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Final Report

THE EFFECTS OF CERTAIN PSYCHOLOGICAL VARIABLES UPON TARGET DETECTABILITY

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SUMMARY

Four studies have been made of the effects of psychological variables upon visual detection thresholds for simple circular targets. These studies were intended to evaluate the significance of certain differences which obviously exist between the conditions under which visual detection is studied in the laboratory, and the conditions existing in military visibility problems. The effect of these differences is in each case expressed in terms of the contrast factor, that is the factor by which target contrast must be multiplied in order to compensate for the presence of the difference.

It was found that when a target appears without previous warning, a contrast factor of 1.40 is required to compensate for the resulting loss in visibility. Absence of warning that a target is to appear, and absence of prior information concerning the target's size and duration of appearance require a contrast factor of 1.49. The absence of knowledge concerning the precise location to be occupied by the target requires a contrast factor of at least 1.31 even though the observers know precisely when to expect the target. This variable requires further study, since the target in these experiments could always be expected in one of two possible locations.

Analyses of a number of previously reported studies suggest that what is called a "commonsense criterion" of seeing results in a contrast factor of 2.40 in comparison with the laboratory method of temporal forced-choice.

Finally, it was shown that reducing the frequency of target occurrence from one presentation on the average every 30 seconds to one presentation on the average every 15 minutes could be compensated for by a contrast factor of 1.19.

Further studies should be conducted before extensive practical use is made of these factors, but they should serve to indicate the magnitude of the effects upon target detectability produced by psychological differences between laboratory and field conditions such as these.

I. GENERAL INTRODUCTION

A number of experiments have been conducted in these laboratories since 1946, designed to provide quantitative data on the visual detection capability of the eye for use in predicting the visibility of military targets. These experiments have been conducted with experimental procedures especially developed in order to maximize reliability and efficiency of experimentation in the laboratory. These procedures include rigid control of the physical characteristics of targets, and the use of psychophysical methods which have been shown to be comparatively free from extraneous psychological variables which might otherwise produce variability, and uncertainty in the use of the data. These methods place the laboratory observer under some unusual restraints, and require that he indicate his ability to detect the presence of targets under study within a rather unnatural framework.

It is of considerable interest to ascertain to what extent the data obtained in the laboratory under these conditions can be directly applied to practical visibility problems in the field, or to what extent conversion factors are required to compensate for differences which exist between the conditions encountered in the laboratory and those prevailing in the field. The present report summarizes four experiments designed to evaluate the effect of differences which obviously exist between the laboratory and usual field conditions.

Two of the experiments are concerned with possible effects due to the fact that in the field the observer seldom has complete information in advance as to the time or place of occurrence or the size or duration of the target, whereas this information is normally provided in laboratory tests. One experiment is concerned with possible effects due to the fact that targets seldom occur frequently in the field, whereas in the laboratory targets are usually presented in rapid succession, in order to make efficient use of the observers. The fourth experiment is concerned with the effects of differences in the criteria used to signify detection in the laboratory and in the field.

For some years now, we have been describing the effects of differences such as are here considered, in terms of contrast factors. That is, we express the effect of a given variable in terms of the increase or decrease in target contrast required to compensate for its effects. This policy not only makes it easy to evaluate the relative importance of various effects, but permits a simple allowance for these effects to be made in the computational techniques used to predict visibility distances for practical military targets.

Thus, the intent of the present report is to evaluate the contrast factors required to compensate for various psychological variables which obviously differ between the laboratory and the field.

Since the four experiments were conducted with different apparatus and procedures, it has seemed appropriate to describe these experiments in separate sections of the present report.

II. EXPERIMENT I

The Effect of Knowledge of Time of Occurrence and of Target Size and Duration Upon Target Detectability*

A. Introduction

This was the first of two experiments in which effects due to restricting the information available to the observer about the target were studied. In the usual laboratory situation, the subject is given nearly complete information concerning where and when to expect the target, and the size, shape and duration of the target presentation. Since almost none of this information is normally available to the military observer, it is important to assess the effect of the amount of information available to the observer in order to compensate for any effects found in applying laboratory data to the solution of practical visibility problems.

Experiments were conducted to investigate the general order of effect due to restrictions in various kinds of target information. Since the experiments were exploratory, they were conducted with quite crude apparatus and procedures. The data were collected, however, under conditions intended to facilitate adequate comparisons between conditions differing with respect to the information variable.

B. Apparatus and Procedures

The apparatus used for these studies consisted primarily of a translucent plastic screen which was illuminated diffusely from in front to provide a background of uniform high luminance, and which was illuminated from behind by a projector to produce a comparatively small target. The diffuse illumination was provided by constructing a white cube in front of the screen, with a large opening in the side opposite the screen through which the observers viewed the screen. Frosted lamps were mounted in the cube in positions which were invisible to the observers. The luminance of the screen was maintained at values ranging from 40. to 160. foot-lamberts in different experiments.

The projection apparatus was built around a Bausch and Lomb Balopticon projector. A wheel was inserted in a plane near the condensor lenses so that metal apertures mounted in the wheel could be imaged by the projection lens on the rear of the translucent screen with uniform luminance. A timer wheel was placed near the plane of the projection lens so that the targets would come on and fade out uniformly as the timer was used to present the targets. A flag shutter was inserted in the beam near the plane of the projection lens which could be manually operated to permit presentation of a target whenever the opening in the timer wheel came past the projection lens. Thus, the temporal characteristics of the target presentation were controlled by the characteristics

*This experiment was conducted by Russell L. De Valois, O. Thomas Law, and Wilson P. Tanner, Jr.

of apertures in the time wheel, rather than by the mode of operation of the hand shutter.

