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Toward a Model of Visual Performance: A Review of Methodologies

by M.S. Rea

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ABSTRACT

Literature in the field of illuminating engineering is critically examined in terms of validation and extension of a suprathreshold visual performance model proposed in a companion paper. It is concluded that very few such studies can be used to assess or advance a visual performance model owing to problems with procedure and measurement. Recommendations are made for future studies of visual performance.

RÉSUMÉ

On a étudié d'un oeil critique la documentation ayant trait à l'éclairagisme au point de vue de la validation et de l'extension d'un modèle de performance visuelle supraliminaire proposé dans un document complémentaire. La conclusion en est que très peu d'études de ce genre peuvent servir à évaluer ou à encourager l'usage de la performance visuelle en raison des problèmes de méthodes et des techniques de mesure. On formule des recommandations en matière d'études futures.

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Toward a Model of Visual Performance: A Review of Methodologies

M. S. Rea

Introduction

The lighting practitioner directly influences the speed and accuracy of visual response (*i.e.*, visual performance) by affecting the illumination level and lighting geometry in a space. Yet visual performance is clearly but one aspect of performance at a realistic task; training, intelligence, motivation, and many other factors also play an important role in defining a person's level of task performance.^{1,2} Special procedures and measurement techniques are required to minimize the impact of these "non-visual factors" on measured behaviour when trying to isolate visual performance from task performance. As argued in this paper, many studies that describe performance of realistic tasks have not incorporated such procedures. What is more, two or more visual factors may be confounded in some of these studies. Thus, the measured behaviour has been influenced by some unknown combination of visual parameters. Although these studies give a general flavor of how lighting and other stimulus parameters affect visual performance, it is usually impossible to delineate unambiguous relations between human visual responses and stimulus conditions. If the lighting practitioner is to follow recent, more refined lighting recommendations,³ it will be necessary to have more refined information from research to implement the recommendations.

The present paper is a critical evaluation of literature from the area of illuminating engineering on the performance of human subjects at a variety of visual tasks. It is concluded that most of these experiments have procedural problems or have dealt with more than visual performance alone. Because visual performance cannot be isolated in the experiments, most of them cannot be used to set precise guidelines for relating lighting and visual performance, nor can they be used to validate or extend the visual performance model (**Figure 1**)* presented in the companion paper.

Experiments to investigate performance at several kinds of visual task are examined in the present paper. Each of the subsequent sections is devoted to one task. Common problems with procedures and measurements are identified and discussed in terms of the difficulties they create in specifying the relationship

between lighting and visual performance. Finally, recommendations for future experimentation and measurement are proposed that can lead to precise lighting recommendations and facilitate assessment and advancement of the visual performance model proposed in the companion paper.⁴

Numerical verification task

Performance of the numerical verification task forms the basis of the model. Data using the same task have already been reported.⁵⁻⁹ Briefly, subjects are required to find and then indicate discrepancies between juxtaposed printed number lists "as quickly and accurately as possible" on a given trial. The lists are examined once from top to bottom, and the time to complete the comparisons and note the number of errors of both omission and commission are recorded for each trial. In one set of numbers lists (response lists) all the sheets are printed in high contrast ink on white paper; in the other set of numbers lists (reference lists) the sheets are printed with different types of ink on white paper. The lists are usually examined under different levels of illumination and lighting geometry. Experimental combinations of all these factors produce different task background (adaptation) luminances and contrasts.

Dependent variable

A performance measure, score, based on total time and errors identified during each trial⁵ was used as the dependent variable in all the numerical verification studies prior to this.⁴

$$\text{Score} = (20 - E)/(S + 5) \quad (1)$$

where

E = number of errors per trial (both misses and false positives)

S = total time (s) to compare the lists.

This measure can affect the characterization of visual performance for a variety of reasons described in *Appendix A* of the earlier report by Rea.⁴ There it was noted that score has a slightly nonlinear relation with the primary measures, misses, false positives, and

*Except where noted, all reported contrast values describe stimuli in which the target is darker than the background. Contrast (*C*) is defined as: $C = (L_B - L_T)/L_B$, where L_B is the luminance of the background and L_T is the luminance of the target.

total time. Further, total time does not completely isolate the changes in visual response due to experimental manipulation because it is a composite of the times to 1) mark discrepancies, 2) read the lists of fixed contrast (*i.e.*, response lists), and 3) read lists of different contrasts (*i.e.*, reference lists).

The visual performance model was based upon time to read the reference lists while performing the numerical verification task. It is argued that this refined measure best characterizes visual performance for reading alphanumeric stimuli. Direct comparison of visual performance model predictions and score values from the numerical verification task can be made in **Figure 2**, which presents score data from *Appendix A* of the previous report and model predictions of relative visual performance (RVP) from equation (C7) of *Appendix C*⁴. The dashed lines through the score data are described by equation (A1), *Appendix A*. All RVP predictions were normalized to units of score using one common factor; in this case the factor was equal to the average of the four values of $R_{\max}$, one for each background luminance, determined from equation (A1).

Although the model predictions are similar to the performance score values, it is clear from **Figure 2(a-d)** that the score data are flatter at medium and high contrasts than the model predictions. Further, the model predictions become less like the performance score values as illumination level (background luminance) is reduced. Thus, the characterization of visual performance depends not only on the experimental design and protocol but also on any transformations of the response measure. The dependent measure used in any study must be carefully scrutinized to determine whether it is an appropriate representation of visual performance. Ideally, the performance measure should be based simply on visual response times or errors.

