

Interim Report

AN EVALUATION OF THE IMPACT OF THE VIRGINIA DRIVER IMPROVEMENT  
PROGRAM ON NEGLIGENT DRIVING: 12-MONTH REPORT

by

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## SUMMARY

This study sought to evaluate the impact of the four basic treatment combinations of the rehabilitation component of the Virginia driver improvement program. This was accomplished through a comparison of the driving records of experimental group subjects who received the various treatments with records of control group subjects not receiving treatment. It was hypothesized that if the treatments were successful, drivers in the experimental group would have significantly better posttreatment driving records during 12 months of observation than their corresponding control groups; i.e., they would have fewer convictions and accidents. Statistical techniques were used to test this hypothesis with regard to (a) the advisory letter — a warning letter issued after a driver accumulates 6 points in one year; (b) the group interview — a one-time classroom meeting held when a driver accumulates 8 points in one year; (c) a combination of the group interview and an advisory letter; and (d) the personal interview-driver improvement clinic — a one-on-one interview, usually followed by a classroom course in defensive driving, administered when a driver accumulates 12 points in one year.\*

Theoretically, should negligent drivers continue to receive points following the personal interview their licenses are suspended. In actuality, because of the heavy work load among driver improvement analysts, there are very few formal suspensions. No attempt was made to evaluate the suspension alternative.

### Summary of Findings and Conclusions

The analyses of the four types of treatment yielded the following results.

1. Receipt of a warning letter from the Division of Motor Vehicles had no effect on the subsequent driving records of program participants. Drivers receiving warning letters experienced just as many posttreatment accidents and convictions as drivers not receiving a letter.

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\*Drivers receive rehabilitation based upon the number of traffic conviction points they accumulate over time. The validity of the point system was not directly evaluated in the study.

2. Drivers attending the group interview as their first contact with the driver improvement program had significantly better posttreatment driving records than similar drivers not attending the interview. Thus, attendance at a group interview resulted in reduced major and minor convictions and increased the probability that a driver would remain conviction-free for a full year. However, no impact on accident experience was noted.
3. Paradoxically, although the group interview alone was extremely successful in reducing the numbers of convictions, when paired with the advisory letter it was completely ineffective. Drivers receiving this combination of treatments had just as many accidents and convictions following a treatment as did drivers receiving the advisory letter only. It was concluded that some aspect of the advisory letter emasculated the effect of the highly successful group interview. It was speculated that persons who had been contacted twice by the driver improvement program, who probably had at least three convictions in order to qualify for treatment, and who still had their licenses were more likely to realize that the driver improvement program was more lenient than the old sanctions, under which two convictions could result in suspension, than were persons who had had only one experience with the system.
4. The personal interview-driver improvement clinic treatment combination was highly effective in reducing subsequent minor convictions and in increasing the length of time a driver remained conviction free. Of all the treatments, the effects of the personal interview were the most permanent, lasting throughout the 12-month observation period. No effect was noted on subsequent accident experience.
5. When all the treatments were considered together, it was found that drivers receiving treatment had fewer major and minor convictions than did drivers not receiving treatment, and that experimental group drivers tended to remain conviction free for a longer period of time. Also, drivers receiving treatment experienced fewer posttreatment injury accidents than did control group drivers. (However, this result could be spurious, since the probability of one variable being significant by chance when many are tested is quite high.) Overall program effects were felt for a 9-month period.

It was concluded that the two most pressing needs of the driver improvement program are those for (1) improvement of the entry level remediation offered by the ineffectual advisory letter, and (2) modification of the program to more directly address accident avoidance as well as conviction reduction.

### Recommendations

The following recommendations were made for modifying the driver improvement program.

1. That the Division of Motor Vehicles seek legislation to allow it to alter the driver improvement program as it deems appropriate, without having to continually change the driver improvement statute.
2. That the entry level treatment program be modified. While an attractive alternative is to simply change the style, content, or format of the letter, or to introduce it earlier in the system, this solution does not address the fact that the letter reduces the impact of other treatments. For this reason it is recommended that the group interview replace the advisory letter as the entry level treatment, since the group interview has been shown to be a very effective first contact. Consideration should also be given to administering the group interview at the 6 rather than the 8- point level.
3. That individual treatment programs be modified to more directly and explicitly deal with the issue of accident avoidance as well as conviction avoidance.
4. That a minimum number of points be awarded to persons involved in an accident. Currently, since points are accumulated only through conviction, the main incentive in avoiding license suspension is to avoid subsequent convictions. In order to emphasize accident avoidance, point values should be assigned to accident involvement as well. To enhance the appearance of "fairness", persons convicted of violations resulting in an accident would receive 2 points, plus those points associated with the conviction itself, while drivers not incurring convictions would receive 1 point to identify them as less probable accident repeaters.

5. That the Division stop awarding safe driving points to accident- and conviction-free drivers. It has been shown that these incentive point programs do not improve driving behavior and often cause distortion and reduce the diagnostic capability of the point system in identifying drivers who need treatment.
6. That the Division make every effort to increase the number of formal suspension hearings and increase the proportion of drivers eligible for suspension receiving a hearing. This is especially crucial, since fear of suspension is the most powerful incentive to change driving behavior.
7. That the Division of Motor Vehicles establish an on-going monitoring system to evaluate program changes and overall impact on a continuing basis.



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## INTRODUCTION

In 1975, the Virginia Division of Motor Vehicles abandoned its strictly punitive system of dealing with traffic offenders in favor of a program of driver improvement. As stated under the provisions of the Virginia Driver Improvement Act (Section 46.1-514.1 of the Code of Virginia), the purposes of this new program included:

to improve and promote greater safety upon the highways and streets of the state; to improve the attitude and driving habits of drivers who accumulate motor vehicle conviction records; to determine whether certain drivers possess mental, physical or skill deficiencies which may affect their ability to safely operate a motor vehicle; to establish a Uniform Demerit Point System which will identify those drivers who are considered by the accumulation of demerit points to be habitually reckless or negligent drivers and frequent violators of the laws regulating the movement or operation of motor vehicles. ...

Obviously, the program designed to meet the above objectives embodies a multifaceted and comprehensive approach to eliminating aberrant driving, and consists not only of a point system for the identification and referral of chronically negligent drivers but also a system of remediation designed to treat these drivers. In Virginia, the treatments involve an advisory letter, group or personal interviews, driver improvement clinics, periods of probation, and any combination of these treatments. Among program participants, the old sanctions of suspension and revocation of the driving privileges are invoked only as a last resort, being reserved for cases in which the extensive system of remediation proves unsuccessful in modifying unsafe driving behavior. The driver improvement program became operational in January 1975 and has treated more than 200,000 drivers.

In 1977, it was decided that the program had been in operation for a sufficient length of time to allow for the evaluation of its impact on negligent driving. With this in mind, the Division of Motor Vehicles approached the Virginia Department of Transportation Safety with a request that the Highway and Transportation Research Council conduct an evaluation of the driver improvement system in the state. This report presents the findings of the resultant study.

## PURPOSE AND SCOPE

The purpose of the study is twofold. The primary goal is to determine the impact of the driver improvement program on Virginia's traffic and safety environment in terms of accidents and traffic convictions averted as a result of appropriate treatment. A secondary, but very important, function of the study is to establish an ongoing system of data collection to be used by the Division of Motor Vehicles to continually evaluate the effectiveness of the driver improvement program after the termination of the four-year study period and to establish statewide norms for administrative evaluations.

The study will be limited to an evaluation of the driver improvement system as it currently operates; it was not designed to —

1. determine whether point values are appropriately assigned to each violation;
2. determine if the order in which treatments are given is appropriate;
3. evaluate whether the criteria for receiving a given treatment are appropriate;
4. evaluate the quality of treatment offered throughout the state; nor
5. evaluate the efficiency or consistency of the administration of the program, except where the impact of the driver improvement program is affected.

## VIRGINIA'S DRIVER IMPROVEMENT PROGRAM

The main purpose of the driver improvement program is to diagnose and offer treatment to chronically unsafe drivers — those drivers who incur frequent convictions and who constitute a hazard to themselves and others. Most traffic offenses mentioned in the Code of Virginia have been assigned point values intuitively determined to be consistent with the degree of hazard attached to them. Drivers become eligible for the various treatments offered in the program based upon the number of points they accumulate in a 1- or 2-year period based upon their convictions.

Figure 1 outlines the operation of the driver improvement program. (A more detailed description of the program appears in an earlier report by Lynn [1980].) As seen at the top of the chart, persons enter the system as a result of being convicted of one or more violations. Once a driver has accumulated a total of 6 demerit points in a 1-year period or 9 points in a 2-year period, he is subject to receiving the first stage of treatment, the advisory letter. This letter informs the driver that he has accumulated sufficient points to warrant the Division's concern and warns him that if he accumulates additional points, he may become eligible for additional administrative action, possibly including suspension.

Should the driver incur no more convictions, no further action is taken against him. However, if he accumulates additional points for a total of 8 points in 1 year or 12 points in 2 years, he becomes eligible for a group interview. This treatment consists of a 1-hour interview with a driver improvement analyst, with a small group of 8 to 12 other drivers. In the course of the hour, the analyst reviews each driver's record, explains what action will be taken should the driver earn more points, and stresses that suspension can be invoked if needed.

Should drivers continue to accumulate points to a level of 12 points in 1 year or 18 points in 2 years, they become eligible for a personal interview with a driver improvement analyst. This interview is viewed as diagnostic rather than a form of a treatment in itself. Based on the personal interview, negligent drivers are most often sent to the driver improvement clinic, an 8-hour course of classroom instruction held over a 4-week period in the violators' communities.

Should the driver continue to accumulate points up to 6 additional points in 1 year or 12 additional points in 2 years, he may become eligible for a formal hearing, at which time his license may be suspended or revoked. Traditionally, these hearings are rather rare due to personnel limitations.

