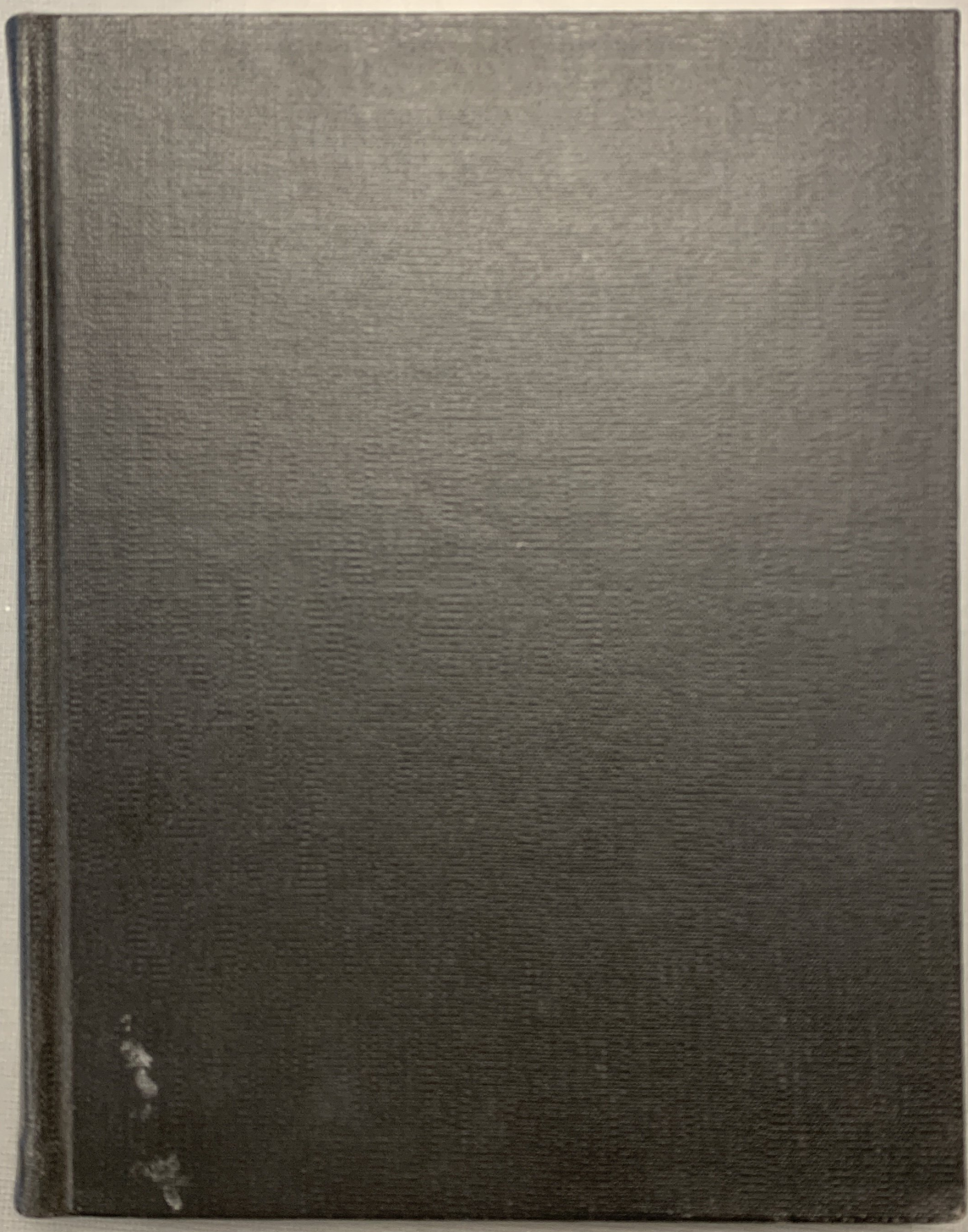
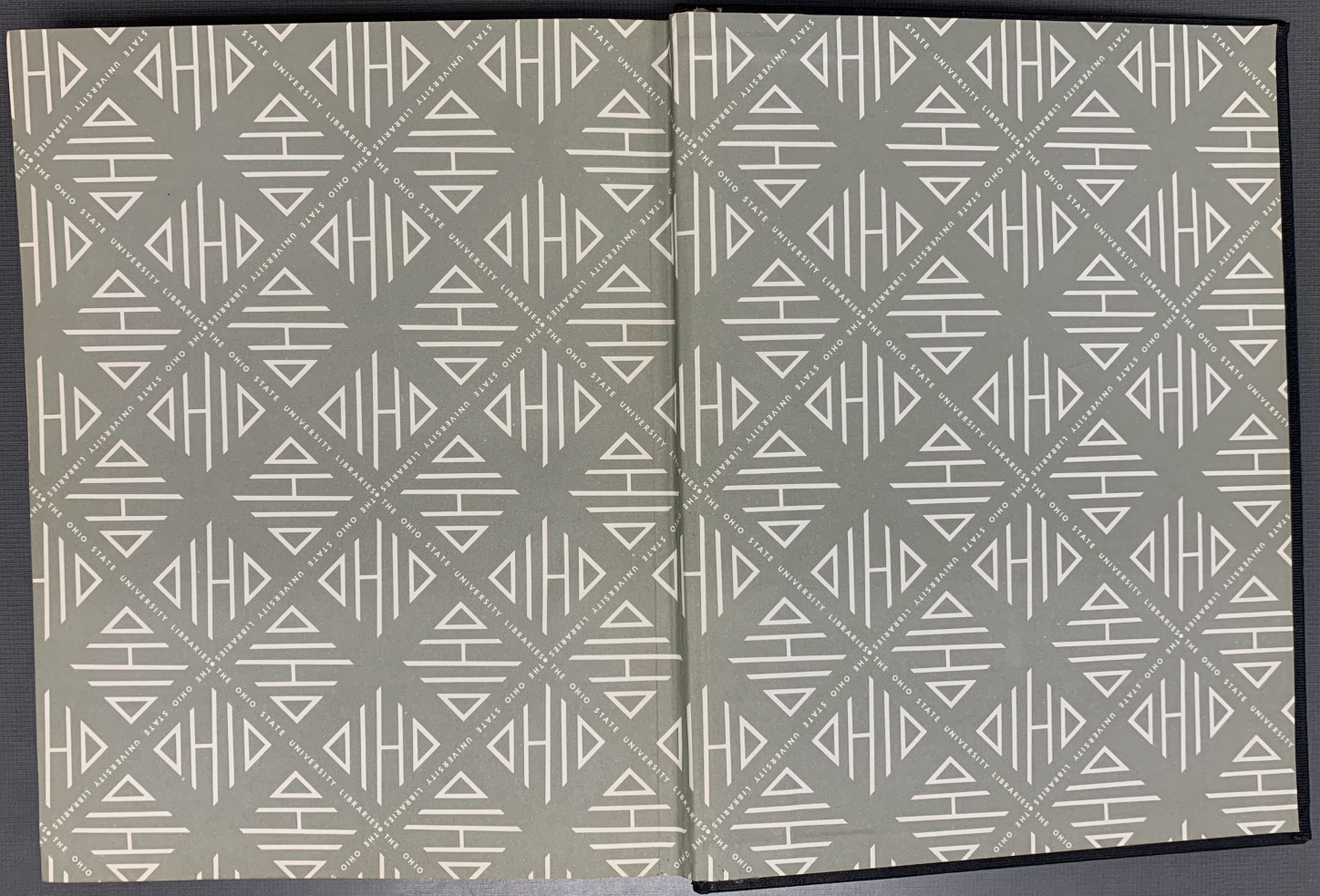



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VISUAL ESTIMATION OF VELOCITY IN CONNECTION WITH HIGHWAY SAFETY

DISSERTATION

Presented in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy in the Graduate School of the Ohio State University

By

THEODORE WATSON FORBES, B. A., M. A.

The Ohio State University

1931

Approved by:

Harold E Burtt

Adviser

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THE VISUAL ESTIMATION OF VELOCITY IN CONNECTION WITH HIGHWAY SAFETY.

Chapter I.

THE PROBLEM

The General Problem

The number of people killed per 100,000 automobiles which was decreasing until 1926, has since been rising regularly.(1)* At the same time deaths due to all other accidents have shown a gradual decrease. A Travelers Insurance Company study (34) indicates that in the United States gasoline consumption in 1930 decreased 10 percent, while automobile accidents increased 12 percent and automobile deaths increased 4 percent. The Policyholders' Service Bureau, Metropolitan Life Insurance Company, report (25) makes the following significant statement:-

"Accidents result from the failure of the average citizen to adapt previously formed habits to the rapidly changing conditions of life brought about by recent scientific, engineering and commercial developments."

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*Numbers refer to references to be found in the bibliography at the end of the dissertation.

It is felt, then, that the increased rate of automotive accidents and deaths is one in which the psychological factors are of greatest importance. The mechanical factors of the car itself have been pretty well perfected and contribute little to accident causation as may be seen from the following figures published by the Travelers Insurance Company.

Condition of Vehicle	Percent of Accidents
Apparently good	94.9
No chains (slippery)	1.4
Brakes defective	1.3
Other defects	2.4

The foregoing study and also that of Lauer and Forbes (21) finds that over 80 percent of the accidents studied take place in clear weather with dry road surface. It seems safe to suggest, therefore, that neither mechanical nor weather conditions are of chief importance in the majority of accidents, but rather certain factors in the reactions of the drivers.

The general psychological problem, then, involves determining the factors resulting in faulty reactions of drivers and thereby causing accidents. The findings may be applied later, perhaps, in the development of scientific tests which will (1) weed out

those who need training in driving habits and (2) eliminate entirely those who can never become competent drivers. An outlined program for such examination of drivers is suggested by Cattell in the Science Monthly, (1926) (7).

The Specific Problem

The part of the problem involved in the present study pertains to visual estimation of velocity on the part of the driver. It has two aspects:

(1) Are there individual differences in ability to discriminate velocity?

(2) Are such differences a factor in highway accidents?

Observation on the road yields situations such as the following in which it is conceivable that such differences in estimation of velocity might be a cause of accidents.

1. Cars approaching an intersection or a railroad crossing, trying to beat the vehicle crossing the line of travel, and colliding.

2. Passing a car or series of cars on the open highway with a car or series of cars coming in the opposite direction. While engaged in the study, the writer has noted several cases in which such judgment may have

been involved. For example in one case on the road to Dayton, Ohio, the car being passed was forced to apply the brakes abruptly to keep from being crowded into the ditch by a car whose driver attempted to pass and found himself caught by an approaching car. On a Virginia highway in December 1930 a car driver pulled out from behind another on a perfectly straight highway, and collided with an on-coming car on the right side, peculiar though that may seem, demolishing both cars rather completely. These are merely two cases where the writer happened to be on the scene, or arrived very shortly after the accident, and are given merely as rough observational data suggesting the possible importance of visual estimation of velocity.

3. Cars moving in opposite directions on street or highway, one car attempting to turn left across the path of the on-coming car, with an accident resulting.

4. Sudden deceleration by the car ahead causing a collision from the rear by the following vehicle.

5. A slow moving wagon being overtaken by one car with another car approaching in the opposite direction. In one such accident reported to the writer the

driver of the car following the wagon slowed down somewhat, planning to allow just enough time for the oncoming car to pass. He overestimated the speed of the wagon, did not slow down sufficiently and hit the wagon in the rear.

It is, of course, recognized that other factors such as carefulness, motor co-ordination, "emotional" or "disorganized" reactions, etc., are also likely to enter. However, if the driver can gauge accurately the velocities concerned, caution in the sense of hanging back due to lack of sureness should not be necessary, nor should motor co-ordination or emotional reactions be of such great importance in situations similar to the preceding, unless poor estimation of velocity has precipitated a critical situation.

It is probable that any such individual differences in ability to estimate velocity will be due in large part to training and experience (learning) as indicated in the following theoretical section.

Chapter II

HISTORICAL

General Theory.

Distance and Space Perception

Since motion involves change of distance as well as change of lateral space relations, it is pertinent to begin our discussion with the subject of distance and space perception. The best starting point is perhaps Berkeley's "New Theory of Vision"(6) Here Berkeley suggests that .."distance itself cannot be seen", criticizes the idea that the optical angles as such can be the basis of the judgment, and himself suggests that "sensations corresponding to the different dispositions of the eyes"(convergence) are connected with the idea of distance, while the degree of "confusion" with which the object is seen, and the "straining of the eye" to prevent such confused appearance" are also factors. Other factors mentioned are figure, size and faintness of the rays with attendant "straining of the eye". The ideas of extension and depth are composites of these and are gained by experience. We are not at present interested in the idealistic explanation of ideas which goes with

Berkeley's theory, but only with the empirical theory of space perception and the factors suggested.

Müller's doctrine of specific energies and Lotze's doctrine of local signs are perhaps assumed in the further explanation and experimentation on depth and motion perception.

The experimental work on depth perception will be treated under three categories, (1) monocular perception of depth, (2) binocular perception of depth, and (3) the question of relative strength of factors in the ordinary use of the eyes. The work on monocular perception of depth seems to have centered around the question of the part played by accomodation. Helmholtz, Wundt, Arrer and Baird present evidence for accomodation as an important factor, while Hillebrand thinks it is inoperative in the monocular perception. Hillebrand stresses retinal size. Carr and Allen later present evidence of the part played by voluntarily controlled accomodation.

Helmholtz's work with the illusion of greater apparent distance for a blue than for a red line explains this in terms of an accomodation for the blue which is similar to that for a greater distance. Dispersion and other factors have since been shown to be of

more importance in this illusion. (36, p.371). Wundt working with the well known thread and tube, finds smaller limens for the perception of change, in the direction toward the subject than in the opposite direction. He concludes that innervation sensations from accomodations are effective since these would be operative in the bulging process but not in the reverse. The neurology must, of course, be revised to include our knowledge of receptors in muscles. Arrer uses a similar technique with similar results but points out that convergence may also enter reflexly in the unused eye. Even, then, however, accomodation might enter, since some cue is necessary for convergence.

Hillebrand tries to eliminate size by using the edge of a cardboard screen as the object fixated, and finds that subjects cannot discriminate changes of distance when the movement is slow. Moreover, by use of the Aubert diaphragm, erroneous judgments are obtained. This shows that the factor of retinal size is rather important, but does not entirely exclude the possibility of some influence from accommodation, since Baird (2) using apparatus similar to Hillebrand's, but with a sharp edge to the screen, finds that subjects are able to judge distance with very slow motion of the screen. Treatments of this work are to be found

in Helmholtz(34) and a survey by Baird. As Baird points out Hillebrand's results with the Aubert diaphragm indicate not that accomodation is inoperative, but rather that retinal size is the stronger factor. Baird substitutes sensations from the muscle for Wundt's sensations of innervation. Baird also had one subject who made judgments in terms of haziness due to sluggish accomodation, as shown by the fact that his judgments alone were disturbed by using slow approach and fast recession movements of the screen.

Carr and Allen(6) report work with a subject exhibiting voluntary control of accomodation and come to the conclusion that depth perception must be a function of innervation. However, this is probably of little practical importance, since very few subjects show such voluntary control.

The binocular work includes that of Meyer, Wundt and Baird. Meyer shows the effect of change of convergence by the use of a stereoscope in which the cards are moved in and out in the slide, producing change in apparent depth. Wundt finds smaller limens for the binocular than for the monocular perception, attributing the difference to convergence. Baird also finds convergence the dominant factor in the

binocular judgment. Whether muscular or retinal factors are operative is a question. Dodge's evidence later, seems to make muscular factors in perception of motion at least, rather questionable.

This again would point toward retinal effects such as dispension, disparate images, etc. as the actual data arising from changes in accomodation or conveyance.

Barrie, Howard and Eaton discuss the relative importance of factors where both monocular and binocular as well as "primary" and secondary" factors are involved. Howard, working with a judgment of distance of two rods monocularly and binocularly finds binocular paraliax more important than size of retinal image, and Eaton(13) reports on the basis of stereoscopic effects, that the combined power of secondary criteria of depth is greater than that of the "primary" binocular mechanism. This result would indicate the need of keeping our total situation as similar as possible to that in the original situation when constructing a test. Barrie (4) disagrees with Eaton that binocular factors are more important than perspective, previous knowledge, etc. claiming that depth does not resist reversal as indicated by Eaton, and that the binocular factors are therefore the more important. The last two authors merely present their

own individual stereoscopic reactions and we suggest that the difference may be due to the attitudes of the two observers and therefore a further evidence of secondary factors.

Such secondary factors as shadows, aerial perspective, previous "set" as to actual size, etc. are well known and are mentioned by Helmholtz (37, p.282-297)..

Perception of Motion

There have been two general theories of motion perception advanced, the one being that perception motion is a distinct, specific perception differing from that of perception of space or of time, and the other that perception of motion is based on perception of spatial succession. Stratton (29, 29a) presents evidence against the former view which is held by Exner. Exner's work is also treated in Helmholtz. Exner used the distance between two disks and the time between sparks and obtained perception of motion at spatially and temporally smaller intervals than that of duality. This is used as an argument that motion perception is different from perception of spatial or temporal succession. Stratton points out that this result is a function of the experimental method, and finds that when using two lines, end to end, as the stimuli whose space and time relations are varied, the spatial dis-

placement for duality is about the same as that for motion while Brewer (3) with the-same-technique finds the time limen for motion only slightly less than that for discrete succession. Motion then would seem to involve "succession but not discrete succession".

Langfeld (19a) also finds in stimulating the two eyes successively, that succession merges into motion which finally becomes simultaneity as the speed of prestation is increased, seeming to substantiate Stratton's view.

In so far as perception of change of distance is involved in the visual perception of velocity, the factors involved in the former may enter into the latter. Specific suggestions of this sort are advanced by Ferree and Rand and by Dockeray. Feree and Rand (14) suggest that there should be a relation between lag in adjustment of the eye for clear seeing, and speed of judging distance. Dockeray has suggested speed of convergence as possibly affecting the accuracy of judgment in the case of high speeds.(11a)

The following bit of evidence shows binocular factors of depth operative in perception of motion. Howard (11) cites an accident in aviation during the war to illustrate misjudgment of "distance" in a situation where the effects of "binocular parralax" were

lost. The situation, however, is one in which the judgment includes not only distance, but also rate of change of distance. We therefore, include it under judgment of velocity, rather than of distance. In this case, the pilot tried to "zoom" up over a series of horizontal wires, but midjudged. Howard attributes this to loss of the effect of binocular parallax, the same effect being obtained in the judgment of distance with his apparatus, by turning the rods through an angle of ninety degrees.

Evidence concerning factors operative in the perception of actual transverse movement under experimental conditions is reported by Dodge and Thelin. Dodge (12) in photographing pursuit movements, in which the angular velocity of the eye corresponds to that of the object finds that they do not start until the image is brought to the fovea. The movements of the eye toward an eccentric stimulus which accomplish this focussing on the fovea, occur with fixed velocity and would therefore not furnish muscular sensations of such nature as to serve ascues in the perception of motion. Therefore, if such perception is to be in terms of muscular or orbital sensations, it must await the arousal of true pursuit movements. Further, the pursuit movements are interrupted by corrective

movements which must be attributed to sensory cues. Such corrective movements would mean that kinesthetic cues would be far from simple. Also, in experiments with a perforated disk so arranged as to present moving stimuli which fuse into a circle with the eye stationary the subject's report an appearance of stationary dots when the eye is allowed to move. Here kinesthetic cues should be operative, but the report of motion is not obtained. Retinal factors therefore seem predominant, at least for lateral motion, and it seems probable that they may also be for motion in the median plane. Thelin (31) however, working with lights in a dark room, finds that motion is more correctly judged where the stimulus is actually moving. This may indicate that some cues do arise from pursuit movements under some circumstances. However, the predominant factor may still be retinal displacement.

Thelin also finds that the dimmer light appeared the smaller and the more distant and was more often correctly judged as moving. This result would seem to indicate the operation of secondary criteria of depth. He also finds noticeable individual differences, as to effect of intensity, fixation, etc.

There remains the discussion of apparent

visual movement. Stratton's and Langfeld's work previously mentioned seem to show that we are probably dealing with the same basic process, in that perception of spatial and temporal interval is involved. The strength of attitudinal factors in apparent motion is shown by Dimmick and Sanders, and DeSilva's results. Dimmick and Sanders(10) find that reports by subjects are influenced by different attitudes induced by instructions. DeSilva (9) finds that motion is enhanced by a "meaningful stimulus" such as a human figure in which the hands move. He also finds that an analytic "set" at the start gives reports of success, while practice brings reports of motion. Furthermore, subjects can be trained to the point where a predisposition set up by instructions, affects the direction of motion. These results also point to the great effect of attitude factors.

That attitude may enter in perception of actual motion is indicated by the individual predispositions found by Thelin, although the factors there were positional, etc. and not definitely shown to be the result of induced attitudes.

In summary, the work of Helmholtz, Wundt, Arrer and Baird gives a certain amount of evidence

for accomodation as a factor in monocular depth discrimination, while Hillebrand has shown that retinal size is more significant. There is general agreement thatbinocular judgment gives more accurate discrimination, and Dodge presents evidence tending to show that retinal, rather than muscular sensation factors are predominant.

Secondary factors such as aerial perspective, shadows, intefvening objects, etc. are well known and are treated in detail by Van Kries and by Helmholtz.

Stratton's and Langfeld's results seem to show that the perception of apparent motion is based on perception of temporal and spatial difference, while the importance of attitude in apparent movement as found by De Silva and Dimmick and Sanders and the predispositions found by Thelin in actual motion perception point to "set" as a factor.

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Chapter III.

HISTORICAL

Tests

The use of tests in the industrial field in general was started by Münsterberg about 1910 and has been gaining ground since that time. Occupational tests may be classified in general, as miniatures, samples of the job, analogues of the job, and tests empirically chosen. Different writers favor different types of tests. Münsterberg, (23) for instance, criticizes the miniature type on the basis that a reduced copy may "arouse ideas, feelings and volitions which have little in common with the processes of actual life." Since our miniature highway apparatus described in Chapter V is of the miniature type, we would call attention to the fact that Münsterberg says "may arouse", and that the same criticism may be levelled at any of the other types of tests.

The first tests for drivers and pilots seem to be those developed during the war. The army test for truck drivers is reported by Chapman.(8) In this, the driver had to drive forward and then backward through an S shaped road, back up to a platform, turn

on an incline, etc. Moss (24) gives a series of tests which includes an operating test for automobile drivers. Dockeray and Isaacs (11), Henmon (17) and Stratton, McComas, Coover and Bagby (29b) report tests used for selecting aviators. The last article is the only one which includes a test of judgment of speed. However, when correlated with flying ability ratings, this seemed the "most promising" of the three best tests, which were: (1) judgment of relative speeds, (2) judgment of parabolic curves and (3) complex reaction time. The correlations are all, however, below ± .25. The test for judgment of speed just mentioned consisted of two spots travelling along converging straight lines and disappearing behind a screen. The subject was required to judge whether one of the spots (a white one on the right) would reach a given point (also hidden) by the screen before, at the same time or after the other spot. The actual correlation of this test with flying rating is +.23 ± .096 as against +.10 to +.17 on the other tests. Other tests used were estimation of prolonation of curves, reaction time with stick and controls, maze learning and recall.

Apparatus similar to that described in Chapter IV was used by Troland in a study of aviation cadets (unpublished). Later tests of especial interest to us

include those of Snow with the Yellow Cab Company, Viteles with the Milwaukee Railway Company, Mira(Spain), Dembitz(Germany) and Lauer and Weiss at Ohio State University. Snow (27) in a series of tests used with the Yellow Cab Company included a test of perception of space and motion, using miniature cars on a table, the cars being drawn across the table by a system of pulleys and weights, in the right-left direction with regard to the subject. The subject was required to indicate the point of passing, and the measures used were (1) "amount of error," (2) "rapidity and regularity of response," (3) "speed of learning", (4) "constancy of attention." Very little in the way of data on validity or reliability is given, aside from the fact of reduction of accidents in the large. We are told that "The theory underlying this is that the subject who is unable, with any degree of accuracy or promptness, to make the necessary judgment, (of distance etc.) suffers from a visual defect - whether central or peripheral - which of course, makes the subject an undesirable candidate for a pilot of any transportation vehicle," but no figures are given to support this statement.

Mira* (Spain) reports a somewhat similar test,

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*This information was acquired in conversation with Dr. Mira.

using however, three moving pointers. The subject is required to tell which pointers will pass each other first and at what point. The test is said to differentiate good from bad drivers, along with other tests, but those who do not pass are not hired so that there is no check as to the validity of this statement. The time of judgment is apparently uncontrolled.

Vitelles (35) and engineers of the Milwaukee Electric Railway Company have developed a test using miniature electric cars controlled by regular motorman's controls in connection with electric railway work in Milwaukee. Shellow (26) reports differentiation among the younger men by the test battery used. A revised edition by McCarter is in use by the Cleveland Railway Company. When used in its most complicated form, the motorman subject must control the one car so as to avoid hitting another car which crosses the track at intervals. The whole course is oval and is mounted on a large table. The motorman stands at the controls at one end looking down on the table.

Kunze (19) reports a study using moving endless bands, with a disappearing mark. The motion used is a lateral one. In most of these studies, the criterion of ability in driving or running a car, is vague or absent.

An apparatus used by Lauer and Weiss (22) may

include to a slight extent, judgment of the lateral motion of a pointer. They, however, assume that the predominant factor being tested is the eye, hand-and-foot co-ordination, which is perhaps a more reasonable assumption, (and applies to most of the investigations reported above.)