Targets of varying sizes could be presented on successive trials, since apertures of different sizes were mounted in the target wheel. There was room for eight different apertures. Targets could be presented for different durations, since several different apertures could be mounted around the rim of the timer wheel. There was room for only three different timing apertures. The wheel bearing the timing apertures could be rotated with respect to the drive shaft of the timer into one of four positions. This rotation was accomplished by holding the wheel while the shaft turned with respect to it. Once rotated, the wheel bearing the apertures was carried along by the drive shaft firmly and continuously. By manipulating the position of the wheel with respect to the shaft, the experimenter could select which of the three exposure durations would be in place at the moment he wished to withdraw the flag shutter and expose the target.

In addition to all these wheels, there was a wheel containing neutral density filters of varying transmittance which were used to vary the target contrast within the threshold range. The experimenter rotated this wheel to the desired filter between successive exposures of the target.

This multi-wheeled apparatus was manually operated by a team of two or three experimenters in accordance with the requirements of a pre-established presentation schedule. There was no way to record the settings of the apparatus actually made but it is believed that the few errors which must have been made had only a trivial effect upon the data from the experiments.

The observers utilized the "yes" - "no" method throughout these experiments, recording their "yes" responses only. A "yes" response was signaled by the observer by pressing a doorbell button. The button activated a circuit which in turn activated one pen of an Esterline-Angus continuous paper recorder. Another pen was used to record periodic time markers. Since the experimenters worked with a rigid time schedule, it was possible to evaluate whether the recorded positive responses on the paper record did or did not correspond to the presentation of a target. The paper records were manually scored and the correct responses and "false alarms" were tallied for each class of target being presented during a given experimental session.

There were basically two experiments. In the first, five conditions were studied representing different amounts of restriction in the information made available to the observers. Description of these experiments can best be given in connection with the presentation of the results in Section III below. In the second experiment, one grand comparison was made between two extremely different amounts of information restriction. Here again, the procedure may best be described in connection with presentation of the results.

C. Results

Experiment I

The background luminance was maintained at 160 foot-lamberts for this entire experiment.

Condition 1

In the first condition, target presentations were made following by 2 seconds a warning buzzer which served to alert the observers and to define nearly exactly when the target was to be presented. A circular target subtending 99 minutes was presented for a fixed known duration of 1.5 seconds once every 28 seconds. The probabilities of "yes" were corrected for "guesses" in accordance with the relation:

$$p' = \frac{p-V}{1-V} \qquad (1)$$

where p' = corrected probability;
p = raw probability; and
V = probability of false alarms

The observers were practiced under these conditions for eleven experimental sessions of two hours each before they were considered trained. Two experimental sessions were conducted before the other four experimental conditions were studied, and two more sessions were conducted after all other conditions had been completed. Threshold contrast values were estimated by visual fits of ogive curves to the experimental probabilities. Contrast thresholds are presented below for three observers, representing averages of the values obtained in the four experimental sessions at the beginning and end of the experiment.

Observer	Threshold Contrast
S	.0171
N	.0120
B	.0091
Average	.0127

Condition 2

Nine sessions were conducted in which the observers were given no warning as to when a target was to be presented. The intervals between presentations were of varying lengths, with an average interval of 28 seconds so that the frequency of target presentations was equivalent to that used in condition 1. In each session, only one target size and one target duration were used and the observers were shown the target in advance at suprathreshold contrast, so that they knew exactly what kind of target was to appear. Three sizes of target were studied as follows: 73, 99, and 121 minutes. Three durations were studied as follows: 1.1, 1.5, and 1.9 seconds. One experimental sessions was devoted to each size for each exposure duration.

(It should be noted that both the size and duration ranges used were not large, due to the requirement that the thresholds were to be essentially equivalent for subsequent experimental conditions to be possible. However, the phenomenal appearances of the nine targets used were considered markedly different by the observers.)

In analyzing the data from this condition, and the other conditions in which no warning was given the observers, the raw probabilities of "yes" responses were corrected for false alarms by means of equation (1). However, we must consider the method used to define V. In the usual experiments in which the observer is warned that a target is about to appear, he is expected to respond within a few seconds and false alarms are clearly defined by those responses coming immediately after the warning when no target was presented. When there is no warning, the observers may respond at any time, and it is somewhat ambiguous what is considered a false alarm and how a probability is computed. We decided to divide the entire experimental session into 2 second time intervals. Any "yes" response within 2 seconds of a target presentation was considered correct; any other response was considered a false alarm. Each 2 second interval was considered to be an opportunity for a false alarm and the percentage of false alarms was computed accordingly. Actually there were very few false alarms so that the corrections for V were seldom of any significance.

At this point, we will consider only the data obtained in the one session with the 99 minute target presented for 1.5 seconds. These data taken in comparison with the data from condition 1 give us a measure of the extent to which warning as to when a target will appear influences the contrast threshold. Thresholds are presented for each of the three observers below.

Observer	Threshold Contrast
S	.0197
N	.0203
B	.0127
Average	.0178

These data suggest that a contrast factor of 1.40 is required to compensate for the loss in target detectability introduced when observers are not notified in advance that a target is about to be presented.

Condition 3

Three sessions were conducted in which the observers were given no warning as to when to expect a target and in which the average of the random intervals between targets was 28 seconds. The 99 minute target was used throughout, but the duration was varied randomly from trial to trial among the three possible values. The three sessions were identical; three were used so that there would be as much data for each of the unknown durations as had been collected for each target condition under condition 1. After all three sessions had been completed, the data were tallied separately for each duration used. The threshold contrast values for the presentations involving the 1.5 second exposure duration are presented below.

Observer	Threshold Contrast
S	.0276
N	.0203
B	.0131
Average	.0203

These contrast thresholds may be compared with those obtained under condition 1 to assess the effect of the absence of warning that a target is to be presented and the absence of knowledge of its exposure duration. A contrast factor of 1.60 compensates for the restriction of information involved in this experimental comparison.

Condition 4

Three sessions were again conducted in which the observers were given no warning as to when to expect a target and in which the average of the random intervals between targets was 28 seconds. The three target sizes were presented in random order with a fixed and known target exposure duration of 1.5 seconds. All targets were used in all three sessions. After completion of the sessions, the data were tallied separately for the different target sizes. The threshold contrast values for the presentations involving the 99 minute target are presented below.

