for 1987 through 1989 (CHP, 1988; 1989; 1990) and work done by Maxwell (1983), time of day represents a better measure for distinguishing between alcohol involvement and noninvolvement in crashes than does day of the week. Times composing the "nighttime" and "daytime" categories were selected to maximize or minimize the number of HBD incidents included in the time segments, respectively. The CHP accident figures showed that in 1989 (the year just prior to the new laws) 69.7% of accidents during these nighttime hours were HBD accidents compared to only 15% during the daytime hours.

- Single-vehicle nighttime accidents involving male drivers (SVNM), fitting multiple vehicle daytime (MVD) accidents (involving either male or female drivers) as the control - SVNM accidents, particularly those occurring very late at night, represent a small but highly alcohol-involved subset of nighttime accidents (Douglass & Filkins, 1974; Maxwell, 1983). In a recent comparison of multiple vehicle and single vehicle fatalities from 1980 through 1989 in Northern Sweden, Öström and Eriksson (1993) found that 58% of the single vehicle crash victims were intoxicated at the time of the crash, compared with only 10% of those in multiple vehicle crashes. They found 52% of the single vehicle crashes occurred between 9 p.m. and 6 a.m. compared to only 11% of the multiple vehicle fatalities, and that 84% of the single vehicle crash victims were male compared to 68% male victims in multiple vehicle crashes.

  Like the nighttime series, the SVNM series here included accidents occurring from 8 p.m. to 3:59 a.m. The MVD series consisted of accidents involving more than one motor vehicle and, like the daytime category, were limited to accidents occurring from 6 a.m. to 1:59 p.m. NHTSA (1992) reported that between 9 p.m. and 3 a.m. 75.8% of single vehicle accidents nationwide were alcohol related in 1992 compared to only 9% of multiple vehicle accidents occurring between 9 a.m. and noon.

- Accidents occurring between 2 and 3 a.m., fitting daytime accidents occurring between 10 and 11 a.m. as the control - While there are very few drivers on the road between 2 and 3 a.m., relative to other times of the day, it is the first hour following California's 2 a.m. mandatory bar-closing time. Consequently, it stands to reason that accidents occurring during this one-hour period are often associated with alcohol involvement. More so than at any other time of the day, more drivers during that one hour are likely to be driving drunk, having just left a bar upon its closing. In random stops of motorists in Minnesota, Foss, Voas, Beirness and Wolfe (1990) found that the proportion of drivers over the legal limit was about four times greater between 12:30 a.m. and 2:30 a.m. than between 10 p.m. and midnight. They also found that "the bar closing hour" (1 a.m. in Minnesota) coincided with the peak hour of impaired and intoxicated driving. Using California fatal accidents, Stein (1989) developed a model of the risk of being involved in a fatal accident with a drunk driver by time of day and found that between 1 and 3 a.m. sober drivers were between 20 and 100 times more at risk than at any of the lowest alcohol-involved hours of 6 a.m. to 4 p.m. CHP (1990) annual statistics show that for the single hour between 2 and 3 a.m. 78.2% of fatal accidents in California were alcohol related in 1989 compared to only 13.0% of daytime accidents occurring between 10 and 11 a.m. This hour was used as the control series in the analyses of these data because it represented the one hour of the day, across all days of the week, for which there were proportionately fewer HBD incidents than at any other time of day in 1988, the year from which this category was established as a control (CHP, 1989). In that year only 11.0% of 10 to 11 a.m. fatal accidents were alcohol-involved.

For each accident variable described above, three subsets of accident severity were assessed. They were: 1) fatal and total-injury accidents combined, 2) fatal and severe-injury accidents combined, and 3) fatal accidents alone. In accord with CHP classifications, a fatal accident was defined as a motor vehicle traffic accident which results in the death of one or more persons within thirty days of the incident (CHP, 1987). An injury accident was defined as a motor vehicle traffic accident which results in injury, but not death, to one or more persons. An "injury" could include a severe wound, other visible injuries, or simply a complaint of pain. A "severe"-injury accident was defined as one which prevents the injured party from walking, driving, or performing activities he or she was normally capable of before the accident. Since injury accidents do not include fatal accidents, the two categories could be summed to form one category of total fatal and injury accidents.

Each of these categories possesses varying degrees of statistical power and, as already suggested, association with alcohol involvement (Hedlund, 1994). While assessment of fatal accidents alone provides the least statistical power to detect an effect (due to the smaller sample size), fatal accidents are far less vulnerable than nonfatal accidents to reporting artifacts and biases, and are associated with a well documented overinvolvement of alcohol (Perrine, Peck, & Fell, 1988). The analysis of combined severe- injury and fatal accidents provided increased statistical power over that of fatal accidents alone by increasing the monthly counts. Furthermore, fatal and severe-injury incidents are a somewhat more specifically alcohol-related measure than are fatal and total-injury accidents, since alcohol involvement and accident severity are highly correlated and the total-injury category includes every conceivable level of "injury" including minor injuries and any "complaint of pain." The fatal and total-injury accidents provide the greatest statistical power but, as just described, are less directly associated with alcohol involvement and contain the largest measurement error due to the underestimate of alcohol.

Without exception, traffic accidents resulting in property damage only, or which involved either a pedestrian or bicyclist, were excluded from all analyses. Accidents involving property damage only are highly underreported and are less likely to involve alcohol, and while those involving pedestrians or bicyclists are also less likely to involve alcohol, they often result in severe injury. Furthermore, when they do involve alcohol, such accidents are as likely to have been caused by the impairment of the pedestrian or bicyclist as by the motor vehicle driver.

Separate time series analyses were performed for each of the subsets described above, resulting in a total of 12 separate assessments. By no means should these assessments be considered independent, however, but rather as overlapping surrogates for the true measure of concern—alcohol-involved accidents.

The combination of all of these accident measures were evaluated since no single one of them represents a perfect alcohol measure (nor do the control series represent only nonalcohol accidents). The nighttime series and their subsets (SVNM accidents, for example) contain obvious measurement error resulting from misclassification; some proportion of nighttime accidents will obviously not have involved alcohol while some proportion of daytime accidents will have been alcohol-involved. Waller (1971), moreover, has shown that even the most direct measure of alcohol involvement, the HBD characterization, is subjective and tends to be underreported. In comparing alcohol measures, Ross (1982) has suggested that analyses involving the nighttime surrogate series are potentially less biased than are analyses involving HBD accidents because they are not influenced by historical changes in police reporting and investigation procedures. Results obtained by Arstein-Kerslake and Peck (1985), Sadler and Perrine (1984), and Tashima and Peck (1986), however, suggest that the police report designations are superior, at least in California, from the standpoint of providing a more sensitive measure of the role of alcohol factors. Further evidence of the superiority of the HBD category over that of the nighttime category of accidents as an index of alcohol-relatedness in California was demonstrated by Peck (1993) who found a far greater correlation between prior DUI offense history and HBD accidents than between such prior history and nighttime accident involvement.

For the purposes of this study, it was thought that a stronger index of the effects of the laws on all alcohol-related accidents could be obtained by evaluating both the alcohol-specific measure of HBD accidents and a comprehensive set of surrogate measures. Although we have argued that the police HBD designation, at least in California, provides a more sensitive measure of alcohol involvement, a potential disadvantage is that this measure is subject to variations in accident reporting and police/forensic investigation policies. Any changes in the quality or accuracy of police accident reports subsequent to the law changes could introduce a bias. The surrogate measures are less subject to this source of bias but, as noted, suffer from other limitations.

It was recognized that a combination of measurement error and the extent to which the series were affected by unmeasured extraneous variables would undoubtedly leave some systematic variation uncontrolled in the dependent series even after accounting for the shared variation with the control series. Consequently, additional explanatory covariate series considered likely to exert a differential effect, such as those representing statewide economic or driving exposure trends, were considered for inclusion in each accident series analysis, but were actually included only if they significantly furthered error reduction.

Background variables

There were four additional explanatory series considered in the analysis of all four accident types. Such series were only considered for inclusion if there was no theoretical basis to expect that they would be impacted by the introduction of the two new laws but could potentially help reduce remaining error variation in the dependent series left unexplained by the control series. In this regard these series should operate much like a covariate would in other non-time-dependent quasi-experimental analyses. Ideally, the control series would adequately account for all of the nonalcohol variation in the dependent series with no need for including additional explanatory variables. As described above, however, it was known that none of the accident series used in this evaluation either as a dependent or control series met this ideal; none were considered purely alcohol or nonalcohol related. Consequently, to the extent that the control series could not amply account for shared nonalcohol variation, such variation might be included in the intervention effect. Then, in that case, introducing the additional explanatory series could actually lead to a more realistic reduction in the magnitude of the initially obtained intervention effect. On the other hand, to the extent that the additional explanatory variables were, themselves, affected by the interventions, (or by other exogenous variables causing a decrease in the covariate series, coincident with the effect of the interventions), some of the variance which should rightfully have been attributed to the interventions would shift to the covariate causing a reduction in the magnitude of the intervention effect. For these reasons, while the results obtained in conjunction with the added explanatory variables characterize the intervention impacts using a somewhat more conservative gauge, they should be interpreted with caution since they do not necessarily produce more accurate results than those obtained without the covariates.

A plot of each of the potential additional explanatory series is presented in Figure 1. The two legislation interventions are indicated in each plot with vertical lines. A 12-month moving average is also indicated in plots of those series containing substantial variation to highlight the overall series trend by removing the visual confusion in the month-to-month fluctuations. These explanatory series were:

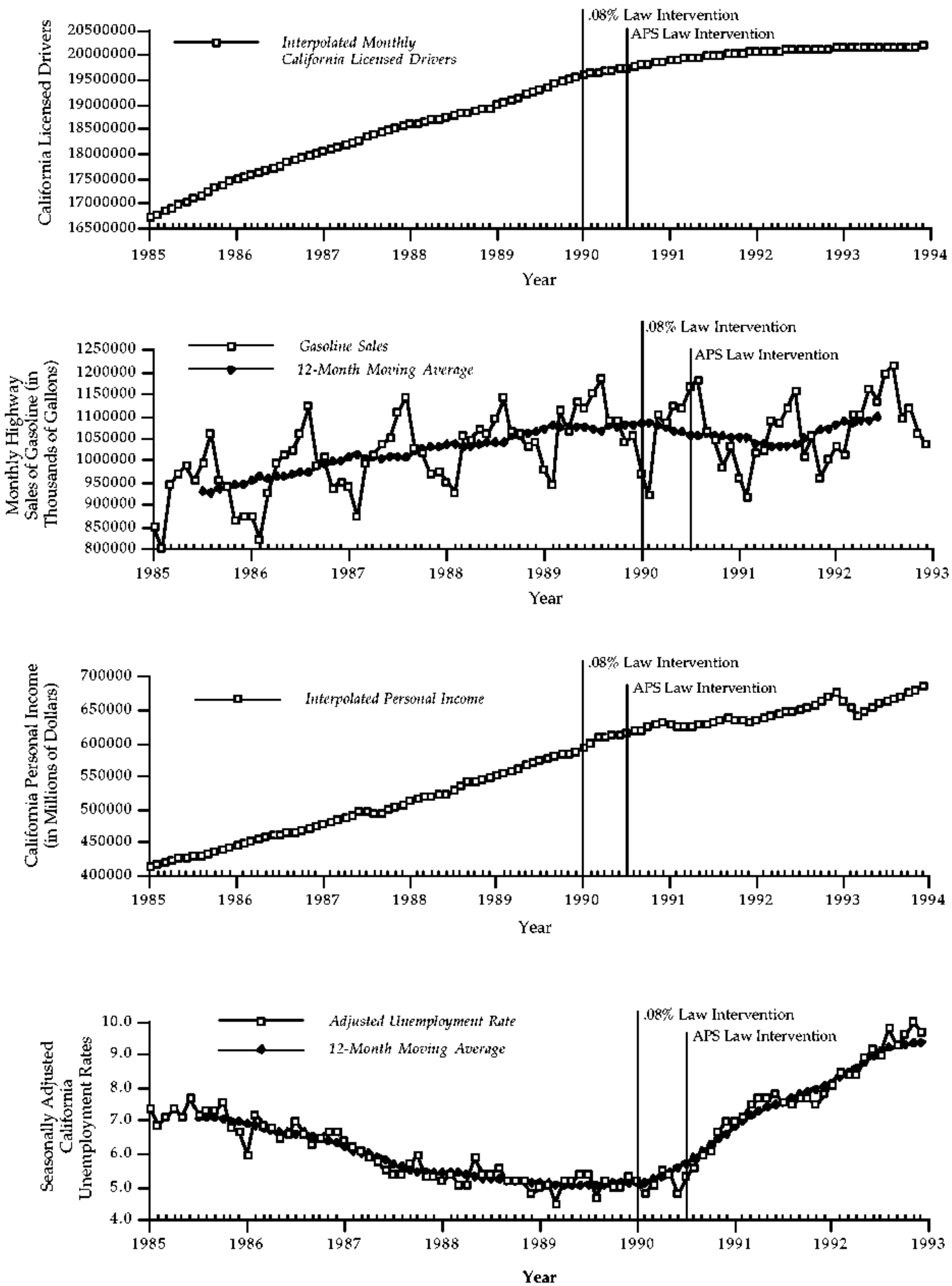

Figure 1. Exposure and economic-indicator covariate series.

- California licensed drivers - These data represent a measure of driving exposure. The annual figures for this series were provided by the Management Information Services Unit of the DMV. The number of licensed drivers in the state provides an index of the number of drivers on the road. However, the actual number of licensed drivers is only available as an annual total. Thus the monthly estimates were interpolated from the actual annual figures. Although the number of licensed drivers was considered an important exposure variable, it was not included systematically in the time series analyses (e.g., as a denominator in calculating an accident rate) because of the month-to-month inaccuracy and overall bias built in by the interpolation process.

- Monthly highway sales of gasoline (in thousands of gallons) - These data are also an important proxy measure of exposure. This series was obtained from the Federal Highway Administration (FHWA), Office of Highway Information Management. This series is estimated by subtracting nonhighway use estimates from total use estimates. For a more complete description of the development of this series, refer to the annual FHWA publications (e.g., FHWA, 1990).

- California personal income (in millions of dollars) - These data provide a measure of the economic trends in the state, which in turn is associated with discretionary travel. Figures in this series were interpolated from adjusted quarterly values obtained from the California Department of Finance[1].

- Seasonally adjusted California monthly unemployment rates - This is also a measure of the economic trends in the state and was also provided by the California Department of Finance.

Monthly estimates of vehicle miles traveled (VMT) are often used as a measure of driving exposure. However estimates of VMT are based on monthly volumes of highway gasoline sales, the measure which is used here directly. Monthly values for all of the covariates were scaled per 100,000 to accommodate the scaling requirements of the software package used in the analyses.

Analyses

All of the time series in this evaluation were analyzed using the BMDP-2T computer software package (Dixon, 1990). In each analysis, the backcasting method, a recursive procedure which is executed by "running" the ARIMA model backward in time, was used for estimating the model parameters.

For each analysis, an ARIMA structure was developed from a mathematical model which predicted the three nighttime or HBD series based on both "random shocks" and systematic patterns of autocorrelation present in the series. As noted, a control series was used to differentiate between alcohol-related accident effects and those consistent across both alcohol and nonalcohol accidents. The control series functioned much as a covariate functions in ordinary least squares regression analysis, to reduce unexplained variation and, ultimately, to strengthen the case for causality.

[1] A complete description of the methodology used for estimating the quarterly California personal income figures may be found in publication FR:7159B, obtained from the California Department of Finance.

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 < .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 <.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 ($p<.10$, 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.

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.

Table 2

Administrative Per Se Departmental Hearings and Percentage of Total Administrative Per Se Actions by Year

Type of APS hearing	Year 1990/91	1991/92	1992/93	1993/94	1994/95
Total hearings held	20,165	20,413	20,587	21,264	19,188
% of total APS actions	7.0%	8.6%	8.9%	10.1%	10.4%
Total BAC hearings	17,285	17,440	17,875	19,004	17,341
% total BAC APS actions	6.5%	7.2%	9.2%	9.7%	10.0%
Total BAC hearings upheld	15,212	15,374	14,700	13,723	14,065
% of total BAC hearings upheld	88.0%	88.2%	82.2%	72.2%	81.1%
Total BAC-refusal hearings	2,880	2,973	2,712	2,260	1,847
% total BAC-refusal APS actions	13.0%	14.5%	15.0%	14.9%	14.5%
Total BAC-refusal hearings upheld	2,424	2,444	2,220	1,758	1,335
% of BAC-refusal hearings upheld	84.2%	82.2%	81.9%	77.8%	72.3%

Long Term Impact on Statewide Alcohol-Related Accidents

The accident, arrest, and exogenous variable series represented throughout this report provide clear visual evidence of broad general changes beginning around, or even before, 1990 which resulted in a directional shift in many of the series presented. Among accidents, this is evidenced as a downward trend in *all types* of accidents portrayed, irrespective of their likely alcohol-involvement status. With few exceptions, these trends are fairly consistent throughout the time periods represented and across both alcohol and nonalcohol accident categories. Consequently, any impact of either alcohol law enacted in 1990 (first the 0.08% law and then the APS law, six months after) may be visually obscured by this overall predominant downward trend among all accident categories.

Because of this larger trend in all series, the reader should be cautious in attempting to identify any indication of an impact associated with the intervention points in the alcohol-related series alone, because the control series in each case show similar decreases. The consistency of the trends across series suggests that, for any given accident series, a large proportion, if not all, of the apparent decrease may be attributable to effects on accident rates in general, rather than effects specific to alcohol-involved incidents.

Background variable development

Plots of the potential covariate time series were presented in Figure 1 in the Methods Section above. As noted, in each of these series, as with all series presented in this report, the legislative implementation dates of January 1, 1990 (the 0.08% BAC operative date), and July 1, 1990 (the APS operative date), are each indicated with separate vertical lines. A 12-month[3] centered moving average (smoothed curve) is also indicated in each of these time series plots. The main power in displaying the moving average is that it provides a good measure of the trend in the data, averaging out the seasonal fluctuations represented in individual monthly values.

Visual inspection of the plots in Figure 1, (presented on page 20) provides clear evidence that 1990 marked a point of change in all of the potential covariate series. Among California licensed drivers, 1990 marks the beginning of a reduced increase in the number of licensed drivers in the state, with the plot having the appearance of a slight flattening out of the series. Similarly, late 1989 marks the beginning of declining gasoline sales, continuing until mid-1991, when gasoline sales again picked up.

Figure 1 also shows that early 1990 marked the beginning of increased personal income for those employed in California. This is in sharp contrast with the plot of seasonally adjusted unemployment rates which indicate that in 1990 unemployment reversed from a general downward trend to that of a rapidly increasing trend of rising unemployment which persists through the end of the series. Given that this reversal coincides with the timing of the implementation of the new DUI laws, it is especially noteworthy that this latter series, as shall be seen, produced no significant cross-correlations with any of the dependent accident series in this evaluation.

Results of the initial bivariate assessments for determining the optimal form of including each of the potential covariates are presented in Table 3. The values indicated in the table represent the number of months for which the respective covariate series should be lagged backward to maximize its predictive potential. Recall that this was determined by cross-correlating each covariate with the prewhitened dependent series. Although it had been initially considered for inclusion, these analyses revealed that the rate of unemployment did not significantly cross-correlate with any of the dependent variable series, and consequently it was neither included in Table 3 nor in any subsequent analyses. The rows with no entries in Table 3 indicate that none of the potential covariates was significantly cross-correlated with that particular dependent variable.

The subsequent bivariate analyses with the dependent accident series revealed that a number of the potential covariates shown in Table 3 did not uniquely account for the variance in the dependent series once entered in combination with the control series. To determine which of the covariates were subsequently excluded, compare those presented in Table 3 with those remaining in the following summaries of the time series model statistics. Presumably their nonsignificance indicates that these covariates provided no further refinement to the model beyond what was already accomplished

[3] The moving averages reported throughout this report were actually based on a 13 month interval due to the requirement of having an odd number of points to generate a centered moving average.

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

	Covariates[a]		
Accident series	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.

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

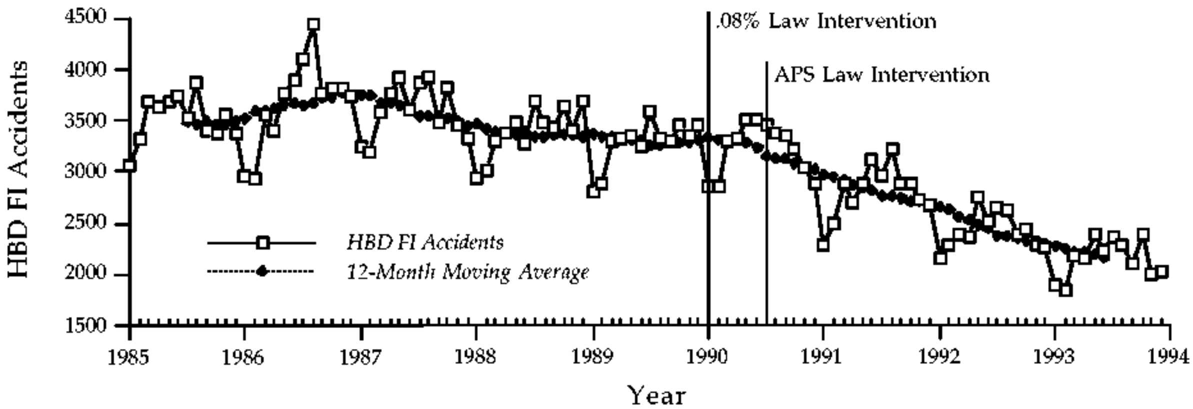

Figure 2.1. California "had-been-drinking" (HBD) fatal and injury (FI) accidents by month, 1985-1994.

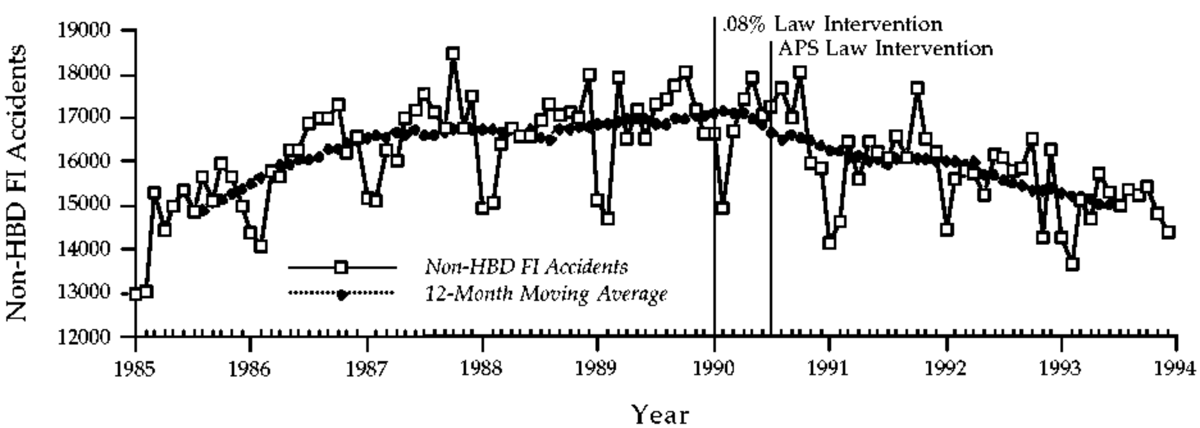

Figure 2.2. California non-"had-been-drinking" (Non-HBD) fatal and injury (FI) accidents by month, 1985-1994.