In summary, tests of various types have been used for selection of automobile drivers or motormen and among them are tests of judgment of distance and velocity. In the one study where correlation with a criterion is made, the test of estimation of velocity is reported to be the most promising of those evaluated, thought all correlations are low. Shellow uses an accident record criterion and finds a certain amount of differentiation by the score on the test battery used. This included a test using moving vehicles. In the other cases no figures are given. The tests using pointers or moving bands utilize predominantly a motion in a lateral direction with respect to the subject. In two of the tests, the subject is allowed to give his judgment at any time he sees fit, thereby introducing an uncontrolled factor.

The first apparatus used by us (that devised originally by Troland - see Chapter IV) is designed to investigate estimation of velocity in rather simple

terms under laboratory conditions, while the second (the miniature highway apparatus) attempts to produce visual conditions as similar as possible to those in the original driving situations, including three velocities, and actual passing of cars. The attempt to maintain such conditions has not been previously made.

CHAPTER IV

*PRELIMINARY EXPERIMENT

Introduction

The apparatus was designed to test the subject's ability to estimate the speed of a moving object which he has seen but which has passed behind an obstruction, the motion being at right angles to the subject's line of vision. Such discriminations are of course commonly made at street crossings where another car comes in from the side and passes behind a vehicle, signboard, or building; likewise where pedestrians walk across the sidewalk toward the street and pass behind a parked car. It seemed probable that the abilities manifest with the apparatus would also be involved in many other driving situations.

The general type of apparatus used was first designed by Troland in connection with a study of aviation cadets during the war (unpublished). The subject sat where he could view the motion of a pointer across his line of vision from left to right. During part of its excursion the motion of the pointer was visible. During the remainder of its path, the pointer was invisible, and the subject pressed a key when he estimated it to have arrived at a certain point. The score was then recorded in terms

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*This section has been published in briefer form in an Ohio State University Monograph (30) p. 62-72



FIGURE 1.



FIGURE 1.

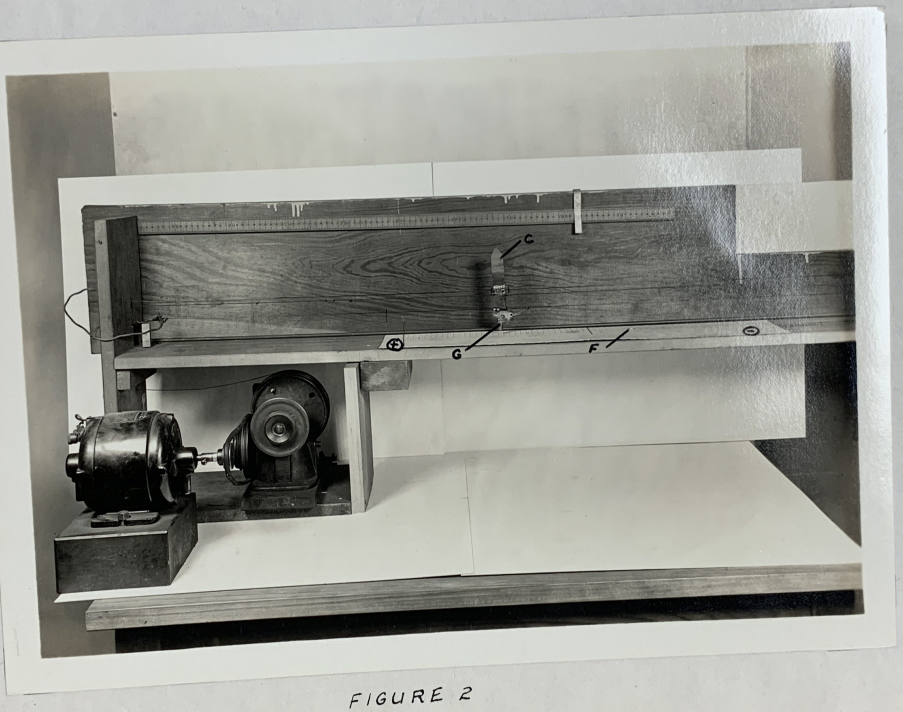


FIGURE 2

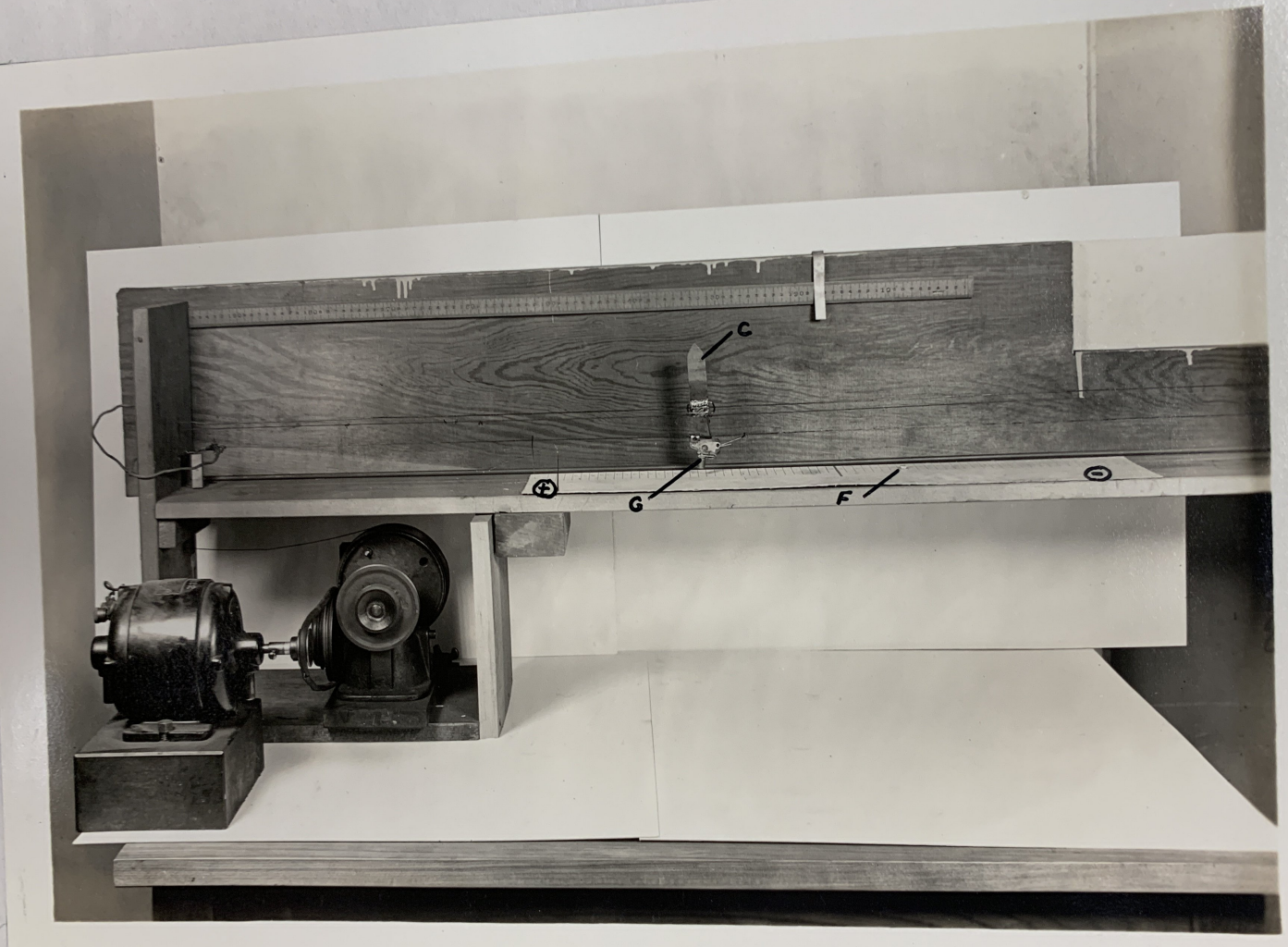


FIGURE 2

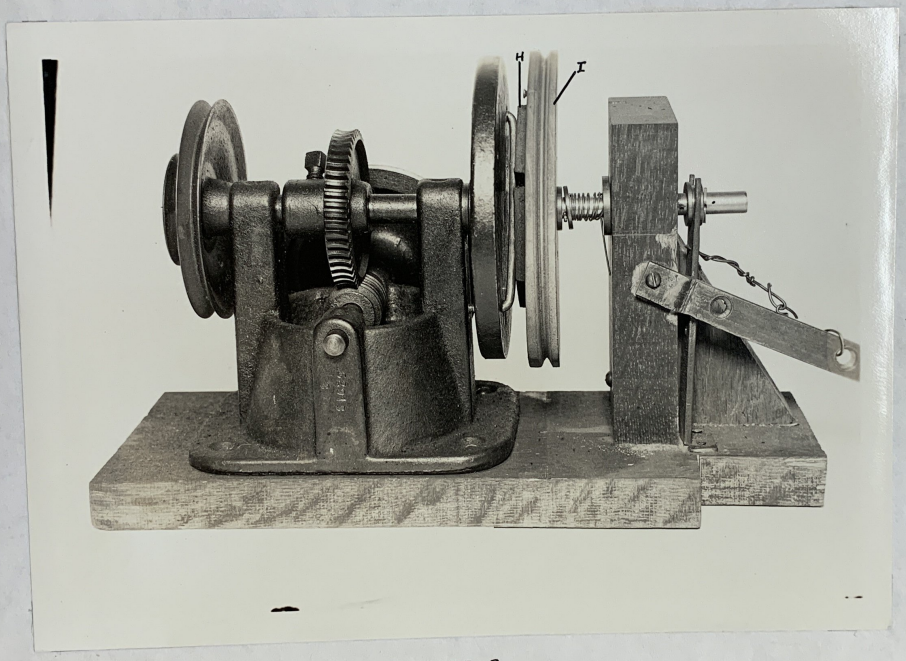


FIGURE 3

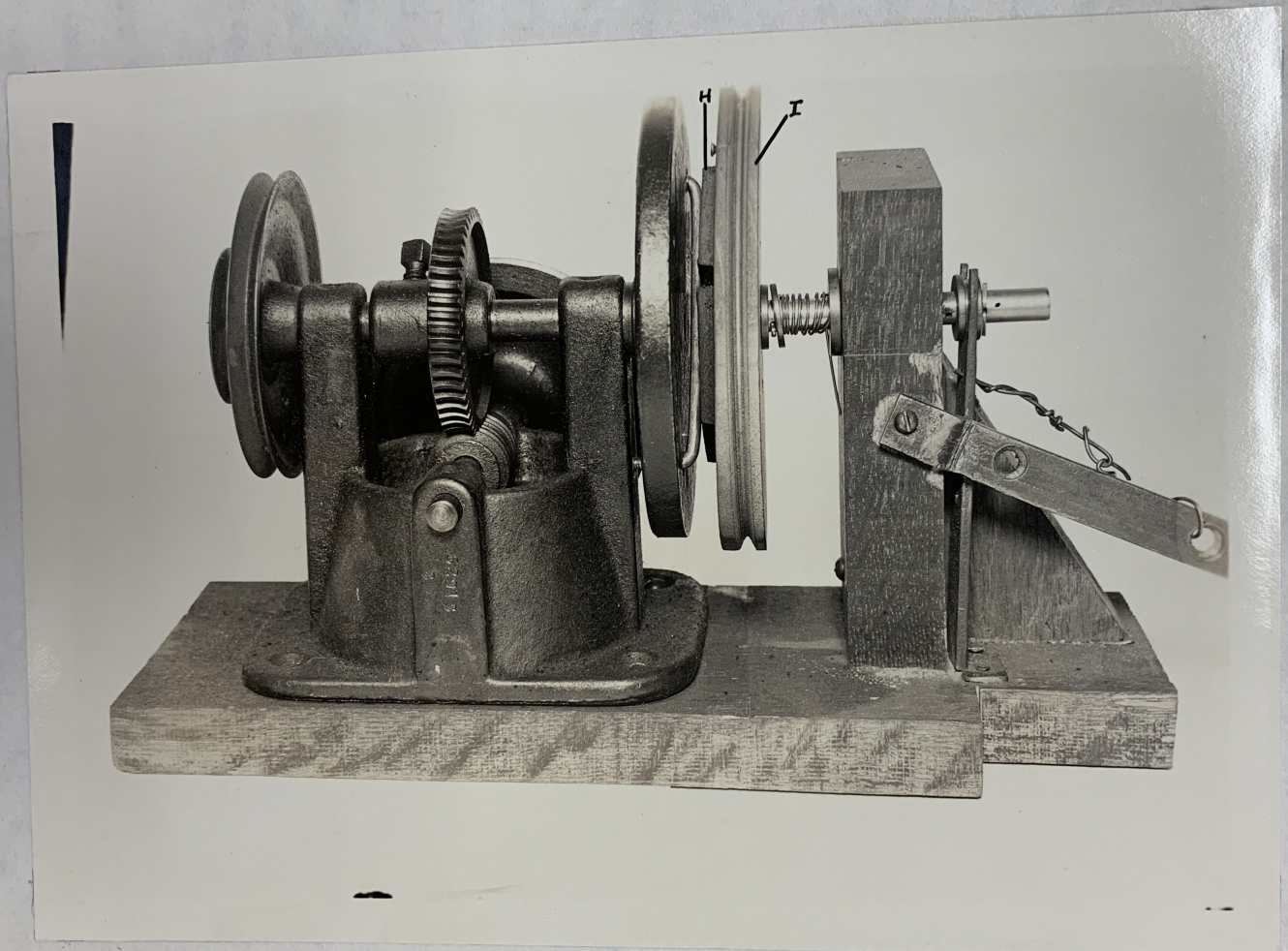


FIGURE 3

of the error in distance, i. e. the distance of the pointer from the designated spot at the moment of judgment.

Apparatus as Revised for Present Study

The apparatus consisted of a wooden frame and cardboard screen,(A, Fig. 1) behind which a vertical pointer moved in a horizontal direction. A window (B) in the screen made the pointer visible at the start of its excursion. The pointer (C) was made of 1/16-inch brass, about 1 inch wide and was mounted on a sleeve which slid along a horizontal wire. (See Fig. 2) It was guided by a second wire below this. The motion of the pointer was produced by means of a piece of light string which ran over pulleys to a grooved wooden drive wheel of 6-inch diameter, (I, Fig.3). A positive-acting clutch consisting of 1/4-inch leather facing (H, Fig.3) on the drive wheel with a groove into which fitted a bar on the drive plate of the reduction gear, connected the wooden drive wheel to the 30-to-1 reduction gear and 1750-r.p.m. electric motor.

The pointer was also connected with heavier twine to a system of pulleys and weights which returned it to its starting position when the clutch connecting to the motor was released. This allowed the operator to start the pointer by throwing in the clutbh and then at the end of the trial to return it to starting position simply by releasing the clutch.

On the lower end of the pointer was mounted a single-pole electro-magnet taken from a small buzzer (G, Fig. 2). This was energized by three dry cells and connections through the wire supporting the pointer and through the guide wire. The key used by the subject in recording his judgment was also in this circuit. It was found advisable to make connection from pointer to wires through copper wire drags, to insure continuous contact, otherwise a slight unevenness in the wire might break the circuit. A piece of soft iron some 3 mm. square was held to the magnet while the circuit was closed. When the subject pressed the key, this broke the circuit and dropped the iron indicator (F, Fig. 2).

A scale marked off on white cardboard in one-centimeter divisions was placed so that the central division lay exactly behind a vertical black marker on the grayish cardboard screen. Divisions on the scale were marked off from 0 to 30 in both directions from this central point. In the few cases where the error was so great as to be entirely off this scale, the excess was stimulated.

During Series A the operator was in full view but beginning with Series B a 4 x 6-ft. Celotex screen (D, Fig. 1) was used to shield him from view. However, since it was desirable to be able to observe the subject, a window (E, Fig. 1) 6 inches square was designed to

permit this and yet to allow the subject no view of the operator.*

This end was accomplished by covering the window with a thin tin plate perforated with holes 1 mm. in diameter and 1 mm. apart. The surface toward the subject was bright and reflected the light thrown on it by a 100-watt bulb. Further, the area behind the screen darkened by covering one of the windows of the room, but ample light remained for the operator.

During Series C the following device was used to measure head movements roughly. A piece of mirror 1 inch square was fastened to an elastic mesh headband which could be easily adjusted. Eight feet behind the subject in the right-left plane was a screen marked off into divisions, which as a matter of convenience were 6 inches wide with every third mark colored and heavier. A strong beam of light from a lantern projector was thrown on the subject from the rear and the mirror adjusted so that it reflected a bright spot on the screen. A displacement of the spot to the extent of one six inch division on the screen, then indicated a head rotation of about $1^{\circ}\ 50''$ of arc. The experimenter looking through the screened observation window had full view of the screen and noted the location of the

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*Forbes, T. W. A Technique for Screened Observation. Amer. J. Psychol., 1930, 42, 114

indicator spot just as the pointer which the subject was to observe appeared in the opening of the apparatus (B, Fig. 1). The experimenter likewise noted the location of the indicator spot just as the subject pressed his key. The difference between the scale readings gave a rough indication of the amount of head movement.

Critical Dimensions and Speeds

The apparatus stood on a table 91 cm. in height, making the top of the horizontal cardboard screen 142 cm. above the floor. This brought it to about eye level with the subject seated. The marker was 1.2 cm. wide. The pointer was 2 cm. wide, and 5 cm. of its upper extremity was observable through the opening as shown. Window length and distance from window to marker differed for various series as follows:

Series	Width of Window	Distance from Window to Marker
A	61 cm.	25 cm. 40 cm.
B, C, D.	20 cm.	69 cm.
Distance, subject to screen (all series)		457.2 cm.

In series A the two distances (25 and 40 cm.) from window to marker were presented in two separate series, but with the same window length (61 cm.). The unforseen introduction of counting by some subjects made it seem advisable to alter these dimensions as indicated below,

and in series B, C, and D a distance of 69 cm. from window to marker was used with a window length of 20 cm., the subject sat at a point such that distance from eye to screen was 15 feet (457.2 cm.) in all series.

Two speeds of the pointer were used. The one referred to as "slow" in the tables was 8.2 cm. per second, or an angular velocity at the eye of 1° 6' per second, while that referred to as "fast" was 16.95 cm. per second, or an angular velocity of 2° 7' 30".

Procedure

The instructions read to the subject were as follows:

"The object of this experiment is to see whether we can find out something about ability to judge speed. You are to sit in this chair and watch thewindow in the apparatus in front of you. There is a vertical pointer which will travel across the opening and then disappear. You are to press this key when you think the arrow has reached this marker (point to marker).

"The pointer travels at the same speed after it disappears as it does when you can see it.

"In using the key, keep your hand just above it but not touching it. Then, when you want to register, tap it firmly for an instant and then take your hand off again.

"Let's try it once."

For Series B instructions to use a counting method during one of the two series of trials were added to the above.

For Series C (main series) and Series D the following was added to the instructions as quoted above:

"We have found in a previous experiment that some people tend to use an artificial method of judgment. However, the apparatus as now arranged gives you about as good chance of success when you make the judgment as you ordinarily would. Since we are not interested in the judgment using artificial aid, we will ask you not to use such a method but to make the judgment as you would ordinarily judge speed."

Counting was not mentioned as such, since it had been discovered that some subjects would try it if suggested, whereas they would not if it was not suggested.

After reading the instructions the experimenter took his post. Beginning with Series B, this position was behind the Celotex screen (D, Fig. 1). He then started the motor, called "Ready" and engaged the clutch, thereby starting the pointer. Keeping his hand on the clutch lever, he observedthe subject through the screened window in the Celotex. This allowed him to watch for lip movements and head or body movements in order to check as to whether the subject was following directions or using a counting or rhythmic method of determination. It also

allowed a rough observation of eye movements during part of the main series. The movement of the subject's hand in recording his judgment was in most cases easily noticeable, and served as a cue for the experimenter to turn from the window to the apparatus and record the position of the marker as the score for that trial. The few cases in which the recording circuit was accidentally broken too soon were omitted from the data.

After observing the score for the trial, the experimenter released the clutch, and thepointer returned automatically by a system of weights and pulleys to its starting position. In passing the iron marker, the magnet picked it up due to the fact that the subject had released his key and the circuit through the magnet was again closed. The pointer was therefore ready for the next run immediately. When using the faster speed, the experimenter usually recorded two scores at once--one from memory--to save time.

Trials

Each subject was given one practice trial at the start to be sure that he understood the task and the operation of the recording key. In the few cases in which this had been misunderstood, further explanation and one further practice trial sufficed. Ten presentations were then made of the one speed.

At the end of this series of ten judgments the

other of the two speeds was introduced with a practice trial as in the first case in order to allow the subject to adapt himself to the changed velocity. Ten judgments were then made using this speed.

The two velocities were given alternate initial presentation with the different subjects in order to balance out possible influence on the judgment of a contrast or other effect due to change from slow to fast, or vice versa.

Procedure was without knowledge, i. e. subjects were not told the results of their judgment until after the series was completed. For this reason it was not thought necessary to give a practice period of any considerable number of trials. However, the effect of a prolonged series of trials was investigated in Series D.

In this series (D) virtually the same procedure was followed as in the foregoing. However, each subject was allowed to make a total of fifty judgments. In two cases there was alternate presentation of velocities in groups of ten trials (giving one hundred judgments altogether) and in the remaining six cases one speed was used for the total fifty trials and the other for the next fifty.

Subjects

Subjects for series A, B, and D were undergraduate and graduate students at Ohio State University. In

TABLE I

Sample Records

Subject H. R.

40 cm.		25 cm.	
Slow	Fast	Slow	Fast
-9	-9	-9	-7
-5	-8	-6-	-9
-10	-12	-2	-10
-5	-17	-10	-6
-10	-6	-16	-8
-18	-13	-11	-13
-4	-14	-14	-7
-12	-16	-12	-10
-13	-14	-10	-9
-13	-21	-9	-6
Average-99	-130	-99	-85

Subject M. T.

40 cm.	
Fast	
-2	-
-4	
-2	
	+1
-1	
	+7
	+3
	0
-2	
—	+7
Total -11	+18
Average 2.9	

the main series (C) fifty-one students and eight drivers from a commercial concern in Columbus, Ohio were used. Five more drivers from a commercial concern in Dayton, Ohio were used later, and their scores are appended.

Results - Series A

Eleven subjects were used in this series. Window length was 61 cm. and distance to marker 25 and 40 cm. Table I following gives a complete sample record for subject H. R. in which it happens that all judgments were made too soon. They therefore appear as minus values, whereas, if the subject had pressed his key too late, the record would show plus values, as indicated on the scale, (Fig. 2.). The last column is one group of scores for subject M. T., showing how underestimation (minus values) and overestimation (plus values were recorded separately). The average error is recorded at the bottom of the column for each series. It did not seem advisable to attempt to use the algebraic signs, since a person having large errors evenly balanced on each side of the zero point would thereby score perhaps as low as one who made much smaller but consistently negative or positive. Therefore, the figure used in all cases as the score was the average of the total error gained by adding the absolute scores, without respect to signs.

Table II gives the figures for the eleven subjects of Series A. As in the sample these indicate average

error in centimeters computed from ten judgments, using magnitude only and not direction of error.

Table II shows that the range of scores is smaller for the 25 cm. distance than for the 40 cm. and further that there are quite a few scores as low as from one to three. Questioning brought out the fact that subjects estimated a distance along the window equal to that to be judged and used counting of one type or another in order to estimate a time equal to that taken by the pointer in traversing the distance. This evidently invalidated the results for the present purpose, and the apparatus was revised to make counting more difficult by changing window length and distance to be judged as given above.

Series B

Beginning with this series the window length was 20 cm. with 69 cm. from window to marker. These dimensions eliminated the possibility of laying off along the window a distance equal to that being judged. Further, if a counting method is used at all, it depends on some sort of initial estimation of the two distances. In order to do this the subject would now have to divide the window length into the longer distance. Since 69 cm. is not an even multiple of 20 and is comparatively long, the relationship of the two lengths became less apparent.

TABLE II

Series A Scores

Subject	25 cm.		40 cm.	
	Slow	Fast	Slow	Fast
H. M. L.	2.8	3.5	3.5	5.1
K. J.	2.3	3.5	3.2	2.0
L. B. Y.	1.2	2.4	4.2	3.8
J. D.	10.3	12.3	9.6	11.1
D. R.	3.9	4.5	4.8	4.4
H. R. J.	6.4	5.9	3.5	7.4
I. M. H.	2.5	5.2	8.8	4.7
H. R.	9.9	8.5	9.9	13.0
C. W.	9.6	6.8	15.4	10.4
M. T.	8.8	7.2	10.5	2.9
C. E.	2.8	3.8	5.9	4.5

Note - Table II is composed of the average errors obtained from records like those in Table I.

In series B four subjects were run for a series of ten judgments at each of the two speeds. Half of each series was done with the aid of counting, the other half without such artificial method.

TABLE III

Series B Scores

Subject	8.2 cm. per sec.		16.9 cm. per sec.	
	Counting	Without Counting	Counting	Without Counting
C. W.	3.9	9.8	4.8	6.9
H. M.	6.6	7.1	6.1	7.3
M. L.	8.8	8.7	12.7	6.2
D. W.	2.1	3.8	4.1	3.9
Average	5.35	7.35	6.9	6.1

The scores are average error in centimeters as in Series A above. Table III shows that with the exception of one case with the slower speed, the advantage gained by counting was not large and that in two cases the subject actually did better in the non-counting group. The averages show only a slight advantage for counting in the slow speed and none in the fast. Moreover, subjects were not able to drop the indicator very close to zero consistently, as one or two had been able to do previously. We therefore felt that the advantage to be gained from

the artificial method of judgment had been considerably reduced, and proceeded to Series C.

