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ABSTRACT

This study was concerned with--(1) the conditions of transfer of training, (2) armed forces research on the use of synthetic training devices in the learning of complex skills, (3) armed forces research on the use of training devices in learning judgmental skills, and (4) training devices used in driver education. A section with a summary, conclusions, and general recommendations is included. (BD)

DRIVER EDUCATION and DRIVING SIMULATORS

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I. INTRODUCTION

In recent years there has been an increasing interest in the use of training devices in driver education, especially the Drivotrainer and the Auto Trainer.* This interest has been intensified by the growing population of the United States which, along with an increasing number of two- and three-car families, is putting more and more automobiles on the road. It is imperative that those who are learning to drive receive the best instruction possible. Time and cost limitations associated with dual-control car instruction may well preclude many eligible high school students from participating fully in the program. The manifest need to provide driver education to more students has led to the development of driving simulators.

Several advantages are suggested for synthetic training devices in driver education. A larger number of students can receive the benefits of the program than is possible when the devices are not used. The per-pupil cost of instruction can be substantially reduced. Fewer teachers are needed. Students can learn the necessary complex skills as effectively as in the dual-control car; and beyond that, they can develop better attitudes. The students can learn appropriate responses to emergency situations without the attendant hazards of actually being in a car on the road. In addition, electric scoring devices can provide for the immediate detection of student errors by the teacher.

There have been questions raised, too, concerning the use of synthetic training devices. The basic question pertaining to the use of any training simulator is—Is it valid? That is, does training on the device increase skill in the task for which the training is appropriate? Presumably, under guidance, learning will occur on the device, but unless this learning is transferred so that the student can perform the actual task effectively, the simulator will lack validity. The basic principle of simulator validity is directly related to the concept of transfer of training. The question—Does it work?—is best answered on the basis of the kind and extent of transfer of training which occurs.

Also, how elaborate do the devices have to be to provide the student with useful learning experiences? Is it essential that the panel display of the device duplicate the dashboard of the car? Would directional signals, for example, add significantly enough to the student's experience to justify their cost? Can a simple, inexpensive device do the same things for the learner as a complex and costly one? The underlying thought here is: When, in the construction of a training device, is its optimum validity achieved so that additional features would add nothing except cost and complexity to the device?

Other appropriate questions pertain to maintenance. How costly are

* For descriptions of these devices, see Section V.

major repairs? How much of the driver education teacher's time is taken up with minor repairs on the devices—time which might otherwise be spent in lesson preparation or student counseling. How many of the devices are likely to be inoperative on any given day? In other words, how much maintenance, costly in time and manpower, is required to keep the devices operative?

Will the devices be dated? That is, will the task of driving in 10 years be significantly different from what it now is? Manipulating the controls of a car with a standard gear shift is a different task from manipulating those of a car with an automatic or push-button transmission. Modifications of existing devices have been necessary to keep pace with the changed task. An important question then is—Will additional changes in the automobile make the existing training devices obsolete or create long delays for design modifications?

How is the role of the driver education teacher affected by the use of training devices? Will his teaching skills be as necessary when more and more of the learning experience occurs through the use of motion pictures? Does the teacher become a clerk, noting errors recorded on a mechanical scoring device? With the use of devices, is there enough time for the teacher to build a student's confidence and eliminate his anxiety? Does the instructor lose the opportunity to "teach on the spot"—for example, by telling the student to pull over and then analyzing mistakes he has made.

These are some of the questions which have been asked; there are undoubtedly others. The most crucial question is, however—To what extent and with what expenditure of time and money does training on a simulator transfer to the real task? If transfer does not occur, there can be no justification for the use of such training devices. This is true even if the actual task is exceedingly hazardous; for, if what is learned on the device is of no value to, or interferes with, the performance of the actual task, then such learning may as well not have occurred.

Purpose

The purpose of this study is to assist those responsible for driver educaiton to evaluate the effectiveness of training devices which simulate driving. It is hoped that the analysis will prove helpful to those engaged in the manufacture and distribution of such devices. Finally, students of the psychological processes of transfer of training may also find this study of value.

Not all the questions raised can be answered in this study. Some would require a fully developed experimental project; others could be answered only after a long period of time. Information pertaining to the basic question of the transfer value of simulators, however, can be presented. As will become clear throughout this study, there is a great deal of confusion regarding the transfer concept. Nevertheless, experimental evidence dealing with the transfer of training should prove useful in evaluating existing simulator devices used in driver education.

I. INTRODUCTION

Finally, it should be especially noted that this study is concerned with *synthetic training devices,* and not with *experimental devices.* Synthetic training devices are designed to train large numbers of individuals; experimental devices are usually single-place units designed to reproduce as faithfully as possible the complete actual task. For example, in driver education, the purpose of synthetic training devices is to assist in teaching large numbers of students; experimental devices, on the other hand, are used as research tools to determine the effects of drugs, alcohol, fatigue, and other factors on driving behavior.

Organization

Section II of the study is concerned with the conditions of transfer of training. It briefly presents these conditions as they have been derived from laboratory experiments, and provides the basic theory of transfer of training.

Section III deals with Armed Forces research on the use of synthetic training devices in the learning of complex skills. Special attention is given to devices which are designed to assist pilots in learning to fly. The applicable conditions of transfer are noted.

Section IV presents Armed Forces research on the use of training devices in learning judgmental skills. There is a partial answer to the question, "Do judgmental skills transfer?" Again, the applicable conditions of transfer are noted.

Section V pertains specifically to training devices used in driver education. A review of the evaluation studies is presented, and the conditions of transfer are applied.

Section VI contains a summary, conclusions, and general recommendations.

This is the fifth study in a series of projects endorsed by the Research Advisory Committee of the NEA National Commission on Safety Education. The first three studies were completed in collaboration with the NEA Research Division, and the material for the fourth study was developed by Ralph H. Ojemann and his assistants.*

The material for the present study was developed by James H. Fox. While it was in draft form, the following members of the Commission's Research Advisory Committee met and critically reviewed the material: Herbert S. Conrad, research coordinator, Division of Higher Education, U.S. Office of Education; Max R. Goodson, dean, School of Education, Boston University;

* National Education Association, National Commission on Safety Education. *Research Needs in Traffic Safety Education.* Washington, D. C.: the Commission, 1956. 20 p.
National Education Association, National Commission on Safety Education. *A Critical Analysis of Driver Education Research.* Washington, D. C.: the Commission, 1957. 60 p.
National Education Association, National Commission on Safety Education. *"Tips" and "Cues": How Experienced Teachers Develop Good Traffic Citizens.* Washington, D. C.: the Commission, 1958. 66 p.
National Education Association, National Commission on Safety Education. *Tests and Evaluation Methods Used in Driver and Safety Education.* Washington, D. C.: the Commission, 1959. 48 p.

and Clara G. Stratemeyer, supervisor of elementary schools, Montgomery County Board of Education, Rockville, Maryland.

In addition, the manuscript was reviewed and commented upon through correspondence by other members of the Research Advisory Committee, as follows: M. R. Trabue (chairman), formerly dean, College of Education, The Pennsylvania State University; J. H. Mathewson, assistant director, Institute of Transportation and Traffic Engineering, University of California; Ralph H. Ojemann, professor, Psychology and Parent Education, State University of Iowa; Dean M. Schweickhard, commissioner of education, Minnesota State Department of Education; and J. Wayne Wrightstone, director of educational research, Board of Education of the City of New York.

Changes were made by the author on the basis of suggestions of the reviewers. The material was finally edited and prepared for publication by S. A. Abercrombie and Norman Key of the Commission's staff.

II. CONDITIONS OF TRANSFER OF TRAINING

Introduction

Transfer of training is the application of previously learned responses to new situations. More precisely, it is the eliciting of responses by situaitons or tasks which are perceived by the individual as being essentially similar to those for which responses have been adequate in the past. These previously learned responses may function in one of three ways in the individual's adjustment to the new situation or task: (1) They may assist the individual in making the adjustment—this is termed positive transfer; (2) They may inhibit the adjustment—negative transfer; or (3) They may have no effect upon the adjustment, in which case there is no transfer of training—zero transfer.

The underlying theory in the use of synthetic trainers is that the responses learned on the devices can be transferred to the actual task for which the training is deemed appropriate. The transfer is presumed to be positive, thus facilitating the mastery of the task to be learned. For example, a student learns to manipulate the controls of a driving simulator, and this training may later transfer so as to assist him in responding adequately to actual driving conditions. Of course, negative transfer may occur. Learning which takes place on the device may inhibit the development of appropriate responses in performing tbe actual task. Finally, trainer learning may have no measurable effect upon the degree of additional learning necessary to master the new task or situation. Since positive transfer is not always achieved, it is important to understand those conditions conducive to positive or negative transfer.

Purpose

The purpose of this section is to present the basic conditions of transfer of training. Attention is given to those factors or conditions which are conducive to positive and negative transfer. The section provides a theoretical framework for the remainder of the study. The evaluation of any training simulator requires an understanding of these basic conditions.

Organization

This section is not intended to be a complete review of the literature on transfer of training; rather, the generalizations derived from laboratory experiments are presented. Those interested in the documented evidence of the conditions of transfer are referred to the works of McGeoch (49) and Stevens (65).*

* Numbers in parentheses refer to items in Bibliography. Pages for certain references are indicated by numbers following a colon, thus—(17:105-06).

The conditions necessary for positive transfer to occur are presented first; these are followed by conditions which induce negative transfer. Finally, there is a summary with a statement of the relevancy of the conditions to synthetic training device use.

Conditions of Positive Transfer

The basic condition for the occurrence of positive transfer is prescribed in the Law of Assimilation. This law states that ". . . each new object elicits those responses that have been associated with similar objects in the past, or we may say that our experiences of all new objects are invariably incorporated into those systems of experience that have been developed in dealing with similar objects." (17:105-06) Cronbach, too, points out that ". . . transfer of a behavior pattern to a new situation can occur whenever the person recognizes the new situation as similar to other situations for which the behavior has been appropriate." (20:253) It is important that the individual recognize the similarity of the two situations, for "objective similarity . . . does not necessarily mean a subjective similarity of the neural activities . . . . Any general agreement as to the degree of neural identity between any two complex problems is impossible." (76:57)

In order for positive transfer to occur, therefore, the two sets of evoking stimuli must be similar, or be perceived by the learner to be similar. Consequently, it is necessary to teach for positive transfer in order to assure its occurrence.

> Transfer is inevitably *selective*. Instead of bringing to a new situation all of one's repertoire of responses, one selects and uses a limited few. If the selected general aspects of prior training are relevant, transfer is positive; if they are irrelevant transfer is negative. What aspects are selected will be a function of the similarities between the old and new situations, and of the set of the subject, which in turn is influenced by prior training. (49:425-26)

A second condition of positive transfer is closely related to the first: "Greater positive transfer is produced by similar stimuli than by those that are different." (36:664) This implies simply that the more realistic the training device, the greater the degree of positive transfer that may be expected. *The function of realism in a training device is to increase the opportunities for the student to perceive the similarities between the training task and the actual task.* This principle has also been stated by Bruce: "Introducing new similarities between two or more of the $S_1R_1S_2R_2$ terms increases positive transfer, and decreases negative transfer." (15:360)

While this is an exceedingly important condition of transfer, it should be applied with caution to training devices. As will be indicated later, there is a point beyond which additional realism adds relatively little to the transfer value of a device. In addition, a device *may appear* to contain all of the characteristics of the actual task, and yet have little transfer value. This is the problem of face validity, which is discussed in Section III.

II. CONDITIONS OF TRANSFER OF TRAINING

A third condition to be noted is that "learning to make an old response to a new stimulus results in a marked degree of positive transfer." (15:360) If an individual is trained to respond in a specific manner on a synthetic training device, and then is confronted with a new situation for which this training is recognized by him as appropriate, he will exhibit a positive transfer of the learned responses. Thus, a person trained on a device to apply the brakes before depressing the clutch pedal will exhibit the same response in approaching a stop sign on a downgrade when actually driving a car (a new stimulus situation). This, of course, is the basic idea underlying the use of training devices—responses learned on the device become old responses and will be applied in new situations when performing the actual task.

A fourth basic factor in positive transfer is stated: ". . . transfer is facilitated when the initial learning can be formulated in terms of general principles applicable to new learning." (36:611) The classic pertinent experiment was presented by Judd (38:28-42) in 1908. To one group of boys the principles of light refraction were explained; another group received no training. Each group did equally well, through trial and error, in learning to hit a submerged object with a dart at a depth of 12 inches. When, however, the object was changed from 12 to 4 inches in depth, the group which had been taught the light refraction principles were able to adjust their aim more quickly to the new condition. A more recent experiment by Hendrickson and Schroeder (33:205-213) corroborated Judd's finding that knowledge of the principle involved facilitates positive transfer. In addition, they found that knowledge of the principles of light refraction aided the group in their initial efforts. In driver education, synthetic training devices may be useful in teaching students the basic principles of driving which can then be transferred to the actual task.

A possible fifth condition of positive transfer is one on which there is inconclusive evidence. In a study using nonsense syllables, Bruce (15:353-54) points out that when a training task and a transfer task require identical responses to similar stimuli, increased practice on the training task will increase positive transfer. From this it would appear that the longer the training time on a simulator which requires responses identical with those of the actual task, the greater should be the positive transfer effect. However, Gagne and Foster in their study dealing with transfer to a motor skill from practice on a paper-and-pencil task indicate that a leveling-off point is reached.

> The results of the present experiment indicate that a considerable degree of positive transfer results from practice on a paper-and-pencil representation of a motor skill. The amount of such transfer has been found to increase as the number of trials of practice on the paper-and-pencil task is increased from 8 to 48. The largest degree of increase seems to occur, in this situation, with amounts of preliminary practice up to 16 trials; thereafter, the increase in amount of transfer is not great. Thus, the effectiveness of continued practice on a 'pictured' task may be seen to have a limit of practical usefulness. (27:350)

It is important to determine, therefore, whether a leveling-off point is reached in practice on a simulator, and if so, when in the training situation this point is usually reached.

Conditions of Negative Transfer

Negative transfer occurs when prior instruction inhibits the learning of appropriate responses to new situations or tasks. Experimental work pertaining to negative transfer is not as extensive as that relating to positive transfer. However, there is one condition of negative transfer which has been found by nearly all investigators. When a subject is required to make a new response to an old stimulus, negative transfer occurs. (49:442) In such a situation the individual must learn to form a new response association with a stimulus for which a previously learned response had been adequate. This kind of negative transfer is familiar to many drivers who, accustomed to the standard gear shift, "depress the clutch" the first few times they drive an automobile with an automatic transmission.

In the example cited, a driver soon learns to make the appropriate response and cease the meaningless striking of the floorboard with the left foot. Bruce has taken cognizance of this tendency with his statement of another condition of negative transfer. "With increasing degrees of integration in the initial learning, there is an increase in the amount of positive transfer; where the amount of negative transfer is slight, it shifts to positive transfer." (15:360) Thus, negative transfer is a temporary phenomenon and tends to disappear with further practice of the new response. It remains true, however, that the learning of the new response is at first slower because of the associative interference, or negative effect, of the old response.

In the training of individuals on simulators it is unlikely that all of the responses learned would exhibit negative transfer. It is more likely that the total transfer effect would be positive. However, as McGeoch points out, this does not mean that negative transfer may not be present.