Confounding subject groups with experimental variables

Despite the use of a common task, other factors specific to each of the studies using the numerical verification task (*e.g.*, subjects, viewing angles, size of numbers) can affect the observed levels of performance. Further, many undefined factors such as motivation, manual dexterity, or intelligence can also influence the results. The experimental conditions employed by Rea⁶ are (not surprisingly) almost identical to those reported later.⁴ In the earlier study a different group of young adult subjects with excellent uncorrected vision and positioned in a chin rest performed the numerical verification task at a fixed background luminance of 67 cd m^{-2} . **Figure 3** shows the mean scores from that study, plotted with the highest (169 cd m^{-2}) and lowest (12 cd m^{-2}) curves through the score data from **Figure 2**. The curve through the score data from Rea's earlier study⁶ was

obtained using equation (A1) in *Appendix A*.

Although the two sets of data are similar, the absolute levels of performance score were different in the two experiments. Subjects in the earlier study had higher scores, on average, than did subjects in the more recent report. Thus, even at a task run under essentially identical conditions and using subjects nominally equivalent, the absolute levels of performance for two populations of subjects differed for unknown reasons.

At present, model predictions of visual performance will have to be normalized to experimental data. Each set of data can differ in the absolute level of performance owing to differences in the experimental variables or, as shown, to unspecified differences among sampled subject populations. A different normalization factor will probably be required, therefore, whenever new subject populations are employed.

Applying the model predictions to different experiments having changed subject populations presents no difficulties, but utilizing various subject populations within one experiment can be very problematic. It is possible to compare model predictions with experimental data only roughly if variations in the stimulus parameters are confused with sub-populations of subjects within an experiment. For example, if one sub-population of subjects is given a task under one illumination level and another sub-population is given the same task under another illumination level,¹⁰ it will be impossible to determine whether the observed differences in performance are due to differences in illumination, to differences in the subject groups, or to both factors.

In principle, very large sub-populations should have very small differences in their average performance potential. It is not always practical, however, to employ very large populations in one experiment. Even fairly large sub-populations (*e.g.*, $n = 40$; see section entitled **Tinker's reading experiments**) may be different in visual capabilities, motivation, manual dexterity, etc. As these factors influence measured performance, it is always better to reduce the likelihood of potential problems by ensuring that subject groups (sub-populations) are not confounded with experimental manipulations. Although confounding subject groups with stimulus variables in an experimental design has not been a problem in studies of the numerical verification task, it was a problem in several of the studies to be discussed later in this paper.

Photometric measurements

Clearly, errors in the photometric specification of the stimulus conditions can adversely affect comparisons between experimental data and model predictions. In studies by Rea,^{4,6} Slater *et al.*,⁸ and Slater and Perry,⁹ so-called "calibration squares" were printed

with the numbers on the reference and response sheets so that target luminances as well as background luminances could be measured to determine task contrasts. For a known lighting-task-subject geometry, then, it was possible to specify task contrast. Only in lighting conditions approaching those in a hemisphere do variations in geometry become inconsequential to task contrast. Directional lighting can greatly influence the perceived task contrast and, therefore, the expected level of visual performance.

In a recent study by Rea *et al.*¹¹ it became clear that subjects deliberately try to avoid contrast-reducing veiling reflections in order to maintain a high level of performance. In previous studies by Rea^{4,6} and by Slater and Perry⁹ subjects' head positions were fixed in a chin rest so that the lighting-task-subject geometry and therefore the task contrast were known. Smith and Rea⁵ seated subjects in front of a white viewing booth roughly simulating hemispherical lighting conditions. Head and body positions were not restrained in any way, so that variations in head position did not seriously affect task contrast because the lighting was non-directional.** But although head position could not seriously affect task contrast, head and body movements could affect apparent size of the stimuli. As Rea *et al.*¹¹ also point out, subjects tend to move closer to the task as illumination levels are reduced, increasing the apparent size of the stimuli and thereby permitting better performance. It is therefore impossible to use the Smith-Rea data to isolate completely the effects of illumination level from apparent task size in assessing visual performance. This type of confounding is also present in several of the studies discussed below.

Conclusions

The numerical verification task can be useful in characterizing visual performance, but special precautions with regard to experimental design and execution must be implemented. In particular, task contrast, luminance, and size must be carefully controlled. An appropriate measure of performance, free of distortion, must also be defined before the data can be used to validate and advance a visual performance model.

Tinker's reading experiments

Early reading performance tests were conducted by

****It should be noted that a calibration square was not printed on the task sheets used by Smith and Rea.⁵ The contrast of the task materials had to be estimated by a comparison method. The luminance of the printed digits was estimated by measurements of a larger piece of paper matched in brightness to the printed digits. Although the task contrasts were estimated under hemispherical lighting and did not vary when subjects adjusted their postures, there may have been some discrepancies between the contrasts actually seen by the subjects and the estimated contrasts that were reported in the study.**

Tinker using different task contrasts and various levels of illumination.¹²⁻¹⁵ In his 1959 experiment, for example, several groups of subjects performed speed-reading tests.¹⁵ Of special interest were two groups of university students of unknown age or ophthalmic capability; one group of forty saw printed passages with dark ink on white paper under six illumination levels, and another group of forty saw passages printed with dark ink on grey paper under the same six illumination levels.