Series C (Main Series)

In this series window length and position of marker were the same as in series B, i. e. 20 cm. and 69 cm. respectively. Fifty-nine subjects were used. A paragraph was inserted in the instructions asking the subject to co-operate to the extent of eliminating any artificial method (see Procedure).

The experimenter further observed the subject's lips and motions as a check, and in case vocalized counting or rhythimical movements of the head, etc. appeared, the subject was questioned as to how he was performing the task. In most cases the subjects were cooperative, and in the few cases where counting at first appeared, it was apparently discontinued on request.

Rough observation indicated that when the counting method was eliminated, subjects used a following movement which was a combination of eye movement and rotation of the head on the vertical axis. In order to investigate this objectively the mirror attached to a headband, previously described, was used.

Eye movement was recorded by observation through the screened window of fixations and number of regressive movements per trial. These observations were necessarily

TABLE IV

Subject	Average Head Movements		Eye Movements			
	Slow 8.2 cm. per sec.	Fast 16.9 cm. per sec.	Slow 8.2 cm. per sec.		Fast 16.9 cm. per sec.	
			Continuous	Fixations	Continuous	Fixations
S	.3	0.0	10	--	8	(2 blinks)
B	.4	.3	10	--	10	--
K	.5	.2	10	--	(6 ((4 unobserved)	
S	2.8	2.4	10	--	(9 ((1 unobserved)	
I	.5	.4	(5	8/3 (2 unobserved)		
Hu	0.	.1	9	2/1	5 (2 unobserved)	5/3
L	1.4	2.1	10	--	10	--
Ha	.6	.3	10	--	10	--
D	2.1	2.7	9	2/1	10	--
Average (No. trials continuous or including fixations respectively			9.2	.55	8	1.2

rather inaccurate due to distance. The experimenter took head-movement readings from the screen both before and after the trial and observed eye movements during the trial.

Table IV, giving average values for head and eye movements, is to be interpreted as follows: A score of 6 units for head movements would indicate rotation of the head through the angle necessary to follow the motion judged from start to finish. This was determined roughly from a series of trials in which the subject was asked to turn his head consciously toward the end points of the excursion.

In Table IV each entry is the average of head movement for ten trials. The heading "Continuous" under eye movements indicates movement in which no definite stop or fixation was observed during the judgment. The figures in this column indicate the number of trials of that type. Trials where observable fixations occurred are recorded under "Fixations" as the denominator of the fraction. The numerator is the total number of fixations for the total number of trials represented by the denominator. Thus 8/3 indicates that fixations observed in three trials totalled eight. Trials in which the experimenter had to attend to something else for one reason or another are recorded under "unobserved."

TABLE V

Series C (Main Series)
51 Students - 8 Commercial Drivers
Scores = Average Error

Name	Av. Error 1	2	Av. Deviation 3	4	Retest 5	6	7	8
	Slow 8.2 cm. per sec.	Fast 16.9 cm. per sec.	Slow 8.2 cm. per sec.	Fast 16.9 cm. per sec.	Slow	Fast	No. Accidents reported	Driven less than one year
1. Nally, Mildred	34.2	26.3	2.6	3.6	29.6	18.6		
2. Marcus, Sam	9.7	15.8	5.9	4.6	7.8	14.0		
3. Morrison, Robt.	27.3	8.0	9.1	4.4			2	
4. Shapero, Jack	6.3	5.5	4.1	5.2				
5. Drum, Elizabeth	18.7	20.0	12.6	8.0				
6. Weld, Hanon	23.0	21.3	5.6	13.3				
7. Hampton, Lynn(1st)	8.4	8.4	4.4	4.4	7.2	8.9		
8. Stubbs, Esther	12.0	15.7	10.6	9.9	20.0	9.0	1	
9. Bennett, L. B.	14.3	5.0	10.1	5.2				
10. Kolton, Clarence	10.0	15.7	9.4	7.2			1	

TABLE V -continued

Name	Av. Error		Av. Deviation		Retest			
	1	2	3	4	5	6	7	8
	Slow	Fast	Slow	Fast	Slow	Fast	No. Accidents reported	Driven less than one year
11. Schoen, Z. J.	11.6	4.6	11.6	6.4	11.8	7.1		X
12. Irwin, O. C.	22.7	23.3	4.4	2.2				
13. Fauver, L. E.	18.9	25.2	4.7	9.8			1	
14. Storer, Wm. G.	7.3	8.0	7.3	6.2				
15. Truman, Lila	30.2	34.5	5.8	3.6				
16. Hairston, Harriet	14.0	15.8	7.0	4.8				X
17. Kramer, Harold	18.0	18.4	4.0	5.9				
18. Hughes, J. D.	7.0	9.3	3.5	5.9				
19. Glick, Lawrence	10.1	5.6	8.0	5.0				
20. Ipp, Herman	22.6	35.1	11.3	5.1				
21. Gephart, W. L.	7.4	9.4	4.2	7.4			1	
22. Lolli, T. J.	8.8	13.1	7.1	7.9				
23. Haigh, Dorothy	7.9	21.9	6.8	9.7				

TABLE V -continued

Name	Av. Error		Av. Deviation		Retest			
	1	2	3	4	5	6	7	8
	Slow	Fast	Slow	Fast	Slow	Fast	No Accidents reported	Driven less than one year
24. Dally, Sybil	26.8	18.4	2.5	4.6				X
25. Robinson, Helen	21.2	14.4	7.6	7.3				X
26. Gartrell, Ann	10.5	9.7	9.3	5.2				X
27. Hummel, Helen	19.6	17.4	4.9	7.5				X
28. Trees, Dorothy	29.7	23.6	0.5	3.9				
29. Mason, Charles	12.2	6.5	5.6	5.7	10.1	4.1	1	
30. Ziegler, Herbert	20.1	13.7	4.5	5.3				
31. Rohe, Alma	13.7	26.9	9.1	3.9				
32. Fisher, Rita	11.4	15.1	5.3	8.7				
33. Valentine, W. L.	16.7	9.6	3.6	8.3				
34. Rini, Michael	30.0	29.3	*0	1.1				
35. Cole, Walter	20.6	22.9	6.6	4.9				
36. Beckett, Dorothy	22.8	21.9	6.6	5.3				

TABLE V - continued

Name	Av. Error		Av. Deviation		Retest			
	1	2	3	4	5	6	7	8
	Slow	Fast	Slow	Fast	Slow	Fast	No. Accidents reported	Driven re-less than one year
37. Ramsay, Leland	25.5	13.7	3.2	4.0				
38. Kellog (Miss)	18.4	19.3	8.2	9.3	17.7	13.2		
39. Keiser, Lois	25.3	12.9	2.4	5.2				
40. Styles, Wilhelmna	11.8	9.6	4.2	9.5				
41. Baskin, Miriam	10.2	10.8	5.8	4.0				
42. Negale, Virgil	6.3	10.3	6.3	7.9				
43. Miller, Edward	15.1	9.6	6.5	7.8	7.5	11.9		
44. McKinley, Wm.	22.4	21.5	4.9	4.1	16.9	11.4	1	
45. Kummick, Lawrence	9.1	15.3	7.5	9.3			2	
46. Levine, Sam	9,9	16.6	9.2	8.0				
47. Devanney, Thos.	5.1	6.5	4.6	6.1	4.7	21.3		
48. Findlay, Wellingtn.	20.2	12.5	6.8	8.9				
49. Sandfort, Helen	12.5	20.3	6.1	4.6				

TABLE V -continued

Name	Av. Error		Ave. Deviation				Retest	
	1	2	3	4	5	6	7	8
	Slow	Fast	Slow	Fast	Slow	Fast	No. Accidents reported	Driven less than one year
50. Forbes, Frances	18.3	5.6	6.1	3.5				
51. Peairs, Frank	10.6	11.1	10.0	7.8	11.2	22.8	1	
Lazarus								
52. Clark, Harry	8.9	9.2	7.3	7.2			1	
53. Myers, Ray	6.6	10.6	4.4	10.3				
54. Evans, T.	8.4	10.6	3.8	8.4				
55. Althorne, Harold	18.9	5.9	10.9	2.7				
56. Bond, John	5.3	3.0	5.4	3.0				
57. Penny, Chas.	10.4	12.7	9.6	6.2				
58. Habeker, Frank	26.1	27.1	13.1	6.1				
59. Tayler	8.4	8.5	7.0	8.1			1	

*In this series all scores were 30 or greater but were recorded as 30.

Subject S and subject D show the largest amount of head movement for any single series, but even this is only half that required to follow the pointer from window to marker.

Under eye movements it will be noted that continuous movement predominates. The averages for the nine subjects show this clearly. Furthermore, when fixations occurred, there were two or more per trial. Therefore, the subjects were evidently not simply fixating the marker and judging the time interval as such, and we may conclude that the subjects were using a following motion of the eyes together with a partial rotation of the head which varied from practically zero to half the angle subtended, depending on the subject.

Table V gives the results of judgments by fifty-nine subjects. Column 1 gives the average error in centimeters regardless of sign for the slow speed, column 2 the same for the fast. These are hereafter called the scores. Column 3 gives the average deviation for the slow and column 4 the average deviation for the fast scores for each subject. Columns 5 and 6 give scores on a retest for the slow and fast speeds respectively. Column 7 gives the number of accidents reported, a blank indicating none reported, while in Column 8, and X indicates a subject with less than one year driving experience.

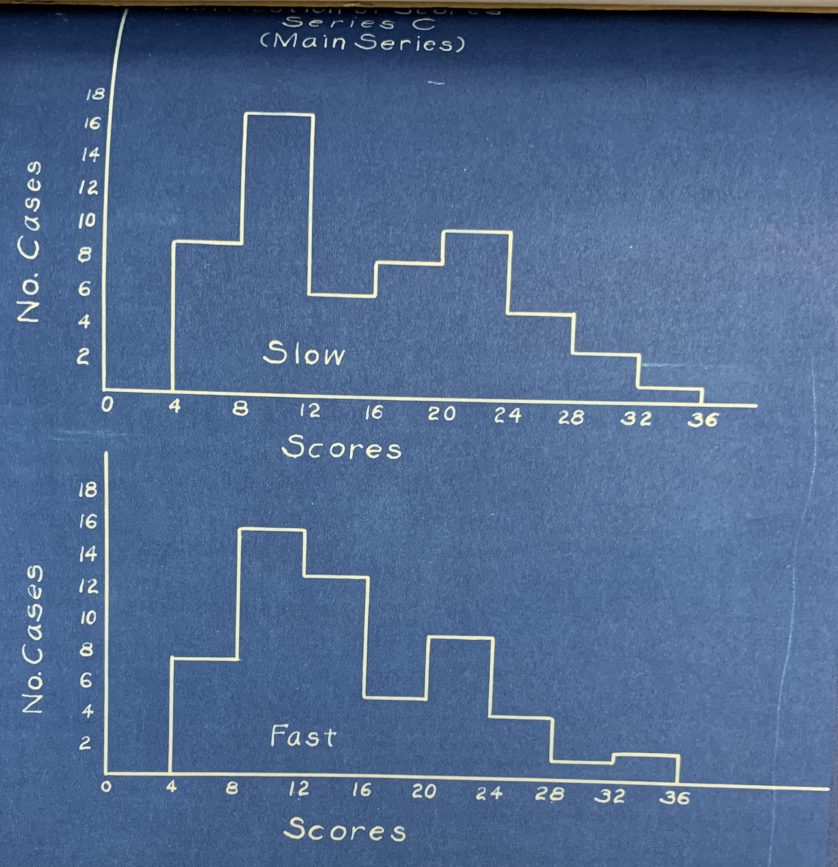

Series C
(Main Series)
No. Cases
18
16
14
12
10
8
6
4
2
Slow
0 4 8 12 16 20 24 28 32 36
Scores
No. Cases
18
16
14
12
10
8
6
4
2
Fast
0 4 8 12 16 20 24 28 32 36
Scores

FIGURE 4

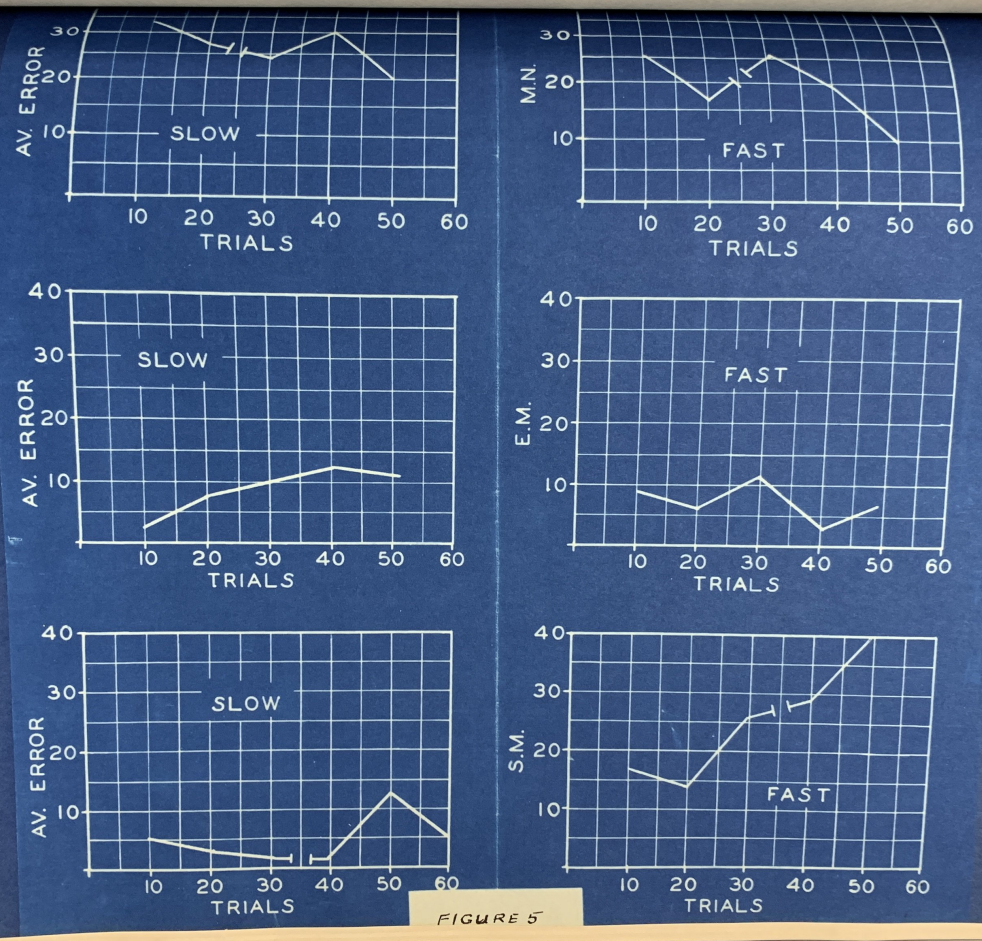


FIGURE 5

It was first thought that the average deviations might yield a significant score, but on examination it seemed that some of the poorest judges were rather consistent and therefore would have a fairly low average deviation. (see subject Nos. 1, 6, 12, 15, 24, 34, 37, 44 et al.) This average deviation was therefore discarded as a measure.

The histograms in Figure 4 show the distribution of scores for the fifty-nine students on each speed. The scores range from an average error of 5 to one of 35 cm. for the slow and from 3 to 34 for the fast, while the mode is between 8 and 12 in both cases as shown by the histograms. The distribution is thus skewed toward the smaller errors. On the other hand in certain cases the errors are so extreme as to constitute a distance over half that of the total distance from window to marker. When it is remembered that these are averages of ten judgments in each case, the difference between an 8 cm. error and one of 36 cm. seems to be significant in relation to the 69 cm. excursion of the moving member.

Reference to Table V will show that the range of scores for the commercial drivers is rather similar to that shown in the histograms, i. e. from an average error of 5.3 cm. to one of 26.1 cm. and from 3.0 cm. to 27.1 cm. for the slow and the fast respectively.

Furthermore,reliabilities seem to indicate a pretty fair consistency.

TABLE VI

Reliabilities				
	Slow 8.2 cm. per sec.		Fast 16.9 cm. per sec.	
First five vs. second five	r	P. E.	r	P.E.
	.75	.038	.74	.039
All ten as in histogram by Brown-Spearman Formula	.856		.850	
Correlation Between Speeds				
Slow with fast	.60	.056		
Actual Retest (ten cases only)				
Slow with retest slow	$r = .65 \pm .124$			

The first two reliabilities of ± .75 and ± .74 resepctively were determined by using the total of the first five judgments of each group for one score and the total from the next five as the second score and computing the product movement correlation coefficient. It is thus really an internal reliability. This was then corrected by use of the Brown-Spearman formula for a test twice as long. The reliability of ± .60 was gained by correlating the slow scores as given in the table with the fast for the same individuals, thus giving a measure of similarity of performance on the two speeds judged. The reliability of ± .65 was gained by use of scores on actual retests. Since N is only 10, the probable error is large. However, taken with the .85 gained by the Brown-Spearman formula and with the ± .65 reliability on actual retest at least seems to indicate corroboration. The reliability of performance, therefore, seems to be rather good for such a short test.

Table VII shows the means, differences, and critical ratios indicating the amount of differentiation between accident and non-accident groups. The means and probable errors were computed separately for students from whom a definite report was obtained (reports were accidentally not recorded from nine subjects) for commercial drivers, for the former plus the latter and finally for the whole group, including the nine unrecorded cases in

TABLE VII

Group Differences

Slow
8.2 cm. per sec.

	Mean Ac.	Mean N-Ac.	Diff.	P. E. Diff.	Diff./ P.E. Diff.
Students Reporting	13.9	17.7	3.8	1.573	2.42
Commercial	9.5	13.5	4.0	2.1096	1.90
Total I	13.16	17.05	3.89	1.412	2.75
Total II	13.16	17.33	4.17	1.291	3.23

Fast
16.9 cm. per sec.

	Mean Ac.	Mean N-Ac.	Diff.	P. E. Diff.	Diff./ P. E. Diff.
Students Reporting	15.1	16.8	1.3	1.709	0.76
Commercial	9.5	12.16	2.66	2.126	1.25
Total I	14.16	16.05	1.89	1.544	1.22
Total II	14.16	16.05	1.89	1.457	1.30

Total I = All accident subjects with commercial and students reporting no accidents

Total II = All accident with all others (either 0 acc. or not reported).

Notation: Mean Ac. = Mean of accident group
Mean -N-Ac. = Mean of hon-accident group

the non-accident group. The table shows that the accident group makes a better average (lower score) than does the non-accident. This is true for both students and commercial drivers. However, the ratios of the difference to the probable errors of the difference are, in general, too small to be significant. The ratios for the 8.2 cm. speed are the only ones which approach significance.

There seem to be two main possibilities in interpreting the wide range of scores obtained, and the doubtful or non-significant differences between accident and non-accident groups. Admitting that the number of accident cases is too small to yield valid generalization, we may still look for clues leading to further experimentation.

First we may be testing a judgment which has nothing to do with auto driving, and therefore nothing to do with accident. In this case we need a further technique for investigating the type of estimation of speed which undoubtedly must enter into auto driving.

Second, this type of ability to judge speed may enter into auto driving, but it may be only one factor involved in accidents and therefore it is possible that it would not show a reliable group difference. This implies that other factors are responsible for the accidents reported in our twelve cases. In other words, the accidents reported may have been such that a judgment of speed was

not involved. The accidents actually were:

1 scraped fender
1 minor (fender)
1 backed into pole
1 slid into car on wet pavement
2 "not serious"
8 details not recorded

It seems reasonable that backing into a pole may be due to merely failure to see the pole. One does not usually back fast enough to make such an accident involve a judgment of speed. Scraped fenders also often occur in city driving from crowding into tight places, rather than from misjudging speed. The accidents reported as "not serious" are likely to have been similar to the above, while those without details may also have been. We may therefore suggest that judgment of velocity does not enter equally into all types of accidents, and that we need a finer classification of our criterion.

Series D (50 trials)

In Series D the procedure was the same as that in Series C (the main series) except that fifty judgments were made by each subject. Two subjects were given ten judgments of one speed alternated with ten trials of the other. The remainder made fifty consecutive judgments of the same speed. Eight subjects were used, six of whom were undergraduate and two graduate students.

TABLE VIII

Effect of Fifty or More Consecutive Trials

Eight Subjects

Average error for first, second, third, etc. series of ten trials

Subject	Series of Ten Trials	Slow 8.2 cm. per sec.						Fast 16.9 cm. per sec.					
		1st	2nd	3rd	4th	5th	6th	1st	2nd	3rd	4th	5th	6th
M. N.		34	30//	26	33	22		26	19//	26	21	8	
S. M.		10	8	8//	5	19	9	16	14	26//	29	41	33
W. V.								13	23	31	6	21	
L. H.		15	12	13	16	18		15	17	17	21	24	
E. M.		4	9	11	11	11		13	11	16	8	11	
H. K.		14	17	14	7	8		16	12	11	10	12	
H. G.								16	15	19	17	10	
								9	12	20	15	27	

Note - Figures are rounded to even units for convenience in comparison of trends.

// indicates a break in the series (see text).

Table VII shows the results of the prolonged series.

Figure 5 shows the performance of three typical subjects showing the range of performance.

The ordinates represent scores (average erros) for each ten trials. The number of trials are plotted on the abscissa.

In the cases of M. N. and S. M. an interval of 2 to 3 minutes in the series is indicated by a break in the curve and by vertical parallel lines in the table. In the cases of L. H. and H. K. the two series were run on different days. No noticeable difference seems to appear which could be traced to this factor, unless it might be a regression to original score in the case of M. N. Fast. The other series seem to progress consistently.

In seven of the fourteen series the score on the first ten trials was better than on any succeeding ten, and in four of the remaining series the score on the first ten is very close to that of the following ones.

Figure 5 also shows how curves for different individuals vary from those in which judgment is poor at first but improves to those in which the judgment is good at first but becomes poorer (higher error) rapidly and consistently. There seem to be two possible causes for the latter type. M. G. and H. G. reported eye fatigue and

feeling of strain. Fatigue, then, especially ocular fatigue, was involved. Part of this was due to a bright source in the periphery (unshaded window). M. N. on completion of the series said, "If you hadn't been shouting 'Ready' I would have been asleep."

Suggestion was a second factor that seems to have been involved, as shown rather distinctly in the case of W.V. On the twenty-eighth trial the subject allowed the moving member to travel to the limit of its excursion, and the experimenter was forced to release the clutch, reversing the motion of the pointer before the subject recorded his judgment. The figures for the twenty-third to the thirty-second trials show that up until the twenty-eighth trial the subject was making rather large errors. After the above mentioned lapse occurred, the magnitude of the error dropped immediately.

Partial Record for Subject W. V.

Trial	Score	
23	40	
24	42	
25	50	
26	26	
27	50	
28	50	-clue
29	14	
30	1	
31	0	
32	4	

The subject reported that he kept thinking that he was pressing the key too soon, and so waited longer in order

to compensate. Then when he heard the apparatus reverse before recording his judgment, he knew he was waiting too long and corrected for this. Even so, however, after the next ten trials, the same steadily increasing error reappeared. Such a factor of auto-suggestion would make it necessary to keep the series short in trying to test an original judgment of projected motion.

In the case of M. N. and H. G. the fatigue factor is found together with progressively better judgments, while with M. G. the reverse is true. It would be interesting to attempt to determine whether approaching somnolence brings a series different from one of waking self-suggestion.

Conclusions

1. Judgment was made with a combination of head and eye movements.

2. The reliabilities of ± .856 and ± .850 seem to show that the judgment as tested is fairly consistent for each subject. However, not enough actual retests are available to fully establish this point.

3. The distribution of scores shows rather wide individual differences with a skewing toward the small error end of the scale.

4. Differences between accident and non-accident groups are significant only for the slow speed, and here only barely so. This seems to indicate that this particular

type of judgment is not enough of a factor in the accidents reported to differentiate groups.

However, inspection of the accidents which happened to occur in the cases tested makes it seem reasonable that other factors than judgment of speed should be responsible. It is therefore possible that we would find more correlation of poor judgment of velocity with accidents if we had more accident cases so as to include a different type of accident.

5. From Series D it would seem that probably

(a) To lengthen the test at a given sitting would introduce factors such as a progressive suggestion and fatigue which would invalidate results.

(b) The prolonged series seems to induce sleep in certain subjects and may thus be valuable in investigating the question of the driver's going to sleep at the wheel.

(c) The prolonged series seems to produce much greater progressive change in judgment in some subjects than in others.

6. Althought the test might yield better differentiation with different types of accident cases, we believe that the task is not sufficiently close to that required in auto driving and that there is too much possibility of the use of artificial aids in spite of careful

observation. The value of the above study lies in showing (1) large individual differences and (2) a fairly stable judgment by a given subject, and that it will be more worthwhile to design a test of judgment of velocity in which all the elements of the actual situation may be preserved as much as possible, rather than to gather further data on the present test.

Chapter V

MAIN EXPERIMENT

Description of Miniature Highway Apparatus.

General Construction

The revised apparatus for investigating the judgment of velocity in connection with highway safety, is essentially an attempt to construct a miniature roadway which reproduces with as little alteration as possible the visual conditions that obtain while driving on the highway and when overtaking traffic in the right hand lane and meeting of cars moving in the opposite direction in the left-hand lane. The subject is required to judge the velocities and operate a rheostat and steering lever in such a way as to avoid collisions between his and the other cars.