> Learning of one activity may . . . decrease the rate at which a second activity of the same class is learned. The conditions which produce it are relatively infrequently present in sufficient degree to make the net transfer effect negative. There may, of course, be negative transfer among the component parts of two samples but not sufficient to make the net effect negative. (49:401)

In evaluating synthetic trainers, it is important to be aware of partial negative transfer. Modification of the simulator, or of the instructional syllabus, may be required to eliminate specific features that induce partial negative transfer.

Summary

If a synthetic training device is employed, its use must be made as effective as possible. This means it should be constructed and used in accordance with the conditions of positive transfer. It should not incorporate elements which will lead to negative transfer. From its inception, it should combine sound psychological and engineering principles.

Basically, the device should confront the student with situations which to him are similar to those of the actual task, and should require him to make responses which are appropriate to the task. The greatest positive transfer occurs when the stimuli of both the training task and the actual task are similar and when there is little variation in the responses required. These conditions are relatively easy to achieve in the laboratory; they are much more difficult to build into complex training devices.

Positive transfer does not occur automatically. The teacher must assure its concurrence through providing the student with the general principles which are applicable to the task. With this knowledge, the student can select and practice the appropriate responses. If the student does not recognize the similarity between the transfer task and the actual task there can be no transfer of training.

Every part of the training device should contribute materially to the total positive transfer. Realism, in general, increases the transfer value of a device, but it may be possible to reach a point beyond which additional realism cannot be justified. Where this point is on any device can be determined only by careful research; however, several hypotheses are presented in this study.

Generally, though not always, students trained on synthetic training devices will exhibit some degree of positive transfer. However, not all parts of the training task transfer equally; some parts may even evoke negative transfer. Partial negative transfer may reduce the usefulness of a device through lowering the over-all positive transfer. A driving simulator must, therefore, be tested against this condition.

Another source of negative transfer occurs when a student is required to make a new response to an old stimulus. If on the training device the student has learned to press the brake pedal half-way to the floor in order to stop at a given rate of speed, and then attempts to do the same thing when driving a car with power brakes, negative transfer will be the least damaging of the results! In this instance, as with most types of negative transfer, additional practice on the actual task will be needed to eliminate the inappropriate response. However, this is wasteful of time, money, and effort. Application of the conditions of transfer to both the construction and use of a simulator can do much to reduce such waste.

III. ARMED FORCES RESEARCH ON SYNTHETIC TRAINING DEVICES—COMPLEX TASKS

Introduction

During World War II, the Armed Forces used synthetic training devices extensively. The influx of recruits and the scarcity of training weapons made the use of synthetic devices mandatory. These devices ranged in complexity from dummy hand-grenades to complicated multi-engine aircraft mock-ups. For many of the devices, no evaluation studies were conducted. The major criterion for use seemed to be face validity. If the device appeared to be realistic, it was employed in the training of recruits. The lack of adequate research was general during the early part of the war; however, in some instances educators and psychologists did make evaluation studies. As the war progressed, educators and psychologists were called upon more and more, and made significant contributions to the knowledge of synthetic training devices.

Armed Forces research, under wartime pressure, had several limitations which have been recognized and recorded by those conducting the studies. Examples of these limitations are: lack of control over changes in student and instructor personnel during the course of a study; accelerating or slowing down of training periods in response to sudden changes in personnel needs in the various theaters of war; and, necessarily, the emphasis placed on applied research.

Purpose

Research on military trainers is reviewed in this section. The purpose is to determine how many of the conditions presented in the previous section are applicable when simulators are used in mass training. In addition, conditions of transfer which appear to be specifically associated with the use of training devices are presented. This review provides information about the conditions of transfer which, when generalized, may be applicable to simulators used in driver education.

Organization

The research studies reviewed in this section pertain to aircraft simulators. Following the review of each study, the relevant conditions of transfer are noted, and in the general summary these are drawn together in a systematic way.

III. ARMED FORCES RESEARCH ON SYNTHETIC TRAINING DEVICES—COMPLEX TASKS

A Note on Study Design

Most of the studies reported in this document used the following experimental design. Two selected groups of subjects, representative of the relevant population, were matched in order to control variables known to influence performance. For example, if previous experience, age, or education might have affected performance level, then each group contained individuals who were identical with respect to such characteristics. If appropriate, a pretest on the skill to be learned was administered. Then one group, the experimental group, was trained on the simulator; the other group, the control group, was trained in the regular way. Upon completion of the training, a post-test was administered to determine the validity of the null hypothesis—*i.e.*, that there was no difference between the groups. If there was a difference between the scores of the two groups, it had to be determined whether this difference resulted from chance, or whether it was a statistically significant difference. Critical ratios or t-tests were used to make this determination. (These statistical measures show whether a difference as great as the one observed could have occurred as a result of chance variation. Generally, if this difference would occur by chance less than five times in 100, it is considered significant; if it would occur by chance less than once in 100 times, it is considered very significant.) If the experimental group achieved significantly higher scores, then the effectiveness of the simulator was established. If the difference in the scores between the two groups could be explained on the basis of chance, then the decision to keep or reject the simulator must have been made on grounds other than its training effectiveness, *e.g.*, danger or cost of training in the actual task. Finally, if the control group scored significantly higher, then the ineffectiveness of the simulator was demonstrated.

Aircraft Simulators

The first two research projects reported are companion studies. They are reported first because they make the important point that error analysis is necessary; for while there might be an over-all positive transfer, it is likely that there is differential transfer among the component parts.

The study of Flexman, and others, is an evaluation of the P-1 contact flight simulator used by the Air Force in primary pilot training (24). The P-1 simulator is a "Link" type device with controls and display panel similar to the T-6 airplane—a basic training aircraft. A Link type device is one in which a cockpit is mounted on a pivot and is made to swing, rotate, pitch, and bank, thus simulating a plane in flight.

The subjects, 95 American aviation cadets, were divided into two matched groups on the basis of "Pilot Stanine." * The simulator-trained group

* Prediction of success in pilot training is by means of a score on the pilot composite of the Aircrew Classification Battery. This score, termed Pilot Stanine, is based upon a nine-point standard score.

received 100 hours of T-6 instruction and 40 hours of contact simulator instruction. The control group received 130 hours of T-6 instruction and no simulator instruction.

After the training period had been completed and proficiency checks administered, the major conclusion of the authors was that ". . . although the simulator group received 30 hours less flying time than did the nonsimulator trained group, their proficiency was in every instance either not appreciably different from or was better than, the proficiency of the nonsimulator group." (24) In addition, it was concluded that there was no significant difference between the groups in terms of attrition and accident data in either the primary or basic phases of training.

In general, it would appear that the P-1 is a rather effective training device. However, Ornstein, Nichols, and Flexman have presented additional research on the P-1 simulator which is particularly significant (58). Their study was concerned primarily with determining the kinds of pilot behavior which transferred from the simulator to the T-6 aircraft.

Daily records were kept of the performance of the simulator and nonsimulator groups. The cadets were checked on their ability to perform in terms of procedural and adjustive responses. "The term 'procedural' refers to a component of performance in which perceptual feedback is of minimal importance. The appropriate response on such a component is of an 'all-or-none' nature. For example, the pilot pulls up the gear or he doesn't. He looks before turning or doesn't, changes gas tanks or doesn't. Counterposed to the procedural components is the 'adjustive' response. Examples of adjustive components are manipulation of the stick to attain a given pitch attitude, movement of the throttle to attain a given manifold pressure reading, and stick and rudder coordination to keep the ball centered." (58:5)

The analysis of the results shows that in all cases but one, some positive transfer did occur. There was considerable variation, however, in the extent to which positive transfer was exhibited in the various maneuvers. The maneuvers with the highest training effectiveness were those which called for procedural responses, while those calling for adjustive responses had relatively little transfer value. Also, the least effective training occurred in teaching advanced maneuvers.

These studies relate directly to two important conditions of transfer: (1) there is differential transfer among the various maneuvers learned in a training device; and (2) procedural activities are more likely to transfer than are adjustive activities.

Mahler in a similar study was concerned with the extent of transfer from practice on a synthetic flight trainer to the task of learning to fly a multi-engine aircraft (50). Two of the four phases of U. S. Naval multi-engine training were considered: (1) Familiarization Phase, during which the students become acquainted with the aircraft and the area; and (2) Instrument Phase, during which they concentrate on instrument work and radio procedures.

The design of this study involved the matching of two groups (experimental and control) on the basis of flight check grades received in basic training. The experimental group received training on the Operational Flight Trainer (OFT); the control group went through the regular training program. Each group was graded on six critical maneuvers in the Familiarization Phase and twelve maneuvers in the Instrument Phase.

The experimental students reached proficiency in fewer trials, and therefore, in less time than did the control students. However, the difference between the groups proved to be statistically significant for land planes on only two maneuvers (Starting Procedure and Engine Check) and no differences were significant for seaplanes.

In the Instrument Phase, there were eight statistically significant differences—four for land planes and four for seaplanes. Each of these favored the experimental group.

These statistically significant differences provide a measure of the amount of instruction time saved through use of the Operational Flight Trainer. In the Familiarization Phase the time saved was 18 minutes per student; in the Instrument Phase it was 53 minutes for land planes and 90 minutes for seaplanes. These amounts do not represent *practical* savings of time; it is necessary to eliminate at least four hours of syllabus time before a practical saving in time can be realized (50:45).

Mahler hypothesizes that the small saving in time may be due to the absence of fear and tension among learners using the trainer, to the lack of kinesthetic and visual cues, to the dissimilarity of control pressures, and to lag in the instruments of the device. Several of these hypotheses have been subjected to systematic experimental study and are reported later in this section.

Another measure of the proficiency of the simulator-trained group was the number of errors made by each group. The experimental students made fewer errors in Starting Procedure and Engine Check for both the seaplanes and land planes, and fewer errors in Traffic Pattern and Three-Engine Landing for land planes. In general, the Operational Flight Trainer experience improved the proficiency of the student in procedural maneuvers. On the other hand, the critical maneuvers of Take-Off and Landing showed no significant difference between the groups. It appears then, that the simulator-trained group made fewer errors *only* on the procedural tasks.

Mahler concludes: *"In general, the assumption that OFT training is of benefit in reducing the number of errors on procedural items is substantiated by the evidence in this study."* (50:86) Four categories of "airwork" were also analyzed. These were: Altitude, Heading, Airspeed, and Attitude of the plane. Airwork items exhibited little transfer and, compared with procedural items, the reduction in errors was much less significant (50:86).

The two conditions of transfer related to the previous studies are applicable here. These are: there is differential transfer among the parts of a task; and those parts which exhibit the greatest positive transfer are usually procedural items.

Williams and Flexman studied the civilian use of the Link SNJ Operational Trainer (79). Six students received training in the simulator and six were trained only in the aircraft. The trainer group required fewer trials in order to reach proficiency in the aircraft and committed fewer errors. It is estimated that 62 percent of the total time required by the non-Trainer group to reach proficiency was saved by the Trainer experience.

Unfortunately, on the basis of the reported results, it is not possible to determine the kinds of errors made by each group, *i.e.,* procedural or adjustive. The authors do point out, however, "If, in routine training, provision is not made for agreed upon standards of proficiency with a detailed objective record of performance, then time in the trainer could be largely wasted or could even result in the student's learning the wrong thing with consequent negative transfer of training." (79:7) Two important considerations are contained in this quotation: First, it is necessary to develop proficiency criteria so that the extent of transfer can be measured; second, a detailed record of performance is necessary to determine the kinds of behavior which transfer, and the record must be objective to assure uniformity among various graders.

Another study conducted at the University of Illinois deals primarily with one part of the flight task—landing (14). A portable type blackboard was placed in front of the School Link Trainer. A perspective drawing of a runway was sketched on the blackboard. The blackboard was so arranged that the instructor could rotate it about its horizontal axis in conformity with the indicated airspeed and rate of descent of the Link Trainer. The students were thus provided with a visual impression of the appearance of a runway during a landing.

The experimental group of students, to a statistically significant degree, made fewer errors in landing an actual plane than did the group which had not received Link training. The control group showed a more rapid rate of improvement because they made a greater number of errors initially; however, after 15 trial landings they did not achieve the level of performance of the experimental group.

The function of the blackboard was to make the Trainer more realistic by providing additional visual cues. Some impression of the relationship between the Trainer's instrument readings and the appearance of the ground was achieved. The condition of transfer involved is: If visual cues can be satisfactorily reproduced in a training device, positive transfer can be facilitated.

An experiment performed at Pensacola, Florida, was concerned with three synthetic training devices (51). There were three experimental groups; one was assigned to each of the devices. In addition there were a control group and a group comprising students who had soloed in civilian life.

The three experimental groups used the C-3 Link Trainer, the SNJ Trainer, and the 12BK Landing Trainer. The authors describe these devices in the following way:

> The C-3 Link is a general type—that is, it does not resemble a specific training plane. It was built originally as a light plane trainer.
>
> The SNJ is a special trainer—it is engineered to the specifications of the SNJ plane and actually uses cockpits taken from scrapped planes. The controls and instrumentation are the same as in the regular plane.
>
> The 12BK Landing Trainer is a novel device operating on a different principle than do the Link Trainers. The cockpit has the basic controls which activate a small airplane on a boom in front of the cockpit. The cockpit is on wheels and can move around in a race track pattern to stimulate take-offs and landings. Landing an airplane is the most crucial maneuver a new student must learn. Take-off is second in difficulty to landing. The Landing Trainer was developed with the expectation that it might be of value in training students on the crucial maneuvers of landing and take-off. (51:18)

The solo group was superior to the other groups. However, the synthetic training reduced accidents by two-fifths and flight failures by approximately one-third. There were no significant differences in extra flight time required to reach proficiency. The differences in mean check flight grades were negligible. In general, these results pertain to each of the devices. The authors indicate that ". . . the results justify neither the abandonment of synthetic training nor the immediate full-scale incorporation of such training into the regular syllabus." (51:2)

No one of the devices proved to be superior to the others. Such improvement as was obtained was attributed to general factors in the synthetic training program rather than to the specific virtues of a particular trainer. It is suggested that ". . . the familiarity with the location and operation of the basic controls (rudder, stick, and throttle) carries over in a manner independent of the circumstances within which this familiarity is acquired." (51:60)

The suggestion that cockpit familiarity transfers reinforces the condition that procedural items are most likely to exhibit positive transfer. The fact that all three of the devices provided essentially the same degree of learning indicates that a precise and faithful reproduction of the actual task is not essential to assure positive transfer.

The question of how faithfully a simulator should reproduce the characteristics of the actual task is generally answered—the closer the better! Yet, the cost of any simulator will increase very rapidly as it is engineered to achieve a closer correspondence with the actual task. One costly phase of simulator construction pertains to kinesthetic feedback. It is important to determine, therefore, whether faithful reproduction of kinesthetic feedback is necessary to simulator effectiveness.

Matheny and others designed an experiment to determine whether the amount of control pressure was a factor in positive transfer (52). This study was based on laboratory research which had shown that transfer of training did not depend upon a correspondence between the control forces present during original learning and the control forces present in the final task. The attempt was made to repeat, in principle, the laboratory experiments in a flight training situation. Original learning was conducted in a synthetic flight trainer—the P-1 Link Trainer; the final task was learned in the air in the T-6 aircraft.

