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DEPARTMENT OF MOTOR VEHICLES

AN EVALUATION OF THE SPECIFIC DETERRENT EFFECT OF VEHICLE IMPOUNDMENT ON SUSPENDED, REVOKED AND UNLICENSED DRIVERS IN CALIFORNIA

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13. ABSTRACT (Maximum 200 words)
While license suspension/revocation has been shown to be effective, it is also known that most suspended/revoked (S/R) drivers violate their illegal driving status and continue to drive, accruing traffic convictions and becoming involved in crashes. In an attempt to strengthen license actions and to better control S/R and unlicensed drivers, California enacted two laws effective January 1995 which provide for the impoundment/forfeiture of vehicles driven by S/R and unlicensed drivers. The current study evaluates the impact of vehicle impoundment on the 1-year subsequent driving behavior of S/R and unlicensed drivers who are subject to it.

The subsequent driving records of drivers whose vehicles were impounded were compared to a very similar group whose vehicles were not impounded. These group comparisons were done for both first offenders (e.g., those with no prior driving while suspended (DWS)/driving while unlicensed (DWU) convictions) and repeat offenders. The results showed that impounded first offenders had 23.8% fewer DWS/DWU convictions, 18.1% fewer traffic convictions and 24.7% fewer crashes than non-impounded first offenders. These group differences are even larger for repeat offenders. Impounded repeat offenders had 34.2% fewer DWS/DWU convictions, 22.3% fewer traffic convictions and 37.6% fewer crashes than non-impounded repeat offenders. These findings provide strong support for impounding vehicles driven by S/R and unlicensed drivers.

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PREFACE

This report is the final product of an evaluation of the specific deterrent effects of vehicle impoundment on suspended/revoked and unlicensed drivers in California. It is part of a larger project funded by the National Highway Traffic Safety Administration (NHTSA) which is being jointly undertaken by the California Department of Motor Vehicles (DMV) and the National Public Services Research Institute (NPSRI). The report was prepared by the Research and Development Branch of the DMV 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 NHTSA, NPSRI or the State of California.

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A number of individuals made valuable contributions to this project, and the author would like to acknowledge and express appreciation for their efforts. The study would not have been possible without vehicle impoundment data provided by police departments and courts. The following individuals were instrumental in providing these data: Connie Weiman, Stockton Police Department; Bea Gin, Stockton Municipal Court; Sergeant Steve Moss and Fiona Greenhalgh, San Diego Police Department; Marita Ford and John Moore, Riverside County Superior and Municipal Courts; Cyndy Ellis, Riverside Police Department, and; Charlene French and Christine Nail, Santa Barbara Police Department.

Raymond C. Peck, Chief of the Research and Development Branch, 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 study, and the author appreciates their expertise and efforts.

The author would also like to thank Eileen Vicedo, student assistant, for her diligent and intelligent work to match incoming data from police departments and courts to driver record data at DMV. Thanks are also extended to Debbie McKenzie, Associate Governmental Program Analyst, for preparing the tables and figures in the report, and for coordinating its production.

EXECUTIVE SUMMARY

Background

The automobile is the primary mode of transportation in the United States, and while it offers the benefits of convenience and quick mobility, crashes involving motor vehicles exact a high societal toll and present a major public health problem. In 1995, there were more than 6.6 million motor vehicle crashes in the United States, with about one-third resulting in injury (NHTSA, 1996).

One avenue that has been pursued to ameliorate the crash problem in the United States is to identify and better control high risk drivers, typically through sanctions applied by the courts or law enforcement. Sanctions traditionally prescribed for high-risk drivers include fines, license actions (restriction/suspension/revocation), jail, community service, and alcohol treatment (and more recently ignition interlock) for alcohol-involved problem drivers. Studies examining the effectiveness of these sanctions have consistently found that license actions (plus alcohol treatment for drivers convicted of driving-under-the-influence [DUI]) are some of the most effective countermeasures

available for reducing the subsequent crash and traffic conviction rate of high-risk drivers (DeYoung, 1997; Peck, 1991; Peck & Healey, 1995; Wells-Parker, Bangert-Drowns, McMillen & Williams, 1995).

While license actions, particularly suspension/revocation, are effective, it has been recognized for some time that they have significant limitations. Perhaps their major weakness is that they don't fully incapacitate the driver—as many as 75% continue to drive during their period of license suspension/revocation (Hagen, McConnell & Williams, 1980; van Oldenbeek & Coppin, 1965). And, while research has shown that suspended/revoked (S/R) drivers drive less often and more carefully during their period of license disqualification (Hagen et al., 1980; Ross & Gonzales, 1988), it has also been shown that they still pose an elevated traffic risk; DeYoung, Peck and Helander (1997) found that S/R drivers in California have 3.7 times the fatal crash rate as the average driver.

So, while license suspension/revocation is one of the most effective countermeasures currently available to attenuate the traffic risk posed by problem drivers, it is clear that there is considerable room for improvement. One relatively recent approach to strengthen license actions, and also to incapacitate S/R and unlicensed drivers, targets the vehicles driven by such drivers. Vehicle-based sanctions can take a number of forms, from marking or confiscating license plates of drivers convicted of driving-while-suspended (DWS)/driving-while-unlicensed (DWU), to actually seizing and impounding/immobilizing the vehicle.

Impoundment/forfeiture programs have been implemented in Manitoba, Canada (1989); Portland, Oregon (1989), and; Santa Rosa, California (1993). While anecdotal evidence suggests that Santa Rosa's program may be associated with traffic safety benefits, the lack of systematic and rigorous study of this program precludes any conclusions about its effectiveness. However, both Manitoba and Portland's vehicle impoundment programs have been formally evaluated. The study of Manitoba's program, while limited due to the lack of statistical or design controls, indicates that impoundment is associated with reductions in both DWS/DWU recidivism and traffic convictions overall (Beirness, Simpson & Mayhew, 1997). The quasi-experimental study of Portland's program did employ statistical controls and thus is more definitive (Crosby, 1995). This study showed that impoundment reduced the recidivism rate of drivers whose vehicles were seized to about half that of a similar group of drivers whose vehicles were not taken.

More recently, Ohio implemented an impoundment and immobilization law for DWS and multiple DUI offenders. Voas, Tippetts and Taylor evaluated the implementation of this law in two counties, one of which impounded vehicles (in press) and the other which towed vehicles to the homes of offenders and immobilized them by installing a "club" device on the steering wheel (1997). The impoundment and immobilization programs were found to be effective, both in preventing recidivism through incapacitation while the vehicle was impounded/immobilized, and in deterring people from reoffending once the vehicle was released.

Current Study

The California legislature passed two bills during the 1994 legislative session prescribing vehicle impoundment (Senate Bill (SB) 1758) and vehicle forfeiture (Assembly Bill (AB) 3148), effective January, 1995 (see Appendix A). SB 1758 authorizes peace officers to seize and impound for 30 days vehicles driven by S/R or unlicensed drivers, while AB 3148 goes a step further by providing for the forfeiture of vehicles driven by S/R and unlicensed drivers who are the registered owners of the vehicles and who have a prior conviction for DWS/DWU.

California's impoundment/forfeiture laws are the first to attempt such sanctions on a large scale; there are about one million drivers in the state who are suspended/revoked at any given time, and another estimated one million who are unlicensed. The few rigorous studies of vehicle-based sanctions that have been conducted to date examine these sanctions undertaken on a relatively limited scale. The current study evaluates California's large-scale attempt at vehicle impoundment, and is designed to provide useful information to policy makers so that informed decisions on traffic safety can be made. This study is part of a joint project funded by NHTSA, which is being undertaken by the California Department of Motor Vehicles (DMV) and the National Public Services Research Institute (NPSRI). The California DMV has primary responsibility for the current study, which evaluates how impounding vehicles affects the subsequent driving behavior of S/R and unlicensed drivers who experience this sanction, as well as a follow-up study, which will examine the effects of impoundment on <u>all</u> S/R and unlicensed drivers in California, regardless of whether their vehicles are impounded.

Research Methods

Because there is no centralized database containing information on vehicles that have been impounded, it was necessary to rely on police departments and courts to provide this information. Four jurisdictions (Riverside, San Diego, Stockton and Santa Barbara) that had record systems which would allow impoundment data to be linked to driver record data in the DMV database were selected for inclusion in the study.

This study compares the 1-year subsequent driving records of subjects whose vehicles were impounded with similar subjects (i.e., S/R and unlicensed drivers) who would have had their vehicles impounded, but who did not because their driving offense occurred in 1994, the year before the impoundment/forfeiture laws were implemented. Because it was not feasible to randomly assign subjects to impound or no-impound groups, statistical controls were used to attempt to control potential biases resulting from pre-existing differences between the groups. While statistical techniques, such as the analysis of covariance (ANCOVA) used in this study, help control bias, they do not ensure that all sources of bias have been controlled. Thus, the results of the analyses do not prove that differences in subsequent traffic convictions/crashes between impound and control group subjects are due to the effects of vehicle impoundment, as much as they portray the associations between the two.

Results and Discussion

Subsequent DWS/DWU convictions

The results from the ANCOVA analysis showed that drivers who had their vehicles impounded had a significantly lower average rate of subsequent DWS/DWU convictions than drivers whose vehicles were not impounded. Furthermore, the effects of impoundment were more pronounced for repeat offenders. That is, while impoundment was associated with lower rates of subsequent DWS/DWU convictions for both first and repeat offenders in the impound group, relative to their counterparts in the control group, this difference was significantly greater for repeat offenders than it was for first offenders. The results are presented in Figure 1, below.

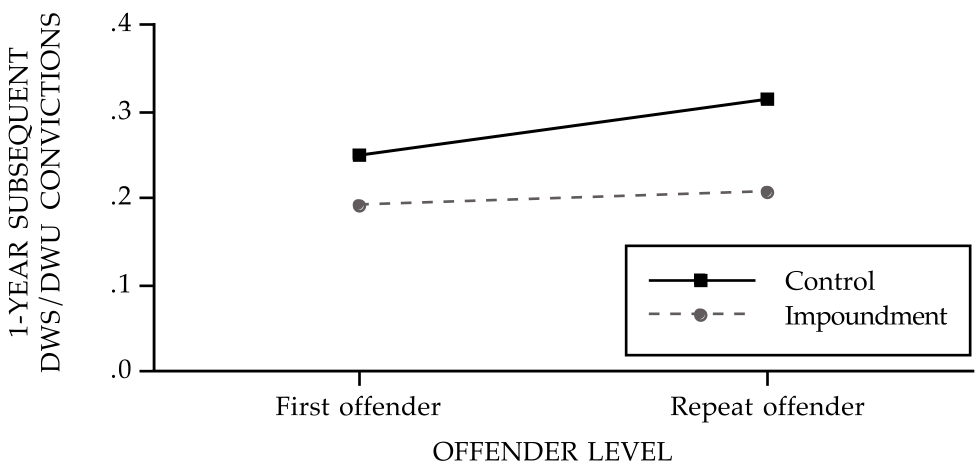

Figure 1. Adjusted subsequent DWS/DWU convictions for vehicle impoundment versus control groups, by number of prior DWS/DWU convictions.

Importantly, the effects of vehicle impoundment on subsequent DWS/DWU convictions are not only statistically significant (i.e., that is, they were unlikely to have occurred by chance), they are also large enough to be meaningful from a policy perspective. For first offenders in the impound group, the subsequent DWS/DWU conviction rate is 23.8% lower than the first offender control group rate, and for repeat offenders it is 34.2% lower. These findings are similar to those found for civil forfeiture in Portland Oregon (Crosby, 1995), and for vehicle immobilization (Voas et al., 1997) and impoundment (Voas et al., in press) in Ohio, and thus provide further evidence that such vehicle-based sanctions can lower recidivism rates of suspended/revoked and unlicensed drivers.