Figure 6 shows a general view of the apparatus, Figure 7 an end view,and Figure 8 and 9 structural details. The apparatus consists of an iron and aluminum fram supporting a horizontal 15 ft. by 6 inch endless belt (C, Fig. 7) of 8-ply canvass which



FIGURE 6



FIGURE 6

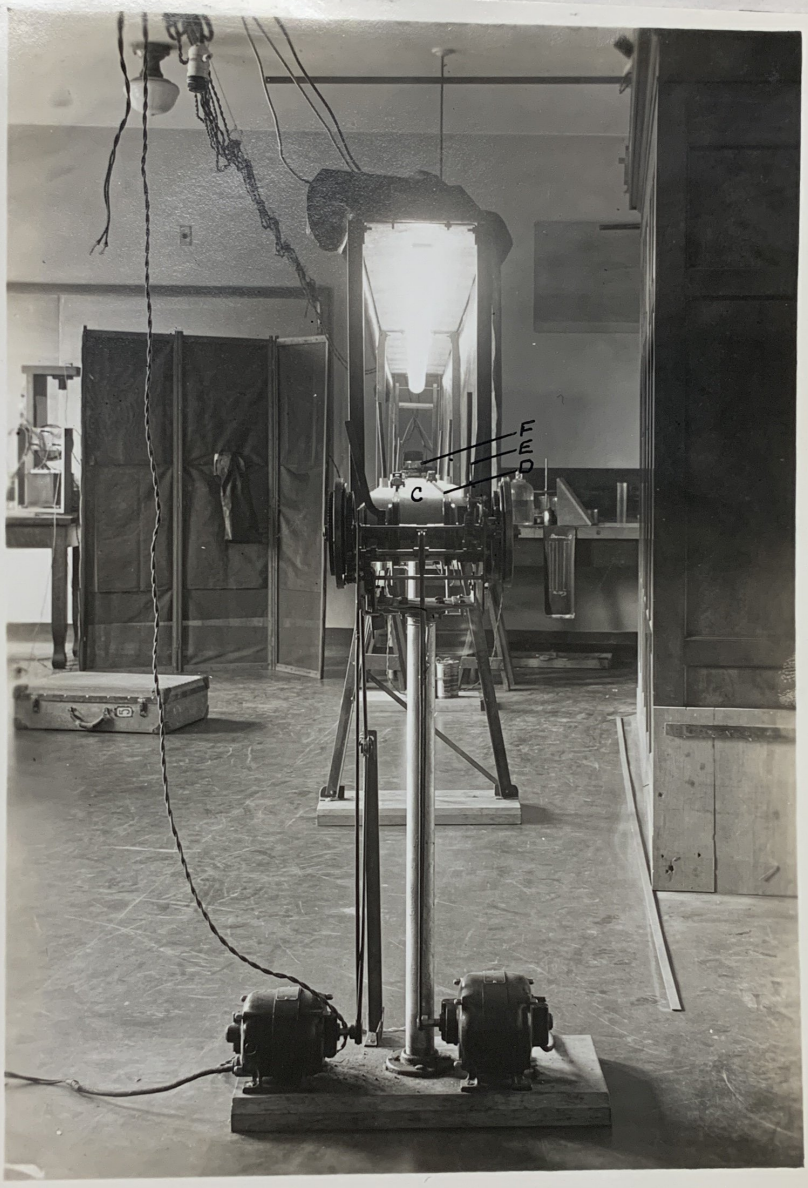


FIGURE 7



FIGURE 7

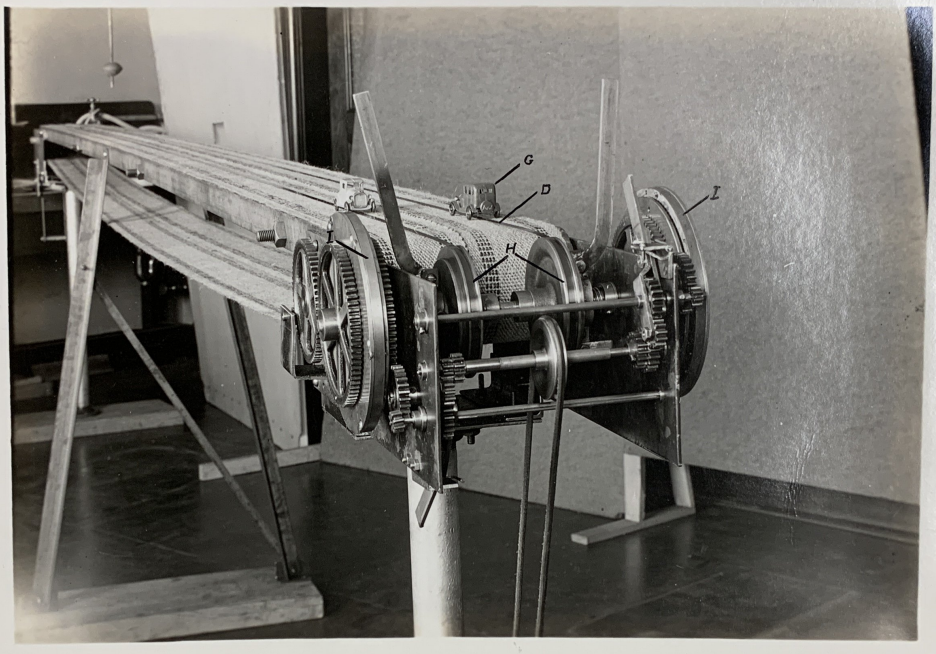


FIGURE 8

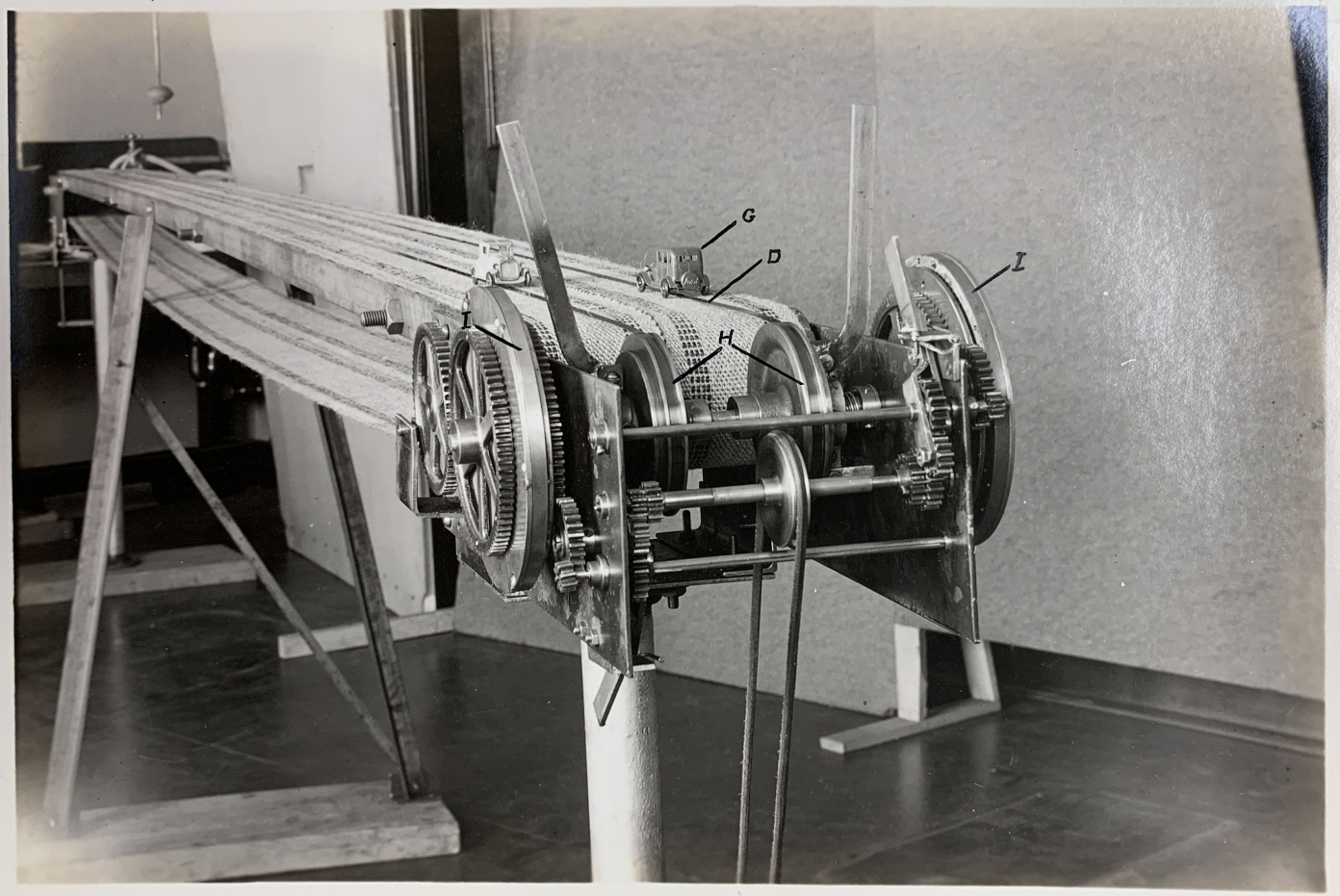


FIGURE 8

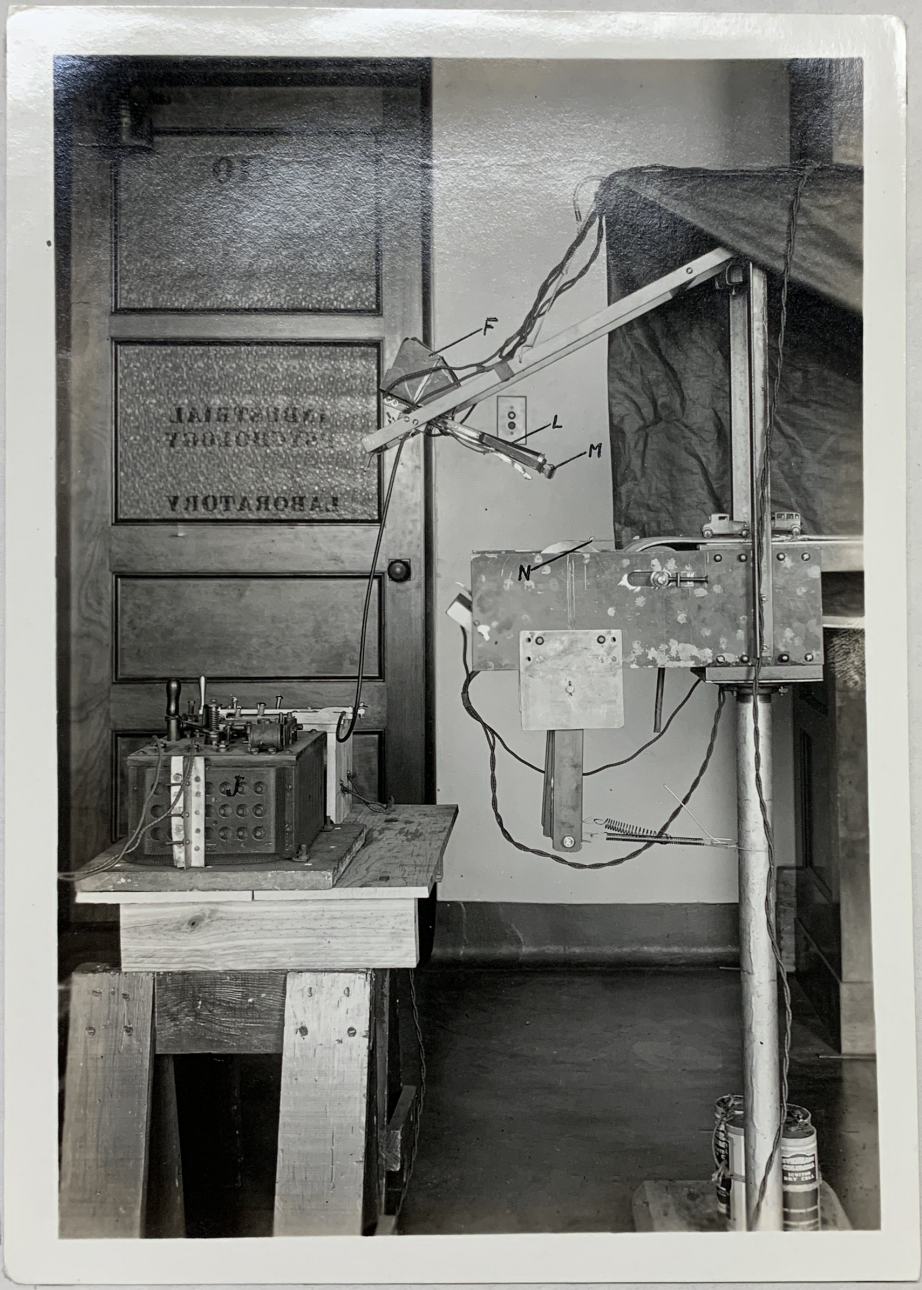


FIGURE 9

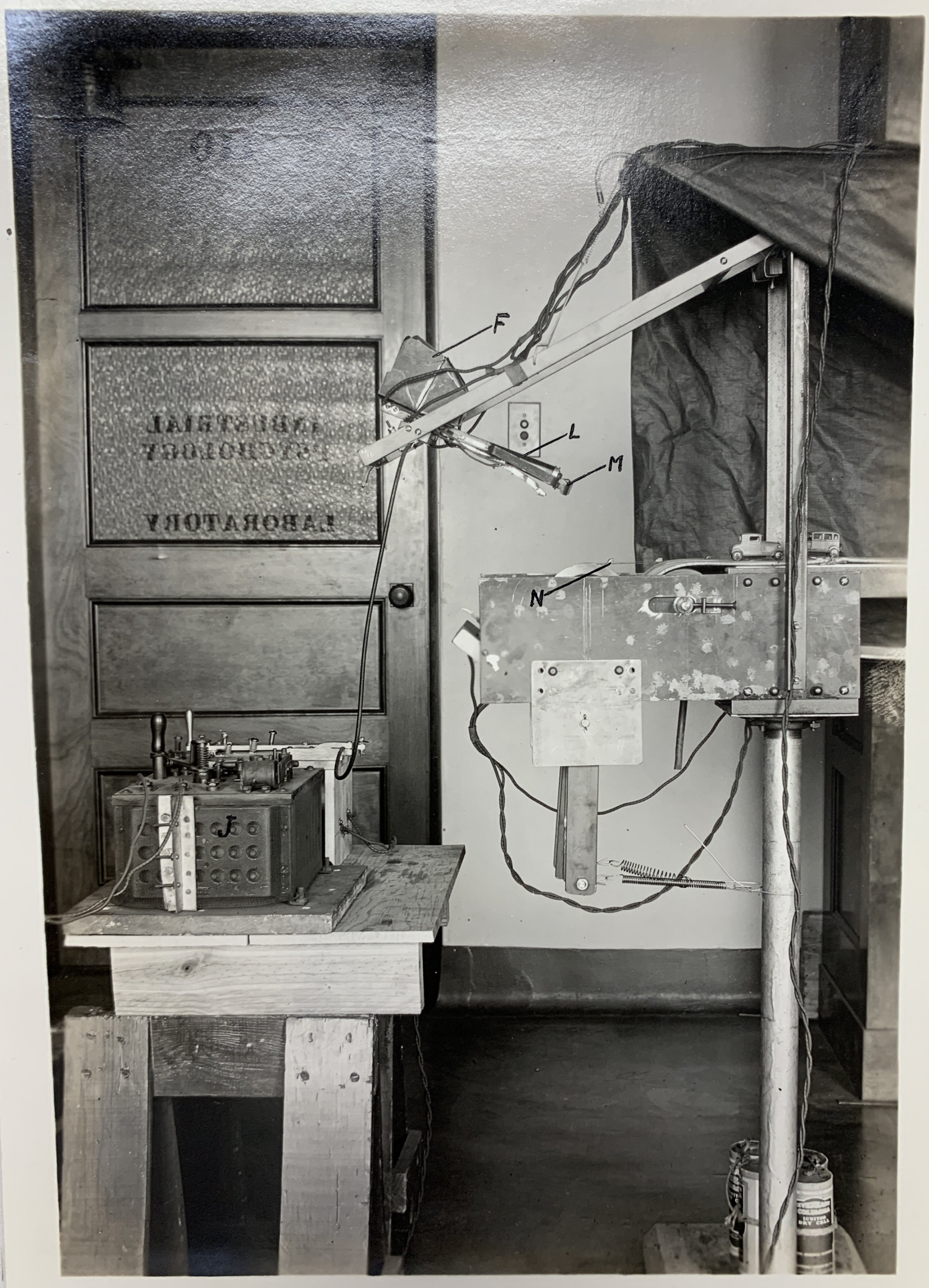


FIGURE 9

runs on crowned wooden rollers. Two $\frac{1}{4}$ inch spring steel endless ribbons (D, Fig. 7 and 8) run over this belt, on brass pulleys. (H, Fig. 8) To these ribbons are attached the miniature cars (G), a standard ten-cent store article. To the canvass belt are attached miniature telephone poles. (E, Fig. 7.)

The two pulleys driving the ribbons are geared at different ratios to a single 1800 r. p. m. A. C. motor. Thus two different but fixed speeds are supplied, the one for approaching cars and the other for those apparently overtaken by the subject.

The canvass belt is driven by a 1/10 H. P. 1800 r. p. m. D. C. motor, the speed of which is varied by the control rheostat (J, Fig. 9) operated by the subject. This motor drives the belt so that the upper surface, which is visible to the subject, is moving towards him.

The belt drive is connected to the ribbon pulley drive (H, Fig. 8) (two brass pulleys) through two systems of planetary gears (I) in such a way that if any given velocity is imparted to the canvass belt (highway) the same speed is added to each of the steel ribbons (cars). This means that the cars will always move at the same rate relative to the highway, no

matter what speed is imparted to the highway through the subject's rheostat.

The subject sits at the end of the apparatus opposite the motors and the experimenter's controls. He has a view of the highway through an eye-piece (F, Figs. 7 and 9) made of black cardboard with rectangular opening, somewhat after the fashion of an auto windshield. The opening is covered with celuloid to represent the windshield, and an imitation hood (L) with radiator and cap can be seen extending out ahead. The hood is fashioned out of wood painted with black enamel, the radiator band being a strip of aluminum and the radiator cap a tinned rivet, sunk until just the head shows.

The carriage upon which the eye-piece is mounted, swings up and back under spring tension when a magnetic release is actuated by safety catches (N) ahead of each car or by a bumper (M) on the eye-piece, so that in the event of failure by the subject to clear a car, no damage will be done to the apparatus. The eye-piece moves laterally under control of a lever operated by the subject's left hand.

A screen of black cloth (A, Fig. 6) surrounds the observer, greatly reducing the illumination so that as he views the highway thru a window in the screen and thru the eye-piece, the light intensity from the

highway will be greater than that reaching the eye from outside the apparatus, thus reducing the danger of adaptation to a changing outside intensity while observing the approaching cars. The actual illuminations as measured by portable illuminometer were approximately as follows:

Canvass belt, 25 foot candles per square foot,

Light transmitted by screen A, 0.4 foot candles per square foot,

Light transmitted thru eye-piece, 0.8 foot candles per square foot.

The total set-up is such that when the belt is stationary; the two streams of cars move as they would on the road, -- one on the right away from the subject and those on the left, towards him. When the wide belt is started this suggests motion of the subject's own car as the road passes under the eye-piece; the cars on the right draw nearer slowly as if overtaken, and the cars on the left approach more rapidly.

Visual Angles

The attempt was made originally with the co-operation of the Department of Optometry, to use mirrors or prisms in such a way as to secure the effect of reducing the interocular distance to correspond to the reduction of the highway. It was hoped that visual factors might thereby be maintained entirely the same

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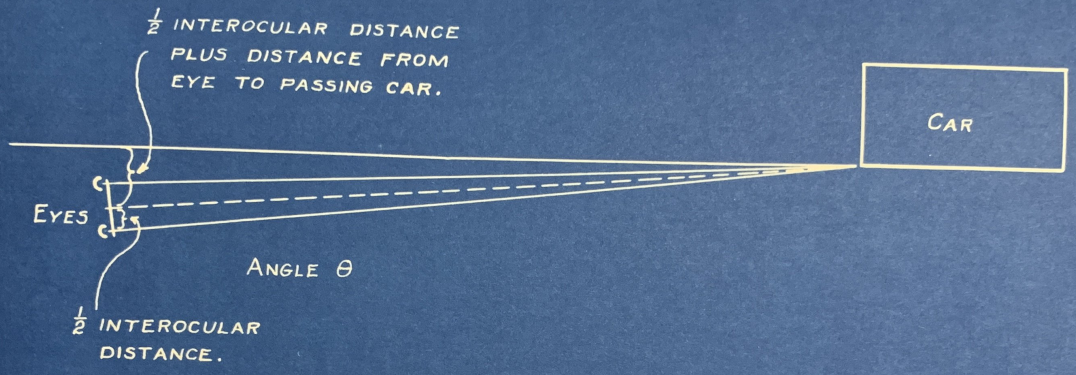
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SHOWING THE ANGLE θ

USED IN COMPUTING de/dt AS AN INDEX
OF THE ANGULAR VOLOCITY OF CONVERGENCE.

FIGURE 10

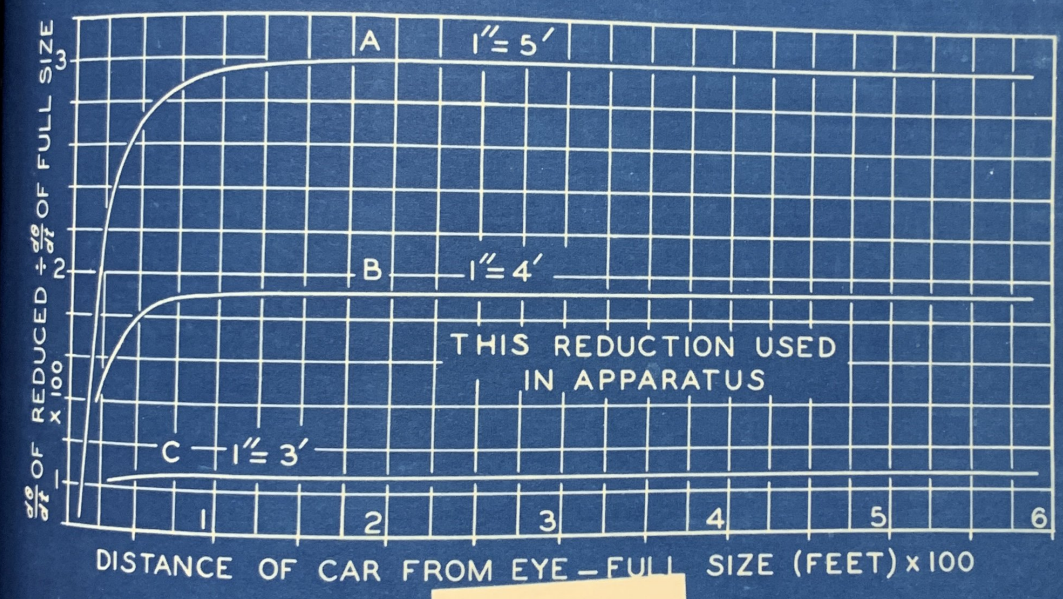

A 1″= 5′
B 1″= 4′
C 1″= 3′
THIS REDUCTION USED IN APPARATUS
dθ/dr OF REDUCED ÷ dθ/dr OF FULL SIZE x 100
DISTANCE OF CAR FROM EYE — FULL SIZE (FEET) x 100

FIGURE 11.

as on the road. The attempt was, however, abandoned as impractical, due to both technical difficulties and the eye strain involved. It was then discovered that the angular velocity found by computing θ/dt, where θ is one-half the angle of convergence, could be kept very nearly a constant function of that in the real situation by making the scale of the miniature large enough.

Curves A, B and C, in Figure 11, represent the quotient of the angular velocity of the reduced scale divided by that of the road situation. The curves show that although the angular velocity does not parallel that on the road in reductions of one inch to five feet and of one inch to four feet, it does much more nearly for a reduction of one inch to three feet (Curve C). It will be seen that curve C representing this quotient for a scale of 1 in. to 3 ft. is nearly a straight line, indicating that the angular velocity for this reduction is very nearly a constant function times that of the original. That is, the angular velocity of convergence is always slightly over one hundred times as great for the miniature as on the road for a similar distance. We therefore used this scale, since it seemed plausible that if this angular velocity of convergence were a constant function of the original, the rate of change of disparity would also be a constant function and the

judgment of velocity therefore should bear some consistent relationship to that in the original or road situation.

The "telegraph poles" mounted on the canvass belt furnished points of reference on the miniature road way. These were of 1/8" by 1/16" rubber tubing reenforced with wood thru the center in such a way as to give the appearance of rigidity but to allow bending which was necessary in passing thru the belt supports.