Three groups of subjects learned to climb and glide. These maneuvers require precise manipulation of the aircraft elevator control through fore-and-aft movements of the control stick. One of the experimental groups (the pressure group) learned the maneuvers in the P-1 with the control forces on the stick adjusted to correspond as closely as possible with the control forces in the T-6 aircraft. The other experimental group (the no-pressure group) learned in the P-1 with practically no control forces on the stick, thus varying the kinesthetic feedback as much as possible from the T-6. The control group learned the maneuver only in the aircraft.

The proficiency criterion was three consecutive errorless trials for each maneuver. When the experimental groups achieved proficiency in the Link (original learning) they then had to achieve proficiency in the aircraft (final task learning). The control group had only to achieve proficiency in the air.

The subjects were 30 Air Reserve Officers Training Corps cadets who had no previous flight experience. They were divided into three equal groups on the basis of scores made on a mechanical comprehension test.

In the original learning there was no difference between the pressure group and the no-pressure group in the average number of trials required to reach criterion performance in the P-1 Link Trainer.

Each of the experimental groups required fewer trials to learn the maneuvers in the T-6 than did the control group. The extent of transfer was measured by the t-test, *i.e.,* comparing the average number of trials required to reach criterion performance by the control group with the average number required by the experimental groups. There were no significant differences—therefore no transfer—for the climb maneuver. However, for the glide maneuver each of the experimental groups exhibited a significant difference from the control group. In each of these groups there was positive transfer.

If it were necessary for the simulator to reproduce faithfully the kinesthetic feedback of the actual task, then the pressure group performance should have been significantly superior to that of the no-pressure group. However, there were no significant differences between the experimental groups. They relearned the maneuvers in the T-6 aircraft in approximately the same number of trials. It can be concluded, then, that the degree of pressure on the stick is not a factor in the positive transfer, and that the trainer is as effective when minimal pressure is used. This, in turn, may mean that a large cost item in the construction of such training simulators can be eliminated.

There is still the problem of the glide maneuver exhibiting positive transfer and the climb maneuver not transferring. The authors state: "Because the climb was made at a constant thrust, it was necessary in the aircraft to apply an increasing back pressure for the duration of the climb in order to compensate for variation in pitching moment with change in air speed. In the trainer this requirement was all but eliminated . . . ." (52:5) This difference made the climb easier in the trainer than in the aircraft. The increasing back pressure requires an adjustive response and, as has been noted, this kind of response tends not to transfer. In the glide maneuver, on the other hand, air speed was constant and there was no need to compensate for changes.

As a result of this study the authors conclude that transfer of training depends more upon a correspondence between the sequences of patterns of control forces than it does upon a correspondence between the absolute amounts of control forces in the trainer and the aircraft.

There are three conditions of transfer to be noted in this study. First, procedural items are most likely to exhibit positive transfer; that is, the items which transferred were the sequence of control movements. Second, control pressures which more faithfully reproduce kinesthetic feedback, while they may enhance the face validity of the simulator, do not necessarily increase its effectiveness. And third, extensive realism built into a synthetic training device does not necessarily increase its validity.

Another study designed to test the importance of kinesthetic cues as well as verbal and visual cues was conducted by Battig (9:371-80). The purpose was to determine the kinds of cues used in a precise lever-positioning movement. The subject was required to move a joystick to a position which would light green lamps corresponding to red lamps located on a stimulus panel.

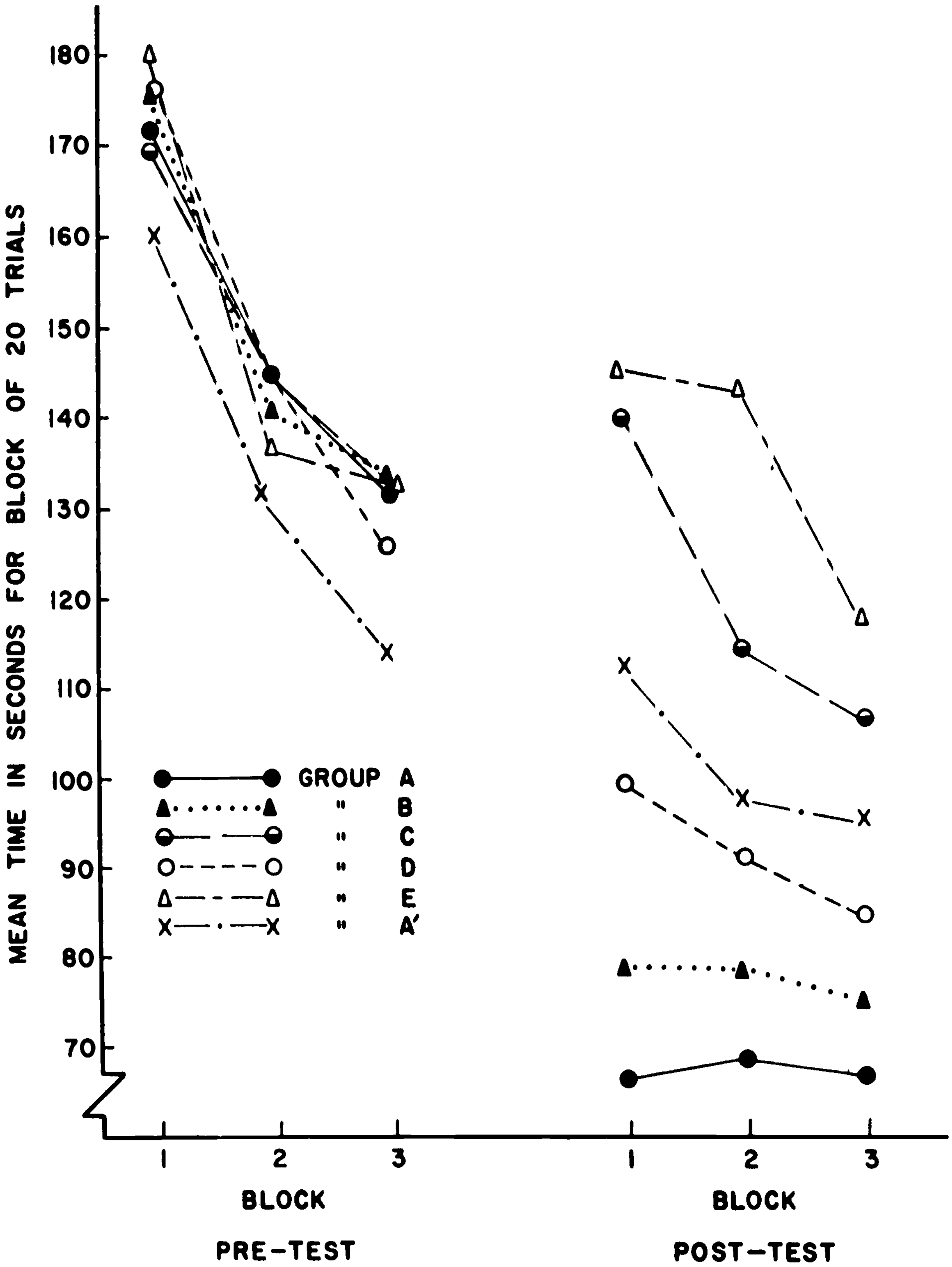

FIG. 1. Mean total time to complete each block of 20 trials on pretest and post-test.

Source: Battig, William F., p. 374.

The apparatus was the complex coordination test, Model E. (Mashburn), as described by Melton. Two major modifications were made for the purposes of the present experiment: (a) the rudder movement and corresponding row of lights were eliminated; (b) the self-stepping feature, by which each correct matching of the lights automatically presented a new stimulus setting, was also eliminated. . . . Instead, a fixed 20-sec. interval between successive settings was used.

The light panel consists of two parallel rows of red and green lights, and two parallel columns of red and green lights. There are 13 lights in each row and column. Movement of the stick from left to right controls the green lights in the horizontal row, movement forward and backward controls the vertical row. When the stick is in neutral position, the center (7th) light in the row and column of green lights is lit.

The S's task was as follows: The stimulus consisted of two illuminated red lights, one in the row and one in the column. The S would move the stick, starting from the neutral position, so as to light the green lights opposite each of the two red lights simultaneously as rapidly as possible. Each correct matching of the red lights constituted a trial. The score was the time required to make a correct match after the stimulus setting had been present. After the completion of each trial, S returned the stick to the center position to await the next trial. (9:371)

Sixty subjects were divided into six groups. A pretest was administered; after practice a post-test was given. The training tasks of the six groups were as follows:

Group A (Standard practice): This group performed the standard task throughout the training period.

Group A′ (No practice): This group did nothing during the training period.

Group B (Verbal stimulus): The stimulus for this group consisted of two numbers read by the experimenter (*i.e.*, "six right, three forward"). The lights were masked. Visual cues were eliminated.

Group C (Verbal response): The red light settings were presented and S described how he would move the stick, but without doing so. Kinesthetic cues were eliminated.

Group D (Kinesthetic response): The red light settings were presented and S attempted to move the stick to the correct position without benefit of the green lights. Visual cues were eliminated; kinesthetic ones emphasized.

Group E (Verbal stimulus—kinesthetic response): Neither the red nor green lights were used. The S's task was to move the stick in response to verbal instructions given by the experimenter. Visual cues were eliminated, and kinesthetic and verbal cues were emphasized. (9:373)

The results of a pretest and a post-test administered after eight practice sessions are shown in Figure 1. Group A had the highest level of performance, indicating that practice on the actual task is superior to practice on a variation of the task. The scores of each of the groups, except Group B, were significantly different from Group A. It is to be noted too, that the verbal response group and the verbal stimulus-kinesthetic response group were inferior to the no-practice group. The author points out that this finding challenges the common assumption that practice on anything remotely related to a motor skill will result in improved performance on that skill. Further, he indicates that the difference between the training procedures for these groups and the standard task group may be no greater than for many training devices now used.

This study related to the relative unimportance of providing kinesthetic cues in synthetic training devices. Groups for whom the training emphasized kinesthetic cues performed poorer than the standard task group, and one of the former did less well than the no-practice group. There is some evidence, too, that the practice of talking through a task, *i.e.*, verbally describing each phase of a task before performing it, may add little to, if not interfere with, the performance of a motor skill.

Summary

The purpose of this section has been to determine the conditions of transfer of training which are especially applicable when synthetic training devices are used in learning complex tasks. Research on the use of devices in pilot training was reviewed. The following conditions were noted:

1. *There is differential transfer among the various parts of the total task.* It is probable that some parts will exhibit a great degree of positive transfer; others, no transfer at all; and still others, negative transfer. In the construction and use of a training device it is important to determine which parts fall in the various categories.

2. *Procedural responses are more likely to transfer than are adjustive responses.* Based on the available evidence, it is reasonable to assume that the procedural responses will fall in the positive transfer category. It also appears that adjustive responses tend not to transfer without special modification of the devices or specially developed syllabuses.

3. *The synthetic training device does not have to be a precise and faithful reproduction of the actual task for positive transfer to occur.* Quite different types of devices, each designed to simulate the same task, may be equally effective. In such cases, some other criterion, such as initial cost or maintenance cost, should be the basis of selection.

4. *A synthetic training device does not have to reproduce kinesthetic feedback faithfully to be effective.* It is usually assumed that a device must reproduce the "feel of the controls." While basing the design of a device on this assumption may add tremendously to its cost, evidence suggests that such reproduction may not enhance its transfer value. A partial explanation

may be that the sequence of movements which does transfer requires large muscle activity, and is, therefore, a type of procedural response. Kinesthetic feedback, on the other hand, provides information for small muscle activity which is a type of adjustive response. For example, large muscle activity is involved in moving the foot to the accelerator (a procedural response), but small muscle activity is involved in maintaining a given pressure on the pedal in order to achieve or maintain a given speed (an adjustive response).

5. *Realism does not necessarily increase the validity of a device.* Each of the preceding conditions is directly related to this one. Characteristics which merely increase the face validity of a device should be eliminated unless they can be justified through empirical research as important motivational factors in student learning.

These are some of the conditions which must be considered when evaluating a synthetic training device for driver education. Research studies which show that positive transfer occurs, while informative, are not sufficient evidence upon which to judge the effectiveness of a training device. Before an evaluation can be made, it is necessary to know which parts of the task transfer and which do not. With this information it becomes possible to see the relationship between the cost of the device and its effectiveness.

IV. ARMED FORCES RESEARCH ON SYNTHETIC TRAINING DEVICES – JUDGMENTAL ACTIVITIES

Introduction

The task of driving an automobile safely not only involves the manipulation of controls, but also requires innumerable judgmental activities. The driver must be concerned with the speed of his car and that of others; he must judge accurately the distance from his moving automobile to other moving vehicles and to stationary objects ahead, behind, and to the sides; he must accurately assess the direction of his vehicle in relation to other cars, pedestrians, and non-moving objects. Further complicating the driver's task is the frequent need for him to make such judgments simultaneously. Thus, even the daily drive from home to work can be infinitely more complex than controlling an airplane in flight.

The driver makes judgments based on cues—auditory, visual, and kinesthetic—which he interprets in the light of his previous experience. Of these, the visual cues are probably the most important. The driver uses visual information to judge factors of speed, distance, and direction. He decides to pass or not to pass a vehicle on the basis of his visual impressions of his car's speed, the speed of the car ahead, and the speed and distance of any oncoming traffic. He alters his direction when he perceives a curve in the road. The distance between his car and the one ahead is determined by his estimate of the speed of the two vehicles. These are but a few of the visual cues used.

Examples of auditory cues would be the sound of a horn, the screech of brakes, the noise of one's own tires and that of others, and the sound of the engine. Kinesthetic cues would include the movements of the car in response to control manipulation, the sensation of skidding, and the "feel of the road." Kinesthetic and auditory cues also give some sense of speed. There are, of course, many other auditory and kinesthetic cues which provide the driver with information about situations to which he must respond.

Purpose

This section reviews Armed Forces research pertaining to the use of training devices to improve judgment. Conditions of transfer related to this use of devices are designated. The review provides useful information for evaluating the various elements of existing simulators used in driver education.

Organization

The studies reviewed are grouped according to the judgments necessary to complete the task successfully. Those concerned with distance judgment appear together, as do those pertaining to tracking behavior. Following the review of each study the relevant conditions of transfer are noted and are grouped together in the general summary.

Distance Judgment

The tasks of antiaircraft gunnery and aerial gunnery require the ability to judge the distance to an approaching aircraft. It is necessary that the gunner estimate when the plane is within effective range—not so far as to waste ammunition, and not so close as to increase risk. For the aerial gunner, the task is complicated because both he and his target are moving.

The sight reticle is an aid in distance judgment. The gunner learns that when a given type of enemy plane fills a specified portion of his ring sight the plane is within effective firing range. (The use of electronic fire control systems is not considered, for different skills are involved in their operation.) Incidentally, the automobile driver does not have such an intricate sight device to aid him in distance judgment even though it is imperative that he not overestimate the distance of an approaching car while passing another.

Several studies have been conducted which deal specifically with the task of judging distance to an approaching airplane. One of these is concerned with a training device known as the Reflectone Trainer (34:142-46). This device is about 10 feet long, five feet high, and two feet wide. The gunner, looking through an opening in one end, sees a reflection of a model plane whose apparent range can be changed by varying the distance between two mirrors. His task is to adjust the apparent range of the plane to 600 yards—a critical range for opening fire.

In the evaluation of the device it is stated: " . . . it seemed that the primary contribution of the trainer should be in the direction of improving the student's ability to differentiate different proportions in his sight recticle filled by various planes, assuming that this ability was subject to modification. The data reported did not indicate that the trainer modified this perceptual ability appreciably." (34:145-46) It is also pointed out that the improvement realized was not due to improvement in the discriminatory ability but, rather, resulted from the subjects' using extraneous cues such as sound and brightness.

