The Effectiveness of Short Individual Driver Improvement Sessions*

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The study's main purpose was to evaluate the effects of short individual driver improvement sessions on the subsequent driving records of negligent operators. Also investigated was the influence of age on the effectiveness of these sessions. The report concludes that (a) those requested to attend a hearing had significantly fewer convictions during the first 12-month period following such a scheduled hearing than did a control group; (b) these effects appeared to shrink during the second 12-month period to a point where differences between experimental and control groups were not statistically significant; (c) accident frequency did not appear to be reduced as a consequence of the driver improvement hearing; (d) the hearing did not reduce the point count of the negligent operator during a one-year follow-up to that of the "average" driver; and (e) the effects of the hearing were constant at all age levels. Among other topics discussed are the effects of attending a hearing vs merely receiving a hearing notice and certain limitations in the findings and research design.

•THE control of licensed drivers is currently one of the major problems confronting driver licensing officials. Although the majority of the licensed driving population does not accrue a large number of violations and accidents, there is at any given point in time a small proportion of the population who violate traffic laws and are involved in accidents to such an extent that they become at least a potential hazard to the safety and welfare of the general public and themselves. Any means of effectively controlling the behavior of such drivers would represent an important contribution to the prevention of needless death and injury.

It is the basic objective of the Driver Improvement Program of the California Department of Motor Vehicles to improve the driving habits and performance of such drivers who, because of traffic law violations and/or accident involvement, are legally classed as negligent operators. According to statute, a prima facie negligent operator is any person whose driving record shows a violation point count of 4 or more points in 12 months, 6 or more points in 24 months, or 8 or more points in 36 months.

One means presently employed to obtain the objective of post-licensing control is the Negligent Operator Informal Hearing, whereby the negligent operator is informed of his record and is allowed to state his case. Such hearings generally consist of a 30-40 minute contact between the subject and a departmental driver improvement analyst, during which the subject's record is discussed, and various suggestions for improvement are made by the analyst. Consistent with the legalistic, social-control orientation of the program, the hearing process is not therapeutically structured, and, as a consequence, the driver improvement analyst (i.e., hearing referee) does not follow or receive training in contemporary counseling techniques. All driver improvement analysts must have at least one year's experience as drivers license examiners and, in addition, all are

*The original report contained extensive statistical analyses and related material in appendixes which are not included herein.

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given specialized in-service training in the form of a six-week course. College training is not required, except for those entering the drivers license examiner series with no prior experience. This latter group must have a B. A. (in any field) from a recognized college or university.

The analyst's basic role is to impress upon the subject the importance of safe driving habits and of the ramifications of continued traffic law violations and accidents. At the end of the hearing, the analyst informs the subject that a final decision as to the department's course of action will be made by headquarters, and the subject is then dismissed. His case is later reviewed, and he is informed by mail of the action taken. Typically, an initial action is to place the subject on probation for a minimum of one year, which was by far the predominant form of action taken in this study. Occasionally, however, the record and prognosis for improvement indicates that a more severe action is needed, such as suspension or revocation of the subject's driving privilege. In arriving at these determinations, the analyst considers the various aspects of the driver's record, including the severity of the record and the attitude of the subject during the hearing. The action indicated by the hearing analyst is in the form of a recommendation to headquarters, with the final decision resting with a review analyst. In the present study, the review analyst concurred with the initial recommendation in 84.2 percent of the cases sampled. One hundred and forty-one cases (14.6 percent) received a less severe action than was recommended, compared with 12 cases (1.2 percent) who received a more stringent action. However, the rationale and effectiveness of the review changes were not within the scope of the research design employed in this study and could therefore not be evaluated. Further particulars regarding the hearing process and philosophy can be obtained elsewhere (6).

Insofar as possible, the present analysis was limited to first-time negligent operators—those who had no prior contact with the department beyond receipt of a warning letter. The reasons for this restriction are twofold. First, the obtainment of a control group of "hard-core" negligent drivers presented administrative and technical difficulties which could not be surmounted. Second, the precedures for handling the hard-core negligent operator are varied, depending on the history of departmental actions with respect to each hard-core subject. An evaluation of the effects of a single hearing session on such a heterogeneous group was not felt to be a particularly meaningful or fruitful enterprise.