Scoring

A number of scores were desired. The first was a measure of speed driven by the subject, a second was the number of midjudgments causing release of the eye-piece (smash-ups), and a third was the magnitude of margin by which a collision was missed in cases of near smash-ups. For the first two measures, electric telephone counters were used, contact being made automatically by each overtaken car as it passed over the free pulley and by the electro-magnetic release of the eye-piece respectively. For the third type of score (magnitude of margin of miss), two special counters were used. These counters were designed to operate at between ten and twenty times per second when necessary. Contact points were mounted on each of the free pulley wheels, and a spring brush underneath the wooden hood of the eye-piece. A sliding contact in the eye-piece itself

made contact thru the steel tapes and the pulleys to a different counter for each side. The steel tapes were then coated with insulating varnish except for a space of fifteen inches ahead of the overtaken car, and a similar distance ahead of the approaching car. With this arrangement, if the subject stayed over to the left of the highway too close in front of the on-coming car or cut in quickly after passing one of the cars on the right, each two inches of the tape were recorded by one counter unit for the appropriate side, e.g. if he cut in ahead of the car overtaken at a distance of two inches ahead of the car, the remaining thirteen inches of bare tape recorded a count of six on the counter for cut-ins (car overtaken). Similarly for the approaching car.

Instructions

The instructions read to the subject were as follows:- (Parenthetical statements were not made verbally),

"The apparatus is designed to give you a view just like the one you would get if you were driving a car down the road, only of course, smaller. And since we cannot very well move you over the road, we are going to move the road under your car.

"That is, this is the car which you are driving with the windshield, hood and radiator (pointing to each as mentioned.) Imagine yourself, then, looking out thru

the windshield of your car over the hood and radiator onto the road. The cars on the right hand side of the road here (pointing) will appear as if they are going in the same direction as you are, only slower so that you will overtake them. The cars on the left hand side will be coming toward you just as they do on the road.

"You can steer the car which you are driving with this lever under your left hand. (Demonstrating). This brings you out into the left hand lane, and this brings you back again to the right. This lever (demonstrating right hand lever) acts just like the accelerator on your own car, allowing you to speed up or slow down.

"Suppose, then, that you want to pass a car. You will do it just the way you do on the road, -- first speed up and catch it (advancing R.H. lever as demonstration), then come out into the left hand lane, give time enough to pass the car (that is, time enough for the little car to get back by your car just the way that on the road, you allow time enough to get by it), and then cut back again into the right hand lane.

"There is not room enough to pass when the approaching car is up close. You must therefore vary your speed ahead of time in such a way that you will have time to dodge around before the approaching car gets there. And another thing, -- you can slow down but you cannot stop. That means that you must pass sometime.

"The general principle of the scoring is this, -- the more cars you pass safely, the better your score. That is, both speed and safety count.

"If, after passing a car, you cut in dangerously close or if you drive too close in front of the on-coming car, this counts an error ("mistake" for commercial drivers) against you, but not as serious as if you actually bump. If you actually bump a car, the eye-piece automatically flies up (illustrate) and counts a "smash-up" against you, that is, a more serious error. In order to be safe, you need to allow about a car length extra leeway.

"Whenever the eye-piece flies up, all you need to do is to bring you car back to the right, and the eye-piece will be re-set from the other end of the apparatus.

"Your job in brief, then, is to drive down the road, and pass as many cars as you can, with as few smahs-ups and errors possible.

"We will give you one minute of practise to get used to the controls, and then ask you to run the apparatus for three periods of five minutes each."

Procedure-

After giving the instructions, the subject's name was recorded and the following questions asked,-

1. How long have you driven?

2. Could you give an estimate of average miles driven per year?

3. How many accidents have you had during that time?

4. How did they happen?

In the case of the commercial drivers, these data were supplementary to those obtained from accident reports of the company, taken in connection with the company's own "traffic court" and with reports to the insurance company. In the case of the student subjects, no other accident criterion was available, and these data were secured in case they should be desired later.

The experimenter now retired to his station and started the motors, allowing the subject to manipulate the controls for one minute. During this period he encouraged the subject, gave instructions as to moving the steering lever, speeding up or slowing down, etc. In any case, where the subject showed by doing the wrong thing that he did not understand the task, repetition of instructions and further practise were given and noted. In very few cases was this necessary. The regular test series was one minute of practise followed by three five-minute periods of operation.

At the end of the practise period, the counter readings were recorded and the first trial started. Readings were taken again at the end of each trial. If the subject ran a trial at consistently slow speed, he was warned at the end of that trial, "Remember that both speed

and safety count." If the subject persisted to the end of the third in running very slowly, never trying to pass when a car was anywhere near, a fourth trial was given with the following instructions, "Now I would like you to try once more, and this time run as far as you think you possibly can and still 'get away with it'. Safety still counts, but see whether you can run any faster with safety."

This trial was introduced in order to force all subjects to run at such a speed that misjudgment would be critical. This trial was used in the final score only in as far as it entered in the "Best trial".

The retest used in the case of the students, followed the same procedure except that a fourth trial was introduced in which the current input to the D.C. motor was varied at the end of the second and the fourth minute, thus changing the acceleration obtained by the subject's control lever.

Chapter VI

MAIN EXPERIMENT

Description of Road Test.

Layout and Apparatus

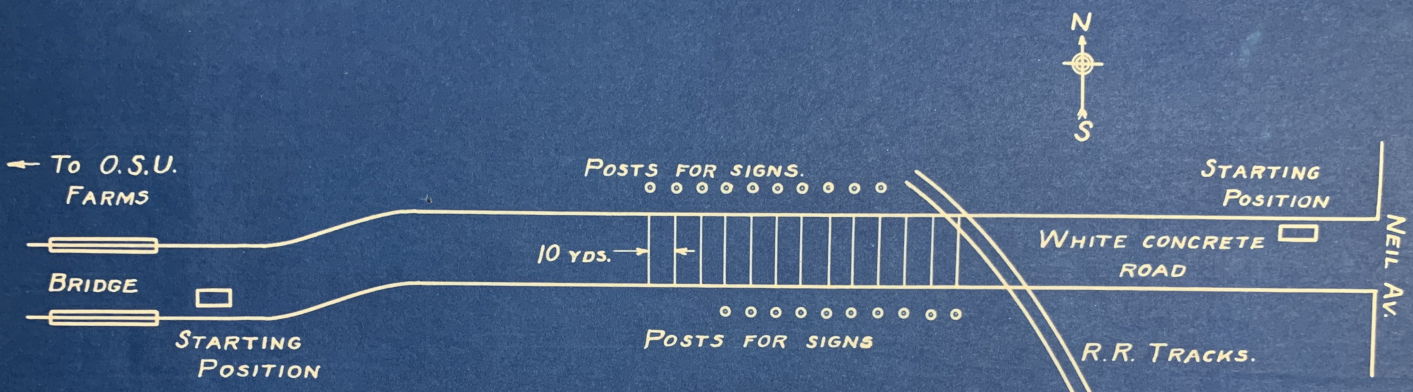
During the Fall and Winter quarters of 1930, a strip of roadway near the Ohio State University stadium was laid off for use in obtaining judgments of velocity in the actual road situation, to be used as a criterion. The general principle was to have the subjects in one car judge where they would meet an oncoming car.

A nearly straight strip of concrete roadway three-tenths of a mile in length was used.

Signs 14 inches x 18 inches with 14-inch letters were made by painting black numerals on white beaverboard. Signs were numbered from one to ten on each side of the road. These signs were placed at ten-yard intervals along the strip of roadway used.

Four-inch lines were painted across the road from post to pst as shown in Fig. 12, in order to provide, if possible, points of reference on the road itself. The lines were alternately orange and white.

LAYOUT OF TEST ROAD





Approximate points of meeting of the two cars
Numbers indicate number of trial.

FIGURE 12

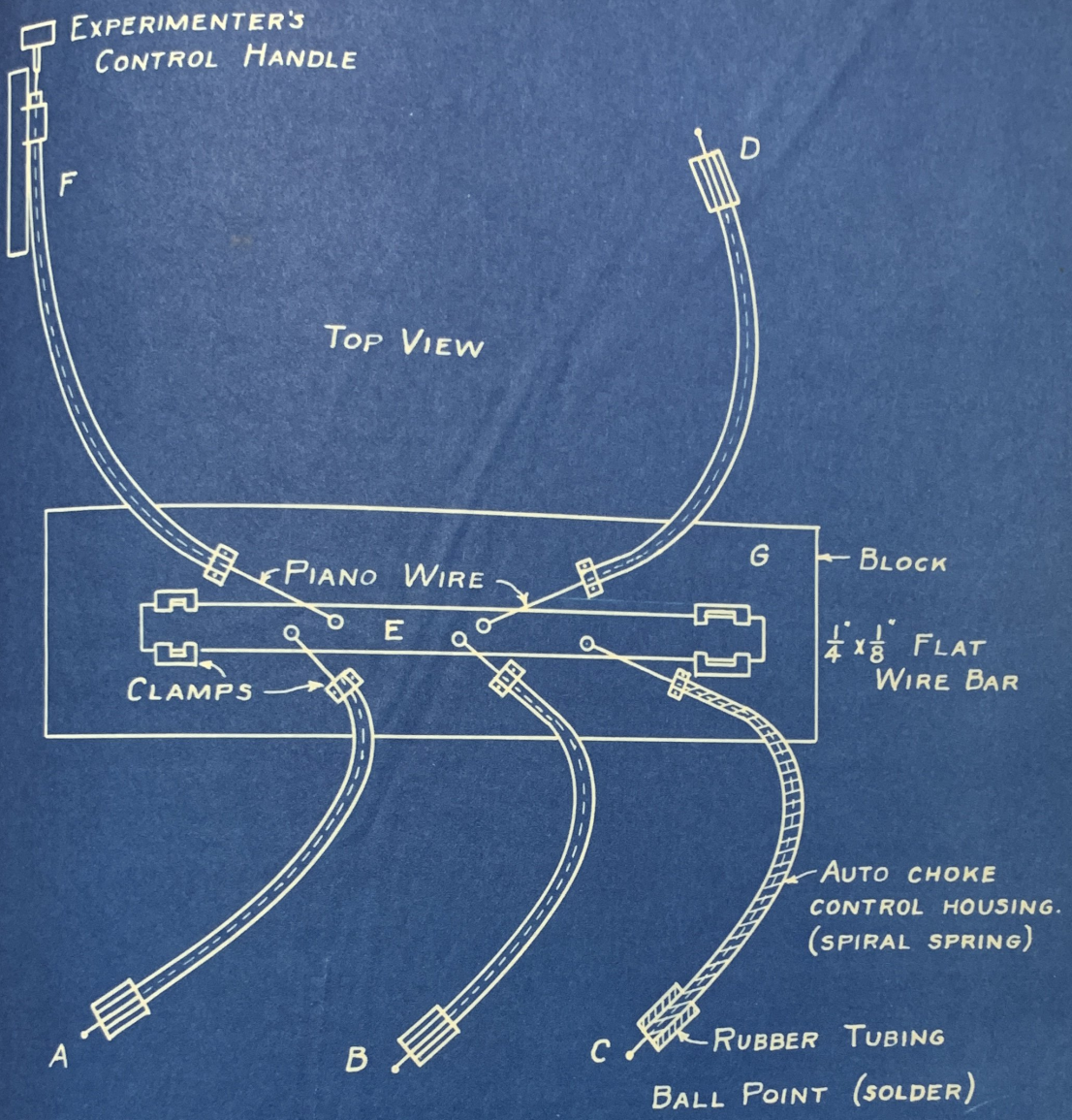


FIGURE 13



FIGURE 13


10
9
8
7
6
5
4
3
2
1



RECORDING APPARATUS

FOR USE IN

ROAD TEST.

FIGURE 14

Figure 13 gives a view of the strip of road with signs set up and cars at point of passing.

An Overland coupe and a Ford Sedan were used. The coupe was loaned and driven by Mr. J. C. Ringwald who generously contributed his time. The Ford sedan was loaned by Dr. H. E. Burtt and was used to transport the subjects.

Four subjects were usually taken at once, but smaller groups were taken when some of the subjects failed to appear as scheduled. The experimenter drove the subjects' car and at a given time the subjects were required to judge at what point the cars would pass. In order to insure control of the time at which judgments were made, the following device was designed.

Four piano wires, A, B, C, D (Fig. 14) running through spiral spring housings such as are used on older type automobile choke controls, were attached to a central master control bar E. The control bar was arranged to slide in brackets attached to the block G, and the spiral housings were also clamped to the block as shown. Thus by moving the bar, the piano wires could be drawn into the tubes at the free ends. A fifth piano wire running through the same kind of flexible housing ran to an ordinary type dash control handle

fastened to a small board which could be clamped under the experimenter's arm while he was driving. This controlled the motion of the master bar.

Ball points were attached to the free ends of the piano wires by dipping in molten solder, and the subjects used these as styli for recording their judgments upon 4 x 6-inch sheets of paper pinned against a writing surface with an intervening sheet of carbon paper, black side up. Rubber tubing fastened at the end of the flexible housing furnished an effective grip and also served to prevent possible use of the end of the housing for recording judgments. With this arrangement, the experimenter could effectively draw the writing points into the housings by pulling the control handle at the predetermined point at which it was desired that all judgments be completed.

In order to guard against possible suggestion or "copying", the subjects wrote inside cardboard boxes about 4 x 6 x 12 inches. The boxes were used vertically and the record blank pinned on one of the inside 4 x 6-inch surfaces. This precaution was probably unnecessary, however, due to the fact that most subjects reported that their own figures were hard to discern due to the fact that the carbon record was on the back of

the paper.

The record sheets bore space for the subject's name, and spaces for ten trials and two practise trials. The subject wrote the number of a sign in the space provided for the given trial. In reading the records, they were viewed against a light.

Procedure

The two cars took positions at opposite ends of the test strip. The experimenter with the subjects in the sedan, read the following instructions after supplying the subjects with record sheets and briefly explaining the use of the recording apparatus.

Instructions -

"You see the roadster waiting on the bridge. When we have become acquainted with the recording device, I will signal with the headlights and both the roadster and our sedan will start toward each other. We will gain a certain speed as quickly as possible and hold it for the remainder of that trial. This will make us meet somewhere among the signs which you see beside the road. The signs are numbered from one to ten and you are to write down the number at which you think the cars will meet. If you think the meeting point will be between

two signs,for instance, between five and six, write five and a half. Use the signs on the right each time. There are lines drawn across the road, in order to help you to connect the signs with points on the road.

"The wires are to be used as pencils, and you might each write your name with them now to gain a bit of practise in using them.

"As the two cars approach, I will give you the signal, "ready" which means that you should immediately make up your mind as to which sign will be the point of meeting, so that when I say, "Now" you can immediately look down and write. I will allow you just time enough to write after giving the signal, and then will withdraw the writing points like this (Demonstrate). We simply want to make sure that everyone makes his judgment by a certain time, because even when subjects are trying to co-operate, as you probably all are, some people are liable to lag a little after the others unintentionally."

The motor was then started, and the headlights flashed. Upon a return signal from the waiting car, both cars were put in motion. A predetermined schedule of speeds which had been empirically selected so as to vary the meeting point rather irregularly was followed

by the approaching car. The attempt was made to run the subjects' car at the same speed for all trials, and to reach running speed at the same point for all trials. This was intended to eliminate possible auditory cues from different motor speeds, etc. The approaching car also attempted to reach running speed at standard points for different trials, in order to keep the series as nearly the same as possible from group to group. Practically, of course, it was impossible to keep each series exactly the same as the others. This will be mentioned in analysis of results.

The speeds used by the coupe were these:-

Practice 28, 32 miles per hour.

Regular 40, 32, 28, 35, 42, 16, 27, 22, 22, 30 miles per hour.

These were designed to place the meeting points as shown in the insert in Figure 12. The diagram represents roughly the stretch of road enclosed by signs. The numbers refer to the trial and indicate the relative meeting points, e. g. for trial one this was near the east end, for trial two at the west end, etc.

Two practise trials were given and then ten regular trials. Procedure was "with knowledge" in that the subjects could see the actual point of passing. On

the retest, which was given to all subjects, the same two practise trials and ten regular trials were given.

The experimenter gave the signal, "Ready" at a point about thirty yards from sign number one and the signal "Now" at a point about twenty yards from the same sign. The styli were then withdrawn at sign number two. These points were picked after actual trial in order to give just time for subjects to write their figures. In a few cases, where the point of meeting occurred before reaching sign two the styli were withdrawn just before meeting. However, in the majority of cases, the meeting point occurred beyond sign two, and the subjects had ample time to record their figure before the styli were withdrawn.

In a few cases, subjects attempted to watch the approaching car continuously so as to correct their judgment up to the last minute, rather than to make it on signal. In these cases, they almost invariably were caught in the midst of the changed figure, and complained of too short time. They were then instructed to make the judgment first, and then look down as they wrote since they would not have time to write the figure unless they started on signal. In no case, were they unable to do so successfully by using this method.

The actual point of passing was observed by the experimenter who noted the number of the signs between which the cars would pass, and then shifted his fixation to a point just ahead of his own left front wheel which he estimated to be the position of his bumper. As the front of the approaching car reached this point, he noted the relative position of the spot between the lines and recorded it to the nearest half, e. g. five or five and a half.

In order to check the experimenter's record of the point of meeting, the second driver also recorded his observation. The difference obtained from seventy-seven trials by subtracting Ringwald's observations from thos of Forbes, are shown in the following table:-

TABLE XV

Difference in Actual Meeting Point as Recorded by Two Experimenters

Difference (Forbes minus Ringwald's) In Terms of Recorded Sign No.	Yards	Frequency (Trials)
1	10	1
$\frac{3}{4}$	7.5	2
$\frac{1}{2}$	5.0	22
$\frac{1}{4}$	2.5	30
0	0.0	21
$-\frac{1}{4}$	-2.5	1

The table shows that the experimenter's record was consistently a larger figure than Ringwald's. In twenty-two trials out of the seventy-seven, this difference amounted to a recorded difference in the actual meeting point of five yards, in thirty trials a recorded difference of two and a half yards, and in twenty-one trials there was agreement.

If the experimenter's record shows a constant error, this should affect the absolute error as indicated in the results but it should not affect the relative standings of the subjects. In so far as this error may not be constant, it may affect the apparent reliability of the subjects' judgments. However, it seems probable that this error is not greater than two and one half yards (since this amount of error by each observer would give a difference of five yards as shown in the table), and there should be some balancing out during the ten trials.

Chapter VII

MINIATURE HIGHWAY RESULTS

Commercial Drivers

The apparatus was taken to Dayton, Ohio, with the co-operation of the Dayton Power and Light Company and set up in the rooms of the Dayton Chamber of Commerce where drivers from various commercial concerns were tested under the auspices of the Dayton Safety Council. The testing was done on week ends during July and August of 1930. Complete records were obtained for thirty-nine subjects of whom eighteen were employees of the Dayton Power and Light Company. The remainder were from five other concerns. Of the thirty-nine four were from small concerns with no accident records or ratings obtainable, and four were cases of known poor vision. These eight records were not used in the analysis, therefore, and the remaining thirty-one cases formed the basis for the statistical analysis which follows.

Criterion

Accident records were obtained from the company in the case of the Dayton Power and Light

TABLE XVI

Non-Accident Group				Accident Prone Group			
Subject	Yrs. Driven	No. of Accidents	Yrs. per Accident	Subject	Yrs. Driven	No. of Accidents	Yrs. per Accident
1	17	1	17	24	9	2	4.5
2	18	0	18*	25		6 plus	0.12 reported
3	13	0	13*	26	Rated acc. prone		?
4		0	?	27	5	3	1.7
5		1	?	28	Rated acc. prone		?
6	Rated good		?	29	7	2	3.5
7	13	0	13*	30	8	2	4.0
8	Rated good		?	31	Rated acc.prone		?
9	5	1	5	32	1	2	0.5
10	13	0	13*Z	33	3	3	1.0
11		0	?	34	2	4	0.5
12	15	0	15*	35	2	4	0.5
13		0	?				
14	Rated good		?				

TABLE XVI -continued

Non-accident Group				Accident Prone Group			
Subject	Yrs. Driven	No. of Accidents	Yrs. per Accident	Subject	Yrs. Driven	No. of Accidents	Yrs. per Accident
14	Rated good		?				
15	5	0	5*				
16	3	0	3*				
17	7	0	7*				
18	8	1	8				
19	12	1	12				

*These figures show years without any accidents. They may therefore indicate a better record than the figure given.

employees. Of the remaining thirteen, four were sent in as accident cases and admitted having had accidents. The remaining nine claimed a good record and were placed in the "non-accident" group on the basis of this claim. There is, always, of course, the tendency for a driver to state his record as better than it should be, in the case of accident prone individuals. There is also the possibility that some of the men had had accidents before entering the employ of the company, in the cases where accident records were kept by the company. One subject reported having had a series of such prior accidents when with another company in a different city. In this respect our grouping on the basis of accidents is probably conservative.

The following table summarizes the data available as to accident records. The table shows that all subjects classified in the accident group were either sent in as accident prone or else had a record of one accident in four years or worse.(20) It will also be seen that, although the cases are treated as two groups the accident records actually show a gradation from one accident per half year and per one year.

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(20) See A. R. Lauer - Frequency of Accidents, Psychological Principles in Automotive Driving, O. S. U. Contributions in Psychol. #11.

The following sample records show the kind of test data obtained for each subject.

Items 1 and 2 come directly from the electric counters mentioned in Chapter V which count the number of cars passed and the number of times the eye piece is released, i. e. smash-ups. Items 3 and 4 come from other counters and are in terms of arbitrary units of two-inch lengths, the higher scores indicating respectively that the subject cuts in more quickly or stays longer in the path of the approaching car. Side swipes were recorded by the examiner. The number of safe passes is obtained by subtracting item 2 from item 1.

In the record for subject 36 one of the better performances while that of subject 24 represents one of the poorer ones, it will be noted that subject 36 not only passes over twice as many cars during 15 minutes but side-swipes fewer in the process than subject 24. Further, even though subject 24 improves from trial to trial he never reaches more than about half the safe passes of subject 36. And, in watching the two performances it seemed evident that the smash-ups in the second record were due to inability of the subject to foresee the relationships of the cars far enough in advance to

TABLE XVII

Sample Records for Good and Poor Subject.

Item	Trial 1	2	3	Total
Subject 36 (Good)				
1. Cars passed	47	47	48	142
2. Smash-Ups	2	1	1	4
3. Cut-in after passing	97	111	123	331
4. Dangerously close to app. car	19	14	11	44
5. Side Swipes	2	1	2	5
6. Safe passes	45	46	47	138
Subject 24 (Poor)				
1. Car Passes	32	30	30	92
2. Smash-ups	17	6	10	33
3. Cut-in after passing	57	46	68	171
4. Dangerously close to app. car.	25	20	24	69
5. Side swipes	4	1	3	8
6. Safe passes	15	24	20	59

avoid hitting them.

Treatment of Data

The data for each measure were recorded, coded, and punched for Hollerith machine correlation. Each trial was treated separately as well as a total of the three, making four variables under each of the following heads:-

Number of cars passing end of apparatus(speed)
Number of releases of eye-piece(smash-ups)
Number of safe passes(car minus smash-ups)
Cut-ins in front of slow car(car approaching)
Cut-ins in front of slow car(car followed)
Number of side swipes.

The best record in terms of safe passes made in any of the trials, was also recorded as a separate variable, and, as a rough measure of learning, the best score minus the score of the first trial in safe passes, was used. This gave a total of twenty-six different scores upon each individual.

The ΣX, ΣX^2, and ΣXX values were obtained by running the cards through the Hollerith sorter and then through the sub-total adder and printer. The sub-totals were then added by hand to give the desired

avoid hitting them.

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Number of safe passes(car minus smash-ups)
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values for use with Toops' formulae. (32) The cards were run in two groups corresponding to the previously mentioned accident classification.

Correction for Small N

Since the number of cases finally used was small, Fisher's "t" function was used in place of the usual critical ratio in the group difference technique. For convenience, Fisher's formula was converted into the more usual terms, i. e. N and T.

Fisher's Formula is:(11)

$$\bar{x} = \frac{1}{n_1 + 1} S(x) \qquad x' = \frac{1}{n_2 + 1} S(x')$$

$$s^2\left(\frac{1}{n_1 + 1} + \frac{1}{n_2 + 1}\right) = \frac{(n_1 + n_2\ 2)}{(n_1 + 1)\ (n_2+1\ (n_1+n_2}\left(S(\bar{x}-\bar{\bar{x}})^2+S(x-x')^2\right)$$

$$t = \frac{\bar{x} - \bar{x}'}{s} \sqrt{\frac{(n_1+ 1)\ (n_2 + 1)}{n_1 + n_2 + 2}} \quad \text{where } n = n_1+ n_2$$

(15) Fisher, R. A. - Statistical Methods for Research Workers, London, Oliver and Boyd, 1930, p.107 and table p. 139.

The formula as revised is:

$$s = \frac{N_1\sigma_1^2 + N_2\sigma_2^2}{N_1 + N_2 - 2}$$

$$t = \frac{M_1 - M_2}{\sqrt{N_1\sigma^2 + N_2\sigma_2^2}^2} \sqrt{N_1 + N_2 - 2} \qquad \sqrt{\frac{N_1 N_2}{N_1 + N_2}}$$

This is obtained from Fisher's formula by substitution of the following values, and simplifying:-

$N_1 = n_1 + 1 \quad N_2 = n_2 + 1 \quad S(x-\bar{x})^2 = N_1T^2$ etc.

Table XVIII shows a summary of the job analysis form for obtaining "t" values for each of the twenty-six variables obtained for the thirty-one commercial subjects.