There are two important considerations for transfer of training involved in these studies. First, extraneous cues should not be present to assist the student in making judgments. If the student is to transfer learning from a synthetic training device to an actual task, the use of extraneous cues will inhibit positive transfer and lead to negative transfer. Second, the question is raised as to whether the gunner's skill in judging distance can be appreciably improved.

A mirror range estimation trainer was also used in teaching antiaircraft gunners how to judge distance. Its technical name was the Mirror Range Estimation Trainer, Device 5c-4; however, it was known simply as the 5c-4. It was used to train 20mm gunners to estimate their opening range of 1500 yards.

A study was conducted to determine the validity of the device (72). Two groups of men were selected; none had previous experience in estimating the range of a real plane. One group was given one hour's training on the 5c-4; the other group was given no training.

Upon being taken on the firing line to estimate when a real plane was 1500 yards from the line, the trained group did not prove superior to the untrained group—both groups underestimated the range. The statistical evidence indicated that the experience gained through practice on the 5c-4 device " . . . did not contribute toward improved performance of estimating the range of a real plane on the firing line." (72:37)

The tendency for both groups to underestimate the range is consistent with the behavior of gunners in combat, who likewise underestimate the opening range. The authors believe that since underestimation is characteristic of gunners in both non-combat and combat situations, the presence of anxiety and fear in the latter situation is not sufficient to explain the tendency. A large part of the error, therefore, may be caused by perceptual factors; if this is so, a program directed toward training the men's visual perception of range might do much to offset the tendency.

This study re-emphasizes an important difficulty in utilizing a synthetic training device to improve judgmental activity. It is the underestimating tendency, however, which is of major interest. One might assume that a degree of anxiety would be present whenever one has to judge distance to an approaching object—whether it be an enemy aircraft, an automobile, or a target plane. Once the aircraft has passed the critical range, the possibilities of opening fire at that point are gone. The anxieties associated with not missing the critical point would seem sufficient to cause an underestimation of the the true range.

Some evidence of the ability to improve distance estimation is provided by another study of the Device 5c-4 (62). This study had three parts: first, the effect of two hours' training on the device; second, comparisons of various combinations of firing line and device training; and third, the retention of range estimation training.

The 164 subjects were 20mm gunners without previous formal training in range estimation. Half of the men were given two hours' instruction on the 5c-4; the others were told how the ring sight could be used to estimate opening range.

Both groups made gross underestimation errors. It was concluded that " . . . Device 5c-4 can serve a useful function in training in range estimation particularly when no other form of training is available." (62:iv)

In the second part of the study one group received firing line experience only, another received firing line plus 5c-4 training, and a third was trained only on the 5c-4. The group which received firing line plus 5c-4 training did not excel the men who had firing line training only. There was a clear tendency for those who had firing line training to surpass those who had been trained only on the device. It was concluded: "Such results warrant the conclusion that range estimation training on the firing line is preferable to training on the Mirror Range Estimation Trainer, Device 5c-4. Particular significance may be attached to the fact that training on the firing line under the experimental conditions involved approximately 45 minutes as compared with two hours on the 5c-4." (62:v)

The third phase of the study was concerned with retention. Two groups of men, one previously trained on the 5c-4, were given two days' training on the firing line. After a lapse of time the performance of the group trained on the firing line was superior to that of the 5c-4 group the day after their initial training.

The authors recommended, first, training on the firing line, and second, use of the Device 5c-4 only when firing line training is not possible (62:vii).

It would appear, at least as far as these devices are concerned, that whatever improvement in distance judgment can be realized is best achieved through practice in the actual task. The evidence is not clear on the basic question—can perceptual ability as related to distance judgment be improved?—which additional research will have to answer.

Tracking Behavior

Adelson defines tracking as ". . . any process by which a person attempts to maintain, by means of the manipulation of a control device, a given relationship between an index [or point of reference] and a target free to move relative to it." (2:1) For example, a gunner pointing his gun at an enemy plane is engaged in tracking. This definition may be expanded to include the process of maintaining a relationship between a target and an index free to move relative to it (*e.g.*, fixed guns on a plane which the pilot aims by pointing his plane at the target), as well as a relationship between a moving index and a moving target (*e.g.*, a plane with movable guns which the gunner aims at another plane).

The automobile driver engages in tracking behavior when he maintains a given relationship between his moving car and the road. When the road curves, he has to turn; when it straightens out, so must he. The driver also "tracks" when he is following another vehicle and when he observes the relative positions of oncoming cars.

The steering wheel is the primary control which the automobile driver manipulates in tracking. A recent publication of the NEA National Commission on Safety Education contains several suggestions for teaching this skill (57). These suggestions point to the similarity, if not the identical nature, of steering and tracking. One instructor tells a student to pick out a point of reference, usually a spot in the middle of his lane about 200 feet ahead of him, and to steer toward it. When approaching a curve, his point of reference moves to the shoulder or the center line depending upon the direction of the curve. Of course, in driving the driver must look at his point of reference intermittently for he must also be aware of general traffic conditions.

Since tracking, or steering, is an important part of driving it will be useful to see how synthetic training devices have been used to develop this skill. In the early days of World War II the sport of skeet shooting was used to teach aerial gunners to track and lead a target (34:191-96). Several modifications of skeet shooting were developed: the shotguns were provided with spade grips; they were mounted on swivels; they were provided with ring sights; and finally, they were mounted on a truck which moved around a track at a speed of 15-25 miles per hour. Each of these developments was designed to make the training more realistic.

A validation of skeet shooting with the above modifications was conducted. Of four groups of gunners, one received training omitting the ring sights; a second omitted the moving truck experience; a third omitted both; and a fourth went through the regular training, including all the modifications. The proficiency criterion used to measure the differences among the groups was gun camera scores. No differences greater than those expected by chance occurred. The results suggested that this aspect of aerial gunnery training could be dropped without seriously affecting the gunners' proficiency.

It is important to note that each modification of skeet made it more realistic, more similar to the actual task of flexible gunnery. Yet those who received this training were no more proficient than those who did not. As regards transfer of training, then, it may be stated that an increase in the face validity of a training device does not necessarily make it a more effective one.

Aerial gunnery is a task which involves distance judgment as well as tracking. The ultimate objective is to destroy enemy aircraft. Because of time, cost, and danger, the task requires that training devices be used in learning the necessary skills. The E-8 Spotlight Trainer was one of these devices (34:151-56).

IV. ARMED FORCES RESEARCH ON SYNTHETIC TRAINING DEVICES—JUDGMENTAL ACTIVITIES

> The E-8 consisted of three parts: an electrically-driven, cam controlled projector which projected a moving spot of light on the ceiling; a photoelectric cell mounted on the turret in place of guns and harmonized with the sight; and a shot and hit counter unit which recorded . . . hits whenever the photocell was pointed directly at the spot of light while the triggers were depressed. The recorder contained a sounder which could be set to represent either shots or hits . . . The sound of shots being fired or hits being scored enhanced the students' interest in the practice. (34:151-52)

A modification of the E-8 was developed by the Sperry Gyroscope Company which required that the gunner frame a variable-sized spot of light by controlling the adjustable reticles on his sights.

A study of the effectiveness of the E-8 was conducted. Fifteen graduate gunners were used as subjects. They flew three gun camera missions, practiced on the E-8, and then flew three additional camera missions. Practice on the trainer reduced the number of tracking errors which the gunners made; however, it did not improve their framing ability.

Framing is an aid in distance judgment. A target is framed when it fills a given portion of the sight reticle which, in turn, provides an estimate of the range from gunner to target. There are two possible interpretations to the relative lack of improvement in framing ability following practice on the Sperry device. It may be that the experienced gunners had already achieved their maximum proficiency in framing so that additional practice added little to their skill. Or, it may be that this particular skill is not subject to much modification through training. Which of these interpretations may be correct was not determined in this study since no control group was used, a fact that also makes it difficult to interpret the reported improvement in tracking. It is not known whether the improvement resulted from use of the device or simply from more practice.

The condition of differential transfer is applicable to this study. If transfer did occur, it occurred for only part of the task and not the whole. The study also presents evidence that perceptual ability in judging distance reaches a plateau, but whether this is an innate limitation or the result of previous maximum learning of the skill is not known.

The psychologists who engaged in research on these and other training devices occasionally recommend rejecting them. One example concerned the DeVry Panoramic Trainer (34:185-88), a complex trainer designed to reproduce realistically the gunner's task and physical environment.

An evaluation study of the trainer was conducted, the proficiency criterion being performance on the E-14 Spotlight Trainer. It was concluded that "whatever the benefit to skill in judging deflections that may have accrued from practice on the DeVry, such benefit did not show up in the E-14 situation." (34:186)

A second experiment, using gun camera missions as proficiency criteria, was conducted. The trainer was the DeVry, Mark II, which incorporated some improvements based on results of the previous study. The subjects were graduate gunners. Gun camera missions were used as pre- and post-tests, in between which the subjects practiced on the DeVry trainer. There was no significant change in the gun camera performance. It is pointed out that the subjects might have reached their ceiling of gun camera proficiency before the test, and that no trainer would aid them in improving their scores. However, rank order correlations between gun camera scores and DeVry scores indicated a very small relationship between the two measures. This was interpreted to mean that either the two scores or the two tasks had very little in common, which argued against use of the DeVry Trainer.

Stated simply, the condition of transfer involved in these studies is that the complexity of a device does not guarantee its validity. Similarly, the realism built into these devices did not improve the proficiency of the gunners.

Radar Studies

With the development of fire control systems, the aiming of guns was taken over by fire control officers. However, the tracking and framing skills of gunners are still necessary in the event of damage to the central control system, in which case guns have to be fired manually. In the central fire control system, targets beyond visual range are presented on an oscilloscope. It is the radar operator who must track the target. Several types of radar presentations were used during World War II. In the A-scan type, the scale of distance is presented as a horizontal line; in the J-scan type, the scale of distance is presented as a large circle.

The Foxboro Trainer was one device developed for teaching pip-matching tracking ability on the A-scan (45). Synthetic targets are fed into the indicator unit, and the operator's task is to keep two pips balanced by means of a hand wheel. Since men trained in the actual task showed a significant transfer of tracking ability to the simulated task, the authors suggested that proficiency acquired by means of the training device is transferable to the actual equipment. Men who learned to track through pip-matching on the A-scan presentation, however, were not able to transfer that ability to the J-scan presentation.

In another device, the J-scan presentation was simulated by means of a device known as the SCR-584 Basic Trainer (46). The device did improve tracking ability; however, men trained in the actual task were not significantly better than untrained men in tracking on the device. This suggests that the trainer is not a valid device for practice. Interestingly, there was no transfer to the A-scan presentation. Apparently these two tasks, tracking on the A-scan oscilloscope and tracking on the J-scan oscilloscope, differ significantly enough to prevent positive transfer from one task to the other.

"Visual noise," so-called, is a characteristic of radar scope presentation. It is unwanted information in the feedback channel, or in the output signal, of a man-machine system. Examples would be the "snow" and the double image which occur on a television picture tube. A study was conducted to determine the effect of visual noise on learning radar tracking (12). Four groups of subjects received training under different noise conditions. Under the no-noise condition all groups achieved comparable levels of proficiency. Proficiency was determined to be inversely related to the amount of noise in the presentation. It was concluded that visual noise depresses performance levels. On the transfer trials, however, all groups showed comparable proficiency. Visual noise apparently has little effect upon learning in this skill. That is, the same amount of learning occurs regardless of the degree of visual noise.

The authors offer the following hypothesis to account for the negligible effect of noise on the learning of this skill.

> Proficiency in a compensatory tracking task is dependent upon the skillful execution of primary, large amplitude corrections and (if necessary) secondary small amplitude corrections. However, if practice on secondary adjustments is limited, as is assumed to be the case with noise in the feedback channel, then *overlearning* of only the primary corrections is sufficient preparation for maximum proficiency in a subsequent task permitting (or requiring) both primary and secondary corrections.(12:14)

Two limitations are placed on the hypothesis. First, "The primary and secondary adjustments may differ in degree but not in kind;" and second, ". . . the training task may restrict practice on secondary corrections but must preserve the major interactions and transformations of the transfer system." (12:14)

Radar scope interpretation is also important in aircraft navigation. The cost involved in training navigators on actual flight missions has spurred the development of synthetic training devices. Motion pictures have been used to teach radar scope interpretation. An evaluation of this technique was conducted in 1954 (48). The device used presented motion pictures of radar scope returns on a simulated radar scope display. Range and bearing data were provided and the students navigated as though on an airborne training mission.

Most of the subjects were experienced pilots without radar experience. They were divided into three groups: one received only motion picture training; the second was instructed both in the air and on the ground trainer;

and the third received only training in the air. To test the hypothesis that there was no difference among the three groups, 19 criterion measures were used. There were no statistically significant differences among the groups; there was no evidence that the "all air" group was superior. It was concluded that motion pictures of the radar scope can be substituted for a part of the air training.

The study related to the degree to which similarity between the simulated task and the actual task is conducive to positive transfer. Radar scope presentation can be faithfully reproduced by motion pictures, and this study indicates that it is not necessary to reproduce the kinesthetic sensation of motion for positive transfer to occur.

Another device developed to teach navigation was the G-2 Trainer. There was no radar component in this device since it was designed to teach dead reckoning.* The G-2 Trainer consisted of a master control panel and 48 individual booths each containing the instruments necessary for dead-reckoning navigation. Two evaluation studies of this device were performed (23). The first was aborted because of inadequate controls. The second was designed ". . . to test the hypothesis that groups trained only in the classroom would not differ significantly from groups receiving extensive training in the G-2 Trainer."(23:174) The experimental group navigated according to data presented on the instrument panels of the G-2; the control group was presented with data on a blackboard or a large mock-up.

After training, the two groups were graded on three flight missions. Analysis of the grades revealed no consistent differences favoring either group. The hypothesis was retained; neither group was inferior to the other. It was concluded that the skills learned on the very expensive G-2 Trainer could be learned as well through relatively simple blackboard and mock-up presentations.

A more detailed report of this research is presented by Carter (18:136-46) who also found no statistical difference between the trainer group and the classroom group. Of the eight variables measured, only one favored the trainer group at the 5 percent level of confidence; for the other seven there were no differences.

Carter points out that the aim of the trainer was to supplant the classroom work when it should have been to supplement it. The device, with certain modifications, might become a useful teaching aid.

Here again the basic condition of transfer involved is that a high degree of face validity does not guarantee the superiority of the device over

* Dead reckoning is the method of determining a plane's approximate position by applying to the last known position the factors of time, distance, and direction flown, and compensating for the wind's force and direction.

simpler training aids. Error analysis indicated that differential transfer took place; but since only one of the variables transferred, this was considered insufficient justification for adopting the device.

Another report raises the question of extraneous cues being present in the training task. If a subject uses cues which are contained in the device but are not present in the actual task, zero or negative transfer will result. For example, in the use of the Link ME-1 (a twin-engine jet basic instrument flight trainer) Townsend points out the following difficulty: "When the single engine procedure was called for by flaming out one of the engines, the pilot could readily hear the flame-out switch being thrown by the instructor. The students developed the habit of immediately looking for a flamed out engine whenever they heard the switch being thrown in the cockpit. In this manner poor cue-response relationships are being developed." (67) A device must contain, therefore, only those auditory and other cues which are present in the actual task.