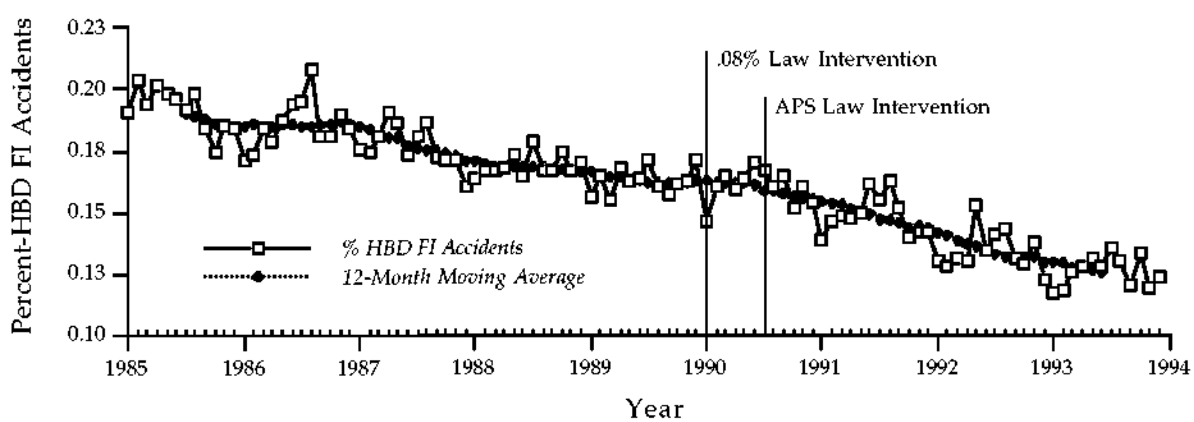

Figure 2.3. California "had-been-drinking" (HBD) fatal and injury (FI) accidents as a proportion of total fatal and injury accidents by month, 1985-1994.

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
		8	1	-.1143	-.23			
	APS intervention	ω	0	-.0126	-.37			
		8	1	1.042	21.21			
	Control	**P**	0	1.004	8.85			
	Noise	**e**	1	.7001	9.65			
		e	7	.3468	3.37			
		e	12	-.3396	-3.36			
		constant	0	-.0048	-2.66			
	.08 intervention	ω	0	-.0313	-.67	26	98	.003
		8	1	-.5382	-.44			
	APS intervention	ω	0	-.0488	-1.08			
		8	1	-.8016	-2.05			
	Control	**P**	0	.9673	8.45			
	Noise	**e**	1	.6716	8.67			
		e	7	.3484	3.38			
		e	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	**P**	0	.9126	8.00			
	Noise	**e**	1	.6854	9.20			
		e	7	.3234	3.28			
		e	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

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 co 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.

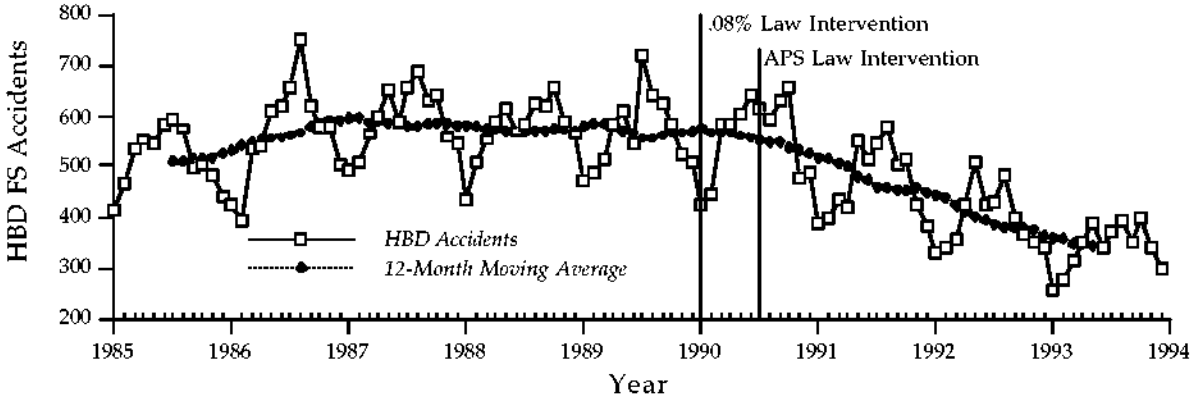

Figure 3.1. California "had-been-drinking" (HBD) fatal and severe-injury (FS) accidents by month, 1985-1994.

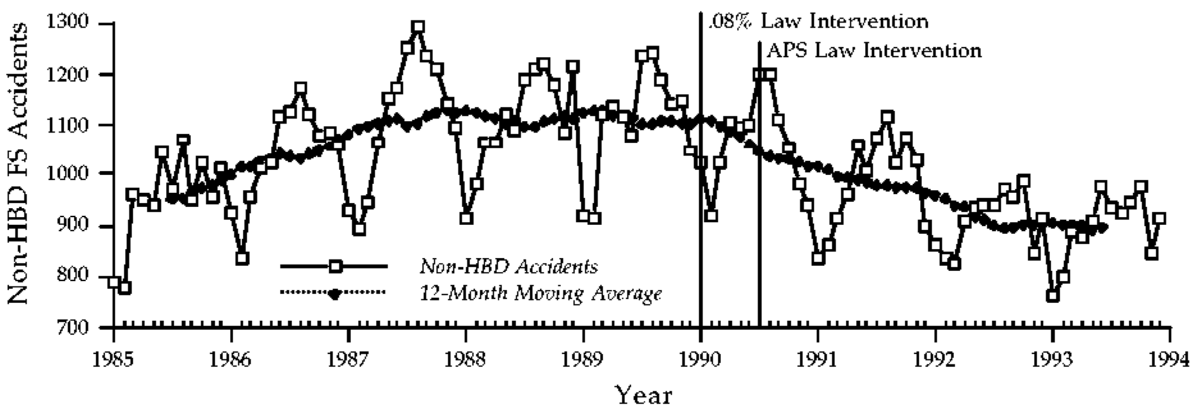

Figure 3.2. California non-"had-been-drinking" (Non-HBD) fatal and severe-injury (FS) accidents by month, 1985-1994.

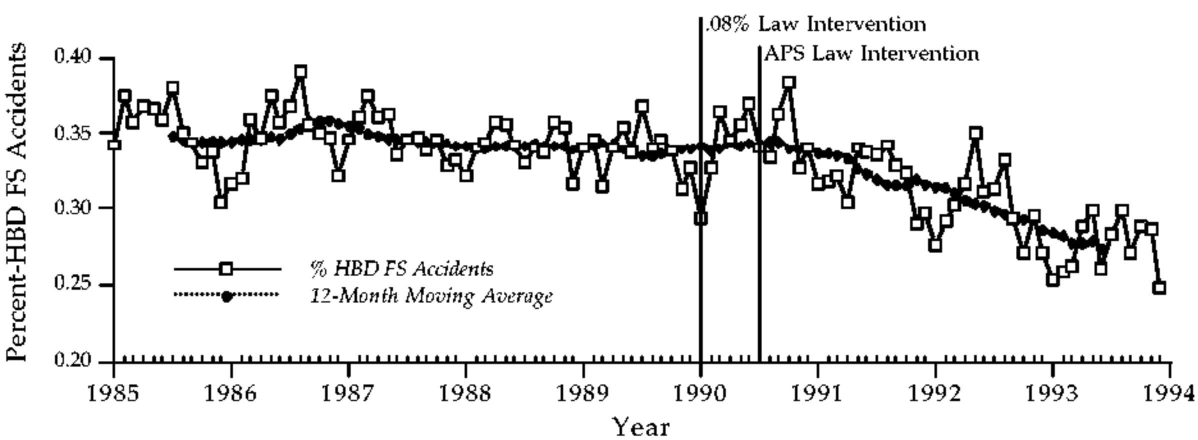

Figure 3.3. California "had-been-drinking" (HBD) fatal and severe-injury (FS) accidents as a proportion of total fatal and severe-injury accidents by month, 1985-1994.

Time series analysis. 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.

Intervention effects of the 0.08% law. 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-BQ (lag 25)	*df*	*RMS*[b]
Abrupt / temporary	.08 intervention	(0	0	-20.91	-2.01	29	100	1707
		5	l	1.045	102.75			
	APS intervention	(0	0	-18.41	-1.03			
		5	1	-.8408	-4.51			
	Control	P	0	.5315	62.69			
	Noise	e	1	-.3634	-3.75			
		e	3	-.2891	-2.90			
Abrupt / temporary with covariate	.08 intervention	(0	0	-13.89	-1.04	17	92	1521
		5	1	1.053	57.23			
	APS intervention	(0	0	-40.34	-.90			
		5	1	.4169	.49			
	Control	P	0	.5235	34.91			
	Licensed drivers	P	-6	183.8	2.22			
	Noise	e	3	-.3731	-3.80			
		e	12	-.3237	-3.36			
		ɸ	1	.4629	4.83			
Gradual/permanent	.08 intervention	(0	0	-18.80	-1.09	19	95	1566
		5	1	-.9307	-11.32			
	APS intervention	(0	0	-2.852	-1.97			
		5	1	1.008	42.03			
	Control	P	0	.5299	77.52			
	Noise	e	3	-.2583	-2.59			
		ɸ	1	.3586	3.69			
		ɸ	4	-.2817	-2.89			
Gradual/permanent with covariate	.08 intervention	(0	0	-5.513	-.24	19	92	1502
		5	1	-.9204	-1.40			
	APS intervention	(0	0	-3.061	-1.36			
		5	1	1.006	29.91			
	Control	P	0	.5254	40.67			
	Licensed drivers	P	-6	218.2	3.21			
	Noise	e	3	-.3328	-3.38			
		e	12	-.3070	-3.20			
		ɸ	1	.4226	4.38			
Abrupt / permanent	.08 intervention	(0	0	-6.473	-.21	17	97	1760
	APS intervention	(0	0	-75.35	-2.41			
	Control	p	0	.5295	45.42			
	Noise	e	3	-.3120	-3.02			
		ɸ	1	.5409	5.73			
		ɸ	4	-.1920	-1.83			
Abrupt / permanent with covariate	.08 intervention	(o	0	-12.24	-.41	14	94	1588
	APS intervention	(o	0	-53.10	-1.70			
	Control	p	0	.5158	27.35			
	Licensed drivers	p	-6	199.2	3.19			
	Noise	e	3	-.3896	-3.99			
		e	12	-.3604	-3.77			
		<$>	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

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 ($r<A$) 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.

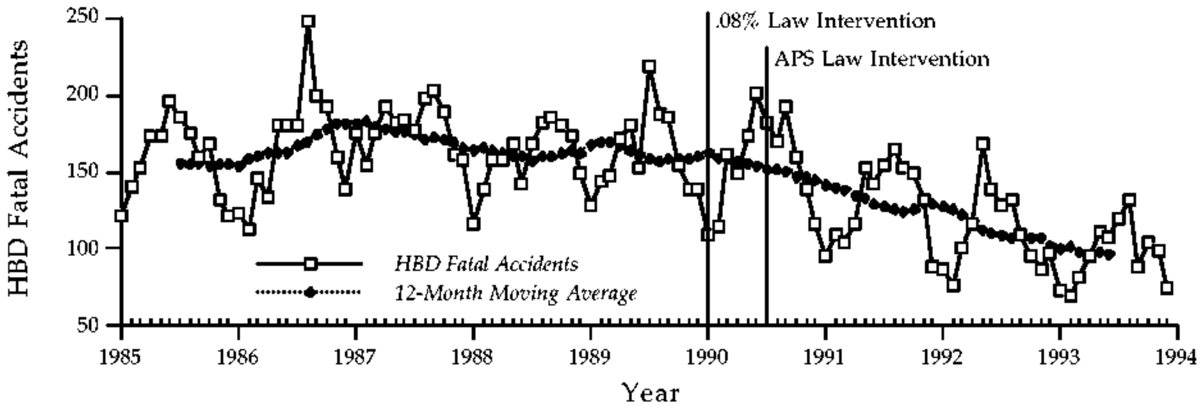

Figure 4.1. California "had-been-drinking" (HBD) fatal accidents by month, 1985-1994.

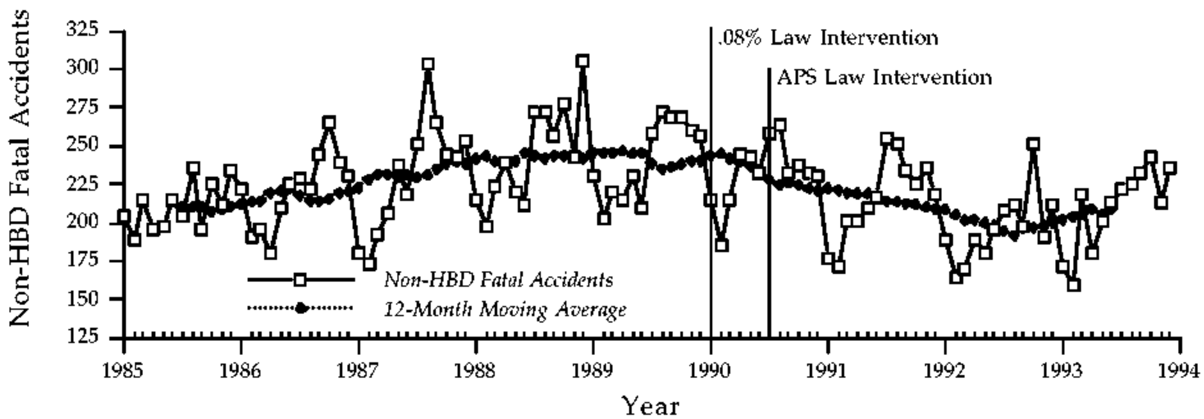

Figure 4.2. California non-"had-been-drinking" (Non-HBD) fatal accidents by month, 1985-1994.

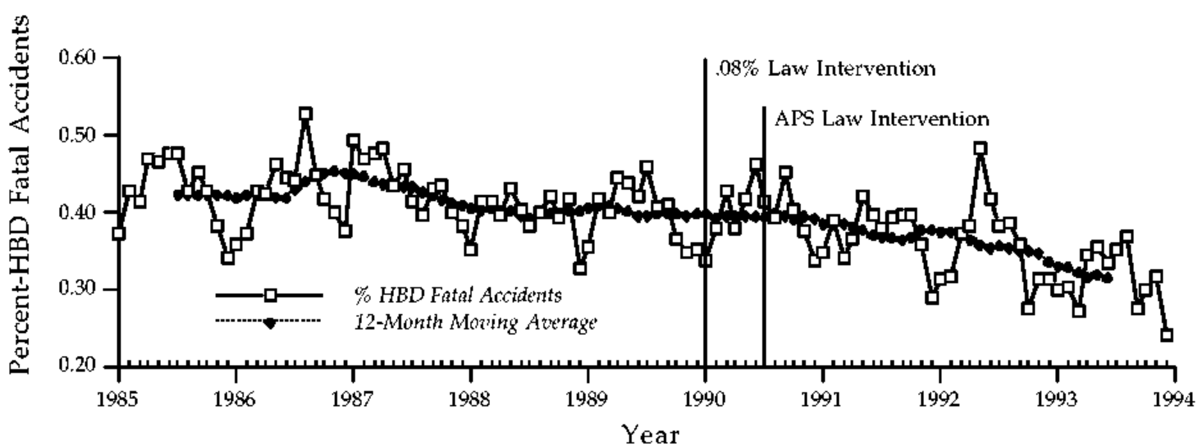

Figure 4.3. California "had-been-drinking" (HBD) fatal accidents as a proportion of total fatal accidents by month, 1985-1994.

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

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.

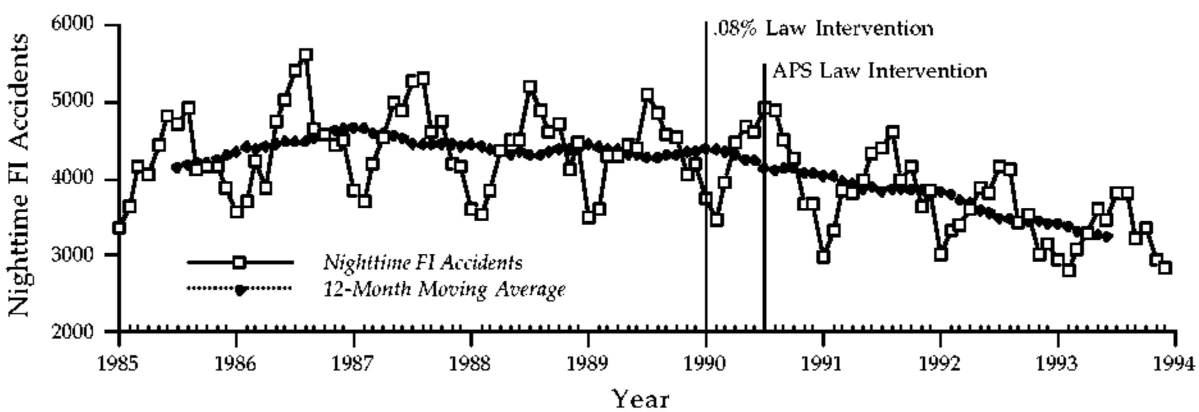

Figure 5.1. California nighttime fatal and injury (FI) accidents by month, 1985-1994.

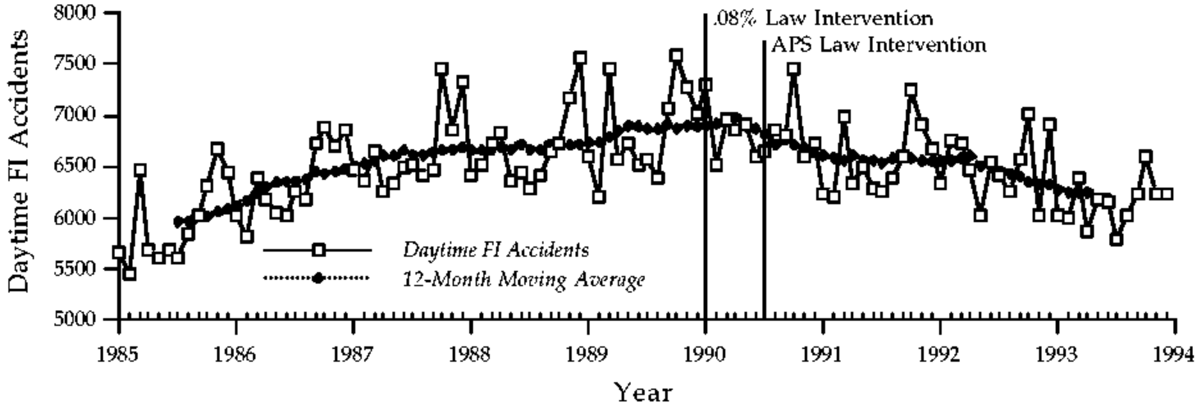

Figure 5.2. California daytime fatal and injury (FI) accidents by month, 1985-1994.

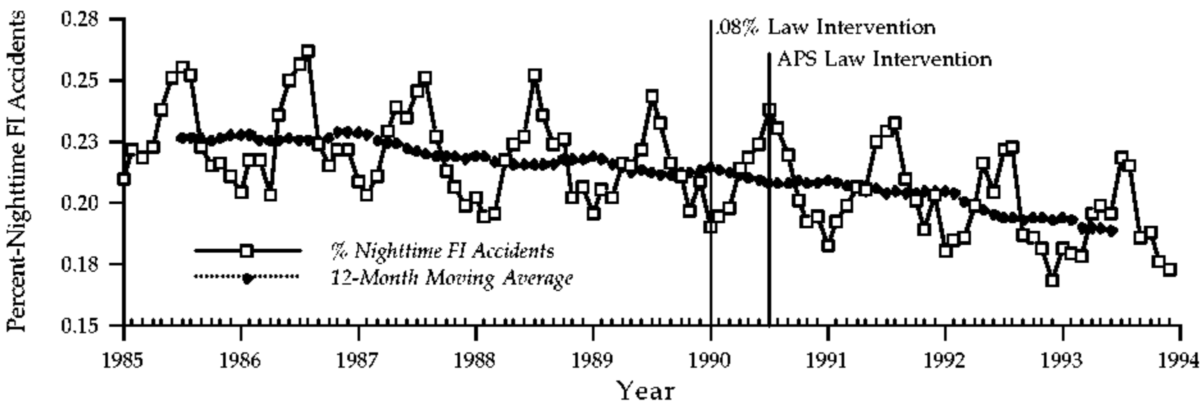

Figure 5.3. California nighttime fatal and injury (FI) accidents as a proportion of total fatal and injury accidents by month, 1985-1994.

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

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.

Nighttime Fatal and Severe-Injury Accidents

Series characteristics. 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.

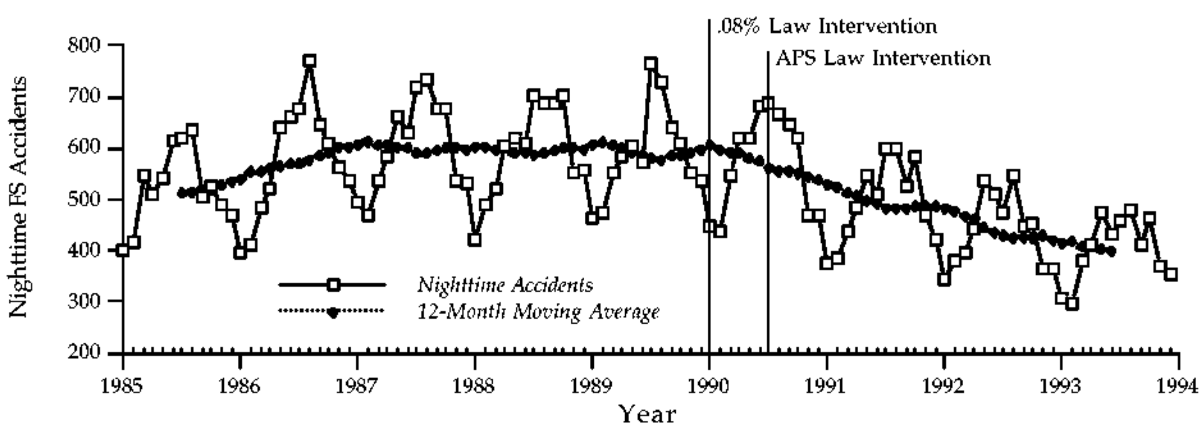

Figure 6.1. California nighttime fatal and severe-injury (FS) accidents by month, 1985-1994.

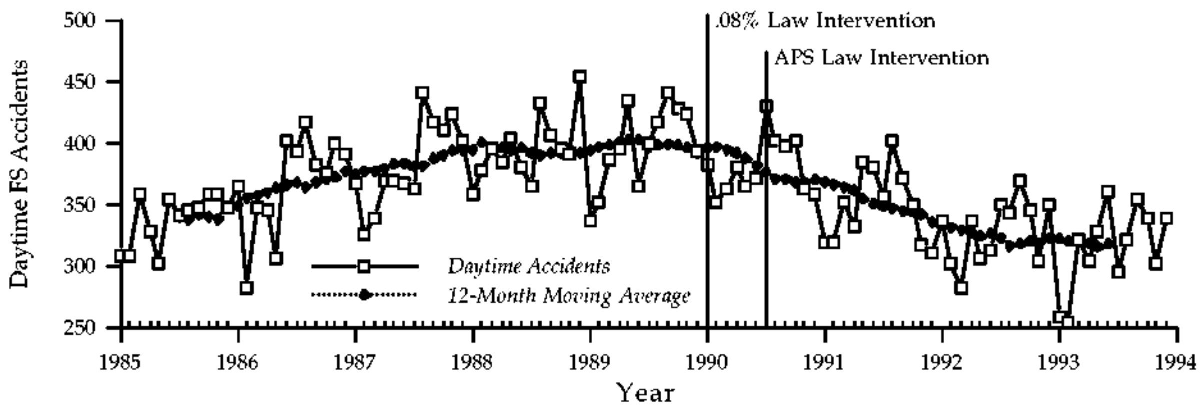

Figure 6.2. California daytime fatal and severe-injury (FS) accidents by month, 1985-1994.