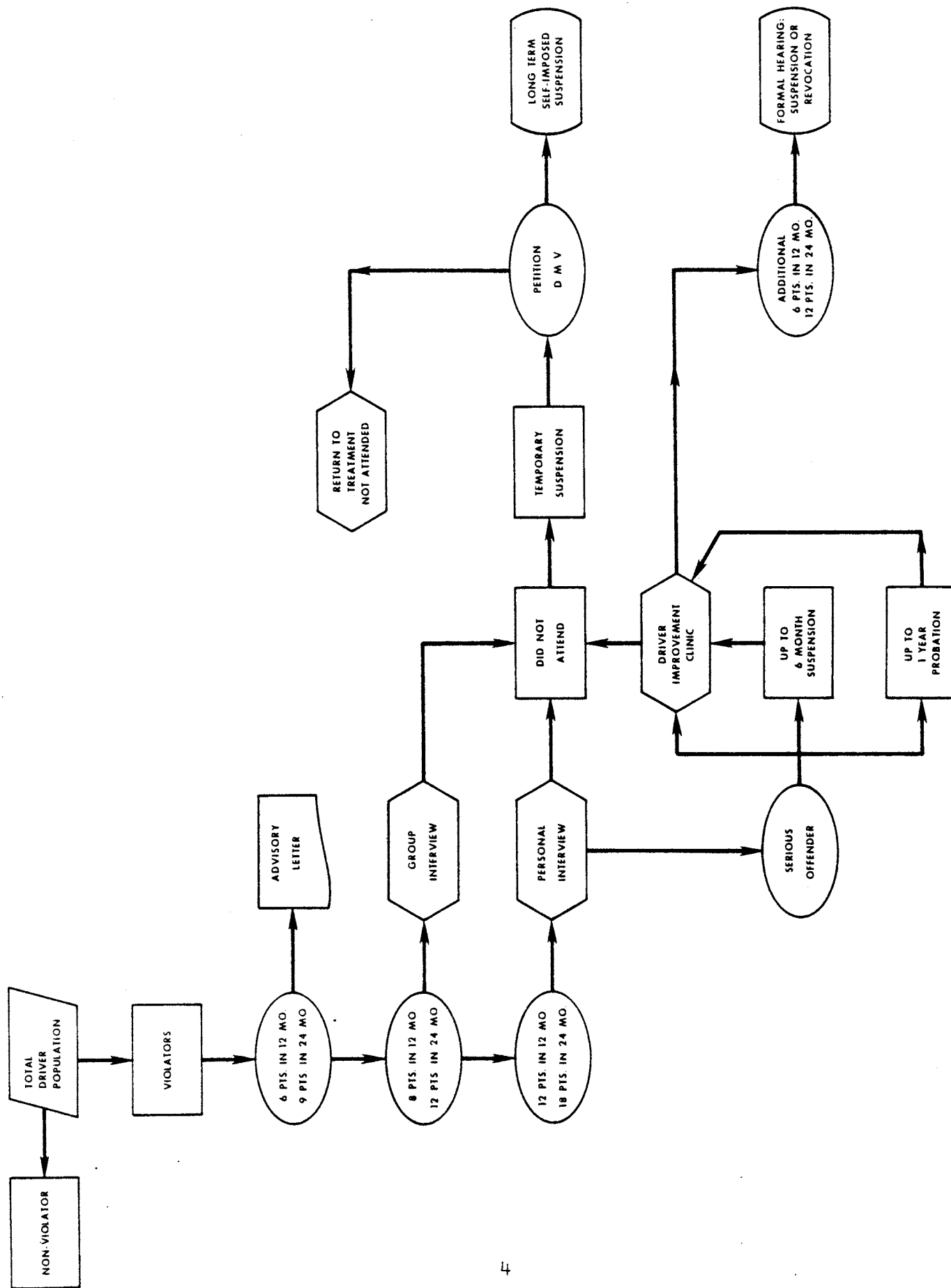


Figure 1. Operations of the Virginia driver improvement program.

Not all drivers receive this sequence of remediation. The system is flexible enough to allow drivers to enter the system at levels consistent with their driving problems. For instance, should a driver become eligible for an upper level treatment, he may enter the program at the group interview or personal interview level and bypass the advisory letter. This would allow for immediate intervention in the person's driving problem and could make successful treatment somewhat more probable.

## EVALUATION OF THE VIRGINIA PROGRAM

The available literature on the driver improvement efforts not only points up effective types of treatment and the need for individual, on-site evaluation, but also examines various methodologies and problems to be avoided in designing experiments. The general design of this study involved the comparison of experimental groups receiving treatment with control groups not receiving treatment to determine the effectiveness of the forms of remediation in the Virginia program. A random assignment of subjects was considered essential; however, assignment to the driver improvement program is not discretionary in Virginia (see Appendix A for the enabling legislation). Special legislation had to be sought to enable the Commissioner of the Division of Motor Vehicles to waive treatment for randomly selected subjects, so as to provide the control groups needed. This legislation, which appears in Appendix B, was passed by the 1978 General Assembly to be operational for one year. (The amendment was thereafter continued until the termination of the study.)

### Study Groups

The subjects were randomly assigned to study groups at three levels as shown in Figure 2. These levels correspond with each of the three levels of treatment — the advisory letter, the group interview, and the personal interview-driver improvement clinic. The series of treatments appears at the top of Figure 2, and the corresponding study groups are described under the following subheadings.

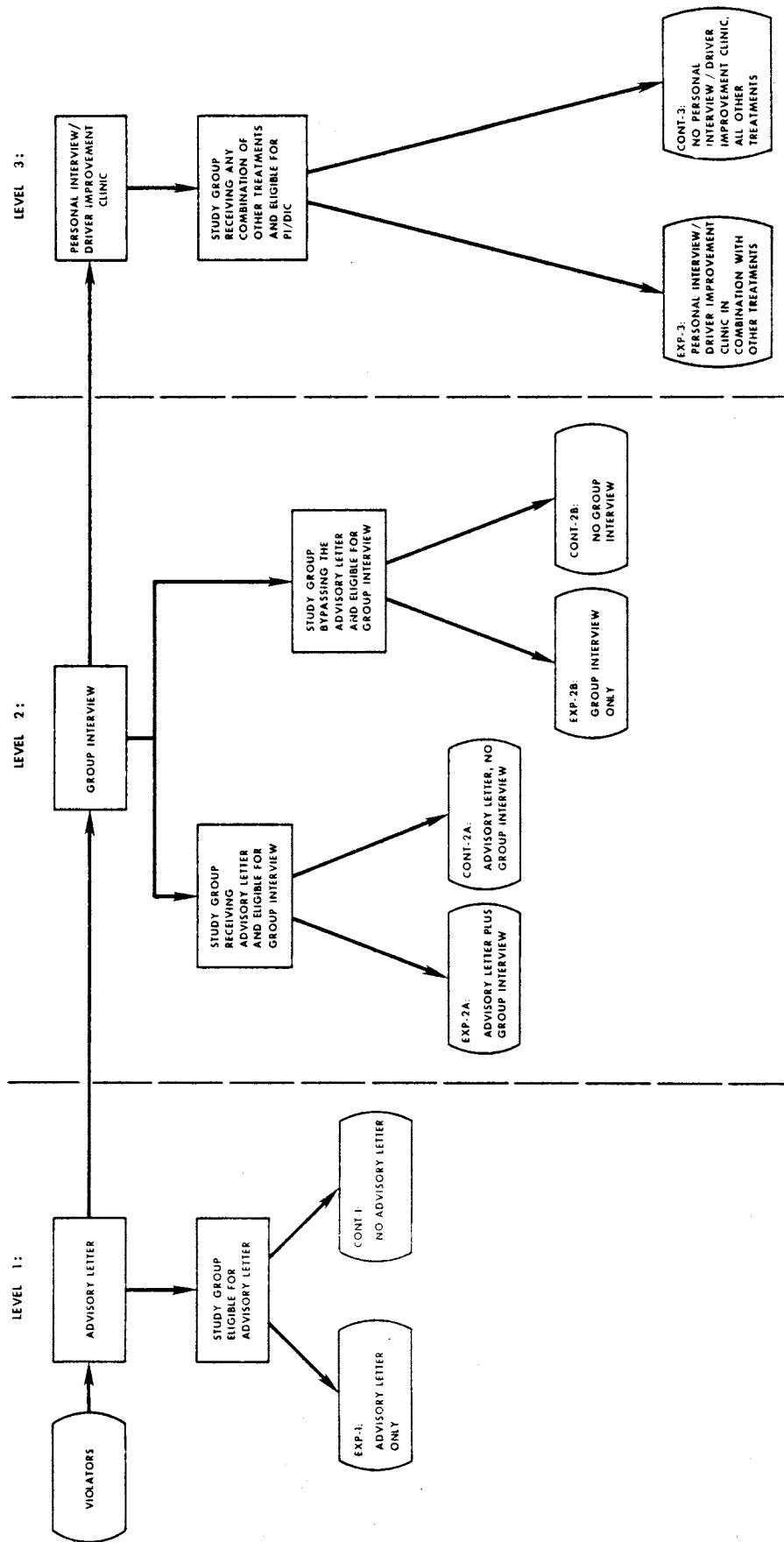


Figure 2. Levels of study groups.

### Level 1: Advisory Letter

Once violators accumulated sufficient points to become eligible for the advisory letter, they were randomly assigned to the level 1 experimental and control groups. The level 1 experimental group (EXP-1) received the advisory letter only, while the control group (CONT-1) received nothing. After they were assigned to the groups, no additional administrative actions were taken against persons in either group. They were allowed to accumulate points without additional contact with the driver improvement system. Their accident and conviction experiences were monitored initially for a 6-month period and later for a 12-month period following their assignment.

### Level 2: Group Interview

The second level of treatment involved the group interview, which has two frequently used avenues of entry. In the first, the driver accumulates 6 points in a 1-year period (or 9 points in a 2-year period), receives an advisory letter, accumulates at least 2 more points in that year (or 4 more points in 2 years) and is assigned to group interview. This could be accomplished by receiving two minor speeding convictions (1 to 9 miles per hour over the posted limit) for a total of 6 points, followed by a third minor speeding conviction. The second avenue of entry involves receiving 8 (or 12) points and being assigned directly to group interview, thus bypassing the advisory letter. This could be accomplished by receiving two or more serious convictions (such as travelling 10 to 19 miles per hour over the posted limit) in a one-year period. These two methods of entry constitute two different treatment groups -- one receiving an advisory letter plus the group interview and one receiving the group interview only. Since rather large volumes of drivers enter group interview through these two methods, both were evaluated.

As those subjects having received the advisory letter became eligible for a group interview, they were randomly assigned to experimental and control groups. The experimental group in this case (EXP-2a) received both the advisory letter and the group interview. The control group (CONT-2A) received the advisory letter but not the group interview. As persons bypassing the advisory letter became eligible for group interview, they too were randomly assigned to experimental and control groups. The experimental group (EXP-2b) received the group interview only, while the control group (CONT-2b) received no treatment. Again, the driving behavior of all four of these groups was monitored for a 6- and a 12-month period, and at the end of the study period treatment groups were compared to no-treatment groups to assess the impact of the remediation employed.

### Level 3: Personal Interview-Driver Improvement Clinics

Those drivers not already assigned to a study group were eligible to continue accumulating points and could become eligible for assignment to the personal interview and the driver improvement clinic. As with the group interview, there are various avenues of entry into the personal interview phase. For instance, a driver could receive an advisory letter, attend a group interview, and attend a personal interview (followed by some additional treatment). The driver could enter the system at the group interview level as described above, and then attend a personal interview; or he could receive an advisory letter, bypass the group interview, and go directly to the personal interview level. Finally, the person could accumulate the necessary points, be assigned to a group interview, but become eligible for a personal interview before he can attend the group session. This would be equivalent to entering the system at the personal interview level. Since the numbers of individuals receiving each of these treatment combinations were too small to allow the separate evaluation of each by statistical methods, all combinations of treatments including the personal interview were evaluated in the aggregate. As subjects became eligible for the personal interview, independent of their previous treatments, they were randomly assigned to experimental and control groups. In this case, the experimental group subjects (EXP-3) received any previous treatments to which they were assigned, and then received a personal interview along with the driver improvement clinic and/or were suspended or placed on probation for some period of time. The control group (CONT-3) received the previous treatments, but did not attend the personal interview and were not assigned to the driver improvement clinic, etc. As with the other levels, the experimental group that had the personal interview was compared to the control group that did not to determine the effect of the personal interview-driver improvement clinic unit of treatment on driver behavior. It should be noted, then, that all conclusions concerning the personal interview phase of treatment are based upon the assumption that the subjects received both the group interview and the advisory letter treatments.

