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DEPARTMENT OF MOTOR VEHICLES

AN EVALUATION OF THE EFFECTIVENESS OF CALIFORNIA DRINKING DRIVER PROGRAMS

**REPORT TO THE LEGISLATURE
OF THE STATE OF CALIFORNIA**

**IN ACCORD WITH SENATE BILL 1344
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There has been longstanding interest in determining which sanctions work best to reduce drunk driving. This study, mandated by the California Legislature (SB 1344) and funded by the California Department of Alcohol and Drug Programs (DADP), examined the degree to which first offender, SB 38 and 30-month drinking driver programs (DDPs) in California reduced drunk driving recidivism, relative to other sanctions, such as driver license suspension. Based on the findings from this study, as well as those from prior research, this report recommends combining alcohol treatment with driver license actions as the best strategy for reducing drunk driving recidivism and enhancing overall traffic safety.

Of lesser importance, but also examined, is whether changes in first offender and SB 38 DDPs (first offender programs were consolidated under DADP licensing and review, and SB38 programs were lengthened from 12 to 18 months) increased their effectiveness in reducing recidivism. The analysis found no evidence to support the hypothesis that changes in these programs increased their effectiveness.

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PREFACE

This report is the final product of an evaluation of first offender, SB 38 and 30-month drinking driver programs that was mandated by the California Legislature (SB 1344 - Seymour), and funded by the California Department of Alcohol and Drug Programs. The report was prepared by the Research and Development Section of the California Department of Motor Vehicles under the administrative direction of Raymond C. Peck, Chief. The opinions, findings and conclusions expressed in the report are those of the author and not necessarily those of the Department of Alcohol and Drug Programs or the State of California.

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Raymond C. Peck, Chief of Research, provided general direction, and Clifford J. Helander, Research Manager I, supervised this study. Both individuals made valuable contributions to the content, methodology and analysis of this project, and the author appreciates their expertise and efforts.

The author would also like to thank Helen N. Tashima, Research Analyst II, for general assistance with computer extraction programs of data in the department's driver record database, and William C. Marsh, Research Program Specialist II, and Alex Exuzides, Research Analyst II, for their help with statistical methodology. Thanks are extended to Debbie McKenzie, Staff Services Analyst, for preparing the tables and figures in the report, and for coordinating its production. Finally, the author would like to acknowledge with appreciation the support of Susan DeYoung, in whose memory this report is dedicated.

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EXECUTIVE SUMMARY

Background

The combination of alcohol consumption with automobile driving poses a major public health problem in the United States. The National Highway Traffic Safety Administration (NHTSA) reports that 39,238 people were killed in automobile crashes in the United States in 1992, and estimates that 17,699 of these were alcohol related

(NHTSA, 1993). In California, 4,185 people died in automobile crashes in 1992, and 1,832 of these fatalities were alcohol related (California Highway Patrol, 1992).

Attempts to control drinking and driving in the United States have traditionally relied upon prescribing a combination of jail, fines and license actions for persons caught and convicted of driving under the influence (DUI). A newer approach attempts to rehabilitate DUI offenders through alcohol education and treatment. Different types of alcohol programs have evolved, tailored to the degree of an individual's involvement with alcohol. In California, there are three types of alcohol programs. First offender programs, which are typically 3 months long, are prescribed for those just entering the criminal justice system with their first DUI conviction. For second and third-plus offenders, there are 18 month SB 38 alcohol programs, and for third-plus offenders living in Los Angeles or San Bernardino counties, 30-month alcohol treatment programs are available, although only those 30-month programs in Los Angeles County are currently accepting new enrollments.

During the past two decades, numerous studies have been conducted in order to determine which of the various sanctions, either alone or in combination with others, is most efficacious in curbing DUI recidivism and crashes. In California, one of the first studies evaluating the effectiveness of sanctions for DUI offenders found that those assigned to SB 38 alcohol programs had a significantly greater number of subsequent crashes and DUI convictions than a matched group of offenders who received license suspension (Hagen et al., 1978). A subsequent study, which used the same subjects but examined them over a longer post-conviction follow-up period, discovered that license suspension was the most effective overall traffic safety measure, due to its efficacy in curbing crashes, but that SB 38 alcohol programs were superior in reducing alcohol-related traffic convictions (Sadler and Perrine, 1984).

A fairly rigorous study of the efficacy of alcohol treatment programs was conducted in Sacramento, California from 1977 until 1981 (Reis, 1982a; Reis, 1982b). While the results showed that alcohol treatment was superior to a no-treatment control group in reducing subsequent DUI arrests (Stewart and Ellingstad, 1988), a reanalysis of the data using a longer follow-up period and additional variables as predictors failed to find a treatment effect (Arstein-Kerslake and Peck, 1985).

Two more recent California studies (Tashima and Peck, 1986; Tashima and Marelich, 1989), have looked at the relative efficacy of various sanctions for DUI offenders, and thus are antecedents to the present study. While the findings are somewhat complex, they can be summarized as follows. For first offenders, those who received stronger license control actions had significantly lower crash and conviction rates, while offenders sentenced to jail had the highest rates. For second offenders, those receiving license suspension had the lowest total crash rates, while SB 38 program referral was associated with the lowest alcohol-related crash and reconviction rates.

In a review of studies examining the effectiveness of various DUI sanctions in California, Peck (1991) concluded that license suspension is superior to alcohol treatment in reducing total crash risk, but that alcohol programs are more effective than suspension in reducing subsequent convictions of DUI, and possibly alcohol-related crashes. Based on these findings, a number of investigators have recommended

combining license suspension with alcohol treatment to best combat drinking and driving (Mann et al., 1983; Sadler and Perrine, 1984; Tashima and Peck, 1986; Helander, 1986; Nichols, 1990; Wells-Parker et al., in press).

Project Objectives

The present study, mandated by SB 1344 (see Appendix A), is designed to provide useful information on the efficacy of alcohol treatment in California to policy makers, so that informed decisions on traffic safety can be made. The specific evaluation issues are:

First offenders

* What are the effects of first-offender alcohol education and treatment programs on recidivism, and how does this compare to other sanctions?

* How has the consolidation of first-offender programs under the supervision of the Department of Alcohol and Drug Programs (ADP) impacted the effectiveness of such programs?

Second offenders

* What is the relative efficacy of SB 38 programs versus other court sanctions in curbing DUI recidivism?

* How have changes in the SB 38 programs impacted the recidivism rate of second DUI offenders?

Third offenders

* How effective are the 30-month alcohol treatment programs compared to SB 38 programs and other court sanctions for offenders receiving a third-or-subsequent DUI conviction?

Research Methods

Study subjects were selected from the Department of Motor Vehicles (DMV) driver record database and also, for third-plus offenders, from records kept by the Los Angeles County Office of Alcohol Programs. Demographic, prior driving history, and subsequent DUI conviction information on subjects was gathered from DMV's database. While sanctions for DUI offenders can have different effects, depending upon what outcome is being measured, SB 1344 limited the present study to an evaluation of subsequent DUI convictions.

The evaluation focused on two basic issues, and different research strategies were used to address each issue. The most important evaluation question examines, within each offender level (first, second and third-plus), the relationships between the various sanctions offenders receive and subsequent DUI convictions. The approach taken to answer this question was to sample offenders from each sanction group, and then compare subsequent DUI convictions among the groups.

The second evaluation question looks at first offender and SB 38 alcohol treatment programs, and attempts to determine whether changes in these programs (e.g.

consolidating first offender programs under ADP licensing and review, and lengthening SB 38 programs from 12 to 18 months) increased their effectiveness in curbing DUI recidivism. To answer this question, subjects were selected prior to changes in the programs (pre group), and their recidivism rate was compared to that of a second sample selected after program change (post group).

The problem confronted by the evaluation strategies is that differences observed between sanction groups on recidivism may be due to pre-existing differences among the groups, rather than to the effects of the sanctions themselves. Because it was infeasible to randomly assign offenders to different sanction groups, both evaluation strategies relied on statistical measures to help control possible bias. The two main statistical techniques used, analysis of covariance (ANCOVA) and Cox proportional hazards survival analysis, both rely on the use of variables called covariates to reduce bias. While using statistical techniques helps control this bias, it does not ensure that all sources of bias have been controlled. Thus, the results of the analyses do not prove that differences in recidivism among the groups are a function of the sanctions the groups received, as much as they portray the associations between the two.

Results and Discussion

Sanction effectiveness: First offenders

The overall ANCOVA analysis showed that there were significant differences in subsequent alcohol convictions among at least some of the 6 first-offender sanction groups. A Tukey-Kramer Multiple Comparison Procedure (MCP) test found that first offenders sentenced only to jail had the highest DUI recidivism rate, and that this rate was significantly higher than that of all other groups. The lowest rate of subsequent DUI convictions were accrued by offenders sentenced to a first offender alcohol program plus license restriction. These results are presented in Figure 1 below.

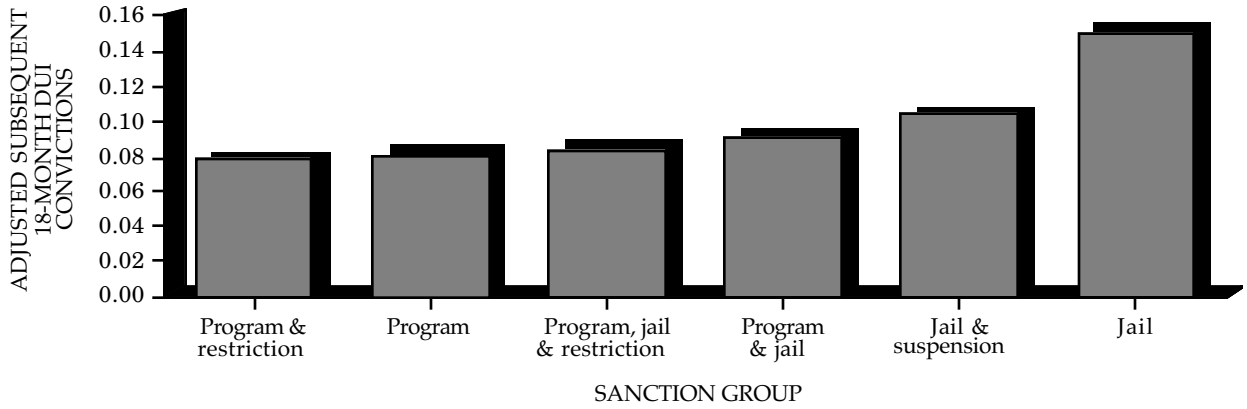

Figure 1. Adjusted subsequent DUI convictions, by sanction group: First offenders.

These results are similar to those of previous studies (Tashima and Marelich, 1989; Tashima and Peck, 1986). It seems clear that jail terms for first offenders, at least as implemented in California, are ineffective in reducing DUI recidivism. It also seems fairly convincing, based on the results of this study as well as prior ones, that first offender alcohol programs are associated with reductions in DUI recidivism, and that traffic safety is enhanced when this is combined with license curtailment (restriction or suspension).

Sanction effectiveness: Second offenders

The main ANCOVA analysis found significant differences in DUI recidivism among at least some of the 3 second-offender sanction groups. The specific group differences were revealed by the Tukey-Kramer MCP test, which showed that second offenders receiving only license suspension had the highest risk of recidivating, and that this group's risk was significantly higher than the SB 38 plus suspension and the SB 38 plus restriction groups. The average recidivism figures for the two SB 38 program groups were very similar, and not significantly different from each other. These results are presented in Figure 2.

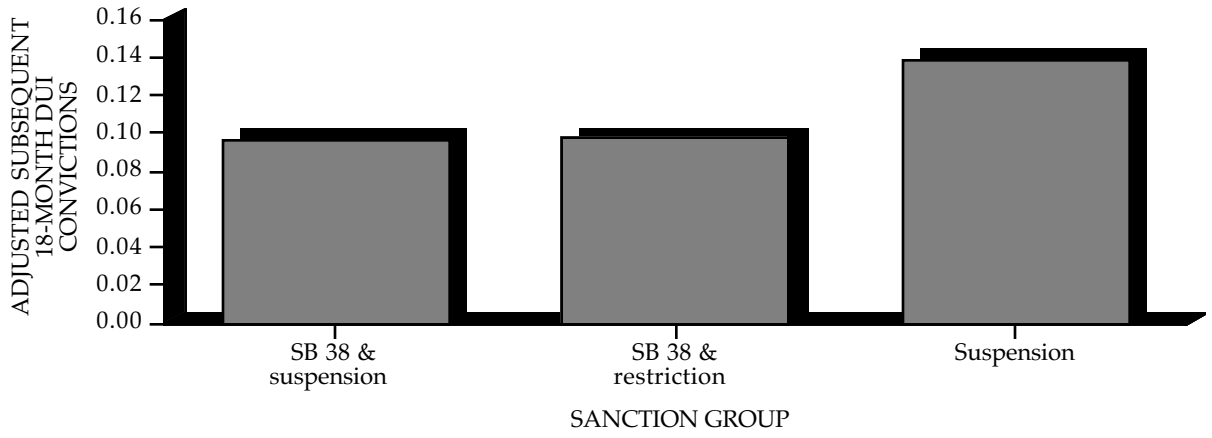

Figure 2. Adjusted subsequent DUI convictions, by sanction group: Second offfenders.

These findings are supported by the results of prior research (Sadler and Perrine, 1984; Tashima and Peck, 1986; Tashima and Marelich, 1989). It appears, based on the results of all studies considered together, that SB 38 plus license restriction is more effective in reducing DUI recidivism than license suspension alone, and that this superiority of SB 38 plus license restriction has increased over time. Thus, it appears that the combination of alcohol program plus license curtailment is the best current strategy for reducing DUI recidivism. It should be pointed out that the relative effectiveness of second offender sanctions on other important outcomes, such as crashes, can not be addressed by this study, because its legislative mandate was limited to an examination of DUI recidivism.

Sanction effectiveness: Third-plus offenders

The Cox proportional hazards survival model was used to examine the relationships between the 3 third-plus offender sanction groups and recidivism, as measured by the number of days to first subsequent DUI arrest which resulted in conviction. The results showed that while recidivism was directionally higher for the SB 38 plus license revocation group than for the 30-month plus license revocation group, this difference was not statistically significant. In contrast, those offenders who received only license revocation had the highest recidivism rate, and this rate was significantly higher than the corresponding rate for those assigned to a 30-month alcohol program. Persons receiving only license revocation had 1.7 times the risk of recidivating than others who were assigned to 30-month programs.

Because of low statistical power resulting from the small number of 30-month program subjects, it is unclear whether there actually is no difference in recidivism between the SB 38 and 30-month program groups, or whether a small difference exists but was undetected in the analysis. This needs to be explored further through subsequent research. Much more confidence can be placed in the finding that the recidivism rate of the revoked group is significantly higher than that of the 30-month plus revocation group. This isn't surprising, given the results of the second offender analysis, which demonstrated the superiority of SB 38 program assignment over license suspension alone.

Program change: First offender programs

The results of the ANCOVA analysis of first offender program changes showed that there were differences in DUI recidivism between the program and other sanction groups, and that there were also differences in recidivism between the pre and post time periods. However, it was hypothesized that there would be a greater change from pre to post for the program group than for the other sanction group, due to changes in the first offender programs, but this hypothesis was not supported by the data.

It is unclear why the analysis failed to find evidence of enhanced program effectiveness, although one explanation is that the programs were examined shortly after the change took place, perhaps too soon for change to have occurred. It is also possible, although unlikely, that the evaluation strategy, while the best available under the circumstances, simply didn't find enhanced program effectiveness that in fact exists. Whatever the explanation, there is no evidence to support the hypothesis that the changes these programs underwent increased their effectiveness in curbing DUI recidivism.

Program change: SB 38 programs

The ANCOVA analysis evaluated whether lengthening SB 38 programs from 12 to 18 months increased their effectiveness in reducing DUI recidivism. The results from this analysis found that changes in recidivism between the 12-month SB 38 programs and the 18-month SB 38 programs were not significantly different than changes in recidivism for the other sanction groups. In other words, there is no evidence to

support the hypothesis that adding 6 months to SB 38 programs enhanced their effectiveness.

While this finding is surprising, it does find support in the literature. A recent scientific review (called a meta-analysis) of more than 200 studies found that neither the duration of an educational/treatment intervention, nor its number of hours, were significantly correlated with DUI recidivism (Wells-Parker et al., in press). While changes in SB 38 programs may have positively impacted participants lives, they don't appear to have had the important effect of reducing recidivism.

Conclusion

The results presented here both confirm and update findings from previous studies, and reveal relationships between previously unevaluated sanctions and DUI recidivism. While limitations inherent in conducting research on programs linked to the criminal justice system (i.e., the inability to randomly assign offenders to different sanctions) do not allow definitive cause-effect relationships to be established, the findings here constitute an important evaluation of the policy effects of assigning various sanctions to DUI offenders.

Prior research has demonstrated that license suspension is the most effective sanction in mitigating overall traffic risk (Peck, 1991). Based on this study, and the results of prior research, it can be persuasively argued that combining alcohol treatment with license actions represents the most effective sanction combination for combating DUI recidivism.

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INTRODUCTION

The mixture of alcohol with automobile driving presents a major public health problem in the United States. The human cost of the problem is reflected in traffic fatality data reported by the National Highway Traffic Safety Administration (NHTSA), which show that traffic crashes are the greatest single cause of death for those between the ages of 5 and 32, and that alcohol is implicated in approximately half of these fatalities (NHTSA, 1991). NHTSA reports that 39,238 people were killed in automobile crashes in the United States in 1992, and estimates that 17,699 of these were alcohol related (NHTSA, 1993). In California, 4,185 people died in automobile crashes in 1992, and 1,832 of these fatalities were alcohol related (California Highway Patrol, 1992). The economic costs are also staggering. The Federal Highway Administration (1991) estimates that, nationally, the cost of all motor vehicle deaths, injuries and property damage amounts to almost 100 billion dollars a year.

Attempts to reduce these costs and better control drinking and driving in the United States have traditionally relied upon applying sanctions to those convicted of driving under the influence (DUI). Prior to the mid-1970's, a combination of jail, fines and license actions were typically prescribed for DUI convictees. These sanctions, which are still commonly used today, have two goals. The first is to reduce the likelihood that the offender will commit the offense again in the future. This is accomplished through specific deterrence, in which the punitive aspect of the punishment deters an offender from reoffending, and through incapacitation, where the driver is unable to reoffend, either because he is physically restrained (e.g., jail) or is otherwise restricted (e.g., license suspension). The second goal of the sanctions is to deter all drivers from drinking and driving, regardless of whether they have ever been arrested for DUI. This is accomplished by general deterrence, which in the short-term deters potential violators through fear of punishment, and in the long-term works through the creation of internal moral standards and habit formation, so that drivers voluntarily refrain from driving while drunk (Ross, 1982).

A newer approach to the DUI problem, first embodied in the DWI Phoenix School in 1966, is to educate and treat offenders in order to reduce their subsequent involvement in drinking and driving (Nichols, 1990). While the traditional sanctions are based on deterrence and/or incapacitation, alcohol education and treatment is predicated upon rehabilitation; DUI offenders engage in drinking and driving either because they are ignorant of the effects of alcohol and thus need to be educated, or because they are addicted and cannot control their drinking, in which case treatment is indicated. The rehabilitative approach was extended throughout the country in the 1970's in 35 Alcohol Safety Action Projects (ASAP's), which combined increased enforcement, prosecution and adjudication countermeasures with short term education and treatment of DUI convictees. There has been broad acceptance of alcohol education and treatment since the ASAP's were first initiated, and today rehabilitation is an integral component of court sanctions for drunk drivers.

The widespread use of alcohol education and treatment for DUI offenders has come under close scrutiny, and there has been long-standing debate about its effectiveness as a traffic safety measure relative to the more traditional sanctions imposed on DUI offenders. It is important to point out that alcohol education/treatment cannot be expected to exert a large overall impact on traffic safety, due to its focus on the convicted DUI offender. Nichols and Ross (1988) provide an example of this, pointing out that fewer than 5% of alcohol-related crashes in any given year involve a driver who was arrested for DUI the previous year, so that even if a treatment program was 100% effective, the next year's fatal crashes would be reduced less than 5%. In contrast, sanctions such as license actions, fines and jail terms are meant both to deter convicted offenders from reoffending, and to deter the general public from committing the offense in the first place, and thus have greater potential to improve traffic safety.

There have been a number of studies conducted in California by the Department of Motor Vehicles (DMV) Research and Development Section, and others, to assess the effectiveness of alcohol treatment, license suspension and other sanctions in curbing drunk driving. However, before reviewing these studies, a brief history of California's efforts to control drunk driving will be presented in order to provide the background in which to examine the relevant research.