Observer	Threshold Contrast
S	.0258
N	.0190
B	.0122
Average	.0190

These contrast thresholds may be compared with those obtained under condition 1 to assess the effect of the absence of warning that a target is to be presented and the absence of knowledge of its size. A contrast factor of 1.50 compensates for the restriction of information involved in this experimental comparison.

Condition 5

Nine sessions were conducted in which the observers were given no warning as to when to expect a target and in which the average of the random intervals between targets was 28 seconds. All three target sizes (73, 99, and 121 minutes) and all three exposure durations were used with a completely random order of both variables. A total of nine sessions were required to insure that the data for each target size and duration would equal in number the corresponding data from condition 1. After completion of all nine sessions, the data were tallied separately for each target. Threshold contrasts are presented below, for the 99 minute target presented for a 1.5 second duration.

Observer	Threshold Contrast
S	.0227
N	.0182
B	.0143
Average	.0184

As before, these thresholds may be compared with those obtained under condition 1 to assess the effect of the absence of warning that a target is to be presented and the absence of knowledge of both its size and duration. A contrast factor of 1.45 compensates for the restriction of information involved in this experimental comparison. It is curious that this factor is actually smaller than the factor for removal of knowledge of size alone (1.50) or duration alone (1.60), although perhaps the difference is not statistically significant.

These data suggest that the absence of warning that a target is to be presented requires a contrast factor of 1.40 and that the additional removal of knowledge concerning target size, or duration, or both size and duration increases the factor to an average value of 1.52 (average of 1.50, 1.60, and 1.45). Thus, the additional contrast factor needed to compensate for removal of knowledge of size or duration or both, when no warning has been provided, is 1.09.

Thus far, we have utilized only a small part of our data, having restricted the analysis to cases in which the target subtended 99 minutes and the duration was 1.5 seconds so that direct comparisons could be made with the data obtained in condition 1. We may, however, make additional comparisons among our data by comparing data obtained in conditions 2, 3, and 4. In all these conditions, there was no warning as to when the target was to be presented so that we are analyzing possible effects which may exist in addition to the effect of warning.

First, we have considerable data on three target sizes and three durations under conditions 2 and 5. (Heretofore we have analyzed only the data for the 99 minute target and the 1.5 second duration.) Combining the data for all nine targets in each case, we obtain the threshold contrasts presented below.

Observer	Threshold Contrast Condition 2	Threshold Contrast Condition 5
S	.0239	.0274
N	.0198	.0203
B	.0188	.0135
Average	.0188	.0204

A comparison between these data evaluates the effect of removing knowledge of both size and duration, with the absence of warning in both cases. A contrast factor of 1.09 compensates for the restriction of information of this sort. (This factor agrees exactly with the contrast factor obtained by averaging instances in which size alone, duration alone, or both size and duration were unknown for the 99 minute target presented at a 1.5 second duration. This agreement is impressive since very few data are common to the two analyzes. It is suggested that, indeed, it makes no difference whether knowledge of size alone, duration alone, or size and duration is removed).

There are two additional comparisons that can be made. We may compare all the data obtained in condition 3 with similar data from condition 5, to evaluate again the effect of removing knowledge of target size when duration is already unknown. Threshold contrast values are presented below.

Observer	Threshold Contrast Condition 3	Threshold Contrast Condition 5
S	.0297	.0251
N	.0209	.0192
B	.0147	.0133
Average	.0218	.0192

These data show the apparently paradoxical result again that the threshold is lower when knowledge of size is removed than when it is present, provided knowledge of duration has been removed.

Finally, we may compare all the data obtained in condition 4 with similar data from condition 5, to evaluate again the effect of removing knowledge of target duration when size is already unknown. Threshold contrast values are presented below.

Observer	Threshold Contrast Condition 4	Threshold Contrast Condition 5
S	.0311	.0255
N	.0209	.0194
B	.0141	.0139
Average	.0220	.0196

These data show the same paradoxical result that the threshold is lower when knowledge of duration is removed than when it is present, once knowledge of size has been removed. Since this result has occurred three times in three analyses, it may be a real finding. It is not at all clear what mechanism can be adduced to explain this result.

Perhaps the most general conclusion which can be reached is that the removal of knowledge of target size or duration or both has little additional effect, when warning as to when the target will be presented is not provided. If we ignore conflicting secondary trends, we may average together the primary data from conditions 2, 3, 4, and 5 and compare the threshold contrasts under these conditions in which warning was removed with the data obtained in condition 1. We conclude that a contrast factor of 1.49 (average of 1.40, 1.50, 1.60 and 1.45) compensates for the absence of warning and for removal of knowledge concerning target size, target duration, and both target size and duration.

Experiment II

In this experiment, the background luminance was maintained at 40. foot-lamberts. This experiment was intended to extend the range of values of target size and duration which the observer had to expect, in order to evaluate to what extent the conclusions of Experiment I were dependent upon the narrow range of values of these variables which the observer had to expect.

In this experiment, the following target sizes were used: 2.0, 5.6, 15.7, 44.2, and 92. minutes of arc. The exposure durations used were as follows: 0.3, 0.6, 0.8, and 1.6 seconds. Basically, two experimental conditions were studied, in neither of which the observer was given warning as to when the target would appear.

Condition 1

Separate threshold determinations were made for two observers under each of the twenty target conditions during which the observers were informed of the target size and duration. The intervals between successive target presentations were random, with an average length of 28 seconds, as in Experiment I. Continuous curves of size and threshold contrast were plotted for each exposure duration. These curves were used to equalize the visibility of the various targets used in condition 2.