In summary, eight study groups were considered — four experimental groups, each receiving a different set of treatments, and four corresponding control groups. The treatments received by each group and the criteria for entry appear in Table 1.



Table 1  
Summary of Study Groups

TREATMENT	Treatments				Criteria For Entry, Points	
	Advisory Letter	Group Interview	Personal Interview Dr. Improv. Clinic		one year	(two years)
Level 1: Advisory Letter						
EXP - 1	X	—	—		6	(9)
CONT - 1	0	—	—		6	(9)
Level 2: Group Interview						
EXP - 2A	X	X	—		8	(12)
CONT - 2A	X	0	—		8	(12)
EXP - 2B	0	X	—		8	(12)
CONT - 2B	0	0	—		8	(12)
Level 3: Personal Interview-Driver Improvement Clinic						
EXP - 3			X		12	(18)
CONT - 3 (Any previous combination)			0		12	(18)

### Sampling Plan

Eligible subjects were randomly assigned to the entry groups mentioned previously, based on the millisecond of entry of the transactions making them eligible for selection; i.e., the time at which the conviction was entered on the driver's record. Time of entry and social security number are pseudo-random elements in the driving record. By using time as the criterion for selection, problems arising from the absence of the social security number are avoided. The subjects' assignments were equally distributed across a 12-month period such that approximately 1/12 of those in any one of the eight groups were persons becoming eligible in any given month.

Sample sizes were determined according to the formula and procedures shown in Appendix C. As this formula indicates, sample sizes are related to the precision or exactness of a study in that the more precision required, the larger the sample sizes must be. For instance, if it is necessary to detect a very small change in the event being measured, say accidents, then it will be necessary to have a very large sample size. On the other hand, if less precision is required, then a smaller sample may be used. In determining the precision of this study, the following assumptions were made.

1. The alpha level was set at 0.05 (meaning that there is less than a 5% chance of finding significant results when in fact there are none).
2. The beta level was set at 0.20 (meaning that there is less than a 20% chance of finding no significant results when in fact there are some).
3. The minimum difference that could be detected in this evaluation was a 10% difference in rate. (For example, if the accident rate for the experimental group was 15%, a difference as small as 1.5% could be detected.)

To ensure that the sample sizes calculated were sufficiently large, a conservative approach was used. Sample sizes were determined for each of the criterion measures (accidents, major convictions and minor convictions) for each group, and then the largest of these was chosen. Additionally, the sample sizes were then inflated by 25% to account for unforeseeable sources of attrition later in the study. The final sample sizes for each group appear in Table 2.

Table 2

Sample Sizes for Study Groups

<u>Treatment</u>	<u>Estimated Sample Size</u>	<u>Actual Sample Size</u>	
		<u>Experimental</u>	<u>Control</u>
1 . Advisory Letter Only	4,729	4,899	4,884
2a. Advisory Letter Plus Group Interviews	2,214	2,293	2,319
2b. Group Interview Only	4,344	4,649	4,617
3 . Personal Interview Combinations	1,763	1,738	1,650

It can be noted from Table 2 that all groups exceeded their required sample size with the exception of experimental and control groups 3, the groups involving the personal interview. The numbers of persons becoming eligible for the personal interview were smaller than expected during the subject selection period and thus, even by selecting 100% of the eligible subjects for these groups, the estimated sample size of 1,763 was not reached. However, since this figure was inflated by 25% at the outset, there were still adequate numbers of subjects to allow for statistical analysis at the originally determined levels.

## LIMITATIONS

There are several limitations to this study which should be recognized. These include limitations on the scope of the study and limitations relating to the treatment of the control groups.

### Limitations on the Scope of the Study

In most experimental studies, small groups of subjects are selected from a population to receive some sort of special treatment. In this case, small samples of drivers were selected from the larger population of drivers entering the driver improvement program to receive or not receive driver improvement treatments. The samples of drivers were randomly chosen from the population so that they would resemble the population as closely as possible, and so that any findings of the study involving the samples would apply to the larger population as well. However, if certain groups of drivers in the population were not included in the study samples, then findings of the study would not apply to them. The following groups of drivers were not included in the study groups.

1. Persons volunteering to attend any form of treatment or persons assigned to any treatment by the courts rather than by the Division of Motor Vehicles.
2. Persons convicted of violations for which no point value is assigned, such as "driving while intoxicated" — for which a suspension or revocation is mandatory — or non-moving violations, such as equipment or financial responsibility violations.

### Limitations Relating to Handling of Control Groups

From a purely research point of view, the control groups in this study should have been allowed to accumulate additional points

relating to accidents and convictions without the intervention of the driver improvement system, so that final comparisons of treatment and no-treatment groups would show the true differences between these groups. However, it was realized that this was not wholly practical. Because of the commitment of the Division of Motor Vehicles to preserving the safety of the driving public, extremely high-risk drivers had to be offered some sort of remediation, regardless of their group assignment. For the purpose of this project the term "high-risk" driver was defined as any driver accumulating 13 or more additional points in a 1-year period after being assigned to a study group.

There were essentially two ways of dealing with high-risk drivers in the two control groups in question:

1. To remove those drivers judged as high-risk from both the experimental and control groups, so that comparisons between the two groups would not be distorted; or
2. to remediate the high-risk drivers in the control group and leave them in their appropriate group for analysis.

Both of these alternatives contain an element of bias concerning removing high-risk drivers from both the experimental and control groups. If drivers removed from the experimental group were essentially the same as those removed from the control group, then the drivers remaining in both groups would still be comparable. However, high-risk drivers in the experimental groups would be removed only when they accumulated points after remediation, while high-risk drivers in the control group would be removed when they accumulated the necessary points without remediation. Thus, drivers removed from the two groups might, and probably would, differ from one another, and if they were removed the remaining groups would not be comparable. In this case, the strength, and even the direction, of this group distortion would be unknown.

On the other hand, if high-risk drivers in the control group were remediated and left in their appropriate groups as suggested in alternative 2, some distortion in groups would still be present, since introducing remediation to this small group of control subjects would contaminate the control group in question. However, the direction of this group distortion would be known. The effect of alternative 2 would be to make proving a significant difference between experimental and control groups somewhat more difficult. Thus, any effect of the program that is found under this alternative would be known to truly represent the impact of the program.

According to the experience of researchers at the California Department of Motor Vehicles, the distortion produced by choosing the first alternative would actually be more than the impact of the treatment itself, while the known distortion in alternative 2 would be less than 5%. For these reasons, alternative 2 was chosen as the method for handling high-risk drivers in control groups. The driving records of control subjects who fell into the high-risk category, and who were not under suspension for conviction of an offense during the data collection period, were manually reviewed and the subjects given appropriate treatment, if deemed necessary, and left in the appropriate control group for analysis.

## RESULTS

The analysis of the first 6 months of data from the driver improvement program was completed and published in April 1981 (C. Lynn, 1980). At that time, information available on subsequent convictions and crashes was suitable only for chi square analysis of sample frequencies for paired experimental and control groups. Also, at that time it was noted that while drivers receiving the upper level treatments (the group interview only and the personal interview-driver improvement school combination) had better subsequent driving records than those not receiving these treatments, their counterparts receiving an advisory letter or a letter paired with a group interview did not have significantly better subsequent records than drivers not receiving these treatments. (It is recognized that group sizes at that time may have been too small to detect significant differences, since the sampling plan was based upon 12-month calculations.) However, these data were contaminated by two factors: (1) There were a few preexisting differences between randomly assigned experimental and control groups which occurred by chance; and (2) there were systematic differences between experimental and control groups which were inherent in the procedures creating those groups. These two confounding factors are discussed below.

Two preexisting demographic and driver history related differences between groups appeared after the subjects were selected. While all other pairs of experimental and control groups were essentially equivalent demographically and with regard to previous driving record, those groups involving the personal interview differed on age and number of previous convictions. In terms of age, the experimental group receiving treatments including the personal interview tended to be somewhat younger than the control group receiving treatments not including the personal interview. Basically,

there were more experimental group drivers in the 16-to-20-year old category and fewer in the 21-to-25-year category than in the control group. The two groups were essentially equal in the other age categories. Also, these experimental group drivers had experienced significantly more convictions during the year preceding their entrance into the study. Thus, by chance, the experimental group drivers seemed to be poorer drivers at the outset of the study, even before receiving treatment. This finding is in agreement with the discovery of age differences, in that it has been shown that younger drivers tend to have more accidents and receive more convictions than do their older counterparts.

Ordinarily, differences between groups with regard to pre-existing traits are considered to be biasing factors. Such is the case in this study; however, and quite fortunately, these differences interjected a conservative bias, in that it is more difficult to demonstrate the positive impact of treatment on a more "hard core" experimental group. In this case, even before applying statistical controls the study was biased against the treatment effect, thus ensuring the validity and probably the underestimation of those effects which were found.

The second set of group differences had to do with exposure to accidents and violations. As demonstrated in Table 3, the experimental groups receiving the group interview only and the personal interview had their licenses suspended for significantly more days than did their corresponding control groups. This difference can be explained by a procedural characteristic of the system. When negligent operators fail to attend an assigned treatment, and do not reschedule, their licenses are automatically suspended until they comply with their treatment assignment. This is the case with experimental group subjects, who are assigned to treatments, but not with the control group subjects, who are not.

Table 3

Differences in Exposure Rates for Group Interview  
and Personal Interview Study Groups, 6-Month Data

<u>Group Interview Only</u>		
<u>Incident Type</u>	<u>Experimental Group</u>	<u>Control Group</u>
Average major convictions	0.1510	0.2043
Average minor convictions	0.0719	0.0830
Average days of suspension	48	37
<u>Personal Interview</u>		
<u>Incident Type</u>	<u>Experimental Group</u>	<u>Control Group</u>
Average major convictions	0.1491	0.2394
Average days of suspension	88	77

Thus, the experimental group subjects were likely to have their licenses suspended more often and for longer periods of time than were control group subjects. Theoretically, the experimental subjects had less exposure time to collect traffic convictions and accidents than did their control group counterparts. While it is known that license suspension does not always have the desired effect of removing selected drivers from the traffic environment, it must be assumed that "failure to appear" suspensions and their corresponding reduction in exposure for the experimental groups only would mask the effects of treatment.

Because of these two sets of biasing group differences, steps were taken to control both for conviction frequency prior to entrance into the study and for differential exposure to accidents and convictions throughout the analysis of the first 12 months of observational data. This was done using the application of covariance analysis on all group comparisons and controlling for age, previous convictions, and number of days of posttreatment suspension.

The resultant analysis sought to answer several basic questions concerning Virginia's driver improvement program:

1. Is The Program Working? Do experimental group drivers who receive treatment have better subsequent driving records than control group drivers not receiving treatment? This question was answered by performing an analysis of variance on the following dependent variables: (a) number of posttreatment mandatory convictions, such as driving while intoxicated, manslaughter, etc.; (b) number of posttreatment major convictions, for which drivers are awarded 6 points; (c) number of posttreatment minor convictions, for which drivers are awarded 3 or 4 points; (d) total number of posttreatment convictions; (e) number of posttreatment accidents; and (f) where there were significant differences in accidents, numbers of fatal, injury, and property damage only accidents. As mentioned earlier, the effects of previous driving record and subsequent days of suspension were held constant during the analysis.
2. For Whom Does The Program Work Best? Do persons who did not incur subsequent convictions during the 12-month period ("survivors") differ from persons who did incur convictions ("non-survivors") on preexisting traits? Can a discrimination be made between survivors

and non-survivors and can subjects be predictively classified into these two groups based on demographic and driver related characteristics? Can these characteristics be used to predict which persons will be able to postpone, perhaps indefinitely, incurring additional convictions as a result of attending the program? These questions were answered using chi square analysis, discriminant function analysis, and regression analysis, respectively.