Confounding with task contrast

As pointed out by Poulton,¹⁰ changing the reflectance of the task background affects not only task contrast but adaptation luminance as well. Performance is known to vary with both parameters, and when they are confounded, as in this experiment, differences in performance cannot be attributed to one factor alone. Nevertheless, the contrasts of the two printed targets employed by Tinker were both quite high (according to the reported reflectances). The compressive visual response to contrast should produce nearly equivalent levels of performance for high contrasts at equal background luminances. Assuming that all other factors are constant, performance differences should therefore be mainly attributable to changes in background reflectance.

A serious confounding in Tinker's experiment (identified in the previous section) was between subject groups and background reflectances. Differences in the visual capabilities of the sub-populations or in their psychological profiles (*eg.*, intelligence or motivation) could have contributed to the observed differences in reading performance at the different task background reflectances. In fact, there were reliable differences between the two sub-populations at matched task background luminances, indicating that even for relatively large sub-populations ($n = 40$), absolute differences in group performance potential can affect the results.

Photometric measurements

For reasons outlined in the previous section, there is also uncertainty about the accuracy of the reported photometric values that define the visual conditions experienced by the two groups of subjects. First, the reported values were reflectance measurements of paper and ink under a lighting geometry different from the lighting conditions actually experienced by subjects during the experiment. Further, documentation was not provided as to how the reflectance measurements were obtained from the very small letters in the printed text. Unless luminance measurements are obtained from some "calibration square" printed with the text it is unlikely that the reported values accurately represent ink density. Second, although both groups of subjects saw the reading passages under six

levels of illumination ranging from 54 to 4300 lx, it is not clear that lighting geometry and thus task contrast did not change with changes in illumination level. Finally, it is possible that subjects systematically adjusted their posture under the different lighting geometries to improve task contrast and thus maintain a high level of performance. In truth, the reported photometric values may only be crudely representative of those actually seen by the subjects.

Although there is a consistent performance difference across illumination level for the two subject groups (confounded, of course, by the two background reflectances), the levels of performance across luminance are approximately constant. Even the relative performance of the two subject groups is questionable, however. As discussed in the preceding section, reading performance can probably be maintained if a subject systematically moves closer to the task as illumination is reduced. It seems very likely that the subjects in Tinker's experiment were unrestrained and behaved in this manner. As in the Smith-Rea study,⁵ it is difficult to use these data to advance a visual performance model since illumination level and apparent size of text were probably confounded.

Dependent variable

The measure of performance for this experiment was the time to read a passage. At least two problems with the dependent measure make it of questionable utility for characterizing visual performance. First, the levels of comprehension were assumed to be high but were never verified, although Tinker reported that previous tests gave high levels of comprehension near 100 percent. In principle, the single dependent measure of reading speed will not reflect any possible differences in "accuracy" of reading (*i.e.*, comprehension). Without some direct verification of a high correlation between time and errors (*e.g.*, Rea⁶), reading speed may be a distorted representation of visual performance. Second, the very high levels of comprehension imply a very easy test for university students. Depending upon the kind of questions, it is conceivable that university students could answer many of the questions correctly without even reading the passages. In essence, Tinker's comprehension measure may be completely insensitive to visual performance. A performance measure should be realistic, but it must also be sensitive to the parameters manipulated in the experiment.

Conclusions

Although Tinker's experiment may give a general indication of visual performance at a reading task, a variety of serious procedural problems make it impossible to use these data, or those from his earlier studies, to evaluate the present, or any other, model of visual performance quantitatively. Other, more care-

fully controlled studies of reading performance would have to be used to validate and advance the visual performance model. Special care must be taken in this kind of experiment, however. Reading for comprehension may actually require more thinking than seeing,¹⁶ and precautions must therefore be taken to ensure that the recorded responses reflect visual performance alone.

Experiments using visual search

Many investigators have employed search tasks.^{10,17-20} The most widely known was first employed by Weston.^{21,22} In this kind of experiment subjects are asked to search an array of Landolt rings for those having a specific gap orientation. The arrays can be printed in different sizes and inks and on different background reflectances. In an attempt to explore parametrically some of the factors important to visual performance, subjects are presented with the arrays under different levels of illumination. As argued above, the results obtained in such experiments depend not only on the visual factors employed in the experiment but also on factors such as the accuracy of the photometric measurements, experimental design and protocol, subjects' behavior, and scoring procedures.

Weston's early Landolt ring experiments

Documentation Although Weston's Landolt ring search tasks^{21,22} are commonly-cited studies of visual performance, they were not adequately documented. Lighting geometry and task specularity (both target and background) interacted to affect luminance and contrast. Nowhere does Weston document how ink and paper reflectances were measured or under what conditions. Consequently, very little is known about the stimulus conditions actually presented to his subjects. Further, in his 1945 studies²² both paper and ink reflectances were varied to change task contrast, so that it is impossible to attribute the measured effects to changes in contrast, background luminance, or both. Weston did not provide details about the positions assumed by subjects during testing. Presumably they were unrestrained and could adjust their posture and position in performing the task to suit themselves. Again, one must assume that illuminance and apparent size are confounded in the study. The visual capabilities of the subjects are unknown. Neither is it clear what experimental design was employed. One does not know, for example, whether all subjects saw all conditions. Weston's early landmark efforts in investigating visual performance must be lauded, but it would be impossible to have confidence in more than the general trends indicated by these experiments because of inadequate documentation.