DUI Control Efforts in California

Legislative history

The DUI countermeasure system in California has undergone a number of important changes since the mid-1970's. In 1976, Senate Bill (SB) 330 (Ch. 1133, 1975) mandated the first formal use of alcohol education and treatment for DUI offenders. Under this legislation, a pilot program was established in four counties in the state in which persons convicted of a second-or-subsequent DUI offense were eligible to participate in an alcohol education and treatment program, thereby avoiding the usual mandatory license actions. The SB 330 pilot program was extended statewide two years later by SB 38 (Ch. 890, 1977).

There was some concern about removing license actions as an inducement to entering treatment, since license actions have been shown to be an effective countermeasure, and this concern was mitigated in January 1982 by Assembly Bill (AB) 541 (Ch. 940, 1981), which requires a 1-year term of license restriction for those enrolling in a SB 38 alcohol education and treatment program. AB 541 also changed sanctions for first DUI offenders, by including alcohol education and license restriction as a court sentencing option.

SB 1601 (Ch. 1338, 1982), effective 6 months after AB 541, modified and strengthened its predecessor by requiring SB 38 participants to establish proof of insurance in order to obtain an unrestricted license at the end of their term of restriction. Another significant DUI law which became effective in 1982 was AB 7 (Ch. 939, 1981), which established a per se blood alcohol concentration (BAC) level of .10%. Under this law it is illegal to

drive with a BAC level of .10% or higher, and drivers found to have a BAC of .10% or higher are presumed impaired.

There was growing acceptance of the use of alcohol education and treatment for DUI offenders during the 1980's, and this is reflected in the enactment of two Senate bills. SB 925 (Ch. 1339, 1985), effective July 1985, requires that those receiving a third-or-subsequent DUI conviction show evidence that they have completed a treatment program in order to have their drivers license reinstated. SB 1365 (Ch. 1041, 1987), effective January 1988, established 30-month alcohol education and treatment programs as an alternative to 12-month SB 38 programs for those receiving a third-or-subsequent DUI conviction.

More recent legislation which affects the DUI countermeasure system includes SB 408 (Ch. 479, 1989), which lowered the BAC per se level to .08% effective January 1990, and SB 1344 (Ch. 803, 1989), which lengthened SB 38 alcohol treatment programs from 12 to 18 months beginning January 1990. However, it is SB 1623 (Ch. 1460, 1989) which represents the most significant change to the DUI countermeasure system in recent years. SB 1623 mandated that an administrative per se (APS) license suspension system be implemented on July 1, 1990. Under this law, if a driver is found to be operating his/her vehicle with a BAC level of .08% or higher, or refuses a chemical test, their drivers license is suspended by a law enforcement officer on the spot. This administrative license suspension represents a separate and additional sanction to others the driver may receive as a result of the adjudication of their DUI case.

Current sanctions

Currently, the sanctions which are prescribed, under a variety of circumstances, for those convicted of DUI in California include fine, jail, license suspension or restriction, community service, ignition interlock, and alcohol education and treatment. The type of alcohol education and treatment program that an offender is referred to, and the amount of some of the other sanctions imposed (e.g., jail), are tailored to the degree of the offender's prior involvement with drinking and driving. First offenders typically receive a minimum fine of $390, probation and license restriction if they enroll in a drinking driver program (DDP) (otherwise probation and jail), and an APS license suspension if their arrest for DUI occurred after July 1990. As with first offenders, second DUI offenders are subject to APS license suspension and a $390 minimum fine. In addition, all second offenders are sentenced to jail and all receive either license restriction (if they enroll in a DDP) or suspension (if they do not)[1]. Finally, for those receiving a third-or-subsequent DUI conviction within a seven year period, a sentence including at least 120 days in jail and a 3-year license revocation is imposed, in addition to APS license suspension and the $390 minimum fine. It should be noted that with the

[1]There is also a group of second offenders who receive both DDP and license suspension, either because they fail to provide proof of insurance and/or pay a restricted license fee, or because they did not receive all of the other court sanctions necessary to obtain a restricted license, or because the court did not correctly report to DMV that they did receive these sanctions.

addition of penalty assessments, the typical cost to someone convicted of DUI is considerably higher than the minimum fine amounts listed here.

Three types of alcohol education and treatment programs have evolved, based on the premise that an offender's number of prior DUI convictions reflects the extent of their problems with alcohol. For those just entering the criminal justice system with their first DUI conviction, there are first offender DDP's. These programs, which are licensed by the Department of Alcohol and Drug Programs (ADP), are typically 3 months in duration and consist of a minimum of 10 hours education (e.g., the effects of alcohol on the body and on driving, DUI laws, etc.), 10 hours counseling and 10 additional hours of education/counseling. In addition, it is required that the client maintain "close and regular" face-to-face interviews with program staff. There are approximately 90,000 annual enrollments in first-offender programs in California.

If an individual is convicted of a second DUI offense within seven years, he/she may be sentenced by the court to attend a SB 38 DDP. SB 38 programs are 18 months in length and require at least 12 hours of education, 52 hours of counseling and biweekly face-to-face interviews. There are approximately 35,000 annual enrollments in SB 38 programs.

The final level of treatment is applied to someone who is convicted of DUI for a third-or-subsequent time within a seven year period. Prior to 1989, alcohol education and treatment for third-plus offenders consisted of SB 38 programs, but since then 30-month programs have been initiated in Los Angeles and San Bernardino counties, although only those programs in Los Angeles are currently accepting new enrollments. 30-month programs consist of a minimum of 18 hours education, 117 hours counseling, 120-300 hours of community service, and "close and regular" face-to-face interviews. There are less than 500 annual enrollments in 30-month programs.

Prior Research

The first concerted effort to provide alcohol education and treatment for convicted DUI offenders in the United States occurred in the early 1970's through the ASAP's. Annual studies of program effectiveness were conducted and several reviews of these studies have been published (Epperson, Harano & Peck, 1975; Ellingstad, 1976; Spiegel and Struckman-Johnson, 1978; Nichols et al., 1980). Results on the effectiveness of ASAP programs in reducing crashes and DUI recidivism generally found little evidence of program impact, although it appears that they did lead to changes in knowledge and attitudes toward alcohol (Stewart and Ellingstad, 1988).

In California, one of the first studies evaluating the effectiveness of sanctions for DUI offenders was conducted by Hagen (1977). Hagen compared the subsequent 6-year driving record of offenders who had a second DUI conviction declared unconstitutional and dismissed by the court, thus avoiding mandatory license suspension, with a matched group of convicted offenders who received license suspension. The results of this study showed that those receiving license suspension had substantially fewer subsequent DUI convictions and crashes than those whose license was not suspended.

While Hagen's study provided important information on the effects of license suspension, the enactment of SB 330, which became effective on January 1, 1976, provided the opportunity to study the relative effectiveness of license suspension versus alcohol education/treatment. SB 330 mandated that pilot alcohol programs be established in four California counties, and specified that program participants not be subject to drivers license actions. This pilot program was evaluated by Hagen et al. (1978), who compared the subsequent 12-month driving records of those who enrolled in an alcohol program with offenders in two other groups, all of whom received license suspension. The first license suspension group consisted of individuals in the four pilot counties, while the second group was comprised of repeat offenders residing in four counties matched to the pilot counties based on demographic and traffic safety variables. The investigators found that alcohol program participants had a significantly greater number of subsequent crashes and DUI convictions than match county subjects receiving license suspension, and concluded that license actions are a more effective traffic safety countermeasure than alcohol education and treatment.

One criticism of the study by Hagen et al. was the short follow-up period used to judge the relative effectiveness of DUI sanctions (Peck, 1991). This led to a subsequent study conducted by Sadler and Perrine (1984) using the same subjects identified by Hagen et al., over a longer 4-year postconviction period. Sadler and Perrine found that persons who received license actions had substantially fewer subsequent non-alcohol-related crashes and convictions than offenders who participated in a SB 38 alcohol program. License suspension was shown to be the most effective overall traffic safety measure, with individuals in this group having an average of 30% fewer crashes than the SB 38 group. However, SB 38 participants had significantly fewer (9%) subsequent alcohol-related convictions than the license suspension group.

One of the more rigorous studies of the effectiveness of alcohol treatment programs for DUI offenders was conducted in Sacramento, California from 1977 until 1981 (Reis, 1982a; Reis, 1982b). This program, called the Comprehensive Driving Under the Influence of Alcohol Offender Treatment Demonstration (CDUI) project, randomly assigned DUI offenders to alcohol treatment or control groups. First offenders were assigned to one of three groups: 1) a four-week in-class education program, 2) a home study program consisting of a set of reading materials, and, 3) a no-treatment control group. The evaluation results showed that both the home study and education program groups had significantly fewer subsequent 20-month DUI arrests than the no-treatment control group, although there were no differences among the groups in subsequent crash involvement (Stewart and Ellingstad, 1988).

Repeat offenders in the CDUI project were randomly assigned to one of six groups: 1) a no-treatment control, 2) biweekly contacts only, 3) a 1-year skills-based education and treatment program, 4) skills program plus drug therapy, 5) educational eclectic therapy, and, 6) eclectic therapy plus drug therapy. The results of analyses assessing the effectiveness of these various approaches demonstrated that subjects in both types of treatment programs had fewer subsequent DUI arrests than the no-treatment control

group, and that adding drug therapy to treatment did not improve the effectiveness of these rehabilitation programs (Stewart and Ellingstad, 1988).

While the results of the CDUI evaluation provided encouraging support for the treatment approach to controlling drunk drivers, subsequent analyses of the CDUI project data produced less promising results. Arstein-Kerslake and Peck (1985) conducted a reanalysis involving CDUI subjects using additional variables as predictors and employing a longer follow-up period. These investigators found that for both first offenders and multiple offenders, treatment and control group assignment was not significantly ($p \leq .05$) related to recidivism (Peck, 1991). The only treatment showing a suggestive positive trend was the first-offender in-class education program, which was significant at $p = .08$ (Reis reported a p of .02 for this program in his analysis).

Following the CDUI project, the State of California sponsored a study pursuant to AB 541 to examine the effectiveness of alcohol treatment programs designed for first offenders. This project was aimed at giving alcohol treatment its best chance to work, and included an extensive program development effort as well as procedures to ensure quality implementation (Stewart and Ellingstad, 1988). Despite these efforts, evaluation results showed that there were no differences between treatment and control groups on self-reported drinking behavior or subsequent DUI convictions.

Two other studies conducted by DMV researchers in California are antecedents to the present study and deserve mention. The first, performed by Tashima and Peck (1986), examined the relative effectiveness of various sanctions for first and second DUI offenders. The investigators found that, for second offenders, those who had their licenses suspended had significantly fewer subsequent 1-year non-alcohol, fatal/injury and total crashes than offenders who received a SB 38 program referral plus license restriction; there was no significant difference between the groups on alcohol-related crashes. However, different results were found on subsequent major traffic convictions (including DUI), with the restricted program group demonstrating a significantly lower rate (24%) than the suspended group.

The findings for first offenders are a bit more complex. Tashima and Peck summarize the results by concluding that first offenders who received stronger license control actions (6-month suspension or 90-day restriction plus program) had significantly lower crash and conviction rates than offenders receiving lesser penalties (fine, jail and program plus jail).

The second antecedent study was conducted by Tashima and Marelich (1989), who evaluated the efficacy of alcohol treatment, license suspension and other sanctions for first, second and third-plus DUI offenders. For first offenders, evaluation results showed that those who received some form of license action had fewer subsequent crashes than those who received no license action, and that license suspension was the most effective sanction in reducing total crash risk. By comparison, first offenders receiving only jail had the highest rates on all outcome measures, suggesting that jail is the least effective of the first-offender sanctions. The results for second offenders

demonstrated that those in the suspended group had fewer subsequent total crashes than offenders in the SB 38 program plus license restriction group. In contrast, the SB 38 group had fewer subsequent alcohol-related crashes and major convictions than the suspended group. Finally, for third-plus offenders, those who received license revocation had fewer subsequent major and total convictions than those who were referred to SB 38 programs, although the SB 38 group was superior on alcohol-related crashes. Interestingly, for both second and third-plus offenders, longer jail sentences were associated with fewer subsequent crashes and convictions, probably reflecting in part an incapacitation effect.

In a 1991 review of studies examining the effectiveness of various DUI sanctions in California, Peck concluded that license suspension is superior to alcohol treatment in reducing total crash risk, although there is evidence that the greater effectiveness of license suspension relative to treatment is declining. Peck also drew the conclusion that alcohol education and treatment programs appear to be more effective than license suspension in reducing subsequent convictions of DUI. Some evidence also exists that alcohol education and treatment programs can be effective in reducing alcohol-related crashes. Based on past research, it appears that combining license suspension with alcohol treatment for DUI offenders will yield better results than simply using either one alone. This strategy has been recommended in the past by a number of investigators (Mann et al., 1983; Sadler and Perrine, 1984; Tashima and Peck, 1986; Helander, 1986; Nichols, 1990; Wells-Parker et al., in press).

Study Objectives

The present study, mandated by SB 1344 (see Appendix A), is one in a series of research projects conducted by the DMV Research and Development Section aimed at identifying the most effective sanctions for DUI offenders. Its goal is to produce useful information which can be used by policy makers to make informed decisions that will improve traffic safety in California.

The study is designed to provide information on the following issues:

First offenders

* What are the effects of first-offender alcohol education and treatment programs on recidivism, and how does this compare to other sanctions?

* How has the consolidation of first-offender programs under the supervision of ADP impacted the effectiveness of such programs?

Second offenders

* What is the relative efficacy of SB 38 programs versus other court sanctions in curbing DUI recidivism?

* How have changes in the SB 38 programs impacted recidivism of second DUI offenders?

Third offenders

* How effective are the 30-month alcohol treatment programs compared to SB 38 programs and other court sanctions for offenders receiving a third-or-subsequent DUI conviction?

While prior research on the DUI countermeasure system has increased our understanding of the effects of various sanctions on drunk driving, recent changes in the countermeasure system may render past findings moot. In addition, the relatively new 30-month alcohol treatment programs have never been studied, and an important policy question is how effective these programs are in reducing recidivism relative to shorter-term alcohol programs and license suspension.

METHOD

Subject Selection

The DMV maintains monthly computer tapes containing information received from courts on convictions of DUI, and reckless driving which is alcohol related (arrests for DUI which are reduced to reckless during the adjudication process, often referred to as "wet reckless"). These tapes, called the BAC tapes, were used as the primary source for selecting subjects for this study. The only exception to this is that third-plus DUI offenders who were sentenced by the court to attend a 30-month alcohol treatment program were selected both from BAC tapes and from records kept by the Los Angeles County Office of Alcohol Programs (only Los Angeles County has ongoing 30-month alcohol treatment programs). The reason for this is that few offenders were sentenced to 30-month programs during the time periods of interest and, since some of these offenders may not be reported to DMV, Office of Alcohol Programs' records were used in order to maximize sample size.

Not all offenders identified from the BAC tapes and Office of Alcohol Programs records were included in the sample. There were a number of duplicate entries which occurred primarily because courts sent data to DMV amending the abstract of a previously sampled conviction, and both the original and amended abstract were included in the sample. The amended abstract usually contains updated or corrected information regarding sanctions prescribed by the court, and thus is more accurate than the original abstract. For this reason, the amended abstract was used and the original dropped.

Another group of excluded subjects were those whose drivers licenses are prefixed with an "X". These are termed X-files, and are created when DMV receives data on an abstract of conviction from a court and is unable to locate the convictee in the master driver record file. In such cases the driver is assigned a number in the driver record database which is prefixed with an X, and information on the conviction is entered for the subject under this number. Because prior and subsequent driver record data may be unreliable for this group of drivers, they were dropped from the samples.

A third group of excluded drivers were those whose residence ZIP Code indicates that they lived outside of California. These individuals were excluded for the same reasons subjects with X-prefixed numbers were excluded; prior and subsequent driving information on their DMV records may be incomplete, and including them could lead to biased results.

The final group of excluded subjects consisted of those drivers who refused to submit to a chemical test when requested to do so by a law enforcement officer. Sanctions for chemical test refusers are different than those for the substantial majority of drivers who do take the test, and these different sanctions make the groups non-comparable. For this reason, chemical test refusers are excluded from the study.

One group which was not excluded were those who were assigned to an alcohol treatment program by the court, but who subsequently failed to complete the program. The rationale for retaining these subjects in the sample is that they represent part of the effect of the program. That is, in examining the effects of a sanction it is important to include failures as well as successes since this provides an overall indication of the effectiveness of the sanction. In addition, it is not possible to identify and delete from the license suspension group those persons who would have dropped out of treatment had they been assigned. Since dropouts are known to represent high risk problem drivers/drinkers, their exclusion would introduce a serious bias into the group comparisons. This rationale has led previous researchers (Sadler and Perrine, 1984; Tashima and Peck, 1986; Tashima and Marelich, 1989; Wells-Parker et al., in press) to similar conclusions about retaining alcohol treatment program dropouts.

Because the types of sanctions a driver receives upon conviction of DUI vary depending on how many prior DUI convictions the driver has had, the evaluation was designed to perform separate analyses for offenders grouped according to their number of prior convictions. Offenders were sorted into three different groups based on their number of prior convictions of DUI or alcohol-related reckless driving. According to California law, prior convictions include those for DUI or alcohol-related reckless driving in which the arrest occurs within seven years of the arrest date of the present conviction. Information on prior convictions is contained in the driver record, and data on the number of priors were obtained for this study through the use of computer programs designed to extract data from the driver record. Drivers were sorted into the following three groups: 1) First offenders, or those subjects having no prior DUI or alcohol-related reckless driving convictions within the previous seven years, 2) Second offenders, those subjects having one conviction within the prior seven year time period, and, 3) Third-plus offenders, or those subjects who had two or more seven-year prior convictions.

Within each offender level subjects are classified into groups based upon the sanctions they received from the court for their DUI conviction. Court sanctions are indicated on the driver record in the form of disposition codes, which consist of letters representing the various court sanctions. In each offender level there were relatively small numbers of offenders assigned to sanction combinations which do not conform to the minimum required by statute (e.g., no fine imposed). These appear to be cases in which the court did not adhere to sanctions prescribed by law, or in which errors were made in

transmitting sanction data or entering such information into the driver record database. In any case, since the integrity of these anomalous sanction groups is questionable, and because they are small, they are omitted from the sample.

Table 1 shows the number of subjects in each sanction group, by offender level and evaluation question. There is considerable variation in the sample sizes among sanction groups, reflecting the frequency with which various sanctions are prescribed by the courts. In order to ensure that a sufficient number of subjects were sampled in each sanction group, the sample timelines varied according to offender level and evaluation question.

Table 1

Sanction Group Sample Sizes, by Evaluation Question and Offender Level

Sanction Effectiveness

First Offenders (n = 88552)

Jail	Suspension & jail	Program	Program & jail	Program & restriction	Program & jail & restriction
5177	2211	2337	42667	29257	6903

Second Offenders (n = 27293)

Suspension	SB 38 & suspension	SB 38 & restriction
5273	6567	15453

Third-plus Offenders (n = 32787)

SB 38 program & revocation	30-month program & revocation	Revocation only
8054	572	24161

Program Change

First Offender Program (n = 103030)

First offender program		Other	
Pre	Post	Pre	Post
40513	51203	5309	6005

SB 38 Program (n = 49625)

SB 38 program		Other	
Pre	Post	Pre	Post
14932	17245	8078	9370

The primary evaluation question addresses the relative effectiveness of the various court sanctions in reducing DUI recidivism. For first and second offenders, the same sample time frame was used to answer this question. Subjects were initially selected if their DUI conviction date was between July 1990 and June 1991. Then, only subjects who were arrested after July 1990 were included, in order to avoid potential confounds associated with APS license suspension, which was implemented on July 1, 1990.

While the primary evaluation question (sanction effectiveness) for third-plus offenders is the same as that for first and second offenders, a different design and analytic approach was used with this group in order to maximize the sample size of an infrequently used sanction. Third-plus offenders were sampled if their DUI arrest and conviction dates were between July 1990 and August 1993.

A second evaluation question which pertains primarily to first and second offenders is whether changes in first offender and SB 38 alcohol programs increased their efficacy in preventing subsequent DUI convictions. To address this question, sanction groups were combined into 2 groups, program and other, and subjects were sampled during two time periods, pre (prior to changes in the programs) and post (subsequent to program change). Subjects in the pre group were selected from the BAC tapes if they were arrested for DUI between January and July 1989, and if they were convicted during 1989, while the post group subjects were sampled if their DUI arrest and conviction dates were in the corresponding months of 1990. Note that none of these sample subjects received an APS license suspension.

Data Collection

Demographic, prior driving history, and subsequent DUI conviction (including alcohol-related reckless driving) data were collected for subjects in the sample. The demographic and prior driving history data, which were used as covariates in the analysis, were collected from DMV's driver record database and the 1990 U.S. Census. The census data are aggregated by ZIP Code, as are the traffic conviction and crash rate data. Subsequent DUI convictions, and for third-plus offenders, the number of days from conviction to first subsequent DUI conviction, were obtained from DMV's driver record database and were used as the outcome measures for evaluating sanction efficacy.