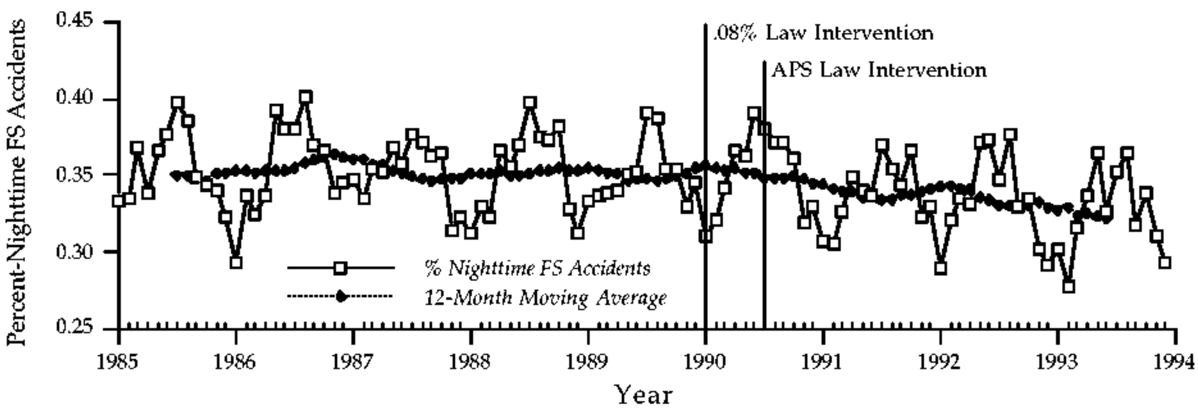

Figure 6.3. California nighttime fatal and severe-injury (FS) accidents as a proportion of total fatal and injury accidents by month, 1985-1994.

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%

law intervention were also nonsignificant. It was only when the third-stage abrupt/permanent effect hypothesis was modeled with the addition of the covariate series that the model components for the 0.08% law intervention were found to exhibit a significant effect *(t = -1.38, p = .08)*.

Table 8

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

Daytime 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	0.08 intervention	ω	0	-5.244	-.79	16	88	1102
		δ	1	-.9811	-22.28			
	APS intervention	ω	0	-51.08	-5.10			
		δ	1	1.032	224.2			
	Control	β	0	.4285	3.70			
	Noise	θ	1	-.4281	-4.96			
		θ	12	.9008	30.34			
Abrupt/temporary with covariate	0.08 intervention	ω	0	3.321	.22	21	83	962.98
		δ	1	1.104	29.58			
	APS intervention	ω	0	-46.05	-1.90			
		δ	1	1.061	37.58			
	Control	β	0	.2652	2.36			
	Licensed drivers	β	-5	-193.0	-2.89			
	Noise	θ	1	-.4750	-5.58			
		θ	12	.8998	27.52			
Gradual/permanent	0.08 intervention	ω	0	-10.19	-.95	14	88	1045
		δ	1	-.9764	-23.59			
	APS intervention	ω	0	-6.475	-3.29			
		δ	1	.9717	54.99			
	Control	β	0	.3956	3.16			
	Noise	θ	1	-.3402	-3.65			
		θ	3	-.3184	-3.35			
		θ	12	.8932	29.52			
Gradual/permanent with covariate	0.08 intervention	ω	0	-.4012	-.13	20	83	949.52
		δ	1	1.039	6.46			
	APS intervention	ω	0	-7.866	-1.19*			
		δ	1	.9446	15.59			
	Control	β	0	.2323	2.07			
	Licensed drivers	β	-5	-218.2	-3.42			
	Noise	θ	1	-.4831	-5.73			
		θ	12	.9044	28.87			
Abrupt/permanent	0.08 intervention	ω	0	-3.592	-.13	14	89	1267
	APS intervention	ω	0	-96.03	-3.47			
	Control	β	0	.4094	3.18			
	Noise	θ	3	-.3402	-3.47			
		θ	12	.8794	25.78			
		ϕ	1	.5660	6.53			
Abrupt/permanent with covariate	0.08 intervention	ω	0	-41.38	-1.38	22	84	1263
	APS intervention	ω	0	-61.40	-1.97			
	Control	β	0	.1338	1.15			
	Licensed drivers	β	-5	-191.8	-2.80			
	Noise	θ	12	.8889	25.28			
		ϕ	1	.7753	11.73			

Note. To stablize annual trend in the data, it was necessary to difference both the nighttime and daytime series at lag 12. 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

Residual mean square

Estimated effect approaches marginal significance *(p = .117)*

The estimated monthly drop in accidents associated with this decrease was 41.4 accidents per month, representing a 7.2% decrease from the series pre-intervention mean of 578.2 accidents per month. Presumably, including the licensed drivers as a covariate served to reduce the error such that the small magnitude of the effect could be identified. However, given that the level of significance was marginal, and lacking a corroborative significant finding before adding the series of licensed drivers, the significance of this finding becomes somewhat less compelling.

Intervention effects of the APS law. All of the parameter estimates for the gradual/permanent effect model for the APS intervention were statistically significant and all of the usual tests of stability were satisfied by this model. Table 8 shows that this intervention effect remained marginally significant even after including the licensed drivers covariate. Without the added explanatory variable, the intervention associated with the timing of the APS law shows a reduction of 6.475 accidents immediately with a very gradual rate change (δ = .9717). The estimated effect pattern suggested by this intervention model nearly approaches a ramp (gradual linear change) intervention. McCleary and Hay (1982) suggest that a ramp intervention describes a process which is trendless in the preintervention period (or has been made so by differencing the series), and then begins to trend in the postintervention period. Such a trend is visibly apparent in both the nighttime FS and daytime FS accident series depicted in Figure 6 (specifically in Figures 6.1 and 6.2.). It is also somewhat apparent in the proportion of total FS accidents that were nighttime FS accidents. (Particularly notice the gradual downward trend of the moving average indicator.) Given these resulting ω and δ parameters, the eventual reduction in accidents calculated as -6.475 ÷ (1 - .9717) would in theory amount to an eventual reduction of nearly 229 accidents per month. Since the change in level from month to month is quite small, however, it would take quite a long time for this level to be achieved. After one year, this estimated effect would have resulted in a reduction of 66.67 accidents per month, accounting for only 29% of the theoretical reduction potential.

Since the asymptotic level is not achieved within the observed postintervention period, the asymptotic accident reduction can only be considered a theoretical maximum. Such a prediction assumes that no other changes will occur prior to reaching the series asymptote, and ignores the inevitable changes caused by such events as the introduction of more new laws or other social influences which are bound to modify attitudes and driving behavior in the future. As already stated, most of the accident series in this evaluation show a downward trend beginning around the time of these interventions which continue through the end of the observed time series. Consequently, that the estimated impact in this model suggests a continuing increasing trend is not surprising. Furthermore, it will be shown that, like the current model, each of the gradual/permanent intervention models accepted in this evaluation suggest that the interventions follow a similar trend. Whether or not such a causal effect is plausible will be addressed in the Discussion section.

Table 8 shows that (with considerably diminished significance) the gradual/permanent effect estimate after including the covariate series produced a slightly enlarged estimate of 7.87 fewer accidents in the first month following the APS intervention. Again however, in this model the asymptotic change in level would be realized quite slowly as indicated by the large δ estimate of .9446. After the first year, this estimated effect predicts a reduction of 70.34 accidents per month, amounting to 50% of its theoretical potential reduction.

Nighttime Fatal Accidents

Series characteristics. Figure 7 presents plots of monthly nighttime fatal accidents, daytime fatal accidents, and the proportion of total fatal accidents that occurred at nighttime. Again, note the scaling differences of the vertical axes.

As in the other nighttime accident categories, Figure 7.1 shows that nighttime fatal accidents also show similar patterns of large 12-month seasonal fluctuations and variability throughout the series. Accident lows generally occurred in January and February and the highs occurred in the summer months. Figure 7.3 presents further visual evidence of the cyclical pattern and the extremely gradual decline beginning about the time the new laws were implemented in 1990.

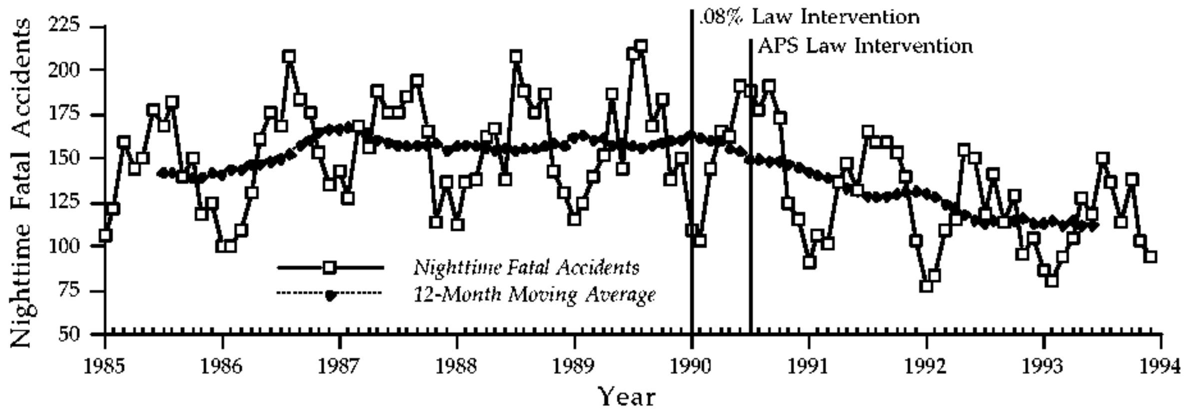

Figure 7.1. California nighttime fatal accidents by month, 1985-1994.

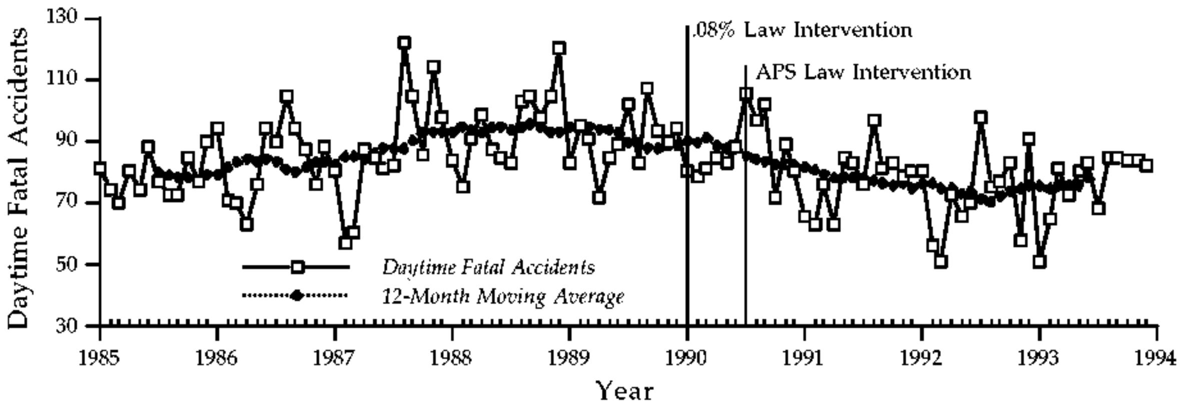

Figure 7.2. California daytime fatal accidents by month, 1985-1994.

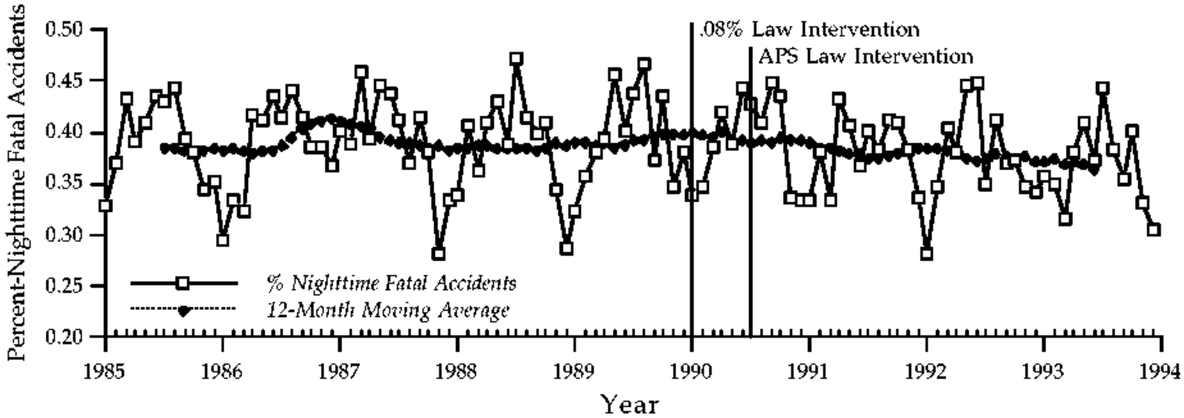

Figure 7.3. California nighttime fatal accidents as a proportion of total fatal accidents by month, 1985-1994.

Nighttime fatals in Figure 7.1 show a slight increase in the first few years of the series followed by a period of stability between 1987 and 1990, with a steady decline thereafter. Daytime fatals in Figure 7.2 show a pattern of accident increases marked by increasing variability through 1988 followed by steady declines through mid-1992, when accidents again increase throughout the remainder of the series. This pattern is very similar to that found among non-HBD fatal accidents, reflecting the large overlap between these two accident categories.

Time series analysis. Final model statistics and their associated diagnostics for nighttime fatal accidents are presented in Table 9. The minimum conditions of stability were met and all models were judged to be the most parsimonious and to provide the best fit as determined by the model diagnostics. Furthermore, the control scaling coefficient β was positive and statistically significant for all tests, again confirming its value in reducing unexplained variation in the treatment series.

As indicated in Table 3 (above), both licensed drivers and personal income had significant cross-correlations with nighttime fatal accidents when they were shifted back 5 and 9 months, respectively. However, in the bivariate assessment of its potential for explaining series variability, controlling for licensed drivers failed to provide any further predictive power than the inclusion of the daytime control alone. Consequently, in the final comprehensive time series analyses presented here, only personal income, shifted nine months back, was used as a covariate.

Intervention effects of the 0.08% and APS laws. Table 9 shows that in accord with the three-step process of analysis, the null hypotheses failed to be rejected for all of the intervention effects tested. Although the table shows that significant decreases associated with the timing of the APS intervention were obtained for the abrupt/permanent and gradual/permanent effect models when the covariate was included, these decreases cannot be attributed to the intervention since their associated 8 parameters are large and negative, once again implying an oscillating effect that cannot reasonably be argued to have been caused by the law. In these, and each remaining model, the 0.08% law intervention ω parameter estimates failed to reach even a minimal level of statistical significance. Collectively, these analyses failed to reveal even a suggestive change in accidents which could be associated with the timing of either the 0.08% law or the APS law.

Table 9

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

Daytime 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	0.08 intervention	ω	0	6.596	.38	32	100	801.93
		δ	1	.9221	2.55			
	APS intervention	ω	0	8.569	.31			
		δ	1	-.1164	-.04			
	Control	β	0	1.647	27.06			
	Noise	θ	1	-.2401	-2.45			
		θ	13	-.3355	-3.61			
Abrupt/temporary with covariate	0.08 intervention	ω	0	2.005	.31	24	75	696.52
		δ	1	-1.006	-11.54			
	APS intervention	ω	0	-23.87	-1.72			
		δ	1	-.8654	-6.58			
	Control	β	0	1.514	15.78			
	Personal income	β	-9	-35.12	-2.70			
	Noise	θ	1	-.2985	-2.82			
		θ	13	-.2664	-2.31			
		φ	3	.3197	2.96			
		φ	11	.3590	3.16			
Gradual/permanent	0.08 intervention	ω	0	3.571	.26	28	100	765.21
		δ	1	.6474	.39			
	APS intervention	ω	0	-31.47	-1.21			
		δ	1	-.8480	-5.67			
	Control	β	0	1.709	34.47			
	Noise	θ	1	-.2868	-2.94			
		θ	4	.2316	2.33			
		θ	13	-.2701	-2.75			
Gradual/permanent with covariate	0.08 intervention	ω	0	5.229	.49	28	90	703.10
		δ	1	.7527	1.34			
	APS intervention	ω	0	-33.42	-1.52			
		δ	1	-.8420	-7.13			
	Control	β	0	1.553	16.83			
	Personal income	β	-9	-23.24	-1.80			
	Noise	θ	1	-.2967	-2.92			
		θ	3	-.3773	-3.81			
		θ	13	-.4018	-4.18			
Abrupt/permanent	0.08 intervention	ω	0	4.345	.30	30	102	730.28
	APS intervention	ω	0	-8.589	-.58			
	Control	β	0	1.628	20.66			
	Noise	θ	1	-.2104	-2.10			
		θ	3	-.3178	-3.28			
		θ	13	-.3736	-4.09			
Abrupt/permanent with covariate	0.08 intervention	ω	0	5.580	.38	29	92	733.02
	APS intervention	ω	0	-7.239	-.49			
	Control	β	0	1.601	19.48			
	Personal income	β	-9	-21.63	-1.49			
	Noise	θ	1	-.2368	-2.28			
		θ	3	-.2761	-2.69			
		θ	13	-.3777	-4.05			

Note. To remove nonstationarity, the personal income series was independently differenced at lag 1. The lag value -9 for the licensed drivers series indicates that it was shifted backward nine months for maximal adjustment in the analyses.

[a]Ljung-Box Q statistic

[b]Residual mean square

2 to 3 a.m. Fatal and Injury Accidents

Series characteristics. Figure 8.1 presents a plot of FI accidents occurring immediately after the mandatory bar-closing hour, that is, those occurring between 2 and 3 a.m. Figure 8.2 presents a plot for FI accidents occurring between 10 and 11 a.m. used as the control series in this analysis. Figure 8.3 presents a plot of the proportion of total FI accidents occurring between 2 and 3 a.m. The scaling of the vertical axes are again different between plots as a result of the greater number of FI accidents between 10 and 11 a.m. As shall become clear in the assessment of these categories for fatal incidents, by far the majority of the accidents in either of these categories are injury accidents.

Visual inspection of the plot of FI accidents between 2 and 3 a.m., shows a pattern of slight increases through mid-1986 followed by a persistent downward trend throughout the duration of the series. Figure 8.2 reveals an equally persistent upward trend among FI accidents occurring during the low-alcohol morning (10 to 11 a.m.) control period.

Figure 8.3 reveals a steady downward trend in the proportion of total FI accidents that occur between 2 and 3 a.m. The vertical axis of this plot shows that the category is proportionately quite small; however, the likelihood of alcohol-involvement is high for accidents occurring during this single hour.

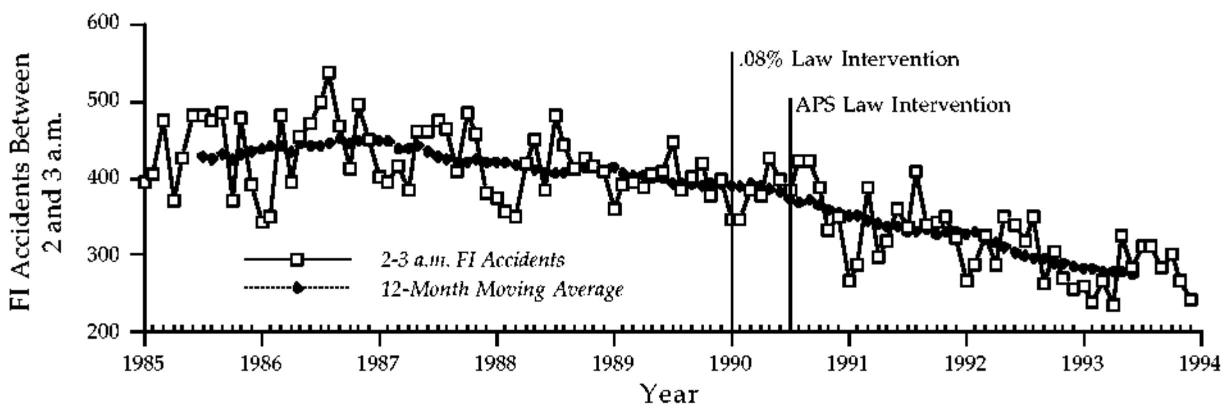

Figure 8.1. California fatal and injury (FI) accidents occurring between 2 and 3 a.m. by month, 1985-1994.

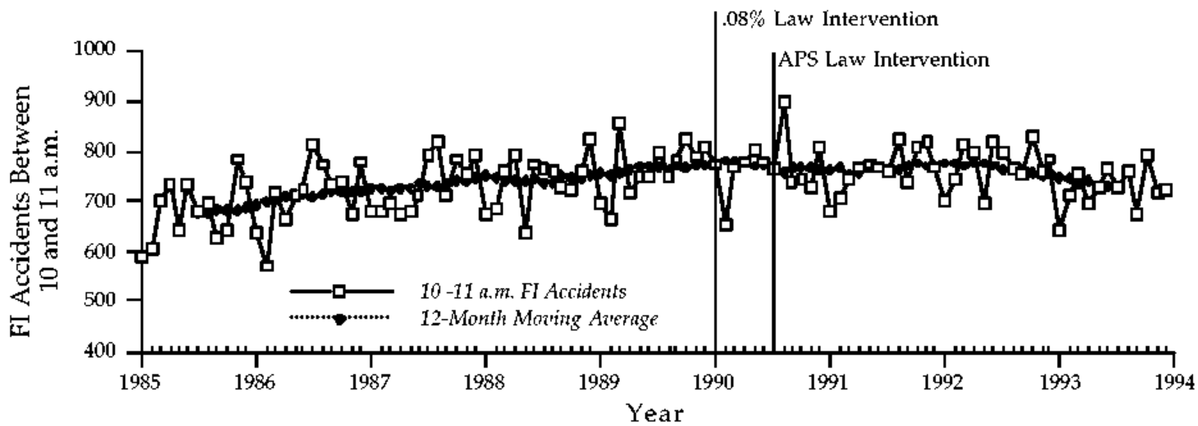

Figure 8.2. California fatal and injury (FI) accidents occurring between 10 and 11 a.m. by month, 1985-1994.

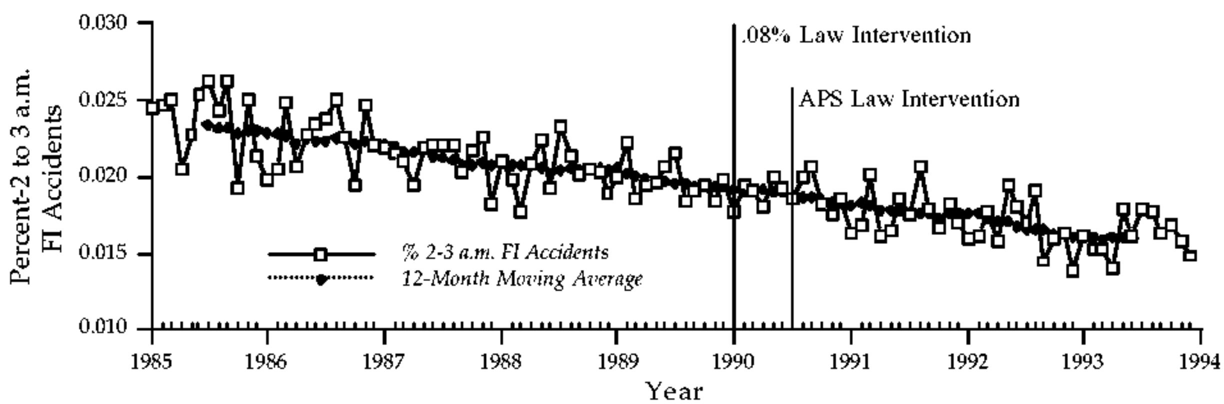

Figure 8.3. California 2 to 3 a.m. fatal and injury (FI) accidents as a proportion of total fatal and injury accidents by month, 1985-1994.