Condition 2

A single experimental session was conducted in which a tremendous variety of target sizes and durations were presented randomly. In all, twenty different target sizes were used at each of the four exposure durations. Each target was presented with an appropriate group of neutral density filters to render it equal in visibility to each other target, based upon the smooth curves developed from the data of condition 1. Under these conditions, all the data from all targets may be treated as one group of data, and the session as a whole may be compared with the earlier sessions to evaluate the effect of removal of virtually all information concerning target size and duration. The threshold conrast in the session with targets of unknown size and duration differed by only 3% from the corresponding values when target size and duration were known. This experiment therefore confirms the results of Experiment I in demonstrating that, once warning concerning the time of occurrence of the target is removed, there is no important additional loss in detection due to removing knowledge of target size and duration.

These two experiments taken together suggest that a contrast factor of 1.49 is suitable to allow for the deleterious effects due to the absence of warning when a target is to appear, and the elimination of knowledge concerning the target's size and duration.

III. EXPERIMENT II

The Effect of Knowledge of Target Location Upon Target Detectability*

A. Introduction

It is almost invariably the case in laboratory studies of visual thresholds that the observer is given complete information as to the location in which the targets will be presented. Obviously, many practical military situations present the observer with targets in unknown locations. The present experiment was designed to assess the effect upon target detectability of restricting the observer's information as to the location it will occupy.

There have been several studies of visual detection thresholds in which the observers were not given complete advance information as to the location in which the target would appear. For example, some of the writer's Tiffany experiments (Ref. 1) involved the presentation of a target in one of eight known locations on a circular orbit around a fixation mark. The observers were instructed to scan among the eight locations within a 6 second period during which the target was exposed. The observers moved their eyes around the target orbit in an unknown manner, picking up the targets at unknown times during their scan. Essentially the same procedure was used by Lamar, Hecht, Hendley, and Shlaer (Ref. 2). In these experiments, precise information about target location was lacking and the observers were allowed to search and scan for the targets.

Since the observers were allowed to move their eyes in the scanning process, it is uncertain to what extent the data reveal the effect of lack of information as to the location of the target. That is, it is not meaningful to compare the detection thresholds obtained in these experiments with thresholds obtained when observers had complete knowledge of the location occupied by the target as an index of the effect of lack of knowledge of target location. In addition to the informational variable, the thresholds for detecting the target in the search case are affected by the fact that the observer often fails to look precisely at one of the target locations during the scanning process. It is also apparent that the effective exposure duration is reduced in the scanning process and this will also affect the data obtained in the search case.

It is essential that we identify the elements of the problem involved in searching for targets which will occur in unknown locations. There is first the effect of information concerning target location

*This experiment was conducted by Wilson P. Tanner, Jr. and Margaret Markert. It was jointly sponsored by Project MICHIGAN, operating under Contract DA 36-039-SC-52654 between the U.S. Signal Corps and the University of Michigan.

per se. There are the additional effects of off-axis viewing and of reduced exposure duration. "Totalistic" experiments such as those conducted earlier (Ref. 1, 2) have the serious limitation that they do not separate these problems and that the data from such experiments cannot have any appreciable generality for application to practical visibility problems. During the Tiffany experiments involving search, the writer used to emphasize this lack of generality by noting that the results of the experiment would presumably have been seriously affected by a change in the dimension of the orbit around which the target could appear, or a change in the cues used by the observer to identify each of the eight possible locations for the target. Thus, the long-dead Mr. Tiffany could have influenced the results of these experiments by having used a different design in his bowling alley which was used as the experimental room.

Accordingly, the writer decided that the problems involved in searching for targets in unknown locations should be identified and subjected to separate study. Once these aspects of the complex act of search and scanning were understood, actual practical visibility situations could be described by a suitable combination of the various effects. A separate report in this series (Ref. 3) contains the results of studies of the effect of targets appearing at various distances off the line-of-sight. A second separate report (Ref. 4) contains the results of studies of the effect of reductions in the duration during which a target was visible on the line-of-sight. The present experiment was concerned with the effects of information concerning the location of the target, and this effect alone.

B. Apparatus and Procedures

A complex apparatus was to have been developed which would have permitted us to present targets at virtually any location in a visual field, without the observer's knowledge. Initial stages of the development were completed and the present experiment was conducted even though the apparatus was never brought to completion.

The apparatus consisted essentially of a projection system mounted on a set of horizontal and vertical ways. By suitable drive mechanisms operating on the two ways, the projector could in principle be positioned at any location within a large area. The entire mobile projection system was mounted behind a large translucent screen and was used to project a small increment of light through the screen. This spot appeared as an increment added to the general luminance of the screen, produced from a system of diffuse luminaires located in front of the screen. The translucent screen consisted of a sheet of plate glass, with a thin sheet of translucent milk-plastic adhered to its front surface. The thinness of the plastic sheet insured that the spots projected from behind retained sharp edges. One-half inch plate glass was used to eliminate troublesome inter-reflections within the plastic-glass sandwich.

Problems related to the precision of movement of the projector along the double ways proved difficult, as did the problem of eliminating all sound clues to the location to which the projector was being moved. These problems were not satisfactorily solved. However, it proved possible to use a single way to move the projector back and forth horizontally. Thus, in the present experiments the projector was restricted to one of two possible locations and was made to occupy these in a sequence unknown to the observer.

The temporal forced-choice psychophysical variant of the method of constant stimuli was used. With this procedure, the observer is required to identify in which one of four possible time intervals the target appears. It should be emphasized that in these experiments the observers were always warned precisely when the target presentations would be made. Successful target detection can be assumed to the extent to which the observers can correctly identify the time interval, allowance having been made for chance successes. The allowance for chance success is made from the relation

$$p' = \frac{p-.25}{1-.25} \qquad (2)$$

in which p' = corrected probability; and
p = raw probability.

All measurements were made with a single target intensity, to simplify equipment requirements. Thus, we did not obtain a frequency-of-seeing curve as is customary in our use of this psychophysical procedure.