Table XIX shows the "t" values alone in a more convenient form for each trial and also for the total score made in three trials. Thus the "t" value for cars passed(speed) is .22 for trial one, .15 for trial two, .06 for trial three and .42 for the total of three trials. The larger the value of "t", of course, the more significant the difference between the means of the accident and non-accident groups. The section at the bottom of the table facilitates the interpretation

TABLE XVIII

Group Differences - Miniature Highway Scores

Commercial Subjects

Variable	Mean k N. Ac.	Mean Ac.	Diff.	Diff x $\sqrt{\frac{N_1 N_2}{N_1 N^2}}$	$\sqrt{N_1+N-2}\ \sqrt{N_1\sigma^2+N_2\sigma_2^2}$	t (Product/$\sqrt{\quad}$)	
				Diff x 14.6047			
2	2.7368	8.4167	.3201	4.6750	21.0380	.222	Cars passed
3	9.9474	9.7500	.1974	2.8830	19.7281	.146	
4	8.0000	7.2500	.7500	1.0954	19.0853	.057	
5	7.8947	7.4166	.4781	6.9825	16.2698	.429	
6.	4.9473	6.8333	1.8860	27.5444	24.5047	1.124	Smashups
7.	3.9473	4.2500	.3027	4.4208	19.2665	.230	
8.	2.9473	3.5000	.5527	8.0720	17.8870	.451	
9.	4.2105	5.7500	1.5395	22.4839	22.2128	1.012	
10.	7.2631	6.2500	1.0131	14.7960	14.7626	1.002	Safe Passes
11.	8.4210	7.9166	.5044	7.3666	16.3569	.450	
12.	11.1052	9.6666	1.4386	21.0103	18.2881	1.149	Best Slow Cut-ins
13.	9.7894	8.5000	1.2894	18.8313	18.3346	1.037	
14.	9.9474	7.5833	2.3641	34.5269	20.8773	1.654	
15.	5.1053	8.0833	2.9780	43.4927	23.2531	1.870	

TABLE XVIII -continued

Group Differences - Miniature Highway Scores

Commercial Subjects

Variable	Mean N. Ac.	Mean Ac.	Diff.	Diff x $\sqrt{\frac{N_1 N_2}{N_1 N^2}}$ $\sqrt{N_1+N^2-2}$ $\sqrt{N1\sigma_1^2+N_2\sigma_2^2}$	t (Product/$\sqrt{\ }$)	
				Diff x 14.6047		
16.	4.1578	8.3333	4.1755	60.9818	22.5209	2.708 Slow Cut-ins
17.	4.4736	7.7500	3.2764	47.8507	25.9804	1.841
18.	4.1578	7.5000	3.3422	48.8117	20.6283	2.366
19.	2.7894	3.7500	.9606	1.4029	14.2621	.098 Fast Cut-ins
20.	3.1579	3.2500	.0921	1.3451	16.0865	.084
21.	2.8421	3.4167	.5746	8.3918	17.0717	.492
22.	3.6842	4.3333	.6491	9.4799	16.6964	.568
23.	1.3684	2.3333	.9649	14.0920	9.9543	1.416 Side-Swipes
24.	1.2105	2.1666	.9561	13.9635	8.5337	1.636
25.	1.1053	2.1666	1.0631	15.4999	9.0253	1.717
26.	3.6842	6.6667	2.9825	43.5584	24.0576	1.811 Best-first
27.	4.4210	4.7500	.3290	4.8049	21.7458	.221

TABLE XIX

Differentiation Obtained in Different Scores on the Miniature Highway Commercial Drivers

	Trial 1	Trial 2	Trial 3	Total
Cars Passed	.22	.15	.06	.42
Smash-ups	1.12	.23	.45	1.01
Safe-Passes	1.00	.45	1.15	1.03
Best Trial) Safe Passes)				1.65
Cut-Ins (Car over-taken)	1.87	2.71	1.84	2.37
Cut-Ins (Car approaching)	.10	.08	.49	.57
Side-Swipes	1.42	1.64	1.72	1.81
Learning (Best-First Safe Passes)				.22

"t" values	1.055	1.311	1.699	2.045	2.462	2.756
n= (N=31)						
Chances of a true diff.	.70	80	90	95	98	99

of the "t" values and merely reproduces one row of Fisher's table for the N involved in the present case.

Table XIX shows that Cut-Ins(car overtaken) are the most significant in differentiating the accident from the non-accident group, that side-swipes are next, that the best trial in terms of safe passes is third, while the number of safe passes in the third trial are about equal in fifth place. The chances of a true difference in the foregoing run from about seventy-five to about ninety-seven out of a hundred. Cars passed, cut-ins (approaching car) and learning are not at all differential.

Since cut-ins (car overtaken) were most differential, and since the total represents more trials and should therefore be more reliable than any single trial, the total cut-in score was used as the best variable upon which to base a combination score.

Table XX shows that Total Cut-Ins correlate highly with Smash-ups (second trial and total) and with Cars passed, first trial. Total Cut-ins also correlates between + .55 and +.60 with Side-swipes (second, third and total).

The correlations were based on the non-accident group in order to eliminate the possibility of a

TABLE XX

Intercorrelations

Total Cut-ins(Car overtaken) with Other Scores.

Commercial Subjects

Non-Accident Group

	Trial 1	Trial 2	Trial 3	Total
Cars passed	.769	.068	.175	.406
Smash-ups	.430	.851	.354	.768
Safe-Passes	.665	-.421	.101	-.003
Best Trial(Safe)				.010
Cut-Ins -Fast Car	.320	.376	.071	.308
Side Swipes	.352	.562	.552	.597
Learning				-.091

Probable Errors

r	.00	.30	.60	.70	.80
P. E.	.16	.14	.10	.09	.06

spuriously high correlation due to selection of the groups. Correlations for the accident group were not computed. The correlations were needed merely to indicate which variables to combine.

Table XXI selects from Table XX those variables showing the highest correlation with cut-ins and takes from table XIX the "t" values for these same variables.

We see that of the variables which correlate with Total Cut-ins to an extent greater than +.50, Smash-ups and Side-swipes show fair group differences, whereas Cars Passed, first trial, shows very little group differentiation. Since Total Cut-ins shows large differentiation, we may conclude that although it is correlated to the extent of +.77 with Cars passed, first trial, for the non-accident group, total Cut-ins is not merely a measure of number of cars passed for all subjects. The possibility is still left that it is a measure of the same factors as Smash-ups and Side-swipes to some extent. The latter is plausible since we also know that side-swiping a car must automatically bring the highest possible cut-in score due to the fact that the counter circuit is closed by moving the eye piece back to the right. The relation between Cut-ins and Smash-ups will be made in connection with the road test criterion.(Chap.VII)

TABLE XXI

Differentiation and Intercorrelation
Commercial Subjects.

Score	"t" value	r with Cut-ins (Car overtaken) (Non-accident group only)
Cars Passed - First	.22	ŧ.77
Smash-ups - First	1.12	ŧ.43
Smash-ups -Total	1.01	ŧ.77
Safe Passes - Third	1.15	ŧ.10*)
Safe Passes - Total	1.03	- .003*))r= ŧ .71
Best trial (safe)	1.65	ŧ .01*
Side-Swipes - First	1.42	ŧ .35
Side-Swipes - Second	1.64	ŧ .56
Side-Swipes - Third	1.72	ŧ .55
Total	1.81	ŧ .60

*Variables giving highest group difference with lowest intercorrelation.

Thus the variables which appear to give the best possibility of higher validity if combined with cut-ins are Safe-passes, (Third and Total) and the Best Trial. Since the first two correlate with each other to the extent of .71, we used only the former because of its higher "t" value.

Table XXII shows the differentiation obtained by combining scores in various ways. Variable 29 represents a score obtained by simply adding scores for Total Cut-ins, Safe passes (third trial and Best trial). The combined score was actually worked out for each case and then analyzed for group difference in terms of the "t" function, yielding a "t" value of 2.39 and a corresponding probability of a true difference of ninety-seven in a hundred. For the variablesweighted t/sigma, the same procedure was followed, except that the attempt was made to weight the components optimally. In order to do this, standard scores were obtained by dividing through by the standard deviation (sigma). This served to equalize the weighting, so that by multiplying by the "t" value the final weighting (t/sigma) weights each score proportionately to its differentiating value as indicated by "t". Since N is the same for all, "t" may be used in this way, while it could not be if N were different for different scores. The combination scores

TABLE XXII

Group Differences

Combination Scores

Commercial Subjects.

Score	"t" value	Chances of Real Diff.
#14 Best Trial(Safe Passes	1.65	89
#29 Total Cut-ins minus (Best + Third Safe Passes)	2.39	97
#30 Same variables as in 29 weighted t/sigma	2.46	98
#31 Ratio Total Smashes to Cars passed	1.53	85
#32 Ratio Total Cut-ins to Safe Passes	3.25	over 99
#33 Best trial and #32 weighted t/sigma	3.33	" 99
#34-31 and 32 weighted t/sigma	2.96	" 99

obtained by this process (variables 30, 33, and 34) yielded slightly higher probabilities of group differences, than the corresponding scores obtained by mere summation.

Table XXII also shows that the ratio of Total Cut-ins, to Safe-passes gives next to the highest differentiation, while this same variable, when combined with the Best trial, weighted t/sigma, gives the highest differentiation which has been obtained.

Table XXIII also shows that Total Smashes (item 31) are more differential when relative to the number of possibilities, i. e. Cars passed. Likewise, Total Cut-ins are more differential when considered relative to the number of possibilities for recording cut-in scores, i. e. the number of safe passes.

TABLE XXIII

Comparison of Absolute and Relative Scores

Score	"t" Value From Table XIX (Absolute)	"t" Value From Table XXII (Relative) to number of possibilities
Total Smash-ups	1.03	1.53
Total Cut-ins	2.37	3.25
	Intercorrelation	
Smash-ups per car and Total cut-ins per car		$r = +.78 \pm .08$

Variable 34,(Table XXII) yields a somewhat lower "t" value than Total Cut-ins per safe pass as might be expected from the "t" values and the high intercorrelation shown for the two scores used (see Table XXIII). However, the chances of a true difference are still greater than ninety-nine out of a hundred, and since it would be possible under some circumstances for a subject to avoid Cut-ins by increased number of Smash-ups, it would probably be wise to retain Variable 34 as well as Variable 33 in case the apparatus should be used for testing purposes.

Reliabilities and Analysis

Correlations between the total score and the score in each trial for that same measure, are shown in Table XXIV. It is obvious that the total score is in most cases related to the score on each trial to a marked degree.

The table shows that the total score is in most cases affected by the score on each trial to rather similar degree. Trial one in Cars passed and Trial one in Safe passes, however, correlate distinctly lower with the total score than the other trials in their respective cases. In the case of cars passed, this may be due to the difference in caution displayed in the first trial by subjects who later, upon familiarity, run at much

TABLE XXIV

Correlation of Total with Each Trial

Miniature Highway Scores

Commercial - Non-Accident Group

19 Subjects

	Trial 1	Trial 2	Trial 3
Cars Passed	+.471	+.787	+.709
Smash-Ups	+.713	+.860	+.597
Safe-Passes	+.294	+.682	+.710
Cut-Ins - Slow Car	+.898	+.918	+.896
Cut-Ins - Fast Car	+.894	+.708	+.834
Side Swipes	+.833	+.886	+.660

TABLE XV

Internal Reliabilities

Best Variables - Commercial Subjects

	r_{12}	r_{23}	r_{13}	Brown-Spearman r (38) x (38)
Smash-ups per Cars Passed	+.295	+.367	+.012	+.474
Cut-Ins per Safe Pass	+.648*	+.570*	+.506*	+.802

*These would be materially higher except for two cases which deviate widely

different rates. There is an increase in speed on the part of some subjects. The difference in the safe passes may be a function of this factor of caution.

TABLE XXVI

Correlations of Safe Passes and Speed

	Cars Passed		
Safe Passes Trial	1	2	3
Trial 1	.78	-.23	-.20
Trial 2		.80	.35
Trial 3			.84

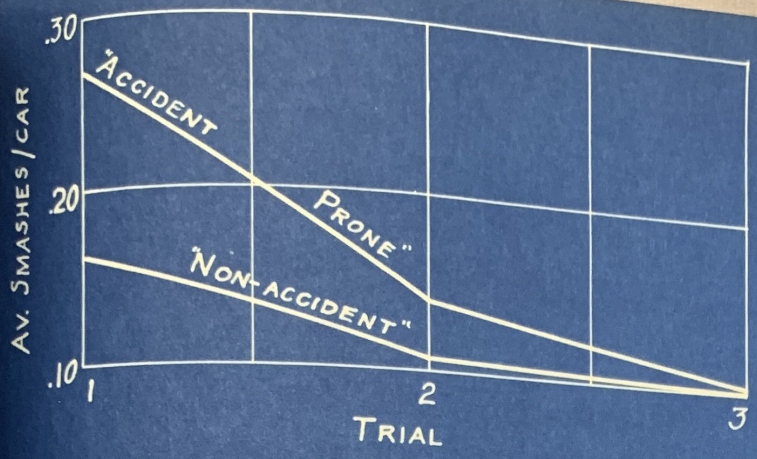
Table XXVI shows the correlations between safe passes and cars passed (speed) for the non-accident group.

For each trial, Safe-passes correlates about .80 with Cars passed for the non-accident group. The correlations of safe passes in one trial with cars passed in another are consistently lower. We may say, therefore, that the safe pass score is probably a function of the cars passed for this group. However, reference to Table XIV will show that Safe passes is much more highly differential than Cars passed, and we may therefore conclude that the two are not as interdependent when the levels of ability of the groups are different. That is,

for subjects of higher ability (as measured by freedom from accidents), the faster they run the apparatus the more Safe passes they attain, but for subjects of lower ability this is not the case. We should have the correlations for the accident group to complete the evidence on this point. This state of affairs is what would be expected, since the score in question might be characterized, from its composition, as a measure of successful speed. Also, on the basis of this reasoning we would expect the Best Trial to be a measure of skill and judgment rather than of caution, due to the fact that where undue caution was exhibited in the first three trials, pressure was brought to bear to overcome it in a fourth.

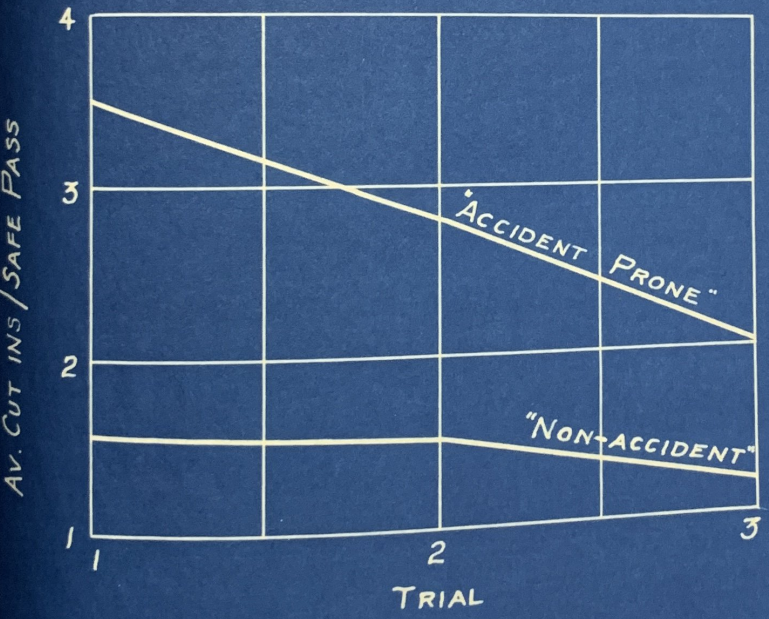
If we may call running at slow speed, cautious manipulation, we have already seen in Table XIX that cautious manipulation of the apparatus (cars passed) is not in itself indicative of accident proneness.

Moreover, the fact that the group differences obtained were increased by taking smash-ups and cut-ins relative to Cars passed and Safe passes in Table XXIII, seems to indicate that those who show cautious manipulation may make a comparatively poor record as regards Cut-ins and Smash-ups. Whether or not cautious manipulation under the circumstances of our test, bears any relation



VARIABLE 31
AVERAGE SMASHES PER CAR

FIGURE 15



VARIABLE 32
AVERAGE CUT INS PER SAFE PASS

FIGURE 16

to cautious driving, is still a problem for investi-
gation. However, it seems clear that we are differ-
entiating the groups on something other than mere
cautious manipulation of the apparatus.

Reliabilities

Table XXV shows that the internal reliability obtained by averaging the intercorrelations of trials one, two and three and using the Brown prophecy formula for N = 3, is .47 for Smash-ups per Car but .80 for Cut-ins per Safe pass. The latter is a fair reliability for a test of this length. The low reliability of the former may perhaps be explained to some extent by Figure 15.

Figure 15 shows that in variable 31 (Smashes per car) differentiation between groups is greatest in the first trial and that with practise, the two groups tend to reach the same level. We are likely to find the relative positions of the subjects changing due to this process, explaining the low reliability above mentioned. The tendency to reach the same level, together with reports of some subjects that they come to react to absolute position, etc., suggests that the task should be made more complex in order to give a better

differentiation on this variable.

Figure 16 shows that although there is some tendency for the groups to approach the same level, in Cut-ins per Safe pass, this tendency is not nearly so great as that shown in the preceding figure. In general the non-accident group does not improve very much beyond the level at which it starts, and although the accident group does improve there is still a rather wide difference at the end of the third trial. This fact suggests that the variable we are measuring is to some extent an habitual reaction rather than skill acquired in the particular experimental situation.

Since this cut-in per safe pass is our most differential score, the question of just what type of measure it is, becomes significant. The following points in the experimental set-up suggest that we have here measurement of a habitual reaction involving "timing" of movements and "grasping" of mechanical relationships in a situation of this sort. (1) The set-up was such that the subjects might cut in very closely and in some instances actually side swipe cars without seeing what was occurring. They would therefore have no cue on the basis of which to correct their procedure. However, when such a reaction caused the eye-piece to

release or a car to be caught, as it sometimes did, subjects often asked "What is the matter? What did I do? " etc., seeming to indicate that those who did side-swipe failed to react to all the factors of a complex situation. (2) Some subjects would seemingly become "rattled" in a pinch and react too soon, cutting in and side-swiping cars when they would actually have had time to clear.

(3) Some of the poorer subjects showed extreme cutting-in and side-swiping most obviously when pushed by speed instructions, into critical situations.

(4) Successful subjects tend to run rapidly and give extremely wide clearance, seeming to indicate reaction to all factors

It must be recognized that these are subjective reports of observations by the experimenter, but they may furnish a rough clue as to factors involved.

Summary of Commercial Study

1. Thirty-nine commercial drivers were tested on the apparatus, and the records of eight eliminated due to lack of an adequate criterion (accident records) or due to known poor vision.

2. The thirty-one remaining records were

differentiation on this variable.

Figure 16 shows that although there is some tendency for the groups to approach the same level, in Cut-ins per Safe pass, this tendency is not nearly so great as that shown in the preceding figure. In general the non-accident group does not improve very much beyond the level at which it starts, and although the accident group does improve there is still a rather wide difference at the end of the third trial. This fact suggests that the variable we are measuring is to some extent an habitual reaction rather than skill acquired in the particular experimental situation.

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It must be recognized that these are subjective reports of observations by the experimenter, but they may furnish a rough clue as to factors involved.

Summary of Commercial Study

1. Thirty-nine commercial drivers were tested on the apparatus, and the records of eight eliminated due to lack of an adequate criterion (accident records) or due to known poor vision.

2. The thirty-one remaining records were

treated statistically for group differences, using Fisher's "t" formula. This was translated into gross score terms, for convenience.

3. The criterion used in dividing the groups was as follows:

the accident-prone group consisted of all drivers with accident records showing greater accident frequency than one in five years. This was taken on the basis of Lauer's study of accident prone and normal groups of drivers. (20) Where accident records were not available, but drivers were rated as poor by companies sending them in, they were included in the accident prone group. The remaining drivers constitute the accident group.

4. A probability of over ninety- nine chances out of a hundred of a true difference was obtained between the two groups for Cut-ins per safe pass, and for three scores of which Best Trial in safe passes, and Smash-ups per car passed were components. The Best trial gave a probability of eighty-nine and Smash-ups per car of about eighty-five chances out of a hundred of a true difference.

5. Reliabilities for the components were as follows when figured on the basis of the average inter-correlation between trials, stepped up by the Brown-

Spearman prophecy formula to the length of the test,- Smash-ups per car passes +.47, Cut-ins per safe pass +.80.

6. The low reliability of the first mentioned score may be partially explained by the fact that the two groups tend to reach the same level by the end of the third trial. This fact, together with reports of the subjects indicate that the complexity of the task might be profitably increased.

7. Caution in manipulation of the apparatus as measured by the speed used by the subject, does not differentiate the groups.

8. It is suggested that the Best trial represents skillful reaction to the situation.

9. It is suggested that Cut-ins-per-safe-pass, represents a measure of habitual reactions to certain factors such as the mechanical relationships involved and timing of movements. It appears to be the best single measure for diagnostic purposes, but should not be used except in conjunction with Smash-ups per car and Best trial.

Chapter VIII

RESULTS WITH STUDENT SUBJECTS

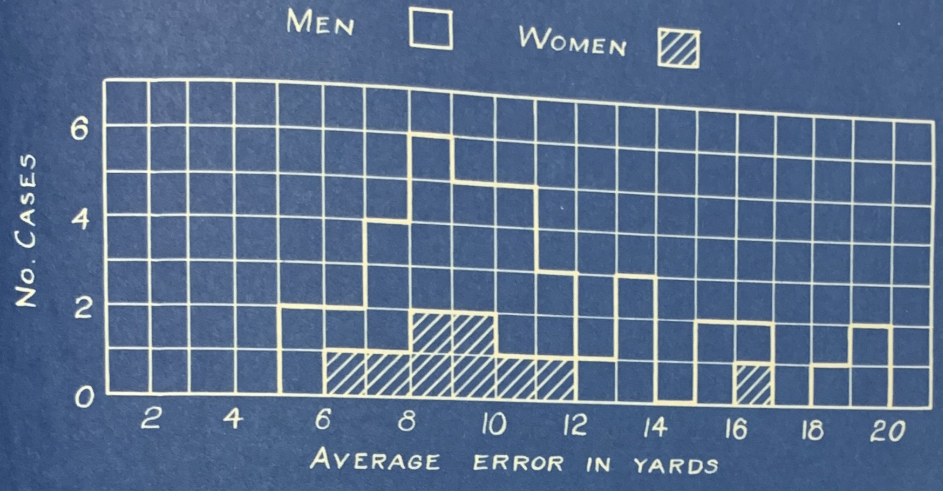
Road Test and Miniature

A total of fifty graduate and undergraduate students of Ohio State University were used as subjects in the final experiment. Of these, thirty-eight were men and twelve were women. Four of the men and three of the women had had no driving experience.

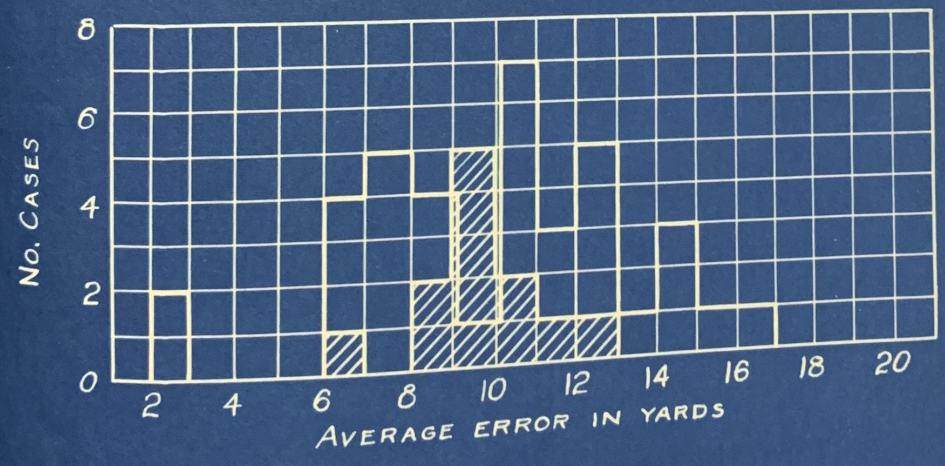
Road Test - Distribution

Figure 17 shows the distribution of scores on the road test for both men and women for trials one and two, in yards of average error. The cross hatched portions indicate score of women subjects, and are superimposed on the histograms of the men's scores. The second trial was given on a different day in all but two cases. It will be noted that the range is from five to eighteen yards and from two to seventeen yards average error for men and women, respectively. The individual errors involved range from zero to sixty yards.