It is the department's philosophy that progress in the area of post-licensing control can be best achieved in conjuction with a thorough, ongoing, empirical validation of present and future programs. Only in this way can the effectiveness of a given program be evaluated with any degree of confidence and scientific rigor. Such evaluation allows one to progress toward the development of optimally effective programs by precipitating refinements in current programs and suggesting or exploring alternative approaches. Toward this end, the current study is just one of several efforts by this department in the general area of individual and group driver improvement techniques.

The authors feel that the findings reported here represent a definite contribution to the area of post-licensing driver control, especially when viewed in relation to the department's (and others') overall research efforts in this area. It will be our specific purpose here to describe and evaluate the effectiveness of the department's individual hearing process in reducing the accident and citation frequencies of negligent drivers. By so doing, we do not wish to imply that an empirically demonstrable citation or accident reduction is the only meaningful criteron for evaluating the negligent operator hearing process. The program may have subtle indirect effects which would not be reflected in an analysis of comparative accident and citation frequencies. One such effect might be the influence which the existence of the program has on the overall public. In other words, mere awareness of the existence of a driver control program may have a deterrent effect on the overall driving population, but not on drivers so extreme as to be classified as negligent operators.

In addition, the hearing program serves another important function—that of providing due process in connection with the social control obligation of the Department of Motor Vehicles. Since the department has a role in maintaining adherence to traffic laws, it is sometimes necessary that these prescribed norms be reinforced by with-

drawing the driving privilege of habitual violators without regard to the improvement possibilities of the program. The hearing process fulfills the due process requirement inherent in taking restrictive legal action against any citizen of the state. In short, it is the department's objective that the Driver Improvement Program achieve the maximum in rehabilitative power while fulfilling the important requirement of due process.

METHODOLOGY

Research Design

As originally conceived, the experimental design of the study was a conventional two-group model, in which one group received some form of "treatment" (experimental condition) and a comparison group received no "treatment" (control condition). In the present study, the "treatment" or experimental condition was the department's conventional negligent operator hearing; the group of subjects who initially received such hearings will henceforth be referred to as hearing subjects or the "hearing group," whereas those who did not receive an initial hearing will be referred to as control subjects or the "control group." Since the subjects were not assigned to the groups at random, it was necessary to adopt a matching design, in which relevant data on a large number of subjects from each group were collected and used in obtaining two groups who matched each other with respect to all available relevant variables (age, sex, marital status, prior accidents and prior traffic citations). These were selected on the basis of known driver record relationships within the overall driving population. The extent of these relationships within the California negligent operator population was not known at the time of the study. By controlling the effects of the relevant variables, significant differences between the two groups on subsequent measures (dependent variables) can be attributed to the effects of the treatment (i.e., hearing). In this study, the dependent variable used to evaluate the effects of the hearing was the subsequent two-year driving performance of the groups, as measured by departmental records of reported accidents and traffic citations (abstracts of court convictions).

This description should suffice to give the reader a general understanding of the research design employed in the present study. A more detailed and comprehensive discussion of the more salient design aspects of the study will be reserved for later.

Data Collection Procedures

Of the approximately 18,000 first contact hearings held in 1961, a pool of 9,000 subjects was identified as having received a hearing during the first eight months of that year. Slightly more than 3,500 of these cases were selected for coding and subsequent keypunching. (All 9,000 cases were initially screened on the basis of the accumulated number of negligent operator points in the 12-month period prior to hearing, and those with point counts in excess of five were eliminated. This was done because no control subjects with point counts in excess of five were available for comparison.) A control or comparison group of 2,000 subjects who also had no prior departmental contact and who had attained negligent operator status during the summer of 1961 was also identified for subsequent coding and keypunching.

Between February and September of 1964, the relevant data were transcribed from driver records to code sheets for the selected subjects. The coding was done in accordance with instructions delineated in a coding manual designed specifically for this study. For the hearing group, the coded categories can be conveniently divided into four general areas: (a) biographical and miscellaneous data available from drivers license application; (b) biographical and miscellaneous data from hearing form (mileage, type of action, etc.); (c) prior (to hearing) 12-month driver record; and (d) subsequent two-year driving record (e.g., court abstracts of traffic citations and reported accidents).

The items for the control group can be categorized in a similar manner with the exception of the data from the hearing form, which did not exist for the control subjects. Instead of a hearing date, the control assignment date was coded for the control group. It is around these dates—the hearing date and control assignment date—that the one-year prior and two-year subsequent driving record was resolved.