The basic procedure was to conduct three related threshold measurements as follows: First, the target was presented in a known location off the line-of-sight, for example 8° to the right of fixation. The subject carefully maintained fixation and did not "cheat" by looking toward the target in the known off-axis location. A run of 50 presentations was made and a value of p' obtained. Then, the target was presented in a second known location off the line-of-sight, for example 8° to the left of fixation and a second value of p' was obtained. Finally, a series was conducted in which the target could be presented in either one of the two locations previously studied. The mobile optical system was positioned in first one and then the other of the two positions, in accordance with the requirements of a random program. The observer was informed that the target would appear in one or the other position, and that the probability of its appearance was equal in the two positions in the long run. The observer carefully maintained fixation midway between the two positions and did not attempt to look at either of the two possible target locations. A run of 50 presentations was made under these conditions, and a value of p' obtained. Under all three conditions, the observers had merely to identify in which of the four possible time intervals the target appeared. In the condition of unknown location, there was no requirement that the observer be aware of which position the target occupied.

Thus, conceivably, the observer could have correctly identified the time interval without ever being aware in which position the target occurred.

After a series of three such measurements, the values of p' obtained with the target in the two known locations were averaged. The value of p' obtained when the target was presented in one or the other position was compared with this value to provide an assessment of the effect of knowledge of target location. The values of p' give us information concerning differences in detection probability due to the presence or absence of knowledge concerning the location of the target. However, it is more useful to determine the target contrast required to compensate for this effect. Values of the contrast factor needed to compensate for this effect were computed from the pairs of values of p', utilizing data on the shape of the frequency-of-seeing curve presented elsewhere by the writer (Ref. 5). These data give a value of the slope of the frequency-of-seeing curve, measured by σ, with respect to the threshold, specified by M. A value of $\sigma/M = .390$ was used for this conversion.

In different series of experiments, different pairs of possible target locations were utilized. In each case, the two locations were separated by equal distances from the line-of-sight, along the horizontal meridian. In different experiments, the separation between the two locations was varied from 0.25 to 8 degrees. Differences in the separation between these locations are considered to represent differences in the knowledge available to the observer about target location.

C. Results

The results of these are presented in Figure 1. The target contrast values are presented in terms of the contrast factor required to compensate for lack of knowledge concerning target location. As indicated in Section B above, these factors were derived from pairs of values of p' by reference to average frequency-of-seeing data. In effect, the contrast factor represents the increase in target contrast required to restore the value of p' obtained without knowledge of target location to the value obtained with such knowledge. Contrast factors are presented as a function of the separation between the two possible locations in which the target could appear.

It is apparent that the contrast factors exceed unity, and in general increase as the separation is increased. (The experimental point for a 2 degree separation between the two possible locations is extremely high for no known reason.) The smooth curve drawn through the rather scattered data has an asymptotic value of 1.31. This means that when the observer must expect a target to appear in either of two possible locations, and when these locations are separated by at least 4 degrees, the target contrast must be increased by a factor of 1.31 to compensate for the reduction in information made available to the observer. It must be emphasized that this factor does not take account of the contrast increase needed to detect off-axis targets. Our procedure compares situations in both of which the targets appeared off-axis to the same extent, but in which the information made available to the observer about target location was the experimental variable.

It is not clear to what extent these data can be used to represent realistic visibility situations in which the target must be expected in many possible locations over a fairly large area of the visual field. It might seem as though two possible positions represent only a very small uncertainty as to target location, in comparison with that encountered in most practical situations. However, it may be that once specific information as to the precise location of a target is removed, further reduction in location information may have very little effect upon target detectability. Obviously, further experiments along these lines should be conducted, in which increasing uncertainty is introduced with respect to the location to be occupied by the target.

These experiments might well be guided by considering the visual system as being "scanned" at higher neural centers by a mechanism with a limited area of "attention". In such a construct, the probability of detection will be the probability that the scanner will intercept the target while it is still exposed. Other theoretical models which should be considered assume that the visual neural system has a decision process in which signals (targets) must be differentiated from noise (no targets). In such a system, certainty with respect to target location can reduce the noise in the system by eliminating from consideration the neural events occurring at other locations in the system.

IV. EXPERIMENT III

The Effect of Observer Criterion Upon Target Detectability

A. Introduction

It has been standard practice in these laboratories for some years now to measure detection thresholds by the temporal forced-choice method of constant stimuli. This method is charaterized by the requirement that the observers indicate their ability to detect the presence of a target by correctly identifying the time interval out of four possible intervals. Extensive studies reported elsewhere (Ref. 6) demonstrate the excellent reliability and validity of this method, which make its choice a happy one for the kind of extended program of research conducted by these laboratories. The forced-choice method requires that the observers select a temporal interval even when they have no confidence that they can detect the presence of a target.

After practice, observers universally become able to identify with a high degree of accuracy targets which were originally "invisible" to them. Studies reported elsewhere (Ref. 6) suggest that observers not only learn to detect targets of very low contrast with the forced-choice method but also learn to estimate how accurate their forced-choices are. The observers apparently come to realize that very dim and indistinct experiences do represent the presence of real targets, provided these experiences have proper characteristics with respect to time and place of occurrence and sharpness of onset and offset. This by no means implies that the observers can utilize what they have learned, in practical visibility situations. Under realistic conditions, targets do not oblige by appearing in known locations at known times and they do not have controlled rates of appearance and disappearance. These considerations suggest that what is learned in the forced-choice laboratory situation may be relatively useless to an observer in a practical situation. It may be that there is some generality in learning to detect dim targets, but this has yet to be established.

The problem, therefore, is to assess the extent to which the criterion used by trained observers utilizing the forced-choice procedure differs from the kind of criterion which will be used by military observers. An evaluation of this difference will permit us to define a contrast factor to compensate for this difference when we apply laboratory detection data to practical visibility situations in the field.

B. Procedures and Apparatus

The data to be reported here were collected under a program sponsored by the Office of Naval Research and have been reported elsewhere (Ref. 6). These data have been analyzed in a new way for the present purpose and hence the conclusion is new even if the data are old. Under these circumstances, it will suffice to provide only a brief description of the apparatus and procedures used to collect the original data.