Since it seemed reasonable that the error might be a function of the distance involved (the



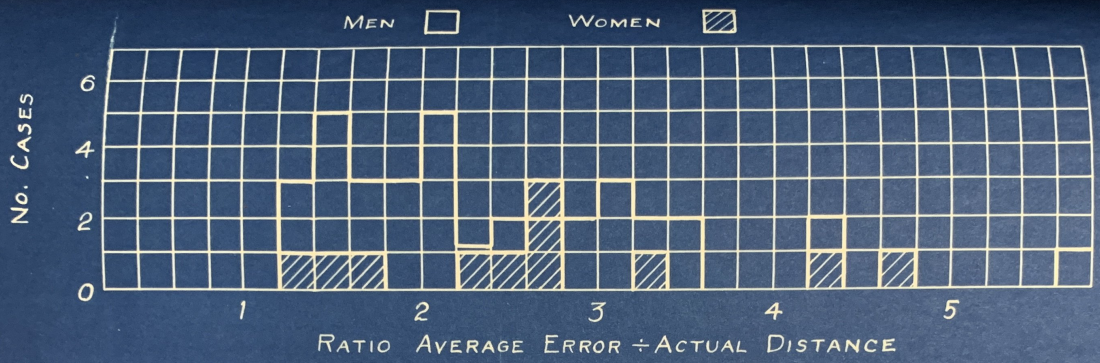
TRIAL ONE



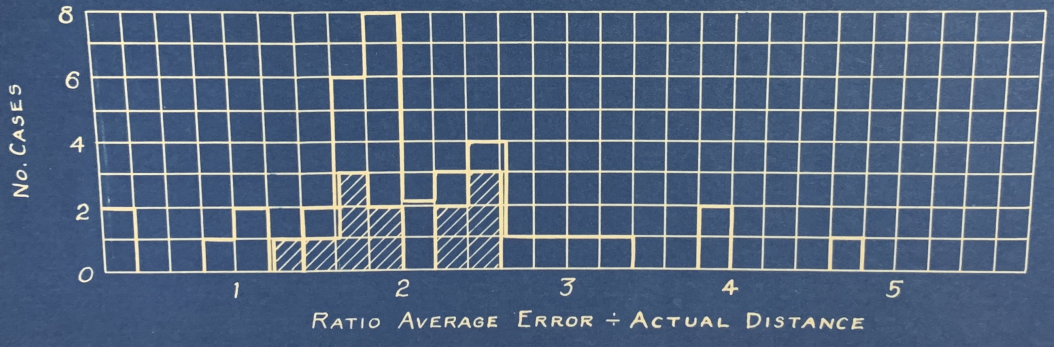
TRIAL TWO

AVERAGE ERROR ROAD

FIGURE 17



TRIAL ONE



TRIAL TWO

AVERAGE ERROR/ACTUAL ROAD DISTANCE

FIGURE 18

greater the distance between cars at the time of estimate the greater the difficulty and therefore the larger the error), an attempt was made to correct for this. Since the speed of the subjects' car was constant, the greater the distance between the cars at the time of estimate, the greater the number on the sign at the actual point of meeting. The errors were therefore divided by the sign number in order to see how this affected the reliability of the scores, etc.

Figure 18 shows the distribution of scores for both men and women when the average error is divided by the actual point of passing, i. e. corrected for distance from the subject at the time of judgment. In both Figure 17 and 18 trial one shows larger errors on the whole than trial two, indicating the possibility of a slight amount of learning. The second trial in both cases, is closest to a normal distribution.

Reliabilities

Both the internal odd-even reliabilities and the reliabilities between the first and second trials are presented in Table XXVII. The lower portion of the table shows the reliability between trial one and the retest (trial two) to be almost negligible. It

TABLE XXVII

Reliabilities-Road Test

Student Subjects

Trial	Odd Even Reliability by Brown-Spearman Formula r for Av.Error	r for Av.Error/Dist.
One	.59	.37
Two	.49	.35
Actual one plus two	.57	.44
Theoretical for one plus two	-.70	-.52
Ratio actual increase to theoretical increase	4/17	8/16
Reliability between trials		
First test with retest	-.14 .08	.06

is even lower for the average error divided by the distance to the point of passing, than for the absolute error. However, both are so small that this difference may be accidental.

The internal consistency of both test and retest, on the other hand, is much greater for both the absolute and the relative average error. This would point to conditions which vary between days. The internal reliability for the relative average error is the more consistent when considered in connection with the theoretical value to be expected from doubling the length of the test. The table shows that the ratio of actual increase to theoretical increase is greater for the relative error. It was therefore felt that the relative score would probably be the better one to use in correlating with scores on the miniature highway in the laboratory.

Analysis of Factors Affecting Reliabilities

The attempt was made to investigate factors which might contribute to the rather moderate reliability. The accuracy of the experimenter's recording has already been mentioned. Seat position, experience in driving, learning, "set", and artificial situation, and small number of trials seem to be the most likely factors and

TABLE XXVIII

Effect of Seat Position

Position Occupied by Subject	N	Diff. (Av. Error)	In favor of	No. cases reverse of average
Same seat	16	.073	Retest	6
Front first - Rear second	12	.128	First trial	2
Rear first - Front second	5	.330	Retest	2

Significance of Diff. of Means

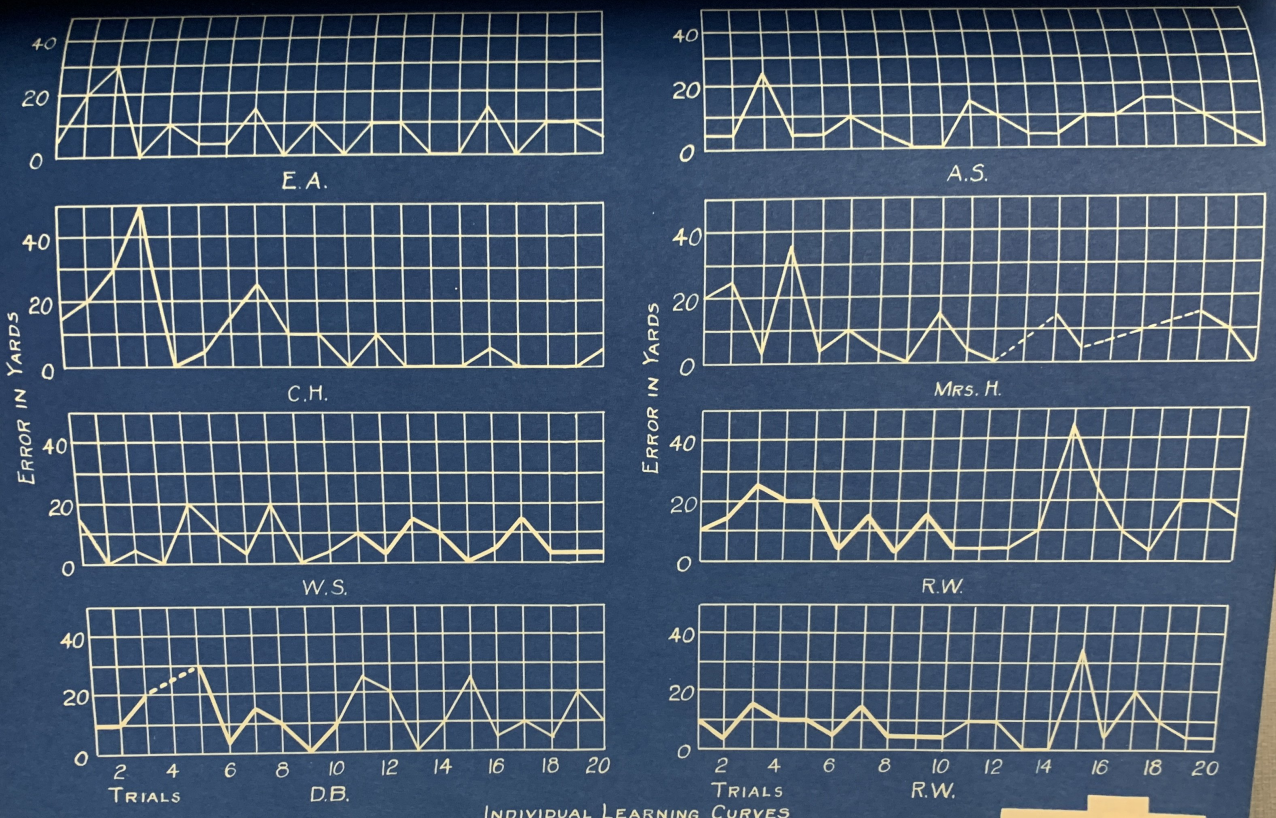
	Diff.	$\sqrt{N_1+N_2-2}$	$\frac{\sqrt{N_1N_2}}{N_1+N_2}$	$\sqrt{N_1\sigma_1^2+N_2\sigma_2^2}$	"t"	Chances of Real Diff.
Same Seat - Front first Rear second	.201		13.3522	1.8363	1.462	80

will be discussed below.

Correlation - Driving Experience with Front Seat Gain

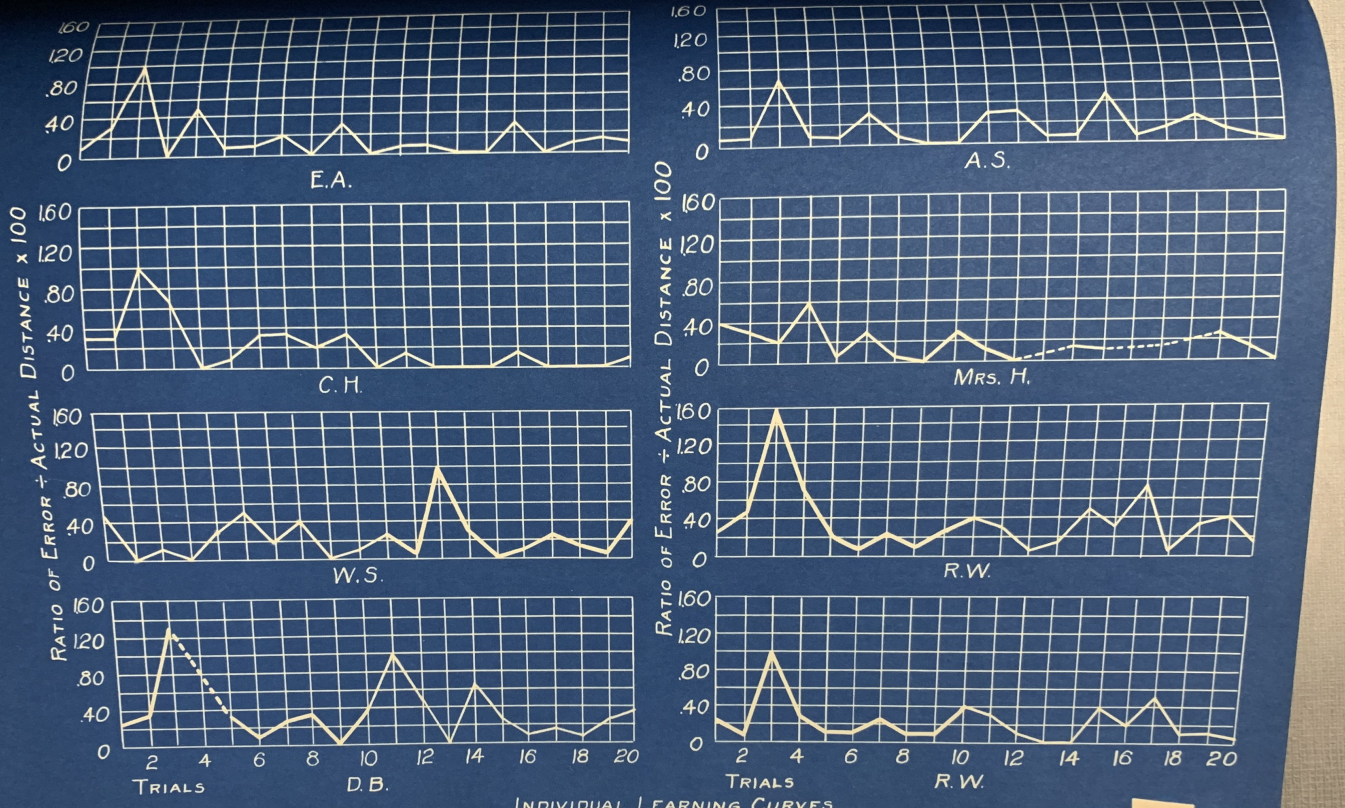
Table XXVIII shows that when the subject occupies the same seat for the retest, the retest score improves on the average to the extent of.073 yards over the first score. However, for the twelve subjects who occupied the front seat first and the rear seat for the retest, the effect is an average of .128 yards in the reverse direction. That is, the change to the rear seat increases their average error. The fact that those who occupied the rear seat first and the front seat on the retest show an average reduction of error which is larger than the reduction when the same seat was occupied, seems to confirm the interpretation that the judgment is slightly more difficult from the rear seat. The low "t" value and the number of cases showing an inversion, indicate that the effect is not very pronounced and that seat position is not entirely responsible for the low reliability.

This explanation is furthered by the fact that the correlation between driving experience, as measured by estimated years driven times number of thousand miles covered per year, correlates insignificantly



INDIVIDUAL LEARNING CURVES
ERROR ROAD

FIGURE 19



INDIVIDUAL LEARNING CURVES
ERROR/ACTUAL ROAD

FIGURE 20

(+.140 ± .165) with gain from front seat position. This would probably be slightly higher if those occupying the rear seat first and the front seat second, were treated separately. However, since the number of cases is so small, it would still probably be non-significant.

Learning may enter to some extent as suggested by the comparison of the distributions from the first test and the retest in Figures 17 and 18. Figures 19 and 20 showing a sample group of individual learning curves, in which abcissae represent trials and ordinates scores in term of average error and relative average error respectively, indicate a tendency toward lower errors in trials eleven to twenty.

Another interesting feature appears in Figure 20 representing the corrected average error. The curves indicated in heavy lines represent the records taken during one particular run. Since the peak at the third trial occurred in all of these, the original records were scanned for the reason. It appears that this was a trial in which the cars met very close to the beginning of the series of signs (at a point designated as $1\frac{1}{2}$). The use of this small number as a division, therefore emphasizes the error,

perhaps unduly. In Figure 19 the cause for absolute error for the same subjects do not show the rise to the same extent. For this reason validity co-efficients have been computed with the second trial absolute average error (see below) as well as for the relative error for trial one plus two as indicated in the discussion of reliabilities.

Evidence of an anticipatory "set" came from remarks of the subjects, such as these, - "He fooled me that time. I thought he was going to go fast," or "He went slow the last time so I thought he would speed up this time," etc. The question of the degree to which this factor influences such judgments cannot be treated here for lack of data, but might be worth investigating.

The fact that the situation is somewhat artificial in that the judgment is made in entirely different terms than that made in driving, might enter and also, of course, the small number of trials would theoretically militate against reliability. However, the latter difficulty is also present in the internal reliabilities.

It therefore appears that seat position, learning and different anticipatory "set"are some of

factors responsible for the fact that the retest reliability is so much lower than the odd-even one.

Miniature Reliabilities

At the same time that the road test of judgment of speed was being conducted on the fifty student subjects, the same subjects were run on the miniature highway. Retests were given on the miniature to twenty-four of the men and eight of the women. Lack of time prevented further retests. Retest reliabilities were then obtained for the scores on the miniature which had previously yielded significant differences in the Dayton study with commercial drivers, to investigate the relationship of retest reliabilities to the previously obtained odd-even reliabilities. These are shown in Table XXIX.

The table shows that the retest reliabilities using twenty-four men and eight women subjects(students), are rather consistently low. The figures for the men are probably much more reliable than the figures for the women due to the greater number of cases involved. The women's reliability for the Best Score (+.683 +.13) is therefore of somewhat questionable worth. It has been shown that tendency for Smash-ups per car is, in the case of the Dayton commercial drivers, to approach a level

TABLE XXIX

Miniature Highway

Students

Reliabilities (Retest)

	Men(N=24)	Women(N=8)
Best	+.202 ±.14	+.683 ±.13
Smash-ups per Car	+.244 ±.13	+.057 ±.20
Cut-ins per Safe Pass	+.128 ±.14	+.063 ±.20
	+.565 ±.10 (when two cases omitted)	
Best -Cut-ins per safe pass	+.051 ±.14	+.364 ±.20

for all subjects. This same tendency appears in the scattergrams for the reliabilities of both Smash-ups per car and Best trial, in the case of the students' retests. This again emphasizes the need of complicating the task to a greater extent than at present.

The low reliability of +.128 ± .14 for cut-ins per safe pass for the men students is, however, somewhat unexpected in comparison with the odd-even reliability of +.802 obtained with the commercial subjects (See Table XV). Examination of the scattergram showed that two cases deviated very widely from the other twenty-two. The reliability was therefore refigured, omitting these two and gave the much higher figure of +.565 ± .10. This seems to compare much more more favorably with the previously mentioned odd-even correlation figure for the commercial scores. It is probable that with a larger number of retest cases, the reliability for the total group would approach the +.565 figure.

Since the last score given in the reliability table (Best Cut-ins per safe pass) includes one score which tends to reach a level, and also the total group on the Cut-ins per safe pass, it is natural that it

should yield a low reliability.

We are therefore inclined to conclude that the low reliabilities of the first two variables are occasioned by lack of individual differentiation on the retest due to a task which is relatively simple. For the third variable, on the other hand, a reliability of +.565 is probably more representative than the negligible one of +.128 caused by two widely aberrant cases. Since the fourth variable is a compound of the first and third, the same factors are operative here.

Validity

The scores of the students on the same variables previously shown to be differential, were correlated with their scores on the road test to see whether or not the differential variables show any relationship to the criterion of judgment of speed, i. e. the road test. Since the reliability of the relative error score on the road increased more consistently with the lengthened test than the absolute error did, we have used the relative error as one of the criteria as shown in Table XX. Further, since there was a possibility that learning entered in the first trial, and that the relative error was distorted where small actual distances were used as the divisor, the absolute error for the

second trial, was used as a second criterion.

Table XXX shows that the first variable (Best Trial) correlates to the extent of .46 and .31 with the two measures of judgment of speed on the road, in the case of the men subjects, while the second variable (Smashes per car) correlates more highly (+.55 and +.42) with the same criteria. These may be interpreted as showing a significant though not high relationship between scores on the first and second variable and ability in estimating velocity as shown in the road test.

The third variable, however, (Cut-ins per safe pass) shows no significant relationship to judgment of velocity as indicated by the road test for the men's scores. We have already pointed out that this variable appeared to be a different measure than the first or the second in connection with the commercial subjects.(See Chapter VII) Here,then, is further evidence of the same thing.

Table XXX shows some interesting relationships in regard to the validity coefficients of the women. The same low relationship between Cut-ins per safe pass and the criterion scores appear. However, where the men show a significant though small relationship between Best trial and the criterion, the women show none.

TABLE XXX

Validity

Miniature Scores Correlated with Road Scores

A. with 20 Trials, Av. Error/Actual.

	r Men(N=38)	Women(N=12)
*Best	+.461±.09	+.001±.20
Smash-ups per car	+.546±.08	-.754±.08
Cut-ins per Safe pass	-.049±.11	-.119±.19
B. With second 10 trials, Av. Error		
*Best	-.306±.10	-.089±.20
Smash-ups per car	+.420±.10	-.728±.10
Cut-ins per Safe Pass	+.109±.12	-.293±.18

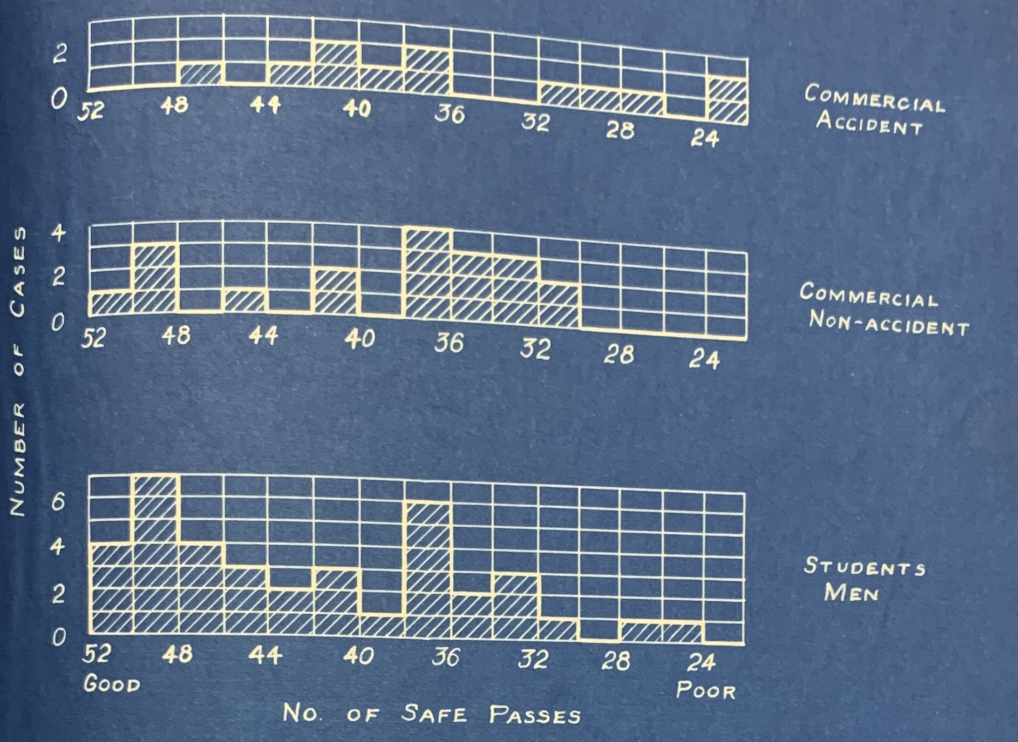
*For this variable, a negative correlation indicates a positive relation between the trials.

Further, in Smash-ups per car the men tend to make a better record if they are better on the road, where as the women show a marked relationship of just the opposite sort. However, since the coefficients for the men are consistent, and since those for the women are based on only twelve cases, it is unsafe to conclude that there is more than a mere suggestion of sex differences here. The figures are, nevertheless, intriguing in the light of previous findings of sex differences in relation to driving.

If we make the assumption that the reactions of the student subjects are similar to those of the commercial group, we may proceed to draw conclusions as to the part played in accident prnneness by individual differences in ability to judge speed. Such an assumption should be based on evidence of the equality of groups in the type of reactions measured. In terms of intelligence, our measures are not comparable since we have scores of the O. S. U. intelligence test for the student subjects, and scores on the Detroit intelligence for the commercial. However, we give the following ranges for what they are worth.

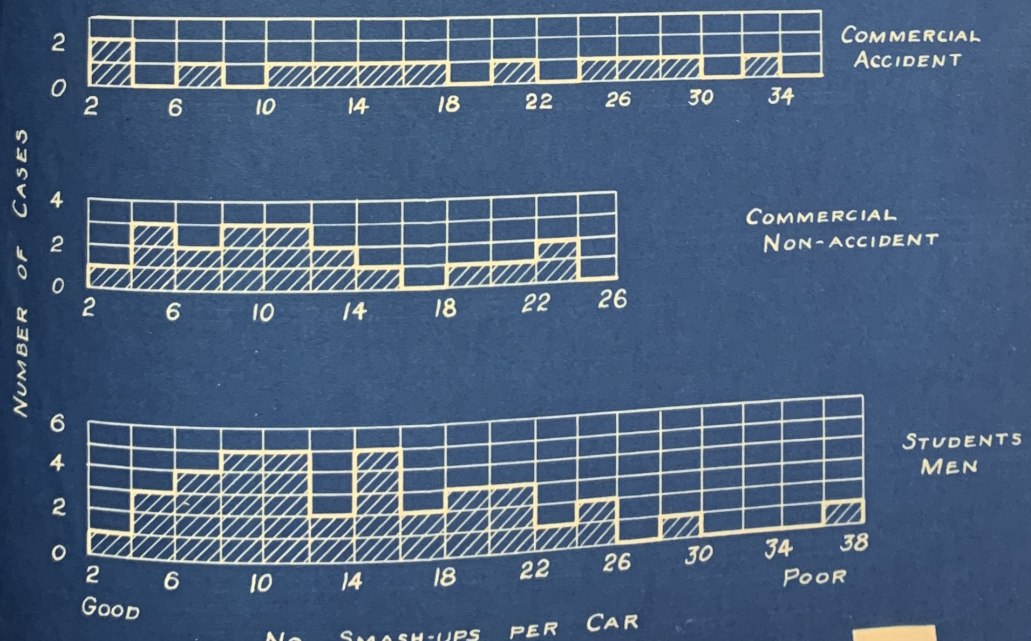
Commercial subjects Lowest 10 yrs. Highest (Detroit)
Students " 5 %ile " 100%ile.