Prior record data were gathered for most subjects for a 3-year period preceding their sampled DUI conviction date. The exception to this were subjects used in the pre-post analyses designed to measure the impact of changes in first offender and SB 38 alcohol treatment programs. For these subjects, prior record data were gathered for a 2-year period preceding their DUI conviction date. The reason for this is that minor traffic conviction and crash data which occurred prior to October 1986 have been purged from DMV records, and since subjects in the pre group were convicted during the first half of 1989, their 3-year prior record would overlap this purge date and thus contain missing data.

Subsequent DUI conviction data were gathered for subjects in the first and second offender groups for a period of 18 months following their DUI conviction, which served

as their treatment date. An additional period of 6 months was included in order to allow sufficient time for most DUI conviction information to be transmitted and updated on their DMV records. The follow-up period for subsequent DUI conviction data was different for subjects in the third-plus offender group. For these subjects there was no set length for their follow-up period, but rather data were collected on the length of time from their DUI conviction to their first subsequent arrest which resulted in a DUI conviction, which of course was different for different subjects. The time periods used to search for the first subsequent DUI conviction on subjects' records ranged from a minimum of 4 months, for those subjects whose conviction date occurred toward the end of the sampling period, to a maximum of 37 months, for subjects whose original date of conviction occurred at the beginning of the sampling period. In addition, a 6-month period was included to allow conviction data to be updated on subjects' records.

Evaluation Design and Statistical Analyses

Design

Separate research designs were used to answer the two evaluation questions addressed in this study. The primary evaluation question examines, within each offender level, the relationships between the various sanctions offenders receive and subsequent DUI convictions. The approach taken to answer this question was to sample offenders from each sanction group, and then compare subsequent DUI convictions among the groups.

The second evaluation question looks at first offender and SB 38 alcohol treatment programs, and attempts to determine whether changes in these programs increased their effectiveness in curbing DUI recidivism. In January 1990, the Department of Alcohol and Drug Programs (ADP) became responsible for licensing and reviewing first offender programs, and this change may have led to a more uniform level of quality among these programs statewide. At this same time, SB 38 programs lengthened from 12 to 18 months, which may have impacted their efficacy.

To investigate whether alcohol program changes have had a measurable impact upon recidivism, subjects were selected prior to the changes (pre group), and a second sample was selected after program changes (post group). Also, to increase the power of the analysis, the various sanctions other than the program being examined were collapsed into one group, called "other," and the analysis compared pre to post differences in DUI recidivism in the program group with corresponding differences in the other group.

The two different approaches to answering the evaluation questions have one thing in common: it was infeasible to randomly assign offenders to the different sanction groups. Thus, both designs are quasi-experimental, and they rely on statistical measures to help control possible bias. The two main statistical techniques used, analysis of covariance (ANCOVA) and Cox proportional hazards survival analysis, both rely on the use of variables called covariates to reduce bias.

The problem confronted by such designs is that differences observed between groups on recidivism may be due to pre-existing differences among the groups, rather than to

the effects of the sanctions themselves. For example, past studies have found that drivers receiving license suspension have, on average, more prior traffic convictions than drivers who are sentenced to attend alcohol treatment programs. Because they are worse to start with, the license suspension group could be expected to have a higher recidivism rate than the alcohol treatment group, regardless of the sanctions they received. The use of covariates accounts for the linear relationship between these variables and the criterion (DUI recidivism), removing their effects, and thus equating the groups on these measures.

While incorporating covariates in the analysis helps control pre-existing group bias, it does not ensure that all sources of extraneous variance have been controlled. It is usually impossible to identify and include variables accounting for all differences between groups, so that possible group differences remains a rival alternative hypothesis to that of treatment impact.

There is another source of potential bias in the second evaluation question measuring the impacts of changes in alcohol programs. It is possible that historical events other than changes in the alcohol programs occurred between the pre and post sample periods, and these events may have affected the composition of the comparison groups and/or impacted recidivism rates apart from program changes. The sample time frames for the pre and post groups were selected in such a way to avoid the confounding influence of an obvious historical event, the implementation of APS license suspension, but other such events may have occurred and this potential bias limits the certainty of conclusions drawn from the analysis of this second evaluation question.

Analysis

Sanction effectiveness: First and second offenders

A series of one-way ANCOVAs was conducted separately for first and second offenders to examine the relationship between sanction assignment and the number of subsequent DUI convictions. Prior to performing the ANCOVAs, a number of data screening analyses were conducted to check normality of variables, outliers, missing or incorrectly coded values and multicollinearity.

Once data screening was complete, covariates were selected for use in the analysis. One source of covariates, the 1990 U.S. Census data, contains a very large pool of possible covariates, and some way was needed to select a more limited set. Two approaches were investigated. The first involved using SAS VARCLUS, a variable clustering program which performs disjoint clustering based on a multiple group principal components method. Clusters of variables are formed in a way to maximize the variance accounted for by the centroid component of each cluster. Using this approach, a solution with 8 clusters was chosen, and the variable with the highest R squared with the cluster component was selected to represent the cluster.

The second approach to reducing the number of census variables was to perform a principal components analysis using SAS FACTOR. Principal components is a statistical technique that is used to find groups of variables that are similar to each other, but independent of other groups. The result is component scores, which are linear

combinations of the variables. In some ways, this is similar to the clustering program described above, except that the result is a number of component scores rather than the variables themselves. Using SAS FACTOR, a solution with 4 components was chosen, based upon observation of the scree test, the value of the eigenvalues for each component, and whether all components were well-defined. Examination of the loadings of the variables on the components revealed that the first component is a high socioeconomic status (SES) indicator, the second a poverty component, the third a working class or lower middle class SES measure, and the fourth an age component. In addition, three variables—percent black, percent urban, and average travel time to work—showed much unique variance, and so were included along with the 4 component scores.

The next step involved determining which covariates were most strongly related to the criterion measure of DUI convictions, so that a final set of covariates could be selected. Two series of analyses were conducted, one using the variables from the cluster analysis, plus the other demographic and prior driving record variables, and the other substituting the component scores for the cluster variables. BMDP was used to conduct a stepwise regression analysis. The results from the regression analyses indicated that the covariate set which included the census variables explained slightly more variance than the set containing the component scores, so for this reason, and also because the component scores are more difficult to intuit than individual variables, the results from the regression analysis containing the census variables was used to choose the final covariate set.[2]

The final tasks conducted prior to performing the ANCOVA analyses were to check the homogeneity of variance and homogeneity of regression assumptions which underlay ANCOVA. Homogeneity of variance stipulates that the variance of the criterion measure is the same for all treatment groups, and this assumption was tested using the Levine test in BMDP. Homogeneity of regression specifies that for each covariate used in the analysis, the slope resulting from regressing the dependent variable on the covariate is the same for each level of the independent variable(s) (Tabachnick and Fidell, 1989). This assumption was tested for the covariates, both individually and as a group, using SPSS and SAS.

There is considerable controversy surrounding the best way to deal with a violation of the homogeneity of regression assumption, with suggested approaches ranging from abandoning the analysis altogether to methods such as computing separate covariate slope adjustments for each treatment group (Searle, 1987), or using the Johnson-

[2]Another method for selecting covariates was also explored. This involved the use of discriminant function analysis to identify variables which reliably separated the sanction groups. The idea is that using these variables as covariates, in addition to those identified in the regression analyses, might reduce the error component of the main ANCOVA model. This method was used with second offenders, but showed virtually the same results as analyses using only the covariates identified in the regression analyses (see Appendix B), so in the name of parsimony it was dropped in favor of the approach described here.

Neyman technique to evaluate group differences at different values of the offending covariate (Huitema, 1980).

The approach taken here was to compute separate covariate slope adjustments for each treatment group. However, instead of giving this treatment x covariate interaction priority over the main effect or interaction of interest, as performed by default using SPSS MANOVA or SAS GLM, the size of the interaction was first compared to that of the effect being evaluated. In all cases the slope interactions were many times smaller than the effect of interest, and so were included in the final ANCOVA model, but with priority given to the effect being evaluated. The rationale for this approach is explained in more depth by Peck and Gebers (1993).

The ANCOVA analyses were conducted using SAS GLM. An alpha level of $p \leq .05$ was used, so that any differences among sanction groups on recidivism were considered significant if their probability of occurring by chance was less than 5 in 100. Because there were more than two treatment groups for both offender levels (6 first offender groups and 3 second offender groups), additional tests were required to identify which groups were significantly different in the event that the main ANCOVA F value was significant. The Tukey-Kramer multiple comparison procedure was used to test specific group differences, while controlling for inflated type I error rates due to the multiple tests conducted. This test uses the sample sizes of the groups in the test formula to control for unequal sample sizes, and so was especially appropriate here (Toothaker, 1993).

Because of concerns that the distribution of the criterion measure, DUI convictions, is highly skewed, with most subjects having only 0 or 1 conviction but some subjects having more than a dozen convictions, a second statistical approach was used with second offenders and the results compared to those from ANCOVA. This second approach consisted of dichotomizing DUI convictions into 0 or 1-or-more, and then using logistic regression to regress DUI convictions on the covariates and the treatment variable. SAS CATMOD and SAS LOGISTIC were used to perform the analyses, and an alpha level of $p \leq .05$ was chosen to evaluate the significance of differences among the treatment groups on recidivism. The results were very similar to those from ANCOVA (see Appendix C), indicating that the non-normal distribution of DUI convictions did not affect the ANCOVA analyses. Only the results of the ANCOVA analyses are presented here.

Sanction effectiveness: Third-plus offenders

The initial steps in the third-plus offender analysis were the same as those described in the previous section; various data screening analyses were conducted to check the integrity of the data. Selection of the final covariate set, using the same cluster of reduced census variables that was employed in the first and second offender analyses, was accomplished using a stepwise regression approach within SAS PHREG.

The criterion measure and statistical technique used to evaluate third-plus offender sanctions were unique to this group. The criterion measure was the number of days

from the entry DUI conviction to the first subsequent DUI arrest resulting in conviction. The statistical technique used was the Cox proportional hazards survival model, and SAS PHREG was used to conduct the analyses.

Survival analysis (or event history analysis, as it is sometimes called) has been used primarily in the fields of medical sciences and sociology to measure the time to occurrence of an event of interest. Note that there is a shift in emphasis in this approach from measuring a criterion variable in terms of numbers of occurrences, to examining the amount of time until the event occurs. However, a problem arises in that some subjects may not have experienced the event, and so their time is unknown. The particular strength of survival analysis is that it can handle such cases, termed censored observations.

The output from survival analysis includes a survival function, which is a cumulative distribution of cases surviving, or not experiencing the event, over time. This is usually plotted, with time along the horizontal axis, to produce a visual picture of how subjects fare over the length of the study. A hazard function is also produced, which is a conditional probability that a subject will experience the event at a particular time, given that he/she has not experienced it to that point (Singer and Willett, 1991). The hazard function can also be plotted, and this provides an important indication of which time periods are particularly risky for subjects to fail, or experience the event of interest.

The Cox survival model was used in this study because it allows the inclusion of covariates, or predictors, in the model. A regression-type of equation is developed which relates the log of the hazard of the criterion measure to the predictors, over time. In the third-plus offender analysis, the log of the hazard of the number of days to first subsequent DUI conviction is predicted by the covariates and the treatment, or sanction, variable. Different hazard and survival functions are produced and plotted for each of the sanction groups, so that the risks of recidivating over time can be compared among groups.

One assumption underlying the Cox model is that the hazards are proportional for the predictors. For example, in the third-plus offender analysis, the hazard rates for each of the three sanction groups are assumed to be proportional over time, so that the relative efficacy of the sanctions does not change over time. There is some controversy over how serious a violation of this assumption is. Allison (1984) claims that even if the proportional hazards assumption is violated, the model is often satisfactory, while Singer and Willett (1991) maintain that researchers can arrive at incorrect substantive conclusions by ignoring violations. Like most assumptions underlying statistical models, the effect likely depends more on the absolute magnitude of the non-proportionality rather than its statistical significance vis-a-vis alpha values.

A conservative approach was adopted here. The proportional hazards assumption was tested by conducting a number of analyses, each time including an interaction of a different predictor with time as the final effect entering the model. The significance of the interaction was assessed by calculating twice the difference between the log

likelihoods of the reduced model and the full model, which included the interaction term. The resulting statistic has an asymptotic chi-square distribution and can be evaluated using chi-square critical values (Allison, 1984). Covariate x treatment interactions were also tested using this same logic. All interactions were evaluated at $p \leq .05$, and if significant were included in the final survival model in order to account for the non-proportionality.

The final survival model included all significant covariate x time, treatment x time and covariate x treatment interactions, as well as 2 dummy variables representing the 3 sanction groups. The dummy variables were adjusted for the covariates and the interaction terms, and the significance of the dummies in predicting days to subsequent DUI conviction were evaluated at $p \leq .05$.

Impact of program change: First offender and SB 38 programs

As with the first and second offender sanction effectiveness analysis, this evaluation began with a series of analyses performed to screen the data, select the covariates, and check the ANCOVA assumptions before proceeding to the main ANCOVA analyses. Note that for these analyses, the 1990 census data were not available, and because the 1980 census data were so out of date as to be of questionable value, the covariate set did not include census variables. Because these initial analyses are described in detail in the first and second offender sanction effectiveness analysis section, they are not repeated here.

SAS GLM was used to conduct several factorial ANCOVAs, where sanction was one factor with two levels (program and other), time period was another factor, also with two levels (pre and post), and, for the SB 38 program analysis, offender level was a third factor, again with two levels (2nd and 3+). Note that it is the interaction between the factors that is of primary interest in these analyses, since we want to know whether the difference in recidivism from pre to post time period is the same for the program group as it is for the other sanction group and, in the case of the SB 38 programs, if this changes depending on offender level.

Because this is a non-orthogonal factorial design, a decision needed to be made on which method to use to calculate sums of squares. The issue here is that with unequal cell sizes, hypotheses about main effects and interactions are not independent, and sums of squares are not additive; the result is that there is ambiguity regarding assignment of overlapping sums of squares to sources (Tabachnick and Fidell, 1989).

The classical experimental ANOVA approach was used to calculate sums of squares. This method gives priority to main effects over interactions, and to lower-order interactions over higher-order interactions (Overall and Spiegel, 1969). In the alcohol program change analysis, the two-way interaction between sanction and time period is the primary effect of interest, and is adjusted for the covariates and factors, and for the other two-way interactions, but not for the three-way interaction. An alpha level of $p \leq .05$ was used to evaluate the significance of the sanction x time period interaction.

RESULTS

Program Change: First Offender Program

Selection of covariates

BMDP2R was used to conduct a stepwise regression analysis in order to determine which covariates were significantly related to the criterion, subsequent 18-month DUI convictions. The strategy used for this particular analysis was to select a low *F*-to-enter value of .15, so that all covariates would enter the model, and then to inspect the mean square error term at each step of the model to see at what point the addition of a variable increased this value. Covariates entering at or after this point were omitted from the final ANCOVA model.

Table 2 shows the results from the regression analysis. Most of the variables are significant at $p \leq .001$, but the very low multiple *R* squared indicates that, as a group, they account for very little variance in the dependent variable. Based on this analysis, 12 covariates were used in the final ANCOVA model.

Table 2

Stepwise Regression Analysis to Select Covariates: First Offender Program

Step	Variable entered	Multiple *R*	RSQ	RSQ change	*F* change
1	2-yr prior convictions	.0680	.0046	.0046	478.72*
2	Sex	.0844	.0071	.0025	259.42*
3	Age	.0921	.0085	.0014	141.72*
4	2-year prior major convictions	.0962	.0092	.0008	79.00*
5	ZIP Code major conviction average	.0990	.0098	.0006	58.12*
6	ZIP Code total crash avg	.1013	.0103	.0004	46.56*
7	ZIP Code moving violation average	.1019	.0104	.0001	13.71*
8	ZIP Code total conviction average	.1026	.0105	.0001	13.47*
9	License class	.1030	.0106	.0001	8.98**
10	ZIP Code injury crash average	.1032	.0106	.0000	4.50***
11	2-year prior had-been-drinking crashes	.1033	.0107	.0000	1.36
12	2-year prior total crashes	.1034	.0107	.0000	3.08***
13	2-year prior fatal & injury crashes	.1034	.0107	.0000	0.69

*$p \leq .001$, **$p \leq .01$, ***$p \leq .10$

Analysis of ANCOVA assumptions

BMDP7D was used to check whether the variances of subsequent 18-month DUI convictions were significantly different among the 4 treatment groups. The results from this analysis showed that there were significant group differences at $p \leq .01$, indicating that the homogeneity of variance assumption is violated. However, these results should be viewed with caution. Because the sample size is so large (>100,000), very small differences in variances will be statistically significant. In addition, the BMDP test is based on deviations from the group means, and thus is sensitive to variables that are not normally distributed. Prior descriptive analyses showed that all variables depart from normality, so the results from the BMDP test are not surprising.

The largest difference between the ratios of group variances is only 2:1. Keppel (1991) states that problems associated with heterogeneous variances only become serious when this ratio exceeds 3:1. For this reason, and also because of the test sensitivities mentioned above, it was decided that the violation of the variance assumption was not serious, and that no correction was needed.

The assumption of homogeneity of regression, or equal slopes, was checked using BMDP1V. The analysis showed that 5 covariates—license class, ZIP Code injury crash average, ZIP Code total crash average, ZIP Code major violation average, and ZIP Code moving violation average—violated the equal slopes assumption at $p \leq .01$. This violation means that there is an interaction between the factors and the covariates, and it was dealt with by forming 3-way interactions between each violating covariate and sanction x time period, and including these interactions in the final ANCOVA model. This strategy yielded a separate regression slope of the criterion on each violating covariate within each group. Two ANCOVA models were developed, one in which the 3-way interactions were included before the sanction x time period interaction, and the other with the 3-way interactions included after the sanction x time period interaction. The results were very similar, and the final model included the 3-way interactions <u>after</u> the 2-way interaction of interest, based on the rationale explained in the methods section.

<u>Group differences</u>

SPSS was used to check for significant group differences on the covariates. Group differences on categorical-level covariates were tested via a chi-square analysis, while interval-level covariates were evaluated using a one-way ANOVA. The results of these analyses are presented in Table 3.

There are significant differences among the groups on all of the covariates, and all but one are significant at $p \leq .001$. Subjects in the program groups are older, have fewer prior total and major convictions, and fewer prior total and had-been-drinking crashes than subjects in the other group. Thus, there are pre-existing group differences, and the covariates have captured some of the ways in which the groups differ.

Table 3

Group Differences on Covariates: First Offender Program

Variable	Group								
	Program pre		Program post		Other pre		Other post		Significance tests & *p* values
License Class									
% ID Card	5.6		6.5		16.0		15.2		
% commercial	3.1		3.2		2.7		2.7		
% automobile	90.6		89.6		79.7		80.5		
% motorcycle	0.1		0.0		0.1		0.1		$X^2 = 1510.96$
% unlicensed	0.6		0.7		1.5		1.5		$p \leq .001$
Sex									
% male	84.4		84.3		87.6		86.8		$X^2 = 63.0$
% female	15.6		15.7		12.4		13.2		$p \leq .001$
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
Age	32.84	10.87	32.51	10.52	31.18	10.06	30.92	9.90	$F = 85.56$ $p \leq .001$
2-year prior convictions	1.03	1.48	1.07	1.51	1.33	1.88	1.49	1.98	$F = 198.72$ $p \leq .001$
2-year prior major convictions	.019	.141	.017	.135	.050	.232	.060	.265	$F = 200.54$ $p \leq .001$
2-year prior total crashes	.297	.546	.297	.553	.319	.577	.309	.575	$F = 3.40$ $p = .017$
2-year prior had-been-drinking crashes	.145	.365	.142	.363	.162	.389	.148	.377	$F = 5.19$ $p \leq .001$
ZIP Code injury crash average	.089	.022	.090	.024	.095	.024	.093	.020	$F = 149.85$ $p \leq .001$
ZIP Code total crash average	.297	.065	.300	.069	.299	.063	.293	.058	$F = 28.61$ $p \leq .001$
ZIP Code major conviction average	.020	.012	.021	.013	.022	.015	.022	.007	$F = 70.33$ $p \leq .001$
ZIP Code moving violation average	.634	.106	.637	.107	.655	.102	.651	.104	$F = 97.23$ $p \leq .001$
ZIP Code total conviction average	1.30	.236	1.31	.239	1.29	.237	1.26	.239	$F = 62.99$ $p \leq .001$

Recidivism analysis

The SAS GLM procedure was used to analyze the final ANCOVA model, and the results are presented in summary form in Table 4. Both of the main effects are highly significant, indicating that the program group has a different average number of subsequent 18-month DUI convictions than the other group, and that when both groups are combined, the pre and post time periods are also different on this measure. However, it is the interaction that is of interest, and it is not significant, showing that changes in recidivism from pre to post time period are not statistically different

between the program and other sanction groups. This can be confirmed visually by inspecting Figure 1, which shows that although the other group has a higher recidivism rate than the program group during both time periods, the change in recidivism from pre to post is approximately the same for both groups. If the change in first offender programs made them more effective, this should be reflected in a greater decline in recidivism from pre to post for the program group than for the other group, which underwent no change. This analysis shows that there is no evidence to even suggest that changes in first offender programs increased their effectiveness in curbing DUI recidivism.