Subsequent total traffic convictions

The overall ANCOVA analysis demonstrated that drivers whose vehicles were impounded had a lower average rate of subsequent total traffic convictions than drivers who did not lose their vehicles, and that this difference was highly statistically significant. The analysis also showed that this lower rate of subsequent traffic convictions for impound versus control group drivers was greater for repeat offenders than for first offenders, although this finding approached but did not quite reach conventional levels of statistical significance. These results are portrayed in Figure 2 below.

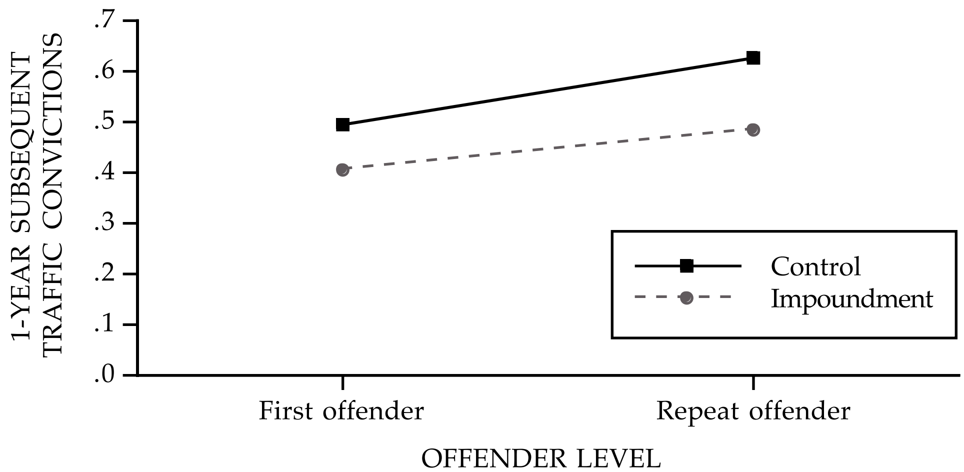

Figure 2. Adjusted subsequent traffic convictions for vehicle impoundment versus control groups, by number of prior DWS/DWU convictions.

The effects of vehicle impoundment on subsequent total traffic convictions are both statistically significant and large enough to be considered meaningful; the rate for first offenders in the impound group is 18.1% lower than for their counterparts in the control group, and it is 22.3% lower for repeat offenders in the impound group relative to repeat offenders in the control group. Thus, these findings show that vehicle impoundment not only keeps S/R and unlicensed drivers from driving when they shouldn't be (e.g., subsequent DWS/DWU convictions), it also appears to have salutary effects on their overall subsequent driving behavior.

Subsequent crashes

The results from the ANCOVA model evaluating the effects of vehicle impoundment on subsequent crashes revealed that drivers whose vehicles were impounded had significantly fewer crashes, on average, than drivers whose vehicles were not impounded. As with the previous analysis (which examined subsequent traffic convictions), the analysis of subsequent crashes showed that, while the difference between impound and control subjects on this measure was greater for repeat offenders than it was for first offenders, this result approached but did not quite reach statistical significance. Given that this trend of stronger effects of impoundment for repeat offenders was observed with all three outcome measures, it is likely that impoundment may, in fact, actually be more effective in curbing crashes for repeat offenders. The results of the analysis are shown in Figure 3 below.

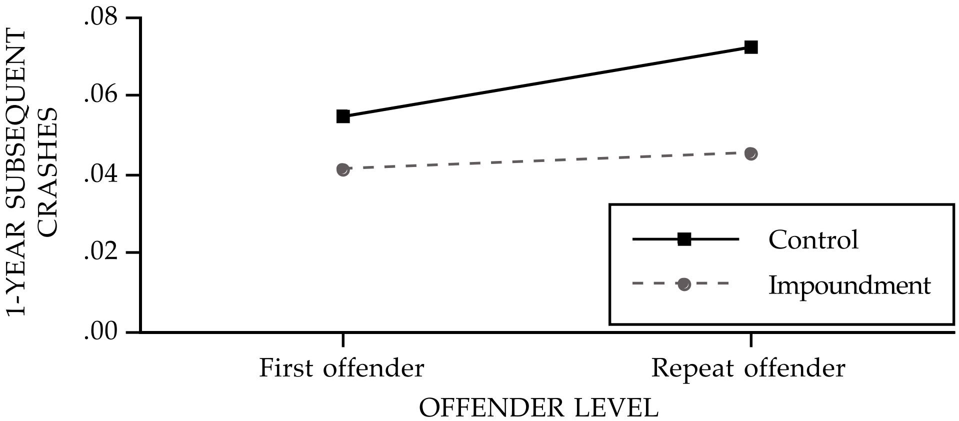

Figure 3. Adjusted subsequent crashes for vheicle impoundment versus control groups, by number of prior DWS/DWU convictions.

The findings from the analysis of subsequent crashes, like those from the other two outcome measures previously described, are of a sufficient magnitude to be both statistically significant and also to have important policy implications. First offenders who have their vehicles impounded have 24.7% fewer subsequent crashes than first offenders in the control group, while repeat offenders in the impound group have 37.6% fewer crashes than their counterparts in the control group. These findings, considered along with those evaluating the effects of vehicle impoundment on traffic convictions, strongly suggest that this countermeasure has a substantial effect in improving traffic safety.

Conclusion

The findings reported here provide strong support for impounding vehicles driven by suspended/revoked and unlicensed drivers. They add weight to a small but growing body of evidence that vehicle-based sanctions, whether they involve immobilizing vehicles for a period of time through such devices as a "club" on the vehicle's steering wheel, or whether they consist of simply seizing and impounding vehicles, are an effective means for controlling the risk posed by problem drivers. It is especially noteworthy that vehicle impoundment appears to be even more effective with repeat offenders, a group whose high-risk driving has traditionally been resistant to change.

Information obtained from a survey of law enforcement agencies in the state has shown that while vehicle impoundment has been widely implemented, forfeiture is simply not being used on any significant scale; thus, this study is really a study of vehicle impoundment, not vehicle forfeiture. While some traffic safety advocates may be concerned about the lack of use of vehicle forfeiture, in the end this lack of utilization of forfeiture may not matter much. Impounding vehicles is having a substantial positive effect in California, and if Crosby's (1995) findings in Oregon hold in California as well, going the extra step of forfeiting vehicles may not produce much added benefit.

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INTRODUCTION

Background

The automobile is the primary mode of transportation in the United States, and while it offers the benefits of convenience and quick mobility, crashes involving autos exact a high societal toll and present a major public health problem. The magnitude of the problem can be seen in data reported by the National Highway Traffic Safety Administration (NHTSA), which state that there were more than 6.6 million motor vehicle crashes in the United States in 1995, with approximately one-third resulting in an injury (NHTSA, 1996). This risk is especially high for younger people: the National Safety Council (NSC) reports that in 1993, motor vehicle crashes were the leading cause of death for persons between 1 and 33 years of age (NSC, 1996). In addition to the considerable human cost of crashes, there are enormous economic costs. The NSC (1996) estimates that in 1995, motor vehicle crashes cost society more than 170 billion dollars.

Over the past several decades, changes have been made in roadway design, vehicle safety and traffic legislation in an attempt to ameliorate the crash problem in the United States, and these have met with some success. A significant area of focus in traffic safety is on identifying and attempting to better control the risk posed by problem drivers, since a substantial body of evidence exists that some groups of drivers are overinvolved in crashes (Gebers & Peck, 1994; Hauer, Persaud, Smiley & Duncan, 1991; Levonian, 1963; NHTSA, 1992; Staplin & Knoebel, 1985; Stewart & Campbell, 1972). Thus, an important policy question asks what countermeasures might enhance traffic safey by deterring, incapacitating or rehabilitating problem drivers.

Sanctions traditionally prescribed for high-risk drivers include fines, license actions (restriction/suspension/revocation), jail and community service, with alcohol treatment (and more recently ignition interlock) meted out to alcohol-involved problem drivers. Studies examining the effectiveness of these sanctions have consistently found that license actions, plus alcohol treatment for drivers convicted of driving-under-the-influence (DUI), are some of the most effective countermeasures available for reducing the subsequent crash and traffic conviction rate of high-risk drivers (DeYoung, 1997; Peck, 1991; Peck & Healey, 1995; Wells-Parker, Bangert-Drowns, McMillen, & Williams, 1995).

While license actions, particularly suspension/revocation, are effective, it has been recognized for some time that they have significant limitations. One drawback is that they don't fully incapacitate the driver—perhaps as many as 75% continue to drive during their period of license suspension/revocation (Hagen, McConnell & Williams, 1980; van Oldenbeek & Coppin, 1965). While as Hagen et al. (1980) and Ross and Gonzales (1988) discovered, suspended/revoked (S/R) drivers drive less often and more carefully during the time their driving privilege is withdrawn, the traffic risk posed by this group is still elevated relative to the average driver. DeYoung, Peck and Helander, (1997) found that S/R drivers in California have 3.7 times the fatal crash rate as the average driver. The overinvolvement rate is even higher (4.9:1) for drivers who have no license at all (i.e., unlicensed).

So, while license suspension/revocation is one of the most effective countermeasures currently available to attenuate the traffic risk posed by problem drivers, there is considerable room for improvement (see DeYoung [1990] and Gebers, DeYoung and Peck [1997] for a more complete discussion of the deficiencies of the license suspension enforcement system and evaluations of projects to correct some of these problems). The Achilles heel of license actions is that they are difficult to enforce, due to the essentially invisible nature of the offense. The difficulty in detecting driving-while-suspended (DWS) or driving-while-unlicensed (DWU) offenses weakens the deterrent value of the laws, and is largely responsible for the poor compliance rate mentioned earlier.

However, if detection cannot easily be improved (driver license checkpoints are a possibility in this regard, but they have as yet not been attempted on a large scale), it may still be possible to increase deterrence by implementing tougher penalties for DWS/DWU offenses. One logical avenue to achieve this is to target the vehicles driven by S/R and other high-risk drivers. Such an approach, in addition to potentially enhancing deterrence, would also incapacitate the driver for some period of time, thus preventing further DWS/DWU offenses. A number of jurisdictions have implemented vehicle-based sanctions in the past decade, and while there is a paucity of research evaluating them, there is some evidence regarding their effectiveness.

Previous Uses of Vehicle-Based Sanctions

There are a variety of sanctions which can be directed at the vehicles driven by S/R and other high-risk drivers. Some, such as ignition interlock devices, are designed to prevent driving under certain circumstances, such as after drinking alcohol. Other sanctions attempt to more broadly prevent driving, such as marking or confiscating the vehicle license plates or registration tags so as to make the invisible DWS/DWU offense more obvious, or more directly, by simply seizing and impounding/immobilizing the vehicle. These broader-based vehicle sanctions are particularly relevant to the present study, and will be the focus of the discussion here.

One of the earliest implementations of a sanction targeting vehicles was Minnesota's license plate impoundment law, which beginning in 1988 required the courts to confiscate and destroy the license plates of vehicles registered to persons convicted of DUI three times within five years, or four-or-more times within ten years. During the first couple of years that the law was in effect it became clear that the courts were not implementing its provisions, so the law was amended to allow peace officers to enforce it administratively. An evaluation of the law (Rodgers, 1994) found that implementation increased markedly when it became administrative in nature, and that it had a significant impact in reducing recidivism among multiple DUI offenders.

At about the same time that Minnesota began impounding the license plates of repeat DUI offenders, the state of Washington initiated a program targeting the plates of vehicles driven by S/R drivers. In Washington, rather than confiscating the plate, officers seized the vehicle registration, issued a 60-day temporary registration, and placed a striped "zebra" tag over the annual sticker on the plate. A year-and-a-half later, Oregon followed suit, implementing a very similar license plate tag program.