3. How Long Do Individual Treatments Keep Working?  
Is there a point at which the effects of treatments subside? Or do improvements in driving behavior have a more long-lasting effect? These questions are answered through the construction of "survivorship" curves and an analysis of the attributes of these curves.

These sets of questions were examined for each of the following treatments individually: (1) the advisory letter, (2) the advisory letter plus group interview, (3) the group interview only, and (4) the personal interview-driver improvement clinic.

#### Advisory Letter

As noted in the earlier project report, during the first 6 months of observation, there were no differences between the driving records of the experimental group subjects who received the advisory letter and the control group subjects who did not (C. Lynn, 1980). A more rigorous analysis was conducted on the 12-month data by applying analysis of variance and controlling for numbers of previous convictions, number of days of posttreatment suspension, and age.\* The results of these analyses appear in Table 4.

No significant differences were noted in mandatory convictions, major convictions, minor convictions, delay in subsequent offenses, or in accidents. With regard to survivorship, there were no more survivors in the experimental group than in the control group, which indicated that receiving an advisory letter did not increase the probability of remaining conviction free for a 12-month period (see Table 5). Since there were no differences between the advisory

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\*It should be noted that the impact of all of these covariates was significant with respect to all criterion variables except numbers of accidents, which was unrelated to number of days of posttreatment suspension.



Table 4

The Impact of the Advisory Letter Only on  
Accidents and Convictions

<u>Measure</u>	<u>F Value</u>	<u>Significance</u>
Number of Mandatory Convictions	0.65	N.S.
Number of Major Convictions	0.84	N.S.
Number of Minor Convictions	0.75	N.S.
Total Numbers of Convictions	1.55	N.S.
Number of Survivors	1.19	N.S.
Number of Days Between Treatment and Next Offense	0.00	N.S.
Number of Accidents	0.01	N.S.
Fatal	2.70	P < .11
Injury	0.06	N.S.
Property Damage	0.01	N.S.

Table 5

Survivorship: Advisory Letter Only

	<u>Experimental</u>	<u>Control</u>
Survivors	68.1% (3365)	68.9% (3335)
Non-survivors	31.9% (1517)	31.1% (1563)

$$X^2 = 0.75 \quad df = 1$$

Not Significant

letter experimental and control groups, the discriminability and predictability of treatment outcomes were not evaluated. For these reasons, further multivariate tests were not performed.

Finally, an analysis of program effectiveness over time was performed. To a certain extent, comparisons involving the number of days elapsed between the receipt of treatment and the first post-treatment offense are time related analyses; however, these comparisons do not evaluate the magnitude of the program's impact at more

than one posttreatment interval. Ideally, in order to examine program effectiveness over time, all posttreatment convictions and accidents would be examined with regard to their date of occurrence. Unfortunately, this type of data will not be available until the final 24-month report.

In order to partially address the question of duration of program impact, survival curves representing the number of persons remaining conviction free at various 5-day time periods were generated. The slopes of the survival curves for experimental and corresponding control groups were then compared. If the slopes of these curves were found to be different, then it would be concluded that the two groups were "losing" conviction free subjects at different rates. For instance, if the slope of the experimental group curve was less than that for the corresponding control group, it could be concluded that persons receiving treatment were incurring their first conviction at a slower rate than persons not receiving treatment and thus that the treatment was having a positive effect on survivorship.

Regressions were run on the corresponding treatment and no-treatment survivorship curves for each of the four treatment combinations under study on a quarterly and a semiannual basis. The lowest value of 0.84 for the coefficient of determination, or  $r^2$ , indicated that the regression explained 84% of the variance in the survivorship data. Outside of this one case, all other  $r^2$  values fell between 0.98 and 0.99.

The survivorship curve and resulting regression information for the advisory-letter-only groups appear in Figure 3. During the first quarter following treatment, the advisory-letter-only-treatment group incurred first convictions at a slower rate than its corresponding no-treatment group; (the slope for the experimental group was -28.53, while the slope for the control group was -31.81). This essentially means that for each 5-day period in that first quarter, the group receiving the advisory letter "lost" an average of 29 subjects to new convictions, whereas the control group lost about 32 subjects during the same period. This would indicate that, at least for the first three months, there was some slight impact associated with the receipt of the advisory letter. However, during the remainder of the year of observation, the slopes were essentially equal for both groups, which indicated that there was no further effect of the treatment upon survivorship.

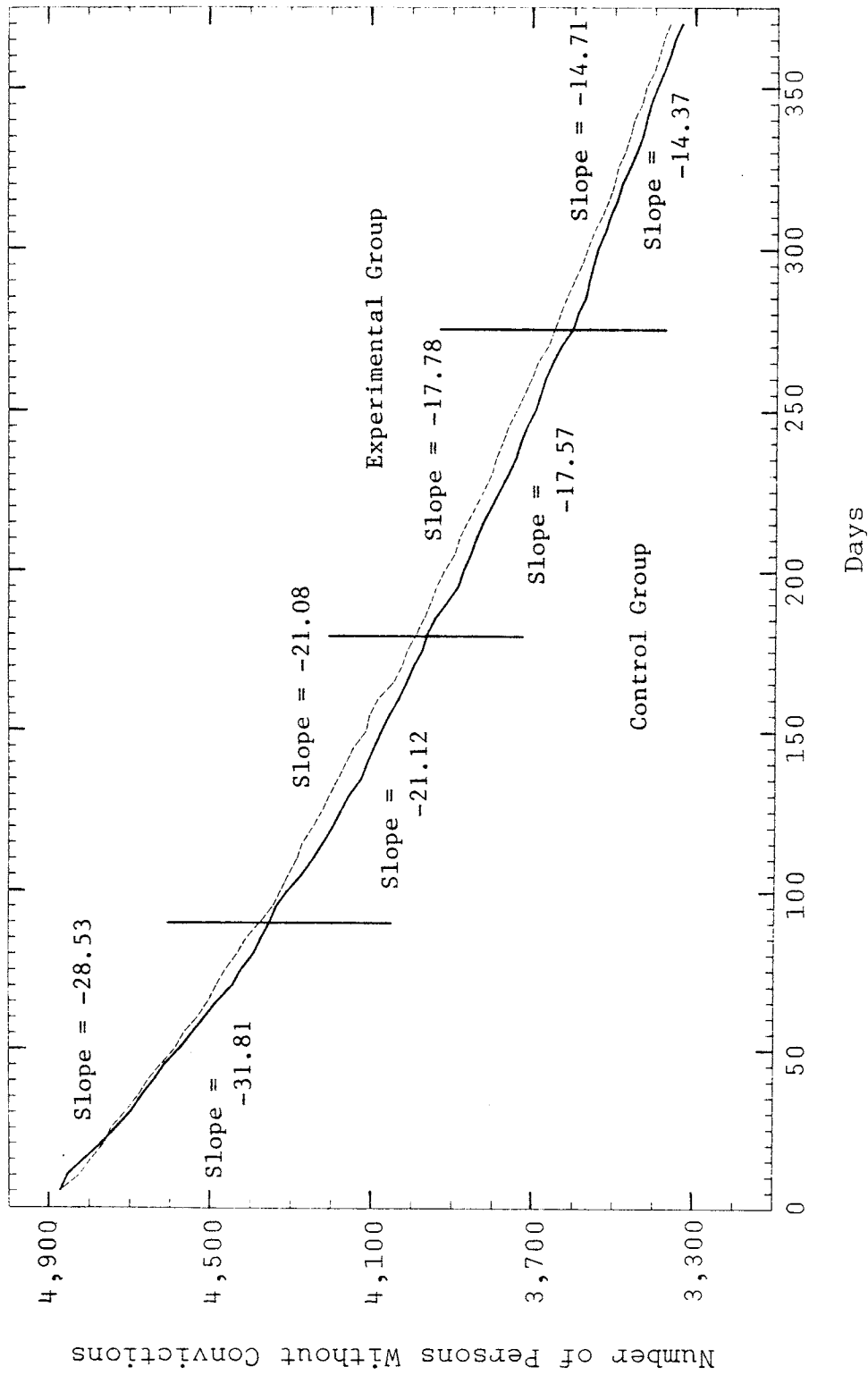


Figure 3. Advisory letter survival curves.

All in all, these results indicate that the current advisory letter is an ineffectual mode of treatment in the context of Virginia's driver improvement system. However, it should be noted that there is some disagreement regarding the adequacy of the sample size used for the advisory letter groups. There are two legitimate approaches to calculating sample sizes for studies of this type. The first, and the method chosen for this study, is to determine the smallest meaningful difference in accidents or convictions that need to be detected, and then to compute the sample size large enough to detect that difference. The second approach involves determining the group difference (in practical terms, the hypothesized reduction in accidents or convictions) needed to prove the particular countermeasure to be cost-effective, and then calculating the sample size necessary to detect that difference. Since the cost of the advisory letter is very low, the reduction in accidents necessary to prove it cost-effective is also very small. Sample sizes needed to detect such a small difference are very large, much larger than the sample sizes employed in this study. This issue is of particular interest with regard to the advisory letter, in that sample sizes calculated via the two methods are radically different. It is of lesser importance in relation to the other, more expensive treatments where the differences are not as great.

There are several reasons why the first method of calculating the sample size was chosen. First, there is quite a bit of dissension in the research community concerning the validity of current cost-benefit analysis procedures. The figures used to represent both societal costs of accidents and potential benefits of their reduction are not well documented and there is considerable disagreement over their appropriateness. There is also conceptual and philosophical dissension among researchers concerning the proper elements for setting societal costs. (As an extreme example, one school of thought goes so far as to hypothesize that benefits accrue from accidents in an overpopulated society.) All of this disagreement is reflected in the differences in the various cost figures assigned to accidents by different user groups; National Safety Council and National Highway Traffic Safety Administration figures differ by an order of magnitude in some cases. In order to set sample sizes using cost-effectiveness analysis, one set of cost figures must be chosen over the others, and in most cases NHTSA figures are chosen, not necessarily because they represent the best estimate of societal costs but because NHTSA funds many of the studies involved. The author believes that there is not enough agreement in the scientific community to justify the use of any one organization's figures, and that to do so would introduce arbitrary factors into the study.

Secondarily, it was believed that such large sample sizes were conceptually unnecessary, in that the detection of accident reductions as small as those detected in the cost-benefit approach was largely meaningless in the real world. In setting sample sizes, personnel from the Division of Motor Vehicles (the program's sponsors) were asked to determine the smallest accident reduction under which the program would still be considered by DMV, the public, and the legislature to be effective. Their smallest (and most meaningful) reduction was used to set sample sizes. Sample sizes necessary to detect the minute differences needed under the cost-benefit approach would have exceeded the total number of persons receiving an advisory letter during the entire 1-year period when subjects were assigned. Sample sizes chosen for this study are quite liberal, in that they were calculated on accident reductions, a very rare event, and then applied to convictions, a much less rare event, and then inflated 25% as a precautionary measure. For all of these reasons, the cost-benefit approach was rejected in favor of one based upon the detection of meaningful differences.