We may at least say that we have a rather extreme range



VARIABLE 14
(BEST TRIAL)

FIGURE 21



VARIABLE 31
(SMASH-UPS PER CAR)

FIGURE 22

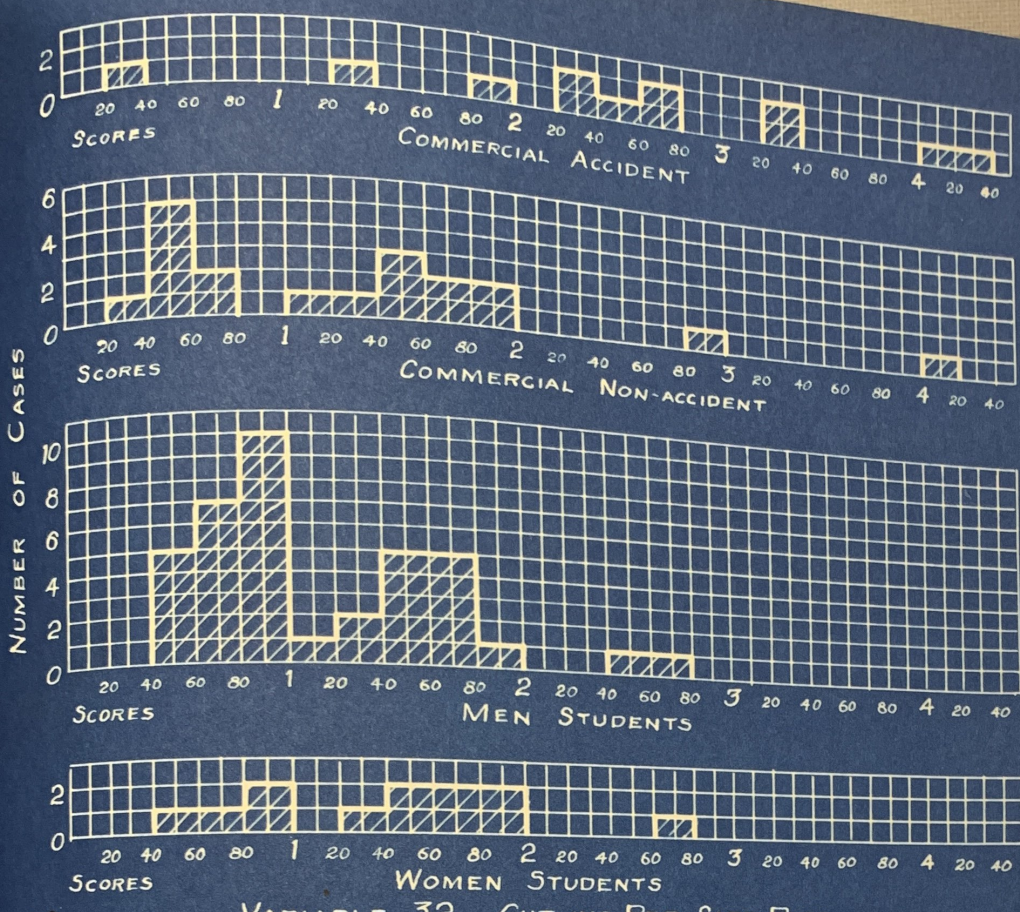


FIGURE 23

VARIABLE 32 CUT-INS PER SAFE PASS

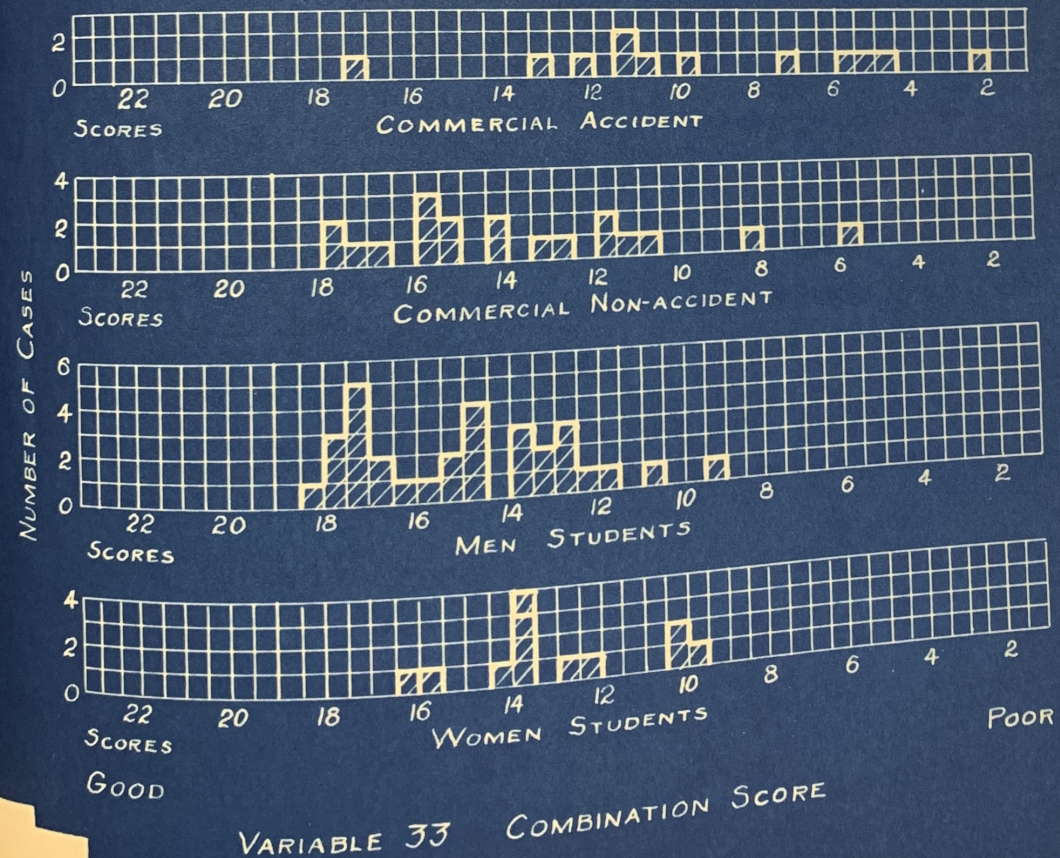


FIGURE 24

VARIABLE 33 COMBINATION SCORE

in both cases.

In driving experience in terms of number of years, the ranges are as follows:-*

Commercial	1 to 22 years
Students	0 to 17 years

We do not have an estimated yearly mileage on the commercial group so are unable to compare them on this basis. However, in terms of years driven, the groups are similar.

In age, of course, the commercial group ranks higher in the majority of cases, although several of the commercial subjects were of college age or slightly older.

In comparison of test scores themselves, it will be seen from Figures 21 and 22 that the student scores tend to fall into a distribution very similar to the commercial distribution with a slight ten dency to skew toward better performance. Figure 21 shows scores for the best trial while figure 22 shows them for smashes per car. Also Cut-in per safe pass and the combination variable #33 (Figure 23 and 24) show again a somewhat similar distribution of the student and commercial scores, with a skewing toward the end of better performance on the part of the students. However, the student scores do run through and below the suggested critical

TABLE XXXI

Extent to which Group Differences

Depend on Estimation of Velocity.

Score		From Table X "t" Value	Chances of Real Diff.	From Table XXX Correlation with Est. of Velocity (Men) First plus second trial Av. Error/ Actual	Ave. Error Second trial
#32	Cut-ins per Safe pass	3.25	over 99	-.049 ± .11	±.109±.12
#14	Best trial	1.65	89	*-.461 ± .09	*-.306±.10
#31	Smash-ups per Car passed	1.53	85	±.546 ±..08	±.420±.10

*In this variable a negative coefficient show a positive relationship between abilities in the traits

scores based on the analysis of the commercial scores. It is very possible that a larger sampling of commercial drivers would have yielded somewhat the same skewing, since we know from Bingham's study with a metropolitan railway(3) that approximately one-third of the men have some two-thirds of the accidents. Our commercial group is therefore probably composed of slightly fewer good drivers than a larger, unselected sample would be. We, therefore, believe that conclusions may be drawn from the foregoing validity coefficients from the students' records, with rather a strong presumption that they would apply to an unselected commercial group. The fact that the original group differences were obtained on the group containing five too few good drivers to fill the one-third against two-thirds proportion does not affect the differentiation since the number of cases enters into the group differentiation technique in the computation of averages and standard deviations.

Conclusions

Table XXI shows the differentiation obtained on the three most differential scores as previously shown in Table X, together with the validity correlations of the same scores with two measures from the road test, as previously shown in Table XX. We see that the last two variables (variables 14 and 31) show a

significant relationship with estimation of velocity on the road by both road test criteria. Variable 31 (Smash-ups per car passed) correlates slightly higher in both cases, than variable 14 (Best trial). Variable 32 (Cut-ins per safe pass) however, correlates practically to no extent with the criterion scores. Since the former variables show a probability of from eighty-five to eighty-nine chances in a hundred of a real difference between groups, and a significant relationship to the criterion, we may conclude that estimation of velocity is one factor in which the accident an non-accident groups differ. However, since variable 32 which yields the highest differentiation, correlates practically zero with the criterion scores, we may conclude that estimation of velocity is not as important a factor in differentiating the groups as another factor or combination of factors.

What the effect of a more reliable road criterion would be is problematic. If lengthening the test can be relied upon to increase the reliability of the scores, according to the correction for attenuation formula

the criterion would correlate much more highly with the Best trial and with Smash-ups per car, but would not increase so

much for Cut-ins per safe pass, due to the much higher initial odd-even reliability of this variable. Such a result would tend to substantiate our previous conclusion.

Our conclusion is obtained from a sample, which though of fairly respectable size considering the conditions under which the results were obtained, is nevertheless rather small. We hope to be able to check the results on a larger sample, possibly introducing greater complication in the judgment as has been suggested.

Summary

1. Odd-even reliabilities of judgments on the road range from +.35 to +.59. The higher individual reliabilities occur with scores consisting of the absolute average error, but scores consisting of average error relative to distance at time of judgment, show a more consistent increase with lengthening of the test.

2. Retest reliabilities on the road are less than the probable error and therefore insignificant. Factors most probably responsible are seat position and different anticipatory "sets".

3. Miniature highway retest reliabilities are also low. The tendency to reach a level found with commercial subjects (see Chapter VII) is also found here for Best trial and Smash-ups per car.

3. This probably accounts for the very great

reduction found in the retest reliability here as against the odd-even reliability in the previous chapter. The low retest reliability for Cut-ins per car, however, appears chiefly due to the results of two subjects and the small number of cases. With more retests, this would probably approach .5 or higher, which is consistent with the .8 odd-even reliability found in the previous chapter.

4. The tendency to reach a level together with remarks of subjects to the effect that they learned to react to absolute position, rather than to relative speeds, indicate that the task should be made more complex.

5. The two variables showing probabilities of group differences of 85 and 89 respectively, correlate for the men with the criterion scores to the extent of ±.31 to ±.55 with probable errors of from ±.08 ±.10. The variable showing the highest differentiation, however, (ninety-nine chances or better) correlates practically zero with the criterion scores.

6. There is a suggestion of sex differences in that the criterion correlations of the women subjects, show much lower relationships than the men's, except in one case where the relationship is high (-.754) and

in the reverse direction. However, since the number of cases is only twelve, it is not safe to draw conclusions here.

7. From the results summarized under (5), it is concluded that estimation of velocity is one factor responsible for accidents, but not as important as another factor or combination of factors.

8. It is suggested that these results should be checked on a larger group, preferably of commercial subjects, and with the introduction of greater complexity in the task.

Chapter IX

SUMMARY

The Problem

The number of people killed per 100,000 automobiles decreased annually until 1926, but subsequently has been rising regularly. Studies of road and mechanical conditions show that eighty to ninety percent of automobile accidents occur under apparently good conditions of weather, road and car. Such considerations raise the general problem of determining the factors that cause faulty reactions by drivers, of picking out drivers who need training, and of eliminating any who can never become competent.

The specific phase of the problem treated in the present study pertains to visual estimation of velocity on the part of the driver. It involves the question of whether there are individual differences in ability to discriminate velocity, and if so, whether such differences are a factor in automotive accidents.

Historical - Theory

Berkeley's New Theory of Vision forms the background for theories on perception of motion, and in so far as change of distance is involved in per-

ception of motion, factors operating in distance discrimination will be included. Stratton's work on the perception of motion indicates that there is no reason to suppose that perception of motion is an unique process, different from that of temporal and spatial succession. We are therefore, interested in the work of Helmholtz, Wundt, Arrer, Hillebrand, Baird and Howard showing the factors of retinal size and accomodation with possibly convergence as effective in the monocular discrimination, and the factors of convergence and retinal size as most effective in the binocular discrimination of depth. In addition, there are the "secondary" factors such as aerial perspective, intervening objects, shadows, etc. In connection with apparent motion, the work of De Silva and of Dimmick and Sanders shows that attitudinal factors strongly influence the perception. In connection with actual motion, Thelin finds that "predispositions" in respect to direction favored sensitivity to effect of fixating one stimulus or the other, etc. affect the perception. In most situations, of course, any such predispositions are probably overcome by other factors. It is possible, however, that in situations where other cues are barely operative, such attitudinal factors or predispositions may influence judgments of actual velocity. Evidence from photography of eye-

movements by Dodge, shows that muscular sensations from pursuit movements of the eyes are not predominant in perceptions of motion, but that retinal displacement is more important.

Tests

Tests for operators of vehicles were initiated by Munsterberg about 1910, and during the war tests were developed for drivers and for airplane pilots. Among the latter is one reported by Stratton, McComas, et al. which includes a test of judgment of speed. This test appeared promising in comparison to other tests, although none showed any great relationship to flying ability ratings. Viteles and Snow have both developed drivers' tests which include a test of judgment of speed. In both cases differentiation of accident groups is reported in terms of the test battery entire, and in the latter case validity of the tests is based on a general reduction of accidents. Kunze in Germany and Mira in Spain also test judgment of speed but we are without data as to the actual relation to driving ability. In nnne of these tests, is the attempt made to preserve factors as nearly as possible like those of the original driving situation.

Preliminary Experiment

The preliminary experiment, used a revised

model of the apparatus originally designed by Troland for use with aviation cadets during the war. The apparatus consists essentially of a vertical pointer which moves in a lateral direction in front of the subject. It is visible for 29 cm. before disappearing behind a screen. At a point 69 cm. from the point of disappearance, a marker is placed on the screen. The subject is required to press a key when he judges the marker to have reached this point. Procedure is without knowledge. Scores are in terms of absolute rather than algebraic average of the errors in centimeters, and ranged from an average of 2.5 cm. to one of 34.0 cm. The latter is a distance half as large as that traversed by the pointer behind the screen (69 cm.). No statistically significant differences were obtained between accident and non-accident groups composed of fifty-one students and eight commercial drivers. Whether this was due to a poor criterion (accidents of a type in which judgment of velocity would not enter) or whether the type of judgment measured is not a factor in accident causation was undetermined.

Series of fifty consecutive judgments obtained from another group os subjects showed marked individual tendencies, ranging from those who did well at first and became steadily poorer and pooer,

through those whose performance remained about stationary, to those who did poorly at first and improved steadily. Eye-strain, general fatigue, and auto-suggestion are probably responsible for some of these individual differences. Further research along this line is suggested.

The large individual differences found in the preliminary experiment, and the discovery that subjects often tended to use a counting method to circumventor "beat" the test, suggested the designing of apparatus involving a more complicated task, with visual conditions as similar as possible to those of the actual driving situation.

Main Experiment - Miniature Highway Apparatus

The apparatus designed in the attempt to present visual factors as similar as possible to those operative on the road was in the form of a miniature highway, all dimensions being reduced as nearly as possible to a scale of three feet to one inch. Three velocities are involved, that of cars overtaken, that of cars approaching, andthat of the car being driven. This effect is accomplished by mounting a six-inch canvass belt on rollers on a rigid frame 15 feet long and moving the belt under the eye-piece which represents the subject's car. The velocities of cars

overtaken and cars approaching are obtained by mounting miniature cars on endless steel tapes, geared in such a way that their velocity respective to the road always remains the same. Actual passing and collisions are obtained and scoring is done automatically by electric counters. Scores consist of Cars passed (speed), Smash-ups, Cut-ins ahead of car overtaken, and Cut-ins ahead of approaching car.

Road Test

In order to determine if possible, whether the miniature was actually measuring judgment of speed, a road test was set up in which the subjects were required to estimate in advance, the point at which two moving cars would meet. The subjects were in one of these cars. A strip of road three-tenths of a mile long running from Neil Avenue to the Ohio State farms was used. Signs numbered from one to ten were placed at ten yard intervals in the middle of this stretch, on either side of the road. Piano wire writing points in flexible housings were connected to a master bar in such a way that the experimenter could withdraw the writing points at will and thereby insure that the judgments were made before a standard point was reached. Scores were in terms of average error.

Miniature Results
(Commercial Drivers)

The miniature apparatus was transported to Dayton, Ohio, during the summer of 1930 and commercial subjects tested there under the auspices of the Dayton Safety Council. The Dayton Power and Light Company co-operated in transporting the apparatus and in supplying subjects. Other companies likewise provided some subjects.

Records were obtained from a total of thirty-nine drivers, of whom eight had to be discarded due to known eye defects or lack of an adequate criterion. The remainder were divided into "accident-prone" and "non-accident" groups on the basis of Lauer's findings on frequency of accidents. Drivers with a record of one or more accidents per 4.5 years of driving were included in the accident prone group and the others in the non-accident group. Cards were punched for Hollerith correlation and scores were analyzed according to the two groups just mentioned. Group differences in terms of Fisher's "t" function were computed in an attempt to correct for the small number of cases. Intercorrelations were also computed, and on the basis of these and the "t" values combination scores were derived.

Of the simple scores, the Cut-ins ahead of

overtaken car gave the best differentiation with a "t" value of 2.3 and a probability of a true difference of about 97 chances in a hundred. The best trial in terms of safe passes yielded the next best differentiation with a "t" value of 1.65 and probability of a true difference of .87. Combination scores yielded higher values. Cut-ins per safe pass gave over 99 chances in a hundred of a true difference, as did also combination scores in which cut-ins per safe pass and the best trial, or cut-ins per safe pass and smash-ups per car passed were weighted to the extent of t/sigma.

The odd-even reliability for Cut-ins per safe pass computed by the Brown-Spearman formula was +.802 while that for Smash-ups per car was +.474. The latter seemed partly explainable by the tendency shown for both groups to reach the same level. This tendency suggests that the task could profitably be made more complex.

Group differences of the magnitude of the highest obtained in the present analysis have not been obtained between accident and non-accident groups previously.

Results - Road Test and Miniature

A group of fifty men and women students,

graduate and undergraduate, were given the road-test, a retest on the road, and the miniature. Twenty-four were given a retest on the miniature.

Two scores were used in statistical analysis of the road performances. One was the absolute average error, the other the average error corrected for distance at which the judgment was made. Odd-even reliabilities for the two trials of the fomer were +.59 and +.49 while those for the latter were +.37 and +.35. When, however, the odd-even reliability for the total of twenty trials was computed, although that for the average error was higher(+.57) than that for the corrected average error (+.44), nevertheless the latter increased more nearly as expected from the Brown prophecy formula for a test twice as long. There was a slight evidence of learning in the first test as shown by individual curves, and also of an undue increase of the corrected error score in certain cases where the actual distance at which the judgment was made i. e. the denominator of the fractions was small. In using the road test as a criterion, therefore, the total corrected error for test and retest was used as one score, and the second trial of the absolute average

error as a second, and validity co-efficients were computed for both.

Actual retest reliability co-efficients for both road test scores were very low (+.14 and +.06), with P. E. of about .08. One factor contributing to this low reliability is change of position from front to rear seat or vice versa in successive trials. The difference between average scores for the front vs. rear seat yields a "t" value of 1.46 and a probability of a real difference of slightly over 80. Another contributing factor is Learning, although the curves indicate that it is not a large factor. Anticipatory "set" may be still another factor. Its presence was indicated by remarks of subjects such as, "He fooled me that time. I thought he was going to go fast." This phase of the judgments is well worth separate investigation.

Reliabilities of the miniature computed from the twenty-four retests, were also very low. They were computed for the three most significant scores as shown by the previous commercial study, i. e. Best trial, Smash-ups per car, and Cut-ins per safe pass. The reliabilities for the first two are +.202, ±.14, and +.244 ±.13 for the men and +.683 ±.13, and

+.057±.20 for the women. The only one which is significant is that of +.683 for the women, and here the number of cases is only eight so that this is probably spurious. These low reliabilities correspond to the low odd-even reliability previously found for Smash-ups per car in the commercial study and may be accounted for in large measure by the tendency of both groups to reach the same level. On the actual retest, this factor would have even more effect than in the odd-even reliability for the first test, since the levelling process has been carried even further. The reliability for Cut-ins per safe pass of +.128±.14 for the men, however, does not conform to the odd-even reliability of approximately +.80 found with the commercial subjects. Examination of the scattergram showed that two of the twenty-four cases deviated widely, the rest conforming rather well to the diagonal, and the computation with the two cases eliminated yielded a reliability of +.565±.10 which corresponds much better with the odd-even value above mentioned. It therefore seems probable that with more retests, the reliability for this score would approach the +.57 value.

Validity co-efficients computed for the same

three best scores on the miniature show a fair relationship though not high, between the Best trial and the criterion and Smash-ups per car and the criterion, the co-efficients ranging from +.31 to +.55 (negative co-efficients appearing in previous tables indicate a positive relationship in abilities). Results from both road test scores are consistent, and Smash-ups per car shows the higher relationship consistently. Cut-ins per safe pass, on the other hand shows no relationship at all with the criterion for either criterion score. Since the student group shows rather close similiarity in distribution of scores, range of intelligence, and years of driving experience, to the commercial group, it is felt that the above figures can be used in connection with the group differentiations obtained in the commercial study.

Since the two measures giving the second best differentiation of commercial accident and non-accident groups (85 to 89 chances) show a significant but not very great relationship to judgment of speed on the road (correlations of +.31 to .55 with road scores for student group), estimation of velocity may seem to be involved as one factor in accident causation. However, since Cut-ins per safe pass shows no relationship to the criterion scores, but yields the highest

group difference ("t" of 3.25 or probability of 99 differences in ability to judge velocity as measured by the road test do not seem to be as important as other factors, which we are measuring on the miniature. It is suggested that this last variable measures habitual reactions to a complex mechanical situation, which involve"timing"of movements, and the ability to react to a large group of factors at once.

Due to the difficulties in the sue of commercial subjects, the data comprise a rather small number of cases and the conclusions should be checked, preferably on a large number of commercial drivers. The apparatus should be revised to procure an even more complex judgment on the part of the subject in order to eliminate as far as possible, the tendency of the groups to reach the same level in two of the variables. This would probably yield both greater differentiation and higher reliabilities.

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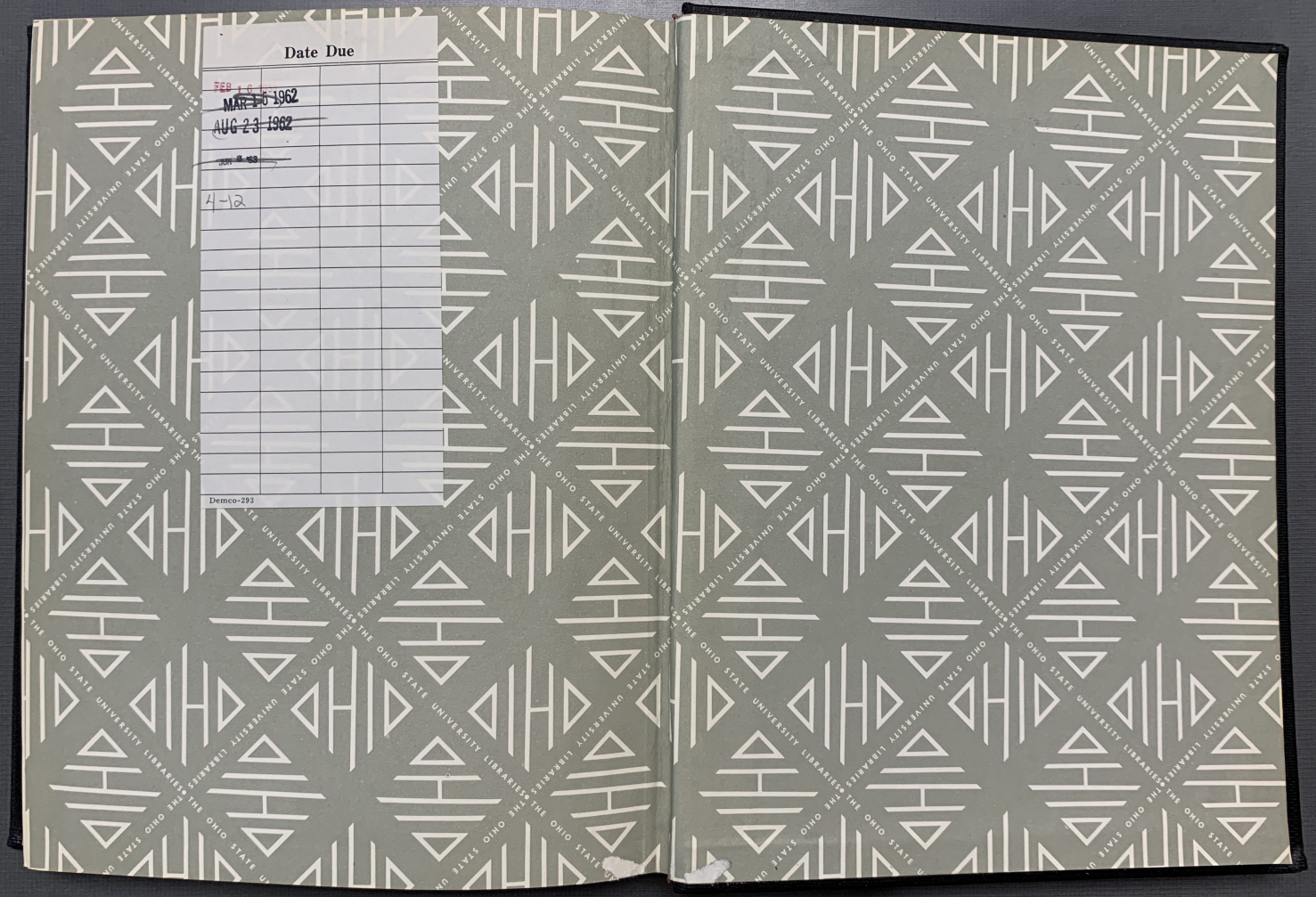
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I, Theodore W. Forbes, was born in Honolulu, T. H., May 21, 1902. I received my elementary and high school education at Punahou Elementary School and Punahou Academy in Honolulu. I received my undergraduate education at Oberlin College, Oberlin, Ohio, from which I received the degree of Bachelor of Arts in 1924. I taught at Oberlin in China as the Oberlin Student Representative from 1924 to 1926, returning to Oberlin the following year when I received from Oberlin College the degree of Master of Arts in 1927. While in residence at The Ohio State University I have held the following positions: graduate assistant 1928-29, and part-time instructor, 1929-30, 1930-31.

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