Time series analysis. Table 10 presents model diagnostics and statistics for the intervention effects detected for the best fitting models of 2 to 3 a.m. FI accidents, by the three-stage modeling strategy. Again, the control scaling coefficient β was positive and statistically significant for all tests, confirming its value in reducing unexplained variation in the treatment series.

Personal income and monthly highway sales of gasoline each had significant cross-correlations with the prewhitened series of FI accidents occurring between 2 and 3 a.m. (see Table 3 above). In the bivariate assessments of these covariates with the dependent variable, however, the personal income series failed to provide any further predictive power than did the inclusion of the control alone. Consequently, in the final analyses, only "monthly highway sales of gasoline," at lag order zero, was used as an added explanatory measure.

Table 10

California Fatal/Injury Accidents Occurring Between 2 and 3 a.m. Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Fatal/Injury Accidents occurring between 10 and 11 a.m. 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	-36.53	-1.16*	21	99	1614
		δ	1	.7197	1.68			
	APS intervention	ω	0	-62.99	-3.75			
		δ	1	1.021	152.97			
	Control	β	0	.5622	39.84			
	Noise	θ	2	-.2400	-2.55			
		θ	3	-.2046	-2.14			
		θ	12	-.4510	-5.30			
Abrupt/temporary with covariate	0.08 intervention	ω	0	-5.399	-.30	29	69	846.23
		δ	1	1.048	21.35			
	APS intervention	ω	0	6.671	.35			
		δ	1	.7552	.46			
	Control	β	0	.2222	4.49			
	Gasoline sales	β	0	-27.12	-4.08			
	Noise	θ	1	.7067	8.98			
		θ	8	.4096	4.42			
		φ	3	.3140	3.00			
		φ	12	.4282	4.86			
Gradual/permanent	0.08 intervention	ω	0	-50.53	-1.76	25	100	1569
		δ	1	-.5094	-.79			
	APS intervention	ω	0	-7.691	-1.92			
		δ	1	.9337	22.85			
	Control	β	0	.5713	42.40			
	Noise	θ	1	-.1887	-1.91			
		θ	2	-.1931	-1.98			
		θ	12	-.4446	-5.24			
Gradual/permanent with covariate	0.08 intervention	ω	0	-3.460	-.96	29	69	824.57
		δ	1	.9811	24.49			
	APS intervention	ω	0	7.457	.33			
		δ	1	-.5838	-.24			
	Control	β	0	.2290	4.85			
	Gasoline sales	β	0	-27.47	-4.30			
	Noise	θ	1	.7350	9.76			
		θ	8	.4483	4.90			
		φ	3	.3129	2.98			
		φ	12	.4639	5.29			
Abrupt/permanent	0.08 intervention	ω	0	-43.17	-1.83	24	102	1701
	APS intervention	ω	0	-59.35	-2.50			
	Control	β	0	.5624	35.19			
	Noise	θ	1	-.2725	-2.74			
		θ	2	-.3159	-3.27			
		θ	12	-.3979	-4.63			
Abrupt/permanent with covariate	0.08 intervention	ω	0	4.534	.24	30	71	830.61
	APS intervention	ω	0	-2.120	-.12			
	Control	β	0	.2278	4.65			
	Gasoline sales	β	0	-27.88	-4.30			
	Noise	θ	1	.7035	8.96			
		θ	8	.3974	4.32			
		φ	3	.32334	3.15			
		φ	12	.4604	5.33			

Note. Introduction of the gasoline sales covariate caused the need to adjust for monthly trend by differencing both the 2 to 3 a.m. and 10 to 11 a.m. series at lag 1. There was no need to difference the series when the covariate was excluded. To remove nonstationarity, the gasoline sales series was independently differenced at lags 1 and 12. Shading indicates a statistically significant (p<.10; one-tailed test) and acceptable intervention effect.

[a]Ljung-Box Q statistic

Residual mean square

Estimated effect approaches minimal significance (p = .123, one-tailed test).

Intervention effects of the 0.08% law. As indicated in Table 10, the model statistics for the initial abrupt/temporary 0.08% law intervention effect were very weakly suggestive of a significant reduction (t = -1.16, p = .123). Under this model, while the estimated intervention-associated effects diminished rather slowly, there were essentially no remaining effects after 12 months following the point of intervention, since the initial maximal effect had been quite small.

Since this initial effect hypothesis fell short of reaching statistical significance, in accord with the three-stage modeling process, the remaining two effect model hypotheses were considered. Table 10 shows that while the gradual/permanent model effects were not significant, the abrupt/permanent intervention model parameters for the 0.08% law intervention were significant with a substantially higher level of confidence (t = -1.83, p = .03). Under this model, the series is estimated to drop 43.17 accidents per month from the preintervention level. This significant finding was not supported upon adding the explanatory variable which, in this case, reduced the error term by half, and dramatically reduced the intervention effect to a nonsignificant level.

Intervention effects of the APS law. All of the parameter estimates for the gradual/permanent effect model for the APS law intervention were statistically significant (t = -1.92, p = .03) and all of the usual tests of stability were satisfied by this model. However, Table 10 shows that this intervention effect was no longer significant after adding the gasoline sales covariate. Nonetheless, without the covariate, the APS intervention point is associated with an immediate reduction of 7.691 accidents, with a very gradual rate change (δ = .9337) indicating that the asymptotic change in level will be realized quite slowly, and as with the other gradual/permanent effects found in this evaluation nearly models a ramp intervention. Given these resulting ω and δ parameters, the eventual reduction in accidents, calculated as -7.691 ÷ (1 - .9337), would culminate in a theoretical reduction of 116 accidents per month foregoing any other factors which would cause the series to change. Since the change in level from month to month is again quite small, and the rate of change quite slow, however, it would again take quite a long time for this level to be achieved making the possibility of actually achieving such an effect unreasonable. After one year, this model's estimated effect predicts a reduction of 65.07 accidents per month accounting for 56% of the estimated reduction potential.

These results should be considered, at best, only suggestive because, as stated, with the addition of the additional explanatory variable, no evidence of any significant intervention effect was obtained for any of the possible effect models. This is especially important given the fact that, by including the covariate in each model, the residual mean square error was greatly reduced.

2 to 3 a.m. Fatal and Severe-Injury Accidents

Series characteristics. Figure 9.1 presents a plot of fatal and severe-injury accidents occurring immediately after the mandatory bar-closing hour (between 2 and 3 a.m.). Figure 9.2 presents a plot of the control series fatal and severe-injury accidents occurring between 10 and 11 a.m. The proportion of total fatal and severe-injury accidents occurring between 2 and 3 a.m. are plotted in Figure 9.3. The scaling of the vertical axes are again different between plots. The appearance of greater variability among the "daytime" accidents is an artifact of the scaling difference between the plots shown in Figures 9.1 and 9.2. On average, there were only 61.27 monthly fatal and severe-injury accidents between 2 and 3 a.m. and only 40.19 monthly fatal and severe-injury accidents between 10 and 11 a.m.

Visual inspection of the accident plot in Figure 9.1 shows an initial pattern of increasing accidents followed by a period of stability through 1989 and then a persistent downward trend beginning in 1990. The plot of fatal and severe-injury accidents occurring between 10 and 11 a.m. in Figure 9.2 shows a flat stable pattern throughout the course of the series.

With the exception of a fair amount of cyclical fluctuation, Figure 9.3 reveals a fairly stable overall pattern in the proportion of fatal and severe-injury accidents that occur between 2 and 3 a.m. Close inspection shows that the series exhibits a slight downward trend throughout its course.

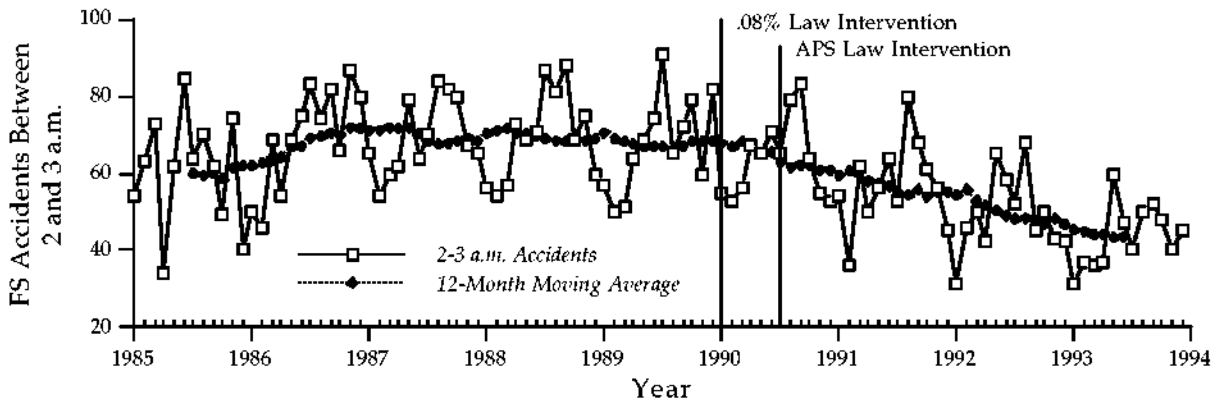

Figure 9.1. California fatal and severe-injury (FS) accidents occurring between 2 and 3 a.m. by month, 1985-1994.

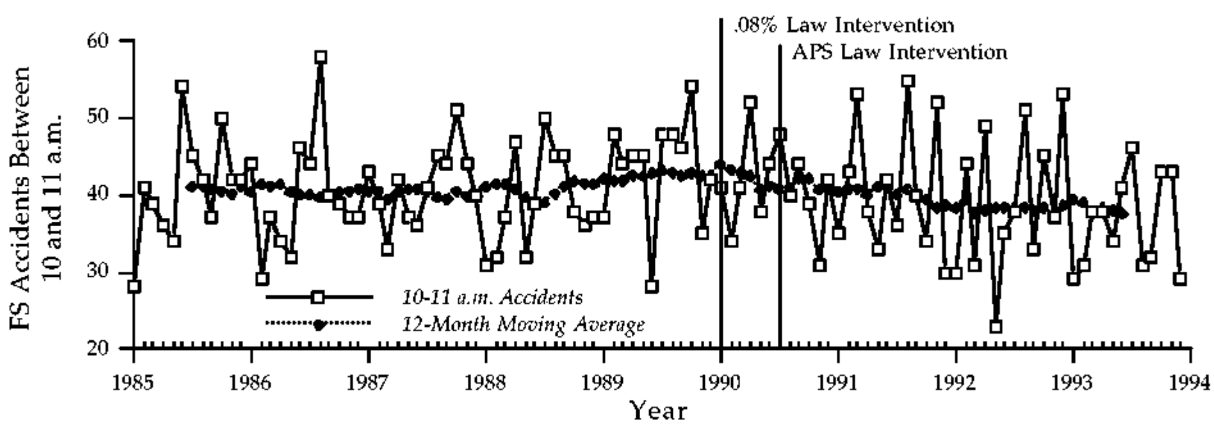

Figure 9.2. California fatal and severe-injury (FS) accidents occurring between 10 and 11 a.m. by month, 1985-1994.

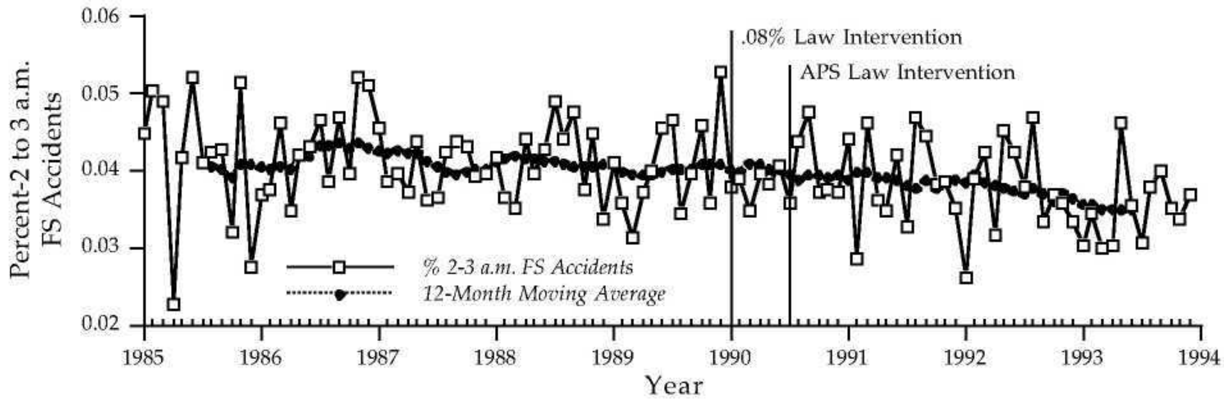

Figure 9.3. California 2 to 3 a.m. fatal and severe-injury (FS) accidents as a proportion of total fatal and severe-injury accidents by month, 1985-1994.

Time series analysis. Table 11 presents model diagnostics and statistics for the intervention effects detected by the three-stage modeling strategy. All the usual tests of stability were satisfied and the control scaling coefficient β was positive and statistically significant for all tests. None of the four potential covariate series were found to improve the predictive ability of the models and so were not included in any of these final time series models.

Table 11

California Fatal/Severe-Injury Accidents Occurring Between 2 and 3 a.m. Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Fatal/Severe-Injury Accidents occurring between 10 and 11 a.m. 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	-11.35	-1.31	29	86	108.13
		δ	1	1.014	20.65			
	APS intervention	ω	0	.7775	.20			
		δ	1	-.9474	-2.56			
	Control	β	0	.2610	2.36			
	Noise	φ	1	-.4951	-6.00			
		φ	12	.4519	5.91			
Gradual/permanent	0.08 intervention	ω	0	-13.53	-1.36	26	87	109.74
		δ	1	-.4819	-.74			
	APS intervention	ω	0	-.7315	-.26			
		δ	1	.9678	4.27			
	Control	β	0	.2824	2.59			
	Noise	φ	1	-.4868	-5.68			
		φ	12	.4604	6.01			
Abrupt/permanent	0.08 intervention	ω	0	-11.14	-1.30	25	89	107.31
	APS intervention	ω	0	-1.383	-.16			
	Control	β	0	.2733	2.53			
	Noise	φ	1	-.4976	-6.07			
		φ	12	.4549	5.99			

Note. To adjust for monthly trend in the data, it was necessary to difference both the 2 to 3 a.m. and 10 to 11 a.m. series at lag 1. Shading indicates a statistically significant ($p<.10$; one tailed test) and acceptable intervention effect.
[a]Ljung-Box Q statistic
Residual mean square

Intervention effects of the 0.08% and APS laws. Table 11 shows that the intervention parameters in the abrupt/temporary effect model were either nonsignificant or outside the bounds of system stability. Moreover, the intervention parameters in the gradual/permanent effect model were also nonsignificant. Only the third-stage abrupt/permanent effect model components for the 0.08% law intervention were found to exhibit a significant effect (t = -1.30, p = .10). No such effect was obtained for the APS law intervention, which was found to be nonsignificant. The estimated monthly drop in accidents subsequent to the implementation date of the 0.08% law was -11.14 accidents per month, representing a 16.5% decrease from the series pre-intervention mean of 67.52 accidents per month. However, given that the level of significance associated with this effect was marginal, interpretation must be guarded.

2 to 3 a.m. Fatal Accidents

Series characteristics. Figure 10.1 presents a plot of fatal accidents occurring between 2 and 3 a.m. Figure 10.2 presents a plot of fatal accidents occurring between 10 and 11 a.m. used as the control series in this analysis. Figure 10.3 presents a plot of the proportion of total fatal accidents occurring between 2 and 3 a.m. On average there were only 16.87 fatal accidents per month between 2 and 3 a.m. and only 9.75 between 10 and 11 a.m. Because of the small number of accidents for this and the preceding analysis, the statistical power for detecting a real effect is limited.

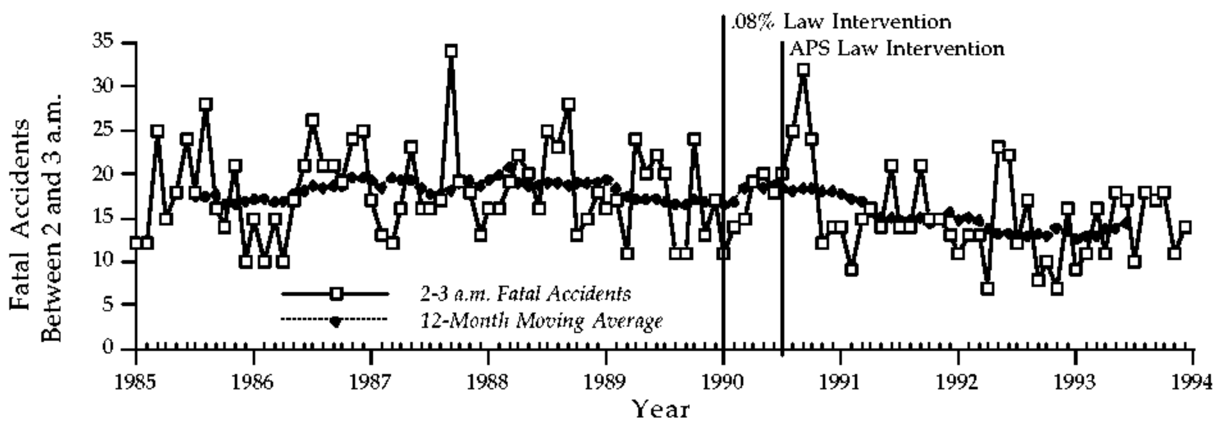

Figure 10.1. California fatal accidents occurring between 2 and 3 a.m. by month, 1985-1994.

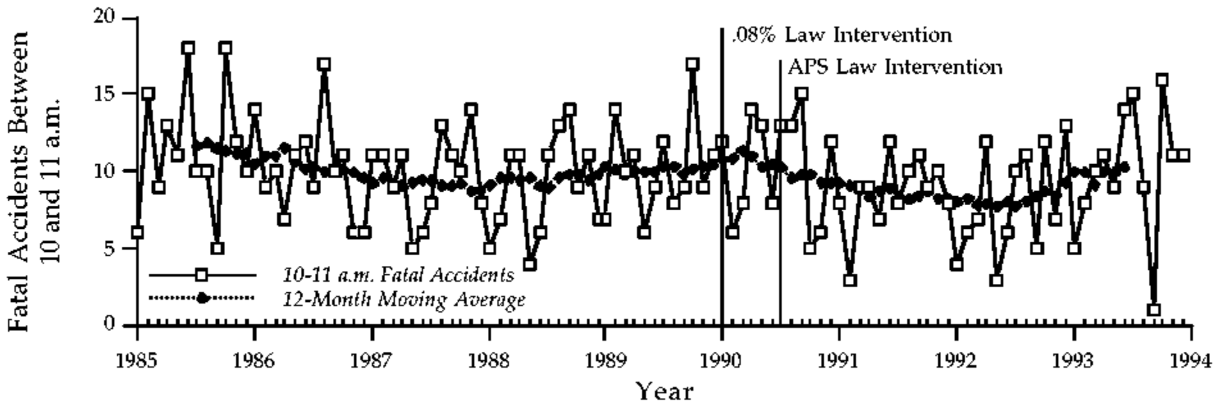

Figure 10.2. California fatal accidents occurring between 10 and 11 a.m. by month, 1985-1994.

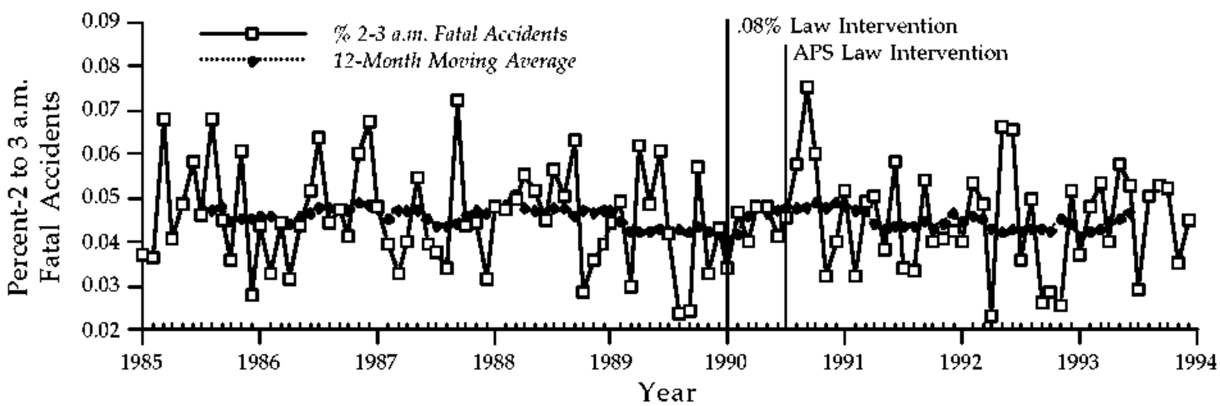

Figure 10.3. California 2 to 3 a.m. fatal accidents as a proportion of total fatal accidents by month, 1985-1994.

These plots are marked by a fair amount of variability around their means and are spotted with an occasional extreme value. To the extent possible, the accuracy of each of the extremely high values was confirmed via the CHP (who provided the data) prior to conducting the time series analyses. So while most likely not erroneous, the two highest figures (which can be readily identified in Figure 10.1) clearly represented "outliers" in the series which likely had nothing to do with the effect of the new laws or alcohol-related factors. For the purposes of this evaluation, because of the degree to which they deviated from the series mean, the two highest figures were viewed as series contaminants which would clearly have had the effect of inflating the series variance (Chen & Liu, 1993; McCleary & Hay, 1982) and could have lead to faulty time series models. Consequently, following the suggestions of McLeod (1983) and McCleary and Hay (1982), separate pulse functions were included for September 1987, (when 34 accidents occurred, represe nting a figure over twice as large as the overall series mean) and September 1990, (when 32 accidents occurred, again representing a figure twice as large as the overall series mean). Except for the first outlier, Figure 10.1, reveals a fairly stable (horizontal) pattern between 1985 and 1990, followed by an unusually large increase in the first three months (August through October) after the July intervention point. The respective accident totals for these three months were 25, 32, and 24, compared with the post-intervention series mean of 15.17 accidents per month. The series concludes with a gradual downward trending pattern, except during 1993, when these accidents increased slightly. The control series in Figure 10.2 reveals a slight downward trend through 1987, followed by an equally slight increase through 1989, returning again to decreases through 1991 and concluding in an upward trend thereafter.

The proportion of total fatal accidents that occurred between 2 and 3 a.m. presented in Figure 10.3 shows a pattern of large variation around a fairly stable mean. The vertical axis of this plot shows that, overall, this category comprises only a small proportion of total fatal accidents but is considered overrepresented in alcohol-involvement.

Time series analysis. Because of the presence of the outliers, the time series analyses results which did not account for the unusually high months are not presented[5]. Instead, Table 12 presents model diagnostics and statistics for the intervention effects detected by the three-stage modeling strategy after adding the pulse interventions for September 1987 and September 1990 among 2 and 3 a.m. fatal accidents. All the usual tests of stability were satisfied and, again, all models presented were judged to be the most parsimonious and to provide the best fit. The control scaling coefficient β was positive and statistically significant for all tests with the exception of the test for abrupt/permanent effects after entering the covariate series, where the control series, though positive in value, no longer significantly contributed to the model. Both pulse interventions were positive and statistically significant for each of the models except in the abrupt/temporary model after including the covariate where the September 1990 intervention did not contribute significantly.