In summary, then, the results of this evaluation indicate that Virginia's advisory letter does not reduce the probability of subsequent accidents or convictions, and thus is not an effective first contact between the offender and the driver improvement program.

#### Advisory Letter Plus Group Interview

During the first 6 months of observation, it was noted not only that the advisory letter alone was largely ineffective in reducing subsequent accidents and convictions, but also that the letter teamed with the very effective group interview produced no significant effect. Upon initial inspection, it seemed as though some aspect of the receipt of an advisory letter was sabotaging the significant effect of the group interview. It was hypothesized that the strong and immediate impact signaled by the group interview alone was absent when it was preceded by the letter, and it was almost as though the subject realized that the driver improvement system was less "strict" than the old system of sanctions, since the driver had been involved with the program twice and still retained his license.

The results of the 6-month analysis are repeated in the 12-month data (see Table 6). Again, there were no significant differences between experimental and control group drivers with regard to mandatory convictions, major and minor convictions, survivorship, accidents or the time period between the receipt of treatment and the next offense, if any. As shown in Table 7, there were no significant differences in the distribution of survivors and non-survivors (those persons not accruing subsequent convictions during the 12-month period vs. those incurring a subsequent conviction). Again, since no major impact of treatment was noted in the criterion measures, additional multivariate analyses were omitted.

Table 6

The Impact of the Advisory Letter Plus the Group  
Interview on Accidents and Convictions

<u>Measure</u>	<u>F-Value</u>	<u>Significance</u>
Number of mandatory convictions	0.09	N.S.
Number of major convictions	0.29	N.S.
Number of minor convictions	2.36	N.S.
Total number of convictions	2.08	N.S.
Number of survivors	0.05	N.S.
Number of days between treatment and the next offense	0.05	N.S.
Number of accidents	0.53	N.S.
Fatal	0.96	N.S.
Injury	0.50	N.S.
Property Damage	0.47	N.S.

Table 7

Survivorship Advisory Letter Plus the Group Interview

	<u>Experimental</u>	<u>Control</u>
Survivors	6.29% (1459)	61.5% (1410)
Non-survivors	37.1% (860)	38.5% (883)

$$\chi^2 = 0.93 \quad df = 1$$

Not Significant

Finally, survivorship curves were examined for the group receiving both the group interview and an advisory letter and for the group receiving the advisory letter only (see Figure 4). During the first 6 months following treatment, the rates at which subjects incurred their first posttreatment violation were essentially the same (the slopes were about equal for both groups). During the third quarter, the treatment group performed somewhat better than the control; however, this trend was reversed during the fourth quarter, when the treatment group lost subjects more rapidly than the control.

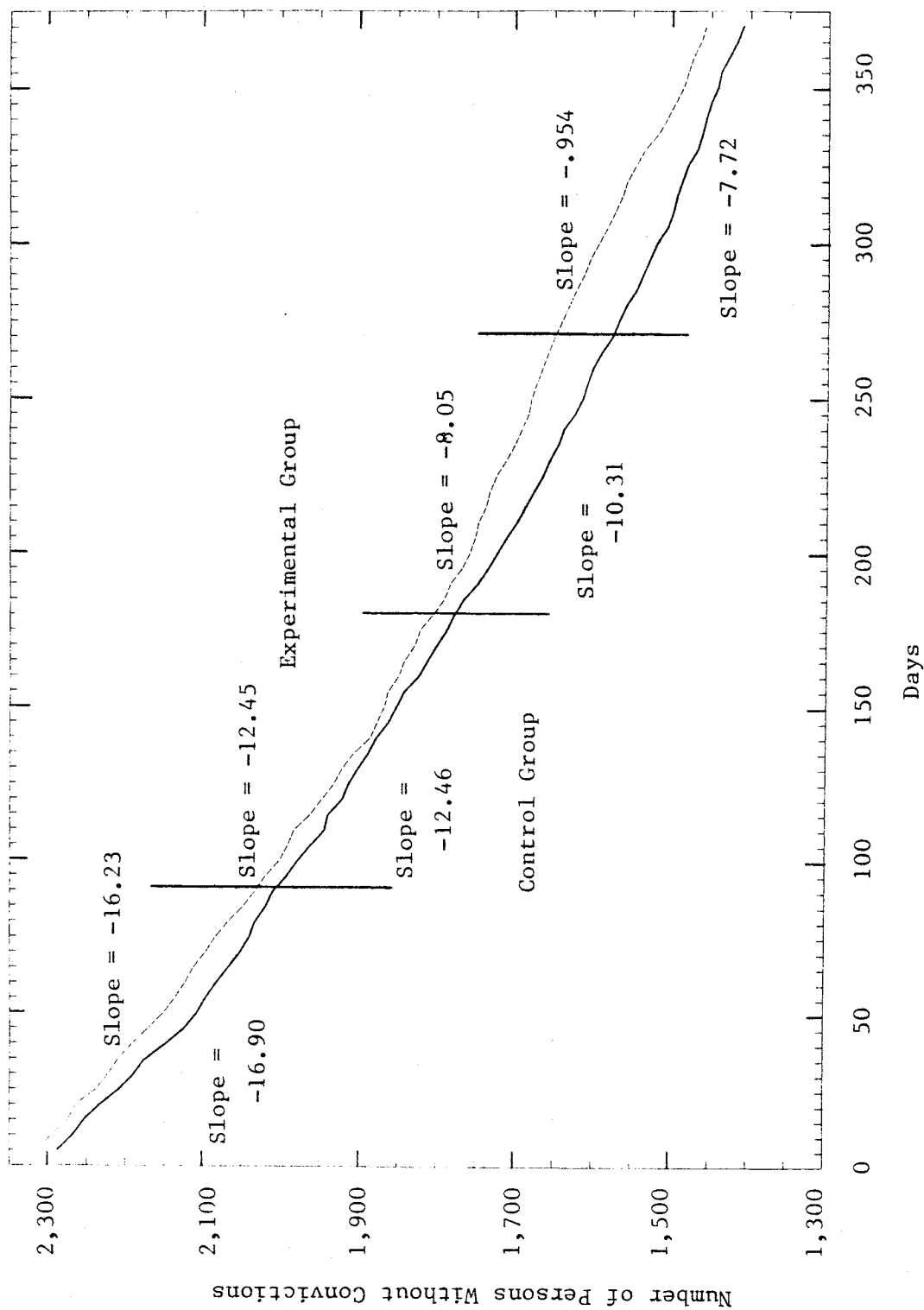


Figure 4. Advisory letter plus group interview survival curves.

From these data, it can be concluded that the advisory letter-group interview treatment combination was no more effective in reducing subsequent accidents and convictions than its corresponding no-treatment condition or than the advisory letter only.

### Group Interview

As noted in the first interim report, during the first 6 months of observation, subjects receiving a group interview had better subsequent driving records than those not receiving the treatment. This finding was confirmed after 12 months of observation (see Table 8). Experimental group drivers incurred significantly fewer major, minor, and total convictions than their control group counterparts. Also, as noted in both Tables 8 and 9, there were significantly more survivors, or persons not incurring subsequent violations during the 12 months of observations, in the experimental group receiving the group interview than in the control group not receiving treatment. No impact on accident involvement was noted except in the case of fatal accidents, where experimental and control group differences approached significance ( $p < .10$ ). It is hypothesized that this difference is largely due to the erratic behavior of rare events such as fatal accidents. (In any case, sample sizes for the study were not calculated such that differences in fatal accidents as a subgroup could be detected.)

Since significant differences were noted with regard to several of the performance measures, additional multivariate analyses were conducted. In order to determine whether study group and survivor group memberships were related to preexisting characteristics such as age, sex, race, and previous driving history, a discriminant function analysis and multiple classification analysis were run. It was found that study groups and survivorship groups could not be discriminated based upon this set of demographic and driving related variables. Additionally, regression analyses were conducted to determine whether group membership, number of posttreatment accidents or convictions, and delay time between treatment and the first subsequent conviction could be predicted based upon driver characteristics. Coefficients of determination were so low that it can be concluded that there was no systematic or predictive relationship between the criterion variables and characteristics of the subjects.

Lastly, survivorship curves for the group-interview-only subjects and the corresponding control group were examined (see Figure 5). For the first 6 months of the year, experimental group drivers incurred their first posttreatment violations at a slower rate than did control group drivers. During the third quarter, the slopes of the survival curves were equivalent; while in the fourth quarter, the experimental group loss rate exceeded the rate for the control group.



Table 8

The Impact of the Group Interview Only on Accidents  
and Convictions

<u>Measure</u>	<u>F-Value</u>	<u>Significance</u>
Number of Mandatory Convictions	2.29	N.S.
Number of Major Convictions	12.43	p < .001
Number of Minor Convictions	7.05	p < .01
Total Number of Convictions	15.36	p < .001
Number of Survivors	6.89	p < .01
Number of Days Between Treatment and the Next Offense	0.88	N.S.
Number of Accidents	0.35	N.S.
Fatal	2.90	p < .10
Injury	0.05	N.S.
Property Damage	0.74	N.S.

Table 9

Survivorship: Group Interview Only

	<u>Experimental</u>	<u>Control</u>
Survivors	67.9% (3136)	64.3% (2989)
Non-survivors	32.1% (1481)	35.7% (1659)

$$\chi^2 = 13.35 \quad df = 1$$

$$p < .001$$

It can be concluded from these observations that the group interview was instrumental in reducing subsequent convictions during the first 6 months of posttreatment experience. No impact was noted with regard to accident reduction.