Table 4

Summary Table of 2 Factor ANCOVAs: First Offender Program

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates*	27	5.70	44.30	.0001
Sanction group	1	55.99	435.43	.0001
Time period	1	5.81	45.18	.0001
Sanction x time period	1	0.00	0.00	.9794
Error	102999	.13		

*Includes the covariate x sanction x time period interaction for each of the 5 covariates that violate the equal slopes ANCOVA assumption.

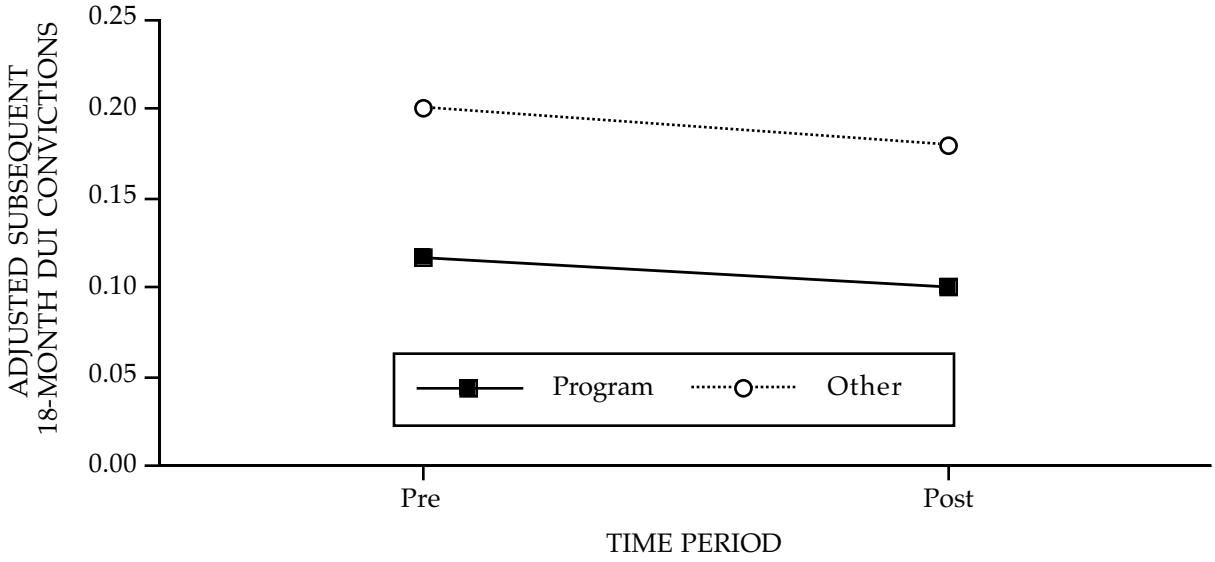

Figure 1. Adjusted subsequent DUI convictions, by time period, for program and other groups: First offender program.

Program Change: SB 38 Program

Selection of covariates

The same covariate selection strategy used in the evaluation of first offender programs was used in the SB 38 program analysis. A stepwise regression analysis was conducted using a low *F*-to-enter value of .15, so that all covariates would be stepped into the model, and covariates were then chosen based on whether their inclusion in the model decreased the mean square error term. The results of this analysis are presented in Table 5. Most of the variables are significant at $p \leq .001$, but as can be seen from the small multiple *R* squared value, as a group they explain little of the variance in subsequent 18-month DUI convictions. The first 11 covariates in the table were selected and used in the final ANCOVA model.

Table 5

Stepwise Regression Analysis to Select Covariates: SB 38 Program

Step	Variable entered	Multiple *R*	RSQ	RSQ change	*F* change
1	2-year prior convictions	.0903	.0082	.0082	408.21*
2	Sex	.0962	.0092	.0011	54.60*
3	ZIP Code total crash average	.0990	.0098	.0006	28.09*
4	Age	.1015	.0103	.0005	24.64*
5	ZIP Code injury crash average	.1038	.0108	.0005	23.79*
6	2-year prior major convictions	.1054	.0111	.0003	16.26*
7	License class	.1066	.0114	.0003	12.93*
8	2-year prior fatal & injury crashes	.1070	.0115	.0001	5.10***
9	2-year prior total crashes	.1077	.0116	.0001	7.40**
10	ZIP Code moving violation average	.1079	.0117	.0000	2.20
11	ZIP Code total conviction average	.1095	.0120	.0003	17.23*
12	ZIP Code major conviction average	.1096	.0120	.0000	0.37
13	2-year prior had-been-drinking crashes	.1096	.0120	.0000	0.19

*$p \leq .001$, **$p \leq .01$, ***$p \leq .10$

Analysis of ANCOVA assumptions

The variances in each group were compared using the Levine's test in BMDP7D. This test showed that group differences were significant at $p \leq .01$, thus violating the homogeneity of variance assumption underlying ANCOVA. A previous analysis revealed that the largest difference between the ratios of group variances was only 1.7:1, a value below that which should cause serious problems (Keppel, 1991). Because of this, and also because of the sensitivities of the variance test which were discussed in more detail in the first offender program section, it was decided that no correction was needed to deal with the violation of the equal variances assumption.`

The initial attempts to check the homogeneity of regression assumption using BMDP1V yielded no test values for a number of variables due to singular variance-covariance matrices. In order to find out why this occurred, squared multiple correlations (SMCs) were calculated for each covariate, with all other covariates serving as independent variables, in a multiple correlation analysis. The two highest SMCs were .69 and .75, values far below that which should cause singularity problems. Due to this anomaly, SAS GLM was used to check for equal slopes.

The analysis showed that 2 covariates, 2-year prior convictions and 2-year prior major convictions, violated the equal slopes assumption at $p \leq .01$. This violation was dealt with by forming two 3-way interactions of each covariate with the two other factors, sanction group and time period, and including these 3-way interactions in the final ANCOVA model after the sanction group x time period interaction of interest.

Group differences

Chi-square and one-way ANOVA tests were conducted to evaluate group differences on categorical-level and interval-level covariates, respectively. The results of these analyses, presented in Table 6, show that there are significant group differences on 8 of the 11 covariates at $p \leq .001$. The other sanction group is younger and more predominately male than the program group, and they have higher average numbers of prior convictions and crashes than those in the SB 38 program group. This analysis shows that the sanction groups are different from the outset, prior to assignment to their respective sanctions.

Table 6

Group Differences on Covariates: SB 38 Program

Variable	Group								
	Program pre		Program post		Other pre		Other post		Significance tests & *p* values
License class									
% ID card	2.4		3.3		7.7		8.8		
% commercial	3.5		3.3		3.0		3.0		
% automobile	93.9		93.0		88.8		87.7		
% motorcycle	0.1		0.1		0.1		0.0		$X^2 = 797.60$
% unlicensed	0.1		0.3		0.4		0.5		$p \leq .001$
Sex									
% male	90.0		90.7		93.4		93.4		$X^2 = 135.24$
% female	10.0		9.3		6.6		6.6		$p \leq .001$
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
Age	34.65	10.40	34.21	10.08	33.98	10.23	33.58	9.82	$F = 22.56$ $p \leq .001$
2-year prior convictions	1.49	1.66	1.54	1.67	1.90	1.91	1.89	1.88	$F = 175.22$ $p \leq .001$
2-year prior major convictions	.413	.574	.431	.591	.695	.835	.687	.806	$F = 595.90$ $p \leq .001$
2-year prior total crashes	.340	.594	.342	.594	.362	.618	.342	.613	$F = 2.68$ $p = .046$
2-year prior fatal & injury crashes	.128	.358	.137	.369	.151	.323	.133	.364	$F = 6.85$ $p \leq .001$
ZIP Code injury crash average	.090	.021	.091	.023	.090	.021	.090	.019	$F = 3.85$ $p = .009$
ZIP Code total crash average	.298	.068	.299	.062	.291	.071	.288	.062	$F = 73.59$ $p \leq .001$
ZIP Code moving violation average	.636	.107	.640	.114	.646	.111	.644	.103	$F = 19.51$ $p \leq .001$
ZIP Code total conviction average	1.29	.239	1.30	.247	1.30	.244	1.30	.236	$F = 2.18$ $p = .089$

Recidivism analysis

The initial ANCOVA model included offender level (2nd and 3+) as a third factor, in addition to sanction group and time period, in order to see whether the comparison between the SB 38 and other groups, in difference in recidivism from pre to post time periods, varied by offender level. The results from this analysis showed that the 3-way interaction between offender level x sanction group x time period was not significant (p = .54), but that the 2-way interaction between sanction group x offender level was significant ($p \leq .001$). While not of theoretical interest, this 2-way interaction was left in the final ANCOVA model to remove bias and reduce the mean square error.

The results from the final ANCOVA analysis are presented in Table 7. The main effect of offender level is not significant, while those of sanction group and time period are.

Table 7

Summary Table of 3 Factor ANCOVAs: SB 38 Program

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates*	17	5.12	38.04	.0001
Offender level	1	.01	.05	.8255
Sanction group	1	6.21	46.13	.0001
Time period	1	.54	3.98	.0460
Sanction x time period	1	0.09	0.69	.4071
Error	49602	.13		

*Includes the covariate x sanction x time period interaction for each of the 2 covariates that violate the equal slopes ANCOVA assumption.

Of primary interest is the interaction between sanction x time period, which is not significant (p = .41), indicating that differences in subsequent 18-month DUI convictions from pre to post time period do not vary according to sanction group. That is, there is no evidence indicating that lengthening SB 38 programs from 12 to 18 months increased their effectiveness. This is confirmed visually by inspecting Figure 2, which shows the absence of interaction between sanction group and time period.

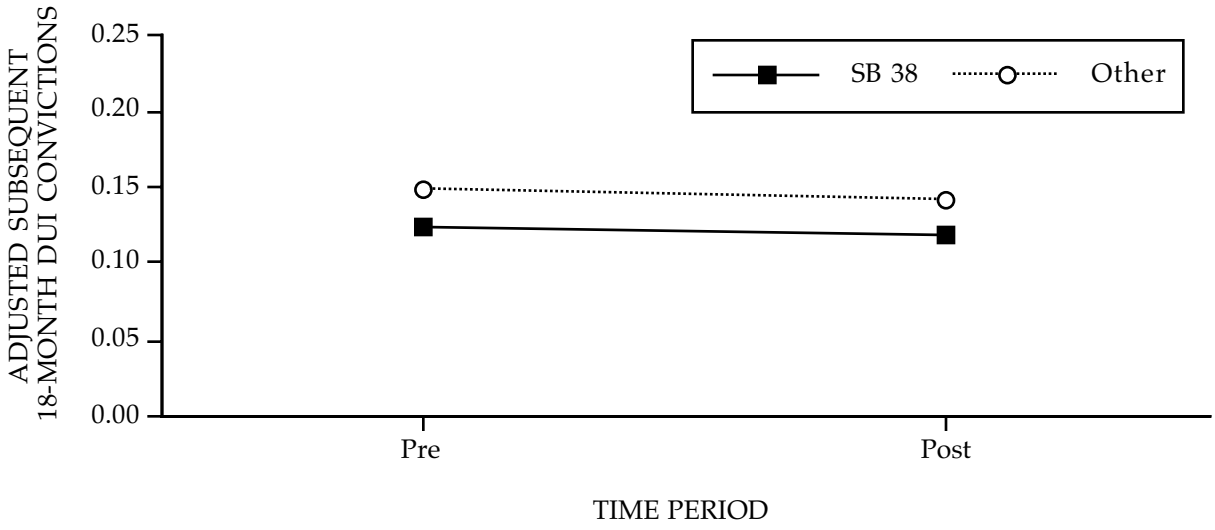

Figure 2. Adjusted subsequent DUI convictions, by time period, for program and other groups: SB 38 program.

Sanction Effectiveness: First Offenders

Selection of covariates

One difference between the program change analyses and the sanction effectiveness analyses is that 1990 census data were unavailable at the time the former were being conducted, but became available and were used in the latter set. Thus, there was a considerably larger pool of potential covariates from which to choose a final set for the evaluation of sanction effectiveness.

A second difference between the two sets of analyses was the selection strategy used for choosing a final set of covariates. Because the sanction analyses had such a large number of covariates entering the preliminary stepwise regression models, choosing a final set based on the point that the mean square error increased proved infeasible, due to the resulting large number of covariates in the final set. Instead, covariates were selected if they were significant at $p \leq .10$. The results from this analysis of the first-offender sample are presented in Table 8.

Table 8

Stepwise Regression Analysis to Select Covariates: First Offenders

Step	Variable entered	Multiple *R*	RSQ	RSQ change	*F* change
1	3-year prior convictions	.0708	.0050	.0050	445.72*
2	Sex	.0817	.0067	.0017	148.26*
3	ZIP Code average travel time to work	.0877	.0077	.0010	89.84*
4	3-year prior major convictions	.0928	.0086	.0009	82.41*
5	ZIP Code % welfare	.0961	.0092	.0006	55.57*
6	ZIP Code % renting	.0994	.0099	.0007	58.26*
7	Age	.1024	.0105	.0006	54.36*
8	ZIP Code moving violation average	.1039	.0108	.0003	26.49*
9	ZIP Code median family income	.1048	.0110	.0002	17.19*
10	ZIP Code injury crash average	.1054	.0111	.0001	11.74*
11	ZIP Code % black	.1058	.0112	.0001	7.66**
12	ZIP Code % hispanic	.1061	.0113	.0001	5.89***
13	ZIP Code % unemployed	.1068	.0114	.0001	12.16*
14	License class	.1070	.0114	.0000	3.85***
15	3-year prior fatal & injury crashes	.1071	.0115	.0000	2.89***
16	3-year prior total crashes	.1073	.0115	.0000	3.36***

*$p \leq .001$; **$p \leq .01$; ***$p \leq .10$

Analysis of ANCOVA assumptions

The equality of group variances on the criterion measure, subsequent 18-month DUI convictions, was checked using the Levine's test in BMDP. This test showed that the homogeneity of variance assumption was violated at $p \leq .01$. This is not surprising, given the large sample size (> 90,000), and the sensitivity of the test to variables which are not normally distributed (prior analyses showed that almost all variables were non-normal). Prior descriptive analyses showed that the largest difference between group variances was 2.3:1. In light of the sensitivities of the variance test, the magnitude of the differences between group variances was not deemed large enough to pose serious problems, and no corrective action was undertaken.

The homogeneity of regression assumption was tested using SAS GLM. This analysis showed that 3 of the 16 covariates—3-year prior convictions, ZIP Code median family income, and ZIP Code average travel time to work—violated the equal slopes assumption. As in the program change analyses, this violation was dealt with by using the violating covariates to form interaction terms, in this case with the one factor in the model, sanction group, and entering these interactions in the final ANCOVA model after the sanction main effect.

Group differences

In order to see how the 6 first-offender sanction groups differed on the covariates, chi-square and one-way ANOVA tests were performed. The results of these tests are shown in Table 9. The groups are significantly different on all of the covariates at $p \leq .001$. Those in the jail and jail & suspension groups are younger, more predominately male, and have higher numbers of prior convictions and prior major convictions than

those in the other sanction groups, suggesting that because they are worse to begin with, they could be expected to have higher recidivism rates than the other groups, apart from any sanctions the groups receive.

Table 9

Group Differences on Covariates: First Offenders

Variable	Group												
	Jail & suspension		Jail		Program		Program & restriction		Program & jail		Program & jail & restriction		Significance tests & *p* values
License class													
% ID card	23.2		19.7		11.1		3.7		12.0		9.1		
% commercial	2.3		2.9		3.2		3.1		3.5		2.5		
% automobile	72.5		75.4		84.5		92.6		83.4		87.5		
% motorcycle	0.0		0.1		0.1		0.1		0.0		0.0		$x^2 = 2756.37$
% unlicensed	2.0		1.9		1.1		0.5		1.1		0.9		$p \leq .001$
Sex													
% male	91.7		87.7		85.4		84.3		84.0		85.1		$x^2 = 140.24$
% female	8.3		12.3		14.6		15.7		16.0		14.9		$p \leq .001$
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
3-yr prior convictions	2.14	2.29	1.70	2.03	1.41	1.75	1.37	1.68	1.53	1.85	1.40	1.74	$F = 103.84$ $p \leq .001$
3-yr prior major convictions	.073	.286	.038	.198	.016	.127	.016	.130	.020	.148	.015	.125	$F = 78.60$ $p \leq .001$
3-yr prior total crashes	.408	.656	.361	.634	.365	.622	.371	.632	.354	.617	.391	.642	$F = 7.75$ $p \leq .001$
3-yr prior fatal & injury crashes	.173	.417	.139	.372	.131	.360	.128	.361	.133	.368	.154	.393	$F = 10.72$ $p \leq .001$
ZIP Code injury crash avg	.090	.018	.085	.018	.089	.017	.089	.019	.084	.018	.087	.019	$F = 363.67$ $p \leq .001$
ZIP Code moving violation avg	.611	.092	.600	.092	.602	.078	.589	.094	.606	.095	.598	.099	$F = 124.12$ $p \leq .001$
ZIP Code % black	.094	.153	.073	.119	.067	.117	.070	.119	.064	.104	.079	.129	$F = 51.10$ $p \leq .001$
ZIP Code % hispanic	.358	.244	.343	.230	.378	.251	.315	.229	.256	.201	.333	.240	$F = 510.19$ $p \leq .001$
ZIP Code avg travel time to work	27.05	4.66	25.92	4.95	26.91	4.67	28.04	4.37	25.94	4.88	25.88	4.86	$F = 775.39$ $p \leq .001$
ZIP Code % welfare	.052	.029	.055	.032	.049	.026	.040	.025	.048	.029	.046	.028	$F = 425.55$ $p \leq .001$
ZIP Code % unemployed	.052	.021	.056	.026	.052	.022	.046	.019	.047	.021	.048	.021	$F = 289.20$ $p \leq .001$
ZIP Code % renting	.475	.166	.446	.140	.454	.162	.461	.174	.437	.149	.497	.176	$F = 212.16$ $p \leq .001$
ZIP Code median family income	36575	12130	35358	11267	36930	12374	41381	14318	38880	12333	37729	12920	$F = 308.88$ $p \leq .001$
Age	29.77	9.22	31.16	10.17	31.19	10.58	32.93	10.69	32.49	10.43	31.62	10.18	$F = 72.06$ $p \leq .001$

Recidivism analysis

The final ANCOVA model was analyzed using SAS GLM. Three interaction terms, one for each of the three covariates which violated the equal slopes assumption, crossed with sanction group, were created and entered after the main effect of sanction group. Type I sum of squares (a sequential decomposition) was specified, in which the main

effect of sanction was adjusted for all 16 covariates, but not for the three interactions. The results from this analysis are presented in Table 10.

Table 10

Summary Table of Single Factor ANCOVA: First Offenders

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates*	31	3.53	35.23	.0001
Sanction group	5	4.21	42.00	.0001
Error	88495	.10		

*Includes the covariate x sanction group interaction for each of the 3 covariates that violate the equal slopes ANCOVA assumption.

The *F* value for sanction group is highly significant, indicating that there are statistically significant differences in subsequent 18-month DUI convictions among at least some of the groups. To ascertain which groups are different, two Multiple Comparison Procedures (MCPs) were used. The first, the Tukey-Kramer MCP, controls for Type I error and also adjusts for unequal group sample sizes, while the second, the Games & Howell MCP, controls for unequal group variances as well as unequal sample sizes. Because the results from both tests were very similar, only those from the Tukey-Kramer are reported here, in Table 11.

Table 11

Pairwise Comparisons of Recidivism: First Offenders

Criterion	*F* Test & significance level	Group	Adjusted group means & t tests†					
			(1) Program & restriction	(2) Program	(3) Program & jail & restriction	(4) Program & jail	(5) Jail & suspension	(6) Jail
Subsequent1 8-month DUI convictions	$F = 42.00$ $p \leq .0001$	$\bar{x}$	.07767	.08145	.08362	.09028	.10349	.15067
		(1)		$t = .55$	$t = 1.40$	$t = 5.25$*	$t = 3.70$*	$t = 15.29$*
		(2)			$t = .29$	$t = 1.31$	$t = 2.35$	$t = 8.77$*
		(3)				$t = 1.62$	$t = 2.57$	$t = 11.52$*
		(4)					$t = 1.91$	$t = 12.96$*
		(5)						$t = 5.86$*

*$p \leq .01$,

†*t* values converted from studentized range statistics

As shown in the MCP test results in Table 11, and graphically in Figure 3, the program & license restriction group has the lowest average number of subsequent DUI convictions; the adjusted mean of this group is significantly lower than that of the program & jail, jail & suspension, and jail groups. The highest number of subsequent DUI convictions, on average, are accrued by those in the jail group, and their average is significantly higher than all other groups. The two highest recidivism averages belong to the jail and jail & suspension groups, the two groups which had the worst prior records. Part of the reason for their poor showing here is probably due to these pre-existing group biases, which the ANCOVA only partially adjusted for.