Table 3 (presented on page 31) showed that personal income, with no lag (order zero), and licensed drivers, when lagged back eight months, each had significant cross-correlations with the prewhitened dependent series. In the bivariate assessments of these covariates with the dependent variable, and after accounting for the two outliers, only the series of licensed drivers improved the predictive value of the model and was therefore the only covariate included in the final analyses presented in Table 12.

Intervention effects of the 0.08% and APS laws. Table 12 shows that only the abrupt/permanent effect model found a marginally significant decrease associated with the timing of the APS law after including the covariate (t = -1.26, p = .104). Although small (only -2.25 accidents per month), this effect, if real, represents a decrease of 12.5% after the introduction of the APS law. However, this significant finding should be interpreted with caution, in light of obtaining small nonsignificant *increases* without the covariate. Recall that this nonsignificant increase in accidents is actually consistent with the unusually large increase in the first three months following the July intervention point. Because the volume of accidents during this late-night one-hour period is quite low compared to other accident categories in this evaluation, the entire analysis is rendered more vulnerable to anomalies, such as this one, when they occur.

5 Although they are considered here as somewhat invalid findings, these results were not substantively different than the results reported and may be obtained upon request made to the author.

Table 12

California Fatal Accidents Occurring Between 2 and 3 a.m. Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects Accounting for Outliers

Fatal Accidents occurring between 10 and 11 a.m. 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	-4.276	-.95	25	89	20.35
		δ	1	.1452	.14			
	APS intervention	ω	0	3.744	.86			
		δ	1	.7251	1.34			
	September 1987	ω	0	16.81	3.87			
	September 1990	ω	0	9.453	1.98			
	Control	β	0	.2533	1.95			
	Noise	ϕ	1	.2322	2.42			
		ϕ	7	-.1993	-2.13			
		Mean	0	14.08	10.22			
Abrupt/temporary with covariate	0.08 intervention	ω	0	-6.088	-1.36	21	89	19.73
		δ	1	.2554	.35			
	APS intervention	ω	0	-3.213	-1.05			
		δ	1	-.8105	-3.65			
	September 1987	ω	0	16.03	3.75			
	September 1990	ω	0	5.524	.92			
	Control	β	0	.2529	1.91			
	Licensed drivers	β	-8	-26.18	-2.20			
	Noise	ϕ	1	.2986	2.90			
		Mean	0	13.98	9.82			
Gradual/permanent	0.08 intervention	ω	0	-.5995	-1.13	20	92	18.17
		δ	1	.9329	27.23			
	APS intervention	ω	0	3.678	.82			
		δ	1	.1044	.10			
	September 1987	ω	0	15.57	3.86			
	September 1990	ω	0	12.71	2.96			
	Control	β	0	.1484	1.27			
	Noise	ϕ	7	-.4042	-4.43			
		Mean	0	16.42	13.19			
Gradual/permanent with covariate	0.08 intervention	ω	0	-.5170	-1.03	23	83	16.77
		δ	1	.9386	25.52			
	APS intervention	ω	0	3.215	.72			
		δ	1	.0623	.05			
	September 1987	ω	0	15.25	3.87			
	September 1990	ω	0	6.896	1.35			
	Control	β	0	.1440	1.20			
	Licensed Drivers	β	-8	-21.58	-1.99			
	Noise	ϕ	7	-.3661	-3.81			
		Mean	0	16.38	12.98			
Abrupt/permanent	0.08 intervention	ω	0	-1.268	-.71	17	94	18.91
	APS intervention	ω	0	-1.850	-1.01			
	September 1987	ω	0	15.60	3.74			
	September 1990	ω	0	16.28	3.86			
	Control	β	0	.1427	1.20			
	Noise	ϕ	7	-.3636	-3.98			
		Mean	0	16.47	12.94			
Abrupt/permanent with covariates	0.08 intervention	ω	0	-.8769	-.50	18	85	17.59
	APS intervention	ω	0	-2.254	-1.26*			
	September 1987	ω	0	15.33	3.75			
	September 1990	ω	0	10.07	1.94			
	Control	β	0	.1356	1.10			
	Licensed Drivers	β	-8	-22.19	-1.97			
	Noise	ϕ	7	-.3224	-3.35			
		Mean	0	16.43	12.68			

Note. To adjust for nonstationarity, the licensed drivers covariate series was independently differenced at lag 1 prior to analysis. The lag value -8 for the licensed drivers series indicates that it was shifted backward eight months for maximal adjustment in the analyses. Pulse interventions were added at September 1987 and September 1990 to account for outliers.

[a]Ljung-Box *Q* statistic

Residual mean square

Approaches marginal statistical significance ($p = .104$)

Single-Vehicle Nighttime Male (SVNM) Fatal and Injury Accidents

Series characteristics. SVNM FI accidents are plotted in Figure 11.1. Figure 11.2 presents a similar plot for the control series, multiple-vehicle FI accidents occurring between the daytime hours of 6 a.m. and 2 p.m. Recall that the rationale for using this series was that multiple-vehicle accidents occurring at any time are less likely to involve alcohol than are single-vehicle incidents, particularly if they occur during daytime hours. (Once again, note that the scaling of the vertical axis is different for each of the two series, reflecting their large differences in volume.) On average, there were over five times as many multiple-vehicle daytime FI accidents as SVNM FI accidents. This was to be expected, primarily as a result of the greater number of vehicles and drivers (of both sexes) traveling during daytime hours.

Figure 11.1 shows that SVNM FI accidents showed a slight increase through 1986, followed by a steady decline throughout the remainder of the series. Conversely, Figure 11.2 shows a pattern of steady increases in multiple-vehicle daytime accidents through 1989, followed by an equally persistent downward trend thereafter.

Twelve-month annual cycles are apparent in both series, with annual highs primarily in July and August among SVNM FI accidents and in October among daytime FI accidents. The lowest volumes for SVNM FI accidents tended to be around January and February, and for daytime FI accidents they were mostly in February and July.

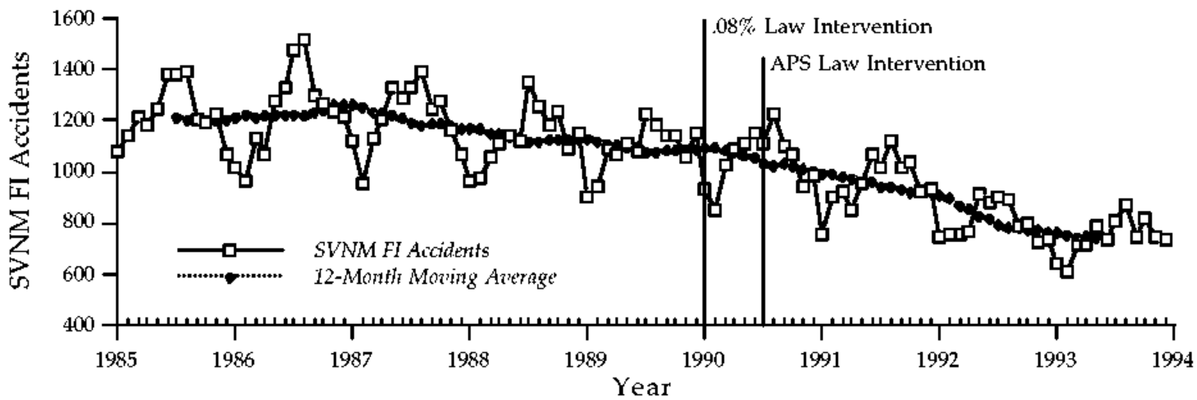

Figure 11.1. California single-vehicle nighttime male (SVNM) fatal and injury (FI) accidents by month, 1985-1994.

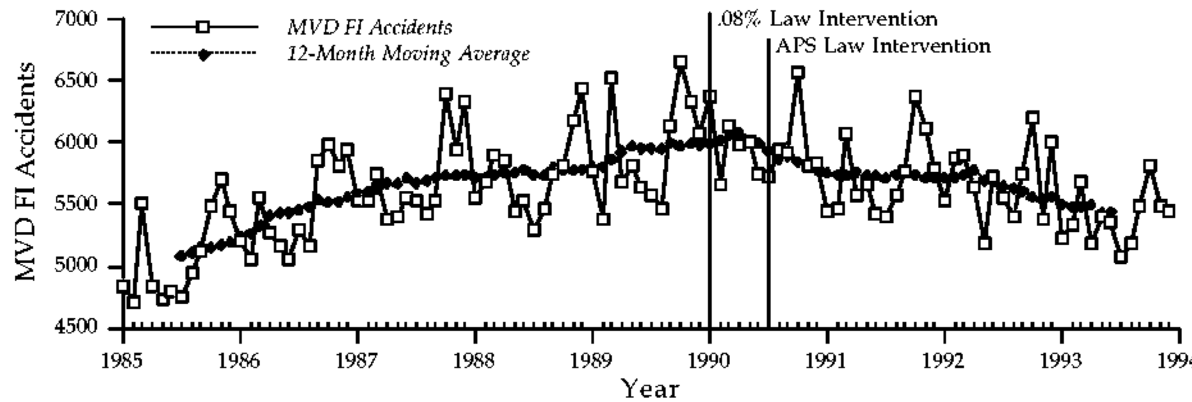

Figure 11.2. California multiple-vehicle daytime (MVD) fatal and injury (FI) accidents by month, 1985-1994.

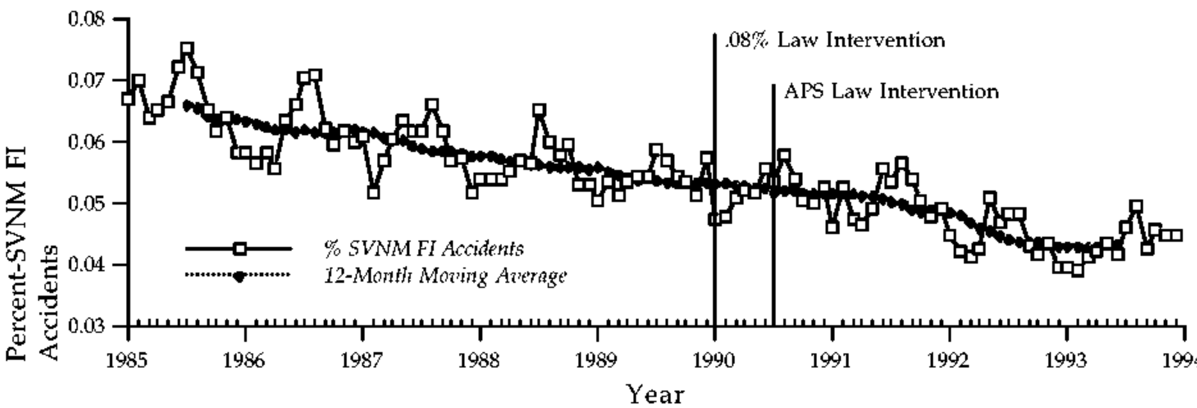

Figure 11.3. California single-vehicle nighttime male (SVNM) fatal and injury (FI) accidents as a proportion of total fatal and injury accidents by month, 1985-1994.

The proportion of total FI accidents which were SVNM, presented in Figure 11.3, also reveals a persistent downward trend across the entire series. The overall pattern shown in this figure is consistent with that of the nighttime FI series (plotted in Figure 5.3), of which this is a subset. These data in Figure 11.3 demonstrate that, in general, SVNM FI accidents make up only a small proportion of total FI accidents, comprising only 5.5% of that total during the years considered in this evaluation. For the same period, SVNM FI accidents represented a 25.65% subset of nighttime FI accidents. Like the accidents that occur in the hour immediately following the mandatory closing of bars, these accidents are not proportionately a large category. However, they consistently prove to be far more alcohol-involved than do the larger alcohol surrogate series.

Time series analysis. Model statistics and their associated diagnostics for SVNM FI accidents are presented in Table 13. As before, all noise parameters in the presented models were considered stable, the residuals were best represented by a white noise process, and the control scaling coefficient β was positive and statistically significant for all tests, confirming its value in reducing unexplained variation in the treatment series.

The series representing total monthly personal income (see Table 3, on page 31) revealed significant cross-correlations with SVNM FI accidents when shifted back nine months. The bivariate analyses with the dependent series showed that it had the potential of reducing the error variance beyond what could be explained using the low-alcohol control series alone. Consequently, personal income, shifted back nine months, was used as an additional explanatory covariate in the final time series analyses.

Intervention effects of the 0.08% law. Table 13 shows that in the first stage of the three-stage modeling strategy, the intervention parameters for the abrupt/temporary effect model for the 0.08% law were nonsignificant. Consequently, temporary effects of this law were ruled out.

In the second stage, the parameter estimates for the gradual/permanent effect model prior to adding the covariate series were not significant. Table 13 shows that after including the personal income covariate, however, the intervention parameters were marginally statistically significant (t = -10.28, p = .102). This model estimates an initial drop of 10.28 SVNM FI accidents subsequent to the timing of the 0.08% law implementation. This represents a drop of less than 1% from the former pre-law series mean level of 1,179 SVNM FI accidents per month. Furthermore, the model indicated an extremely gradual rate change (δ = .9804), indicating that the asymptotic change inlevel would be realized over an extremely long time. After one year, this estimated effect would have resulted in a reduction of 110.89 accidents per month and represents about 21% of the estimated theoretical reduction.

Intervention effects of the APS law. Both with and without the added explanatory power of the personal income series, the abrupt/temporary model parameters were outside the bounds of system stability and were therefore rejected as unreasonable outcomes of the APS law. In the second stage modeling strategy, without the added explanatory power of the covariate, the gradual/permanent intervention parameters associated with the timing of the APS law indicated a statistically significant (t = -1.39, p = .08) monthly reduction of 11.17 accidents immediately, with an extremely gradual rate change (δ = .9753) indicating that the asymptotic change in level would again be realized over a very long period. After one year, this estimated effect would have resulted in a reduction of 117.25 accidents per month, representing 26% of the theoretical asymptotic potential. Conversely, in the presence of the covariate, the APS law intervention component gradual/permanent effect parameters were rendered unstable.

Since significant gradual/permanent effects were found associated with the two interventions (although only marginal in the case of the 0.08% intervention parameters) there was no need to conduct the third stage hypothesis tests for abrupt/permanent effects.

Table 13

California Single-Vehicle Nighttime Fatal/Injury Accidents Involving Male Drivers Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Multiple-Vehicle 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	34.16	.93	31	97	4112
		δ	1	-.4263	-.57			
	APS intervention	ω	0	-20.07	-1.40			
		δ	1	-.9729	-24.80			
	Control	β	0	.0381	2.23			
	Noise	θ	1	.3406	3.67			
		θ	6	.4752	5.44			
		θ	7	.2352	2.45			
		θ	12	-.8780	-27.16			
Abrupt/temporary with covariate	0.08 intervention	ω	0	-13.08	-1.13	27	88	3732
		δ	1	-.9964	-36.74			
	APS intervention	ω	0	-67.19	-1.71			
		δ	1	1.031	69.95			
	Control	β	0	.0456	3.01			
	Personal income	β	-9	-75.54	-3.14			
	Noise	θ	1	.4174	4.68			
		θ	6	.5371	6.54			
		θ	7	.2859	3.17			
		θ	12	-.8946	-26.91			
Gradual/permanent	0.08 intervention	ω	0	-92.69	-2.44	23	98	3843
		δ	1	.2855	.89			
	APS intervention	ω	0	-11.17	-1.39			
		δ	1	.9753	26.10			
	Control	β	0	.0444	3.00			
	Noise	θ	1	.5248	6.40			
		θ	6	.5650	7.05			
		θ	7	.3466	3.79			
		θ	12	-.8879	-27.52			
Gradual/permanent with covariate	0.08 intervention	ω	0	-10.28	-1.27*	24	88	3652
		δ	1	.9804	29.86			
	APS intervention	ω	0	-59.77	-2.11			
		δ	1	-.9562	-26.74			
	Control	β	0	.0429	2.90			
	Personal income	β	-9	-62.82	-2.77			
	Noise	θ	1	.4125	4.57			
		θ	6	.5579	7.15			
		θ	7	.2988	3.30			
		θ	12	-.8837	-26.49			
Abrupt/permanent	0.08 intervention	ω	0	-95.24	-2.55	23	100	4026
	APS intervention	ω	0	10.88	.29			
	Control	β	0	.0506	3.31			
	Noise	θ	1	.4485	5.26			
		θ	6	.4905	5.84			
		θ	7	.2935	3.22			
		θ	12	-.8863	-27.88			
Abrupt/permanent with covariate	0.08 intervention	ω	0	-74.32	-2.06	22	90	3619
	APS intervention	ω	0	-41.79	-1.09			
	Control	β	0	.0561	3.67			
	Personal income	β	-9	-66.06	-2.80			
	Noise	θ	1	.4411	5.08			
		θ	6	.4692	5.69			
		θ	7	.3202	3.65			
		θ	12	-.8958	-26.52			

Note. To adjust for monthly trend in the data, it was necessary to difference both the SVNM and multiple-vehicle daytime series at lag 1. Similarly, the personal income covariate series was independently differenced at lag 1 prior to analysis. The lag value -9 for the personal income series indicates that it was shifted backward nine 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

Residual mean square

Approaches minimal statistical significance *(p* = .102)

Single-Vehicle Nighttime Male (SVNM) Fatal and Severe-Injury Accidents

Series characteristics. SVNM FS accidents are plotted in Figure 12.1. This series shows a slightly increasing pattern through mid-1990, followed by steady declines throughout the remainder of the series. Multiple-vehicle daytime (MVD) FS accidents, plotted in Figure 12.2, show a pattern of first sharp, then smaller increases through 1989. Beginning in 1990, the series began a rapid decline which continued through the first few months of 1992, when they again rose for a period of four months only to drop again, this time to an all-time low in January 1993. The last 10 months of the series concludes with another pattern of slow increases. With some differences occurring primarily prior to 1990, these plots are similar to those of SVNM FI accidents and MVD FI accidents in their overall pattern of trending.

The proportions of total FS accidents which were SVNM are presented in Figure 12.3. In this evaluation, SVNM FS accidents comprised a somewhat larger proportion of total FS accidents (9.8%) than did SVNM FI accidents of their total category *(5.5%)*. These data demonstrate a fairly flat pattern through late-1989, when the expected end-of-the-year decrease did not occur; instead, SVNM FS accidents remained proportionately high until January/February 1990, when they dropped briefly, only to return to a high rate culminating in their highest level in August 1990, one month after implementation of the APS law and seven months after the 0.08% law. Subsequent to this highest level, the series began a fairly rapid decline through 1992, when the proportions rose slightly again.

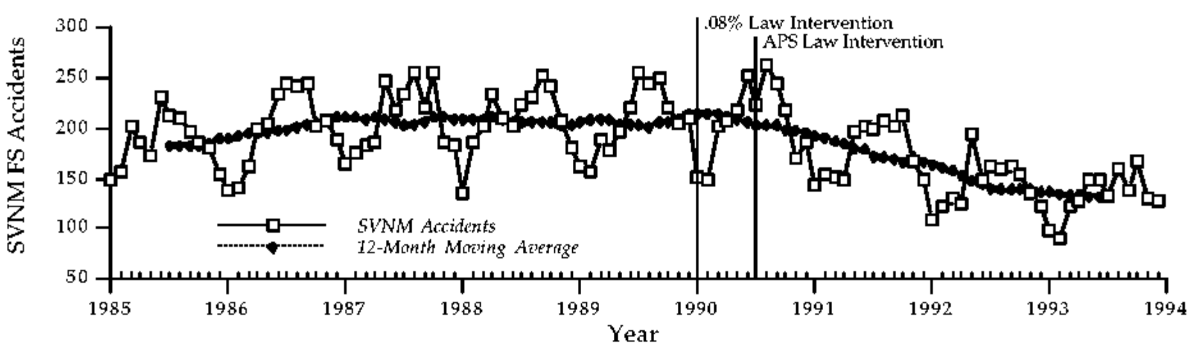

Figure 12.1. California single vehicle nighttime, male (SVNM) fatal and severe-injury (FS) accidents by month, 1985-1994.

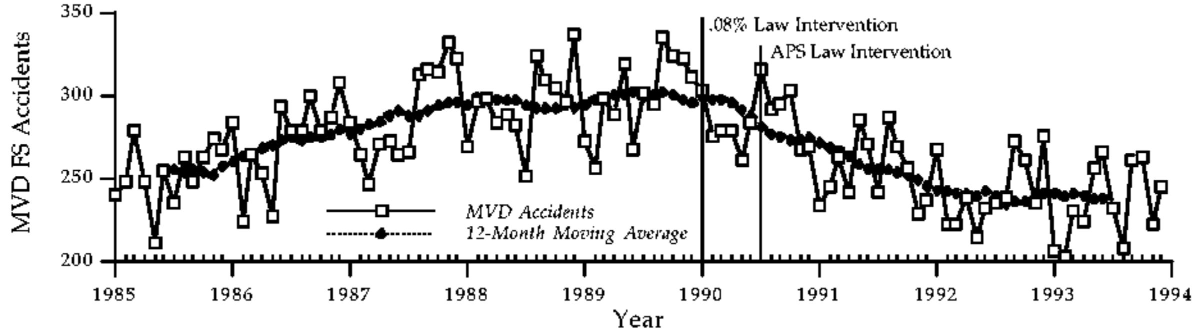

Figure 12.2. California multiple vehicle daytime (MVD) fatal and severe-injury (FS) accidents by month, 1985-1994.

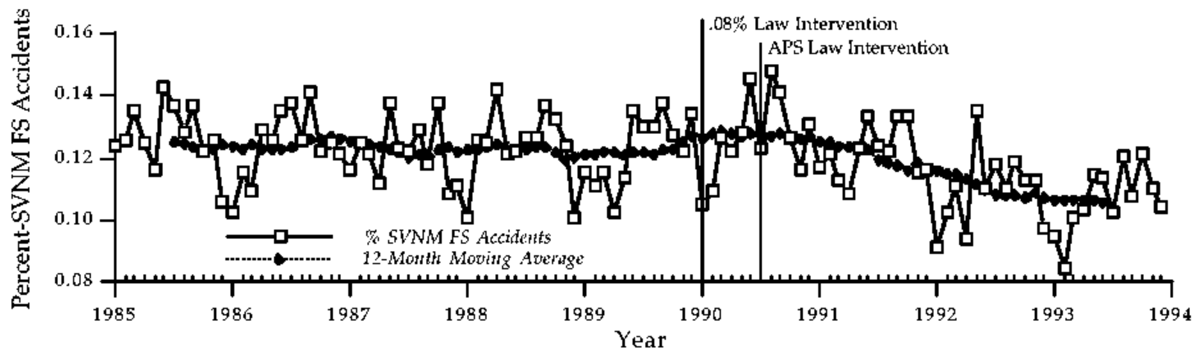

Figure 12.3. California single vehicle nighttime male (SVNM) fatal and severe-injury (FS) accidents as a proportion of total fatal and severe-injury accidents by month, 1985-1994.

Time series analysis. Model statistics and their associated diagnostics for SVNM FS accidents are presented in Table 14. The initial pattern of cross-correlations between the potential covariates and the SVNM FS accident series indicated that none of the covariate series would reduce the dependent series error variance beyond what could be explained using the low-alcohol control series alone. Consequently, no covariates were included in the full models. As before, all noise parameters in these models were considered stable and the residuals were best represented by a white noise process. The control scaling coefficient β was positive and statistically significant for each intervention model obtained.

Intervention effects of the 0.08% and APS laws. Table 14 shows that none of the estimated models were statistically significant resulting from the three modeling strategies associated with either law intervention. Although nonsignificant, in the second modeling stage, the parameter estimates for the gradual/permanent effect model for the 0.08% law intervention were weakly suggestive of an effect (t = -1.17, p = .12). Were the effect real, this model would suggest a reduction of only 1.82 accidents (less than a 1% drop) immediately following implementation of the 0.08% law with an extremely gradual rate change (δ = .9953) indicating that the maximum change in level would be realized over a very long period and would, at most, amount to a reduction of only 5%.