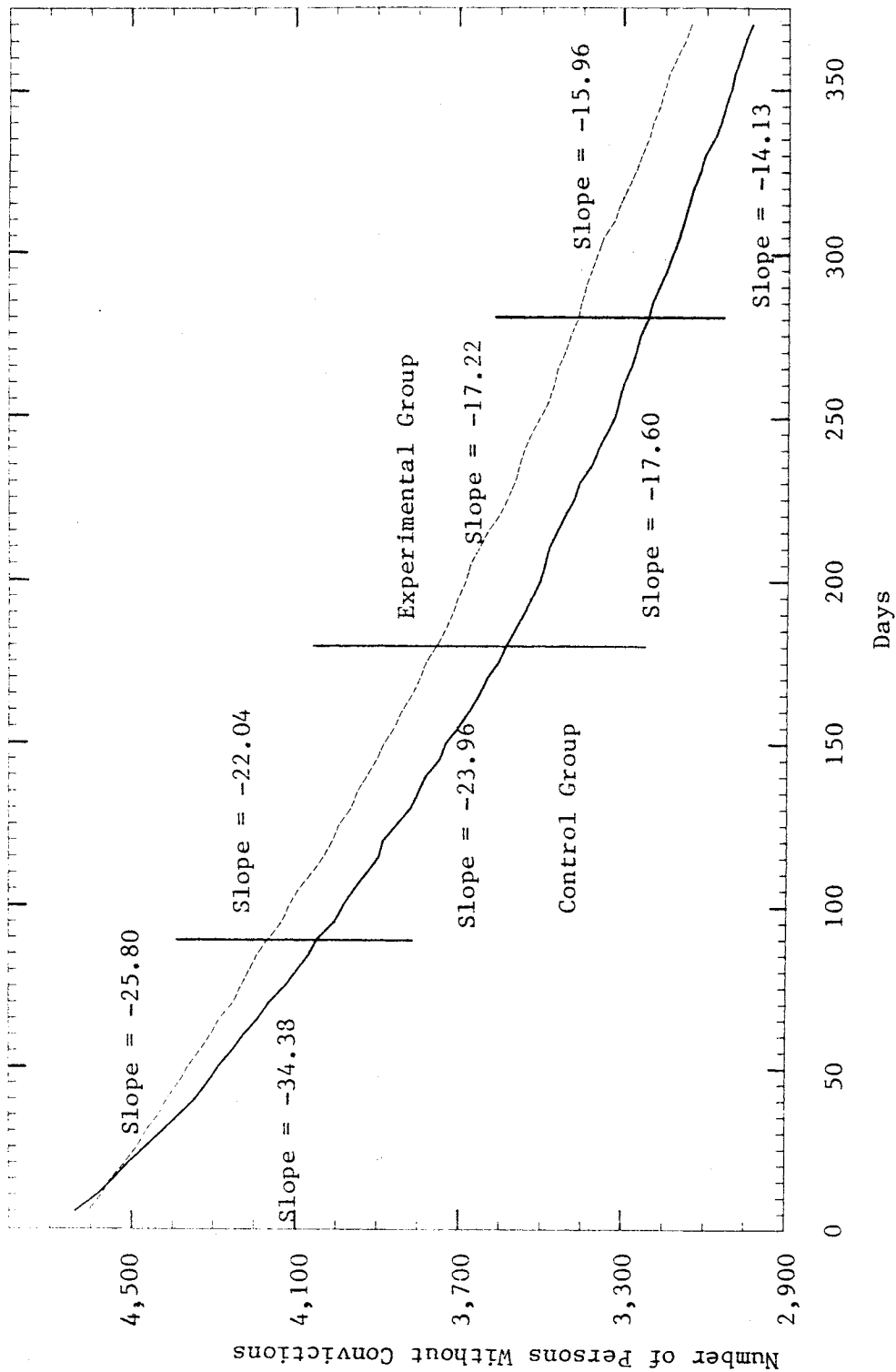


Figure 5. Group interview only survival curves.

Paradoxically, whereas the combination of the group interview and the advisory letter was unsuccessful in reducing subsequent convictions, the group interview alone was highly successful. From this it would appear that not only is the advisory letter an ineffective treatment, but it also emasculates the otherwise highly effective group interview. It was hypothesized in the earlier report that the receipt of two treatments without license forfeiture leads the offender to the realization that the driver improvement program is less punitive than the old system of sanctions, since under the old system two common convictions often led to suspension. In any event, it appears that the group interview may be a more effective first contact between the negligent operator and the driver improvement system than the advisory letter.

#### Personal Interview-Driver Improvement Clinic

In only one instance did study results after 12 months of observation differ from those noted after 6 months. In the case of the personal interview, after 6 months significant differences between the experimental and control groups were found for subsequent major convictions but not for subsequent minor convictions. In the 12-month analyses, however, experimental group drivers had significantly fewer minor convictions, while differences in major convictions merely approached significance (see Table 10). There are several explanations for the discrepancy, but the most likely one is that the 12-month analyses controlled for preexisting differences in both age and previous convictions whereas the 6-month analysis did not. This covariance analysis would be expected to produce larger differences in the personal interview group, where there were significant pretreatment discrepancies, than in other groups, where there were none. It is also expected that the covariance analysis produced a more accurate and more sensitive evaluation even in cases where there were no significant pretreatment biases. Subsequent convictions among experimental group drivers were delayed longer than those for the control group subjects. As with other treatment types, no differences were noted with regard to accidents.

Persons receiving treatment were also more likely to avoid additional convictions for the full 1-year period than were control group drivers (see Table 11), with 65.8% of the experimental subjects being survivors versus 56.0% for the control subjects.

Table 10

The Impact of the Personal Interview-Driver Improvement  
Clinic on Accidents and Convictions

<u>Measure</u>	<u>F-Value</u>	<u>Significance</u>
Number of Mandatory Convictions	2.04	N.S.
Number of Major Convictions	3.01	p < .10
Number of Minor Convictions	22.19	p < .001
Total Number of Convictions	23.80	p < .001
Number of Survivors	25.31	p < .001
Number of Days Between Treatment and the Next Offense	6.56	p < .01
Number of Accidents	1.27	N.S.
Fatal	0.06	N.S.
Injury	0.03	N.S.
Property Damage	1.50	N.S.

Table 11

Survivorship: Personal Interview-Driver Improvement Clinic

	<u>Experimental</u>	<u>Control</u>
Survivors	65.8% (1085)	56.0% (972)
Non-survivors	34.2% (565)	44.0% (765)

$$\chi^2 = 33.63 \quad df = 1$$

$$p < .001$$

Since significant differences between treatment and no-treatment groups were noted, a discriminant function analysis was conducted to determine whether both survivor and study group memberships were discriminable based upon preexisting variables. Very low level discriminability was found, which indicated that there was no systematic relationship between study group and survivorship group memberships and the characteristics of the individual

subjects. Regression analysis was then run to retest the predictive ability of the preexisting variables on survivorship, group membership, numbers of posttreatment accidents and convictions, and number of days between treatment and the next offense. The very low coefficients of determination indicated that there was very little predictive ability.

The personal interview-driver improvement clinic survivorship curves appear in Figure 6. During all four quarters of observation, the rate at which the experimental group incurred their first posttreatment convictions was lower than the same rate for the control group. This would indicate not only that the personal interview was effective in reducing the posttreatment conviction rate during the first 6 months, but also that that effect continued throughout the full year of observation. Clearly, the personal interview is the most long-lasting of the treatments studied.

In general, then, the personal interview-driver improvement clinic combination was successful in reducing subsequent convictions, especially minor convictions, and in increasing the period over which participants remained conviction free. However, the personal interview was not instrumental in reducing subsequent accidents.

#### All Treatment Combinations

As a final step in the analysis, all treatment groups were combined, as were all no-treatment control groups, and comparisons were made of their posttreatment driver histories. It was noted that the program as a whole was successful in reducing both major and minor convictions, as well as in increasing the time period over which subjects remained free of convictions (see Table 12). It should be noted that since there were more subjects in the advisory-letter-only and group-interview-only study groups than in this analysis these treatments are more heavily represented. It was also noted that, as a whole, experimental group drivers experienced fewer injury accidents than their control group counterparts. (Again, it should be noted that no impact was noted for accidents as a whole and that when numerous statistical tests are performed on the same data, 1 test in 20 will be significant by chance factors alone.) Subjects receiving treatment were more likely to be survivors or conviction free drivers (see Table 13). Again, there was no subject-treatment interaction, in that no one subgroup of subjects responded better to treatment than others.

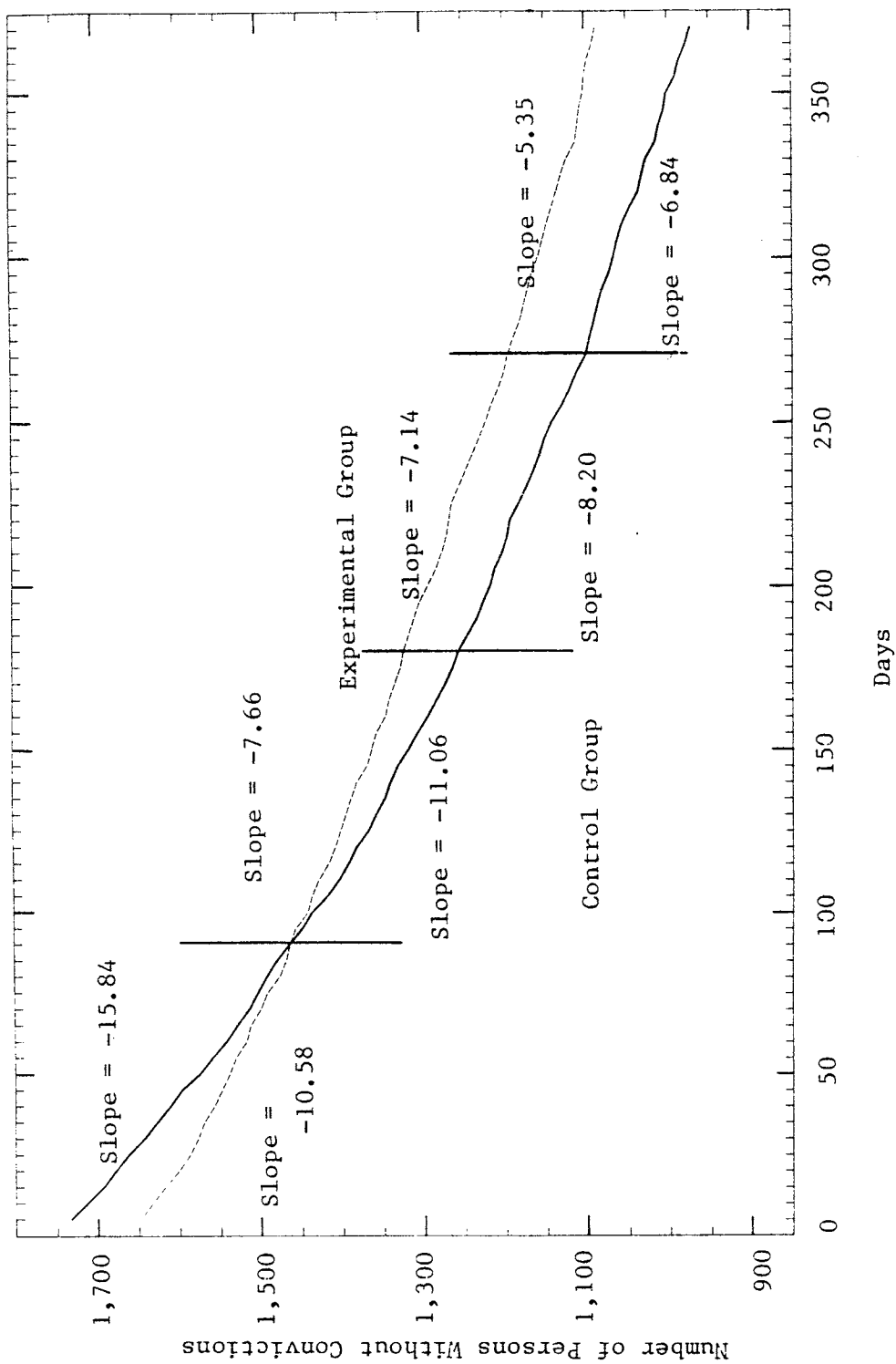


Figure 6. Personal interview survival curves.