The coding of the two groups (hearing and control) was consistently alternated throughout the data collection period, in order that any temporally related biases or distortions in coding precision and judgement would be counterbalanced (i. e., affect each group equally). In addition, the coders were alternated between groups in order to counterbalance any bias which might have resulted from differing sets and idiosyncracies among the coders.

All coders were thoroughly trained, and a complete accuracy check was made on all coding during the initial phases of the project. Afterwards, systematic spot checks were made on each coder's accuracy to insure against coding deterioration over time. Based on an ongoing tally of spot checks, the coding error on the driver record categories was estimated at about one percent—one error per every 100 coded driver record categories. Errors on the non-driving record portion were almost nonexistent. Although some driver categories were coded incorrectly more often than others, there was no evidence of differential coding errors between the control and hearing group.

In order to further verify the coding accuracy, a correlational analysis was done by coder and item on a random subsample of 50 records—25 control and 25 hearing cases. The codes assigned to each of the items by the raters were correlated with those assigned by two professional analysts. High correlations between coder and judge reflect a high degree of accuracy and concurrence, whereas low correlations are indicative of inaccuracy and non-concurrence. The overall accuracy was very high for most item categories. However, three items were considered deficient in accuracy and therefore removed from any analytical interpretation.

After all cases had been coded, the data were scanned, punched into card format and subsequently converted to tape. Detailed machine edit checks were performed and any detected inconsistencies were corrected. At this point, the matching of subjects was ready to commence.

Derivation of Matched Samples

In order to determine whether any selective bias had occurred in the assignment of subjects to the two groups, preliminary distributions were derived on the entire coded pool with respect to the variables of age, sex, marital status and prior driver record. Statistically significant differences were found between the hearing and control group on each variable, indicating that the two groups were not representative of the same underlying population of negligent operators and, therefore, not comparable. It was therefore necessary, as mentioned earlier, to resort to a matching design, in which the coded pool of subjects comprising the two groups were matched on age, sex, marital status, and number of accidents and countable traffic citations in the 12-month period prior to hearing or control assignment, including Failure to Appear stops (FTA's) for moving violations. FTA's represent traffic citations for which the cited subject has not appeared in court as promised.

In addition, it was decided to split the experimental group by season of hearing assignment—non-summer (January-May) and summer (June-August). This was done to control for the possible influence of seasonal effects, since the control subjects could only be drawn from the summer months. Because the possible influence of season would only be controlled for the summer-hearing matches, it was decided to utilize them in deriving the matched control sample, and afterwards to match as many non-summer hearing subjects to the matched control sample as possible. Thus, the goal was to match two hearing subjects—one summer and one non-summer—to each control subject. In this way, the summer and non-summer hearing subjects would be matched to each other, as well as to the control subjects. (This goal was not quite attained since a non-summer hearing match could not be found for 35 of the control subjects.)

Since the number of exact matches derived through the collating procedure was disappointingly small, it was decided to relax the matching requirements slightly by allowing subjects to match who were no more than two years apart in age, but who were identical with respect to all other matching variables. Of the summer matches, 322 were exact, 179 inexact; of the non-summer matches, 304 were exact, 162 inexact.

A total of 501 summer hearing matches and 466 non-summer hearing matches was

derived from the original pool of 3,500 hearing subjects and 2,000 control subjects. Since only the 501 control subjects who matched summer hearing subjects were used in deriving the non-summer hearing sample, the control subjects in each group are largely the same subjects, minus the difference between them. In other words, all 466 of the controls who matched a non-summer hearing subject are also among the 501 who matched the summer hearing subjects. Further details concerning the matching outcome are given later.

Limitations of Data

Before commencing with a description and analysis of the results, certain limiting facets of the data should be made clear since they place qualifications on the inferences one can draw from the findings. Although implicit in any study of this nature, it should be emphasized that the driving performance criteria are those events (accidents and citations) contained on the departmental record. It is known that many accidents and violations are not reported to the department. In order to generalize the treatment effects (hearing) from departmental records to actual driving behavior, one must assume the events to be linearly correlated. This assumption permeates all studies of this type and to the authors' knowledge, no data are available to either support or refute it. However, it is felt that the assumption is a reasonable one.

A second limiting factor concerns the generality of the data. The hearing sample, strictly speaking, is only representative of the population of hearings from which it was drawn—in this case, 1961 hearings. Consequently, any changes in the hearing program subsequent to sample selection would not be reflected in the sample selected for this study. Also, to the extent that the matching procedure produced samples which were not representative of the overall negligent operator population on variables such as age or prior driver record, any generalization of sample findings to such a population must rest on the assumption that the hearing effects are relatively homogeneous with respect to these attributes.