Voas and Tippetts (1995) evaluated both the general and specific deterrence effects of the license plate tag programs in Washington and Oregon. General deterrence refers to how peoples' perception of the consequences for violating a law affects their behavior regarding it. If people believe that the consequences for violating a law are swift, certain and/or severe, they will more likely be deterred from offending; a great strength of general deterrence is that it acts on all people who are in a position to violate the law. In the present situation, if there was a general deterrent effect of the license plate tag laws, then the overall rate of crashes/convictions DWS would be lower for all S/R drivers once the law was passed. Voas and Tippetts found that the plate tag laws did have a significant general deterrent impact in Oregon, but not in Washington.

Specific deterrence occurs when the subsequent behavior of persons who are caught and punished for violating the law is changed by the experience. Analyses measuring recidivism are assessing "specific deterrent" effects. Voas and Tippetts found that drivers whose plates were tagged in Oregon had a lower rate of subsequent crashes than drivers whose plates were not tagged. As with the general deterrence analysis, there was no specific deterrence effect for Washington drivers.

A more forceful vehicle-based sanction is to actually physically prevent the vehicle from being driven, either by immobilizing it or by impounding it. In 1989, Manitoba Canada enacted both an administrative license suspension program, and a vehicle seizure and impoundment program for drivers apprehended violating their license suspension. Vehicles were subject to impoundment for 30 days, and the driver was liable for all costs related to the impoundment (reported to average about $250 in 1993). Beirness, Simpson and Mayhew (1997) evaluated the specific deterrent effects of Manitoba's program, and found that it was associated with significant declines in DWS recidivism and traffic convictions, although their findings should be regarded as suggestive only due to the lack of statistical or design controls.

Portland, Oregon also implemented a vehicle impoundment/forfeiture program in 1989, focusing on habitual traffic offenders and those caught DWS, but their program went a step beyond Manitoba's by providing for the civil forfeiture of an impounded vehicle. Crosby (1995) conducted an evaluation of Portland's forfeiture program and found that while the program was not economically self-supporting, it did reduce the recidivism rate of drivers whose vehicles were seized to about half that of drivers whose vehicles were not taken. Interestingly, having a vehicle forfeited did not affect the recidivism rate any more than if it was simply seized and held for a relatively brief period.

The police department in Santa Rosa, California designed and implemented a vehicle impoundment program for suspended drivers in late 1993. This program, dubbed the STOP program (Santa Rosa Traffic Offender Program), has become a model for more than a dozen such programs throughout California. While no formal evaluation of the program has been undertaken, statistical records maintained by the police department show a decline in crashes once the program was underway (Santa Rosa Police Department, 1995).

In 1993, Ohio also implemented an impoundment and immobilization program for DWS and multiple DUI offenders. Evaluation studies of different implementations of this law in two Ohio counties have recently been completed. The first study (Voas, Tippetts & Taylor, 1997) examined Franklin County's program, which towed vehicles to offenders' homes and immobilized them by installing a "club" device on the steering wheel, while the second (Voas, Tippetts & Taylor, in press) evaluated the program in Hamilton County, where vehicles were simply impounded. Both programs were shown to be effective in reducing the rates of subsequent DUI and DWS offenses, both through incapacitation while the vehicle was actually impounded/immobilized, and in deterring persons from reoffending once the vehicle was released.

Vehicle Impoundment in California

Based on the experiences of various jurisdictions using vehicle impoundment, such as the program undertaken by the Santa Rosa Police Department, plus recommendations for dealing with the DWS/DWU problem made by California DMV researchers and others, the California legislature passed two bills during the 1994 legislative session prescribing vehicle impoundment and forfeiture for DWS and DWU drivers. Senate Bill (SB) 1758 authorized peace officers, beginning January 1995, to seize and impound for 30 days vehicles driven by persons with a suspended/revoked driver license, or by those driving without ever having been issued a license. Vehicles can be impounded under this law regardless of whether the driver is the registered owner of the vehicle, and there are provisions allowing law enforcement agencies to charge a fee upon release of the vehicle to cover their costs of administering the impoundment program.

During the 1994 legislative session the state legislature also passed Assembly Bill (AB) 3148. This bill goes a step beyond its counterpart in the Senate by prescribing vehicle forfeiture for suspended and unlicensed drivers who are repeat offenders and who are driving vehicles registered in their name. Note that unlike Portland's forfeiture program, which is civil and administrative in nature, California's program proceeds through the courts. There is a provision in California's forfeiture law which allows family members or others who have a community property interest in the vehicle to obtain its release prior to forfeiture, if they sign a 'stipulated vehicle release agreement' promising not to allow a suspended/revoked or unlicensed driver to operate the vehicle, under penalty of forfeiting the vehicle.

The original legislation specified an implementation date of January 1995, although many jurisdictions did not begin impounding vehicles until several months into the year. Although subsequent 'clean-up legislation' was enacted, the major components of both laws remain intact.

Current Study

Vehicle impoundment/forfeiture programs are a relatively new phenomenon, and there have been few rigorous scientific studies conducted of their effectiveness. The evaluations that have been done of programs in Portland, Oregon and Franklin County, Ohio show that forfeiture and immobilization can be effective in reducing DWS/DUI recidivism. However, California's program is the first to attempt impoundment and forfeiture on a large scale; there are about one million drivers in the state who are suspended or revoked at any given time, and another estimated one million who are unlicensed. Thus, there are tens of thousands of vehicles that could potentially be seized and impounded/forfeited each year.

The current study is part of a joint project funded by the National Highway Traffic Safety Administration which is being undertaken by the California Department of Motor Vehicles (DMV) and the National Public Services Research Institute (NPSRI). There are a number of separate studies within the overall project, which will evaluate vehicle impoundment/forfeiture in California and impoundment/immobilization in Ohio. The California DMV has primary responsibility for the current study, which will evaluate the specific deterrent impact of the laws in California, as well as a follow-up study, which will examine the general deterrent effect of impoundment/forfeiture in the state. Thus, the focus of this report will be on whether vehicle impoundment affects the subsequent driving behavior (as measured by DWS convictions, total traffic convictions and crashes) of those drivers whose vehicles are impounded, as compared to a similar group of drivers whose vehicles are not seized.

METHOD

Subject Selection

There does not exist a centralized database containing information on vehicles that have been impounded. The driver record database maintained by the DMV contains data on traffic convictions and crashes for drivers, but does not have information on vehicles which have been impounded or forfeited because these actions are not reported to the department. Thus, it was necessary to rely on local law enforcement agencies for vehicle impoundment/forfeiture data.

As part of the overall joint project between DMV and NPSRI, the latter agency conducted a survey of the largest law enforcement agencies in California regarding their use of vehicle impoundment. One important finding from this survey was that while vehicles were being impounded on a significant scale, few vehicles were being forfeited. Based on this information, agencies which were actively impounding vehicles were contacted and queried to determine whether their record systems were adequate to allow impoundment data to be linked to driver record data in the DMV database. This process yielded four departments from which impoundment data were eventually collected. These agencies are: Riverside Police Department, San Diego Police Department, Santa Barbara Police Department, and Stockton Police Department. Two of these agencies are located in Southern California, one is on the Central Coast, and the fourth is in Northern California.

It was important to locate a comparison group that was comprised of individuals as similar as possible to people in the vehicle impoundment group. The comparison group chosen consists of individuals in each of the four geographical study areas who were convicted of the same offense (DWS or DWU) as drivers whose vehicles were impounded, but whose vehicles were not impounded because their offense occurred during the year prior to implementation of the vehicle impoundment and forfeiture laws. More specifically, the impoundment group consists of all drivers in the four study areas whose vehicles were impounded pursuant to California's impoundment/forfeiture laws between January 1, 1995 and December 31, 1995. The comparison, or control group, consists of all drivers who were convicted of a DWS/DWU offense which would have made them eligible for impoundment, but who violated between January 1, 1994 and December 31, 1994, prior to the effective date of the laws.

While the vehicle impoundment data were gathered from police departments, the comparison group data were obtained from either the courts having jurisdiction over the areas covered by the police departments (Stockton Municipal Court for the Stockton Police Department; Consolidated/Coordinated Superior & Municipal Court of Riverside County for the Riverside Police Department), or from the police departments themselves (San Diego Police Department; Santa Barbara Police Department).

Once the data were obtained from the police departments and courts, a process was initiated to attempt to locate each subject's record on the DMV's driver record database. In many cases, locating the record was straightforward, because the incoming data from the courts/police departments contained the subject's driver license number. However, in other cases it was necessary to attempt to locate the subject's record using other identifying information from the incoming data, such as name and date of birth. While it was usually possible to find subjects' driver records, some subjects were dropped from the study because no DMV record for them could be located.

Not all subjects whose driver record was located were included in the study. A small number of drivers were excluded because it was discovered that they had two driver licenses. This occurs because when the DMV receives abstracts of conviction from the courts and is unable to locate the convictee in the master record file, a new record is created in the database prefixed with an "X." In some cases the person already has a driver record, but it could not be located due to a variation in the spelling of the name, the birthdate, or other identifying information used for matching purposes. Because prior and subsequent driver record data may be incomplete and unreliable for persons with two different driver records, such subjects were omitted from the study.

A second group of drivers dropped from the study were subjects in the control group who had been convicted of California Vehicle Code Section (CVC) 12951—driving without a driver license in possession—but who actually were validly licensed. It was important to include drivers convicted of CVC 12951 in the control group, because some of them were S/R or unlicensed, and would have been eligible for impoundment had their offense occurred once the law was in effect (some of the drivers in the impoundment group were convicted of CVC 12951). However, it was necessary to review the driver records of all CVC 12951 convictees in the control group and exclude those who were validly licensed at the time of their offense, as they would not have been subject to the new laws.

A third group of excluded drivers were those whose residence ZIP Codes indicated that they lived outside of California. These drivers were omitted from the study because both prior and subsequent incidents on their driving records might be incomplete, and including them could lead to biased results. A final group of excluded drivers were those whose driving record indicated that they were deceased.

In examining the data, it was discovered that some drivers in both the impoundment and control groups were sampled more than once. In these cases, the offense which occurred first was selected for inclusion in the study, and the later offenses were counted as subsequent incidents. The rationale for this approach is that we are interested in knowing the effects of impoundment; the first incident represents a "treatment" in this regard, while subsequent incidents, regardless of whether they again result in impoundment, are indicative of the effects of the initial treatment. While it would have been interesting to separate these cases with multiple treatments from the others and then conduct analyses on them separately, there were too few such cases to maintain adequate statistical power, so this approach was not taken.

In two of the jurisdictions—Santa Barbara and Riverside—peace officers appear to be impounding the vehicles of all drivers who meet the criteria for impoundment. Because of this, the numbers of drivers in the impound and control groups are roughly equivalent. However, this wasn't the case for the Stockton and San Diego Police Departments. In these two areas, not all drivers eligible for impoundment actually lost their vehicles, due to limited resources within the department and other factors, and consequently the control groups contained many more subjects than the impoundment groups. The problem here is that, because there very well could be selective enforcement of vehicle impoundment, the impound and control groups may not be similar in composition, and this pre-existing difference between drivers in the groups could lead to biased results. While statistical controls were used to control for pre-existing group differences in the final analyses, it was decided to take an additional step at this stage by matching control group subjects to subjects in the impoundment group (for the nonequivalent Stockton and San Diego samples). This approach has the additional advantage of leading to more equivalent group sample sizes in these two regions.

The matching technique involved the use of propensity scores, and was based on the methods described by Rosenbaum and Rubin in a 1985 article published in *The American Statistician*. Propensity scores can be thought of as conditional probabilities; as used in this study, they are the probability that a driver was in the impoundment group versus the control group, given her scores on a number of predictor variables or covariates.