Table 12

The Impact of Driver Improvement Treatment  
on Accidents and Convictions

<u>Measure</u>	<u>F-Value</u>	<u>Significance</u>
Number of Mandatory Convictions	1.16	N.S.
Number of Major Convictions	5.56	p < .001
Number of Minor Convictions	5.19	p < .001
Total Number of Convictions	11.28	p < .001
Number of Survivors	9.48	p < .001
Number of Days Between Treatment and the Next Offense	5.06	p < .001
Number of Accidents	1.22	N.S.
Fatal	1.54	N.S.
Injury	2.26	p < .05
Property Damage Only	0.99	N.S.

Table 13

Survivorship: All Treatments

	<u>Experimental</u>	<u>Control</u>
Survivors	67.2% (9045)	64.1% (8706)
Non-Survivors	32.8% (4423)	35.9% (4870)

$$\chi^2 = 27.41 \quad df = 1$$

$$p < .001$$

As seen in Figure 7, the rate at which treatment groups lost subjects to their first posttreatment convictions was lower than that for no-treatment groups during the first three quarters of observation. After that, however, the experimental group rates exceeded the control group rates. In general, it can be concluded that the total program was effective in reducing convictions, especially during the first 9 months following treatment.

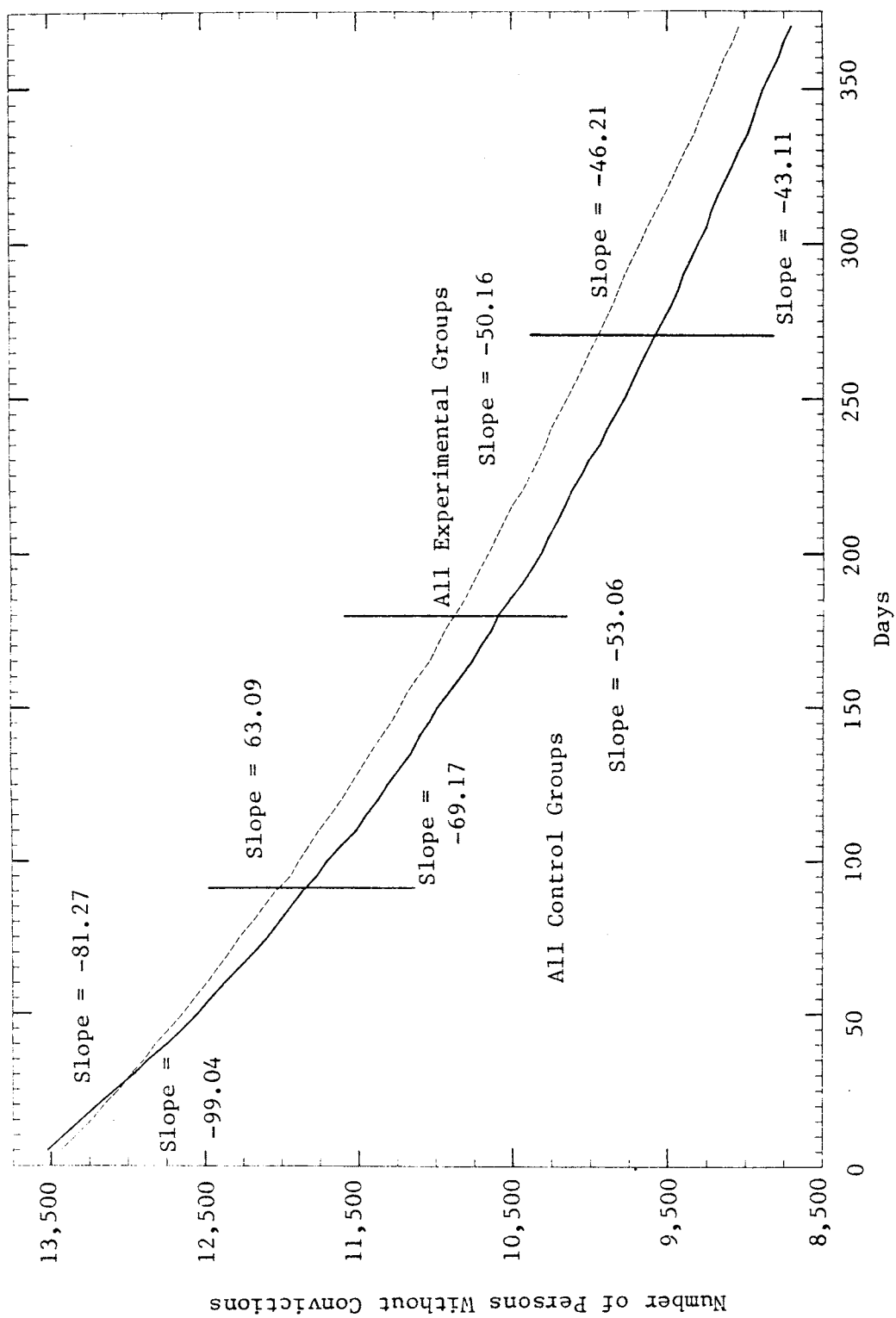


Figure 7. Treatment versus no-treatment survival curves.



## DISCUSSION

A number of conclusions can be drawn from the results of this study. First, a problem involving the advisory letter phase of treatment clearly exists. Not only is the letter largely ineffective in reducing subsequent accidents and convictions, but it also appears to emasculate the effect of the highly successful group interview when paired with that treatment. There are several possible explanations for this. There may be something about the content or the format of the letter that reduces its credibility with program participants. The timing of the letter, coming only after the expenditure of the driver's safe-driving points, may also reduce its impact. However, neither of these hypotheses explains why receipt of the advisory letter subverts the impact of the group interview. A more likely explanation, one that is consistent with the rest of the study findings, relates to the individual offender's perception of the system. As hypothesized in previous reports, an offender receiving an advisory letter requiring no action on his part may come to the realization that the current system is nowhere near as strict in terms of license suspension as the older system of sanctions. Persons receiving a subsequent group interview can then conclude that since they have had two separate occasions to participate in the driver improvement program and still retain their licenses, the program again is more lenient in terms of suspension. This would be especially true if the persons had expended their 5 safe driving points in advance of being contacted. They may have accrued as many as four convictions for a total of 11 or 12 points prior to even receiving an advisory letter. If it is assumed that the fear of suspension is the underlying power that drives the driver improvement apparatus, the removal of this threat may undermine the entire system. On the other hand, the individual receiving a group interview after two convictions may view the sacrifice of an evening of his time, and the subsequent emotional trauma of attending the meeting, as somewhat comparable to the older sanction of suspension — different but similar in severity. This could account for the effectiveness of one treatment and the ineffectiveness of the other. This is also consistent with other study findings, in that the further the offender has advanced in the system and the closer he is to license suspension, the more likely the treatment is to be effective.

There are several possible solutions to the advisory letter problem. Since the letter is relatively inexpensive to produce and distribute, retaining it with a different format or with different wording seems an attractive alternative. However, this does not deal with the problem of dilution of the suspension threat by multiple levels of treatment, including the advisory letter.

Since the group interview has been shown to be a more effective first contact with offenders, a more direct solution may be to do away with the advisory letter altogether, or to send it out as a warning when a person has expended all of his safe-driving points or as a congratulatory letter upon receipt of new safe driving points. It may also be appropriate to introduce the group interview at the 6-point level as an immediate intervention. In this way, the negligent operators become subject to one of the DMV's more potent treatments as soon as they enter the system and intervention in their driving problem is more likely. Also, this would help simplify the system. Of course, arrangements should be made to reevaluate any of these changes prior to their institution.

A second, and possibly more serious, problem detected through this evaluation is the driver improvement program's inability to affect the subsequent accident involvement of participants. Clearly, the system is designed to reduce convictions only inasmuch as they lead to accidents. Granted, it is extremely difficult to detect a change in accident related behavior because of the many factors independent of driving skill that go into causing crashes. However, attempts can be made to tailor the program to more directly affect accident causation on a specific level. For instance, the various interviews and clinics can be restructured to stress accident rather than conviction avoidance. However, this does not address the problem of lack of incentive. Negligent operators in the driver improvement program can be assumed to change their driving behavior to avoid the negative outcome of license suspension. Again, it is the fear of suspension that provides the impetus for change in driver behavior. However, persons do not lose their licenses because they've been involved in an accident; they lose them because they have incurred convictions on their record. The incentive here, then, is to avoid convictions rather than accidents, and in that the program is quite successful. (It may be argued that accident and conviction avoidance go hand in hand since faulty driving leads to both convictions and accidents, but this is not necessarily true. The bulk of convictions do not come from accident involvement but rather from such events as speed traps. In fact, most property damage only accidents do not result in convictions.) In order for the program to reduce subsequent accident involvement, it must provide some incentive to avoid accidents. One simple way of doing this would be to develop a scheme to award points for accident involvement similar to that currently used in California.

There are two distinct forms this scheme can take: (1) a system can be developed for awarding points based upon fault, or (2) a scheme can be developed for awarding points regardless of who was at fault. There are impressive and equally valid arguments

on both sides of the issue. From an intuitive point of view it would be fair for persons who theoretically cause an accident to receive more points than persons who are not at fault. However, this is a very difficult determination to make in most circumstances, and if the decision is made by the investigating officer, it has legal implications. Also, there is some evidence that whether the driver is at fault in an accident has very little to do with driving behavior, in that persons who are at fault in accidents have no more serious driving records than persons who are not at fault. McMillan (1975) concluded that "it is probably extremely rare for there to be a completely innocent victim (in an accident)." Sunier (1960) concluded that there is a close connection between the active and the passive agents in an accident, who were found not to differ on personal characteristics. Finally, Shaw (1965) found that the extent to which a driver could be considered blameworthy was largely irrelevant in distinguishing the accident repeater from the non-repeater. Since the point system is designed to identify the likely accident repeater for treatment, it appears that whether the driver was at fault does not have to be factored into the point structure to adequately diagnose repeat offenders. Clearly, the objectives of treatment are not only to teach drivers not to cause accidents, but also to teach them not to be involved. This can be most clearly illustrated by the classic example of the driver who is repeatedly rear ended because he stops at the mouth of an on ramp before merging into traffic. This is not "at fault" behavior; however, it does result in accidents, and it is behavior that has to be unlearned and can be corrected in treatment. The point system should identify this driver as a possible accident repeater as well as identifying the at-fault drivers, and should channel both into treatment.

Taking all of these facts into account, the following scheme is proposed. When an accident occurs, all of the parties involved receive 1 point. The officer investigating the accident does not decide who is at fault. Rather he decides whether a violation has been committed. If a conviction is forthcoming, the person receiving the conviction could be awarded an additional point for being technically at fault, if that is deemed an appropriate part of the point system. (Diagnostically, this extra point is unnecessary, but it does give an aura of fairness to the program.) Thus, the technically at-fault driver would receive 2 points for being in the accident and would also receive conviction points, while the not-at-fault driver would receive 1 point to identify him as a potential accident repeater. This system would make accident avoidance as well as conviction avoidance the goal of the driver improvement program. Again, a change of this type should be evaluated carefully to see if the desired outcome of increased accident avoidance is reached.