A further assumption which must be made is that there was no appreciable difference between the two groups with respect to the types and temporal spacing of citations in the prior 12-month driver record period. If either of these assumptions were false, subsequent driver record comparisons could be distorted. A similar assumption concerns the exposure and socioeconomic variables. Since these variables were not available for the control group, the groups could not be matched on them. It seems reasonable to assume, however, that these variables would vary at random between the two groups, especially in view of the between-group homogeneity introduced by the matching procedure. This is particularly true for variables such as mileage and occupation, which were not available at the time of hearing-control assignment, and which therefore could not be utilized selectively in assigning subjects to the treatment groups.

Probably the most trenchant limitation to the study concerns the confounding of the hearing and control conditions as a result of differential treatment in the subsequent-to-assignment periods. Since a subsequent hands-off policy could not be adopted at the time this study was initiated, subjects in both the control and hearing groups were scheduled for hearings if they continued to violate subsequent to the initial assignment. Although not optimal, such a procedure is permissible from a design standpoint as long as the criterion for assigning subsequent treatments is the same for both groups. In the present study, however, the control group subjects were assigned subsequent hearings much more readily than the initial hearing group subjects, thereby confounding the initial distinction between the two groups. Fortunately, the direction of the findings was such that relatively definitive conclusions could still be derived concerning the intital treatment effects. This factor will be explored more comprehensively in the next section.

RESULTS AND FINDINGS

Adequacy of Matching and Composition of Groups

Hearing-Control Comparisons—It will be recalled that the hearing and control groups were matched on five variables—age, sex, marital status, number of prior accidents,

and number of prior citations. In all cases the matching between control and hearing subjects was very satisfactory. In fact, an exact match was obtained on all variables except age, where only negligible deviations occurred. Statistical tests indicated all age differences to be the result of chance occurrences.

Since area of residence within the state is known to be related to driving record, the reader may question why this was not included as a matching variable. To avoid further reduction of sample size, the authors decided to allow area to vary between groups on the assumption that its effects would be randomized. This proved to be the case. Statistical tests of significance on the area distribution proved to be non-significant, as were differences in the overall citation and accident rates derived from the various areas of the state. Thus it can be concluded that the hearing and control groups represent the same underlying population with regard to all three area variables.

Summer and Non-Summer Hearing Comparisons—It was pointed out earlier that the hearing group was subdivided into two categories—those who had received their initial hearing during the summer months, and those who had received their initial hearing in the non-summer months. This dichotomy was necessitated by the fact that control subjects were available for only the summer months, and the effects of season of hearing upon subsequent driving record were not known. Because the non-summer hearings and summer controls were selected from different periods of time, the possibility that subtle differences might exist in the composition of the two groups must be considered. Such differences could occur either as a result of true differences between groups who met the negligent operator definition at different times of the year or they could be artifacts produced by subtle differences in selection and scheduling procedures. Also it must be remembered that the prior and subsequent records between two groups selected at different points in time are not perfectly parallel. It was therefore decided to split the hearing group by season and to obtain the maximum number of matched control subjects for each hearing group. Such a procedure has the advantage of allowing for separate analyses within season of hearing, thereby isolating any distortion due to seasonal variation.

Before undertaking any hearing-control (treatment) comparisons, the summer and non-summer hearing groups were cross-compared with respect to a number of biographical and driver record variables. Statistical tests of significance indicated a significant difference between the hearing groups with respect to only one variable, area citation index. Thus, we have some evidence in support of our initial speculation—namely, that summer and non-summer hearing cases may not represent an identical population of negligent drivers. Such a speculation is also consistent with the fact that the subsequent driving records of the two groups were consistently different. Whether such differences were a result of pre-existing differences in the composition of the groups or temporal changes in the hearing structure and its effectiveness could not be determined from this analysis. Fortunately, comparisons between the hearing and control group within season of hearing produced the same decision with respect to hearing effectiveness, so that the issue has no bearing on the basic hypotheses which the study was designed to test.

Since the matching controls for the non-summer hearings were selected entirely from the summer months, subsequent driver record comparisons between these two groups (non-summer hearing vs summer controls) may be slightly biased. Driver record analyses with respect to treatment effects should therefore be considered more accurate in the case of the summer hearing vs summer control comparisons. In the pages which follow, emphasis will be placed on the latter comparisons, especially when evaluating the magnitude of the hearing effects.