There were several steps involved in the development of propensity scores. First, using SAS statistical software, separate bivariate logistic regression analyses were conducted using 11 potential covariates to identify those that were significantly related to group membership (control versus impound). Based on these analyses, five variables were statistically significant at $p \leq .05$ (class of driver license, and the numbers of 3-year prior traffic convictions, crashes, DWS convictions, and crash/conviction incidents occurring while driving S/R). These five variables were entered into a backward elimination stepwise logistic regression model to determine which covariates were significantly related to group membership within the context of all of the covariates. The results showed that all of the covariates were significant. Finally, a logistic regression model was used with the five covariates to produce propensity scores for all control and impound group subjects in Stockton and San Diego.

A matching program was developed in SAS to find, for each impoundment group subject, a driver in the control group who had the closest propensity score. Based on this matching program, subjects in the control group who were matched to an impoundment group subject were retained in the control group, while non-matched subjects were dropped from the sample. This process led to the control group subjects being very similar to the impound group subjects on those dimensions measured by the covariates.

It was theorized that vehicle impoundment might affect those drivers who had prior DWS/DWU convictions differently than those drivers who had no such prior convictions. In order to investigate this, each subject's driving record was examined for prior DWS/DWU convictions, and subjects were categorized into first offender (i.e., no prior convictions) and repeat offender (i.e., one or more prior convictions) groups. The final sample sizes for the groups are shown in Table 1 below.

Table 1

Group Sample Sizes (N = 12,724)

	Control	Impound
<u>Riverside</u>		
First offenders	933	964
Repeat offenders	788	730
<u>San Diego</u>		
First offenders	404	330
Repeat offenders	481	553
<u>Santa Barbara</u>		
First offenders	964	844
Repeat offenders	367	455
<u>Stockton</u>		
First offenders	1,271	1,317
Repeat offenders	1,189	1,134

Data Collection

Data on whether drivers had their vehicles impounded were collected by means of the procedures described in the previous section. In addition to data on impound status, data were also gathered on demographics, prior driving history and subsequent driving incidents for all subjects in the study. These data are of two general types. The first is subject specific, such as a subject's age, sex, number of priors crashes, etc. Subject-specific data were gathered from DMV's driver record database.

The second type of data is aggregate level, which provides important information about the driving and social context where a driver lives. It is important to point out that the characteristics of the environment where drivers live do not necessarily tell us about the drivers themselves. For example, a driver may belong to a relatively high socioeconomic group, but reside in a diverse area where the preponderance of the populace are of a substantially lower socioeconomic class. To assume that an individual driver has the same characteristics of the larger context where he or she lives is to commit what is known as an "ecological fallacy."[1] The aggregate-level data provide important information about the driving context, but not necessarily about the driver. Aggregate-level data were grouped by ZIP Code, and were gathered from the 1990 U.S. Census. Some examples of this type of data are the median household income, average level of education, and average crash rate.

Prior driving history data and demographic information were used as covariates in the analyses. For the subject-specific variables, data were gathered for a 3 year period preceding subjects' sampled offense dates (i.e., violation date for control group subjects, impound date for subjects in the impoundment group). Aggregate-level data were generally based on the 1990 U.S. Census, except for the average crash/traffic conviction data, which were normalized over a 5 year period preceding the sampled offense.

Subsequent driving incidents, which were used as outcome measures to evaluate the efficacy of vehicle impoundment, included crashes, total traffic convictions and DWS convictions which occurred within 1 year following the date of the sampled offense/impoundment. In addition, for a 1-year subsequent crash/conviction to be counted, it must have been posted to DMV's driver record database within 18 months of the date of the offense. It was necessary to use this 18-month posting criteria for two reasons. The first is that it is important to allow sufficient time for an abstract of conviction to be transmitted and updated to DMV's database. The timeframes used in this study allowed a minimum of 6 months for this to occur. The second, and perhaps more important reason for the 18-month timeframe, is that because the control group subjects had violation dates the year before subjects in the impoundment group (1994 versus 1995), on average they would have had an extra year for violations to be updated on their DMV records. This could seriously bias the study results, since it could be expected that control group subjects would have more subsequent violations than those in the impoundment group simply because more time had elapsed for violations to be posted to their DMV records. By specifying an 18-month 'window' for both groups, this source of bias was eliminated.

Evaluation Design and Statistical Analyses

Design

The research design used to answer questions about the effects of vehicle impoundment on subsequent driving involves comparing a sample of subjects who had their vehicles impounded with another group of subjects who did not have their vehicles impounded. Ideally, subjects would be randomly assigned to either an impound or control group, and then the subsequent driving records of the groups would be compared to provide a measure of the effectiveness of impoundment. Random assignment would assure that, on average, the groups were equivalent to start with.

[1] It should be noted that using covariates that are a mixture of subject-level and aggregate-level variables can be problematic. The issue is that these two types of variables represent different sampling units, and as such would have different sampling errors. Thus, the *p* values for the covariates reported in the Results section should be regarded as approximate. However, this limitation does not affect the adjusted means produced by the ANCOVA models.

Unfortunately, it was not feasible to use random assignment in this study. The quasi-experimental nature of the study means that it is possible that the control and impound groups are different to start with, and may have different expected rates of subsequent crashes/convictions totally apart from whether or not their vehicles were impounded. In an effort to control potential biases resulting from pre-existing differences among the groups, statistical controls were used in the study. The first level of such controls, matching control group subjects to impound subjects in San Diego and Stockton based on propensity scores, was described in the previous section. Additionally, variables were selected and used as covariates in the statistical analyses.

Covariates are simply variables upon which it is expected that the groups may differ, and which are also related to the outcome measure(s) of interest. For example, if subjects in one of the groups are, on average, younger than their counterparts in the other group, then the former could be expected to have more crashes and convictions <u>irrespective of the treatments each group received</u> simply because research has shown that the crash/conviction rate is higher for younger drivers. By using covariates—age in this example—in the analysis, the linear relationship between the covariates and the outcome measure is accounted for, thus removing their effects and equating the groups on these measures.

It should be noted that while the use of propensity score matching and analyses employing covariates help control pre-existing biases, they do 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 the impact of vehicle impoundment. In addition, since the groups were not measured at the same time, it is possible that historical events other than the enactment of vehicle impoundment may have occurred to differentially affect the subsequent crash/conviction rates of the groups. Although there are no readily identifiable historical events of this kind, there remains the possibility that they do exist.

<u>Analysis</u>

The statistical technique used in this study was a factorial analysis of covariance (ANCOVA). However, before developing the ANCOVA models, a number of preliminary analyses were conducted to screen the data, select covariates to use in the models, and to check the assumptions underlying ANCOVA.

The SPSS statistical software program was used to conduct descriptive analyses of all variables used in the final analyses. The data were screened to check for missing values, out-of-range values and distributional patterns (e.g., skewness and kurtosis). At this stage, it was found that there were a number of cases with missing values for the covariate 'sex.' For these cases, sex was recoded so that the missing value was replaced with the mean value for the group (group x priors) that the subject belonged to. In addition, the BMDP statistical software package was used to compute bivariate correlations between the variables, and to produce a table of squared multiple correlations. The later was useful in looking for variables that might be multicollinear to such a degree that they would pose either logical problems (e.g., a variable that is so highly correlated with other variables that it is redundant), or problems in computing ANCOVA models.

There were a large pool of covariates (47) available to use in the ANCOVA models. To reduce this large number to a more manageable level, and to choose only those variables that were significantly related to the criterion measures, a two-step process was followed. In the first step, SAS GLM was used to conduct a series of bivariate regression analyses, regressing each of the three criterion variables (crashes, convictions, DWS/DWU convictions) separately against each of the 47 potential covariates. This step produced, for each criterion measure, a reduced set of potential covariates which, considered singly, were significantly related to it. For the total convictions and DWS/DWU convictions outcome variables, an alpha level of $p \leq .01$ was used to select covariates. Based on this criteria, a pool of 27 covariates was selected for each of these two criterion measures (however, the set for each criterion was slightly different).

The situation was somewhat different for the criterion measure of crashes. For this variable, the bivariate regression identified relatively few significant covariates, probably due to the large random component inherent in vehicle crashes. In order to increase the number of covariates to include at this first step for crashes, an alpha level of $p \leq .10$ was used, which yielded 13 potential covariates.

Once this initial reduced set of covariates was identified for each criterion variable, the BMDP 2R program was used to run several backward elimination stepwise regression analyses. For each analysis, one of the criterion variables was regressed against its reduced set of covariates. The covariates were all entered initially, then stepped out of the equation if they did not meet a set level of significance. The idea here is to identify a final set of covariates for each outcome measure that is arrived at by testing each covariate's relationship to that measure within the context of all of the covariates. In effect, redundant variables whose variance is accounted for by other remaining variables are excluded from the final covariate set. For total convictions and DWS/DWU convictions, variables were considered significant at $p \leq .01$. This resulted in a final set of 10 covariates for both measures (the final set was different for each measure). Because of the aforementioned problem with the crash measure, a different level of significance was used to select covariates. In this case, covariates were left in the model until the point that stepping one out led to an increase in the mean square error of the model. Using this criteria resulted in a final covariate set of 8 variables for crashes.

Once the final set of covariates was selected, analyses were conducted to check two major assumptions which underlie the analysis of covariance. The first assumption —homogeneity of variance—stipulates that the variance of the criterion variables should be the same for each group in the analysis. Many of the tests available in statistical packages to check this assumption are problematic because they are also affected when variables are not normally distributed, which is the case with the data in this study. There is one test in SAS that is less sensitive to non-normality, because it is based on deviations from group medians rather than group means. This test, known as the Brown and Forsythe's Test, was used to test the homogeneity of variance assumption for each of the three criterion measures.

The second assumption tested was the homogeneity of regression, or equal slopes assumption. This assumption specifies that, for each covariate used in the analysis, the slope resulting from regressing the criterion on the covariate is the same for each level of the independent variable(s) (Tabachnick & Fidell, 1989). For the analyses conducted here, this means that for each criterion measure, when the measure is regressed against each of the covariates, the resulting slope is the same in each of the 4 groups (i.e., impound first offenders, impound repeat offenders, control first offenders, control repeat offenders). The homogeneity of regression assumption was tested both for the covariates as a group and individually using the SPSS MANOVA and SAS GLM programs.

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 group (Searle, 1987), or using the Johnson-Neyman technique to evaluate group differences at different levels of the offending covariate (Huitema, 1980).

The approach taken here was to compute separate covariate slope adjustments for each treatment group. This was done by first creating an interaction involving the group x (violating) covariate, and then performing an ANCOVA with a sequential sum of squares decomposition where the slope violation interaction effect was ordered <u>after</u> the main effect or interaction of interest. That is, priority was given to the main effect or interaction being evaluated (note that this is different than the way SPSS MANOVA and SAS GLM handle separate slope adjustments, where priority is given to the slope violation interaction). This resulted in the slope interactions affecting the overall mean square error of the model, and thus the *F*- ratio of the effect of interest, but not the mean square of 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. Separate models were developed for each of the three criterion measures using the specific covariate set that was developed for each. Each model had two, 2-level factors: group (impound, control) and priors (first offender, repeat offender).

The primary effect of interest in these analyses is group membership; that is, did vehicle impoundment lead to reductions in subsequent crashes, convictions and DWS/DWU convictions? Also of considerable interest is the interaction between group and priors. This interaction tests whether the effect of impoundment is different for first versus repeat offenders. Main effects and interactions were tested at an alpha level of $p \leq .05$, so that an effect would be considered significant if its probability of occurring by chance was less than 5 in 100. In the case of a significant interaction, the simple effects of group were tested at each of the two levels of priors to determine the specific ways vehicle impoundment differed for subjects of different offender levels (e.g., was it more or less effective with multiple offenders).