Dependent measure The performance scoring procedure is one of the most serious problems with

Weston's experiments. He was the first to attempt to combine speed and accuracy in a performance metric, and many others have followed his example; several are discussed in this paper. Accuracy in Weston's experiment was defined as the number of hits (*i.e.*, targets found and marked) relative to the total number of targets in the array.

A proper measure of accuracy should also consider the number of correct rejections (*i.e.*, non-targets found identified and not marked). Further, without knowing both hits and correct rejections it is impossible to tell how many stimuli were actually inspected during the recorded time (one minute or shorter) and therefore impossible to make a proper estimate of speed (stimuli inspected per unit time). Consequently, his performance score is completely inappropriate as a measure of visual speed and accuracy. More will be said about the importance of determining the number of stimuli actually inspected per unit time in the **Boynton and Boss** section of this paper.

Consistency and reasonableness of results The two sets of performance curves for 3.0-min gap sizes reported by Weston in 1945²² are shown in **Figure 4**. Not only are the two sets inconsistent in their prediction of performance but some of the smoothed functions are counter to known trends of visibility versus luminance. For example, the slopes of some of the performance versus luminance functions for low contrast stimuli are flatter than those for high contrast stimuli. In effect, this indicates that light level is less important to low-contrast material than to high-contrast material, a prediction that can be easily dispelled by a simple demonstration. If one looks through rotating, crossed-polaroid filters at high and low contrast stimuli on a luminous background, the low-contrast stimulus disappears much more quickly than the high-contrast stimulus as background luminance is reduced.

Conclusions One is left to conclude that Weston's studies cannot be used as a basis of validation or extension of a visual performance model owing to inadequate procedural details, inappropriate scoring procedures, and inconsistent results.

Modern experiments

Boyce Boyce²⁰ employed Weston's Landolt ring search task with some procedural differences. Boyce used total, self-paced scanning time as a measure of visual performance. Unlike Weston, who identified the importance of eliminating response times from estimates of visual performance, Boyce ignored response (action) time. Errors (of both omission and commission) were also used to index performance.

Although total time and errors changed with variations in illumination, these dependent variables were not compared directly to discover whether they gave similar or dissimilar representations of visual performance. Actually, the data were probably too variable to

permit proper comparison. As it is not known whether errors and total time are alike, it is uncertain whether either response measure actually characterizes visual performance.

Four sets of Landolt rings were presented to all subjects, one set for each combination of two gap sizes (1.5 and 2.4 min) and two contrasts ($C = 0.4$ and $C = 0.7$). Actually, it is not clear from the published report²⁰ whether the reported task contrasts were obtained under the actual experimental conditions. In a personal communication, Boyce has confirmed that the photometric measurements were obtained under the conditions actually experienced by the subjects. The different sets of Landolt rings were presented under five different levels of illumination. Subjects' viewing angles and distances were controlled, avoiding systematic confounding of illumination level and apparent size, but a different set of subjects was used for each of the five light levels. Because subject groups are confounded with illumination levels, comparisons between performance values and the predictions from a visual performance model would not necessarily coincide.

Waters and Loe In the experiment by Waters and Loe,¹⁹ arrays of high-contrast ($C = 0.9$) Landolt rings (gap size = 1.5 min) were presented to subjects under different light levels and different light sources. Every subject saw all combinations of light level and light source in a Latin square experimental design.

It is not clear how the contrast value reported for the Landolt rings was obtained, but measurements were made "in the position they were placed for the tests (p. 7)."¹⁹ For very high contrast targets like those used in the experiment, however, it is not important to determine the contrast values accurately owing to the predicted saturation in visual response at medium to high contrasts. Despite uncertainties in the measurement technique, therefore, predictions of visual performance at a high contrast task should not be grossly inaccurate. Unfortunately, subjects were not adequately restrained while performing this task. As illumination levels were reduced, they could move closer to the task to maintain a high level of performance.

Performance was defined as the sum of the hits and correct rejections divided by the "net inspection time." Net inspection time was taken to be the total time to scan the array, row by row, minus the "action time" to mark targets in the array. There was no justification for combining net inspection time, hits, and correct rejections in this way; the measure of performance must therefore be suspect. Nevertheless, this combination seems to be appropriate since both hits and correct rejections as well as action time are taken into account.

Conclusions Modern experiments using the classic Landolt ring search task have used methodologies that are

improvements on those used earlier by Weston.^{21,22} Largely typified by the two discussed above, these experiments incorporate techniques that do not completely overcome possible confoundings. Boyce²⁰ controlled viewing angles and distances but used different subject groups for different illumination levels. Waters and Loe¹⁹ used the same subjects throughout, but did not constrain head position or posture. Beyond these problems, and for reasons to be outlined, even the best controlled search tasks may be inappropriate for estimating visual performance.