Table 14

California Single-Vehicle Nighttime Fatal/Severe-Injury Accidents Involving Male Drivers Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Multiple-Vehicle Daytime 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	0.08 intervention	ω	0	-8.494	-1.03	28	89	463.30
		δ	1	-.9209	-6.93			
	APS intervention	ω	0	-.0047	-.08			
		δ	1	-1.210	-3.69			
	Control	β	0	.1883	2.46			
	Noise	θ	4	.5158	5.80			
		θ	12	-.2528	-2.35			
		ϕ	1	-.5244	-5.71			
		ϕ	7	-.4789	-5.26			
Gradual/permanent	0.08 intervention	ω	0	-1.820	-1.17*	27	86	425.26
		δ	1	.9953	33.06			
	APS intervention	ω	0	-.0188	-.07			
		δ	1	-1.185	-4.43			
	Control	β	0	.1524	2.20			
	Noise	θ	4	.5189	6.15			
		θ	12	-.2740	-2.66			
		ϕ	1	-.5168	-5.80			
		ϕ	3	-.2274	-2.32			
		ϕ	7	-.5091	-6.06			
Abrupt/permanent	0.08 intervention	ω	0	-13.50	-.91	28	92	453.46
	APS intervention	ω	0	-12.19	-.81			
	Control	β	0	.1907	2.48			
	Noise	θ	4	.4721	5.20			
		θ	12	-.2708	-2.64			
		ϕ	1	-.5535	-6.43			
		ϕ	7	-.4812	-5.57			

Note. To adjust for monthly trend in the data, it was necessary to difference both the SVNM and multiple-vehicle daytime series at lag 1.

[a]Ljung-Box *Q* statistic

[b]Residual mean square

Estimated effect presents suggestive evidence of approaching significance (p = .12, one-tailed test).

Single-Vehicle Nighttime Male (SVNM) Fatal Accidents

Series characteristics. Figure 13.1 presents a plot of SVNM fatal accidents. With some degree of variability, as indicated by the fluctuations in the data and even in the moving average, between 1985 and 1989, SVNM fatal accidents dropped slightly followed by accelerated decreases from 1990 through 1992, when these accidents again began to rise. Conversely, Figure 13.2 shows that MVD fatal accidents exhibit a steady increase between 1985 and 1989 followed by an equally steady decline through 1991, with increases thereafter. Although both of these series show a certain amount of variability, neither series showed statistically significant seasonal trends.

The proportion of total fatal accidents which were SVNM is plotted in Figure 13.3. These data demonstrate a fairly flat to downward trending pattern through mid-1988, when the proportions rise slightly until 1991, when they begin another down turn until a slight rise in 1993. In this evaluation, SVNM fatal accidents represented a 27.09% subset of SVNM FS accidents. Consequently, its not surprising that the overall pattern and relative proportions of these SVNM fatal accidents are consistent with those of the SVNM FS accidents presented in Figure 12.

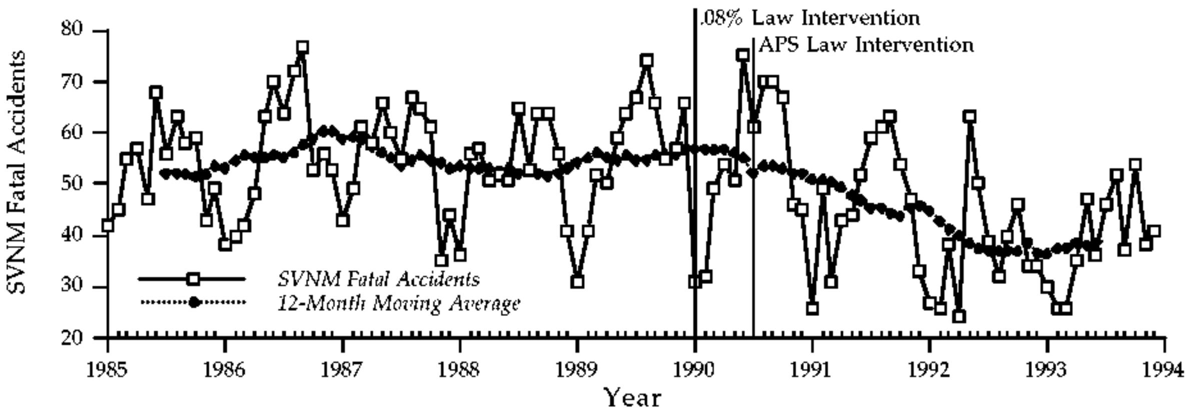

Figure 13.1. California single-vehicle nighttime, male (SVNM) fatal accidents by month, 1985-1994.

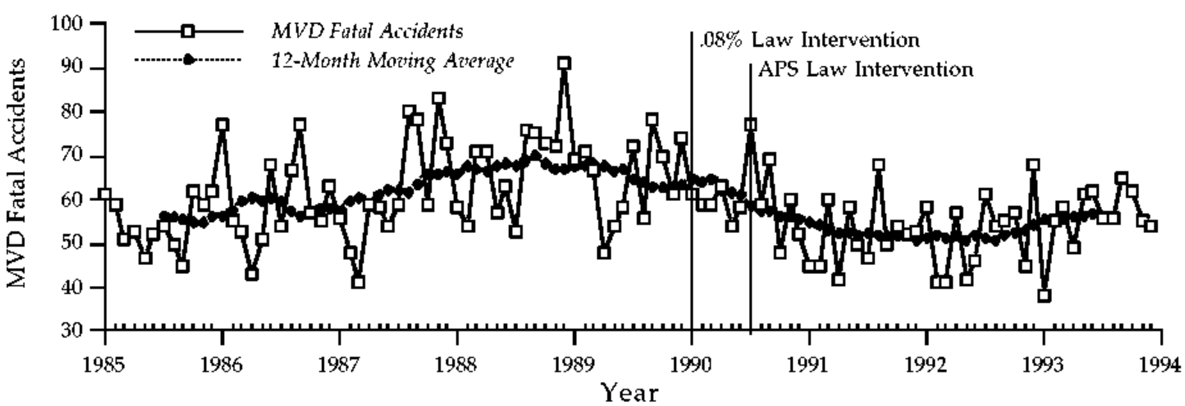

Figure 13.2. California multiple-vehicle daytime (MVD) fatal accidents by month, 1985-1994.

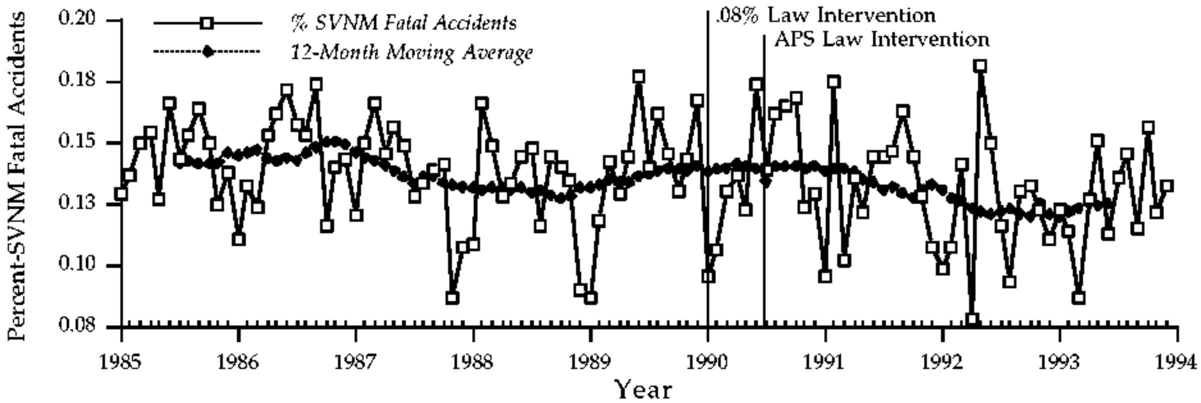

Figure 13.3. California single-vehicle nighttime male (SVNM) fatal accidents as a proportion of total fatal accidents by month, 1985-1994.

Time series analysis. Table 15 presents model statistics and diagnostics from the time series analysis of SVNM fatal accidents. As before, all noise parameters and their associated diagnostics for SVNM fatal accidents were considered stable and the series residuals were best represented by a white noise process. The table shows that the control scaling coefficient β was positive and statistically significant for all tests, confirming the value of including the MVD fatal accident series in the ARIMA models as a means of significantly reducing otherwise unexplained variation in the treatment series.

None of the four potential covariate series was found to be significantly cross-correlated with the dependent variable; that is, none provided further systematic variance reduction beyond that provided by the MVD fatal accident control series. Consequently, none was included in the final time series models used for assessing the intervention impacts on SVNM fatals because doing so would not have significantly improved the predictive ability of the transfer function to detect an intervention effect.

Intervention effects of the 0.08% and APS laws. Table 15 indicates that the null hypotheses failed to be rejected for all of the time series intervention effects tested among SVNM fatal accidents, since with the exception of the APS intervention parameters in the abrupt/temporary effect, the parameter estimates failed to reach even a minimal level of statistical significance. While the APS intervention parameters in the abrupt/temporary effect model were significant, the large negative δ parameter precluded the effect from being associated with the introduction of the new law. Consequently, using MVD fatal accidents as a control, the three-stage time series modeling strategy failed to reveal a statistically significant change in SVNM fatal accidents associated with either the timing of the 0.08% law or that of the APS law shortly thereafter.

Table 15

California Single-Vehicle Nighttime Fatal Accidents Involving Male Drivers Time Series Model Statistics for Implementation of 0.08% BAC and APS Legislation Intervention Effects

Multiple-Vehicle Daytime 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	0.08 intervention	ω	0	-15.42	-1.41	24	100	146.17
		δ	1	.2544	.36			
	APS intervention	ω	0	-12.54	-1.86			
		δ	1	-.8192	-6.86			
	Control	β	0	.7974	22.16			
	Noise	θ	1	-.3205	-3.36			
		θ	13	-.4482	-4.94			
Gradual/permanent	0.08 intervention	ω	0	1.667	.68	20	100	150.77
		δ	1	.9093	1.12			
	APS intervention	ω	0	-.9798	-.13			
		δ	1	.9684	14.55			
	Control	β	0	.7688	15.27			
	Noise	θ	1	-.2731	-2.61			
		θ	2	-.2152	-2.00			
		θ	13	-.3554	-3.43			
Abrupt/permanent	0.08 intervention	ω	0	-.6659	-.10	20	102	150.76
	APS intervention	ω	0	1.136	.16			
	Control	β	0	.7678	14.99			
	Noise	θ	1	-.2911	-2.84			
		θ	2	-.2278	-2.12			
		θ	13	-.3846	-3.89			

[a]Ljung-Box Q statistic
[b]Residual mean square

Accidents prevented by the 0.08% and APS laws

Table 16 presents a summary of the estimated number of accidents prevented by implementation of the 0.08% BAC and APS laws in California, as computed from the preceding time series models. This table also presents the estimated percent reduction for each accident measure relative to its preintervention mean. Because a separate reduction is presented corresponding to the onset of each of the two interventions in total disregard for the large, but indecipherable, overlap of the accident reductions, the reader is cautioned not to attempt to sum the reductions within a given analysis. The laws' effects as presented are not independent. Furthermore, these estimates are subject to the same assumptions underlying the associated time series model and, in particular, they are vulnerable to the duration specified by the model effect hypothesis. The computation of the number of accidents prevented is based directly on the magnitude of the intervention parameters. Given the uncertainties involved in estimating and extrapolating trends, there is considerable doubt about the actual size of the effect over time even though there is compelling evidence that the effect is significant. Another complication which will be discussed later stems from the different form of the intervention parameters (abrupt/permanent, gradual/permanent etc.).

Table 16

Estimated Accident Reductions and Effect Durations Associated with the Implementation of 0.08% BAC and APS Legislation

Accident Measure: Severity level	Exogenous covariate series	Intervention Type									
		0.08%B•> BAC per se law					Administrative per se law				
		Estimated initial monthly accident decrease	t-value	p-value (one-tailed test)	Estimated effect type	Estimated 1-year % reduction from prelegislation mean	Estimated initial monthly accident decrease	t-value	p-value (one-tailed test)	Estimated effect type	Estimated 1-year % reduction from prelegislation mean
Had-Been-Drinking:											
Fatal/Injury	Control only*	NS					NS				
Fatal/Severe Injury	Control only	NS					75.35	-2.41	.008	A/P	13.4%
	Covariate(s)	NS					53.10	-1.70	.045	A/P	9.4%
Fatal	Control only	NS					20.83	-1.31	.095	A/P	12.7%
	Covariate(s)	NS					NS				
Nighttime:											
Fatal/Injury	Control only'	NS					NS				
Fatal/Severe Injury	Control only	NS					6.48	-3.29	<.001	G/P	11.6%
	Covariate(s)	41.38	-1.38	.084	A/P	7.2%	61.40*	-1.97	.025	A/P	10.7%
Fatal	Control only	NS					NS				
	Covariate(s)	NS					NS				
2-3 a.m. (Bar Closing Hour):											
Fatal/Injury	Control only	43.17	-1.83	.034	A/P	10.2%	7.69	-1.92	.027	G/P	15.5%
	Covariate(s)	NS					NS				
Fatal/Severe Injury	Control only'	11.14	-1.30	.097	A/P	16.5%	NS				
Fatal	Control only	NS					NS				
	Covariate(s)	NS					2.25	-1.26	.104	A/P	12.5%
Single Vehicle Nighttime Male:											
Fatal/Injury	Control only	NS					11.17	-1.39	.082	G/P	10.1%
	Covariate(s)	10.28	-1.27	.102	G/P	9.4%	NS				
Fatal/Severe Injury	Control only^	NS					NS				
Fatal	Control only'	NS					NS				

No additional covariates were found to improve error reduction beyond that of the control series.
NS Either none of the effect models predicted statistically significant accident reductions or the effect parameters were found to be unstable.
G/P Gradual / permanent intervention effects.
A/P Abrupt / permanent intervention effects.
While the figures presented here are from the abrupt/permanent effect hypothesis, the estimated gradual/permanent intervention effect estimate with the covariate added approached marginal significance (*p* = .117) and estimated a 12% accident reduction one year subsequent to APS. See Table 8 for these specific parameter estimates.

In general, Table 16 reveals some evidence of significant reductions in alcohol-related accident measures associated with the implementation of both the 0.08% BAC and APS laws in California. Given the conceptual concerns expressed in the Method section regarding the inclusion of the covariate series, it cannot be assumed that those analyses which include a covariate provide a truer indication of the intervention impact. Among the analyses where a covariate series was included, the largest monthly reduction in accident measures is an estimated 53 fatal and severe injury had-been-drinking accidents prevented by implementation of the APS law. The largest proportional decrease (-16.5%) was found for 2 - 3 a.m. fatal and severe injury accidents, with the reduction being associated with the 0.08% BAC law. Significant reductions were most evident among the fatal plus severe injury accident combined measures and are stronger and more consistent for APS than the 0.08% law. While the relative dearth of significant findings among fatal, and fatal plus all injury accidents may be attributable to lack of statistical power (for fatal) or alcohol-involvement (for fatal and injury), the preponderance of nonsignificant results in these analyses serves to underscore the somewhat marginal nature of the effects. Given the marked long term decline in alcohol-related accident measures over the past decade, however, it is perhaps not surprising that the small but significant effects of these laws would tend to become obscured in the overwhelming downward trend.

Long term reduction estimates are not presented in Table 16 because there is no compelling reason to believe that the small magnitude interventions assessed here would actually produce measurable long term reductions independent of the greater social forces apparently underlying the overwhelming downward trend persisting throughout most of these series.

Intervention time series analyses of DUI arrests

Series Characteristics

DUI and total arrests are presented in Figures 14.1 and 14.2, respectively. Again, as with most of the dependent accident series and their controls, the differences in volume are reflected in the different scales of the vertical axes. While the number of total arrests rose steadily between 1985 and mid-1990, DUI arrests remained fairly stable for most of the same period except in late 1989 (just prior to the DUI legislation) when the volume of DUI arrests rose and remained at a high level until late 1990, several months after the law's introduction. This high in DUI arrests was followed by an unprecedented, rapid decline throughout the remainder of the series, with some indication of tapering off during 1993.

The rapid decline in DUI arrests roughly parallels that of total arrests as indicated in Figure 14.2. Proportionately, Figure 14.3 show that DUI arrests were declining among total arrests until 1989 when they show a marked increase until they began a rapid decline in 1991.

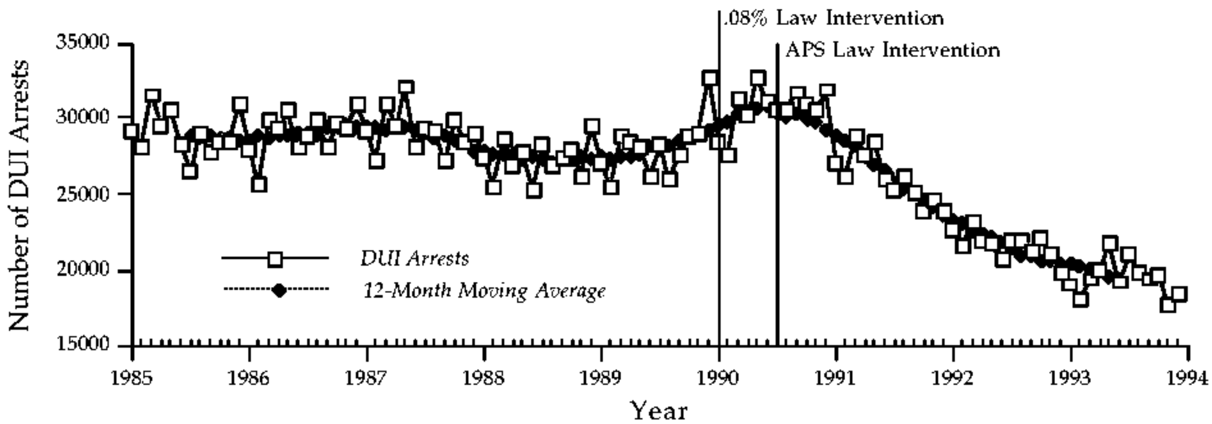

Figure 14.1. California DUI arrests by month, 1985-1994.

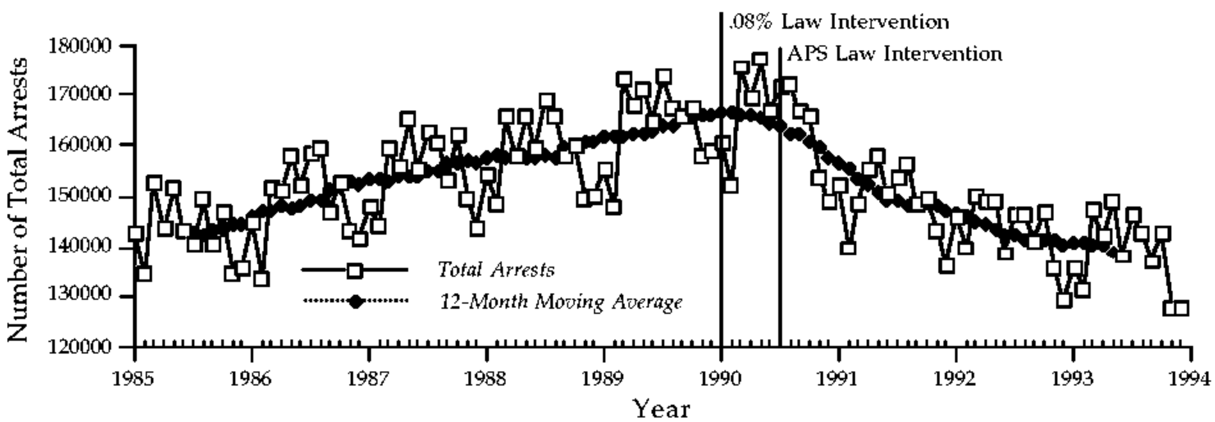

Figure 14.2. California total arrests by month, 1985-1994.

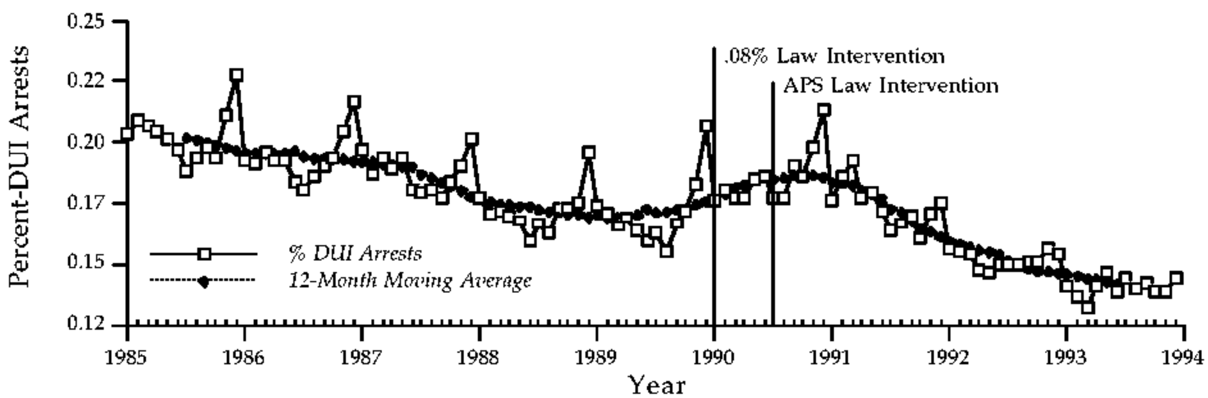

Figure 14.3. California DUI arrests as a proportion of total arrests by month, 1985-1994.

Time Series Analysis

Table 17 presents model statistics and diagnostics from the time series analysis of DUI arrests. The table shows that the decline in DUI arrests following the APS legislation was found to be statistically significant when modeled as an abrupt/permanent effect ($t = -1.73, p = .08$). Unlike the accident assessments, a two-tailed test of significance was employed here since it was conceivable that implementation of these laws might have caused a greater or reduced volume of DUI arrests. The drop of 1,144 arrests represents a 4.0% decrease from the preintervention mean of 28,620 monthly DUI arrests. The estimated decreases associated with the timing of the 0.08% law intervention were not statistically significant for any of the intervention hypothesis models.

Table 17

California DUI Arrests Time Series Model Statistics for
Implementation of 0.08% BAC and APS Legislation Intervention Effects

Total Arrests 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	-785.7	-1.07	22	72	847691
		δ	1	.8049	1.15			
	APS intervention	ω	0	-210	-.45			
		δ	1	-.6416	-.67			
	Control	β	0	.1393	5.83			
	Noise	θ	1	.2936	2.58			
		θ	3	-.3894	-3.49			
		θ	12	.5858	5.47			
		ϕ	13	-.4271	-4.05			
Gradual/permanent	.08 intervention	ω	0	-677.2	-.93	23	73	819975
		δ	1	-.5789	-.96			
	APS intervention	ω	0	-1069	-1.47			
		δ	1	.1218	.19			
	Control	β	0	.1460	6.14			
	Noise	θ	1	.2965	2.66			
		θ	3	-.3972	-3.64			
		θ	12	.5444	5.26			
		ϕ	13	-.4271	-4.04			
Abrupt/permanent	.08 intervention	ω	0	-664.7	-.94	21	75	797210
	APS intervention	ω	0	-1144	-1.73			
	Control	β	0	.1380	6.44			
	Noise	θ	1	.2863	2.62			
		θ	3	-.4117	-3.91			
		θ	12	.5661	5.66			
		ϕ	13	-.4748	-4.57			

Note. To adjust for monthly trend and to stablize annual trend in the data, it was necessary to difference both the series of DUI arrests and total arrests at lags 1 and 12. Shading indicates a statistically significant ($p<.10$) and acceptable intervention effect.