While offenders receiving program & restriction have the lowest recidivism average, their average is not significantly different than those receiving program only, which suggests that alcohol treatment may be more responsible for curbing subsequent DUI convictions for first offenders than license restriction.

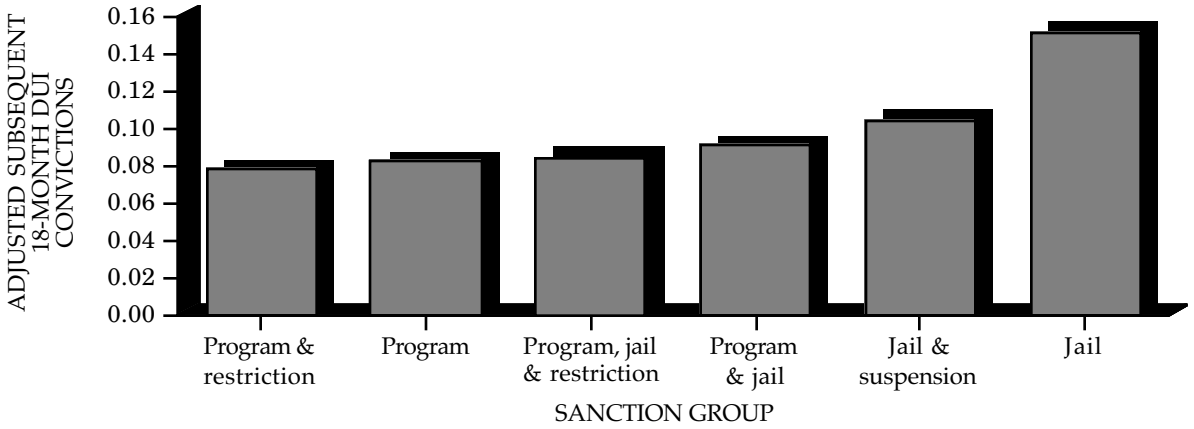

Figure 3. Adjusted subsequent DUI convictions, by sanction group: First offenders.

Sanction Effectiveness: Second Offenders

Selection of covariates

All demographic and prior driving record variables were entered into a stepwise regression model in order to determine which of these covariates were significantly related to the criterion, subsequent 18-month DUI convictions. Based on the results of the regression analysis, which are presented in Table 12, 11 covariates were selected from the original pool to use in the ANCOVA model.

Table 12

Stepwise Regression Analysis to Select Covariates: Second Offenders

Step	Variable entered	Multiple *R*	RSQ	RSQ change	*F* change
1	3-year prior convictions	.0589	.0035	.0035	94.86*
2	ZIP Code % welfare	.0699	.0049	.0014	38.81*
3	ZIP Code injury crash average	.0761	.0058	.0009	24.94*
4	License class	.0809	.0065	.0008	20.87*
5	Sex	.0851	.0072	.0007	18.89*
6	3-year prior major convictions	.0871	.0076	.0003	9.57**
7	ZIP Code moving violation average	.0890	.0079	.0003	9.09**
8	ZIP Code % driving alone to work	.0909	.0083	.0003	9.51**
9	ZIP Code % unemployed	.0930	.0086	.0004	10.60**
10	ZIP Code % black	.0941	.0089	.0002	5.74***
11	Age	.0946	.0089	.0001	2.46

$p \leq .001$; **$p \leq .01$; ***$p \leq .10$

Analysis of ANCOVA assumptions

The Levine's test in BMDP7D was used to check the homogeneity of variance assumption for the second offender sample. As with previous analyses, this one found that the variance assumption was violated at $p \leq .01$. Because group variances were only moderately different—the ratio of the largest to smallest was only 1.7:1—and due to the sensitivities of the Levine's test described in previous sections, no actions were taken to correct for the unequal group variances.

SAS GLM was used to test the homogeneity of regression assumption. This analysis showed that 2 of the 11 covariates, 3-year prior convictions and ZIP Code moving violation average, violated the equal slopes assumption at $p \leq .01$. The violation of the equal slopes assumption was dealt with by including in the final ANCOVA model two interaction terms, one for each of the violating covariates crossed with sanction group, placed after the main effect of sanction group.

Group differences

Pre-existing differences among the 3 second-offender sanction groups were assessed by conducting chi-square and one-way ANOVA tests on each of the covariates included in the model. As shown in Table 13, there were significant differences among the groups on all of the covariates, with all but one significant at $p \leq .001$. Not surprisingly, those in the suspension group are younger and more predominately male, and have higher average numbers of total and major traffic convictions, than offenders in either of the other two alcohol program groups. It is typically the case that those receiving only license suspension have worse prior records than offenders who the court assigns to alcohol treatment programs, and the suspended group will be at higher risk of recidivating than the program group, absent any intervention the groups receive.

Table 13

Group Differences on Covariates: Second Offenders

Variable	Group						Significance tests & *p* values
	SB 38 & suspension		SB 38 & restriction		Suspension		
License class							
% ID Card	5.8		3.6		11.9		
% commercial	3.4		3.2		2.9		
% automobile	90.2		92.9		84.4		
% motorcycle	0.1		0.0		0.0		$X^2 = 543.49$
% unlicensed	0.5		0.3		0.8		$p \leq .001$
Sex							
% male	91.3		90.0		91.9		$X^2 = 19.72$
% female	8.7		10.0		8.1		$p \leq .001$
	Mean	SD	Mean	SD	Mean	SD	
3-year prior convictions	2.15	1.94	2.08	1.90	2.42	2.07	$F = 61.15$ $p \leq .001$
3-year prior major convictions	.565	.548	.512	.530	.646	.561	$F = 123.20$ $p \leq .001$
ZIP Code % welfare	.048	.028	.046	.028	.054	.030	$F = 150.96$ $p \leq .001$
ZIP Code moving violation average	.592	.092	.603	.097	.605	.093	$F = 33.26$ $p \leq .001$
ZIP Code % driving alone to work	.726	.103	.735	.100	.728	.095	$F = 25.22$ $p \leq .001$
ZIP Code % black	.071	.119	.066	.108	.065	.105	$F = 6.11$ $p = .002$
ZIP Code % unemployed	.049	.021	.046	.019	.053	.024	$F = 233.75$ $p \leq .001$
ZIP Code injury crash average	.087	.019	.086	.018	.084	.017	$F = 46.95$ $p \leq .001$
Age	34.10	10.02	34.18	9.91	33.01	10.21	$F = 28.02$ $p \leq .001$

Recidivism analysis

The ANCOVA model used to test for significant differences in subsequent 18-month DUI convictions among the 3 second-offender sanction groups used Type I sum of squares (sequential) to adjust the sanction group effect for the 11 covariates in the model, but not for the 2 interaction terms involving the violating covariates. The results from this analysis are presented in Table 14.

Table 14

Summary Table of Single Factor ANCOVA: Second Offenders

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates*	15	2.08	18.11	.0001
Sanction group	2	3.34	29.08	.0001
Error	27275	.11		

*Includes the covariate x sanction group interaction for each of the 2 covariates that violate the equal slopes ANCOVA assumption.

The main effect of sanction group is highly significant, indicating that there are significant differences in recidivism among at least two of the groups. Because the overall *F* only indicates that some groups are different, it was necessary to conduct pairwise comparison tests to identify the particular groups that have different mean numbers of subsequent DUI convictions. Both the Tukey-Kramer and Games & Howell MCP tests, described in the previous section on first offenders, were used and the results were found to be very similar. Table 15 shows the results from the Tukey-Kramer test, while Figure 4 presents a graphical depiction of the differences among the groups on adjusted means of subsequent 18-month DUI convictions.

Table 15

Pairwise Comparisons of Recidivism: Second Offenders

Criterion	*F* Test & Significance level	Group	Adjusted group means & *t* tests† (1) SB 38 & suspension	(2) SB 38 & restriction	(3) Suspension
Subsequent 18-month DUI convictions	$F = 29.08$ $p \leq .0001$	$\bar{x}$	.09648	.09687	.13853
		(1)		$t = .08$	$t = 6.71$*
		(2)			$t = 7.71$*

*$p \leq .01$;

†*t* values converted from studentized range statistics

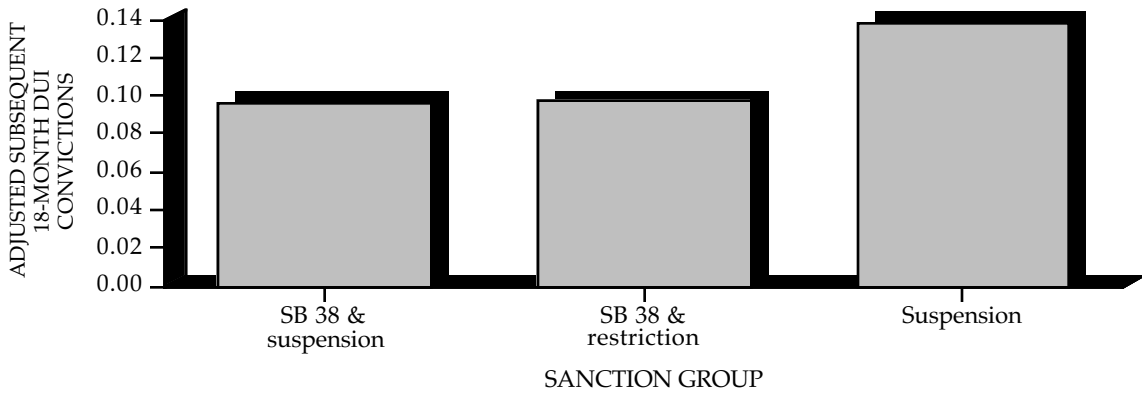

Figure 4. Adjusted subsequent DUI convictions, by sanction group: Second offenders.

The SB 38 & suspension group has the lowest adjusted mean on the recidivism measure, while the suspension group has the highest mean. The mean of the suspension group is significantly higher than both the SB 38 & suspension and SB 38 & restriction group means, while the means of the two SB 38 groups are very similar and not significantly different. Thus, those in the suspension group have the highest risk of recidivating, although, as mentioned previously, part of this may be due to pre-existing group differences rather than just the effects of the different sanctions. This analysis shows that alcohol treatment, plus some form of license curtailment, is associated with the lowest subsequent DUI rate for second offenders.

Sanction Effectiveness: Third-plus Offenders

Selection of covariates

The third-plus offender analysis used a different criterion measure and statistical technique than that employed to measure sanction effectiveness for first and second offenders. For third-plus offenders, the criterion measure is the number of days to first subsequent DUI, and for many offenders (e.g., those who have not recidivated) their ultimate time to this event is unknown, or censored. Because of this, the analyses, including those conducted to select covariates, need to appropriately handle the censored observations. Survival analysis was used because of its ability to handle censored observations.

SAS PHREG was used to perform stepwise regression within the survival model, in order to select those covariates that were significantly related to the criterion. This analysis revealed that 8 covariates were significant, and these covariates, which are used in the final survival model, are shown in Table 16.

Table 16

Stepwise Regression Analysis to Select Covariates: Third-plus Offenders

Step	Variable entered	Score chi-square	Probability chi-square
1	3-year prior convictions	101.70	.0001
2	License class	26.96	.0001
3	ZIP Code % renting	5.98	.0144
4	ZIP Code moving violation average	7.23	.0072
5	ZIP Code total conviction average	11.23	.0008
6	ZIP Code % welfare	4.36	.0367
7	Age	4.66	.0308
8	ZIP Code % black	4.22	.0399

Analysis of Cox proportional hazards assumptions

The primary assumption underlying the Cox proportional hazards survival model is that the hazard rates for the levels/groups defined by each predictor variable are proportional over time. SAS PHREG was used to test this assumption, first for each covariate, and then for the treatment variable, sanction group.

The proportionality assumption was tested for each covariate by creating an interaction of the covariate with time, entering this interaction term last in the survival model, and then assessing whether it made a significant contribution. This set of analyses revealed that 3 covariates—3-yr prior convictions, license class and age—violated the assumption. This means that the effect of each of these 3 covariates is not constant across time. In order to account for this non-proportionality, the interaction terms of the violating covariates were included in the final survival model (Allison, 1984; Singer and Willett, 1991).

This same logic guided the analyses conducted to determine whether the treatment variable, sanction group, met the proportionality assumption. Two dummy variables were created to represent the 3 sanction groups, and each was crossed with time to create an interaction term. Both interaction terms were included as final steps in the survival model, and the significance of their contribution to the model was evaluated. The results from this analysis showed that sanction group also violated the proportional hazards assumption at $p \leq .05$. This violation tells us that differences in recidivism between sanction groups are not constant over the course of the study, that these differences among groups either increase or decrease over time. To account for this, the two dummy x time interactions were included in the final survival model.

The final assumption tested was whether there was an interaction between the covariates and sanction group membership. Each covariate was crossed with the dummy variables to create interaction terms, and these were entered as the final steps in the model, and then checked for significance. This set of analyses revealed that one covariate, ZIP Code percent black, interacted significantly with sanction group, and the

interactions of this covariate with the two dummy treatment variables were left in the final survival model.

Group differences

Pre-existing group differences were assessed using chi-square and one-way ANOVA tests. There are significant differences among the groups on all 8 covariates, and these are displayed in Table 17. On 2 of the covariates which are not ZIP Code aggregates, it can be seen that subjects in the 30-month alcohol treatment program are, on average, older and have fewer prior convictions than subjects in the other two sanction groups. Based on this, it appears that the 30-month group is a lower-risk group than the other two sanction groups, and thus would tend to have a lower recidivism rate than the other two groups apart from the sanctions the groups received.

Table 17

Group Differences on Covariates: Third-plus Offenders

Variable	Group						Significance tests & *p* values
	SB 38 & revocation		Revocation		30-month program & revocation		
License class							
% ID card	6.4		9.7		1.6		
% commercial	2.7		3.1		2.4		
% automobile	90.3		86.7		96.0		
% motorcycle	0.1		0.0		0.0		$X^2 = 132.32$
% unlicensed	0.5		0.5		0.0		$p \leq .001$
	Mean	SD	Mean	SD	Mean	SD	
3-year prior convictions	2.73	2.09	2.63	2.04	2.31	1.74	$F = 15.84$ $p \leq .001$
ZIP Code % renting	.436	.144	.443	.144	.458	.177	$F = 11.05$ $p \leq .001$
ZIP Code moving violation average	.607	.098	.605	.093	.582	.078	$F = 18.55$ $p \leq .001$
ZIP Code total conviction average	1.24	.221	1.24	.218	1.28	.163	$F = 10.42$ $p \leq .001$
ZIP Code % welfare	.046	.029	.052	.030	.041	.023	$F = 150.46$ $p \leq .001$
Age	33.94	9.44	34.59	9.57	35.80	9.80	$F = 20.17$ $p \leq .001$
ZIP Code % black	.061	.099	.066	.108	.079	.136	$F = 11.60$ $p \leq .001$

Recidivism analysis

Before fitting a model to the data, the sample survival and hazard functions were calculated and plotted for the sanction groups using SAS LIFETEST. These functions and plots do not include the effects of the covariates, or the interaction terms to account for non-proportionality, and because of this they do not adjust for the pre-existing group differences. However, it is still valuable to examine them because they reveal important descriptive information about survival over time, and periods of particular risk of recidivating.

Figure 5 is a plot of the sample hazard function for third-plus offenders, over the course of the study. The most noticeable feature of the plot is that the hazards for the groups are quite different. They are clearly non-proportional, confirming the results of the statistical analyses which tested the proportional hazards assumption for the treatment variable. It is also interesting that the times at which the risk of recidivating are highest are different for each group. Offenders in the 30-month alcohol program are at highest risk of recidivating immediately after conviction, with a steady decline in risk over the 3-year time period of the study, except for a slight increase at about 2 years. In contrast, subjects in the revoked group have a steadily increasing risk of recidivating from the time of conviction until about one-and-a-half years, with their risk leveling off after this peak.

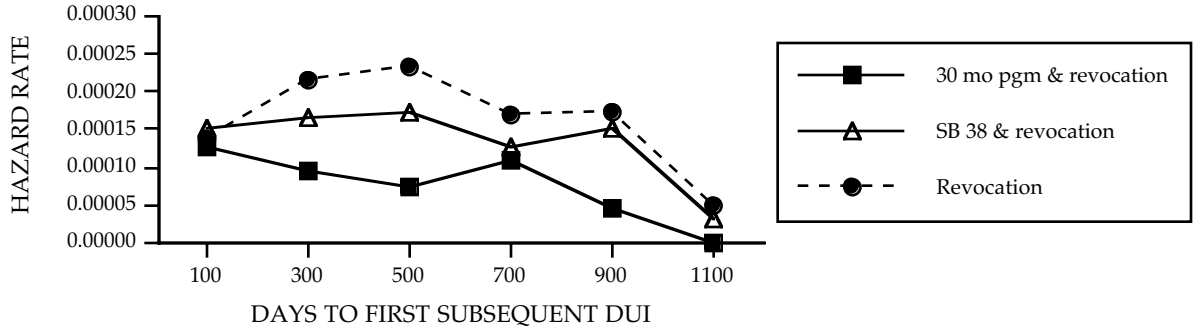

Figure 5. Sample hazard function for third-plus offenders.

The times of highest risk of recidivating are also different between the SB 38 and 30-month alcohol program groups. While the 30-month group's risk declines immediately after conviction, the risk for the SB 38 group is about even from the time of conviction until about a year-and-a-half, at which point it begins declining. This has implications for monitoring and intervention strategies for those running these alcohol programs. For example, because SB 38 program clients remain at high risk of recidivating early on, relative to 30-month program clients, it would appear that monitoring, support and frequent client contact may be more essential in the early phases of the SB 38 program than in the 30-month program.

While there are different patterns of recidivism risk among the sanction groups, there are also differences in levels among the groups. These must be viewed with great caution, however, because pre-existing differences between the groups have not been removed. The hazard rates are consistently highest for those in the revoked group, and lowest for the 30-month group, with SB 38 between the two. Interpretation of group differences in recidivism must wait until the results of the final survival model are described.

SAS PHREG was used to produce the final fitted hazard model for third-plus offenders. The final model included 8 covariates, 3 covariate x time interactions, the treatment x time interaction, and the covariate x treatment interaction. The parameter estimates, chi-square and probability values, and risk ratios for all effects in the model are presented in Table 18.

Table 18

Maximum Likelihood Estimates, Cox Proportional Hazards Model, Third-plus Offenders

Variable	Parameter est.	Wald chi-sq	*p* value	Risk ratio
3-year prior convictions	.0657	46.02	.0001	1.068
License class	-.0977	23.52	.0001	.907
ZIP Code % renting	-.3753	7.10	.0077	.687
ZIP Code moving violation avage	1.3127	16.59	.0001	3.716
ZIP Code total conviction average	-.3570	6.13	.0133	.700
ZIP Code % welfare	2.7204	16.59	.0001	15.186
Age	-.0016	.49	.4820	.998
ZIP Code % black	-2.2269	1.33	.2485	.108
Treat 1: SB 38 vs 30-month	.2082	.75	.3870	1.231
Treat 2: Suspension vs 30-month	.5234	4.88	.0272	1.688
Treat 1 x time	.0918	.36	.5492	1.096
Treat 2 x time	.2167	2.06	.1509	1.242
3-year prior conviction x time	-.0109	1.76	.1851	.989
License class x time	-.0375	4.91	.0268	.963
Age x time	.0051	5.81	.0160	1.005
Treat 1 x ZIP Code % black	2.7613	1.99	.1582	15.821
Treat 2 x ZIP Code % black	1.6186	.70	.4037	5.046

The primary question of interest is whether the hazards of recidivating vary by sanction group. Treat 1 is a dummy variable representing treatment group, coded in such a way as to allow a comparison between the SB 38 and 30-month alcohol treatment programs. The chi-square value for this variable is only .75, with a probability of .39, indicating that there is no statistically significant difference between the SB 38 and 30-month groups in days to first subsequent DUI arrest resulting in conviction.