Along the same lines, another change in the point system is recommended. Given that the point system is designed to identify and diagnose negligent drivers, a direct correspondence between driving behavior and points awarded is necessary. The award of safe driving points for both accident and conviction free drivers distorts this diagnosis in several ways. First, drivers of the same ability may have differing numbers of safe driving points based on experience or attendance in a defensive driving course. Thus, one driver would be assigned to treatment while an equally needy driver would not. Also, as stated earlier, a person starting with the maximum of 5 safe driving points could accumulate 11 points before even receiving an advisory letter. This creates a situation where the driver can accumulate a number of convictions with impunity, which makes the driver improvement program appear extremely lenient. In light of the very small number of suspensions processed each year, the program does not need to appear any more lenient than it already does. Studies have shown that the award of safe driving points does not constitute a positive incentive for drivers, and, in that these bonus points distort diagnosis, it is recommended that safe driving points not be awarded in the future.

Finally, with regard to suspension, the small number of formal hearings and license suspensions and revocations resulting from the driver improvement program was quite surprising. In that fear of suspension is both a powerful incentive to program participants and the focus of the program, more effort should be invested in processing suspensions, if possible.

#### SUMMARY OF CONCLUSIONS AND RECOMMENDATIONS

In general, the results of this study indicate that the Virginia driver improvement program has been effective in reducing subsequent convictions among participants. The group interview and personal interview-driver improvement clinic have proven to be highly successful in modifying conviction behavior. The personal interview proved so successful that its impact continued to be felt during the entire 12-month observation period. On the other hand, the advisory letter had no effect on convictions and seemed to drastically reduce the impact of the group interview when the two treatments were paired. No treatment had any effect upon accident involvement.

It was concluded that the two most pressing issues facing the administrators of the driver improvement program are the need to improve the entry level treatment and the need to modify the program as a whole to more directly address accident avoidance. It was also felt that the diagnostic elements of the program should be made more rigorous and that the Division of Motor Vehicles should adopt a more active stance with regard to suspensions. Toward these ends, the following recommendations were put forth.

1. That the Division of Motor Vehicles seek legislation to allow administrators to alter the driver improvement program as they deem appropriate, without having to continually change the driver improvement statute.
2. That some modification of the entry level treatment program be made. While an attractive alternative is to simply change the style, content, or format of the letter, or to introduce it earlier in the system, this solution does not address the fact that the letter reduces the impact of other treatments. For this reason, it is recommended that the group interview replace the advisory letter as the entry level treatment, since the group interview has been shown to be a very effective first contact. Consideration should also be given to administering the group interview at the 6- rather than the 8-point level.
3. That individual treatment programs be modified to more directly and explicitly deal with the issue of accident avoidance as well as conviction avoidance.
4. That a minimum number of points be awarded to persons involved in an accident. Currently, since points are accumulated only through convictions, the main incentive in avoiding license suspension is to avoid subsequent convictions. To emphasize accident avoidance, point values should be assigned to accident involvement. To enhance the appearance of fairness, persons convicted of violations resulting from an accident would receive 2 points (plus those points associated with the conviction itself), while drivers not incurring convictions would receive 1 point to identify them as less probable accident repeaters.
5. That the Division terminate the procedure of awarding safe driving points to accident and conviction free drivers. It has been shown that these incentive point programs do not improve driving behavior and often cause distortion and reduce the diagnostic capability of the point system in identifying drivers who need treatment.

6. That the Division make every effort to increase the number of formal suspension hearings and increase the proportion of drivers eligible for suspension that receive a hearing. This is especially crucial, since fear of suspension is the most powerful incentive to change driving behavior.
7. That the Division of Motor Vehicles establish an ongoing monitoring system to evaluate program changes and overall impact on a continuous basis.

## ACKNOWLEDGEMENTS

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## APPENDIX A

§ 46.1-514.1

CODE OF VIRGINIA

§ 46.1-514.5

### ARTICLE 1.

#### *General Provisions.*

**§ 46.1-514.1. Short title.** — The short title of this chapter is the "Virginia Driver Improvement Act." (1974, c. 453.)

**§ 46.1-514.2. Purpose; educational and training programs; rules and regulations; appeals.** — (a) The purpose of the Virginia Driver Improvement Act is to improve and promote greater safety upon the highways and streets of this State; to improve the attitude and driving habits of drivers who accumulate traffic accident and motor vehicle conviction records; to determine whether certain drivers possess mental, physical or skill deficiencies which may affect their ability to safely operate a motor vehicle; to establish a Uniform Demerit Point System which will identify those drivers who are considered by the accumulation of demerit points to be habitually reckless or negligent drivers and frequent violators of the laws regulating the movement or operation of motor vehicles; to provide uniform educational and training programs for the rehabilitation of persons identified as habitually reckless or negligent drivers and frequent violators; and to suspend or revoke the license of those persons who do not respond to the rehabilitation programs.

(b) The educational and training programs shall be developed to improve the knowledge and skill of drivers in the operation of motor vehicles and to help eliminate their aggressive driving attitudes and habits or other driving problems through the media of advisory letters, group interviews, personal interviews and driver improvement clinics.

(c) The Commissioner shall, subject to the provisions of § 46.1-26, adopt those administrative rules and regulations which he deems necessary to carry out the provisions of this chapter. The Commissioner shall publish all administrative rules and/or regulations which he adopts to carry out the provisions of this chapter and shall furnish them to any person requesting them.

(d) Any person receiving an order of the Commissioner to suspend or revoke his driver's license or licensing privilege or to require attendance at a driver improvement clinic may, within thirty days from the date of such order file a petition of appeal in accordance with the provisions of § 46.1-437. (1974, c. 453.)

**§ 46.1-514.3. Designation of driver improvement analysts; analysts to conduct group interviews, personal interviews and driver improvement clinics.** — The Commissioner shall designate, appoint and empower such persons as he shall see fit to act for the Division as driver improvement analysts to examine and evaluate the driving records of the problem drivers and to conduct group interviews, personal interviews and driver improvement clinics. (1974, c. 453.)

**§ 46.1-514.4. Section 46.1-418 not applicable.** — The provisions of § 46.1-418 shall not apply to any person whose license or other privilege to operate a motor vehicle is suspended or revoked in accordance with the provisions of this chapter. (1974, c. 453.)

**§ 46.1-514.5. Persons included within scope of chapter.** — (a) Every person who possesses a driver's license issued by the Division regardless of whether such person is a resident or nonresident is included within the provisions of this chapter.

(b) Every resident of this State regardless of whether such person possesses a driver's license issued by the Division is included within the provisions of this chapter. (1974, c. 453.)

## ARTICLE 2.

*Uniform Demerit Point System; Safe Driving Points.*

**§ 46.1-514.6. Uniform Demerit Point System.** — (a) The Commissioner shall assign numerical point values to those convictions, or findings of not innocent in the case of a juvenile, which are required to be reported to the Division in accordance with § 46.1-413 for traffic offenses committed in violation of the laws of this State or any valid town, city or county ordinance paralleling and substantially conforming to such State law.

(b) The Commissioner shall assign numerical point values to those convictions received from any other state of the United States, the United States, the Dominion of Canada or its provinces or any territorial subdivision of such state or country, of an offense therein, which if committed in this State, would be required to be reported to the Division by § 46.1-413.

(c) Notwithstanding the provisions of (a) and (b) herein, no point assignment shall be made for those convictions that require the mandatory revocation or suspension of the license by the Commissioner.

(d) The Uniform Demerit Point System standard for rating convictions of traffic offenses shall be based on the severity of the offense and the potential hazardous exposure to other users of the highways and streets. The Commissioner shall designate the numerical point values assigned to convictions, or findings of not innocent in the case of a juvenile, on a graduated scale not to exceed six demerit points for any single conviction, except that no demerit points shall be assessed for any conviction when the court suspends the driver's license because of the conviction. The Commissioner shall develop point system assignments as follows:

(1) Serious traffic offenses such as reckless driving in violation of § 46.1-189, speeding twenty or more miles per hour above the posted speed limit, racing in violation of § 46.1-191 and other serious traffic offenses as the Commissioner may designate, shall be assigned six demerit points.

(2) Relatively serious traffic offenses such as failure to yield the right of way in violation of § 46.1-221, speeding between ten and nineteen miles per hour above the posted speed limit, following too close in violation of § 46.1-213, failure to stop when entering a highway in violation of § 46.1-190 (j) and other relatively serious traffic offenses as the Commissioner may designate, shall be assigned four demerit points.

(3) Traffic offenses of a less serious nature such as improper driving in violation of § 46.1-192.2, speeding between one and nine miles per hour above the posted speed limit, improper passing in violation of § 46.1-208, failure to obey a highway sign in violation of § 46.1-173 and other offenses of a less serious nature as the Commissioner may designate, shall be assigned three demerit points.

(e) In order to ensure that demerit points are assessed in a uniform manner, the following method will be used effective January one, nineteen hundred seventy-five to assess demerit points:

For any conviction where the offense was committed on or subsequent to January one, nineteen hundred seventy-five, demerit points will be assessed according to the point values contained in (d) (1), (d) (2) and (d) (3) herein and any other point value assignments which are designated by the Commissioner.

(f) When a person is convicted of two or more traffic offenses committed on a single occasion, such person shall be assessed points for one offense only and if the offenses involved have different point values, such person shall be assessed points for the offense having the greater point value. (1974, c. 453.)

APPENDIX B

CHAPTER 288

*An Act to amend the Code of Virginia by adding in Chapter 6.1 of Title 46.1 a section numbered 46.1-514.21, relating to evaluation of the driver improvement program.*

[H 608]

Approved 3/25/78

Be it enacted by the General Assembly of Virginia:

1. That Chapter 6.1 of Title 46.1 of the Code of Virginia is amended by adding a section numbered 46.1-514.21 as follows:

*§ 46.1-514.21. Notwithstanding the provisions of §§ 46.1-514.9 through 46.1-514.12, the Commissioner may waive the action usually taken by the Division in order to conduct an evaluation of effectiveness of the driver improvement program. This evaluation, when conducted, shall be performed in accordance with generally accepted scientific principles such as the establishment of control groups and comparisons of driving records between groups receiving the treatment and the control groups.*

2. That this act shall cease to be effective on and after July one, nineteen hundred seventy-nine.

\_\_\_\_\_  
President of the Senate

\_\_\_\_\_  
Speaker of the House of Delegates

Approved:

\_\_\_\_\_  
Governor



## APPENDIX C

### SAMPLE SIZE CALCULATIONS

The necessary sample sizes for the study groups were computed using the formula

$$n = \frac{(Z_{1-a} + Z_{1-b})^2 pq}{d^2} \left[ \frac{Nn}{N + n} \right],$$

where

$Z_{1-a}$  = normal value corresponding to the alpha level (i.e., the probability of finding significant results when there are none);

$Z_{1-b}$  = normal value corresponding to the beta level (i.e., the probability of finding no significant results when there are some);

$p$  = probability of occurrence of the event ultimately being measured (in this case, accidents or convictions);

$q$  =  $(1 - p)$ ;

$d$  = the minimum detectable change in the event being measured;

$N$  = population size;

$n$  = sample size; and

$\left[ \frac{Nn}{N + n} \right]$  = the correction for a finite population size.

In these calculations, the following assumptions were made:

1. The alpha level was set at 0.05 (meaning that there is less than a 5% chance of finding significant results when in fact there are none).
2. The beta level was set at 0.20 (meaning that there is less than a 20% change of finding no significant results when in fact there are some).

3. The minimum difference that could be detected in this evaluation was a 10% difference in rate. For example, if the accident rate for an experimental group was 15%, a difference as small as 1.5% could be detected.