Boynton and Boss

Boynton and Boss¹⁸ conducted an unusual and well documented study that did not incorporate the Landolt ring matrix. An array of sixteen, randomly located stimuli was presented to young adults with normal visual acuity under different conditions of stimulus contrast (luminous decrements) and background luminance. Viewing distance was held constant while subjects searched for a small square (9 min on a side) among 15 circles (10 min in diameter). For half the trials 16 circles were presented (*i.e.*, no square was presented). The time between onset of the stimulus array and a button press by the subject, indicating that a square was or was not included in the stimulus array, was measured for every trial. A maximum of 20 s was allowed for a trial. **Figure 5** shows the results of the study by Boynton and Boss¹⁸ characterized by the two measures of performance. The cumulative percentage of trials in which subjects correctly reported no square (percentage of correct responses) and the cumulative percentage of trials in which subjects correctly identified squares (percentage of targets reported) are plotted as a function of search time, τ , in seconds. For a stimulus array of 100 percent contrast, the search times required to reach the maximum level of performance regularly decrease as background luminance increases (**Figure 5a, c**). Similarly, for a background luminance of 1370 cd m⁻², the search times to reach maximum levels of performance regularly decrease as the stimulus array contrast increases (**Figure 5b and d**). Qualitatively, these findings are like those predicted in the visual performance model; visual performance should increase regularly with both background luminance and target contrast. Quantitatively, however, it is impossible to compare the visual performance model and the search data because the experimental paradigm used by Boynton and Boss, and possibly in all search experiments, probably confounds the number of targets searched with the difficulty of the visual conditions.

Response time and number of stimuli searched Response times can be affected by the visual processing time for each target (*i.e.*, visual performance) or, obviously, by the number of targets actually processed; the fewer to be searched the shorter the response times. The main

problem with the Boynton and Boss procedure is that one does not know how many stimuli were actually processed. Their recorded times are based upon an unknown mixture of visual processing time and searching time. To isolate visual performance in these experiments one would have to estimate, and then eliminate, search time, which is comprised of time for each eye movement and total number of eye movements. It is probably not possible to use a simple constant to estimate search time; it is logical that subjects will systematically look at fewer stimuli under better visual conditions (high luminance and high contrast). By definition, targets become more visible as the stimulus conditions improve. Under these more conspicuous stimulus conditions, then, subjects would make fewer eye movements to find the target. The manner of visual search changes, therefore, with the level of visibility, so that the number of targets actually searched tends to be systematically fewer as visual conditions improve. As a consequence, searching time is probably confounded with the difficulty of the stimulus conditions. Without accurate eye movement data, yielding both time of each eye movement and how many were made, it would be impossible to estimate searching times or to evaluate the data in terms of a visual performance model.

Conclusions In the Landolt ring search task the stimuli are usually arranged in a matrix. One might suppose that subjects would scan the stimuli in a regular manner (*e.g.*, top left to bottom right), looking at every target once. This may not always be true. Some rows or rings within rows may be missed inadvertently. Similarly, some rows or rings may be scanned repeatedly. If these are random occurrences, it is only necessary to collect more data to determine the relative changes in visual performance with variations in contrast, luminance, size, etc. It seems possible, particularly with the configuration of stimuli in the Boynton and Boss experiment, that there may be systematic changes in the number of stimuli scanned, depending upon the quality of the stimulus conditions. Because the number of targets scanned may be confounded with the difficulty of the visual conditions (even in the Landolt ring search studies), precautions must be taken to determine the number of targets actually searched as well as the time to perform the task. Otherwise visual performance cannot be evaluated.

Contrast threshold task

Several investigators²³⁻²⁵ have used contrast threshold tasks to identify the lower limits of visual performance. Usually, luminous increments are presented to subjects on various luminous backgrounds. The probabilities of detecting luminous increments (contrasts) of different magnitude on a given luminous background are obtained only after many trials. A sigmoid function generally describes the relation be-

Figure 1—Three-dimensional representation of visual performance model for alphanumeric reading material.⁴ Relative visual performance, scaled linearly, is plotted as a function of task contrast and background luminance, both scaled logarithmically

Figure 2—Comparison of score values for the numerical verification task and predictions of visual performance model.⁴ The four curves through the score data were obtained from equation (A1). Model predictions (solid lines) are normalized using one factor equal to 60.66

Figure 3—Comparison of score values at the numerical verification task.⁴⁶ The shaded area is bounded by the 12 and 169 cd m^{-2} score curves from Figure 2. The solid curve through the score values defined by Rea^6 is obtained from equation (A1)

Figure 4—Comparison of visual search performance in two experiments conducted by Weston.²² Both sets of curves represent performance scores for subjects searching for Landolt rings with a specific (3-min) gap orientation. The two sets of data were scaled differently in an attempt to match the trends in performance score at high and medium contrasts. Dashed lines are the performance curves at three task contrasts (top to bottom, 0.92, 0.68 and 0.36) smoothed by Weston for his first experiment; score values may be obtained from the right ordinate. Solid lines are the performance curves smoothed by Weston at four task contrasts (top to bottom, 0.97, 0.56, 0.39 and 0.28) for his second experiment; score values may be obtained from the left ordinate. (Reprinted with permission HMSO, London)