Treat 2 represents a comparison between the recidivism of those who received license revocation and those assigned to a 30-month alcohol treatment program. The chi-square value for this dummy variable is 4.88, with an associated probability of .027, showing that there is a statistically significant (at $p \leq .05$) difference in recidivism between the revoked and 30-month groups. The positive parameter estimate (.5234) for treat 2 means that those in the revoked group have a higher hazard rate, or greater risk of recidivating, than those in the 30-month group (the 30-month group was coded as the reference group for the dummy variables). The relative risks of recidivating for the groups is given by the risk ratio, which for treat 2 is 1.69. The interpretation of this is that those receiving license revocation have 1.7 times the risk of recidivating compared to offenders in the 30-month group. Alternatively, it can be said that revocation is associated with an increased recidivism risk of 69%.

There is another way of conceptualizing the differences between the sanction groups, and that is by examining their respective cumulative failure (recidivism) rates at the conclusion of the study. These failure rates are presented in Table 19 below, both for a survival model which has no covariates (analogous to *unadjusted* means in ANCOVA), and one which has covariates to correct for pre-existing group differences, but no interaction terms to correct for non-proportional hazards (available software does not provide survival/hazard rates for models with time-dependent covariates, or covariates with interact with time).

Table 19

Failure Rates at 1100 Days, Third-Plus Offenders*

Group	Model without covariates (unadjusted)	Model with covariates (adjusted)
30-month program & revocation	9.8%	11.0%
SB 38 & revocation	15.0%	14.7%
Revocation	17.5%	17.1%

*Due to different times of entry into the study, most subjects were not followed for a full 1100 days. The average length of time in the study was 623 days.

Note that the failure rates are lowest for the 30-month program group and highest for the revoked group in both survival models, with the SB 38 & revocation group falling in the middle. Because the 30-month group is better/lower risk to start with, their failure rates are adjusted upward in the survival model which incorporates covariates, while the reverse is true for the SB 38 & revocation and revocation groups.

Unfortunately, the group differences in hazard rates cannot be graphically displayed, due to the software limitations described above for models with time-dependent covariates. However, it is possible to produce functions which include the effects of covariates, but which do not account for non-proportionality, and these are of some use. Figure 6 is a fitted hazard plot which includes the covariates, but no interaction with time. The hazards for the groups are forced by the model to be proportional (the baseline hazard rate is the same for all groups, and group membership only shifts this up or down), and thus don't represent the interactions. Despite this, it can be seen that the highest hazard belongs to the revocation group, the lowest to the 30-month group, with SB 38 between the two.

Because the hazards were not proportional for the treatment groups, an interesting question is how the differences between the groups change over time. The answer to this lies with the interaction of the dummy variables with time. These interactions were significant in a model with no other interaction terms, but have become insignificant in this final model, indicating that the differences between the groups in recidivism do <u>not</u> change significantly over time when other interactions are controlled for in the analysis. Thus, because the parameter estimate for the interaction is positive but non-significant, we can say that, <u>directionally</u>, the difference in recidivism between the revoked and 30-month groups increases over time, but that this difference is not statistically significant.

In summary, the survival analysis shows that those receiving only license revocation are at greater risk of recidivating than those assigned to a 30-month alcohol program, and this difference is statistically significant. There are no differences in recidivism between the SB 38 and 30-month alcohol groups, although this may stem in part from questionable statistical power resulting from the small number of subjects (<600) in the 30-month group. That the 30-month group has the best recidivism figure may be partly a function of pre-existing group differences, since they appear to be lower risk, based on their averages on two of the non-aggregated covariates used in the analysis.

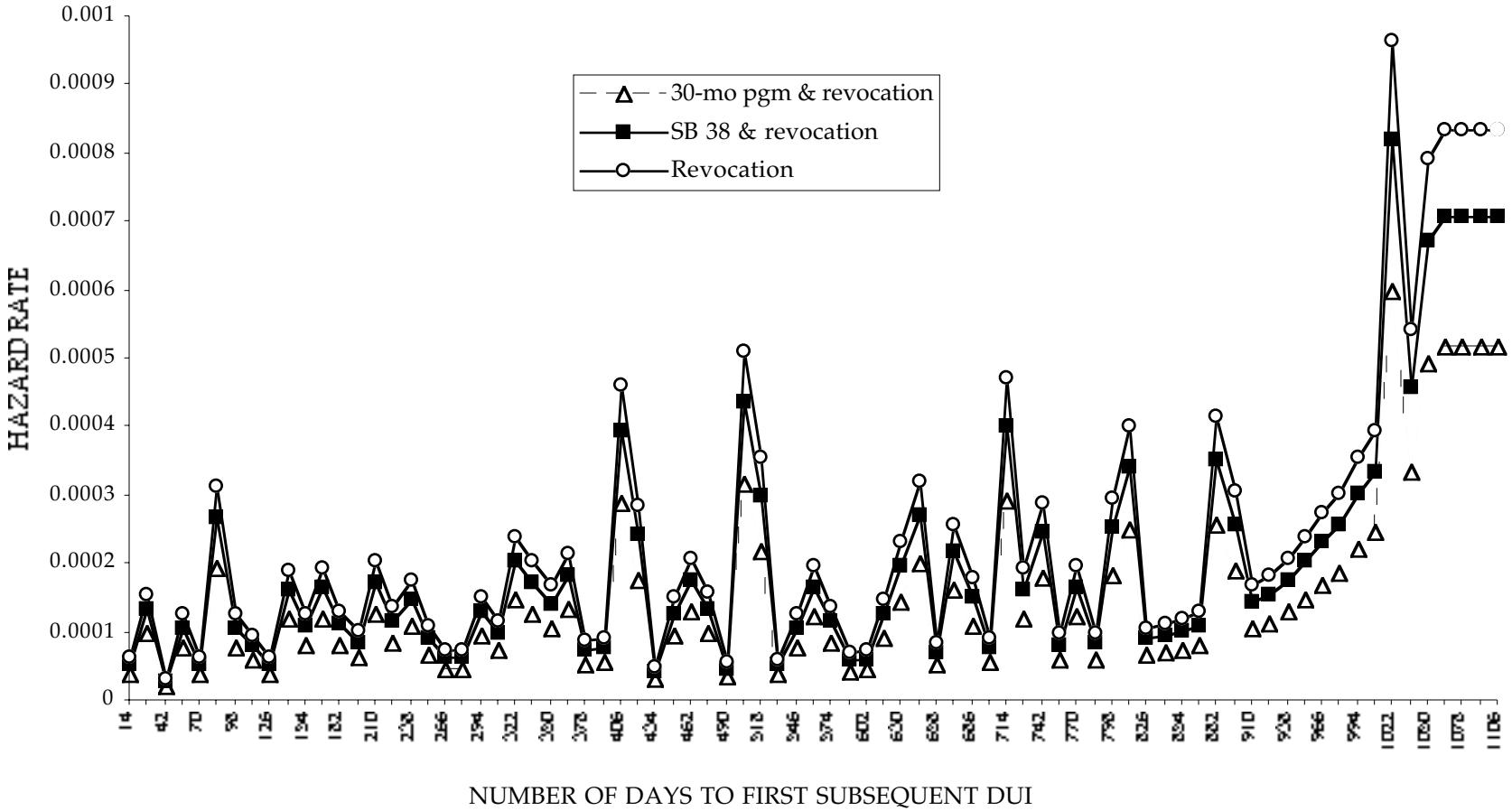

Figure 6. Fitted hazard plot, third-plus offenders: No correction for non-proportionality.

DISCUSSION

Program Change Analyses

Before discussing the results of the program change analyses, some caveats regarding the analyses need to be mentioned so that the results can be interpreted appropriately. The methodology used to assess pre to post changes in the alcohol programs, while the best available under the circumstances, is inherently weak in a number of respects. One problem is that we are assuming that while there were changes in the alcohol programs from pre to post time periods, there were no significant changes in the composition of the comparison groups. In other words, the comparison groups act as a benchmark to which the alcohol programs can be compared, and if there were changes that affected the comparison groups (but not the program groups), then actual changes in the program groups may appear attenuated or enhanced by comparison. We are unaware of any such changes impacting the comparison groups in this way, but the possibility remains that such changes did occur.

In addition to possible changes in the composition of the groups from pre to post time periods, there is a possibility that historical events occurring between the time periods may have produced different recidivism rates (even if the composition of the comparison groups didn't change). An example of this would be a change in legislation, such as an increase in jail days for those not attending an alcohol program, that would affect the recidivism of those offenders in the post-time-period comparison group, but not the post-time-period program group. Again, we are unaware of such historical changes, but the possibility remains that such changes did occur.

What these problems mean is that there are potential rival alternative hypotheses to those being investigated that may account for the results. While plausible support for these rival hypotheses seems lacking, the structure of the research methodology does not allow us to discount them entirely. Thus, the results from these analyses should be viewed with caution, suggesting an answer but certainly not proving one.

First offender program change analysis

The results from the analysis of first offender program changes showed that there were differences in recidivism between the program and the other sanction groups, and that there were also differences in recidivism between the pre and post time periods. However, it was hypothesized that there would be a greater change from pre to post for the program group than for the other sanction group, due to changes in the first offender programs, but this hypothesis was not supported by the data. In fact, while the number of subsequent DUI convictions was lower for both groups in the post time period, the decline in such convictions was directionally greater for the other sanction group. This was not statistically significant and probably is simply due to chance.

The type of changes which first offender programs underwent, from standardization to regulation and review by the Department of Alcohol and Drug Programs (ADP), intuitively would suggest the possibility of an increase in their effectiveness in preventing DUI recidivism. At a minimum, it would seem that enhancing the uniform

quality of such programs would have at least a small aggregate effect in reducing recidivism.

One possible explanation for the failure to find evidence of enhanced program effectiveness is that the time period for selecting subjects in the post program group began immediately upon the date ADP became responsible for licensing first offender programs. Like the start-up of any new program, it usually takes time to work through the problems and get things running smoothly, and it is possible that measuring the post time period program group later may have produced different results. Unfortunately, this option was not possible, because Administrative Per Se (APS) license suspension became effective 6 months after ADP became responsible for licensing first offender programs, and selecting post program offenders later would have overlapped APS and thus introduced a bias into the analysis.

It is also possible that there was an increase in the effectiveness of some smaller first-offender programs, but that this effect was lost in the aggregate analysis. It would have been infeasible to examine first-offender programs at the county level, due to the large sample and long timelines needed to obtain sufficient statistical power, so this possibility was not evaluated. In any case, in looking at the aggregate effect of first-offender programs, there is no evidence to support the hypothesis that the changes these programs underwent enhanced their effectiveness in curbing recidivism.

SB 38 program change analysis

During the same time period that first-offender programs changed, SB 38 alcohol treatment programs also underwent changes. The most salient change in the SB 38 programs is that they were lengthened from 12 to 18 months. The idea behind adding 6 months to SB 38 programs is to ease the transition of program participants from a supportive treatment environment to reentry into life without the support. During the reentry phase there are still occasional support group meetings and conferences with program counselors.

The ANCOVA analysis evaluated whether these changes in the SB 38 programs enhanced their ability to reduce DUI recidivism, relative to a group receiving other sanctions. The results showed that there was a difference in recidivism between the program and other sanction groups, and a difference between the pre and post time periods, but that there was no significant difference in recidivism from pre to post according to sanction group. In other words, changes in recidivism between the 12-month SB 38 program and the 18-month SB 38 program were not significantly different than changes in recidivism for the other sanction groups. In fact, the other group showed a greater decline in recidivism from pre to post period than did the SB 38 group, but this was not statistically significant.

This finding is surprising. Lengthening treatment would seem to be of benefit, and adding a reentry period makes logical sense. However, this finding of no increased program effectiveness has support in the literature. Elizabeth Wells-Parker and others (in press) conducted a large scale meta-analysis of the effects of remedial intervention, which included education, counseling, probation, and other treatments. They found that neither the duration of the intervention, nor the number of hours of the

intervention, were significantly correlated with DUI recidivism. Given that the meta-analysis included a large number of studies (215), it appears that there is significant evidence in support of the findings here.

It should be pointed out that, as with the analysis of first offender programs, the SB 38 program analysis looked at programs in the aggregate, and thus provides no information on whether individual programs may have become more or less effective. Also, as mentioned by Wells-Parker et al., an intervention shouldn't be judged solely on its impact on DUI recidivism, and this holds true here. Lengthening SB 38 programs by 6 months may have had some beneficial impacts on the lives of those who participated; however, it doesn't appear to have had the important effect of reducing recidivism.

Sanction Effectiveness Analyses

There are limitations in the analyses of sanction effectiveness that should be pointed out so that the results of the analyses can be more clearly interpreted. The most salient potential problem is that the sanction groups differ in a number of ways, some of which are probably related to the criterion measure of recidivism. Because of this, there is no guarantee that differences in recidivism that are observed among the groups are a function of the sanctions the groups received.

We have attempted to control pre-existing group differences by using available data as covariates, so that the groups are statistically made equivalent on these variables. There are, undoubtedly, other dimensions on which the groups differ for which data are not available, and these unmeasured factors may be influencing the recidivism of the groups. In some cases, the bias can be inferred by examining group differences on covariates that are related to the criterion measure, and the results can be interpreted in light of this. For example, if a particular group appears to be higher risk based on covariate scores, yet does better on recidivism than lower-risk groups, then more certainty can be placed in the results. Even this approach is not without its pitfalls, because biases are being inferred based only on data which are available. In summary, pre-existing group differences represent alternative hypotheses to those of sanction impact, and this limits the conclusions that can be drawn. Thus, the results do not prove the relationships between sanctions and recidivism as much as they portray the associations between the two.

Another potential problem concerns the integrity of the sanctions. Jail, for example, is notorious because many offenders sentenced to jail never serve time, or serve substantially less time than called for in the sentence. It is infeasible to check all sanctions, due to the time and effort involved, and even the courts are many times unaware of the actual jail time served. Despite this, the results still provide information on the policy impact of sanctions. That is, the jail sanction reflects the actual impact of sentencing offenders to jail, given jail overcrowding, early release, community service, etc. Additional support for findings on the impact of sanctions comes from replication provided by other studies.

First offender analysis

The results from the first offender analysis showed that there were significant differences among the 6 sanction groups on subsequent 18-month DUI convictions. The multiple comparison tests revealed that those receiving jail had the highest recidivism, and that their recidivism was significantly higher than the remaining 5 sanction groups. In fact, their average number of subsequent DUI convictions was almost double that of the alcohol program plus license restriction group, which had the lowest average. The only other significant differences were between the program plus restriction group and two other groups receiving jail: program plus jail, and jail plus suspension. Thus, program and license restriction is associated with the best outcomes on recidivism, while jail, either alone or in combination with license restriction or alcohol treatment, is related to the worst recidivism outcomes.

These findings are similar to those of previous studies. Tashima and Marelich (1989) found that, for first offenders, those sentenced only to jail had a significantly higher average number of subsequent 2-year major convictions than all other sanction groups. Tashima and Peck (1986) found that first offenders who received alcohol program plus license restriction had the lowest rate for major convictions. Tashima et al. (1993) looked at the number of weeks to first subsequent DUI conviction for first offenders. They found that those who were assigned to an alcohol program and who also received license restriction had the lowest recidivism rate, while those receiving only jail had the highest rate.

It seems clear that jail terms for first offenders, at least as implemented in California, are ineffective in reducing DUI recidivism. Part of the idea behind jail is to physically incapacitate offenders, so that they are unable to commit the offense while incarcerated. Obviously, at some level, jail could be 100% effective in curbing recidivism. However, as practiced in California over the last decade, sentencing first offenders to jail has not proved effective. Jail overcrowding and fiscal constraints within the criminal justice system have mitigated the potential efficacy of this sanction. In addition, prescribed jail terms for first offenders are so minimal as to yield only limited incapacitation effects. This argument is also made by Ross (1992).

Offenders sentenced to jail also tend to have the worst prior records, and thus are high risk regardless of what sanctions they received. It is unclear to what extent the ANCOVA analyses removed pre-existing group biases. Are their subsequent DUI conviction rates high because they are high-risk drivers, or because jail was ineffective, relative to other sanctions, in modifying or restraining their behavior? This question can not be answered definitively, although their recidivism rate is probably a function of both.

The positive findings for the effectiveness of first offender alcohol programs in reducing DUI recidivism also appears fairly convincing, based not only on the results of the previously cited studies undertaken in California, but studies conducted elsewhere as well. In a review of the DUI treatment and deterrence literature, Nichols (1990) concludes that alcohol education programs can be effective in reducing DUI recidivism for first-time offenders. In a similar, earlier review, Mann et al. (1983) came to the same conclusion.

A number of authorities in the field have suggested that the best approach combines both alcohol education/treatment with some form of license curtailment (Wells-Parker et al., in press; Nichols, 1990; Peck, 1991; Mann et al., 1991). While the findings here do not unequivocally support this recommendation, they are not at variance with it either. While the lowest average number of subsequent DUI convictions was associated with program and license restriction, this was not statistically different than program alone. This suggests the possibility that it is the alcohol program more than license curtailment that is responsible for curbing recidivism. However, this interpretation is open to question, because the number of subjects in the program only group was relatively small (2,337 compared to 29,257 in the program plus restriction group). This raises the possibility that there is a real, albeit small, difference in recidivism between the program plus restriction and program only groups, but that the relatively small sample size of the program only group did not allow sufficient power to detect this. Because of this, and because the program and restriction group directionally had the lowest recidivism rate, the results here lend guarded support to findings of other studies which show the benefits of combining license curtailment and alcohol education/treatment. It should also be noted that license curtailment has repeatedly been shown to be effective in reducing total traffic crashes and convictions, quite apart from its impact on DUI recidivism.

Second offender analysis

The results from the second offender analysis demonstrated that there were significant differences among the sanction groups in their average number of subsequent 18-month DUI convictions. Multiple comparison tests showed that the recidivism means for the SB 38 plus license suspension group and the SB 38 plus license restriction group were very similar and not statistically different from each other. By contrast, the recidivism average for those offenders who received only license suspension was approximately 43% higher than both of the SB 38 alcohol treatment groups, and this difference was statistically significant.

It should be pointed out that the SB 38 & suspension and SB 38 & restriction groups do not provide an unambiguous comparison of the relative effects of license suspension versus license restriction due to pre-existing differences between the groups. For example, SB 38 & suspension is a somewhat anomalous group which includes drivers who agree to enroll in the alcohol program but who don't have the financial means to purchase insurance, and so receive license suspension rather than restriction. Thus, one significant difference between these groups is economic. Such group differences may cause the SB 38 & suspension group to have a higher expected recidivism rate than the SB 38 & restricted group absent any sanctions the groups receive, and thus vitiate potential comparisons between suspension and restriction.

The findings on the differences between SB 38 plus license restriction/suspension and only license suspension are supported by the results of prior research. Sadler and Perrine (1984) examined differences between offenders assigned to a 12-month second-offender alcohol treatment program and offenders receiving only license suspension on a number of crash and conviction measures. They found that on subsequent DUI convictions, the mean of the alcohol program group was 9% lower than that of the

suspended group, and this difference was statistically significant. A subsequent California study undertaken by Tashima and Peck (1986) found that those assigned to a SB 38 alcohol program had a recidivism rate that was 24% lower than offenders receiving only license suspension, a difference that was highly significant statistically. A 1989 study conducted by Tashima and Marelich discovered that, for second offenders, those sentenced to attend a SB 38 program plus license restriction had 32% fewer 1-year major convictions and 26% fewer 2-year major convictions than offenders receiving only license suspension. These differences were also statistically significant.

It appears that the superiority of SB 38 plus license restriction over license suspension in reducing DUI recidivism has increased over time, and the findings of this study confirm this trend. The SB 38 alcohol programs which were evaluated by previous studies were 12 months in length, while those examined in this study were 18 months long. However, given the trend of increasing efficacy on the part of 12-month programs shown in prior studies, and also considering the results of the SB 38 program change analysis conducted as a part of this study, the increased effectiveness (43%) of program plus license restriction over suspension found here is probably part of a long term trend rather than indicative of effects of the added length of the SB 38 programs.

Another possible explanation for the larger superiority of SB 38 program plus license restriction over license suspension found in this study is that all offenders in this study were subject to a 1-year pre-conviction APS license suspension. This suspension often overlaps the post-conviction follow-up period, and would act to reduce driving exposure during this time, thus reducing conviction rates. The previous studies were conducted prior to the implementation of APS, and so this was not a factor affecting recidivism rates in these studies.

In summary, the findings from the second offender analysis confirm results of prior research which shows that it is the combination of alcohol program plus license curtailment that is associated with the lowest recidivism rates for second offenders. Given the number of studies which show this to be the case, the benefits of combining these two sanctions seem fairly convincing. It should be kept in mind, however, that almost all of the research conducted to date is quasi-experimental in nature, and thus the potential effects of pre-existing group differences on recidivism cannot be ruled out entirely. It also should be pointed out that the legislative mandate for this study limited its scope to evaluating DUI recidivism; prior research has shown that results depend on the choice of criterion measure (e.g., alcohol versus non-alcohol, crashes versus convictions). The effects of second offender sanctions on other important outcomes cannot be addressed by this study.