The first phase of the present analysis involves comparing detection probabilities with the forced-choice method with those obtained with the "yes - no" method, in which the observers indicate by "yes" or "no" whether or not they have detected the presence of the target. For this purpose, data were utilized which were obtained on 4 unusually experienced observers. These observers utilized the forced-choice and the yes - no procedures alternately from day to day in a series of daily experiments extending over more than 10 months. The target and background conditions were maintained constant during this period so that these observers had a staggeringly large amount of experience with this one detection situation. The general luminance was 4.71 foot-lamberts. The target subtended 18.5 minutes of arc, and was presented always 7 degrees from the line-of-sight for about 0.072 seconds.

These observers had every opportunity to develop confidence in their forced-choice responses and to attach the verbal symbol "yes" to the experiences of dim and vague targets which led to correct forced-choice responses. Under these conditions, the difference between the probabilities of detection obtained with the forced-choice and the yes - no methods should be minimal.

The second phase of the present analysis involves an evalua-tion of the extent to which the probabilities of detection with the yes - no procedure improve with practice. For this purpose, we have analyzed data obtained under somewhat different experimental conditions on an entirely different group of observers. A group of 70 observers was used in several series of experiments in which different methods of training were evaluated. In all cases, a point source target was employed in a known location on the line-of-sight. The exposure duration was 1.5 seconds. Background luminance varied in the different experiments from 17.9 to 18.7 foot-lamberts.

Each observer was introduced to the experimental situation with a set of instructions intended to elicit what might be called a "commonsense criterion" of seeing. The observers were told: "We are going to turn on a light from time to time. If you see a light, say 'yes'. If you don't, say 'no'". (We did not inform the observers that we were presenting blank trials to evaluate their criteria.) If the observers asked, "How will I know?", we told them: "Oh, you'll know when you try it." These instructions were intended to keep the observers as naive as possible and to prevent them from developing a laboratory frame of reference.

Subsequently, these observers were divided into groups who were given different instructions and training. Some of these led the observers to improve their detection probabilities a great deal more than others. In order to assess the relation between the commonsense criterion and the usual laboratory criterion, we have compared the results of all the observers in the initial experimental session with results from the observers who utilized the most effective regimes of instructions and training. Large enough groups of equivalent observers are involved so that this comparison is not seriously affected by sampling differences.

The final phase of the present analysis involves combining the results from the two phases. It is assumed that the yes - no criteria adopted by the 4 highly practiced observers were at least as effective as the criteria adopted by the observers from the large group who were found to have experienced the most effective regime of instructions and training. (It is not possible to test this assumption because of the differences in the experimental situations involved in the two experimental phases.) On this basis, the data from the second phase can be combined with the data from the first phase, to reveal the extent to which the commonsense criterion results in fewer detections than the laboratory criterion used in the forced-choice procedure.

C. <u>Results</u>

The data from the two phases are presented in Figure 2. The frequency-of-seeing curve plotted the farthest to the left represents the results obtained by the 4 highly practiced observers with the forced-choice procedure. The relative contrast is set equal to 1 for a detection probability of 0.50. The frequency-of-seeing curve is constructed with $\sigma/M = .390$, which was found to be the average of many detection experiments, as has been reported elsewhere (Ref. 5).

The frequency-of-seeing curve second from the left, which represents the left border of the hatched area, has been constructed with a contrast factor of 1.20. That is, this curve is constructed with $\sigma/M = .390$ as with the first curve, but the contrast at which the detection probability = 0.50 is set at 1.20. The two curves farthest to the left in the figure represent the data obtained by the 4 highly practiced observers with the yes - no method, in comparison with their data obtained with the forced-choice method.

The frequency-of-seeing curve farthest to the right, which represents the right border of the hatched area, has been constructed with a contrast factor of 2.40. That is, this curve is constructed with $\sigma/M = .390$ as with the first curve, but the contrast at which the detection probability = 0.50 is set at 2.40. This curve has been constructed to represent the fact that the commonsense criterion resulted in threshold contrast values for the 70 observers 2.0 times the values obtained by the observers from the group who were given the most effective instructions and training. Thus, it is assumed that the commonsense criterion results in threshold contrast values <u>at least</u> 2.0 x 1.20 = 2.40 times the values obtained by the 4 highly practiced observers, utilizing the forced-choice procedure.

The hatched area is considered to represent a range within which different criteria with the yes - no method may occur, depending upon the extent to which observers have learned to be aware of the most dim and vague cues available to them in the laboratory. It is not entirely apparent what factor should be utilized in converting laboratory data for use in practical visibility problems. Since the field observer rarely has opportunity to use vague cues which occur in the laboratory, the 2.40 factor is probably most reasonable. Use of this factor does not involve an extreme allowance for the effect of observer criterion, since our process of combining the data from the two experimental phases was conservative.

V. EXPERIMENT IV

The Effect of Frequency of Target Occurrence Upon Target Detectability*

A. Introduction

It is customary in laboratory investigations of visual thresholds to present targets frequently, and to warn the observer before each presentation is made. Military targets usually occur without warning and they seldom occur with the frequency of laboratory test targets. The present experiment was designed to assess the effect of the frequency of targets upon their detectability, when the targets were presented without warning.

There has been considerable interest among the English psychologists in the last few years in what they call vigilance (Ref. 7). By vigilance is meant the alertness of the observer, measured by his sensitivity to stimulus presentations. Results of several experiments suggest that vigilance is reduced by reducing the frequency of stimulus presentations. These results have only been obtained in comparatively complex sensory-motor tasks. We have wondered to what extent reducing the frequency of target presentations will influence the threshold of visual detection. Presumably, the observer could easily maintain his vigilance, if he were always warned before a target was to be presented. Therefore, we have studied the influence of the frequency of target presentations upon visual detection in a situation in which the observer was not warned before the target was to be presented.

We have measured the probability of visual detection with each of two frequencies of unwarned target presentations. In each case, the observer was required to detect the presence of a point source target which was added to a screen of moderately high luminance.