[a]Ljung-Box Q statistic

Residual mean square

Impact of the APS media campaign on regional HBD accidents

Monthly HBD accident figures for drivers in the four counties (Los Angeles, San Diego, San Francisco, and Sacramento) which were targeted for focused media campaigning were combined to form aggregated categories of HBD FI, HBD FS, and HBD fatal accidents. The combined monthly HBD FI accidents are plotted in Figure 15.1. Figure 15.2 presents the comparable HBD FI accident totals for drivers in all other counties of the state. This same aggregation of HBD FS accidents involving drivers in the four media-target counties and all other counties are plotted in Figures 16.1 and Figure 16.2, respectively. HBD fatal accident totals, similarly aggregated, are plotted in Figures 17.1 and 17.2, respectively.

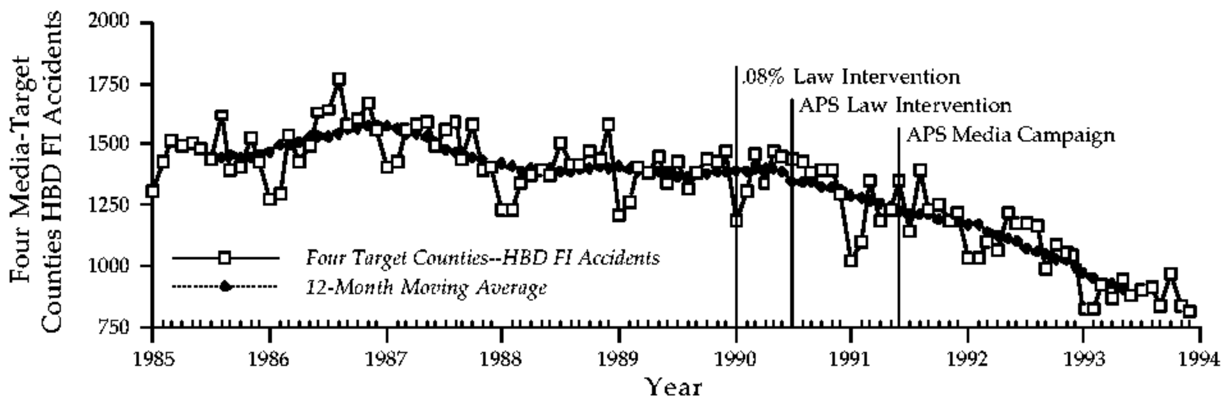

Figure 15.1. California four media-target counties "had-been-drinking" (HBD) fatal and injury (FI) accidents by month, 1985-1994.

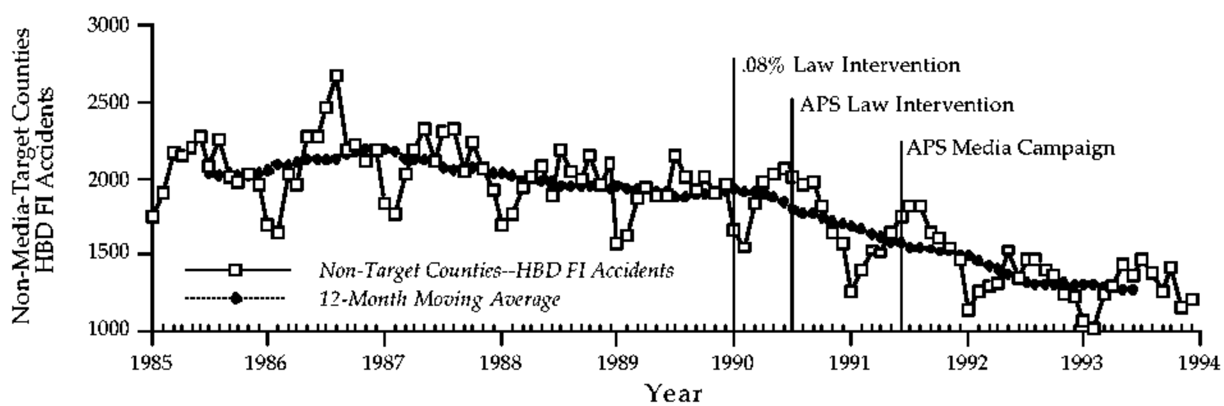

Figure 15.2. California non-media-target counties "had-been-drinking" (HBD) fatal and injury (FI) accidents by month, 1985-1994.

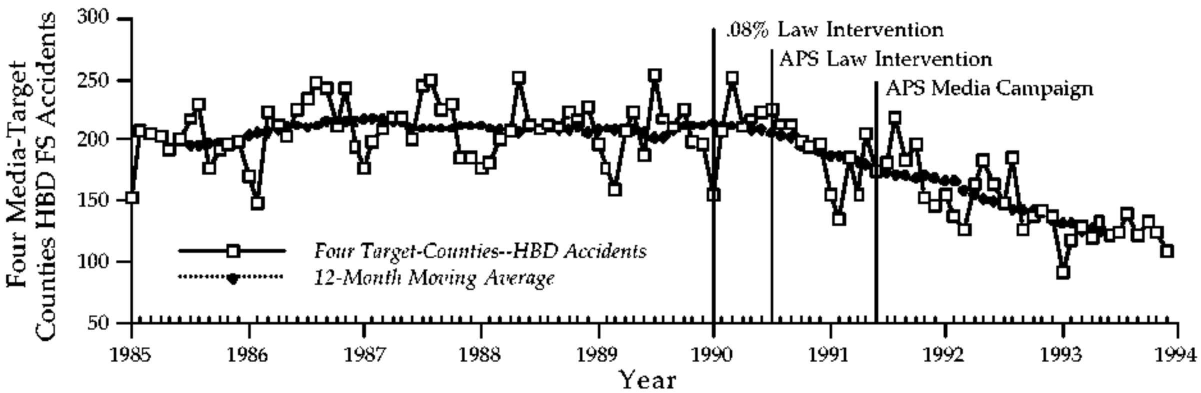

Figure 16.1. California four media-target counties "had-been-drinking" (HBD) Fatal and Severe-Injury (FS) accidents by month, 1985-1994.

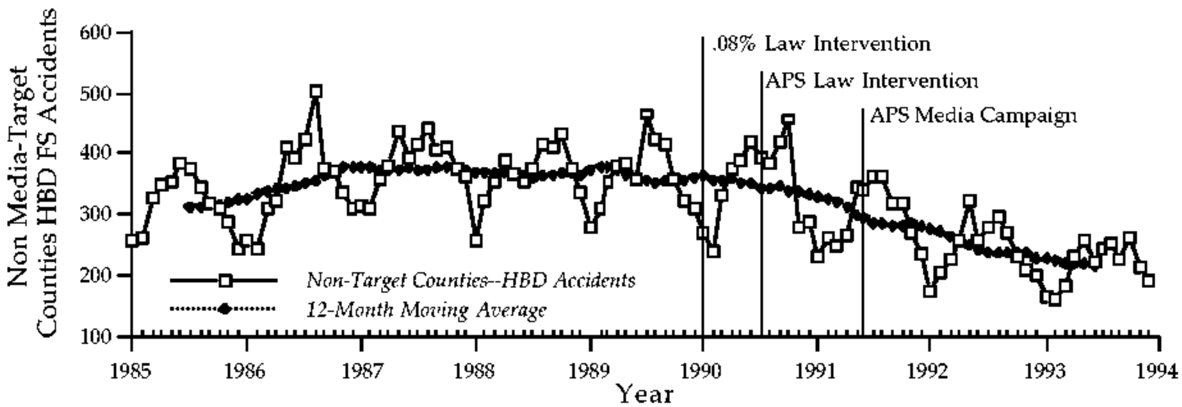

Figure 16.2. California non-media-target counties "had-been-drinking" (HBD) fatal and severe-injury (FS) accidents by month, 1985-1994.

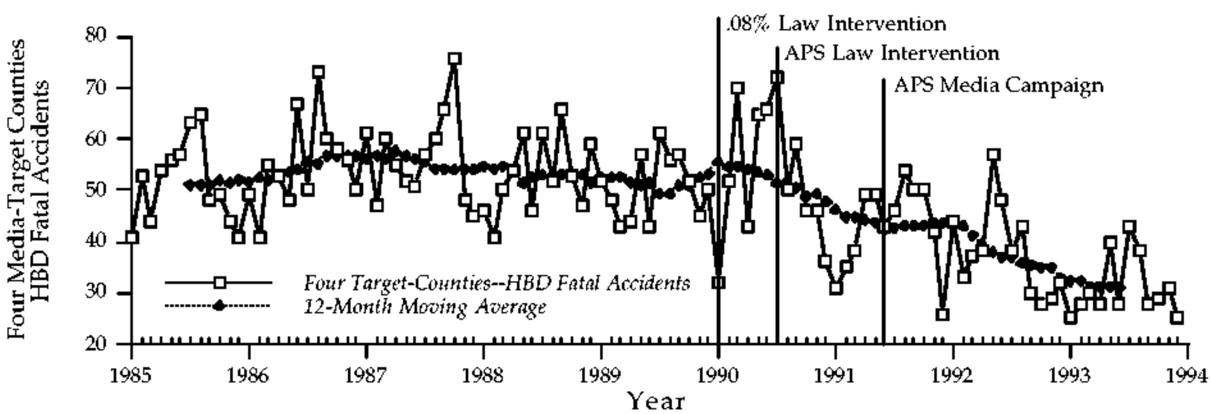

Figure 17.1. California four-media-target counties had-been-drinking (HBD) fatal accidents by month, 1985-1994.

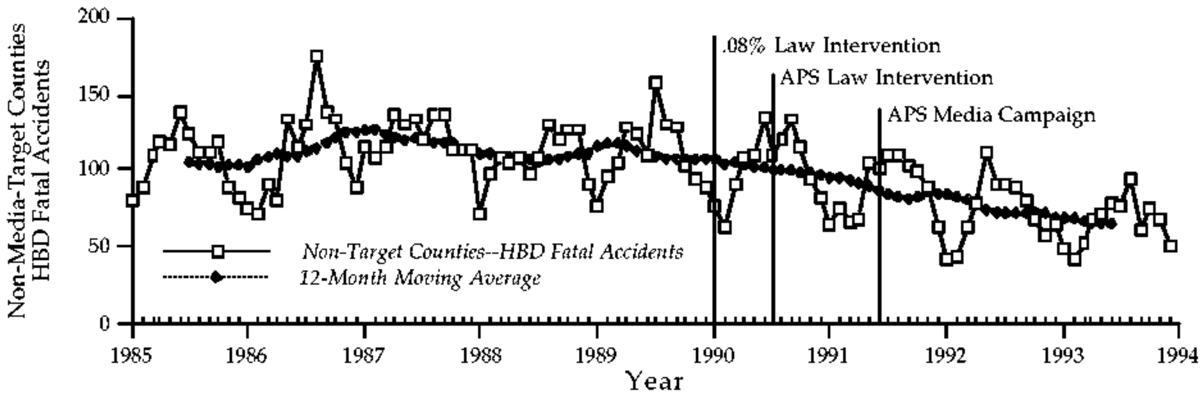

Figure17.2. California non-media-target counties had-been-drinking (HBD) fatal accidents by month, 1985-1994.

Time Series Analysis

Only the model diagnostics and statistics obtained for HBD FI accidents are presented (in Table 18) because none of the other accident series showed any significant evidence of a campaign intervention effect. However, in each analysis, all parameters were considered stable and the residuals were best represented by a white noise process. Table 18 shows that, with one exception, all of the models leading up to the abrupt/permanent model produced nonsignificant intervention parameters associated with both of the DUI laws and the media campaign. The single exception is the indication of an accident increase and large negative δ parameter associated with the abrupt/temporary model of the APS publicity campaign. As with the other accident analyses, such an unstable oscillating effect could not be reasonably attributed to the media campaign. The analyses did produce evidence of a statistically significant (abrupt/permanent) intervention effect for the media campaign *(t = 1.80 $p<.05$),* suggesting that the cities subject to intensive publicity about the APS law experienced a reduced rate of HBD FI accidents. However, the reality of this effect must be tempered by the fact that none of the analyses produced evidence of a significant campaign intervention effect on HBD fatals or HBD FS accidents.

In addition to the intervention dates assessed in the prior analyses, June 1991 (the beginning of the media campaign), is also indicated in these plots with a separate vertical line.

All accident plots in both the four media-target counties and in the other 54 "control" counties show stable or slightly increased accidents followed by patterns of slowly downward trending accidents beginning in 1987 and accelerated decreases in 1990. The overall downward trend is particularly apparent in both HBD FI plots.

Table 18

Four Media-Target Counties "Had-Been-Drinking" Fatal/Injury Accident Time Series Model Statistics for Legislation and APS Media Publicity Campaign Intervention Effects

Non-Media-Target Counties "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	-130.9	-2.25	20	97	3337
		δ	1	-.1284	-.30			
	APS intervention	ω	0	-10.08	-1.11			
		δ	1	1.087	54.99			
	APS publicity campaign intervention	ω	0	120.8	2.91			
		δ	1	-.7098	-5.44			
	Control	β	0	.4528	12.80			
	Noise	ϕ	1	.3363	3.45			
		Mean	0	533.0	7.37			
Gradual/permanent	.08 intervention	ω	0	1.099	.13	27	96	3795
		δ	1	.9566	.96			
	APS intervention	ω	0	-16.35	-.65			
		δ	1	-1.012	-20.44			
	APS publicity campaign intervention	ω	0	4.535	-.68			
		δ	1	1.046	19.73			
	Control	β	0	.4459	14.39			
	Noise	ϕ	3	.2353	2.29			
		Mean	0	541.7	8.48			
Abrupt/permanent	.08 intervention	ω	0	-20.70	-.48	19	91	4322
	APS intervention	ω	0	1.369	.03			
	APS publicity campaign intervention	ω	0	-75.59	-1.80			
	Control	β	0	.4586	13.70			
	Noise	ϕ	1	.2598	2.45			
		ϕ	3	.3725	3.69			
		ϕ	5	.2803	2.63			
		Mean	0	513.3	7.16			

Note. Shading indicates a statistically significant ($p<.10$) and acceptable estimated intervention effect.

[a]Ljung-Box *Q* statistic

Residual mean square

DISCUSSION

General Deterrent Impact of the DUI Legislation

This study demonstrated qualified evidence of a significant general deterrent effect associated with the implementation of an administrative per se (APS) license suspension law in California and somewhat less support for such an effect associated with California's 0.08% BAC per se limit law. More specifically, the time series analyses conducted in this study identified statistically significant decreases in the incidence of seven of the 12 alcohol-related California accident categories following implementation of either or both the reduced illegal per se BAC level or the APS license suspension law just six months later. Larger proportions of the accident reductions were associated with the timing of the APS law than with the lowering of the illegal per se limit from 0.10% to 0.08% BAC. Some but not all of these reductions were significant even after accounting for additional socioeconomic factors shown to contribute to the consistent downward trend observed in both alcohol- and nonalcohol-related accident series. The duration of the apparent influence specific to the timing of the legislation varied across accident categories but, when present, generally showed that a significantly increased number of drivers were, for whatever reason, either immediately or eventually influenced to refrain from driving drunk following implementation of the two new DUI laws.

Impact of lowering the per se limit to 0.08% BAC

While there were reductions in some of the alcohol surrogate accident categories following implementation of the 0.08% law, that legislation could not be linked to any significant decreases in the direct measures of alcohol-involved accidents—"had-been-drinking" (HBD) accidents. Furthermore, none of the fatal accident series produced any evidence of a decrease associated with the 0.08% legislation. There were, however, statistically significant immediate or gradual reductions following the implementation of the 0.08% BAC law in four of the nine alcohol surrogate series: fatal and severe-injury nighttime and bar closing hour accidents, as well as fatal and injury bar closing hour and single vehicle nighttime male accidents.

The magnitude of the accident reductions associated with implementation of the 0.08% BAC law were generally smaller than those associated with the APS law. The time series analyses produced estimates of accident reductions between 7.2% and 16.5% one year after the enactment of the 0.08% law. It is noteworthy, however, that while the 16.5% reduction in fatal and severe-injury bar closing hour accidents was proportionately the study's largest accident decrease, it was only associated with a marginally significant (p = .097) effect. By the same token, it is also worth noting the fact that the largest proportional accident reduction in this study was among fatal and severe-injury bar closing hour accidents—a highly alcohol involved category.

The fact that only the surrogate measures showed evidence of any impact of the 0.08% legislation may indicate that its effect was primarily limited to individuals who generally restrict their alcohol consumption before driving anyway. Studies have shown that many driving-related abilities are impaired by the consumption of even low levels of alcohol (Moskowitz & Robinson, 1988). Consequently, if drivers who have always restricted their drinking before driving decrease that consumption even further in response to the new law, there would be fewer drivers with even low levels of impairment after its implementation. Showing less outward appearance of impairment, both before and after the new law, these individuals would consistently be less likely to be identified as "HBD" by an officer responding to an accident, and their accidents would be less often included in the HBD category.

Response to the lower BAC limit may have been moderated by the fact that the 1990 legislation did not actually introduce an illegal per se limit in California. As has already been discussed, the state's initial per se limit law was implemented in January 1982 (Assembly Bill 7-Hart). That law, enacted in combination with others which enhanced DUI penalties and restricted judicial discretion, was found to have a significant general deterrent impact (Rogers & Schoenig, 1989) when it was introduced eight years prior to the 0.08% law. The 1990 law assessed here simply modified the older existing law by reducing the per se limit from .10% to 0.08% BAC.

The results reported here differ from those of an earlier study by Research and Evaluation Associates (1991). In their univariate evaluation of the 0.08% BAC law, that study estimated a 12% drop in California alcohol-related fatal accidents linked to the implementation date of the 0.08% law, with no such drop in nonalcohol-related fatalities assessed as a separate univariate series. By contrast, this current assessment, using a multivariate technique, revealed no statistically significant effects associated with the timing of the 0.08% law among HBD fatal accidents. It did, however, produce an estimated 12.7% in HBD fatals subsequent to enacting the APS law. Even this effect, however, was rendered nonsignificant and could no longer be tied to that intervention upon adding the explanatory exposure covariate. As will be explained fully below, because the series were highly correlated, entering them separately, as Research and Evaluation Associates did, probably led to an inflated variance estimate being attributed to the intervention. Like that earlier effort, no significance was found in this study among alcohol-involved (HBD) FI accidents for either intervention. On that measure, Research and Evaluation Associates actually identified increases in such accidents in 2 out of 4 of their regional study sites.

The results of this study are more consistent with the preliminary percent-change figures obtained from the study of the five states which have currently adopted the reduced limit (NHTSA, 1994). In California, among "intoxicated" drivers with a BAC > 0.10%, NHTSA reported a decrease of only 4% compared to 11% and 31% drops in the same category in Oregon and Vermont, respectively. The measure employed in that study however neither accounted for time-dependent fluctuations nor did it employ any sort of nonalcohol control measure.

Impact of the APS license suspension law

Unlike the 0.08% law intervention, the timing of the APS law was associated with significant abrupt permanent reductions in HBD accidents. These significant reductions were found among the two categories for which BAC testing is most complete—fatal, and fatal and severe-injury accidents, making them the most sensitive alcohol categories in the evaluation. The significance of these results is somewhat tempered by the fact that the reduction in fatal HBD accidents was no longer statistically significant after the covariate (licensed drivers) was included in the assessment.

Among all categories of fatal accidents, only HBD and bar-closing hour accidents produced evidence of significant reductions in the form of abrupt permanent reductions after enacting the APS law. In the case of HBD accidents, as already mentioned, this reduction was no longer significant after adding the covariate, and in the case of bar closing hour fatal accidents the reduction became significant only upon adding the covariates, and then only at marginally significant levels (p = .095 and p = .104, respectively).

Among alcohol-surrogate accidents, the APS intervention was associated with significant gradual permanent reductions in fatal and severe-injury nighttime accidents and fatal and injury bar closing hour and SVNM accidents. The gradual permanent effect on the nighttime category of fatal and severe-injury accidents was reduced to only weak evidence of an effect upon adding the covariate. Subsequently, in accord with the three-stage hypothesis testing strategy used throughout this evaluation, the abrupt/permanent hypothesis which followed, estimated a significant reduction that was proportionately comparable to the earlier gradual reduction hypothesis but with greater statistical confidence that the overall effect was not simply due to chance.

In short, the timing of the APS legislation was associated with significant permanent reductions in each accident category for at least one, and sometimes more, of the accident severity levels assessed. However, since none of the fatal accident categories showed compelling evidence of a significant intervention effect, and only two of the fatal and severe-injury accident categories did, the results are somewhat equivocal. The time series analyses produced estimated accident reductions of 9.4% to 15.5% one year after implementing the APS law. As was the case with the 0.08% law, the largest proportional decrease was among accidents occurring between 2 and 3 a.m.

The magnitude of the significant accident reductions obtained are consistent with those reported in numerous APS studies from other states. However, the failure to obtain significant intervention effects across all severity levels, accident categories and covariate models introduces some equivocation as to the precise causal role of the California APS law.

Timing of the Two Laws

The intervention effects associated with the 0.08% BAC and APS laws have been evaluated as though they are independent of each other; however, in reality, the temporal proximity of the two laws (only six months apart) makes it very difficult to unravel the separate effects of each. From a technical standpoint, this close temporal contiguity of the two interventions creates a sensitivity problem in applying time series analysis and detecting intervention effects uniquely attributable to either of the two laws. That is, effects which appear to be associated with the APS law might be more accurately attributable to lingering effects of the 0.08% BAC law. Conversely, the earlier effects associated with the implementation of the 0.08% law might actually be reflecting anticipatory effects of the upcoming APS law. The inclusion of both interventions in the time series model provides only six months of post-intervention data for modeling the 0.08% law's distinct intervention effect, and any delayed effect of the 0.08% law would be confounded with the APS law. Finally, the effects of APS could reflect a synergistic or interactive relationship between the two laws.

In consideration of this problem, and as discussed in the Results section, several time series analyses revealed moderate to high parameter-estimate correlations between the 0.08% and APS laws, which led to performing another set of analyses in which the two interventions were assessed separately. It was reasoned that if one of the interventions is assessed without consideration for the other, too much of the variance might be falsely attributed to the one intervention examined. These subsequent analyses consistently showed that regardless of the original correlation magnitude, when the original assessment entering both interventions simultaneously had revealed significance of one or both interventions, the latter analyses resulted in significance for each intervention entered separately. Conversely, when the intervention parameter estimates in the original analysis had failed to detect a significant effect of either law, these latter analyses revealed comparable nonsignificance and relatively unchanged effect magnitudes.

While these analyses have suggested an interrelated pattern of significant effects associated with the two laws, they do not encompass all of the conceivable interdependent effects that the two laws might have had on the other. It is conceivable that it wasn't until APS was implemented that the public became aware of the fact that one could now be arrested, and have their license suspended, after drinking less than had previously been allowed. The extent to which the public was, or is, aware of the distinction between these two laws is not known. So while empirical distinctions have been made between the effects of the two laws, from a practical standpoint, the results should be considered collectively. While public awareness of the 0.08% law may have been lacking initially, as evidenced by the lack of compelling significant findings associated with this law, gradually, over the next six months, awareness might have been reinforced by the passage of yet another DUI law.

It should be noted that the *permanent* accident reductions indicated in this study, as well as the lack of consistent significance within and across accident categories, may indicate that some of the effects found here were attributable to other persistent influences and not specifically to alcohol-related factors. This argument is given credence by the strong persistent downward trends in all of the accident types assessed (both alcohol- and nonalcohol-related), and generally across most of the years evaluated.