Composition of Hearings by Response Categories—Based on the outcome of the hearing notice, the hearing groups can be divided into three "response" categories: (a) appeared—subjects who attended the hearing; (b) non-appeared—subjects who did not attend and whose notices were not returned "unclaimed"; and (c) notice returned—subjects who did not attend and whose notices were returned "unclaimed," indicating that these subjects were possibly no longer residing at the address. The breakdown of the summer and non-summer hearing groups in terms of the delineated response categories

showed that, of the summer hearings group, 420 appeared, 57 did not appear, and 24 had the notices returned. Of the non-summer group, 417 appeared, 38 did not appear, and 11 had the notices returned.

Although a detailed analysis of the comparative driving records of these groups will be reserved for later, some general comments concerning the implications which these response categories have with respect to the present research design should be included at this point. In order to construct valid treatment comparisons from which unbiased inferences may be derived, it is necessary that the entire hearing group ("appeared," "did not appear" and "notice returned") be included when making comparisons with the control group. Such a procedure is dictated by the fact that those persons who did appear may differ in a number of respects from those who did not appear, and that these differences may be related to driving record. Therefore, if we were to limit our treatment comparisons to the appeared group, the outcome could be biased since the control group could not be given the same "opportunity" to have subjects (potential "did not appear" and "notice returned" cases) removed.

Subsequent Driving Record by Type of Treatment

In this section, the effects of the hearing program are evaluated with respect to the following driving record criteria variables:

1. Citation reduction in the first and second year subsequent to hearing-control assignment;
2. Accident reduction in the first and second year subsequent to hearing-control assignment;
3. Months till first citation subsequent to hearing-control assignment;
4. Months till first accident subsequent to hearing-control assignment; and
5. Net months till first incident (accident or violation) subsequent to hearing-control assignment.

All comparisons will be between the control and hearing groups by age within season of hearing. Because there were so few females in the sample, the analyses will be confined to the combined male and female samples.

Subsequent Citation Frequency—The mean number of subsequent citations by treatment (control vs hearing) within season of hearing are as follows:

Season of Hearing	First-Year Record		Second-Year Record	
	Hearing	Control	Hearing	Control
Summer	1.06	1.43	0.93	1.01
Non-summer	1.21	1.44	0.99	1.02

In the first subsequent year, both hearing groups have significantly fewer citations than their respective control groups. Thus, one can be confident that the hearings resulted in a real (non-chance) reduction in citation frequency during the initial one-year subsequent period. The most dramatic difference occurred with respect to the summer hearing comparison, and for reasons discussed earlier, the latter comparisons probably represent the more accurate reflection of the magnitude of the hearing effects. In terms of percentage difference, the summer hearing group had 35 percent fewer citations in the first subsequent year than did their control counterparts. For the non-summer hearing, a citation reduction of approximately 19 percent was noted. An analysis of the second-year differences indicates that the hearing effects shrank dramatically over time. In fact, the second-year citation differences, though favoring the hearing groups, are not statistically significant. In other words, one cannot be confident that the latter differences are anything but random sampling variations (chance). For reasons which will be discussed later, there are grounds for suspecting that the full hearing effects have been suppressed, especially with regard to second subsequent year comparisons.

In order to determine whether the hearings were differentially effective by age, the treatment comparisons with respect to subsequent citation frequency were split into four age groups. Although there appeared to be a tendency for the hearing effects (on citation frequency) to decrease with increasing age, statistical tests of the age by treatment interaction did not reach significance. We therefore cannot conclude that the various age groups are affected in different degrees by the hearing process.

Subsequent Accident Frequency—The comparative performance of the hearing and control groups relative to subsequent accident frequency are as follows:

Season of Hearing	First-Year Record		Second-Year Record	
	Hearing	Control	Hearing	Control
Summer	0.25	0.24	0.18	0.16
Non-summer	0.25	0.23	0.19	0.17

It is immediately apparent from these figures that the situation relative to subsequent accident frequency deviates considerably from the outcome encountered with respect to subsequent citations. With accidents, there is practically no difference between any of the hearing and control accident means, and in each instance the direction of the difference is in favor of the controls. Statistical tests confirm that all differences reflected in the subsequent accident data can be attributed to chance. Although there is a theoretical possibility that contaminations in the research design could have suppressed subsequent accident reduction, the findings relative to subsequent accident frequency present a disappointing picture of the accident-reducing power of the individual hearing program, at least as the program was constituted in 1961.