Summary

The purpose of this section has been to determine the conditions of transfer associated with the learning of judgmental skills. The uncertainty and, in some cases, contradictory nature of the evidence precludes the designation of specific conditions. However, certain conditions presented in Section III are equally applicable here, as, for example:

1. *The greater the similarity between the training task and the actual task, the greater will be the positive transfer.*

2. *Merely increasing the face validity of a device does not necessarily increase positive transfer.*

3. *There is differential transfer among the component parts of the task.*

4. *Adjustive responses tend not to transfer.*

There is one additional condition which can be stated: *The presence of extraneous cues in the training task tends to inhibit positive transfer.*

The research raises several questions, however, with which persons involved in the construction and evaluation of synthetic training devices should be concerned. For example, can judgmental skills be improved by practice on a training device? It appears that some improvement in distance judgment can be realized by practice in the actual task; it is much less certain that such improvement will result from practice on a device. That is, there will be little transfer unless the device is especially designed to maximize it.

Another related question is: does distance judgment ability reach a plateau beyond which additional practice on a training device is valueless?

If this ability can not be modified, or if at most only slight improvement can be realized, then the cost of including special characteristics in the device to accomplish this improvement can not be justified.

Many different cues are used in making judgments. Precisely which cues are used in making judgments about speed, or distance, or direction in driving an automobile must be determined. When these are known, it will be possible to evaluate more accurately the existing synthetic training devices used in driver education.

V. DRIVER EDUCATION TEACHING DEVICES

Introduction

Among several types of driving simulators used in driver education, two have been developed to the level of commercial production and distribution. These are the Drivotrainer and the Auto Trainer. Through supplementing the practice driving learning experience, the general purpose of each of these devices is to assist the student in acquiring the necessary skills to be a safe driver. In its use, then, each device is analagous to the Link trainer used to teach pilots.

It is assumed that certain driving skills can be learned on a training device. As a result, some schools which use driving simulators have reduced the amount of practice driving instruction in the car from six to three hours. Since a number of states are permitting experience on a synthetic device to be substituted for actual driving experience in the driver education program (39:31) and since there is general interest in the use of the devices, a critical analysis of their effectiveness will be useful.

Purpose

The purpose here is to review the evaluation studies of the Drivotrainer and Auto Trainer. The conditions of transfer of learning presented in the preceding sections will provide a basis for the analysis. The studies on the effectiveness of these devices will be reviewed and interpreted in light of these conditions.

Organization

The Drivotrainer studies are reviewed first. Following a description of the Drivotrainer, two studies (conducted in Los Angeles and in Iowa) are presented and discussed in terms of the conditions of transfer; and comments on the device are made. The Auto Trainer studies are then reviewed, following the same pattern. A brief summary concludes the section.

Description of the AEtna Drivotrainer

The description of the AEtna Drivotrainer given here is taken from the report of the study (5) discussed later. Notations indicate certain changes introduced in more recent models of the Drivotrainer. The description is as follows:

> A. A number of small AEtnacars, equipped with all of the controls found in a manual gear shift model automobile. [Current models are available also with the controls found in automatic transmission automobiles.]

B. A master control and recording cabinet electrically connected to each of the AEtnacars.

C. A projector.

D. A Servofilm unit. [Not present on current models.]

E. A screen.

F. A set of teaching films.

The complete installation operates on a 110-volt alternating current.

A. The AEtnacar

The AEtnacar is a single-place, immobile, non-enclosed unit with the basic configuration of an actual automobile. It has the following operational controls: clutch and brake pedals, accelerator pedal, steering wheel, gearshift lever, directional signal, horn ring, light switch [not present on current models], starter, and hand brake lever . . . . All of these controls are wired to the master unit which electrically records the operation of each one simultaneously. There is a speedometer, a group of non-operating gauges, as well as a seat control lever. A mirror is mounted on each AEtnacar for use when backing. The mirror is placed to the right rear of each car, and as the student looks over his right shoulder the screen image is reversed to appear as the scene would in an actual backing maneuver.

Under the hood of the AEtnacar . . . are the electro-mechanical parts which make it possible to simulate the operation of an automobile. The steering mechanism has a turn resistance comparable to a modern passenger vehicle, and is constructed so that three positions in either direction may be recorded. The brake pedal mechanism of hydraulic construction is designed to simulate actual brake resistance. No brake, slight brake, medium brake, and hard brake can be recorded. Special wiring allows for recording of brake pumping. The accelerator permits variation of engine speeds. The master unit records four different accelerator ranges—0 to ¼, ¼ to ½, ½ to ¾, and ¾ to full throttle. [As many as 40 different driving actions could be checked and recorded.]

The number of AEtnacars is limited by the size of the available classroom, and the capacity of the master control . . . [which] will accommodate fifteen AEtnacars [25 with present models].

B. The Master Control

This control is housed in a cabinet 44" x 40" x 48", and is centrally located at the rear of the room. The top of this cabinet serves as a platform for the projector and the Servofilm mechanism, ... This central unit is composed of the following component parts:

1. *Two-control panels* are located on the right side of the cabinet (looking forward). One panel contains switches for power to the AEtnacars, automatic and manual control of recording; it also contains a socket for a Jones Plug which is used in automatic recording. This is accomplished by notches on the film at key points. The other panel contains the push button controls for: (a) the nine manual scoring groups, and (b) paper feed. Thus the teacher may check manually reactions to the film or skills and habits may be checked on the command of the teacher.

2. *The printing mechanism* is in the top part of the cabinet where it records the actions called for and records those performed. This is a motor driven revolving stamp pad with ten stamps corresponding to ten scoring groups [eight scoring groups on present models]; a unit of sixty magnetically operated plungers which print the scores; and a score sheet which automatically feeds as the operations are recorded.

3. *The master switch* is located below the control panels, and is of the circuit breaker type.

4. *The score sheet* . . . is a permanent record of each student's performance. It forms a basis for individual diagnosis and classroom discussion. Each vertical column represents the performance of one student. A

horizontal column presents the reactions of the entire class on one of the ten [now eight] scoring groups. The sheet is perforated between each scoring group for easy separation.

C. A 16mm Motion Picture Projector

This projector is placed on top of the [control] cabinet adjacent to a Servofilm unit.

D. The Servofilm Unit

This unit is a continuous feed mechanism which eliminates rewinding of the film.

E. The Projection Screen

This screen [should] be at least 8′ from the nearest AEtnacar, and four feet from the floor with the maximum viewing angle not to exceed forty-two degrees.

F. The Eighteen Instructional Films [there are now 24 films]

These training films, developed for use in the AEtna Drivotrainer, were educationally planned to teach the . . . skills of driving. Each film is a complete lesson in itself and links new material to that already taught in previous films. The films are notched for automatic operation of the recorder.

Two techniques are used in these instructional films. The first of these is termed, 'first person.' In first person scenes or films, the student is actually driving. The second of these is termed, 'third person' or 'third person demonstration.' In third person scenes or films, an instructor is demonstrating the driving lesson . . . . A film may utilize one or both of these techniques. (5:16-23)

In the classroom, the students, seated in their AEtnacars, listen to a preliminary lecture on the content of the film they are about to see. In the procedure recommended by the producers of the device, the driver education teacher points out the significance of various parts of the film lesson. When all questions have been answered the film is started. The students, having adjusted their seats, make certain that the gear shift levers are in neutral position, depress their clutches, and start their engines. In response to filmed or recorded instructions they shift through the various gears, brake, accelerate, and turn. The teacher may circulate around the room assisting individual students as necessary, or he may withhold comment until the film is completed and use the record sheet to feed information back to the class. Upon noting a pattern of errors occurring, he may stop and discuss with the class the nature of the mistakes. When all questions have been answered, the film is started again. After the first run has been completed the film is re-run. The film may be repeated several times to provide students with more practice.

In a general way, the structure and function of the AEtna Drivotrainer have been described. There seems to be a great deal of realism built into this complex device and it appears to possess a high degree of face validity. In order to determine the true validity and reliability of the Drivotrainer, three studies sponsored by the AEtna Casualty and Surety Company have been conducted. Two of these, the Los Angeles* and Iowa studies, will be reviewed; the third, the New York City study,† was not published in time to be available for analysis.

The Los Angeles Study

"The purpose of this study was to determine whether students trained in a program involving the AEtna Drivotrainer would have the same driving ability as those trained by the present methods of the Los Angeles City Schools." (5:2)

For various reasons the initial 240 subjects in the study (divided equally between the control and experimental groups) were reduced to 143—75 in the control group and 68 in the experimental group. Of the original 240 subjects, six control and three experimental subjects dropped out during the first week, and 88 others (39 control and 49 experimental subjects) completed their instruction but were not included in the final tabulations. To be included in the analysis, the subjects had to complete their instruction and all pre- and post-tests. Unfortunately, the recording mechanism of this early model of the Drivotrainer failed to function properly during a part of the experiment. This fact, and the failure of some subjects to complete either the pretest or the post-test, presumably account for a large part of the loss of 39 control and 49 experimental subjects.

Apparently, these losses did not materially affect the statistical control of the variables which had been established. However, an attrition of approximately 40 percent of the subjects would call for a cautious and tentative interpretation of the results. Although there is no statistically significant difference between the proportionate losses in each group, their magnitude suggests the need for further research before the conclusions of the study can be unqualifiedly accepted.

With the exception of the sex distribution, there is unfortunately no description of the characteristics of the drop-outs. It is not known whether they differed significantly from those included in age, grade level, or previous driving experience. It is shown, however, that the proportions of boys and girls who dropped out of each group were not significantly different from the proportions in the original groups.

Four conditions governed the selection of the original 240 subjects: (1) That the student be at least 15 years and six months of age; (2) That the student shall have taken or be taking a course in driver education;

* Another Los Angeles study concerning the Drivotrainer was not available in time for analysis: *An Experimental Study of the Teaching Effectiveness of an Electro-Mechanical Device and Conventional Driver Training Methods* by Louis I. Bernoff. (Doctoral dissertation, University of Southern California, 1958. 248 p.)

† Forlano, George, and Wrightstone, J. Wayne. *An Evaluation of the AEtna Drivotrainer in Selected New York City High Schools.* Divisional Bulletin No. 3. New York: Board of Education of the City of New York, October 1959. 14 p. (Offset.)

(3) That the student shall have had no driving experience at the time he began his instruction; (4) That the student could be enrolled from a study hall. Because of a scarcity of students meetings these conditions, the third —no previous driving experience—had to be waived.

The final groups of 68 experimental and 75 control subjects were compared on the basis of six variables: size of groups, grade placement, sex, age, IQ, and previous driving experience. It was determined with one exception (there were significantly more students from grade 11A in the experimental group) that the two groups were statistically equated for these variables. This being so, any differences between the groups could not be attributed to these factors.

The study design called for six proficiency criteria to measure the differences between the experimental and control groups. These were: the *Siebrecht Attitude Scale; Achievement Test in Driver Education for Man and the Motor Car,* (5th edition); *Achievement Test in Driver Education for Sportsmanlike Driving* (Form 5A); *AEtna Road Test; On-the-road Test;* and *Skill Test.* The first three criteria are paper-and-pencil tests; the last three are skill tests.

The *Siebrecht Attitude Scale,* the *Man and the Motor Car Test,* the *Sportsmanlike Driving Test,* and part of the *AEtna Road Test* were administered as pretests. After instruction, the attitude scale, the two knowledge tests, the complete *AEtna Road Test,* the *On-the-road Test,* and the *Skill Test* were given as post-tests. The *Skill Test* results were eliminated from the study, however, because clerical errors invalidated some of the scores.

The instruction of the control group consisted of six hours of observation in the car and six hours of actual practice at the wheel of the car according to the then current plan of instruction in the Los Angeles City high schools. Thus, each student in the control group had a total of 12 hours of practice driving instruction.

The instruction of the experimental group consisted of 16 hours in the Drivotrainer, six hours' observation in the car, and three hours of actual practice at the wheel of the car. Thus, each student in the experimental group had a total of 25 hours of practice driving instruction.

The research design called for the experimental and control groups to be compared on the pretest proficiency criteria; then for the groups to receive their instruction; and finally, for the comparison of the groups on the post-test proficiency criteria.

On the pretest there were no significant differences between the groups in attitude, knowledge, and driving ability as measured by the *AEtna Road Test.* With one exception, the post-test scores revealed no significant differences between the groups. It can be concluded, therefore, that the 25 hours of instruction received by the experimental group, in classes of eight per period under one teacher for the Drivotrainer portion, was as effective as the 12 hours of in-the-car instruction received by the control group, in classes of two per period under one teacher. Thus, less teacher time was required in the experimental program to achieve essentially the same effectiveness.

The experimental subjects did significantly better than the control subjects on the AEtna Drivotrainer final road test—a not unexpected result and one which indicates that practice on the Drivotrainer will yield improved performance on the Drivotrainer.

Mention should be made of the attitude measurement scores. The evidence indicates that the shift toward favorable attitudes was significantly greater for the experimental group than for the control group. However, the evidence also indicates that the mean post-test scores on the *Siebrecht Attitude Scale* were not significantly different for the two groups. Thus, it should be noted that the experimental group showed a significantly higher growth in attitudes than did the control group even though, after instruction, the two groups had equally favorable attitudes.

Comments on the Los Angeles Study

In general this study was rather well conceived and executed. However, there were certain inherent difficulties in its design. One limitation is that the universe to which the results are applicable was not adequately defined. The basic conditions for selection were that the subject had to be at least 15½ years old, to have had no previous driving experience (later waived), and to have taken or be taking a course in driver education. In addition, the subjects were selected on the basis of availability and interest. The results presumably would be applicable to all other students possessing these characteristics. However, it is not known whether the subjects used were representative of the driver education students in Los Angeles or in other locations throughout the country. Therefore, only tentative generalizations can be made.

The transfer-of-learning study design was called for in this experiment. The authors recognized this and incorporated some basic elements of such a design. However, results would be firmer and interpretations more nearly conclusive if in a future study the following elements were to be included:

1. Two additional experimental groups might be used: one to receive no Drivotrainer experience, six hours' observation time in the car, and *only* three hours' actual practice at the wheel of a car; the other, to have *only* the Drivotrainer experience and six hours' observation time. Comparisons could then be made among groups with only Drivotrainer experience and

those with varying degrees of practice driving instruction; and among the groups with combined Drivotrainer and practice driving instruction and those with only Drivotrainer experience or those with only practice at the wheel of a car. Such comparisons would permit statements about the relative merits of the various plans of instruction, thus leading to more precise identification of the contributions of the Drivotrainer.

2. Information about the distribution in the experimental and control groups of subjects who had had, and who were currently taking, a course in driver education would be useful, especially in interpreting the attitude data.

3. It would be helpful if some of the transfer-of-training formulas were used. These measures would provide an idea of the proportion of the instruction on the device which transfers to the actual task. Thus, the extent of the Drivotrainer's contribution to the learning of the actual task could be more adequately assessed.