Because the sample sizes in the groups were not equal, the factorial design is non-orthogonal. The issue with non-orthogonal designs is that hypotheses about main effects and interactions are not independent, and that sums of squares are not additive; the result is that there is ambiguity regarding assignment of overlapping sums of squares to sources (Tabachnick & Fidell, 1989). The approach used in all ANCOVA analyses was to adjust the main effect of interest for the covariates, and the two-way interaction of interest for all covariates and main effects. This was done by ordering the effects and using type I sum of squares in SAS.

RESULTS

Subsequent DWS/DWU Convictions

Analysis of ANCOVA assumptions

To check the homogeneity of variance assumption, the Brown and Forsythe's Test in the SAS GLM program was used to determine whether the variance of subsequent DWS/DWU convictions differed among the 4 groups being evaluated (i.e., impound first offenders, impound repeat offenders, control first offenders and control repeat offenders). The results showed that the variance was significantly different among the groups at $p \leq .0001$. This violation was probably due in part to the large sample size (> 12,000), which would provide sufficient statistical power to detect very small differences among the groups, as well as the non-normal distribution of variables, which the test partially controls for.

The descriptive analyses which were conducted as part of the data screening showed that the differences among group variances ranged from a low of 1.4:1 to a high of 3.4:1. Monte Carlo demonstrations have shown that there is a complex relationship between patterns formed by different sample sizes combined with unequal group variances, and tests of main effects and interactions, which can lead to either overly liberal or conservative statistical tests (Milligan, Wong, & Thompson, 1987). However, these studies typically examine variance differences larger than those found here, with sample sizes that are much smaller. Keppel (1991) suggests that researchers consider alternatives, such as more stringent alpha levels, once the maximum differences between group variances exceed 3:1. Because the differences in group variances found here were borderline in size, it was decided to continue with the ANCOVA, being aware that the p values found are approximations.

The assumption of homogeneity of regression, or equal slopes, was checked for the covariates as a group, and then for each separately, using SPSS MANOVA and SAS GLM. This test is performed by creating interaction terms between the covariates and factors, and then checking whether they are significant. Because there were a large number of covariate interaction terms (30), it is possible that significant interactions indicating slope violations might really simply be type I errors.[2] The group covariate tests showed that at least some covariates violated when crossed with group, and also when crossed with priors, but not when crossed with group x priors. The individual covariate tests revealed that both sex and ZIP Code % receiving public assistance had significant interactions with group, that ZIP Code average crash rate interacted with priors, and that the number of 3-year prior crashes DWS interacted with group x priors. It is possible that the three-way interaction is a type I error, since the group covariate test for group x priors was not significant (p = .517).

Preliminary ANCOVA models were developed to assess the significance of the slope interactions relative to the main effect or interaction of interest (the relative significance of the effects were determined based on the sizes of their mean square values). These models showed that: the main effect of group was almost 10 times the size of the covariate interaction of sex x group; group was approximately 22 times the size of ZIP Code % receiving public assistance x group, and the latter was not statistically significant; priors was of similar magnitude as ZIP Code average crash rate x priors, and; group x priors was about three times the size of 3-year prior crashes DWS x group x priors, and that this three-way interaction was not statistically significant. Based on these preliminary analyses, the significant two-way covariate interactions (i.e., sex x group and ZIP Code average crash rate x priors) were left in the final ANCOVA model to compute separate regression slopes for the groups, although priority in the assignment of shared variance was given to the effect of interest, as explained in the Methods section. The non-significant two-way and three-way interactions were omitted from the final model.

Group differences on covariates

In order to determine whether there were pre-existing differences among the groups on the dimensions measured by the covariates, SPSS was used to perform chi-square analysis for categorical-level variables, and one-way analysis of variance (ANOVA) for those measured on an interval level. Because the main analysis examines both the main effect of group, and the interaction of group x priors, these preliminary analyses also focus on the levels of both group and group x priors when checking for different average covariate values. This was important to do because significant group x priors interactions with covariates could help explain any significant interactions between these factors on subsequent DWS/DWU convictions. The results of these preliminary analyses are presented in Table 2.

[2] The possibility that some of the slope violations found here are really type I errors is given more weight based on the research of Alexander and DeShon (1994). These researchers found that heterogeneity of error variance in the regression test for slopes (which is likely if the equal variance ANCOVA assumption is violated, as it was here) leads to inflated power and excessive type I error rates when the group with the smallest sample size has the greatest variance, which was the pattern found for the groups on DWS/DWU convictions.

Table 2

Group Differences on Covariates (Subsequent DWS/DWU Convictions Analysis)

Variable	Group: Control, First offenders		Group: Control, Repeat offenders		Group: Impound, First offenders		Group: Impound, Repeat offenders		Significance tests & p values
Sex									Grp: $x^2 = 15.49$, $p \leq .001$
% male	73.5%		86.0%		77.6%		86.7%		Grp x Priors: $x^2 = 234.21$, $p \leq .001$
% female	26.5%		14.0%		22.4%		13.3%		
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
3-yr prior convictions	2.12	2.04	4.46	3.08	2.00	2.04	4.28	3.19	Grp: $F = 9.57$, $p = .002$; Grp x Priors: $F = .362$, $p = .547$
Age	27.34	9.27	28.01	7.80	28.19	9.73	30.03	8.32	Grp: $F = 76.35$, $p \leq .001$; Grp x Priors: $F = 14.12$, $p \leq .001$
3-yr prior DWS conv	.232	.497	1.36	1.47	.243	.585	1.35	1.48	Grp: $F = .015$, $p = .901$; Grp x Priors: $F = .211$, $p = .646$
3-yr prior crashes DWS	.058	.256	.175	.435	.045	.212	.175	.447	Grp: $F = 1.49$, $p = .222$; Grp x Priors: $F = 1.30$, $p = .255$
3-yr prior conv DWS	.588	.987	2.45	2.19	.561	1.09	2.47	2.34	Grp: $F = .024$, $p = .876$; Grp x Priors: $F = .648$, $p = .421$
ZIP Code crash avg	.236	.043	.242	.039	.238	.043	.237	.041	Grp: $F = 2.30$, $p = .130$; Grp x Priors: $F = 21.89$, $p \leq .001$
ZIP Code % some high school education	.152	.054	.160	.057	.157	.055	.158	.057	Grp: $F = 4.91$, $p = .027$; Grp x Priors: $F = 12.83$, $p \leq .001$
ZIP Code % welfare	.060	.040	.066	.041	.064	.044	.066	.042	Grp: $F = 5.60$, $p = .018$; Grp x Priors: $F = 7.84$, $p = .005$
ZIP Code median house price	172,304	95,829	148,410	80,366	165,208	92,486	153,883	87,297	Grp: $F = 1.03$, $p = .309$; Grp x Priors: $F = 16.13$, $p \leq .001$

It can be seen from the last column in Table 2 that there were statistically significant differences between the groups on about half of the covariate tests. Arguably, the differences between the groups on subject-specific covariates are most important, because they reflect the actual driving history and demographic characteristics of the subjects themselves, while group differences on the aggregate-level covariates are important in reflecting the driving environment and general social milieu of the areas where drivers live. There were significant group differences on three of the subject-specific covariates—sex, age and the number of 3-year prior traffic convictions. The impound group subjects were more predominately male than their counterparts in the control group, and this gender difference is especially pronounced for first offenders. In addition, subjects in the impound group were older and had fewer 3-year prior traffic convictions than control group subjects. There were no differences between the groups on 3-year prior DWS convictions, 3-year prior crashes DWS, or 3-year prior convictions DWS.

The groups were also different on all four ZIP Code variables. In all of the analyses, the interactions were larger than the main effects, meaning that differences between impound and control subjects were modified by whether or not subjects had prior DWS/DWU convictions. Subjects in the impound group with no prior convictions, relative to those in the control group, live in areas with: a higher average crash rate; a higher proportion of the population having only some high school educational background; a higher proportion receiving public assistance, and; a lower median house price. These relative group differences are reversed for repeat offenders, with the exception of the proportion receiving public assistance, where the groups have approximately equal rates.

These results show that the groups are somewhat different from one another. On the subject-specific covariates, the differences on two of the three variables—age and the number of prior convictions—suggest that impound subjects are lower risk than subjects in the control group, and could be expected to have better subsequent driving records, apart from any effects of vehicle impoundment. On the third covariate, sex, those subjects in the impoundment group, especially first offenders, appear to be higher risk. On the aggregate-level covariates, it appears that impound subjects with no prior DWS/DWU convictions reside in lower socioeconomic status areas and perhaps face a riskier driving environment than control subjects, while this situation is reversed for subjects with prior convictions.

Recidivism analysis

The SAS GLM program was used to fit an ANCOVA model to the data. The covariates are entered first, followed by the main effects of priors and group, the interaction of group x priors, and finally the 2 two-way slope violation interactions. The results of this analysis are shown in Table 3.

Table 3

Summary Table of Two Factor ANCOVA: Subsequent DWS/DWU Convictions

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates*	12	26.76	77.56	.0001
Priors	1	3.52	10.19	.0014
Group	1	21.31	61.74	.0001
Group x Priors	1	2.31	6.68	.0098
Error	12,694	.35		

*Includes the covariate x group/priors interaction for each of the two covariates that violated the equal slopes ANCOVA assumption.

All effects in the model are highly significant. In fact, the significance levels are of such a magnitude that they allay concern that the unequal group variances might affect interpretation of the results. It is not very surprising that the main effect of priors is significant (e.g., subjects with prior DWS/DWU convictions have a higher subsequent rate of convictions for these offenses than those with no such priors). Of greater interest, however, is the significant main effect of group, which shows that drivers who had their vehicles impounded had a significantly different rate of subsequent DWS/DWU convictions than those who did not lose their vehicles. Figure 1 illustrates the nature of these differences.

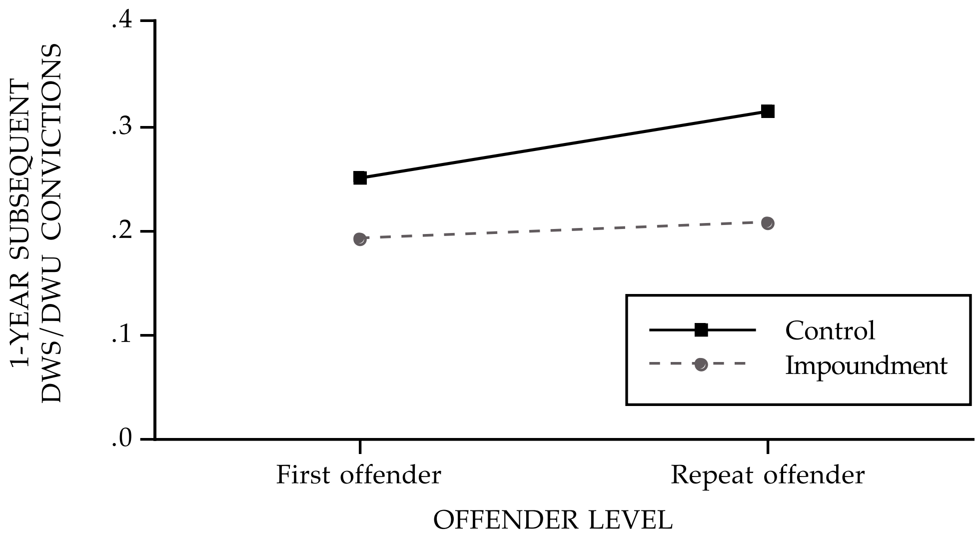

Figure 1. Adjusted subsequent DWS/DWU convictions for vehicle impoundment versus control groups, by number of prior DWS/DWU convictions.