Third-plus offender analysis

The third-plus offender analysis examined the relationships between 3 sanction groups—SB 38 program plus license revocation, license revocation only, and 30-month alcohol program plus license revocation—and recidivism, measured by the number of days to first subsequent DUI arrest which resulted in conviction. The results of this analysis revealed that while recidivism was directionally higher for the SB 38 group than for the 30-month group, this difference was not statistically significant. In contrast, those offenders who received only license revocation had the highest recidivism rate,

and this rate was significantly higher than the corresponding rate for those assigned to a 30-month alcohol program. In fact, those receiving license revocation had 1.7 times the risk of recidivating than offenders assigned to 30-month programs. This relative risk between the groups can be stated another way as well; revocation is associated with an increased risk of 69%, compared to 30-month program assignment.

The primary focus in the third-plus offender analysis is the performance of subjects assigned to 30-month alcohol treatment programs. These programs are relatively new, and this study marks the first time they have been scientifically evaluated. Thus, the findings here cannot be directly compared to results of prior studies.

However, consideration of both prior research as well as the strengths and weaknesses of this study can shed some light on the relative efficacy of the sanction groups found here. There are two possible explanations for the results showing no significant difference in recidivism between the SB 38 and 30-month alcohol program groups. The first is that there are, in fact, no differences between the groups, and this can be understood as a function of the 30-month program simply being an extended version of the 18-month SB 38 program. Wells-Parker et al. (in press), in a meta-analysis of 215 studies of sanction effects for DUI offenders, found that neither the duration of the intervention, nor the number of hours of the intervention, were significantly correlated with DUI recidivism. If the 30-month program is simply 12 more months of the SB 38 program, then the finding here of no difference between the groups is not surprising.

It is also possible that there is a real difference between the two alcohol program groups that was simply not captured in this analysis. This is a reasonable possibility. The timelines for selecting subjects for the study were extended as long as possible (two-and-one-half years) in order to maximize the number of subjects in the 30-month group, but even given this the sample size in the 30-month group was very small (<600). A post hoc power analysis was conducted, which showed that the group sample sizes yielded a power of less than .5. This suggests that treatment group differences may have been present, and that with more 30-month program clients, differences here that were only directional may have become statistically significant.

Supporting the view that differences among the groups may exist that were not discovered in this study are those working in the alcohol treatment field who maintain that the 30-month alcohol programs are not just extensions of the SB 38 programs, but rather are qualitatively different interventions. This contention can not be quantified. Most of the differences involve "more of the same;" additional education/counseling sessions, longer attendance in self-help groups, and more case-management meetings with program staff. There are some additional requirements in the 30-month programs—doing book reports, 120 hours of community service and letters of reference—but whether these constitute a qualitative difference that would impact recidivism is an open question.

It is also likely that if differences exist between programs, it is at least partly the result of different client populations. In conversations between the author of this report and several 30-month program directors, it was pointed out that 30-month clients tend to be motivated individuals. Many of these people have "hit bottom," are further out of denial that they have a problem, and want to make changes in their lives. The

willingness to participate for a long period of time in rehabilitation would certainly suggest that this group is motivated to change. If this is the case, then it could be expected that 30-month clients may have better prospects for not recidivating than offenders in the SB 38 group, apart from any effects the programs may have.

Unfortunately, a conclusive answer to the question of whether real differences exist between the SB 38 and 30-month alcohol programs can not be provided. Given possible self-selection differences between the groups, and power deficiencies of the present study, it is quite possible that such differences do exist. However, based on the data analyzed here, the greater recidivism of the SB 38 group as compared to the 30-month group is directional only, and could well have occurred by chance. Thus, the results do not provide compelling support for the hypothesis that real differences exist between the programs in their impacts on recidivism.

Much more confidence can be placed in the important finding that the recidivism rate for those in the revoked group is significantly higher than the rate for offenders assigned to 30-month alcohol treatment programs. This is not too surprising, especially in light of the results of the second offender analysis, which showed that the SB 38 program groups had significantly fewer average numbers of subsequent DUI convictions than those in the suspended group. As was pointed out in the second offender analysis, this is congruent with prior research (Sadler and Perrine, 1984; Tashima and Peck, 1986; Tashima and Marelich, 1989; Tashima et al., 1993; Tashima and Helander, 1994).

The reasons for the superiority of the 30-month alcohol program relative to the revoked group on subsequent DUI convictions are not completely clear. As shown in the analysis of group differences, offenders in the 30-month group appear to be lower risk than their counterparts in the revoked group. Their willingness to address their alcohol problems, and the apparent lack of such desire on the part of revoked offenders, would suggest that 30-month program clients are more motivated to change. It is plausible to expect that a greater motivation on the part of 30-month clients, as well as other, unknown, differences between these offenders and those in the revoked group, would result in a lower recidivism rate for the 30-month group regardless of sanction impact.

It is also likely that, in addition to pre-existing group differences, the effects of the sanctions themselves were responsible for the differential recidivism results between the two groups. Due to the nature of this quasi-experiment, a final answer can not be definitively provided. However, it can still be said that the results reflect the policy impact of assigning third-plus offenders to 30-month alcohol programs. That is, those offenders assigned to 30-month programs have lower DUI recidivism rates than those receiving only license revocation.

Conclusion

The results presented here both confirm and update findings from previous studies, and reveal relationships between previously unevaluated sanctions and DUI recidivism. While limitations inherent in conducting research on programs linked to the criminal justice system do not allow definitive cause-effect relationships to be established, the

findings here constitute an important evaluation of the policy effects of assigning various sanctions to DUI offenders.

Based on this research, and the results of prior studies, it can be persuasively argued that combining license actions with alcohol treatment represents the most effective sanction combination for combating DUI recidivism. While not evaluated in this study, prior research has demonstrated that license suspension is the most effective sanction in mitigating overall traffic risk (Peck, 1991), and this strengthens the recommendation to combine license actions with alcohol treatment. License curtailment reduces driving exposure and prompts more cautious driving (Sadler and Perrine, 1984; Ross and Gonzales, 1988; DeYoung, 1990), while alcohol education/treatment programs allow offenders to address their alcohol problems. There are both logical and empirical reasons for combining the two.

This approach has been adopted in California, with the advent of APS license suspension in July, 1990. While the effects of APS are currently being investigated and are not available at this time, the results of the second offender analysis in this study, viewed in the context of prior research, suggest the possibility that APS may have enhanced the effectiveness of SB 38 plus license restriction, relative to suspension alone, in reducing DUI recidivism.

Finally, while license actions combined with alcohol treatment represent the best currently available and utilized sanction combination for reducing DUI recidivism, other sanctions may become available in the future which may, when added to this combination, further enhance its positive effect. One possibility is ignition interlock devices. Unfortunately, the research conducted to date on the efficacy of these devices is generally not of high quality, and provides mixed results. However, properly utilized, perhaps being required for a period of time following the license curtailment period, such devices may provide added benefit to the effectiveness of alcohol treatment plus license curtailment.

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APPENDIX A

Senate Bill No. 1344

CHAPTER 803

An act to amend sections 11837, 11837.1, 11837.2, 11837.4, and 11837.5 of the Health and Safety Code, to amend Sections 23161, 23166, 23181, and 23186 of the Vehicle Code, and to repeal Section 13 of Chapter 1041 of the Statutes of 1987, relating to driving offenses.

[approved by Governor September 25, 1989, Filed with Secretary of State September 26, 1989.]

LEGISLATIVE COUNSEL'S DIGEST

SB 1344 Seymour. Driving offenses: alcohol service programs.

(1) Under existing law, the State Department of Alcohol and Drug Programs licenses alcohol service programs to which are referred persons convicted of 2nd or subsequent offenses of driving while under the influence of an alcoholic beverage, any drug, or both, driving with an excessive blood-alcohol concentration, or driving while addicted to any drug. The programs are of one year or 30 months' duration, and existing law authorizes referral of 1st offenders to a licensed program under specified conditions in any county which has approved the program.

This bill would increase the shorter of those programs to require a duration of 18 months, would add specified requirements to those programs, and would make conforming changes.

The bill would also require counties to establish alcohol and drug education and counseling programs, as specified, for 1st offenders, which the bill would require to be licensed, unless they make a specified showing to the State Department of Alcohol and Drug Programs, thereby imposing a state-mandated local program. The bill would make confirming changes. The bill would also require programs attended by 1st offenders to be licensed.

The bill would require the Department of Motor Vehicles to make a specified study of the program participants' recidivism and report its findings to the Legislature on or before January 1, 1995. The bill would also repeal provisions requiring the State Department of Alcohol and Drug

Programs to evaluate the effectiveness of 30-month treatment programs and report thereon to the Legislature on or before January 1, 1993.

The bill would require the license fees charged by the State Department of Alcohol and Drug Programs for the licensing of programs to be sufficient to cover the administrative costs of the state department and to reimburse the Department of Motor Vehicles for the costs of evaluation and report.

(2) The California Constitution requires the state to reimburse local agencies and school districts for certain costs mandated by the state. Statutory provisions establish procedures for making that reimbursement, including the creation of a State Mandates Claims Fund to pay the costs of mandates which do not exceed $1,000,000 statewide and other procedures for claims whose statewide costs exceed $1,000,000.

This bill would provide that, if the Commission on State Mandates determines that this bill contains costs mandated by the state, reimbursement for those costs shall be made pursuant to those statutory procedures and, if the statewide cost does not exceed $1,000,000, shall be made from the State Mandates Claims Fund.

The people of the State of California do enact as follows:

SECTION 1. Section 11837 of the Health and Safety Code is amended to read:

11837. (a) Pursuant to the provisions of law relating to suspension of a person's privilege to operate a motor vehicle upon conviction for driving while under the influence of an alcoholic beverage or under the combined influence of an alcoholic beverage and any drug, as set forth in paragraph (3) or (4) of subdivision (a) of Section 13352 of the Vehicle Code, the Department of Motor Vehicles shall restrict the driving privilege pursuant to Section 13352.5 of the Vehicle Code, if the court has notified the department pursuant to Section 13352.5 of the Vehicle Code that the person convicted of that offense has consented to participate for at least 18 months in a program designed to offer alcohol services to problem drinkers that is licensed pursuant to this chapter.

(b) In determining whether to refer a person, who is ordered to participate in a program pursuant to Section 23161 in a licensed alcohol or drug education and counseling services program or pursuant to Section 23166, 23171, 23176, 23181, 23186, or 23191 of the Vehicle Code, to a licensed 18-month or 30-month program, the court may consider any relevant information about the person made available pursuant to a presentence investigation, which is permitted but not required under Section 23205 of the Vehicle Code, or other screening procedure. That information shall not be furnished, however, by any person who also provides services in a privately operated, licensed program or who has any direct interest in a privately operated, licensed program. In addition, the court shall obtain from the Department of Motor Vehicles a copy of the person's driving record to determine whether the person is eligible to participate in a licensed 18-month or 30-month program pursuant to this chapter.

(c) The court may, as a condition of probation, refer a first offender to a licensed program pursuant to Section 23161 or 23181 of the Vehicle Code to attend all of the education, group counseling, and interview sessions described in Sections 9834, 9836, and 9838 of Title 9 of the California Code of Regulations if ordered to participate in six, nine, or 12 months of program activities. Notwithstanding Section 13352.5 of the Vehicle Code, if a first offender is referred to a licensed program pursuant to Section 23161 or 23181 of the Vehicle Code, that person may participate in a program if convicted of another offense punishable under Section 23165 or 23185 of the Vehicle Code.

(d) the court may, subject to Sections 11837.2, 11837.3, and 11838.2 and as a condition of probation, refer a person to a licensed program, even though the person's privilege to operate a motor vehicle is restricted, suspended, or revoked. An 18-month program described in Section 23166 or 23186 of the Vehicle Code or a 30-month program described in Section 23171, 23176, or 23191 or the Vehicle Code may include treatment of family members and significant other persons related to the convicted person with the consent of those family members and others as described in subdivision (e) of Section 9838 of Title 9 of the California Administrative Code, if there is no increase in the costs of the program to the convicted person.

(e) As used in the chapter, "program" means a program which has been recommended by the county board of supervisors to the department and possesses a valid license issued by the department.

SEC.2. Section 11837.1 of the Health and Safety Code is amended to read:

11837.1. (a) In utilizing any program defined in subdivision (e) of Section 11837, the court may require periodic reports concerning the performance of each person referred to and participating in a program. The program shall provide the court, the Department of Motor Vehicles, and the person referred to and participating in a program, with an immediate report of any failure of the person to comply with the program's rules and policies.

(b) (1) If, at any time after entry into or while participating in a program, a participant who is referred to an 18-month program described in Section 23166 or 23186 of the Vehicle Code fails to comply with the rules and policies of the program, and that fact is reported, the Department of Motor Vehicles shall revoke or suspend the privilege of that person to operate a motor vehicle for the period prescribed by law in accordance with section 13352.5 of the Vehicle Code, except as otherwise provided in this section. The Department of Motor Vehicles shall notify the person and the court of its intended action and shall inform the person of the opportunity to be reinstated in the program and to avoid suspension of the driving privilege.

(2) Notwithstanding paragraph (1), the Department of Motor Vehicles shall not suspend the driving privilege pursuant to paragraph (1) if the court, upon the petition of the person, determines that the person should be reinstated in the 18-month program. The determination by the court shall be made within 45 days of the notice from the department of the

intended action or such additional time, not to exceed an additional 45 days, for the determination as is required, ordered, and transmitted to the Department of Motor Vehicles by the court, which additional time is not caused by any action or failure to act by the person. Upon timely presentation by the person to the Department of Motor Vehicles of evidence satisfactory to the Department of Motor Vehicles that the court of jurisdiction has consented to the person's reinstatement in the program and that the person is currently participating in the program, the Department of Motor Vehicles shall continue in effect the restriction of the driving privilege prescribed by Section 13352.5 of the Vehicle Code unless the person has previously been reinstated in the program on two or more prior occasions, in which case the department shall suspend or revoke the driving privilege for the time prescribed in paragraph (3) or (4) of subdivision (a) of Section 13352 of the Vehicle Code. If the court fails to grant the person's petition for reinstatement in the program, the court shall suspend the person's driving privilege until the time the person's driving privilege is suspended or revoked by the Department of Motor Vehicles pursuant to subdivision (d) of Section 13352.5 of the Vehicle Code.

(c) If, at any time after referral to or while participating in a program, a participant who is referred to an alcohol or drug education and counseling program pursuant to Section 23161 or 23181 of the Vehicle Code, to an 18-month program pursuant to Section 23166 or 23186 of the Vehicle Code, or to a 30-month program pursuant to Section 23171, 23176, or 23191 of the Vehicle Code, refuses, for any reason, to consent to a chemical test that he or she is required to submit to pursuant to Section 23157 of the Vehicle Code, his or her participation in the program shall be terminated and, the Department of Motor Vehicles shall immediately suspend or revoke his or her privilege to operate a motor vehicle for the period prescribed in Section 13352 of the Vehicle Code, according to the offense for which he or she was convicted immediately prior to referral to the program. The period of suspension or revocation shall be in addition to the period of suspension imposed by the Department of Motor Vehicles pursuant to Section 13353 of the Vehicle Code.

(d) if the department withdraws the license of a program, the department shall immediately notify the Department of Motor Vehicles of those persons who do not commence participation in a licensed program within 21 days from the date of the withdrawal of the license of the program in which the persons were previously participating. The Department of Motor Vehicles shall suspend or revoke, for the period prescribed by law, the privilege to operate a motor vehicle of each of those persons referred to an 18-month program pursuant to Section 23166 or 23186 of the Vehicle Code or to a 30-month program pursuant to Section 23171, 23176, or 23191 of the Vehicle Code.

SEC. 3. Section 11837.2 of the Health and Safety Code is amended to read:

11837.2. (a) The court may refer persons only to licensed programs. Subject to these provisions, a person is eligible to participate in the program if it is operating in (1) the county where the person is convicted,

or (2) the county where the person resides, or (3) a county that has an agreement with such person's county of residence pursuant to Section 11838, or (4) a county to which a person may request transfer pursuant to subdivision (d).

If a person granted probation under Sectin 23166 or 23186 of the Vehicle Code cannot be referred to a licensed 18-month progrram pursuant to this section, Section 13352.5 of the Vehicle Code does not apply.

(b) If a person has consented to participate in a licensed program and the county where the person is convicted is the same county in which the person resides, the court may order the person to participate in a licensed program within that county, or, if that county does not have a licensed program, the court may order that person to participate in a licensed program within another county, pursuant to Section 11838.

(c) If a person has consented to participate in a licensed program in the county in which that person resides or in a county in which the person's county of residence has an agreement pursuant to Section 11837, and the county where the person is convicted is not the county where the person resides, and if the court grants the person summary probation, the court may order the person to participate in a licensed program in that county. In lieu of summary probation, the court may utilize the probation officer to implement the orders of the court. If the county in which the person resides does not have a licensed program or an agreement with another county pursuant to Section 11838 and the person consents, the court may order the person to participate in a licensed program within the county where that person is convicted or in a county with which the county has an agreement pursuant to Section 11838.

(d) Except as otherwise provided in subdivision (e), subsequent to a person's commencement of participation in a program, the person may request transfer to another licensed program (1) in the same county in which the person has commenced participation in the program, upon approval of that county's alcohol program administrator, or (2) in a county other than the county in which the person has commenced participation in the program, upon approval of the alcohol program administrator of the county in which the person is participating and the county to which the person is requesting transfer.

(e) Subdivision (d) does not apply (1) if the court has ordered the person to participate in a specific licensed program, unless the court orders the transfer or, (2) if the person is under formal probation, unless the probation officer consents to the transfer. The department shall establish reporting forms and procedures to ensure that the court receives notice of any program transfer pursuant to this subdivision or subdivision (d).

(f) Jurisdiciton of all postconviction matters arising pursuant to this section may be retained by the court of conviction.

(g) The department, in cooperation with the Department of Motor Vehicles and the alcohol program administrators, shall esstablish procedures to ensure the effective implementation of this section.

SEC.4. Section 11837.4 of the Health and Safety Code is amended to read:

11837.4 (a) No program, regardless of how it is funded, may be licensed unless all of the requirements of this chapter and of the regulations adopted pursuant to this chapter have been met.

(b) Each licensed program shall include, but not be limited to, the following:

(1) For the alcohol or drug education and counseling services programs specified in subdivision (b) of Section 11837, each program shall provide for close and regular face-to-face interviews. For the 18-month programs specified in subdivision (a) of Section 11837, each program shall provide for close and regular supervision of the person, including face-to-face interviews at least once every other calendar week, regarding the person's progress in the program. In the last six months of the 18-month program, the provider shall monitor the participant's community reentry activity with self-help groups, employment, family, and other areas of self-improvement. Unless otherwise ordered by the court, the provider's monitoring services is limited to not more than six hours. For the 30-month programs specified in subdivision (b) of Section 23171, subdivision (b) of Section 23176, and subdivision (b) of Section 23191 of the Vehicle Code, each program shall provide for close and regular supervision of the person, including regular, scheduled face-to-face interviews over the course of 30 months regarding the person's progress in the program and recovery from problem drinking, alcoholism, chemical dependency, or polydrug abuse, as prescribed by regulations of the department. The interviews in any of those programs shall be conducted individually with each person being supervised and shall occur at times other than when the person is participating in any group or other activities of the program. No program activity in which the person is participating shall be interrupted in order to conduct the individual interviews.

(2) The department shall approve all fee schedules for the programs and shall require that each program be self-supporting from the participants' fees and that each program provide for the payment of the costs of the program by participants at times and in amounts commensurate with their ability to pay in order to enable these persons to participate. Each program shall make provision for persons who can successfully document current inability to pay the fees. Only the department may establish the criteria and procedures for determining a participant's ability to pay. The department shall ensure that the fees are set at amounts which will enable programs to provide adequately for the immediate and long-term continuation of services required pursuant to this chapter. The fees shall be used only for the purposes set forth in this chapter, except that any profit or surplus, that does not exceed the maximum level established by the department, may be utilized for any purposes allowable under any other provisions of law. In its regulations, the department shall define, for the purposes of this paragraph, taking into account prudent accounting, management, and business practices and procedures, the terms "profits" and "surplus." The department shall fairly construe these provisions so as not to jeopardize fiscal integrity of

the programs. The department may not license any program if the department finds that any element of the administration of the program does not assure the fiscal integrity of the program

(3) The licensed programs described in paragraph (1) shall include a variety of treatment services for problem drinkers, alcoholics, chemical dependents, and polydrug abusers or shall have the capability of referring the persons to, and regularly and closely supervising the persons while in, any appropriate medical, hospital, or licensed residential treatment services or self-help groups for their problem drinking, alcoholism, chemical dependency, or polydrug abuse problem. In addition to the requirements of paragraph (1), the department shall prescribe in its regulations what other services the program shall provide, at a minimum, in the treatment of participants, which services may include lectures, classes, group discussions, group counseling, or individual counseling in addition to the interviews required by paragraph (1), or any combination thereof. However, any group discussion or counseling activity, other than classes or lectures, shall be regularly scheduled to consist of not more than 15 persons, except that they may, on an emergency basis, exceed 15, but not more than 17, persons, at any one meeting. At no time shall there be more than 17 persons in attendance at any one meeting. For the 30-month programs specified in subdivision (b) of Section 23171, subdivision (b) of Section 23176, and Subdivision (b) of Section 23191 of the Vehicle Code, each licensed program shall include a method by which each participant shall maintain a compendium of probative evidence, as prescribed in the regulations of the department, on a trimonthly basis demonstrating a performance of voluntary community service by the participant, including, but not limited to, the prevention of drinking an driving, the promotion of safe driving, and responsible attitudes toward the use of chemicals of any kind, for not less than 120 hours and not more than 300 hours, as determined by the court, with one-half of that time to be served during the initial 18 months of program participation and one-half of that time to be served in the final 12 months. In determining whether or not the participant has met the objectives of the program, the compendium of evidence shall also include, and the court shall consider, the participant's demonstration of significant improvement in any of the following areas of personal achievement:

(A) Significant improvement in occupational performance, including efforts to obtain gainful employment.