B. Apparatus and Procedures

Four simple light boxes were constructed to provide fields of uniform luminance. Wooden cubes were constructed, measuring 24 inches on a side. The observers viewed the back wall of each cube through an open aperture on the front wall. The back wall consisted of a thin milk-plastic screen which was uniformly illuminated by frosted lamps mounted inside the cube, but invisible to the observers. A metal plate was mounted flush with the rear of the plastic screen, with a small hole which could be illuminated by a projection lamp mounted behind the screen. When the aperture was illuminated, a small spot of light was added to the luminance of the screen. The intensity of the luminance increment produced when the spot was exposed was governed

*This experiment was conducted by Celeste M. Crossman by Wratten neutral density filers interposed between the lamp and the screen. The target could be occluded with an opaque shutter, or presented by removal of the shutter.

In the present experiments, the shutter was operated manually. It was always opened for 2 seconds, the timing being controlled by the experimenter with the aid of a stop watch. The luminance of the plastic screens in the four boxes varied from 6.67 to 7.29 foot-Lamberts. Variations in these luminance values from day to day were negligible throughout the experiment.

Each observer sat at a distance of eight feet from the screen. He was required to view the screen continuously and to report whenever a target was added. In order to control accommodation and orientation, four black dots in the form of a diamond were painted onto the center of the plastic screen. The angular subtense of the target and of the black dots was 1 minute of arc. Each of the black dots was located 10 minutes of arc away from the central location occupied by the target. Each observer indicated that he detected the presence of a target by depressing a doorbell button. The button activated a neon glow lamp located in the anteroom behind the light-box where the experimenter sat.

Four light-boxes were assembled in a row so that four observers could be used simultaneously. It was essential that multiple observers be used, in order that reasonably efficient use could be made of the experimenter's time. Black drapes were hung from ceiling to floor between the light-boxes so that the observers were unable to see each other, or to see more than their own light-box. The experimental room in which the apparatus was located was provided with an air-conditioning unit. This unit was operated at all times. The steady noise it made was more than adequate to mask any sounds made by the observers or the experimenter.

The basic experimental plan was to compare the probability of detection of unwarned target presentations under two conditions, designed to represent extremes with respect to the frequency of target presentations. "Frequent presentation" was defined by one target presentation every thirty seconds, on the average. "Infrequent presentation" was defined by one or two presentations every twenty minutes, on the average.

The observers were first given several experimental sessions in which the presentations were frequent. They were then given a number of sessions in which the presentations were infrequent. The first condition was then repeated in order to provide an approximate control for temporal effects, such as learning.

The observers were obtained from among University students desiring part-time employment, and were paid for the time devoted to the experiment.

It was expected that the observers would indicate that they had detected the presence of a target when a target was not presented an appreciable percentage of the time. The number of such spurious responses was so small throughout the entire experiment, however, that they may be safely ignored in the analysis of the data.

Twelve observers were scheduled to be used in three groups of four each. These observers observed for five experimental sessions of two hours each under conditions of frequent presentation. Each session was divided into five sub-sessions of twenty minutes each, with a rest period of five minutes following each sub-session. In each sub-session, forty presentations were made without warning, at randomly selected times. The targets were presented simultaneously to all four observers since any other arrangement would have been impossible for the experimenter to manage. Equal numbers of each of five target intensities were presented in random order in each experimental session.

The observers were then instructed as follows: "From now on, you will see fewer lights. Remain as attentive as possible and try to see as many as possible." They were each given a total of sixteen experimental sessions under conditions of infrequent presentation. Each sub-session was designed to have either zero, one, two, or four detections by the observer. Successive sub-sessions varied in the number of detections which it was desired that the observers make, within these limits. The number of sub-sessions in which each number of detections was to occur was the same, and the schedule was arranged so that a sub-session with zero detections followed a sub-session with one detection as often as it followed a sub-session with two detections, and so on. In these experimental sessions, the experimenter employed a different schedule for each of the four observers working at the same time, never presenting targets to two observers at the same time.

In a given sub-session, the experimenter kept presenting targets until each observer had obtained his quota of detections for that sub-session. This arrangement was impossible to adhere to precisely, but only minor exceptions were necessary. In order to obtain the desired number of detections, the experimenter found that she often had to repeat the presentation of targets in fairly rapid succession. Two restrictions were made on the way in which the targets could be presented. First, target presentations could never be closer together than 15 seconds. Second, no presentations were permitted during the last 2 minutes of each sub-session.

Five target intensities were presented in random order as before. The intensities were selected on the basis of the results of the earlier experimental sessions, with an allowance for the extent of the reduction in detections which was expected to occur when target presentations were reduced in frequency. The allowances made for the expected effect turned out to be reasonably accurate.

During the last few experimental sessions, two of the observers gave evidence that they had become aware that the experimenter repeated targets which were not detected the first time they were presented. One observer reported that if he thought he saw something, he waited until it was presented again to be sure of his response. The other observer told the experimenter he thought she repeated target presentations every 15 seconds, some of the time. As noted above, this interval was exactly the shortest allowable interval separating successive target presentations. The demands on the experimenter to keep four observers to their own prearranged schedules were so exacting that she was often forced to use this short a separation between successive presentations. The fact that one observer surely was aware of the mode of presentation, and a second probably was aware of the mode of presentation, suggested that the data obtained in these sessions should be discarded. At this point in the study, one of the twelve observers quit for personal reasons and the two observers who had suspected that targets were being presented repeatedly, if missed, were dropped from the experiment.

The remaining observers were next told that the targets would be presented more frequently again, as in the original experiments. Two sessions were given each observer, in which forty target presentations were made in each sub-session. Following these sessions, the observers were told that the targets would be presented less often again. The succeeding fourteen sessions involved infrequent target presentations, in accordance with a revised plan. Instead of specifying that the observer should obtain a certain number of detections in a given sub-session, the number of target presentations to be given in each sub-session was set in advance and rigidly adhered to. The fourteen experimental sessions contained seventy sub-sessions of twenty minutes each. Forty of these sub-sessions contained exactly one target presentation; twenty contained two presentations; and ten contained four presentations. All the presentations were made at one target intensity in order to concentrate the data to the maximum possible extent. The order of sessions involving one, two, and four target presentations was randomized. The times during the sub-sessions at which the target presentations were made was controlled to the extent that an equal number of presentations was made on the average in each eighth of the sub-session.