Two things are noteworthy about Figure 1. The first is that the rate of subsequent DWS/DWU convictions is higher for subjects in the control group than for subjects in the impound group at both offender levels, which is a pictorial representation of the significant main effect of group presented in Table 3. In addition, it can also be seen that the difference in the subsequent DWS/DWU conviction rate between the groups appears larger for repeat offenders than for first offenders; this illustrates the significant group x priors interaction revealed in Table 3.

While the main effect of group is about 5 times the size of the group x priors interaction, it is still important to consider how the effects of impoundment on subsequent DWS/DWU convictions is moderated by prior convictions. A subsequent SAS GLM analysis was performed, this time to examine the simple effects of group at each level of priors. The results of this analysis are shown in Table 4.

Table 4

Simple Effects of Group at Each Level of Priors: Subsequent DWS/DWU Convictions

Offender level	Adjusted group means and *F* tests				Significance level
	Control	Impound	Mean square	*F* value	
First offenders	.25092	.19115	6.18	17.90	.0001
Repeat offenders	.31493	.20724	16.13	46.74	.0001

The results shown in Table 4 indicate that the impound group has a lower rate of subsequent DWS/DWU convictions at both levels of priors. It is interesting to note that the effect of vehicle impoundment is more pronounced for repeat offenders. In fact, the adjusted means for first and repeat offenders in the impound group is about the same; this is clearly not the situation for subjects in the control group. This difference in simple effects explains the significant group x priors interaction, but it can be seen that the strong main effect of group (along with this ordinal interaction) provides a clear interpretation—vehicle impoundment results in lower rates of subsequent DWS/DWU convictions regardless of offenders status.

While the effects in the model were statistically significant, it is important to consider whether they are large enough to be meaningful. The effect sizes for the adjusted means are substantial. For first offenders in the impound group, the subsequent DWS/DWU conviction rate is 23.82% lower than the first offender control group rate, and for repeat offenders it is 34.19% lower. The effect sizes were generally larger before the means were adjusted for the covariates—30.39% and 33.0% for first and repeat offenders respectively—and it can be seen that the adjustment favored first offenders in the control group. Thus, the effects of vehicle impoundment on subsequent DWS/DWU conviction rates are both statistically significant and substantively meaningful.

Subsequent Total Traffic Convictions

<u>Analysis of ANCOVA assumptions</u>

The Brown and Forsythe's Test in SAS GLM was used to check whether the four groups had equal variances on the criterion measure, 1-year subsequent total traffic convictions. The results of this test showed that the group variances on subsequent convictions were significantly different ($p \leq .0001$), thus violating the homogeneity of variance assumption. Prior descriptive analyses showed that the greatest difference in group variances was 2.5:1. Given the large sample sizes (which would provide sufficient statistical power for the Brown and Forsythe's Test to find even small group differences in variances significant) and a relatively moderate difference between the largest and smallest group variances, it was decided to continue with the ANCOVA analysis because any threat to the robustness of the model does not appear to be large. The rationale for this is provided in more detail in the results section describing DWS/DWU convictions.

SPSS MANOVA and SAS GLM were used to test the homogeneity of regression assumption for the covariates, both as a group and also each individually. The group covariate tests showed that at least some covariates violated the slopes assumption by interacting significantly with priors; however there were no significant interactions with either group (p = .117) or group x priors (p = .402). The test for the covariates singly revealed that the covariates responsible for the significant interaction with priors were the ZIP Code average crash rate and age. The individual covariate tests also showed that one covariate—3-year prior convictions DWS—interacted with group x priors. Note that there is a possibility that this is simply a type I error, given the large number of tests conducted and the non-significant group covariate test for group x priors.

Preliminary ANCOVA models were developed to compare the size of the mean squares of the interactions involving covariates which violated the slopes assumption with those associated with the corresponding main effect or interaction of interest. These models showed that: ZIP Code average crash rate x priors was smaller, but on par with the main effect of priors; priors was about 4 times as large as age x priors, but the latter was still significant, and; group x priors was about 7 times the size of 3-year prior convictions DWS x group x priors, and this three-way interaction was not statistically significant. It is quite possible, as mentioned above, that the three-way interaction was simply a type I error rather than a true violation of the equal slopes assumption. In any case, the three-way interaction was omitted from the final ANCOVA model, but the significant two-way interactions were included so that separate regression slopes could be fit for the groups.

While analyzing these preliminary models, it was discovered that the main effects for two covariates, ZIP Code % starting work at 12 AM and ZIP Code having only an elementary school education, were no longer significant when considered along with all of the effects included in the final model. Since the former was the least significant of the two, it was backed out of the model first. At this point, ZIP Code % having only an elementary school education was still non-significant, so it was also removed from the final model.

Group differences on covariates

In order to check for pre-existing differences among the four groups on dimensions measured by the covariates, SPSS was used to perform chi-square analysis (categorical-level variables) and one-way ANOVA analysis (interval-level covariates). As explained in the section covering subsequent DWS/DWU convictions, pre-existing group differences on the covariates were examined for both group and group x priors. The results of these analyses of group differences on covariates are shown in Table 5.

Table 5

Group Differences on Covariates (Subsequent Total Traffic Convictions Analysis)

	Group								Significance tests & p values
	Control				Impound				
Variable	First offenders		Repeat offenders		First offenders		Repeat offenders		
Sex % male % female	73.5% 26.5%		86.0% 14.0%		77.6% 22.4%		86.7% 13.3%		Grp: $x^2 = 15.49$ $p \le .001$ Grp x Priors: $x^2 = 234.21$ $p \le .001$
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
3-yr prior convictions	2.12	2.04	4.46	3.08	2.00	2.04	4.28	3.19	Grp: $F = 9.57$, $p = .002$ Grp x Priors: $F = .362$ $p = .547$
Age	27.34	9.27	28.01	7.80	28.19	9.73	30.03	8.32	Grp: $F = 76.35$, $p \le .001$ Grp x Priors: $F = 14.12$ $p \le .001$
3-yr prior DUI conv	.120	.375	.270	.592	.128	.395	.235	.550	Grp: $F = 1.67$, $p = .197$ Grp x Priors: $F = 6.03$ $p = .014$
3-yr prior crashes DWS	.058	.256	.175	.435	.045	.212	.175	.447	Grp: $F = 1.49$,$p = .222$ Grp x Priors: $F = 1.30$ $p = .255$
3-yr prior conv DWS	.588	.987	2.45	2.19	.561	1.09	2.47	2.34	Grp: $F = .024$, $p = .876$ Grp x Priors: $F = .648$ $p = .421$
ZIP Code crash avg	.236	.043	.242	.039	.238	.043	.237	.041	Grp: $F = 2.30$, $p = .130$ Grp x Priors: $F = 21.89$ $p \le .001$
ZIP Code % begin work at 12 AM	.127	.060	.139	.056	.131	.058	.136	.057	Grp: $F = .180$, $p = .671$ Grp x Priors: $F = 10.39$ $p \le .001$
ZIP Code % elementary school education	.151	.101	.156	.106	.158	.104	.159	.108	Grp: $F = 9.00$, $p = .003$ Grp x Priors: $F = 1.32$ $p = .250$
ZIP Code % renting	.490	.161	.469	.155	.498	.161	.481	.158	Grp: $F = 11.27$ $p \le .001$ Grp x Priors: $F = .484$ $p = .487$

A number of the covariates used to examine the effects of vehicle impoundment on subsequent traffic convictions are the same ones used in the previous analysis of subsequent DWS/DWU convictions; thus, some of the pre-existing group differences have already been discussed. On the subject-specific covariates, drivers in the impoundment group are significantly older, more predominately male (especially first offenders), and have fewer 3-year prior traffic convictions than their counterparts in the control group. In addition, first offender drivers in the impoundment group have significantly more 3-year prior DUI convictions than first offenders in the control group, but this group difference is reversed for repeat offenders.

There are also pre-existing differences among the four groups on all four aggregate-level covariates. Subjects in the impoundment group are, on average, more likely than control subjects to live in areas where a greater proportion of the population has only an elementary school education and where a higher percentage rent their residences.

Also, first offenders in the impound group live in areas where the average crash rate is higher and more people begin work at 12 a.m. than first offender control subjects, while just the opposite is true for multiple offenders.

In sum, there are pre-existing differences among the groups. Differences on two of the subject-specific covariates—age and 3-year prior convictions—suggest that impound subjects are lower risk than subjects in the control group, while differences on a third variable, sex, indicate that they are higher risk. On the fourth subject-specific covariate, 3-year prior DUI convictions, first offender impound subjects appear riskier but repeat impound subjects seem less risky than their respective peers in the control group. The mean values on two of the aggregate-level covariates suggest that impound subjects reside in a lower socioeconomic status area than control subjects. On one of the remaining two covariates (average crash rate) first offenders in the impound group appear to face a riskier driving environment than first offenders in the control group, while this situation is reversed for repeat offenders. These pre-existing group differences are complex, and the adjustments undertaken within the final ANCOVA analysis will depend not only on these patterns of differences, but also on the strength of the relationship between each covariate and subsequent traffic convictions.

<u>Recidivism analysis</u>

The final ANCOVA model was analyzed using the GLM program in SAS. As with all of the final models, covariates were given priority in the analysis, followed by main effects, two-way interactions and three-way interactions (if any were specified). The results of the analysis evaluating the effects of vehicle impoundment on subsequent traffic convictions are presented in Table 6.

Table 6

Summary Table of Two Factor ANCOVA: Subsequent Traffic Convictions

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates*	10	89.90	110.85	.0001
Priors	1	23.09	28.47	.0001
Group	1	41.67	51.38	.0001
Group x Priors	1	2.73	3.37	.0664
Error	12,696	.81		

* Includes the covariate x priors interaction for each of the two covariates that violated the equal slopes ANCOVA assumption.

It can be seen from Table 6 that while the main effect of group is highly significant, the interaction of group x priors approaches but does not quite reach conventional levels ($p = .05$) of statistical significance. This means that there are significant differences in the rate of subsequent traffic convictions between subjects whose vehicles were impounded and those who didn't lose their vehicles, and that this difference holds regardless of whether or not subjects have prior DWS/DWU convictions. These effects can be seen visually in Figure 2.

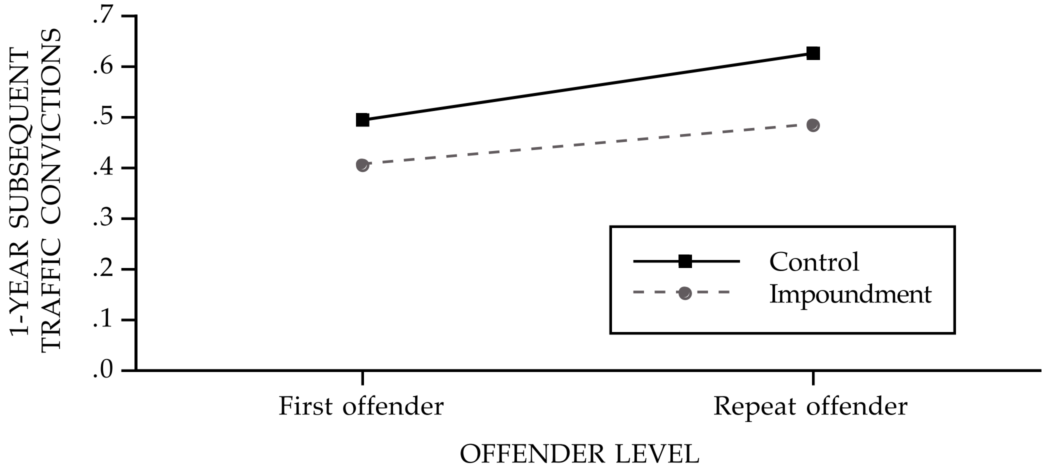

Figure 2. Adjusted subsequent traffic convictions for vehicle impoundment versus control groups, by number of prior DWS/DWU convictions.