Although the use of various "nonalcohol" control series and exogenous covariate series provided some control over the effect of latent factors, in seeking to attribute accident reductions plausibly to the new DUI laws the possibility of bias and confounding still exists. Although the inclusion of significant covariates normally decreases bias and increases precision, there are a number of limitations to their use in this study. One such limitation relates to the absence of a compelling a priori theory for hypothesizing the structure of the causal relationship between the covariate and impaired driving as measured by alcohol involved accidents. The time series technique essentially lags the covariates post hoc in order to produce the maximum relationship with the accident series. This approach capitalizes on any chance significant covariation between the series. The degrees of freedom used in locating the optional lag is not incorporated into the statistical significance of the covariate. As a result, nonsignificant covariates can be declared significant and their inclusion can spuriously remove variance from the intervention parameter which might rightfully have been attributable to the interventions.

A critical assumption underlying a causal interpretation of the legislative intervention effects on night accidents is that the relationship between exposure (amount of driving) and time of day (day versus night) has remained relatively constant over the pre- and postintervention periods. An additional assumption relating to the HBD versus non-HBD designation is that police accident investigation procedures and criteria did not change subsequent to the legislative interventions.

A shift in exposure could not be directly assessed, because mileage data by time of day are not available in California. However, estimates of total vehicle miles traveled (VMT, obtained from CHP) indicate that estimated VMT per licensed driver steadily increased between 1985 and 1991, with a slight decrease during 1991, returning again to increased rates during 1992 and 1993. Klein (1989) presented evidence that VMT estimates may be less valuable than they once were for gauging fatal accident vulnerability based on exposure. He cautioned that, "the relationship between fatalities and VMT, represented by the fatality rate, has been almost steadily decreasing over time in an evolutionary manner (as opposed to abrupt revolutionary change)." As safety countermeasures such as improved vehicle design, mandatory seat belt, child restraint, or motorcycle helmet use and the like have been introduced gradually into the traffic system (Evans, 1990; Transportation Research Board, 1994), failing to control for this traffic safety factor will inevitably lead to inflated estimates of reduction given the generally linear increases in VMT in California (CHP, 1986 through 1994), and nationwide (Klein, 1989). As already explained, the inclusion of the nonalcohol control series and additional covariates, where beneficial, was intended to provide some measure of control for such changes in the traffic system. Of course, it is not known the extent to which they actually achieved this goal.

To assess the constancy of police investigation/HBD designation policy. The HBD designation could be affected by changes in the proportion of police personnel allocated to accident investigation and the proportion of accident-involved drivers who are subjected to chemical testing. Evidence obtained from the Fatal Accident Reporting System (FARS) records (NHTSA, 1988) and from independent tabulations (Rogers, 1995) indicate that, in California, the percentage of fatal accidents in which BAC tests were administered to involved drivers (regardless of time of day) has consistently been high throughout all years covered in this evaluation. These data imply that, at least with respect to fatalities, there is no evidence that the significant decrease in HBD incidents is attributable to testing or reporting artifacts. Between 1989 and 1990 the proportion of total nighttime fatal accidents considered by law enforcement to have been alcohol-involved (HBD) decreased by 1.6%. Over the same time period, the proportion of total daytime fatal accidents considered to have been alcohol-involved decreased by 1.1%. Together, these findings provide evidence that police policy and practices were fairly consistent between 1989 and 1990 and across day and night. Given the significant intervention effects of the APS law on HBD accidents, one might expect a greater change in these proportions. The small observed change in nighttime HBD accidents from pre- to post-law years suggests that while the effects of the APS legislation on HBD fatal accidents (six months into 1990) were significant, they were not large enough to be evidenced in these comparisons.

There is considerable evidence of other changes affecting the general driving environment—both in California and nationwide—which might have contributed disproportionately to reductions in both the nighttime and daytime accident categories. Nationwide, fatal accident trends have been similar to those in California, with large decreases in total traffic fatalities occurring across all the years assessed in this study (Transportation Research Board, 1994; NHTSA, 1995). Hedlund et al. (1984) showed that the state of the economy is among the strongest factors influencing VMT, arguing that people tend to travel less, particularly on discretionary trips, when the economy worsens. But Zador et al. (1988) found alcohol-related accidents to be less affected by the economic climate than are other types of accidents. The series of monthly unemployment figures (presented in Figure 1 in the Method section) indicated that unemployment rose sharply in California beginning in 1990. While this measure did not reduce error variance in any of the time series analyses beyond what was accomplished by the respective control series employed, it still provides a strong indication that California's economy worsened at the time of the two new laws. Figure 1 showed that around the time of the interventions, gasoline sales (an index of driving exposure) also began a decreasing trend. However, as already mentioned, estimates of total VMT in California obtained from CHP indicate that the VMT rate steadily *increased* between 1985 and 1991. Taken together, this evidence suggests that while the state's economic decline, as measured by the unemployment rate and decreased gasoline sales, may provide a partial explanation for the reduction in accidents at the point of the interventions, it is somewhat offset by the conflicting estimate provided by the CHP of an *increased* VMT rate, and therefore does not fully explain the accident reductions. It is also possible that these covariate series may have accounted for variance which should have been attributed to the interventions. The covariates might have introduced a bias leading to either an underestimation of the intervention effects or the complete failure to detect a real intervention effect by removing effect variance. It may therefore be, that the findings resulting from the analyses which included an exogenous covariate series represent a conservative estimate of the effects.

One problem in this statistical evaluation is that the attempt here is to detect an alcohol-specific effect in a domain which is steadily becoming less alcohol-specific. In general, there is growing empirical evidence that alcohol use by drivers involved in fatal crashes has steadily declined since 1982. This is evidenced by the consistent downward trends found in the proportions of alcohol-related incidents presented above. Annual national statistics from FARS show consistent decreases in fatal accidents involving alcohol, between 1985 and 1994, from 52% to only 40.8%. This reduction is apparent for all age groups, but particularly among under 15-19-year-olds (NHTSA, 1995).

Limitations of the Accident Categories

The particular accident categories examined in this evaluation have historically been shown to be related to alcohol involvement. Nevertheless, as alluded to earlier, each measure has limitations as an alcohol indicator which are further complicated by recent evidence suggesting that, with the exception of a high BAC level, DUI offenders are not unlike other high crash-risk drivers (Hedlund, 1994).

The three nighttime series are limited by the fact that they are surrogate measures and not direct measures of alcohol. That is to say, a great deal of misclassification is built into these measures because some percentage of nighttime accidents will not involve alcohol and some percentage of daytime accidents will involve alcohol. Heeren et al. (1985), found no significant differences in the proportion of accidents which were alcohol-involved among five categories of nighttime proxy measures suggesting that the surrogate measures are about equal with regard to estimating alcohol involvement. They named the general category of total nighttime fatal accidents as the best nighttime accident surrogate measure of alcohol-involvement since it is the largest group and would offer the greatest statistical power. They also found that the larger category, while underestimating the actual alcohol trend, better predicted it overall than did single-vehicle nighttime crashes. Of course this is not to say that any of the alcohol surrogates are optimal alcohol categories since they do not provide the exact BAC of all of the accident-involved drivers. The surrogates used here, and, for that matter, any surrogate measure, is, at best, an imperfect measure subject to change over time.

Among nighttime fatal accidents in California, CHP annual statistics reveal that in 1989 69.7% were identified as being alcohol-involved. This percentage dropped to 68.1% in 1990 and to 67.3% in 1991. In contrast, only 45.3% of the nighttime injury accidents in 1989 were reported as being alcohol-involved. In 1990 this proportion remained about the same, and then dropped to 42.5% in 1991. While these percentages indicate that nighttime fatal accidents are more likely to be attributable to alcohol than are nighttime injury accidents, nighttime fatal accidents comprise only about 5.5% of total nighttime accidents in the years assessed. Among nighttime fatal and injury accidents combined, only about 43% to 46% were reported as being alcohol-involved.

Hedlund (1994) presented evidence that DUI offenders reported doing between 40% and 60% of their drinking in licensed establishments as opposed to drinking in private homes (18%-34%). CHP annual reports show that for the single hour following the mandatory bar closing time in California (2 to 3 a.m.), 78.2% of total fatal nighttime accidents were characterized as involving alcohol in 1989, dropping to 76.3% in 1990 and to 72.4% in 1991. By comparison, only 9.5% to 14.4% of fatal accidents in the 10 to 11 a.m. hour involved alcohol.

Historically, SVNM accidents have proven to overinvolve alcohol (Douglass & Filkins, 1974), particularly when they occur very late at night (Clark, Compton, Douglass, & Filkins, 1973). More recent evidence (FARS, 1993; Öström and Eriksson, 1993) has estimated that between 52% and 53% of single vehicle nighttime fatal accidents (compared to the 43% to 46% of nighttime FI accidents) involve alcohol. Alcohol has also been shown to be more prevalent in SVNM accidents than in multiple-vehicle accidents occurring at night (Perrine et al., 1988). The latter may involve female drivers as well, and in California a fatal drunk driving accident, regardless of who was at fault, is about seven times more likely to involve a male driver than a female driver (CHP, 1989; 1990; 1991). Taken together, this evidence suggests that, overall, the 2 to 3 a.m. and SVNM accident categories are somewhat more representative of alcohol involvement than are total nighttime accidents.

Each of these alcohol-surrogate measures are vulnerable to other latent factors such as weather conditions or other variables completely unrelated to alcohol that might differentially contribute to accidents. Use of accidents involving a specific demographic subgroup, such as young males, also constrains generality, in addition to being subject to historical changes in youth driving and drinking patterns.

HBD accidents, unlike the nighttime-surrogate measures, constitute a relatively direct measure of alcohol-related accident incidence. However, accidents reported as HBD incidents are also susceptible to a number of biases resulting from the fact that they are classified as such on the basis of an officer's subjective evaluation. Addressing this potential bias, Rogers and Schoenig (1989) stated that:

> Much of the relationship between nighttime and HBD incidents is explained by the fact that the greatest proportion of drinking occurs during nighttime hours. It follows that most traffic accidents resulting from incidents of drinking and driving should occur at night. However, a certain degree of bias may account for some of the disparity between the percentage of night as opposed to day accidents said to involve alcohol, simply by virtue of the fact that a reporting officer responding to a nighttime accident is probably more predisposed to suspect the influence of alcohol, and to test for it, than he or she is when responding to a daytime accident. Positive test results obtained from these more common nighttime tests would lead the officer to characterize more drivers involved in nighttime accidents than in daytime accidents as HBD.

Consequently, the HBD accident category may underrepresent the actual incidence of alcohol-related accidents, particularly among injury accidents, from which fewer BAC tests are obtained, and among drivers exhibiting few signs of intoxication subsequent to their involvement in an accident. Nevertheless the HBD category represents the most complete direct measure of alcohol involvement generally available and are a much better indicator of problem drinking and driving than are nighttime accidents (Peck, 1993; Peck & Gebers, 1992; Sadler & Perrine, 1984).

Finally, pertaining to accident severity, the reliability of an alcohol designation for total injury accidents is open to question because a substantial proportion of injury accidents do not result in a chemical test. The time series analyses of accidents limited to <u>serious</u> injury and fatal accidents are more likely to provide a more reliable and valid barometer of alcohol involvement because they are less vulnerable to reporting artifacts and more likely to be based on chemical tests. In addition, there is strong evidence that alcohol accidents are more likely to produce serious injuries, particularly fatalities (CHP, 1993; Perrine et al., 1988).

Impact of the 0.08% and APS Laws on DUI Arrests

Given the support expressed by law enforcement for the new DUI laws, it seems rather unlikely that the sharp decline in DUI arrests could be attributed to reduced DUI enforcement. Recall that the DUI arrest series showed an increase around the time of the new laws. In fact, the time series analyses revealed that the significance of the reduction was strongly mitigated by a similar decrease in total arrests. Comparing 1987 to 1992, the California crime index (a measure of crimes chosen for gauging fluctuations in the overall volume and rate of crime) increased 20.9% in volume, but after accounting for population growth, increased only 5.8% in rate (DOJ, 1992). Compare this to the 17.2% decrease in the rate of total arrests and the 34.3% decrease in the rate of DUI misdemeanor arrests between 1987 and 1992. The significant drop in DUI arrests is also not directly attributable to decreases in enforcement personnel. On the contrary, between 1987 and 1992, law enforcement personnel increased 13.0%. Besides the possibility that arrests were down because the incidence of DUI was down, one alternative explanation for the decreases in DUI arrests might be that law enforcement resources were shifted to other offenses. The DOJ (1992) reported 25.1% increase in the rate of arrests for "violent offenses" between 1987 and 1992 may reflect such a shift in the allocation of law enforcement resources. The possibility that the actual incidence of DUI might have decreased in California is supported by the significant decreases in the alcohol- and alcohol-surrogate accident categories.

Limitations of the Media Campaign Evaluation

The authors of several reports concerning administrative license actions have observed that the only hope these laws have of maintaining their effectiveness among potential offenders is with a continued high perceived risk of apprehension either through publicity or by actual increases in the risk of arrest (Ross, 1992; Vingilis et al., 1988). The time series analyses of the four combined media-target counties revealed little evidence of a differential impact on the target counties. However, this does not imply that the APS media campaign itself was ineffective. The control series used in these analyses was undoubtedly largely "contaminated" by the media campaign because the ultimate intent of the campaign was to impact the whole state. Furthermore, the selected intervention point, June 1991, may not have adequately pinpointed the campaign onset in all four counties.

The nonsignificant step increases in accidents observed in the four counties at the point of the campaign intervention, relative to those in California's other counties, is unlikely to have been caused by the campaign effort. Rather, it is more likely that the legislative interventions and other socioeconomic trends had more impact on accidents in these counties than did the subsequent brief campaign. Furthermore, these results may simply be due to a reduction in statistical power resulting from a much smaller number of accidents in the four-county series.

Volume 2: Specific Deterrent Effects of the APS Law

Largely evolving out of numerous studies showing the effectiveness of license suspension as a specific deterrent, California has a long history of relying on postconviction license suspension or revocation as an important DUI countermeasure. The evidence here suggests that the California APS law has been somewhat less effective in producing a general deterrent effect than was expected, given: 1) the demonstrated past effectiveness of license actions as a specific deterrent, and 2) the consistency of significant general deterrent effects of similar laws reported by other states. However, as was pointed out above, most other states which have implemented administrative license action laws have done so when their DUI rates were quite high (Feimer, 1987). In contrast, California's DUI-related accidents had been in a pattern of steady decline for several years prior to implementing APS, perhaps in part due to the effectiveness of sanctions already in place. In addition, California has historically had by far the highest rate of DUI enforcement, as measured by per capita DUI arrest rate. Consequently, it will be of some importance to identify what types of sanctions were imposed in these other states and, specifically, the extent to which they actually employed mandated license suspension or revocation as a DUI sanction prior to implementing their administrative license action law. Such an assessment will be made in Volume 2 of this evaluation. In Volume 2 we will also attempt to quantify the extent to which the two laws may have produced a specific deterrent effect. To answer this question we will evaluate recidivism rates for first and repeat offenders both before and after the new legislative interventions. As noted earlier, it is possible for a sanction to have substantial specific deterrent effects on convicted offenders but little or no effect on the general driving population.

Conclusions

We have attempted to answer the question of whether the 0.08% law or the APS law alone, or in combination, produced a general deterrent effect among the target population of potential drunk drivers. It is evident from the results presented here that the timing of these laws coincides with modest evidence of decreased alcohol-related accidents beyond that occurring as a result of other influences prevalent in the general driving environment. These effects are stronger for the timing of the APS law, which showed significant 9% to 13% declines among direct-alcohol measures and which were perhaps bolstered somewhat by the media campaign begun one year after the law's enactment. However, the strength of these findings must be qualified first, by the failure to corroborate these results with a clear pattern of comparable reductions in the alcohol-surrogate measures and second, by the tendency for the effects to be reduced by the inclusion of covariates.

The timing of the law reducing the illegal per se limit to 0.08% BAC coincides with smaller, albeit significant, reductions on some of the alcohol surrogate measures, with no compelling evidence of reductions in the more directly alcohol-related measures.

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APPENDIX

ADMINISTRATIVE PER SE PROCESS MEASURES

	7/90 - 6/91	7/91 - 6/92
Administrative per se (APS) documents received from law enforcement[a]	298,718	276,359
Total APS actions taken (including actions later set aside)	286,226	263,639
Suspensions	277,975	253,830
Revocations	8,251	9,809
Total APS actions set aside	10,440	13,816
Suspensions set aside	10,321	13,578
Revocations set aside	119	238
Net total APS actions taken (excluding actions later set aside)	275,786	249,823
Suspensions	267,654	240,252
Revocations	8,132	9,571
APS Actions by Offender Status/Occupation:[c]		
APS suspension for drivers with no prior DUI convictions[d]	190,858	172,083
4-month license suspensions	N/A	151,857
30-day suspensions plus 3-month restrictions	N/A	5,855
First-offender chemical test refusals	11,101	10,068
Total APS actions taken for drivers with prior DUI convictions	84,928	77,740
Suspensions	76,796	68,169
Revocations	8,132	9,571
Total commercial driver (CDL) APS actions taken	8,344	7,126
CDL APS first offender suspensions/restrictions	5,081	4,303
CDL APS suspensions of commercial drivers in commercial vehicles	27	41
CDL APS license revocations of commercial drivers in commercial vehicles	0	0
Total APS Hearings (BAC or Refusal):		
Total hearings scheduled	20,462	24,419
Total hearings actually held and/or completed[e]	20,165	20,413
Total suspensions sustained or upheld following a hearing	17,636	17,818
APS Chemical Test Refusal Process Measures:		
Chemical test refusal documents received from law enforcement[a]	23,438	21,012
Total APS refusal actions taken (including actions later set aside)	22,152	20,448
Suspensions	13,902	10,639
Revocations	8,250	9,809
Total APS refusal actions set aside	527	776
Suspension set aside	409	538
Revocations set aside	118	238
Net total APS refusal actions (excluding actions later set aside)	21,625	19,672
Suspensions	13,493	10,101
Revocations	8,132	9,571
APS refusal suspensions for subjects with no prior DUIs	11,101	10,068
APS refusal actions for subjects with prior DUIs	10,524	9,604
Suspensions	2,392	33
Revocations	8,132	9,571
APS refusal hearings scheduled[g]	2,925	3,287
APS refusal hearings actually held and/or completed	2,880	2,973
APS refusal actions sustained or upheld following a hearing	2,424	2,444

[a]Figure obtained from Driver Safety Review Unit Weekly APS workload summaries.

[b]Action taken on the basis of a chemical test refusal or Blood Alcohol Concentration (BAC) test result.

[c]All entries in this category exclude actions later set aside but, where possible, include actions taken on the basis of either a chemical test refusal or a BAC test result.

[d] Prior DUI convictions consist of any such conviction where the violation occurred within the seven years prior to the current violation.

[e] Seven percent of 1990/91 and nine percent of 91/92 total APS actions resulted in a hearing. Both numerator and denominator include those actions set aside as a result of the hearing.

[f] The APS suspension or revocation was upheld in 87% of the BAC or refusal hearings held, for both 90/91 and 1/92 years.

[g]APS chemical test refusal hearings represent 14% of 1990/91 and 13% of 91/92 total APS hearings scheduled.

[h]The APS action was sustained or upheld in 84% of the 1990/91 and 82% of the 91/92 chemical test refusal hearings.

ADMINISTRATIVE PER SE PROCESS MEASURES (Continued)

	7/92-6/93	7/93-6/94	7/94-6/95
Total APS actions taken (including actions later set aside)[1]	231,491	211,380	185,266
.08[2] Suspensions	223,481	200,029	169,845
.08 Revocations	8,010	7,020	5,469
.01[3] Suspensions	N/A	4,331	9,952
Total APS actions set aside	12,548	14,189	13,764
.08 Suspensions set aside	12,373	13,838	13,107
.08 Revocations set aside	175	214	216
.01 Suspensions set aside	N/A	137	441
Net total APS actions taken (excluding actions later set aside)	218,943	197,191	171,502
.08 Suspensions	211,108	186,191	156,738
.08 Revocations	7,835	6,806	5,253
.01 Suspensions	N/A	4,194	9,511
APS Actions by Offender Status/Occupation:[4]			
APS .08 suspensions for drivers with no prior DUI convictions[5]	151,752	133,166	114,365
4-month license suspensions	133,614	116,563	95288
30-day suspensions plus 3-month restrictions	5,356	5,584	4,254
30-day suspensions plus 4-month COE[6] restrictions	N/A	N/A	5,022
First-offender chemical test refusals	8,999	7,546	6,527
CDL first offender suspensions/restrictions	3,782	3,473	3,274
Total APS .08 actions taken for drivers with prior DUI convictions	67,191	59,831	47,626
Suspensions	59,355	53,025	42,373
Revocations	7,836	6,806	5,253
Total commercial driver *(CDL)* APS actions taken	6,190	5,443	4,695
CDL APS suspensions of commercial drivers in commercial vehicles	38	28	28
APS Chemical Test Refusal Process Measures:			
Total APS refusal actions taken (including actions later set aside)	17,454	15,145	12,696
.08 Suspensions	9,445	8,056	7,027
.08 Revocations	8,009	7,020	5,469
.01 Suspensions	N/A	69	200
Total APS refusal actions set aside	619	726	728
.08 Suspensions set aside	444	510	500
.08 Revocations set aside	175	214	216
.01 Suspensions set aside	N/A	2	12
Net total APS refusal actions (excluding actions later set aside)	16,835	14,419	11,968
.08 Suspensions	9,001	7,546	6,527
.08 Revocations	7,834	6,806	5,253
.01 Suspensions	N/A	67	188
Net .08 APS refusal suspensions for subjects with no prior DUIs	8,999	7,546	6,527
Net .08 APS refusal actions for subjects with prior DUIs	7,836	6,806	5,253
Total .08 and .01 APS refusal hearings scheduled	2,988	2,343	2,133
APS Hearings			
Total .08 and .01 hearings scheduled[7]	24,497	21,682	21,774
.08 hearings held and/or completed	20,587	21,264*	18,300
.01 hearings held and/or completed			888
.08 actions sustained or upheld following a hearing	16,920	15,481*	14,657[8]
.01 actions sustained or upheld following a hearing			743[9]
.08 APS refusal hearings held and/or completed	2,712	2,260*	1,836
.08 APS refusal actions sustained or upheld following a hearing	2,220	1,758*	1,326[10]

*Figure represents the combined total .08 and .01 hearings for FY93/94.

[1]Action taken on the basis of a chemical test refusal or blood alcohol concentration (BAC) test result.

[2] .08 refers to APS actions taken subsequent to obtaining evidence of a BAC equal to or in excess of the .08% per se level. Such an action is taken in conjunction with a DUI arrest.

[3].01 refers to APS suspensions taken against drivers under the age of 21 with BACs in excess of .01%.

[4]All entries in this category exclude actions later set aside but, where possible, include actions taken on the basis of either a chemical test refusal or a BAC test result.

[5] Prior DUI convictions consist of any such conviction where the violation occurred within the seven years prior to the current violation.

[6] Introduced 1/1/95 this restriction allows driving to, from, and during the course-of-employment.

[7] This figure excludes subsequent departmental review hearings or procedures. In FY93/94 10% of total APS actions resulted in a hearing and increased to 12% in FY94/95. Both numerator and denominator include those actions set aside as a result of the hearing.

[8]In FY94/95 the .08 APS suspension or revocation was upheld in 80% of the total .08 APS hearings held.

[9] In FY94/95 the .01 APS suspension or revocation was upheld in 84% of the total .01 APS hearings held.

[10]In FY93/94 the action was sustained or upheld in 78% of the combined .08 and .01 APS chemical test refusal hearings held and in FY94/95, the action was sustained or upheld in 72% of .08 APS chemical test refusal hearings