Figure 5—Visual search data, from Boynton and Boss.¹⁸

Figure 6—Probability of detection curves for luminous increments on high and low background luminances. The probability of detection equal to 0.5 is taken as the constant criterion giving visual threshold for any background luminance. Abscissa values for points A and B are contrast thresholds at the two background luminances, defined as equally visible in the CIE model.³¹ Abscissa values for points C and D are contrast values four times those at A and B, respectively, and at their respective background luminances are defined as equally visible in the CIE model. Contrast is defined as the difference between the luminance of the target and that of the background divided by the luminance of the background

Figure 7—Visibility Reference Function (VL1) and Visual Performance Criterion Function (VL8) for the CIE visual performance model (see Figure 3-24, IES³²). VL1 represents the threshold contrast for a circular, 4-min diameter, luminous increment on various background luminances. Contrast in this figure is defined as the difference between the luminance of the target and that of the background divided by the luminance of the background. VL8 is an assumed equal-visibility curve for contrasts eight times those for VL1

Figure 8—Lines of constant criterion, visual performance (see Figure 11, Rea⁴). Curves are labelled in units of relative visual performance (RVP)

Figure 9—Comparison of performance at a numerical recitation task (from McNelis¹), solid lines, and predictions from the visual performance model (from Rea⁴), dashed lines. Model predictions not normalized.

tween the logarithm of the luminous increment on a fixed background and the probability of detecting the increment, ranging from 0 to 1 (Figure 6).

From the probability of detection curves a visual threshold can be determined. This is defined as a constant, psychophysical criterion producing a fixed level of sensation, usually taken as the break-point between perceptible and imperceptible. For a detection task at one background luminance, contrast threshold is represented by a fixed probability of detection, p ; usually $p = 0.5$. Once the criterion has been established it is possible to derive a contrast threshold function that defines contrast threshold at various background luminances. That is, the contrast required to reach threshold, obtained from each probability of detection curve, is plotted against the background luminance for which the probability of detection data were obtained (e.g., VL1 in Figure 7).

Visibility level

A model based largely upon detection threshold studies by Blackwell and his co-workers²⁴⁻²⁸ has been developed to characterize suprathreshold as well as threshold visual performance.^{30,31} In this CIE model suprathreshold levels of visual performance are represented by so-called Visibility Levels (VL) derived from the contrast threshold values. The contrast threshold curve for detecting a small disc, called the Visibility Reference Function, VL1, has been taken to represent the basic relation between background luminance and threshold contrast.³² Higher criterion levels representing constant suprathreshold levels of visual performance are obtained by simply multiplying the Visibility Reference Function by a fixed factor (see caption, Figure 6). VL8 in Figure 7, for example, is the equal visibility curve relating background luminance to contrasts eight times those at VL1.

Probabilities of detection equal to unity In the CIE model,^{30,31} two parameters, M and s , of a cumulative normal distribution describe the sigmoids (or so-called normal ogives) fitting the probability of detection data at each background luminance (e.g., Figure 6). M is the mean of the normal distribution of contrast values (corresponding to the 0.5 probability of detection) and s is its standard deviation. The latter parameter characterizes the steepness of the sigmoid. Blackwell reports (see Technical Note 16³¹) that the ratio s/M is a constant equal to 0.37 for all background luminances.

The probability of detection curves for all background luminances eventually become equal to unity at high contrast. It is quite unlikely, however, that the high contrast targets will, in fact, produce the same level of visual performance at different background luminances. Reaction times to equally high contrast stimuli are different at different background luminances.³³ Similarly, the levels of performance at the

numerical verification task are systematically different for the same high-contrast targets at various background luminances.⁴ These results are consistent with the wealth of literature on the Pulfrich pendulum phenomenon (e.g., Lit³⁴).*** It is therefore impossible to use this methodology directly to discriminate between levels of suprathreshold visual performance.

By the same token, the probability of detection methodology cannot be used to validate a central assumption in the CIE model, namely, that contrasts on the same VL function (e.g., VL8 in Figure 7) will produce the same level of suprathreshold visual performance. When all of the probabilities of detection associated with high contrast targets are equal to unity, it is not only impossible to use those data to discriminate among levels of suprathreshold visual performance (discussed in the previous paragraph) but also impossible to validate the assumption that all the points on a single Visibility Level function give the same level of visual performance. Another technique, perhaps using reaction time, would have to be employed to test the assumption directly.

It seems unlikely, however, that this assumption is valid.⁴ It has been shown that constant criterion curves from the visual performance model for luminous decrements are not simple multiples of the lowest (threshold) criterion function. Graphically, the constant criterion curves are not parallel in a space defined by log contrast (ordinate) versus log background luminance (abscissa) axes; the separation in contrast between constant criterion curves is greater at low luminances than at high (Figure 8). This increased separation at low luminances happens because the level of maximum visual performance associated with maximum contrast at a given background luminance increases systematically as background luminance increases. Since it was impossible to attain as high a level of visual performance at low background luminances as at high, even for maximum contrast (unity for luminous decrements), it is impossible for the constant criterion curves (i.e., Visibility Levels) to be parallel throughout the log contrast versus log background luminance space, as supposed in the CIE model and illustrated for luminous increments in Figure 7.