The target intensity at which all the presentations were made was one of the five intensities which the observer had used during the sessions with frequent presentations conducted just prior to the sessions with infrequent target presentations. When all fourteen sessions had been completed, the observers were again given two experimental sessions in which frequent target presentations were made. The same five target intensities were employed as in the sessions with frequent presentations which preceeded the sessions with infrequent presentations.

C. Results

All the data obtained in the fourteen sessions in which targets were presented infrequently were combined. The data obtained with the target intensity in the sessions involving frequent presentations, conducted just before and just after the sessions with infrequent presentations, were also combined. The resulting proportions, P, and the corresponding values of N, the number of target presentations made, are presented in Table I for each of nine observers.

Values of σ_p were computed by means of Bernouilli's theorem, and the significance of the difference between the pair of proportions obtained by each observer was evaluated by the critical ratio test. Values of P(CR) represent the probability that a difference in either direction as large or larger than that obtained could have occurred by chance.

We see that eight of the nine observers show highly significant differences in P, dependent upon the frequency of target presentations. Six observers show the expected loss in detection probability for infrequent presentations, whereas two observers show a significant gain in detection probability.

The "average effect" may be judged by the average proportions obtained by all observers under the two conditions. There is a comparatively small loss in detection probability, on the average.

It will be of interest to express the average effect of the frequency of target occurrence in terms of the contrast increase required to compensate for a low frequency of occurrence. Data on the form of the psychophysical curve obtained in visual detection experiments presented elsewhere (Ref. 5) may be used to compute a suitable contrast factor. Utilizing a value of $\sigma/M = .390$ as before, we obtain a contrast factor of 1.19. This means that a 19% increase in target contrast will compensate for the effect of reducing the frequency of target occurrence as was done in the present experiment. Since the frequency reduction was quite extreme, use of a 1.19 contrast factor to allow for this factor in practical military visibility problems seems quite safe.

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TABLE I

Detection Probabilities for Frequent and Infrequent Target Presentations

Observer	Frequent presentations		Infrequent presentations		Significance of difference		Direction of effect
	p	N	p	N	Critical ratio	P(CR)	
1	.810	100	.438	114	6.11	<.001	Loss
2	.800	40	.426	115	4.77	<.001	Loss
3	.891	120	.478	115	7.56	<.001	Loss
4	.741	116	.393	117	5.74	<.001	Loss
5	.629	116	.828	105	3.44	<.001	Gain
6	.500	80	.792	120	4.34	<.001	Gain
7	.736	110	.325	120	6.84	<.001	Loss
8	.692	120	.687	115	0.08	.936	None
9	.760	75	.475	120	4.24	<.001	Loss
Averages	.729		.537				

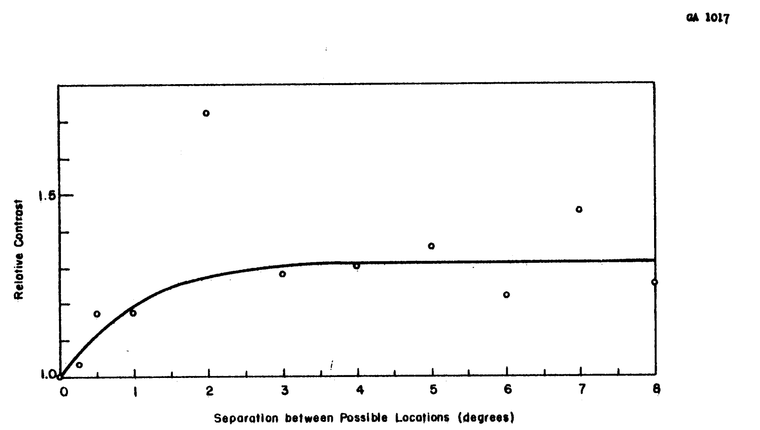

Fig. 1. Effect of absence of knowledge of location.

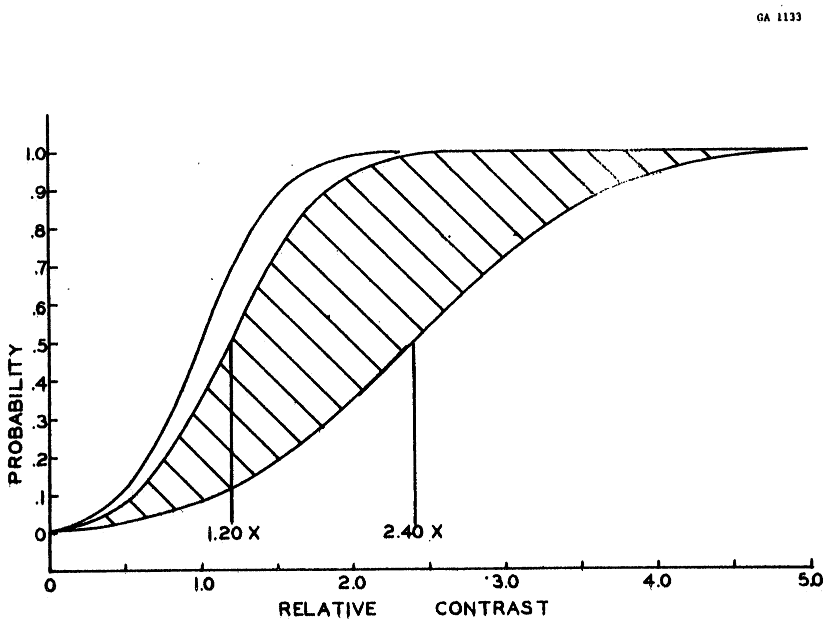

Fig. 2. Effect of observer criterion (see text).