4. An error analysis would contribute substantially to the theoretical and practical implications of the research. Such an analysis involves the designation of specific errors committed by the subjects in each group as they perform the transfer task. For example, the *On-the-Road Test* provides a quantitative measure of performance on various skills involved in driving a car. Do the experimental and control groups execute each of the skills with equal competence? This question could be answered through an error analysis, with the test items classified as to whether they call for procedural or adjustive responses. Such a classification would permit differences between the groups to be stated in statistical terms and would enable inferences to be made pertaining to the specific transfer contributions of the device, *i.e.*, whether they relate to procedural or adjustive responses (or both). In addition, an error analysis would reveal which parts of the task exhibit positive, negative, and zero transfer. Such information would be valuable to the makers of the Drivotrainer as well as to school administrators who have to decide whether to use the device.

5. Another variable which could be investigated by an error analysis is the influence of seating arrangement. There is some evidence in a previously cited study (34:249) which indicates that there is a relationship between distance from the screen and the amount of learning. Students who sat farthest from the screen had the poorest performance. If it were determined that students using the Drivotrainer have similar learning difficulties, then special attention could be given to those seated farthest from the screen.

It is realized that practical considerations may preclude the inclusion of additional experimental groups in a study. However, its value would be considerably enhanced if the other four elements could be made a part of the experimental design.

The Iowa Study

The stated purpose of the Iowa study (60) was very similar to that of the Los Angeles study: "Research with the AEtna Drivotrainer was undertaken at the Iowa State Teachers College in an effort to assess the worth of the device as a replacement for part of the dual-control-car instruction found in standard high school driver education programs." (60:3) A five-place Drivotrainer unit was installed in the college driver education laboratory. High school students from nearby Waterloo and Cedar Falls were used as subjects.

The experimental design called for comparisons between experimental and control subjects. Six separate sets of experimental and control groups were used. Although the use of six pairs of small groups was probably dictated by the equipment limitation, it had the advantage of providing repetitions of the experimental situation, thus serving to establish the reliability of the experiment.

Two proficiency criteria were used in the study: *The Iowa Driver's License Examination Score Sheet* which is used in initial driver licensing examinations in Iowa; and a *Final Road Test*. In the *Final Road Test*, developed by Woodcock, the student driver is rated on items drawn from state licensing examinations, items from other tests, and especially devised items. The validity of the *Final Road Test* was established, and its reliability for use with groups determined. The *Iowa Driver's License Examination Score Sheet* was used as a criterion for only the first pair of groups. The *Final Road Test* was used for all the pairs.

Various methods of controlling variables were used in this study. For the first pair of experimental and control groups, the subjects were matched for educational development, IQ, silent reading scores, grade point average beginning with sophomore year, family ownership of automobile, and sex. The subjects in the second pair of groups were matched for educational development, IQ, grade point averages in 8th grade academic subjects and personal traits, previous driving experience, and family ownership of automobile. The subjects in the third pair of groups were simply distributed randomly between the experimental and control groups. Distribution of subjects for each of the remaining pairs was described as random.

The instruction of the control groups consisted, on the average, of eight and one-third hours of basic instruction, 18 hours of observation in the car, and six hours of actual practice at the wheel of the car. (See Table 1.) The control students received a total of 32⅓ hours of instruction.

The instruction of the experimental groups consisted, on the average, of eight and one-third hours of basic instruction; 15 class periods (12½ hours) of Drivotrainer experience; 12 hours of observation in the car; and three and one-half hours of actual practice at the wheel. (See Table 1.) The experimental students received a total of 36⅓ hours of instruction.

The sizes of the various groups, the amount of instruction exclusive of basic instruction and observation time, and the results of the study are shown in Table 1.

TABLE 1.—RESULTS OF THE IOWA STUDY ᵃ

Group Number	No. of Students in Exp. Group	No. of Students in Control Group	Class Periods in Drivotrainer* Exp. Group	Class Periods in Drivotrainer* Control Group	No. of Hours of Dual-Control Instruction Exp. Group	No. of Hours of Dual-Control Instruction Control Group	Group with Higher Criterion Mean	Difference in Criterion Means Significant at 5% Level
1	9	9	16	0	3⅓	6	control**	no
1	9	9	16	0	3⅓	6	exp.	no
2	5	5	15	0	4	6	exp.	no
3	9	9	14	0	4	6	exp.	no
4	7	7	14	0	4	6	exp.	no
5	22	22	15	0	3	6	exp.	***
6	18	25	15	0	3	6	exp.	***
5&6	40	47	15	0	3	6	exp.	no

Source: Rhum, Gordon J.; Woodcock, Bertram L.; and Lamke, Tom A. *The Effectiveness of the AEtna Drivotrainer in Driver Education.* Cedar Falls, Iowa: Iowa State Teachers College, 1956. p. 16.

ᵃ Title provided.

* Class periods were 50 minutes in length.

** The criterion in this case was the score on the *Iowa Driver's License Examination Score Sheet.* In all other cases reported the criterion was the score on the *Final Road Test.*

*** The experimental design was such that groups 5 and 6 could be combined before a statistical test was made of the significance of the difference in criterion means.

In no case is there a statistically significant difference between the experimental and control groups. The authors conclude:

> The experimental design and the outcomes of the experimentation establish that, with the given criterion, result produced using the Drivotrainer and 3 hours of dual-control-car instruction were at least as satisfactory as the results produced by the standard method of instruction involving 6 hours of dual-control-car instruction. Since groups 3 through 6 were random samples of students who are taking driver education at the East or West Waterloo, Iowa, high schools, or both, the findings are applicable to any group of students with characteristics like those of the student body at these high schools. (60:16-17)

Comments on the Iowa Study

While the published report is not very extensive, several comments can be made. The authors designated the universe to which the results are applicable, defining it as " . . . any group of students with characteristics like those of the student body at these high schools." An even broader definition is provided: "On the basis of present knowledge it seems probable that the findings would apply to any population of normal high school students in the United States. Additional research should be undertaken before these findings are generalized to include students with markedly unusual IQ's or physical defects, to include other age groups, or in the case of unusual socio-economic, social, or driving conditions." (60:17) Thus, it is assumed that the usual group of high school students would respond in a manner similar to the experimental subjects. This assumption would be considerably strengthened if the basis for the selection of the subjects were presented. It is stated that groups three through six were random samples of students taking driver education at two high schools. What random process was used in their selection? For example, how were the 18 subjects in group three chosen? Did they volunteer? Were they arbitrarily assigned? In other words, was there a selective factor operating in the composition of the various groups?

Controls were established to equate the first two sets of groups; reliance was placed on random assignment to equate the other groups. In the latter cases, it would be helpful to know whether any tests were conducted to determine their equality. Further, it is not known whether the experimental or control groups differed in any way from either the driver education students or the student population in general.

The comments about the Los Angeles study also apply to the Iowa study. Transfer-of-learning theory provides the basis for the research. Additional groups, including subjects with no prior instruction and those taught according to various combinations of instructional plans, would have strengthened the conclusions. The use of transfer-of-training formulas would have indicated the extent of transfer which occurred, and the contributions of the Drivotrainer could have been more readily assessed.

Error Analysis (Iowa Study)

It has been pointed out that the value of the Drivotrainer studies would have been increased if error analysis had been performed. The authors of the Iowa study (60) have provided data on the errors committed on the *Final Road Test*. (See Appendix.) The numbers of errors made by 77 control group subjects and 70 experimental group subjects were recorded. These data can be used to demonstrate the value of an error analysis.

Conclusions drawn from the analysis should be considered as tentative; firm conclusions would require that error analysis be a part of the original study design. For example, the graders would require special instructions for recording errors; ambiguities would have to be eliminated from the proficiency criteria; and provision would have to be made for the number of trials required to reach proficiency. However, as they now exist, the data can be used to test, tentatively, the conditions of transfer which have been presented.

The error data have been placed in five categories: Adjustive Response Errors; Procedural Response Errors; Steering Errors; Judgment Errors; and Hazardous Acts Errors. Some items such as "Stalled Motor" could not be classified because of their ambiguous nature, *i.e.*, Did the subject's car stall because he tried to start in third gear (a procedural response error), because he fed too little gas (an adjustive response error), or for some other reason? If the reason for the stall were known, then the error could be placed in the appropriate category.

Items for the first two categories, Adjustive Response Errors and Procedural Response Errors, were the most difficult to classify. Adjustive responses are those in which perceptual feedback is important; procedural responses are those in which such feedback is relatively unimportant. An adjustive response item would be: "Use of clutch, released too quickly." "Did not adjust seat" is an example of a procedural response item. A panel of six judges classified the items on the *Final Road Test* as requiring either adjustive or procedural responses. Majority opinion in some cases, consensus in most, determined the classification of the items.

The following tables show the classification of the items (excluding those for which neither group committed an error).

TABLE 2.—ADJUSTIVE AND PROCEDURAL RESPONSE ERRORS OF 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS

Item	Number of Errors		Type of Response
	Control Group	Experimental Group	A=Adjustive P=Procedural
CHECKING			
Did not adjust seat	0	1	P
Did not adjust mirror	12	11	P
STARTING MOTOR			
Did not depress clutch	6	7	P
Did not check gear lever	8	8	P
Starter—not long enough	0	2	A
too long	2	2	A
Raced motor	6	5	A
Did not feed gas	9	6	P
STARTING CAR			
Use of clutch			
Released too quickly	13	17	A

TABLE 2.—ADJUSTIVE AND PROCEDURAL RESPONSE ERRORS OF 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS (continued)

Item	Number of Errors		Type of Response
	Control Group	Experimental Group	A=Adjustive P=Procedural
Released too slowly	11	6	A
Improperly depressed	4	1	P
Use of accelerator			
Applied too suddenly	4	1	A
Too much gas to start	14	10	A
Too little gas to start	16	10	A
Held down between shifts	3	0	P
Use of gears			
First gear—went too far	16	15	A
not far enough	0	2	A
too fast	1	2	A
too slow	2	1	A
Second gear—went too far	10	5	A
too fast	0	2	A
too slow	3	0	A
Third gear—too slow	4	0	A
rode clutch	4	0	P
Started with hand brake on	2	3	P
Fed gas in spurts	12	7	A
Clashed gears	6	6	A
Selected wrong gear	6	4	P
Eyes down when shifting	5	6	P
STOPPING CAR			
Too hard on brake	10	8	A
Forgot to use clutch	7	5	P
Too soon with clutch	4	4	A
Late with clutch	4	5	A
DRIVING IN TRAFFIC			
Drove too fast	1	2	A
Drove too slow	6	2	A
Too close to car in front	2	6	A
Braked too suddenly	9	8	A
Too fast at intersection	10	11	A
Rode clutch	9	3	P
Tried to start out of gear	5	0	P

TABLE 2.—ADJUSTIVE AND PROCEDURAL RESPONSE ERRORS OF 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS (continued)

Item	Number of Errors: Control Group	Number of Errors: Experimental Group	Type of Response A=Adjustive P=Procedural
PARKING			
Did not set hand brake	1	1	P
Too slow getting in	5	3	A
STARTING ON HILL			
Improper use of clutch/brake	7	2	A
Not enough gas	8	2	A
Too much gas	4	1	A
Too slow, held up traffic	4	2	A
Jerked passenger	8	9	A
SIGNALING			
No signal for left turn	6	7	P
No signal for right turn	3	3	P
No signal for stop	34	21	P
No signal from parking area	16	11	P
No signal continuously, 100 ft.	12	9	P
TOTAL	354	265	

Source: see Appendix.

TABLE 3.—STEERING ERRORS OF 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS

Item	Number of Errors: Control Group	Number of Errors: Experimental Group
STARTING CAR		
Unsteady on steering	6	5
DRIVING IN TRAFFIC		
Drove too far to the right	7	5
Drove too far to the left	6	2
Cut corner on right turn	10	1
Cut corner on left turn	27	17
Too wide right turn	15	5
Too wide left turn	3	6
Improper steering	4	7
TOTAL	78	48

Source: see Appendix.

TABLE 4.—JUDGMENT ERRORS OF 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS

Item	Number of Errors: Control Group	Number of Errors: Experimental Group
STOPPING CAR		
Stopped in wrong position	7	4
DRIVING IN TRAFFIC		
Blocked pedestrian lane	5	6
PARKING		
Too far from curb	12	10
Poor judgment of direction	9	5
Poor judgment of distance	16	12
Bumped other car or curb	11	10
SIGNALING		
Bumped curb	2	0
TOTAL	62	47

Source: see Appendix.

TABLE 5.—HAZARDOUS ACTS ERRORS OF 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS

Item	Number of Errors: Control Group	Number of Errors: Experimental Group
DRIVING IN TRAFFIC		
Failed to look for other cars	18	17
Ran through stop sign	2	0
Violated arterial stop sign	0	2
Went through on amber	1	0
Started up on amber	1	1
Passed in wrong places	1	0
Inconsiderate of motorists	4	1
TOTAL	27	21

Source: see Appendix.

V. DRIVER EDUCATION TEACHING DEVICES

Chi-square tests were made to determine whether the differences in the numbers of errors committed by the control group and the experimental group were significant for the various categories. Chi-square indicates the probability that the observed differences in the number of errors could be explained on the basis of chance variations. Generally, a P value of .05 (5 percent level) or less is considered significant. That is, only once or less in 20 times would the observed difference in the number of errors occur because of chance—the difference, therefore, being considered significant at the 5 percent level.

Another statistical measure used is the percent transfer. Since the number of errors is being measured (a larger value means poorer learning), the following transfer formula is appropriate:

$$\text{Percent Transfer} = \frac{\text{Control Group Score} - \text{Experimental Group Score}}{\text{Control Group Score} - \text{Total Possible Score}} \times 100$$

Since the control group (N = 77) is 10 percent larger than the experimental group (N = 70), the number of errors committed by the experimental group has been increased by 10 percent. In addition, since an errorless trial would result in a score of zero, the total possible score is zero. The procedural response category will show how the formula has been applied.

$$\text{Percent Transfer} = \frac{CGS - [EGS + .10\ (EGS)]}{CGS - TPS} \times 100$$

$$= \frac{152 - 118}{152 - 0} \times 100$$

$$= 22 \text{ percent}$$

TABLE 6.—SIGNIFICANCE OF DIFFERENCE AND PERCENT TRANSFER BETWEEN 77 CONTROL AND 70 EXPERIMENTAL SUBJECTS FOR FIVE CATEGORIES OF ERRORS

Category	Number of Errors: Control	Experimental	Total	df	Chi-Square	P	Significance of Difference	Percent Transfer
Procedural	152	107	259	1	4.22	.05>.02	Significant	22
Adjustive	202	158	360	1	2.34	.20>.10	Not Significant	14
Steering	78	48	126	1	4.20	.05>.02	Significant	32
Judgmental	62	47	109	1	.75	.50>.30	Not Significant	24
Hazardous	27	21	48	1	.19	.70>.50	Not Significant	15

Table 6 presents the results of the statistical analysis. It is to be noted that for the procedural responses the experimental group exhibited significantly fewer errors than the control group. However, there was no significant difference between the numbers of adjustive errors committed by the two groups. These findings tend to confirm the conditions of transfer reviewed earlier in this report. The numbers of judgmental errors made by the two groups were not significantly different; this finding, too, fits in the theoretical framework developed. The significant difference in the numbers of steering errors committed by the groups was not anticipated. On the one hand, steering is a type of adjustive response where perceptual feedback is very important; on the other, steering is a type of tracking which (as has been shown) did tend to transfer. Perhaps the explanation lies in the similarity between the actual task and the film portrayal; at any rate, it is apparent that positive transfer in steering ability did occur from practice on the device. Finally, there was not a significant difference in the numbers of hazardous acts errors made by the two groups.