Effects of guesswork or response bias on probability of detection In these experiments the probability of chance detection (guessing) is determined in an attempt to isolate the visual threshold from the non-sensory

***A high contrast pendulum swinging in one plane will appear to swing in an elliptical path when viewed binocularly if the two eyes are not adapted to the same light level. The latency of the signal from the eye adapted to the lower light level is lower in reaching binocular cells in the brain than is the signal from the eye adapted to the higher level. This difference in latency, dependent upon the difference in the adaptation luminances, presumably produces the illusion.³⁵

contributions to the subjects' responses. One of two options is generally employed to characterize the visual threshold. With the first option a criterion equal to chance detection is chosen as the probability of detection to represent visual threshold. If, for example, one is forced to decide whether a target is presented to the left or to the right of a fixation point, the subject has a 50 percent chance of guessing the correct location. Probabilities greater than chance (in this case $p = 0.5$) are taken as visual events. Thus, a 0.5 probability of detection criterion is taken as visual threshold, the break-point between perceptible and imperceptible. Response bias presents a difficulty with this approach. For example, if subjects are biased to say "right" all the time, the best score would be $p = 0.5$, no matter how high the contrast. This, of course, would never happen, but if subjects have some degree of response bias and it is unknown, all of the probability of detection data would reflect both visual response and psychological bias.

With the second option, the raw probability of detection (p) data are mathematically transformed, according to the following formula, into a new set of probability of detection (p') data in an attempt to correct for guesswork.²⁹

$$p' = (p - q)/r \quad (2)$$

where q is the probability of guessing the correct answer r is the probability of guessing the wrong answer and $r + q = 1$.

As with the first method, chance behavior is typically taken as an estimate of q . For the example given, both r and q are equal to 0.5. Thus, for $p = 0.5$, $p' = 0.0$. Values of p' greater than 0.0 can be taken as greater than threshold. As with the first method, response bias can play a part in the transformed probabilities of detection. Blackwell and Scott,²⁹ for example, have shown that the values of q and r cannot always be characterized simply by chance detection; their transformed probability of detection data are still combinations of seeing and non-random guessing. Steps had to be taken to transform these data further in order to eliminate response bias. Blackwell and Scott used an empirical, graphical correction procedure to adjust values of p' . It applies only to their data, but such efforts illustrate the difficulty of isolating visual response in probability of detection data.

In many experiments (not just detection tasks) response biases have been neither assessed accurately nor documented. In these cases the probability of detection data should be suspect. The choice of criterion probabilities to represent threshold may be arbitrary and unrepresentative of the visual response. Occasionally, special experimental techniques or methods of analysis (as in the Blackwell and Scott experiments) must be employed to eliminate response bias.

Conclusions The probability of detection methodology has been used to model suprathreshold visual performance, but there can be fundamental difficulties in the response measure that limit its utility. First, high contrast targets associated with probabilities of detection equal to unity (or the same Visibility Level value) at different background luminances do not always produce the same level of visual performance. Second, special care must be taken to ensure that the probability of detection curves are not contaminated by subject response biases.

A timed recitation task

McNelis¹ conducted an unusual study of visual performance when on each trial an observer was required to name two briefly-presented (0.4 s) small letters (8 min in size) separated by a 10-degree visual angle. The letters were presented at different contrasts (luminous decrements) and at different background luminances. Accuracy (correct identifications) was used as the dependent variable. Performance was better for the left letter than for the right letter. As confirmed by eye movement data, there was not enough time for subjects to identify the second (right) letter in 0.4 s except at high luminances and contrasts. McNelis' accuracy data for the left letter have been redrawn from data supplied by him and presented in **Figure 9**.

Methodological issues

The McNelis study was well documented, but it did not make entirely clear whether subjects' heads were restrained. He has confirmed in a personal communication that head positions were indeed restrained by a chin rest. Postural changes in response to reductions in luminance were prevented, so that apparent size and luminance were not confounded in the study. McNelis also notes in a personal communication that contrast measurements were difficult to make and that there may be some uncertainty in the values; such uncertainty makes predictions of performance, particularly for low contrast stimuli, more tenuous.

Professor S.W. Smith points out in discussion of the McNelis study that subjects did not operate at a chance level of accuracy; correct identifications approached zero under difficult conditions. Subjects were apparently biased in their responses against errors of commission (*i.e.*, false positives). Because the number of correct responses due to guesswork were apparently very small, the simple measure of accuracy as the dependent variable was probably (but not certainly) representative of visual performance.

Comparison with predictions from the visual performance model

Other than the potential problem of estimating absolute contrast, the methodologies employed in the McNelis study appear to avoid most of the problems

outlined in this paper. For this reason it seems appropriate to compare predictions of the visual performance model developed by Rea in an earlier paper⁴ with McNelis' accuracy data. Both the model predictions and McNelis' accuracy data are shown in **Figure 9**. The model predictions at the three contrasts were all normalized by use of a single factor of 100.

Agreement between McNelis' data and the model predictions are excellent at the two higher contrasts. Even at the lowest contrast they are reasonably good. Difficulties in both model predictions and the McNelis data are evident, however, in **Figure 9**. First, the model predictions at the lowest contrast are probably inaccurate to some degree; the slope of this lowest function does not continue to decrease with increases in background luminance, as one would expect. More refined estimates of visual performance at the lowest contrast values are required. Second, the contrast values reported by McNelis are, as he indicated, probably inaccurate. Examining his data for the highest luminance, it is clear that the difference in performance is greater for contrasts of 0.5 and 0.25 than for 0.25 and 0.125. It would be more reasonable to expect the difference in performance to be larger for the latter two contrasts than for the former pair. Inaccuracy in evaluating the contrast of task materials is, of course, problematic in evaluating the visual performance model. Again, care must be taken to specify contrasts of task materials accurately, particularly at the lowest contrasts, because visual performance changes most rapidly just above contrast threshold.