The significant main effect of group can be seen in Figure 2; subjects in the impoundment group have fewer subsequent traffic convictions than control group subjects at both levels of prior DWS/DWU convictions (offender level). It can also be seen that the difference between the groups is larger for repeat offenders than it is for first offenders, although as described previously this difference approached but did not reach statistical significance. Thus, there is suggestive but not definitive evidence from this analysis that vehicle impoundment might be even more effective with repeat offenders.

In order to ascertain whether the difference in conviction rates for impound versus control subjects was large enough to be considered meaningful (i.e., is the difference large enough to matter) as well as significant from a statistical standpoint, the effect sizes for group differences in adjusted means were computed and are presented in Table 7.

Table 7

Adjusted Group Means and Effect Sizes: Subsequent Traffic Convictions

Offender level	Group means		Effect size
	Control	Impound	
First	.49498	.40557	18.06%
Repeat	.62324	.48404	22.34%

The data in Table 7 show that the effects of vehicle impoundment on the rate of subsequent traffic convictions are of a substantial magnitude, with the conviction rate for impound subjects ranging from about 18% to 22% lower than the corresponding rate for control subjects. The differences were even larger before the means were adjusted for the covariates; for the unadjusted means, the effect sizes were 21.98% and 25.08% for first and repeat offenders respectively (the ANCOVA adjustment brought the impound and control group means closer together, thus making the group differences less significant). Thus, the results reveal that the effects of impounding vehicles are not only statistically significant, but substantively significant as well.

Subsequent Crashes

Analysis of ANCOVA assumptions

The homogeneity of variance assumption underlying the analysis of covariance (in this particular analysis, the assumption that the variance of 1-year subsequent crashes is the same in all four groups being analyzed) was checked using the Brown and Forsythe's Test in SAS. As with the other two criterion variables previously described, this assumption was also violated for subsequent crashes ($p \leq .0001$). The results of prior descriptive analyses show that the largest difference between the four groups on the variance of subsequent crashes is 1.73:1. This value is the lowest among all three criterion variables, and is probably not large enough to cause significant problems with the robustness of the ANCOVA model; thus, work proceeded to develop a final model.

The homogeneity of regression, or equal slopes assumption, was tested for the covariates, both as a group and individually, using SPSS MANOVA and SAS GLM. The results from the group covariate tests were assessed first, and they showed that no covariates violated when crossed with group ($p = .182$), or group x priors ($p = .820$), but at least one may have violated when crossed with priors ($p = .081$). Next, the individual tests were assessed. These results showed that even though the covariates as a group did not interact significantly with group, one covariate—<u>ZIP Code average crash rate</u>—was significant ($p = .008$). As discussed in more detail in the previous Results section, there is a possibility that significant violations shown in the individual covariate tests might simply be type I errors, and that is also the case here. It was decided to include this slope violation in the final model, and remove it in the final analysis if it was no longer significant within the context of all effects in the model.

The group covariate test for interactions with priors showed the possibility of a slope violation, and this was confirmed by the individual tests, which revealed that <u>3-year prior convictions DWS</u> violated at $p = .014$. This effect was included in the final ANCOVA model. The individual tests also showed that no covariates violated when crossed with group x priors, also confirming the results from the group covariate test.

The next step was to develop preliminary ANCOVA models to compare the size of the main effects of interest with the slope violations involving those effects crossed with the violating covariates. The results from these analyses showed that the mean square for the main effect of group was not quite twice the size of that for ZIP Code crash average x group, but that this slope violation was still significant. In addition, these preliminary analyses showed that the slope violation involving 3-year prior convictions DWS was about 12 times the size of the main effect of priors, and that this slope violation was also significant. Based on the results from these preliminary models, the interactions involving both of the covariates that violated the slopes assumption were left in the final model so that separate regression slopes could be computed for the groups.

The preliminary ANCOVA models were also used to determine whether any covariate main effects were no longer significant within the context of all of the effects in the model, and thus should be removed from the final model. Initially, only one covariate—ZIP Code % single—appeared to be non-significant. However, once this was removed from the model a second covariate, ZIP Code % receiving social security, became non-significant as well (it may have been significant initially only because ZIP Code % single was acting to suppress irrelevant variance in it). Based on these findings, both covariates were removed from the final model.

Group differences on covariates

As a check for possible biases created by pre-existing differences among the groups, SPSS was used to compute one-way ANOVA analyses for interval-level covariates, and chi-square analyses for categorical-level covariates. Potential group differences on the covariates were checked both for the impound and control groups, as well as for the four groups created by considering prior DWS/DWU convictions along with group membership. The results of these analyses are presented in Table 8.

Table 8

Group Differences on Covariates (Subsequent Crashes Analysis)

	Group								Significance tests & *p* values
	Control				Impound				
Variable	First offenders		Repeat offenders		First offenders		Repeat offenders		
Sex % male % female	73.5% 26.5%		86.0% 14.0%		77.6% 22.4%		86.7% 13.3%		Grp: $x^2 = 15.49$ $p \leq .001$ Grp x Priors: $x^2 = 234.21$ $p \leq .001$
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
3-yr prior convictions	2.12	2.04	4.46	3.08	2.00	2.04	4.28	3.19	Grp: $F = 9.57$, $p = .002$ Grp x Priors: $F = .362$ $p = .547$
Age	27.34	9.27	28.01	7.80	28.19	9.73	30.03	8.32	Grp: $F = 76.35$, $p \leq .001$ Grp x Priors: $F = 14.12$ $p \leq .001$
3-yr prior conv DWS	.588	.987	2.45	2.19	.561	1.09	2.47	2.34	Grp: $F = .024$, $p = .876$ Grp x Priors: $F = .648$ $p = .421$
ZIP Code crash avg	.236	.043	.242	.039	.238	.043	.237	.041	Grp: $F = 2.30$, $p = .130$ Grp x Priors: $F = 21.89$ $p \leq .001$
ZIP Code % drive alone to work	.720	.087	.726	.084	.715	.085	.719	.089	Grp: $F = 13.41$, $p \leq .001$ Grp x Priors: $F = .440$ $p = .507$
ZIP Code % single	.310	.076	.297	.071	.314	.074	.305	.074	Grp: $F = 21.04$, $p \leq .001$ Grp x Priors: $F = 2.47$ $p = .116$
ZIP Code % receiving social security	.103	.030	.104	.034	.103	.029	.102	.031	Grp: $F = 4.13$, $p = .042$ Grp x Priors: $F = 1.87$ $p = .172$

It can be seen from Table 8 that there are significant group differences on most of the covariates. On the subject-specific covariates, drivers in the impound group are older and have fewer 3-year prior traffic convictions than control group subjects. While these differences would suggest that the impound group is lower risk than the control group, it can also be seen in Table 8 that the former is more predominately male, and thus higher risk, than the latter. There is no difference between the groups on 3-year prior convictions DWS.

There are also differences between the groups on the aggregate-level ZIP Code covariates. Subjects in the impound group live in areas where fewer people drive alone to work relative to control subjects. Also, first offenders in the impound group live in areas with a higher average crash rate than their counterparts in the control group, while this group difference is reversed for repeat offenders.

The results of these tests of differences between groups on covariates used to analyze the effects of vehicle impoundment on subsequent crashes, while indicating that pre-existing differences do exist, do not paint a clear picture of the overall effects of these biases. On some variables impound subjects appear riskier, while on others they appear less risky than subjects in the control group. The final ANCOVA adjustment to the mean crash rates of the groups will provide an indication of how the overall pattern of group differences on the covariates relates to their risk of subsequent crashes.

Recidivism analysis

The SAS GLM program was used to conduct the ANCOVA analysis to examine the effects of vehicle impoundment on subsequent crashes. The six covariates which remained in the final model were given priority in the assignment of overlapping variance. The covariates were followed by the main effects of prior DWS/DWU convictions and group, which in turn were followed by group x priors and the two slope violation interaction effects. The results of this analysis are presented in Table 9.

Table 9

Summary Table of Two Factor ANCOVA: Subsequent Crashes

Source of variation	Degrees of freedom	Mean square	*F* value	Significance level
Covariates *	8	.64	10.52	.0012
Priors	1	.14	2.29	.1301
Group	1	1.18	19.39	.0001
Group x Priors	1	.17	2.78	.0954
Error	12,698	.06		

*Includes the covariate x group/priors interaction for each of the two covariates that violated the equal slopes ANCOVA assumption.

The data in Table 9 show that the main effect of group is highly significant, and that while the group x priors interaction approaches statistical significance at p = .0954, it does not reach the conventional p = .05 level. This means that there are differences in the rate of subsequent crashes between subjects whose vehicles were impounded and those who did not have their vehicles seized, and that this difference between the groups did not change to a significant degree as a function of the number of prior DWS/DWU convictions. These effects are shown in Figure 3.

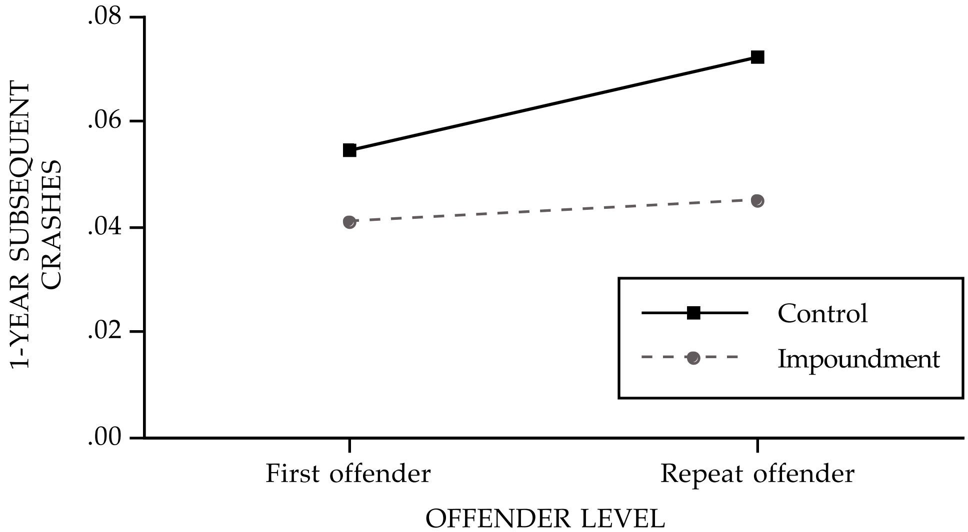

Figure 3. Adjusted subsequent crashes for vheicle impoundment versus control groups, by number of prior DWS/DWU convictions.

The most salient feature of Figure 3 is that the crash rate is lower for subjects in the impound group than it is for control group subjects. This illustrates the significant main effect found for group. It can also be seen that the difference in subsequent crashes between impound and control subjects appears to be larger for repeat offenders than for first offenders, although since the interaction of group x priors was not statistically significant, this stronger effect of impoundment for repeat DWS/DWU offenders is suggestive only.

While the findings show that drivers who had their vehicles impounded had a statistically significant lower rate of subsequent crashes than drivers who did not lose their vehicles, it is important to determine whether this difference in crash rates is large enough to be meaningful as well as statistically significant. Table 10 shows the adjusted group means and their associated effect sizes.

Table 10

Adjusted Group Means and Effect Sizes: Subsequent Crashes

Offender level	Group means		Effect size
	Control	Impound	
First	.05450	.04103	24.71%
Repeat	.07204	.04498	37.56%

It can be seen from the data in Table 10 that the effect sizes are substantial, especially for repeat offenders. Interestingly, the effect sizes of the unadjusted means–23.12% for first offenders and 39.34% for repeat offenders–are not much different than the ones reported here for the adjusted means. The means are adjusted in such a way as to <u>increase</u> the difference between the crash rates for the <u>first</u> offender impound and control group offenders, but to <u>decrease</u> it for <u>repeat</u> offenders. In any case, the results are meaningful and significant from both a substantive and statistical standpoint.