Months Till First Citation—Another method of evaluating hearing effectiveness is to determine whether the hearing delayed the onset of violation behavior in the subsequent-to-treatment period. This was accomplished by coding the number of months till first subsequent citation for each subject in the study and employing statistical tests of significance on the tabulated results. The mean number of months till first citation by treatment and season for all those who received at least one countable citation in the two year subsequent driver record period are as follows:

Season of Hearing	Mean No. of Months Till First Citation (Citation-Free Drivers Excluded)	
	Hearing	Control
Summer	7.31	5.57
Non-summer	7.08	5.50

It can be seen from these data that all comparisons favored the hearing group. A statistical test indicated that the hearing did produce a real delay in the receipt of initial citations in the subsequent-to-treatment period. The amount of the delay for the summer comparisons was 1.7 months. Thus, coupled with the earlier finding that the hearing process reduced the subsequent frequency of traffic citations, it can also be concluded that the hearing process produced an initial delay in committing traffic violations.

Months Till First Accident—An identical analysis was performed with respect to delay of an initial subsequent accident. The descriptive details are as follows:

Season of Hearing	Mean No. of Months Till First Subsequent Accident (Accident-Free Drivers Excluded)	
	Hearing	Control
Summer	9.70	8.18
Non-summer	9.92	8.27

Although the comparison tends slightly to favor the hearing group, it is not statistically significant and could therefore be attributed to chance variation. Thus, it cannot be concluded with any assurance that the hearing delayed the occurrence of reported accidents. In direct contrast to subsequent citation incidence, there is no evidence that the hearing either delayed or reduced the occurrence of accidents in the subsequent driver record intervals.

Net Months Till First Incident—Because some of the hearing subjects received initial suspensions and also had a greater likelihood of receiving subsequent suspensions, it was anticipated that the months of net temporal exposure for the hearing group would be less than that of the control group. A statistical test on the net temporal exposure variable indicated that the hearing group did, in fact, have significantly less net temporal exposure subsequent to treatment than did the control. Because of this, one could speculate that the comparative reduction in subsequent citation frquency for the hearing group could possibly be attributed to reduction in exposure. To test this hypothesis, the authors formulated a "net months till first incident" variable; this variable consisted of the number of months accrued by each subject between treatment assignment and his initial subsequent incident (citation or accident) minus the number of months each subject was suspended during this interval. The means for all those with at least one incident on their record are as follows:

Season of Hearing	Mean No. of Net Months Till First Subsequent Incident (Incident-Free Drivers Excluded)	
	Hearing	Control
Summer	6.61	5.15
Non-summer	6.34	5.12

As can be seen, all comparisons still favor the hearing groups and the margin of the differences does not appear to have been appreciably affected by holding net temporal exposure constant. A statistical test on the data indicated that the hearing resulted in significant net delay in the occurrence of initial incidents during the 24-month period subsequent to hearing-control assignment. It would appear from this analysis that the hearing effects are relatively independent of net temporal exposure. To the authors' knowledge, this is the first successful attempt at analyzing this factor.

Subsequent Driving Record by Response Category

In order to make driving record comparisons within the hearing group by response category, the summer and non-summer hearings were combined into one group and their prior and subsequent driving records tabulated for analysis. The analysis of variance

employed on the prior driver comparisons indicated that the three response categories did not differ significantly with regard to prior number of citations and accidents:

Variable	Response Category		
	Appeared	Did Not Appear	Notice Returned
Accidents	0.26	0.18	0.20
Citations	1.14	0.91	1.34

Inspection of the first subsequent year's driving records of the groups proved to be considerably more interesting. In every case, the non-appeared group was found to have the superior subsequent record, with the appeared group occupying the intermediate position. Although statistical tests of the citation and accident differences failed to reach significance, the direction and consistency of the differences is notable. This finding cannot be interpreted as indicating that the overall hearing process (including receipt of hearing notice) is ineffective, but it does serve to reinforce suspicions concerning the effects of the hearing contact per se. In other words, one is tempted to speculate that the primary source of the hearing program's effectiveness lies in communicating to the subject the possibility of impending action, rather than the face-to-face interaction with the hearing analyst. Another possibility is suggested in the response category by net temporal exposure comparisons:

Variable	Response Category		
	Appeared	Did Not Appear	Notice Returned
First subsequent year	11.26	9.43	10.34
Second subsequent year	11.15	9.97	11.00

The numbers represent the average number of months which the subjects in each response category could legally drive during the subsequent 12-month interval. From this it can be seen that the driving of the "did not appear" group was the most restricted in the first one-year subsequent-to-hearing period. In other words, this group was suspended more than any other, thereby reducing their legally permissible temporal exposure below that of the other two groups. Statistical tests indicate that these exposure differences are real and not attributable to chance sampling variations. If the "did not appear" group actually adhered to their suspensions by not driving, then this reduced exposure could have decreased their incidence of accidents and citations subsequent to hearing. Another possibility is that the more severe action (increased suspensions) imposed on the "did not appear" group resulted in increased improvement. Finally, it can be argued that persons who refuse to appear for their scheduled hearings are, at the very outset, different from those who do appear, and that any subsequent difference could be a function of these pre-existing differences. More will be said about these response category findings in the next section.

DISCUSSION AND CONCLUSIONS

This section will relate certain of the study's research design qualifications to the findings in the previous section and will provide an overall interpretation of the data. Based on this interpretation, recommendations will be made as to future research needs and the direction of program development relative to control of the negligent driver.

Biased Nature of Driver Record Comparisons

It will be recalled from the discussion of research design and methodology that the two groups—hearing and control—were not treated equivalently in the period subsequent to treatment assignment. In verification of this statement, the following table shows that the control group received subsequent actions to a much greater extent than did the hearing group:

Season of Hearing	No. of Actions per Negligent Operator Point in Subsequent Two-Year Period		Months Till First Action	
	Hearing	Control	Hearing	Control
Summer	0.13	0.18	10.7	5.1
Non-summer	0.14	0.19	9.5	5.2

Thus, not only was the control group "treated" with hearings subsequent to control assignment, but they were treated to a much greater extent than was the hearing group. Apparently, the fact that the control group did not initially receive an action made it more likely that they would be called in for a hearing or action soon after violating in the subsequent period. In the hearing group, on the other hand, the mere presence of a hearing form on record delayed scheduling for subsequent hearings, despite continued traffic involvements.

The implications of this factor are very far-reaching and important. Since the control group received more actions per subsequent negligent operator point count than the hearing group, any effect which the initial hearing may have had would be obscured by the greater likelihood of a subsequent hearing for the control group. In effect, then, each time this happened the "pureness" of the control group was lessened. The question thus raised is how the findings are affected by such a bias. In view of the fact that the hearing group still received significantly fewer citations than the control group, we can be even more confident that the hearing reduced the number of subsequent citations. In other words, had the control group remained "pure," larger differences would probably have occurred. However, the accident comparisons were not significant, and we have no way of knowing whether a reduction would have occurred had the groups received equal treatment subsequent to hearing or control assignment. One thing seems certain, however; if the hearing process does have an effect on accident frequency, it must be a small one, detectable by only the purest of measures.

Comparison With Overall Driving Population

How does the subsequent one-year record of the hearing group compare with that of a similarly stratified (re. age, sex, marital status composition) group selected from the overall population of California drivers? By utilizing data from the 1964 California Driver Record Study (8), and adjusting it to the age, sex, and marital status composition of the hearing group, mean accident and citation rates of 0.14 and 0.58, respectively, are derived. Compared with the respective values for the combined summer-non-summer hearing group of 0.25 and 1.13, it can be seen that the hearing group still has almost twice the accident and citation incidence subsequent to hearing as does the adjusted overall California driving population. A previous study by Coppin and Van Oldenbeek (7) produced a similar finding. It is quite apparent, then, that the hearing program does not reduce the count level of negligent drivers to the average level of all California drivers.

Effects of Attending a Hearing

As indicated by our results relative to the response category comparisons, there is no evidence that it is the face-to-face contact with a hearing analyst which results in an improved subsequent record. In view of the finding that the "did not appear" group had a record that was as good, if not better, than the "appeared" group, it is quite possible that a large amount of the hearing program's effectiveness is a result of receiving the hearing notice.

However, it should not be inferred from this speculation that a mere warning letter would necessarily have the same impact as the overall individual negligent operator hearing program. In the hearing notice, the subject is requested, with threat of penalty, to appear at the hearing. A mere warning letter, no matter how severe, may not carry the same impact as notice of a legally constituted hearing scheduled on the subject's behalf.