(B) Significant improvement in physical and mental health.

(C) Significant improvement in family relations, including financial obligations.

(D) Significant improvement in financial affairs and economic stability.

The compendium of evidence shall be maintained by the participant for review by the program, court, probation officer, or other appropriate governmental agency. The program officials, unless prohibited by the referring court, shall make provisions for a participant to voluntarily enter, using the participant's own resources, a licensed chemical dependency recovery hospital or residential treatment program which has a valid license issued by the State of California to provide alcohol or drug

services, and to receive three weeks of program participation credit for each week of that treatment, not to exceed 12 weeks of program participation credit, but only if the treatment is at least two weeks in duration. The program shall document probative evidence of this hospital or residential care treatment in the participant's program file.

(4) In order to assure program effectiveness, the department shall require, whenever appropriate, that the licensed program provides services to ethnic minorities, women, youth, or any other group that has particular needs relating to the program.

(5) The goal of each program shall be to assist persons participating in the program to recognize their chemical dependency and to assist them in their recovery.

(6) Each program shall establish a method by which the court, the Department of Motor Vehicles, and the person are notified in a timely manner of the person's failure to comply with the program's rules and regulations.

(c) No program may be licensed unless the county complies with the requirements of subdivision (b) of Section 11812. The provider of a program that offers an alcohol or drug education and counseling services program, an 18-month program, or a 30-month program or any or all of those programs described in this section shall be required to obtain only one license. The department's regulations shall specify the requirements for the establishment of each program. The license issued by the department shall identify the program or programs licensed to operate.

(d) Departmental approval for the establishment of a 30-month program by a licensed 18-month program is contingent upon approval by the county alcohol program administrator, based upon confirmation that the program applicant is capable of providing the service and that the fiscal integrity of the program applicant will not be jeopardized by the operation of the program.

The court shall refer a person to a 30-month treatment program only if a 30-month program exists or is provided for in the jurisdiction of the court.

(e) A county or program shall not prescribe additional program requirements unless the requirements are specifically approved by the department.

(f) The department may license a program on a provisional basis.

SEC. 4.5 Section 11837.5 of the Health and Safety Code is amended to read:

11837.5. (a) No person may participate in any program that has not been licensed by the department pursuant to this chapter.

(b) The department shall charge reasonable fees for licensing drinking driver programs. The department shall set the fees in an amount sufficient to cover all administrative costs incurred by the department and to reimburse the Department of Motor Vehicles for the costs of the evaluation and report required by Section 9 of Senate Bill 1344 of the 1989-90 Regular Session.

(c) The department may fine a provider who is delinquent in the payment of licensing fees. The department shall deposit fines collected

from delinquent providers in the Drinking Driver Program Licensing Trust Fund, and the revenues from the fines shall be used, upon appropriation, to offset costs incurred by the department in the administration of the program and to reimburse the Department of Motor Vehicles for the costs of the evaluation and report required by Section 9 of Senate Bill 1344 of the 1989-90 Regular Session.

(d) If a program fails to pay licensing fees or assessed fines, the department may deny an initial license or revoke an existing license.

(e) There is established in the State Treasury a Drinking Driver Program Licensing Trust Fund. All fees and fines collected from drinking driver programs shall be deposited in this fund. The money in the fund shall be available when appropriated by the Legislature.

(f) The department shall prepare an annual report on the assets, liabilities, and balance in the Drinking Driver Program Licensing Trust Fund. The report shall also include an itemized statement of income and expenses for the trust fund since the last report. The department shall submit the report to the Legislature and shall furnish a copy of the report, upon request, to any provider of a drinking driver program.

(g) Licensing fees shall be evaluated annually and based on the department's projected costs for the forthcoming fiscal year. Any excess fees remaining in the Drinking Driver Program Licensing Trust Fund at the close of the fiscal year shall be carried forward and taken into consideration in establishment of fees for the subsequent fiscal year. If the department proposes to increase the licensing fees, the department shall justify the increase to the Legislature by showing that sufficient assets are not currently available in the Drinking Driver Program Licensing Trust Fund and that current licensing fee collections are not sufficient to support current or planned expenses of the department for drinking driver program licensing activities.

(h) Licensing fee collection procedures, which include the assessment of fines for delinquent fee payments, shall be defined in regulations adopted pursuant to this chapter.

SEC. 5. Section 23161 of the Vehicle Code is amended to read: 23161. (a) Except as provided in subdivision (f), if the court grants probation to any person punished under Section 23160, in addition to the provisions of Section 23206 and any other terms and conditions imposed by the court, the court shall impose as a condition of probation that the person be subject to on of the following:

(1) Be confined in the county jail for at least 48 hours but not more than six months and pay a fine of at least three hundred ninety dollars ($390) but not more than one thousand dollars ($1,000). The court may order the Department of Motor Vehicles to suspend the privilege to operate a motor vehicle pursuant to paragraph (1) of subdivision (a) of Section 13352 when this condition of probation is imposed.

(2) Pay a fine of at least three hundred ninety dollars (4390) but not more than one thousand dollars ($1,000) and, if the person gives proof of ability to respond in damages as defined in Section 16430 to the Department of Motor Vehicles, have the privilege to operate a motor vehicle restricted for 90 days to necessary travel to and from that

person's place of employment and to and from the program described in subdivision (b) and, if driving a motor vehicle is necessary to perform the duties of the person's employment, restricted to driving in that person's scope of employment. Whenever the driving privilege is restricted under this section, proof of ability to respond in damages as defined in Section 16430 shall be maintained for three years.

(b) (1) Each county shall, through the county alcohol program administrator, determine its ability to establish, through public or private resources, a program, which is self-supporting through fees collected from program participants, of alcohol or drug education and counseling services, of at least three months' duration and totaling at least 30 hours of direct education and counseling services, which shall be authorized by each county and licensed by, and operated under general regulations established by, the State Department of Alcohol and Drug Programs. A county which shows the State Department of Alcohol and Drug Programs that it has insufficient resources, insufficient potential program participants, or other material disadvantages is not required to establish such a program.

(2) The state Department of Alcohol and Drug Programs may license an alcohol or drug education program that is less than 30 hours in length in any county where the board of supervisors has provided the showing pursuant to paragraph (1), and the State Department of Alcohol and Drug Programs has upheld that showing. The shorter program is subject to all other applicable regulations developed by that state department pursuant to paragraph (3) of subdivision (b) of Section 11837.4.

(3) In any county where the board of supervisors has approved and the State Department of Alcohol and Drug Programs has licensed such a program or programs, the court shall also impose as a condition of probation that the driver shall participate in, and successfully complete, an alcohol or drug education and counseling program, or both of these programs, in the drive's county of residence or employment, as designated by the court.

(c) Each county which has approved an alcohol or drug education program or programs and which is licensed by the State Department of Alcohol and Drug Programs shall make provision for persons who can document current inability to pay the program fee, in order to enable those persons to participate. The county shall require that the program report the failure of a person referred to the program to enroll in the program to the referring court.

(d) In order to assure effectiveness of the alcohol or drug education and counseling program, the county shall provide, as appropriate, services to ethnic minorities, women, youth, or any other group that has particular needs related to the program.

(e) (1) Any person required to successfully complete an alcohol or drug education and counseling program as a condition of probation shall enroll in the program and, except when enrollment is required in a program that is required to report failures to enroll to the court, shall furnish proof of the enrollment to the court within the period of time and in the manner specified by the court. The person shall also participate in and

successfully complete the program and, shall furnish proof of successful completion within the period of time and in the manner specified by the court. The court shall revoke the person's probation pursuant to Section 23207, except for good cause shown, for the failure to comply with this paragraph.

(2) An alcohol or drug education and counseling program shall report to the court within the period of time and in the manner specified by the court any person who fails to successfully complete the program.

(3) The court, in establishing the reporting requirements in this subdivision, shall consult with the county alcohol program administrator. The county alcohol program administrator shall coordinate the reporting requirements with the Department of Motor Vehicles and the State Department of Alcohol and Drug Programs. That reporting shall ensure that all persons who, after being ordered to attend and complete a program, may be identified for either (1) failure to enroll in, or failure to successfully complete, the program, or (2) successful completion of the program as ordered.

(f) Notwithstanding subdivision (a), if the offense occurred in a vehicle requiring a driver with a class 1 or class 2 driver's license or with a certificate issued pursuant to Section 12804.1, the court shall upon conviction order the department to suspend the driver's privilege pursuant to paragraph (1) of subdivision (a) of Section 13352.

SEC. 6. Section 23166 of the Vehicle Code is amended to read:

23166. If the court grants probation to any person punished under Section 23165, in addition to the provisions of Section 23206 and any other terms and conditions imposed by the court, the court shall impose as conditions of probation that the person be subject to either subdivision (a) or (b), as follows:

(a) Be confined in the county jail for at least 10 days but not more than one year and pay a fine of at least three hundred ninety dollar ($390) but not more than one thousand dollars ($1,000). The person's privilege to operate a motor vehicle shall be suspended by the Department of Motor Vehicles pursuant to paragraph (3) of subdivision (a) of Section 13352.

(b) All of the following:

(1) Be confined in the county jail for at least 48 hours but not more than one year.

(2) Pay a fine of at least three hundred ninety dollars ($390) but not more than one thousand dollars ($1,000).

(3) If the person gives proof of ability to respond in damages as defined in Section 16430 to the Department of Motor Vehicles, have the privilege to operate a motor vehicle be restricted by the Department of Motor Vehicles pursuant to Section 13352.5, for one year, to necessary travel to and from that person's place of employment and to and from the applicable treatment program described in paragraph (4), and if driving a motor vehicle is necessary to perform the duties of the person's employment, restricted to driving in that person's scope of employment. The Department of Motor Vehicles shall not suspend the person's privilege to operate a motor vehicle under Section 13352, as provided in Section 13352.5, unless the offense occurred in a vehicle requiring a driver

with a class 1 or class 2 driver's license or with a certificate specified in Section 12804.1.

(4) Either of the following:

(A) Participate, for at least 18 months subsequent to the underlying conviction and in a manner satisfactory to the court, in a program approved pursuant to Chapter 9 (commencing with Section 11837) of Part 2 of Division 10.5 of the Health and Safety Code, as designated by the court. The program shall provide for persons who cannot afford the program fee pursuant to paragraph (2) of the subdivision (a) of Section 11837.4 of the Health and Safety Code in order to enable those persons to participate.

(B) Participate, for at least 30 months subsequent to the underlying conviction and in a manner satisfactory to the court, in a program approved pursuant to Chapter 9 (commencing with Section 11837) of Part 2 of Division 10.5 of the Health and Safety Code. A person ordered to treatment pursuant to this subparagraph shall apply to the court or to a board of review, as designated by the court, at the conclusion of the program to obtain the court's order of satisfaction, Only upon the granting of that order of satisfaction by the court may the program issue its certificate of successful completion and report the completion to the Department of Motor Vehicles. A failure to obtain an order of satisfaction at the conclusion of the program is a violation of probation. In order to enable all required persons to participate, each person shall pay the program costs commensurate with the person's ability to pay as determined pursuant to Section 11837.4 of the Health and Safety Code. No condition of probation required pursuant to this subparagraph is a basis for reducing any other probation requirement or for avoiding the mandatory license revocation provisions of paragraph (5) of subdivision (a) of Section 13352.

SEC. 7. Section23181 of the Vehicle Code is amended to read:

23181. (a) If the court grants probation to any person punished under Section 23180, in addition to the provisions of Section 23206 and any other terms and conditions imposed by the court, the court shall impose as a condition of probation that the person be confined in the county jail for at least five days but not more than one year and pay a fine of at least three hundred ninety dollars ($390) but not more than one thousand dollars ($1,000). The person's privilege to operate a motor vehicle shall be suspended by the Department of Motor Vehicles pursuant to paragraph (2) of subdivision (a) of Section 13352.

(b) (1) In any county where the county alcohol program administrator has certified, and the board of supervisors has approved, such a program or programs, the court shall also impose as a condition of probation that the driver shall participate in, and successfully complete, an alcohol or drug education and counseling program, established pursuant to subdivision (b) of Section 23161, as designated by the court.

(2) In any county where the board of supervisors has approved and the State Department of Alcohol and Drug Programs has licensed such a program or programs, the court shall also impose as a condition of probation that the driver participate in, and successfully complete, an

alcohol or drug education and counseling program, or both of these programs, in the driver's county of residence or employment, as designated by the court.

(c) Each county which has approved an alcohol or drug education program or programs and which is licensed by the State Department of Alcohol and Drug Programs shall make provision for persons who can document current inability to pay the program fee, in order to enable those persons to participate. The county shall require that the program report the failure of a person referred to the program to enroll in the program to the referring court.

(d) In order to assure effectiveness of the alcohol or drug education and counseling program, the county shall provide, as appropriate, services to ethnic minorities, women, youth, or any other group that has particular needs related to the program.

(e) (1) Any person required to successfully complete an alcohol or drug education and counseling program as a condition of probation shall enroll in the program and, except when enrollment is required in a program that is required to report failures to enroll to the court, shall furnish proof of the enrollment to the court within the period of time and in the manner specified by the court. The person shall also participate in and successfully complete the program and shall also participate in and successfully complete the program and shall furnish proof of successful completion within the period of time and in the manner specified by the court. The court shall revoke the person's probation pursuant to Section 23207, except for good cause shown, for the failure to comply with this paragraph.

(2) An alcohol or drug education and counseling program shall report to the court within the period of time and in the manner specified by the court any person who fails to successfully complete the program.

(3) The court, in establishing the reporting requirements in this subdivision, shall consult with the county alcohol program administrator. The county alcohol program administrator shall coordinate the reporting requirements with the Department of Motor Vehicles and the State Department of Alcohol and Drug Programs. That reporting shall ensure that all persons who, after being ordered to attend and complete a program, may be identified for either (1) failure to enroll in, or failure to successfully complete, the program, or (2) successful completion of the program as ordered.

SEC. 8. Section 23186 of the Vehicle Code is amended to read:

23186. If the court g4rants probation to any person punished under Section 23185, in addition to the provisions of Section 23206 and any other terms and conditions imposed by the court, the court shall impose as conditions of probation that the person be subject to all of the provisions of either subdivision (a) or (b), as follows:

(a) Be confined in the county jail for at least 120 days and pay a fine of at least three hundred ninety dollars ($390) but not more than five thousand dollars ($5,000). The person's privilege to operate a motor vehicle shall be revoked by the Department of Motor Vehicles pursuant to paragraph (4) of subdivision (a) of Section 13352.

(b) All of the following:

(1) Be confined in the county jail for at least 30 days but not more than one year.

(2) Pay a fine of at least three hundred ninety dollars ($390) but not more than one thousand dollars ($1,000).

(3) Pursuant to Section 13352.5, have the privilege to operate a motor vehicle be suspended for one year by the Department of Motor Vehicles and, after that one-year period, if the person gives proof of ability to respond in damages as defined in Section 16430 to the Department of Motor Vehicles, have the privilege restricted by the Department of Motor Vehicles for two additional years to necessary travel to and from that person's place of employment, and to and from the treatment program described in paragraph (4) and, if driving a motor vehicle is necessary to perform the duties of the person's employment, restricted to driving in that person's scope of employment. The Department of Motor Vehicles shall not revoke the person's privilege to operate a motor vehicle under Section 13352, as provided in Section 13352.5, unless the offense occurred in a vehicle requiring a driver with a class 1 or class 2 driver's license or with a certificate specified in Section 12804.1.

(4) Participate, for at least 18 months and in a manner satisfactory to the court, in a program approved pursuant to Chapter 9 (commencing with Section 11937) of Part 2 of Division 10.5 of the Health and Safety Code, as designated by the court.

SEC. 9. The Department of Motor Vehicles shall conduct an evaluation of the effects of the alcohol or drug education program authorized pursuant to Sections 23161 and 23181 of the Vehicle Code, the 18-month program authorized pursuant to Sections 23166 and 23186 of the Vehicle Code, and the 30-month program authorized pursuant to Sections 23171, 23176, and 23191 of the Vehicle Code. For the purposes of this section, the evaluation shall be limited to the study of program participants' subsequent violations of section 23103, as specified in Section 23103.5 or Section 23152 or 23153, of the Vehicle Code. The department shall report its findings to the Legislature on or before January 1, 1995. The department shall be reimbursed for the costs of the evaluation and report pursuant to Section 11837.5 of the Health and Safety Code.

SEC. 10 Section 13 of Chapter 1041 of the Statutes of 1987 is repealed.

SEC. 11. Notwithstanding Section 17610 of the government Code, if the Commission on State Mandates determines that this act contains costs mandated by the state, reimbursement to local agencies and school districts for those costs shall be made pursuant to Part 7 (commencing with Section 17500) of Division 4 of Title 2 of the Government Code. If the statewide cost of the claim for reimbursement does not exceed one million dollars ($1,000,000), reimbursement shall be made from the State Mandates Claims Fund. Notwithstanding Section 17580 of the Government Code, unless otherwise specified in this act, the provisions of this act shall become operative on the same date that the act takes effect pursuant to the California Constitution.

APPENDIX B

Comparison of Mean Squares, *F* Values and Significance Levels for Two Approaches to Select Covariates, Second Offenders

Table B1

Summary ANCOVA Table: Regression Approach

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates*	15	2.08	18.11	.0001
Sanction group	2	3.34	29.08	.0001
Error	27275	.11		

*Includes the covariate x sanction group interaction for each of the 2 covariates that violate the equal slopes ANCOVA assumption.

Table B2

Summary ANCOVA Table: Regression Plus Discriminant Analysis Approach

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates*	31	1.09	9.46	.0001
Sanction group	2	3.31	28.85	.0001
Error	27259	.11		

*Includes the covariate x sanction group interaction for each of the 5 covariates that violate the equal slopes ANCOVA assumption.

APPENDIX C

Comparison of ANCOVA with Logistic Regression Results for Second Offenders

Table C1

ANCOVA Results, Second Offenders*

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates				
Sex	1	2.249	19.59	.0001
Class	1	1.295	11.28	.0008
3-year prior convictions	1	4.787	41.70	.0001
3-year prior major convictions	1	0.754	6.57	.0104
ZIP Code % welfare	1	2.764	24.08	.0001
ZIP Code moving violation average	1	1.124	9.80	.0018
ZIP Code drive alone	1	1.506	13.12	.0003
ZIP Code % black	1	0.487	4.24	.0394
ZIP Code % unemployed	1	0.611	5.33	.0210
ZIP Code injury crash average	1	1.873	16.32	.0001
Age	1	0.257	2.24	.1347
Sanction group	2	3.335	29.06	.0001

*The ANCOVA results presented in the body of the report are based upon Type I SS (sequential), with sanction being adjusted for the covariates but not for the 2 covariate x sanction interactions. However, because the SAS LOGISTIC/CATMOD procedures are based upon Type III SS (unique), for the sake of comparability the ANCOVA results presented here are also based upon Type III SS.

Appendix C (continued)

Table C2

Logistic Regression Results, Second Offenders

Source of variation	Degrees of freedom	Chi-square	Significance level
Covariates			
Intercept	1	64.42	.0000
Sex	1	18.21	.0000
Class	1	10.83	.0010
3-year prior convictions	1	41.86	.0000
3-year prior major convictions	1	7.53	.0061
ZIP Code % welfare	1	23.45	.0000
ZIP Code moving violation average	1	13.75	.0002
ZIP Code drive alone	1	10.77	.0010
ZIP Code % black	1	1.81	.1785
ZIP Code % unemployed	1	3.94	.0471
ZIP Code injury crash average	1	19.56	.0000
Age	1	2.57	.1092
Sanction Group	2	48.37	.0000