DISCUSSION

Before discussing the results of the analyses evaluating the efficacy of vehicle impoundment, some limitations inherent in the quasi-experimental nature of this study need to be mentioned so that appropriate conclusions can be drawn. One potential problem is that because it was not feasible to randomly assign drivers to impoundment or control groups, it is possible that selection processes operated to produce groups that were different to begin with. The issue here is that such pre-existing group differences might lead to different expected crash/conviction rates for the groups, and this would be an alternative hypothesis to that of the effects of vehicle impoundment in explaining the results of the study.

The study design attempted to control potential pre-existing group bias in two ways. In the first, control group subjects in two of the study areas where it is likely that vehicle impoundment was being used selectively (thus potentially leading to somewhat different types of drivers in each of the groups) were matched to impound subjects based on propensity scores. This produced a group of control subjects who were very similar to impound subjects on those variables that formed the propensity scores. In addition, on a more general level, the analyses used to evaluate the impact of vehicle impoundment employed the use of covariates, which statistically made the groups equivalent on those dimensions measured by the covariates.

While undeniably useful, there are limits to the effectiveness of statistical controls. Perhaps the most significant problem is that it is difficult to capture and measure all of the dimensions on which the groups differ and which would affect their recidivism rates. Thus, there remains the possibilitiy that there are uncontrolled selection biases operating which may have influenced the results. Because of this, the results do not so much prove the efficacy of vehicle impoundment as they portray relationships between impoundment and recidivism that are suggestive of its effects.

The quasi-experimental design used in this study is also open to biases resulting from historical events that may have differentially affected one of the groups. While it is not possible to completely account for such threats to the validity of the study, the main threats -- historical changes in statewide rates of DWS/DWU convictions, total traffic convictions and crashes -- were examined to check for bias. All three measures show a decline from 1994 to 1995. The rates of declines for two of the three measures, total traffic convictions and crashes, which dropped 4.6% and 1.7% respectively, are too small to represent a plausible alternative explanation for the results to that of vehicle impoundment.

The year-to-year decline for the third measure, subsequent DWS/DWU convictions, was substantial, dropping 24% from 1994 to 1995. Since the results from the study showed that impounded drivers had from 24% (first offenders) to 34% (repeat offenders) fewer subsequent DWS/DWU convictions, it is clear that statewide trends account for much, but not all, of the findings for the effects of vehicle impoundment on this recidivism measure. This drop in statewide trends from 1994 to 1995 is remarkable. While there is an overall decline in DWS/DWU during the past several years (e.g., from 1992-1993 there was a drop of 8.0%, and from 1993-1994 there was a decline of 3.3%), the decline from 1994 to 1995 is notable for its magnitude. Because this decline is contemporaneous with the implementation of vehicle impoundment/forfeiture, it is quite possible that this represents, all or in part, the effects of these laws at the level of a general deterrent impact. A recently commenced general deterrence analysis will shed more light on whether the decline in recidivism found in this study is simply due primarily to statewide trends, or whether it represents the impact of vehicle impoundment operating at both the specific and general deterrent levels.

It would have been desirable to sample subjects from throughout California, but due to both the lack of a centralized database and to the structure and quality of local impoundment databases, subjects were selected from four geographical areas in the state where useable data existed. Strictly speaking, the results of the study pertain to these four areas only. This said, however, it is likely that the results generalize reasonably well to those jurisdictions where the enforcement level is similar to the regions sampled in this study. Having a vehicle seized in Southern California is likely to affect a driver in similar ways that it would a driver in the northern part of the state. In addition, this study does not stand in isolation, and the positive findings for civil forfeiture in Oregon (Crosby, 1995), and immobilization (Voas et al., 1997) and impoundment (Voas et al., in press) in Ohio, suggest that the beneficial effects of vehicle impoundment are not a localized phenomenon.

A final preliminary issue of import is the integrity of the sanctions—what are we measuring? The present study is really a study of vehicle impoundment, not vehicle forfeiture. Although SB 1758 and AB 3148 provided for both impoundment and forfeiture, the latter is simply not being used on any significant scale; during 1995, fewer than 400 vehicles were reported forfeited in California, while more than 100,000 were seized and impounded. It is also important to note that some undetermined number of control subjects also had their vehicles impounded for a very brief period under a section of the Vehicle Code predating the new impound/forfeiture laws which provides for the impoundment of vehicles driven by suspended/revoked and unlicensed drivers (the vehicle is typically held under this section for a day or two; it must be released to the registered owner, or their agent, when they present a valid driver license and proof of current vehicle registration). This means that the analyses done here were conservative, in that they underestimate the effects of impoundment relative to a no-impoundment control group. This study can therefore be characterized as an evaluation of the policy effects of implementing a 30-day vehicle impoundment law in the context of an existing law which allows for a very brief period of impoundment.

Subsequent DWS/DWU Convictions

The ANCOVA analysis evaluated whether subjects who had their vehicles impounded for DWS/DWU had a different rate of subsequent DWS/DWU convictions than subjects who were cited for DWS/DWU but whose vehicles were not seized. This is really a direct measure of recidivism—will subjects be less likely to again violate their invalid driving status if their vehicles are impounded, than if they are not? The results showed that drivers whose vehicles were impounded did, in fact, have fewer subsequent DWS/DWU convictions than a similar group of non-impounded drivers. In addition, the results revealed that the relative effectiveness of vehicle impoundment is even stronger for repeat offenders, or those drivers with prior DWS/DWU convictions.

These findings were not only statistically significant, they are also of a sufficient magnitude to represent a meaningful impact. Drivers who had their vehicles impounded had, depending upon their offender level, between 24% to 34% fewer DWS/DWU convictions in the year after their vehicles were impounded than similar drivers whose vehicles were not taken. Since the maximum length of the impoundment period is 30 days (unless a driver chooses not to reclaim his vehicle), it is clear that the effects found here go beyond simple incapacitation.

Thus, there exists some level of specific deterrence associated with vehicle impoundment. What is not clear is whether S/R drivers who lose their vehicles are choosing not to drive during their period of disqualification, or whether they are driving even more carefully and less frequently. It has been known for some time that S/R drivers drive more carefully and less often during their period of license disqualification (Hagen et al., 1980; Ross and Gonzales, 1988). Because this study compares two groups of S/R drivers, one of which has suffered vehicle impoundment, the effect of losing their vehicle is causing impounded drivers to either drive even more carefully and less often than they were previously, or to give up driving illegally altogether, to avoid again having their vehicle impounded.

While few rigorous evaluations of vehicle impoundment, forfeiture or immobilization have been conducted to date, the results of this study are in accord with those found by Crosby (1995) for civil forfeiture in Portland, Oregon. Crosby also found sizable effects, with DWS recidivism reduced by half for drivers whose vehicles were seized. The results of this study are also similar to those reported for vehicle immobilization (Voas et al., 1997) and impoundment (Voas et al., in press) programs in Ohio.

Subsequent Total Traffic Convictions

The analysis measuring the impact of vehicle impoundment on the rate of 1-year subsequent traffic convictions is less an evaluation of recidivism than it is a general indication of how impoundment affects lawful driving overall, because impounded drivers may or may not still have been suspended/revoked when they were cited for a subsequent traffic offense. The results from the conviction analysis showed that drivers whose vehicles were impounded had significantly fewer subsequent convictions than similar drivers whose vehicles were not impounded. While there was suggestive evidence that impoundment's effects were even larger for repeat offenders, these findings came quite close to but did not actually reach statistical significance, leaving this somewhat of an open question.

As with the effects of vehicle impoundment on subsequent DWS/DWU convictions, the effects of impoundment on all traffic convictions were also of considerable magnitude. Impounded drivers had between 18% and 22% fewer traffic convictions, depending upon their offender level, than their counterparts in the control group. These differences are substantial enough to represent a meaningful impact, and tell us that not only does vehicle impoundment positively influence recidivism rates, it also has salutary effects on the overall subsequent driving behavior of drivers experiencing it, regardless of their license status. Thus, at least when traffic safety is framed in terms of traffic convictions, the results show that there are traffic safety benefits to impounding vehicles.

Subsequent Crashes

The analysis evaluating how impounding vehicles affects drivers' subsequent crash rate is perhaps the bottom line regarding the traffic safety impact of this countermeasure. Crashes exact a huge toll from society, both in terms of economic impacts and human suffering, and any countermeasure that can ameliorate this cost is especially worthy of attention.

The results from the analysis show that impounded drivers have fewer 1-year subsequent crashes than similar drivers whose vehicles are not seized. Again, as with the traffic conviction results, there is suggestive evidence that the effects are even stronger for repeat offenders, although the results for this (interaction) effect approached but did not quite reach statistical significance. However, the consistency of the trend of stronger effects for repeat offenders across all of the outcome measures of impoundment make it likely that impounding vehicles really does reduce crashes more strongly for repeat offenders. Importantly, the results are of both statistical and substantive significance. Drivers with no prior DWS/DWU convictions whose vehicles are impounded have 25% fewer subsequent crashes than similar offenders in the control group, while repeat offenders who lose their vehicles have 38% fewer subsequent crashes than repeat offenders whose vehicles are not seized.

These findings, considered along with those evaluating the effects of vehicle impoundment on traffic convictions, strongly suggest that this countermeasure has a substantial effect in improving traffic safety. These positive benefits appear to last beyond the period that the vehicle is actually impounded, indicating that there is not only an incapacitative effect of impounding vehicles, but a specific deterrent effect as well.

Conclusion

The findings reported here provide strong support for impounding vehicles driven by suspended/revoked and unlicensed drivers. They add weight to a small but growing body of evidence that vehicle-based sanctions, whether they involve immobilizing vehicles for a period of time through such devices as a "club" on the vehicle's steering wheel, or whether they consist of simply seizing and impounding vehicles, are an effective means for better controlling the risk posed by disqualified drivers. This is encouraging news, because while suspending or revoking driver licenses is known to be effective, research has also shown that it is widely violated and that S/R and unlicensed drivers continue to pose a significant safety risk on the highways.

Vehicle impoundment appears to reduce both subsequent DWS/DWU convictions (i.e., recidivism), and subsequent crashes and convictions overall. Of significant interest is the finding that impoundment is even more effective in reducing recidivism, and possibly crashes and convictions as well, for repeat offenders relative to those with no prior DWS/DWU convictions. Repeat offenders are, by definition, characterized by a pattern of high risk driving that is resistant to change; impounding their vehicles appears to be making a dent in that.

The salutary effects of vehicle impoundment on subsequent crashes is also of significant interest, because crashes represent a high cost to society in several ways. Many vehicle impoundment programs in California are self-supporting, or close to self-supporting, because law enforcement agencies charge an administrative fee at the time impounded vehicles are released in order to cover their costs of running the programs. Thus, while no formal cost-benefit analysis was undertaken in this study, the generally self-supporting nature of impoundment programs, plus the finding that these programs appear to reduce crash rates, suggests that they may be a relatively low cost avenue to enhancing traffic safety.

Finally, there has been some concern expressed about the failure of California law enforcement agencies and courts to utilize vehicle forfeiture; there are probably many reasons for this, including resource constraints among district attorneys who must prosecute such cases and concerns among public officials about the political sensitivities of asset seizure. But in the end the lack of utilization of vehicle forfeiture may not matter much. Impounding vehicles is having a substantial positive effect in California, and if Crosby's (1995) findings in Oregon hold in California as well, going the extra step of forfeiting vehicles may not produce much added benefit.

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