The percent transfer values indicate that positive transfer occurred in all categories, and significantly so for procedural responses and steering. For the other categories the proportion of positive transfer was not greater than could be expected on the basis of chance, and they exhibited therefore, zero transfer.

While the conclusions drawn from this error analysis are tentative and should be accepted with caution, the results nevertheless indicate the nature of the special contributions which the Drivotrainer might make to the driver education program. Additional research could point out the specific strengths and weaknesses of the device and of the syllabus used.

The Los Angeles and Iowa studies have contributed to our knowledge of driving simulators. They provide a basis for additional research which will permit further determination of the effectiveness of the Drivotrainer. The questions raised in this report are designed to show some of the ways in which additional research would be helpful to the manufacturers and users of the device. It is realized that limitations of time, funds, and personnel may have precluded the suggested types of analyses; nevertheless, it should be recognized that these studies are important pilot investigations of the possible advantages of using driving simulators.

Application of the Conditions of Transfer

The conditions of transfer presented earlier provide a theoretical framework for further analysis of the effectiveness of the Drivotrainer. The application of these principles will serve to indicate what can reasonably be expected of the device. Manufacturers and users of the device as well as others may expect a level of performance which is beyond the capability of *any* teaching device. Reasonable expectations can be derived from the application of the conditions of transfer to the Drivotrainer research studies.

With reference to the following statements, the reader should keep in mind that a substantially greater number of students can be accommodated in a given period of time by one teacher using the experimental group plan of instruction than would be the case with one teacher using the control group plan of instruction. Also, there is the factor of cost of automobiles as against cost of driving simulators needed to accommodate a given number of students in the same period of time.

The Los Angeles study (5) demonstrates that a student with 25 hours of combined instruction (Drivotrainer and dual-control car) will perform as effectively as a student with 12 hours of instruction in the car. The Iowa study indicates that a student with 36 hours of combined instruction will perform as effectively as one with 32 hours of regular instruction. Stated in another way, the experimental subject with three hours of actual practice at the wheel of a car, plus other instruction, achieved the same level of proficiency as the control subject with six hours of actual practice in the car and six hours of observation time. It is evident, then, that some positive transfer occurred from instruction on the device to the actual task, and it is appropriate, therefore, to apply the conditions of transfer. It will be necessary to apply the conditions in the form of hypotheses—hypotheses which, however, are derived from studies in related experimental situations and from the Iowa Study Error Analysis.

1. *There is differential transfer among the various parts of the total task.* It can be assumed that there was not total positive transfer from the instruction task to the actual task; if this were so, there would be no need for the three hours of practice in the car. Maximum effective use of the Drivotrainer requires the determination of which skills transfer, which do not, and which interfere with the performance of driving a car. For example, the behavior involved in selecting the correct gear may transfer positively; the behavior involved in not racing the motor may not transfer at all; and finally, the necessity to look straight ahead in order to view the film is likely to interfere with the requirement to look ahead, to the sides, and in the rearview mirror while actually driving. In the latter instance the student would have to learn a new response to an old stimulus—a condition inducing negative transfer.

2. *Procedural responses are more likely to transfer than are adjustive responses.* Procedural responses are those in which perceptual feedback is of minimal importance. For example, the student either shifts to neutral before starting the engine or he doesn't; he does not partially shift to neutral, and then make an adjustment, and then shift a bit more, and make another adjustment, and so on. These types of responses, procedural in

nature, exhibit positive transfer. On the other hand, those which require continuous adjustment in response to perceptual feedback information tend not to transfer, although the error analysis discussed earlier did show a significant degree of positive transfer for the category of steering.

The graduated amount of brake pressure necessary to bring a car to a smooth stop for example, is dependent upon the speed of the vehicle, other traffic, the brake adjustment, and grade (up, down, level) of the road. While it is true that the brake pressure is either applied, or is not applied (and in this sense this is a procedural response), once it has been applied the response becomes adjustive—the amount of pressure varying with the perceptual factors mentioned. Thus, the adjustive application of the brake to the conditions present (including the slowing of the vehicle) is not expected to have a significant transfer from the Drivotrainer. In this adjustive response—varying the brake pressure—the hypothesis of no transfer is reinforced by the lack of correspondence between the film and the motor behavior of the student. That is, whether or not the student applies the brake, the film will portray the car slowing down. It is realized that the lack of correspondence is dictated by the requirements of mass instruction; nevertheless, this lack will affect the amount of positive transfer from the device.

3. *The task on the synthetic training device does not have to be a precise and faithful reproduction of the actual task for positive transfer to occur.* The fact that some transfer from the Drivotrainer did occur is adequate demonstration of this condition. However, the question remaining is this: do other less elaborate devices such as simple mock-ups prove just as effective? The studies presented in Section III suggest this. Future research may reveal that transfer may result from practice on a device which emphasizes *only* procedural response instruction.

4. *A synthetic training device does not have to reproduce kinesthetic feedback faithfully to be effective.* While some tension on the controls of a driving simulator is desirable, there does not appear to be the need for calibrated tension. For example, the recording of brake pressure from no brake to hard brake, or the recording of accelerator pressure from no throttle to full throttle are refinements which may add very little to the transfer value. The sequence of responses is more important than control pressure in helping the student acquire a desirable habit pattern. That is, the sequence of movements necessary to start the car and to shift through the gears is more likely to exhibit positive transfer than is the "feel of the controls."

5. *Realism does not necessarily increase the validity of a device.* This is the problem of face validity. It is recognized that the correct use of directional signals is an important part of the habit sequence involved in turning a car. It must be taught as part of the lessons on turning maneu-

vers. However, whether the skill is sufficiently complex to justify the cost of recording errors should be determined by additional research. The presence and functioning of the directional signals are highly desirable; the need for a mechanism to record errors in signaling is much less certain. The same point can be made in regard to learning the skill "use of the horn" on the Drivotrainer. Is there a significant difference between trained and naive students in the errors made and time required to reach proficiency in this skill? While, undoubtedly, the presence of the directional signals and horn ring increases the face validity of the device, they may not increase its true validity. The other skills which the student learns on the device should be subjected to the same test—*i.e.,* do trained students achieve proficiency in fewer trials than naive students? In this way, those characteristics which contribute solely to face validity can be isolated and, perhaps, modified.

There is, however, another facet to the problem of face validity. If it is determined that a characteristic of the device contributes primarily to face validity, but that it is also important to the student as a motivational factor or as a means of his more readily acquiring a desired habit sequence, then the characteristic should be retained. In the examples cited, the horn ring and directional signal lever might be retained for their motivational quality and habit sequence value if these are shown to exist on the basis of research.

6. *The presence of extraneous cues in the training task will inhibit positive transfer.* It does not appear that the Drivotrainer includes many extraneous cues that would be likely to influence the student's performance. One possibility is that the clicking of the recording device might remind some students that a maneuver should have been made, *e.g.,* shifting from second to third gear. However, since the click cue would occur after the maneuver was supposed to have been completed, it would not affect his score. In an actual driving situation, if the student forgets to shift from second to third gear, some cue other than the clicking of the recording machine would have to be used.

Description of the Auto Trainer

The American Automobile Association's Auto Trainer is another driving simulator. In general, it consists of a driver's compartment with regular controls, a continuous canvas belt (mounted in front of the compartment) which moves in response to the accelerator and brake, and a miniature car which can be steered over a roadway painted on the belt. Specifically, it has been described as follows.

> This trainer consists of the conventional controls such as the gearshift lever [for either standard or automatic transmission], accelerator, brake, clutch, ignition switch, starter button, etc., which control the speed and direction

of a roadway painted on a belt. A steering wheel controls the front wheels of a miniature car which rides on the moving roadway.

Power is provided by a 1/6 H.P. variable speed motor and is transmitted through a transmission with three forward speeds and reverse. Inertia is provided by a 15# flywheel.

The car and roadway are scaled about 1" equal to 20". Twenty grease-sealed ball bearings are used at major points of wear. A speedometer gives approximate speeds. A sign changer presents a different instructional sign for each revolution of the belt. A miniature traffic signal ordinarily shows green, but can be changed to red by the instructor.

The current model has two timers and a counter. One timer measures the total time to complete the test. The other timer gives the average of six reactions to a red light. The reset electrical counter gives a measure of steering ability by recording one unit for each 6 inches of travel during which the car is kept properly centered in the traffic lane.

With the Auto Trainer it is possible to practice such exercises as starting the engine, starting and stopping the car, gear shifting, steering and parallel parking.

Eight charts are provided for self instruction. Each chart gives step by step procedures for a given lesson so that the student can follow these steps with very little assistance from the instructor. (4)

Each Auto Trainer is a single-place unit and requires about 3 feet by 9 feet of floor area. Several single-place units can be used simultaneously, thus creating a multi-place installation.

Two studies of the effectiveness of the Auto Trainer as a training device will be reviewed.

The Springfield, Pennsylvania, Study

"This study was undertaken to determine the value of the Auto Trainer in the development of the knowledge and skills regarded as prerequisite to the driving of an automobile on the public highways." Or, "Specifically, the study was concerned with the extent to which the Auto Trainer may be expected to reduce the time otherwise spent in a training car." (42:1)

The study design called for two groups of students: one, the experimental group, received Auto Trainer experience; the other, the control group, received the regular instruction. Records of the amount of time spent by each student behind the wheel of the Auto Trainer and in the practice driving car were kept. After instruction, each student was given an on-the-road driving test. The experimental and control groups were then compared for the relationship between amount of instruction time and driving ability.

The driving ability necessary to handle the Auto Trainer was also determined. Three groups of students were tested. The first received enough training on the device to control the model car in the basic maneuvers.

The second had received all their training in the practice driving car and had 20 minutes' instruction on the Auto Trainer. The third group were drivers who had at least one year of driving experience and practiced 10 minutes on the device. Each group was then given the Auto Trainer Test.

The road test scores for the experimental and control groups were compared with their scores on the Auto Trainer test in order to determine whether the latter measured driving ability.

The Auto Trainer was used in the initial instruction of the experimental group. The various lessons were demonstrated, explained, discussed, practiced, and reviewed. The instructor sat beside the student during practice on the Auto Trainer.

The subjects were juniors and seniors. Thirty were given Auto Trainer experience, and 25 received the regular instruction program. Only students with minimum driving experience were used in the study. Originally there were two experimental groups and two control groups; however, for purposes of analysis the experimental groups were combined as were the control groups.

The two proficiency criteria used to measure the effectiveness of instruction—the Auto Trainer and the regular program—were the Auto Trainer test and a road test given in traffic.

The experimental groups were tested for steering, steering wheel movements, errors, and reaction time on the Auto Trainer when they had completed their experience on the device. After their instruction in the practice driving car, they were again tested on the device. Error scores were kept on such items as attempting to shift without depressing the clutch. The subject was also asked to give an honest opinion of what he had learned on the device after his instruction had been completed and he had received his driver's license.

The road test was divided into three parts.

A. Handling of the car. This consisted of checking the vehicle, checking the driver, starting engine, starting the vehicle in low, backing, clutching and shifting gears, steering, and smoothness of operation (Part I).

B. Attitude and observance of the law. This consisted of railroad crossing, speed control, stopping, stop streets, uncontrolled intersections or through streets, signaling failures, signal violations, and passing other vehicles going the same direction (Part I); inattentive, nervous, overconfident, fails to use rear view mirror, and fails to anticipate or respond to hazardous driving conditions (Part II).

C. Road position. This consisted of traffic lanes, turning right, and turning left (Part I). (43:24)

On the basis of his analysis of the data, the author arrived at the following conclusions:

1. Approximately 148 minutes of instruction on the Auto Trainer will save 47.5 minutes' instruction time in the car. The road test scores were approximately the same for the experimental and the control groups.

2. There were no statistically significant differences between the two groups in driving ability.

3. The basic skills required in the instruction task and the actual task seem to be similar, for the Auto Trainer scores of the experimental group, the control group, and the experienced group of drivers were not significantly different.

4. Driving ability as measured by the road test scores was not related to driving ability as measured by the Auto Trainer scores. Three of the test items (errors, steering, and reaction time) showed a positive relationship, while the fourth (steering movement) showed no relationship. It is thus concluded that the steering movement scores lack reliability.

5. The students indicated a definite interest in the Auto Trainer for instructional purposes.

Comments on the Springfield Study

The unique purpose of this study was to determine how much training time could be saved through using the device. It was determined that the savings in time amounted to about 45 minutes. Since the road test was administered when the individual student demonstrated his competence, it would be erroneous to conclude that the 45 minutes could be substituted for an equivalent amount of the recommended six hours of practice driving instruction. The average time for the control group to reach the proficiency level was 538.4 minutes, or approximately nine hours.

The universe to which the results are applicable is not adequately defined. It is not clear how the subjects were selected nor, once selected, how their placement in either the control or experimental group was determined. It is thus difficult to generalize from the conclusions.

Transfer-of-learning theory also provides the basis for this study. The study design might well have been improved by inclusion of the transfer-of-learning formulas. The Auto Trainer test scores of the beginning students, along with those of the experienced drivers, provide some measure of abilities of naive and trained subjects. However, this measure would have been improved if an analysis of the kinds of errors made had been presented. In much the same light, an error analysis would have served to designate more precisely the Auto Trainer's contributions to the student's learning of the complex task—driving.

V. DRIVER EDUCATION TEACHING DEVICES

The Washington, D. C., Study

A more recent study (3) of the Auto Trainer's effectiveness was made at the Anacostia High School in Washington, D. C. "This study was conducted primarily to learn if the Auto Trainer could be used to give instruction to several students at one time, thus increasing the number of students one instructor could teach during a semester." (3:2) The study is in two parts which will be considered separately.*

In Part I, 50 subjects were taught. Twenty-nine received Auto Trainer instruction, and 21 followed the regular program. The experimental group, on the average, practiced 7.8 clock hours and had 12.9 hours of observation on the Auto Trainer. In addition, this group had 4.07 hours of actual practice in the car, while the control group practiced 5.67 hours in the car.

Comparisons between the experimental and control groups were made for the following variables: sex, age, grade in school, driving experience (percent having over eight hours before starting course), bicycle use, steering experience (average hours steered while another drove car), and family car possession. For only one of these variables was there a statistically significant difference between the groups—a significantly greater proportion of the control group had at least eight hours of previous driving experience.

A series of tests was administered before the instruction programs began. The series included knowledge tests, attitude scale, IQ level, and various psychophysical tests. There were significant differences in favor of the experimental group for reaction time and IQ level.

After instruction the groups were given four tests: (1) *Driving Knowledge*—AAA test of 25 questions; (2) *Driving Knowledge*—AAA test form 3-A, 50 questions; (3) attitudes—*Siebrecht Attitude Scale;* and (4) a road test. Only for the 50-question *Driving Knowledge* test was there a significant difference between the groups—and this difference favored the Auto Trainer group.

In order to determine the effects of the differences between the two groups for the variables, a series of intercorrelations was run. There were significant positive correlations between IQ level and attitude (first test), between IQ level and knowledge test form 3-A, between years of bicycle experience and the road test, between knowledge (first test) and the road test, and finally, between the ratings on the sections of the road test and the examiner's over-all rating. Significant negative correlations were found between reaction time and the road test, between errors made on the road

* Parts III and IV of this study were not available in time for analysis, but are referenced in the bibliography (3). All four parts are summarized in *Effectiveness of a Driving Simulator, 1956-1958*, 1959. 4 p. This summary includes a graphic comparison of errors made by the control and experimental groups on 21 items.

test and the rating on the section of the road test, and between errors made and the examiner's rating.