Conclusions

The study by McNelis¹ appears to avoid most of the procedural problems outlined in this paper. One would expect, then, good agreement between predictions from the visual performance model and the reported accuracy data. Indeed, there was marked similarity between model predictions and the data, despite obvious procedural differences between the numerical verification task and those used by McNelis. It appears that the McNelis procedures could easily be used to extend and validate the visual performance model.

Discussion and conclusions

In the past it may have been less important to obtain precise estimates of stimulus conditions or visual performance as distinct from overall task performance. Traditional recommendations of illumination levels for various tasks were made without specifying factors important to vision, such as task reflectance and contrast.³² Only very general information about task performance was necessary to make lighting recommendations of this kind and, perhaps rightly, investigators did not strive for experimental precision greater than that required by the various national and

international sanctioning bodies. Perhaps, too, practitioners have been concerned with lighting real tasks and many investigators deliberately studied overall performance at different kinds of simulated, realistic tasks. These experiments gave a general indication of how performance is affected by lighting, but for many of them it was not possible to isolate the contribution of each visual factor to performance nor to disentangle the visual factors from non-visual ones that contribute to performance. It was not possible, therefore, to determine how important lighting is to visual performance at a task.

There has been recent emphasis on more exacting specification of the conditions important for visual performance, as reflected in more refined recommendations of illuminance.³ Size, contrast and the age of the worker are important factors that must be specified when determining levels of illumination. If it is necessary to follow the more precise recommendations of lighting quantity and quality for visual performance, then it is necessary to have more explicit knowledge of the relationships between luminous parameters and visual performance.

The model proposed by Rea⁴ is an attempt to provide quantitative predictions of visual performance at an alphanumeric reading task from the photometric specification of the luminous stimuli. Based upon a variety of arguments, the proposed model seems to be an adequate, first-order characterization of visual performance at that kind of task. It is not necessarily far-reaching in its predictive capabilities (*e.g.*, different text sizes and age groups are not included), nor has it been completely verified. For reasons outlined in the present paper, problems with experimental procedures, photometric measurements, and indices of performance limit the utility of many earlier studies for validating and extending the visual performance model. It is therefore necessary to design and control future experiments more carefully in order to assess and extend the model predictions. The recommendations for future experiments on visual performance identified in this paper are summarized in **Recommendations** below.

Although more experiments must be executed to validate and extend the visual performance model, the close correspondence of the model predictions with well controlled electrophysiological³⁶ and psychophysical¹ studies implies that it will be an important step in developing a general model of supra-threshold visual performance. When the necessary experiments have been completed and a final visual performance model is established, it will be possible to make more precise lighting recommendations, and, importantly, to justify them.

Recommendations

It goes without saying, perhaps, that experiments

should be well designed, executed, and analyzed. Barber,³⁷ for example, identified important issues in conducting experiments and describing the results. Care should be taken to adhere to this credo in future experiments on visual performance. Common, recurring problems specific to past studies of visual performance have been identified in this paper and deserve special mention.

Independent variables

Accurate measurements of apparent luminance, contrast, and size must be obtained, and must represent the conditions actually experienced by the subjects.

Dependent variables

Indices of performance should not distort visual response. Simple measures of time and errors rather than arbitrary scoring indices should be used. The measures should be sensitive to changes in visual response across experimental conditions. Equal probabilities of detection at different background luminances, for example, do not necessarily result in equivalent levels of suprathreshold visual performance. The response measures should also limit the influence of non-visual factors.

Experimental design

Proper experimental designs must be used to eliminate various sources of confounding. Subject sub-populations should not be confounded with experimental variables. Absolute differences in subjects' response levels can mask differences in performance due to experimental variables. Systematic behavioral changes in response to experimental variables should be eliminated. For example, leaning forward as illuminance levels are reduced confounds the effects on vision of apparent size and background luminance. Similarly, in a search experiment the number of stimuli inspected may be confounded with background luminance.

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Appendix A

All the score data in **Figures 2** and **3** were fitted with the same general form of equation (1) developed by Rea.⁴ This equation is repeated in equation (A1) below in a slightly different form. Parameter estimates for each set of data were obtained by a least squares criterion using a non-linear regression computer routine.

$$R/R_{\max} = \Delta C^n / (\Delta C^n + k_0^n) \quad (A1)$$

where

- R = score in equation (1)
- $R_{\max}$ = a free parameter equal to the maximum score for a given background luminance, L_B
- n = a free parameter affecting the shape of the curve
- k_0 = a free parameter, also affecting the shape of the curve, representing the increment in contrast above threshold contrast (C_t) producing half maximum response.

$$\Delta C = C_v - C_t \quad (A2)$$

$$C_v = \text{task contrast} = (L_B - L_T/L_B) \quad (A3)$$

where

- L_B = task background (e.g., paper) luminance
- L_T = task target (e.g., printed digit) luminance
- $L_B \geq L_T$
- $C_t = C_v$ at contrast threshold

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