Generality of Findings

As mentioned earlier, the matching restrictions rather severely reduced the heterogeneity of the hearing group with respect to variables such as age and prior driver record. In fact, not one subject in the study had a prior negligent operator count in excess of 5 points at the time of selection. The question is then raised as to how far the findings can be extrapolated beyond the population which is represented by the matched samples. The authors are of the opinion that the generality of the findings has not been excessively restricted, except possibly with respect to sex and prior driver record. In other words, since negligent operators at the more extreme count levels (6, 7 and above) were not included in the samples, and females were included to a very limited degree, the findings cannot be legitimately generalized beyond male 5-count subjects, unless one assumes that the effects of the hearing are homogeneous with respect to prior count level and sex.

Unresolved Issues

The authors wish to emphasize that the present research sheds little light on the actual mode by which the individual hearing is effective. Does the hearing actually change the attitudinal process of drivers, or is the improvement merely a function of the authoritarian aspects of departmental contact? What types of psychological makeups are most affected and least affected by this hearing process? Can any of the hearing effects be attributed to possible subsequent exposure reduction? Is the improvement which results from the present program the maximum which can be expected from any treatment? To what extent have the improvement and its duration been suppressed by the previously mentioned design contaminations? Are some analysts more effective than others in bringing about improvement? Are some types of actions more effective than others? Why does the hearing reduce violations, but (apparently) not accidents? Can accident frequency be reduced by any treatment short of completely removing the negligent driver from our roads and highways?

The answers to these questions can only be derived through more extensive research in the area of the negligent driver and treatment techniques. Toward resolving at least some of these issues, the department has undertaken a massive, rigorously controlled, multi-treatment research project in which subjects are assigned at random to one of a variety of control and experimental conditions. This multi-treatment study, combined with the results from the present study and a former study on group techniques (5), should go a long way toward developing an empirically based approach to the effective treatment and rehabilitation of the negligent driver.

SUMMARY

Analysis of the subsequent-to-treatment driving records of the hearing and control subjects indicated the following:

1. The hearing groups had significantly fewer citations in the first subsequent year than the control groups. No significant differences were found between the hearing and control groups with respect to citation frequency in the second subsequent year. However, because of research design contaminations which could theoretically have suppressed the full hearing effects—especially in the second year—it could not be concluded with assurance that the hearing effects diminished completely after one year.

2. No significant differences were found between the hearing and control groups with respect to subsequent accident frequency in either the first or second subsequent year. However, because of the previously mentioned design contaminations, it could not be concluded with complete assurance that the hearing program was completely ineffective as a reducer of accidents.

3. The various age groups did not appear to have been differentially affected by the hearing process. In other words, there was no evidence that the hearing was more (or less) effective with one age group than another.

4. The hearing significantly delayed receipt of intital citations in the subsequent-to-treatment interval, and the magnitude of the delay was too large to be attributed to the reduced net temporal exposure of the hearing groups. No significant difference in the onset of subsequent initial accident involvement was noted between the groups. In other words, it could not be concluded that the hearing program delayed the receipt of initial accidents in the subsequent-to-treatment period.

5. The subsequent citation and accident frequencies of those who attended their scheduled hearing were not significantly different from the frequencies of those who did not attend. Thus, it could not be concluded that it was the face-to-face contact with the hearing analyst which effected the subsequent reduction in citations for the hearing group.

6. The subsequent accident and citation frequencies of the combined hearing groups were approximately twice as high as a similarly stratified sample from the overall California driving population, indicating that the hearing did not reduce the point count of the negligent driver to the state average.

The authors propose the following speculative interpretation of the above findings:

1. The overall individual hearing program is an effective means of reducing subsequent citation frequency, but the effects probably diminish with time.

2. The overall hearing program, at least as constituted in 1961, either does not reduce subsequent accident involvement or reduces it to such a small extent that the reduction can only be detected with the purest of research designs, employed on very large samples.

3. Receipt of the hearing notice and/or initial action probably constitutes an important source of the hearing program effects, apart from face-to-face interaction with a hearing analyst.

Recognizing the study's limitations and findings, the department has initiated one comprehensive, multi-treatment approach, in which a variety of rehabilitative techniques will be compared within the structure of a definitive research design. This latter approach should provide answers to many of the unanswered questions raised by the presently completed study.

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