The authors conclude that "Since the performance of the two groups was about equal, it would appear that 7.8 hours on the Auto Trainer was about equal to 1.6 hours behind the wheel of the car." (3:2) Although the control group had a significantly higher proportion of subjects with at least eight hours of driving experience, it performed no better than the experimental group on the road test. In much the same way, although the experimental group had a significantly higher intelligence rating, it performed no better than the control group.

In Part II, 59 students were taught: 17 of these were in the control group and 42 were in the experimental group. The control group had 6.72 hours of practice driving in the car; the experimental group had 4.00 hours.

Again, comparisons between the groups were made on the basis of sex, age, grade in school, driving experience, bicycle experience, steering experience, and family car possession. The control group had a significantly greater proportion of boys; while the experimental group was significantly older, was significantly further advanced in school, and had a significantly larger proportion with previous driving experience.

The same series of tests was administered before instruction. On only two of the psychophysical tests were there significant differences between the two groups: the experimental group excelled in tests of visual acuity (left eye) and steadiness. The tests given after training revealed no significant differences between the groups. It was determined, however, that students with more than 8 hours of previous driving experience made significantly better road test scores than did the others. Boys made significantly higher scores on the road test than did girls.

The authors point out that "Persons who did well on the final road test also were more advanced in their grade status ($r = .27$), had more years of bicycle experience ($r = -.30$), had a faster reaction time ($r = .34$), and did better on the driving knowledge test ($r = -.27$)." (3:2)

Comments on the Washington Study

It is indicated that the "Results included should be considered as applying to this group only and not generalized until substantiated by additional data from comparable studies." (3) On this basis it is not possible to make any inferences which might be applicable to other groups in other situations. All that can be said is that additional studies will have to be made to determine the reliability of the limited conclusions of this research.

It is not known whether any selective factors were present in the assignment of students to the control and experimental groups. There is also the question of whether eight hours might not be too crude a measure of driving experience. An error analysis * and the use of transfer-of-learning formulas would be extremely helpful in interpreting the results. While the various intercorrelations are useful, further analysis of the effects of the variables on the final performances would aid in measuring the effectiveness of the Auto Trainer.

Application of the Conditions of Transfer

The conditions of transfer are applied in the form of hypotheses. If these hypotheses were to be tested, then the specific contributions of the Auto Trainer to the learning of the driving task could be determined.

1. *There is differential transfer among the various parts of the total task.* As is true with any training device, it can be assumed that not all skills exhibit the same degree of transfer. Some of the skills learned on the Auto Trainer may facilitate the learning of the driving task; others may inhibit such learning.

2. *Procedural responses are more likely to transfer than are adjustive responses.* This condition of transfer would indicate that learning to manipulate the gear shift lever will transfer positively, while the responses involved in accelerator use will not. It will be recalled that adjustive responses are those in which perceptual feedback is very important.

3. *The task on the synthetic training device does not have to be a precise and faithful reproduction of the actual task for positive transfer to occur.* The Auto Trainer varies quite markedly in some respects from the actual task, yet some positive transfer did occur. It may be that the skills could be learned just as easily with a more simplified device.

4. *A synthetic training device does not have to reproduce kinesthetic feedback faithfully to be effective.* It is not necessary that a device, for teaching purposes, reproduce the sensation of motion. The manipulative skills can be learned in a stationary device. In the same manner, since the sequence of movments is most likely to transfer, the degree of kinesthetic feedback tends not to be important.

5. *Realism does not necessarily increase the validity of a device.* The Auto Trainer does not appear to provide a high degree of realism. In light of the conditions of transfer, however, this may not detract from its teaching value.

6. *The presence of extraneous cues in the training task will inhibit positive transfer.* It is not known whether extraneous cues are present in this

* A graphic comparison of errors made by the experimental and control group subjects is included in *Effectiveness of a Driving Simulator, 1956-1958* (3).

device. However, conceivably a "smudge" on the painted belt roadway could cue a student that a railroad crossing or other "hazard" is about to appear. Again, an intensive study is required to determine whether extraneous cues may be present in the task as performed on the device.

Summary

The purpose of this section has been to review the evaluation studies of the Drivotrainer and the Auto Trainer within the framework of the conditions of transfer presented earlier. Ways in which the various studies might have been strengthened were indicated.

In general, the evaluation research was systematically developed and possessed many of the design characteristics of transfer-of-learning studies. However, with the exception of the Iowa study, the universes to which the results are applicable were not adequately defined. Experimental and control groups were used; but the study designs would have been improved if additional groups had been included, one of these having only three hours of instruction on the actual task and the other having only 15 hours' training on the Drivotrainer or the Auto Trainer. It would then have been possible to compare the group with 15 hours of device training plus three hours' car training with a group which had only three hours' car instruction; and any differences between the groups would have reflected the effect of the device training. In much the same manner, if a group were taught on a simulator and then tested in the car, the skills acquired on the device could be specifically determined.

The value of the studies would have been increased if error analyses had been performed. The specific contributions of the simulators to the task of driving could then have been isolated. The determination of these contributions, in turn, would alert the teacher to those skills most likely to require special attention. Also, the use of transfer-of-learning formulas would have permitted greater precision in determining the specific contributions. These determinations, too, would provide the manufacturers of devices with the information necessary to improve the existing simulators.

To indicate its value, an error analysis was performed on the Iowa data. For the reasons presented, the conclusions of the analysis are very tentative. The results do show, however, that the Drivotrainer-taught students commit significantly fewer procedural and steering errors than do students in the regular program. The results also show that there are no significant differences between the experimental and control groups in committing adjustive, judgmental, and hazardous acts errors. If these findings are reinforced by additional research, the teacher, during the practice driving phase of driver education, would then be able to concentrate on the previously determined needs of simulator-trained students.

V. DRIVER EDUCATION TEACHING DEVICES

The Drivotrainer and the Auto Trainer are devices designed and intended to assist schools in the mass training of young people in safe driving. They have been found to aid such teaching in significant ways. To meet the manifest need for driver education, some type of effective and economical simulator seems desirable. The development of these two devices represents an important step toward meeting this need. Further effort should be made in the refinement of these devices, or in the development of comparable ones, so that their potential effectiveness may be more fully realized.

VI. SUMMARY

The purpose of this study has been to assist those responsible for driver education and those who develop and distribute driving simulators in evaluating the effectiveness of such devices. The decision to purchase a synthetic teaching device rests ultimately upon the relationship between learning outcome and cost. Among a number of reasons often cited for using a synthetic teaching device in driver education, the two major ones are: (1) the actual task is so hazardous as to preclude practice; and (2) the practice driving automobiles and personnel needed to teach large numbers of individuals are too costly. The latter is the primary justification for using driving simulators. The need to educate large numbers of high school students in safe driving is recognized; the availability of necessary funds for such teaching is less certain. A synthetic teaching device in driver education can help meet both needs; however, it must first be demonstrated that the device is providing the optimum transfer at minimum cost.

A synthetic teaching device must possess validity; that is, some desirable learning must occur through use of the device, and that which is learned must transfer to the task for which the learning is appropriate. The conditions of transfer presented in this report can be formulated as questions which, when answered, will provide some measure of the validity of a device.

1. Which of the various parts of the learning task exhibit positive, negative, or zero transfer? Even though there may be over-all positive transfer, some parts of the learning experience may not transfer, and other parts may interfere with the performance of the actual task. If this question is not satisfactorily answered, construction costs may tend to exceed the teaching value of the device. It is natural to assume that all parts of the task transfer equally and, therefore, to design a device accordingly, thus increasing its cost.

2. Has the device been constructed so as to facilitate the transfer of procedural responses? Certain procedural responses do transfer. It is important to determine the point beyond which increasing the complexity of the device adds nothing of significance to the transfer of these responses. If a simpler and less expensive trainer facilitates the transfer of procedural responses as effectively as a complex device, then the additional cost of the latter can not easily be justified.

3. Has the device been constructed so as to facilitate the transfer of adjustive responses? Adjustive responses tend not to transfer. If the device has been constructed in an attempt to achieve the transfer of these responses when research indicates that such transfer tends not to occur, then the design of the device might well be changed.

4. Does each characteristic of the device add significantly to its teaching value? Characteristics—*i. e.*, parts of a device—may be built in to make it more "realistic." If these characteristics only increase face validity and have no teaching or motivational value, they could probably be eliminated with concomitant savings in construction costs.

5. Does the device attempt to reproduce kinesthetic feedback accurately? It has been demonstrated that a device does not have to reproduce kinesthetic cues to accomplish its purpose with procedural responses.

6. Can the students make use of extraneous cues? Since the presence of extraneous cues in the use of a device may inhibit positive transfer, they should be eliminated.

7. Does the device provide the student with knowledge of his efforts, and is the scoring system reliable? The student should be informed of the results of his efforts soon after he has completed each trial. This factor relates especially to learning on the device, and not necessarily to the transfer of such learning to the actual task. However, learning must precede transfer and the device must include those factors most conducive to learning. For the student to gain accurate information about his progress on a simulator, the scoring mechanism must be reliable. Reliability tests should be conducted.

The review of the Armed Forces research indicates that answers to the above questions are necessary to determine adequately the instructional value of a synthetic training device. It has also been shown that answers can most easily be derived from error analyses which, in turn, permit the isolation of the specific contributions of a device to the acquisition of the required skills. Thus, to be most useful, the study design employed in evaluation research should include provision for error analyses. The conditions of transfer presented in this report provide a theoretical framework within which such analyses can be performed.

The review of the Drivotrainer and Auto Trainer research clearly indicates that some transfer of learning occurs when these devices are used; however, what kinds and how much has not yet been determined precisely. It would appear that instruction time spent on these devices can be substituted for part of the recommended minimum of six hours of practice driving instruction in a dual-control car. Whether optimum transfer value is being realized is not known. Certain next steps would be valuable both to the makers and to the users of driving simulators, as, for example:

1. Conduct additional research to determine specifically the kinds of behavior which transfer from the devices to the actual driving task. Error analysis based on the conditions of transfer should be an integral part of the research design from its inception.

2. Research that includes error analysis will reveal specific strengths and weaknesses of the simulators. The manufacturers will then have a basis for modifying the construction of the devices—perhaps eliminating or adding certain characeristics. These changes would serve to increase the effectiveness and possibly to reduce the cost of synthetic teaching devices for driver education.

3. Additional research would point the way toward improving the existing syllabuses and/or films used with the devices. In addition, special instructions for teaching students on simulators can be devised—instructions designed to fill the gaps in skill and judgment learning which practice on the devices may involve. The instructional value of the devices can be improved in these ways.

4. Another important question which should be considered in any further studies is that of the extent to which economy of student time (as well as teacher time) can be effected through the use of driving simulators.

Synthetic teaching devices can be used to increase the number of students receiving driver education at a lower cost than for the regular program employing real automobiles for practice driving instruction. Additional research is needed, however, to assure adequate transfer.

The conditions of transfer presented in this report reflect the current level of transfer theory. It is well to recognize that transfer theory appears to be on the verge of significant changes. Many of the conditions of transfer presented are based on stimulus-response constructs. However, recent developments in psychological theory view the operator as an integral part of a man-machine system, and may well point the way toward increasing significantly the transfer effects of simulator teaching. In this event new conditions of transfer will need to be developed and tested.

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APPENDIX

ERRORS COMMITTED BY 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS ON THE IOWA *FINAL ROAD TEST*

(Data supplied by Bertram L. Woodcock, Director of Safety Education, Iowa State Teachers College, Cedar Falls, Iowa)

Item	Number of Errors	
	Control Group	Experimental Group
CHECKING		
Did not adjust seat	0	1
Did not adjust mirror	12	11
STARTING MOTOR		
Did not depress clutch	6	7
Did not check gear lever	8	8
Forgot to turn on ignition	0	0
Starter—not long enough	0	2
too long	2	2
Raced motor	6	5
Did not feed gas	9	6
Wrong order	0	0
STARTING CAR		
Use of clutch		
Released too quickly	13	17
Released too slowly	11	6
Improperly depressed	4	1
Use of accelerator		
Applied too suddenly	4	1
Too much gas to start	14	10
Too little gas to start	16	10
Held down between shifts	3	0
Use of gears		
First gear—		
went too far	16	15
not far enough	0	2
too fast	1	2
too slow	2	1

APPENDIX — (Continued)
ERRORS COMMITTED BY 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS ON THE IOWA *FINAL ROAD TEST*

Item	Number of Errors	
	Control Group	Experimental Group
Second gear—		
went too far	10	5
not far enough	0	0
too fast	0	2
too slow	3	0
Third gear—		
too slow	4	0
rode clutch	4	0
Started with hand brake on	2	3
Fed gas in spurts	12	7
Stalled motor	20	16
Clashed gears	6	6
Selected wrong gear	6	4
Unsteady on steering	6	5
Eyes down when shifting	5	6
STOPPING CAR		
Stopped in wrong position	7	4
Too hard on brake	10	8
Forgot to use clutch	7	5
Too soon with clutch	4	4
Late with clutch	4	5
ATTITUDES		
Nervous	13	11
Hesitant	1	1
Easily distracted	1	2
Over-confident	1	2
Not alert	1	1
Discourteous	0	0
DRIVING IN TRAFFIC		
Failed to look for other cars	18	17
Drove too fast	1	2
Drove too slow	6	2

APPENDIX

APPENDIX — (Continued)
ERRORS COMMITTED BY 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS ON THE IOWA *FINAL ROAD TEST*

Item	Number of Errors	
	Control Group	Experimental Group
Ran through stop sign	2	0
Violated arterial stop sign	0	2
Ran through red light	0	0
Went through on amber	1	0
Started up on amber	1	1
Passed in wrong places	1	0
Cut in too soon—passing	0	0
Drove too far to the right	7	5
Drove too far to the left	6	2
Too close to car in front	2	6
Drove off roadway	0	0
Braked too suddenly	9	8
Inconsiderate of pedestrians	0	0
Inconsiderate of motorists	4	1
Blocked pedestrian lane	5	6
Cut corner on right turn	10	1
Cut corner on left turn	27	17
Too wide on right turn	15	5
Too wide on left turn	3	6
Improper steering	4	7
Too fast at intersection	10	11
Slouched when driving	0	0
Too tense when driving	11	5
Rode clutch	9	3
Tried to start out of gear	5	0
PARKING		
Too far from curb	12	10
Did not set handbrake	1	1
Too slow getting in	5	3
Poor judgment of direction	9	5
Poor judgment of distance	16	12
Bumped other car or curb	11	10

APPENDIX—(Continued)
ERRORS COMMITTED BY 77 CONTROL GROUP SUBJECTS AND 70 EXPERIMENTAL GROUP SUBJECTS ON THE IOWA *FINAL ROAD TEST*

Item	Number of Errors: Control Group	Number of Errors: Experimental Group
STARTING ON HILL		
Rolled back	18	8
Improper use of clutch/brake	7	2
Stalled motor	14	4
Not enough gas	8	2
Too much gas	4	1
Too slow, held up traffic	4	2
Jerked passenger	8	9
SIGNALING		
No signal for left turn	6	7
No signal for right turn	3	3
No signal for stop	34	21
If no signal—didn't look	0	0
Bumped curb	2	0
No signal from parking area	16	11
No signal continuously, 100 ft.	12	9
TOTAL	601	431

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