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Automated-Manual Transitions: Human Capabilities and Adaptive Cruise Control

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Khoi M. Nguyen, Tieuvi Nguyen, Natsuko Toyofuku**

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Final Report for Task Order 4221

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Human Capabilities
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I. INTRODUCTION

ITS innovations in California are likely to include automated systems for vehicle guidance. Such systems will supplant manual controls during certain types of vehicle operation. However, the alternative manual control must remain intact in the vehicle. Thus, epochs of automated-manual transition (A-MT) are inevitable. The problem is how to characterize a given transition type and then how to optimize it. In this study we examined one of the several predictable transitions that use of the Adaptive Cruise Control (ACC) will lead to. This predictable A-MT is the event that would ensue when a lead vehicle (LV) suddenly brakes maximally and a following vehicle (FV) under the control of ACC must react appropriately (hereafter referred to as the LV Braking Scenario). Such an event is neither unlikely nor is it benign. It can be shown that within the current design of ACC, this event will lead to a collision. Collision avoidance is possible but only if the human operator (HO) of the FV assumes manual control in a timely way. In TO 4221 the conditions required for a graceful A-MT in this scenario were investigated. We focused attention on two features of the HO, those visual capabilities required by the HO to determine the *need* to assume manual control and also those features of an in-vehicle warning signal (initiated by either vehicle) that could reliably prompt appropriate HO action. This was accomplished in the context of three tasks¹, summarized below.

Task 1: Literature Review

The visual cues that human observers use to detect and avoid impending collisions have been speculated on, but never fully resolved. In addition, the collision avoidance literature is nowhere summarized, so we began the project with a thorough literature review that explored the pertinent human factors, perception, and vision science literatures. The goal of this task was to identify those visual cues that prior research identified as likely being the most important to human observers in detecting and avoiding impending collisions. With these we next designed an experimental program (see Task 3) to quantitatively evaluate human reaction to these cues in the context of the LV Braking Scenario.

Task 2: Development Of Laboratory MicroSimulation of the LV Braking Scenario

The second task for this study was to develop a laboratory microsimulation of the LV Braking Scenario for use in the experimental studies. The microsimulation research facility developed in PATH Project MOU323 was used as a reference and starting point for this effort. The development of a single “general purpose” microsimulation for use in all of the experiments proved impractical, so separate, simpler microsimulations were developed for each experiment. Since this development was carried out as each of the individual experiments was set up, descriptions are included with those of the experiments that they were applied to (and not in a separate section).

Task 3: Laboratory Studies of Cues to Headway Change Judgment

In this task an experimental program was designed and carried out to investigate the cues identified in Task 1. In all, four experiments were conducted, as listed in Table 1 (here θ_{init} refers to the angle the LV subtends in the FV driver’s visual field at the onset of LV braking and $d\theta/dt$ its subsequent rate of change).

Task 4: False Alarms Statistics

In this task we studied the statistics of false alarms gathered in Task 2, Experiment 2b, in order to identify their structure and the precise manner in which HO’s trade false alarms for misses. The results take the form of ROC curves, which can in turn be used to design ACC systems to respond more effectively in the LV Braking Scenario. This analysis was carried out in conjunction with Experiment 2b and is reported on there (and not in a separate section).

¹ This task summary differs in form from that presented in the original Task Plan, to better reflect the manner in which the work was actually carried out. The individual activities performed, however, remain consistent with those that were originally proposed.

| # | Experiment | Cue(s) Investigated |
|---|--|----------------------------------|
| 1 | A Test of Leibowitz Hypothesis | θ_{init} and $d\theta/dt$ |
| 2 | A Test of the Threshold Assumption | $d\theta/dt$ |
| 3 | Making the Onset of FV Braking More Detectable | $d\theta/dt$ |
| 4 | The Effects of Texture Dilation | Texture |

Table 1: Experiments Conducted

II. TASK RESULTS

A. Literature Review

The study of how people navigate through their environment has a vast literature associated with it, in part because it is important to avoid collisions and plan paths through the world and in part because it is still unclear exactly how it is done so efficiently and well (and alternatively, what factors contribute to mistakes).

The manner and direction in which an object moves contains a great deal of information, even when considering a single edge or point on the object. The edges of the object can translate (move parallel to the visual horizontal or vertical horopters), they can rotate within or out of the plane, and they can contract or move in depth. These types of motion are thought to be processed individually by the visual motion perception system [Regan (1), Regan and Beverly (2)] and thus could be expected to have differing reaction times and resolution capabilities.

The perception of objects that are expanding in visual size (“looming”) is particularly important as it often indicates objects that are about to collide with the observer. While optical expansion does not always give rise to the sensation of looming [Regan (3)], it is perhaps unsurprising that in the absence of other cues humans tend to assume an object is looming rather than changing in size [Yilmaz and Warren (4)]. Hypothetically this could be because looming is more likely to be an immediate threat. Regan (1) and Regan and Beverly (2) hypothesized that there are distinct “looming detectors” in human visual motion processing which detect this type of motion. Not only are these cells or channels particularly sensitive to looming motion but there is some evidence that they are even separate from detectors of the opposite type of motion in depth—contraction [Regan and Beverly (2)].

Conversely, studies conducted by Braddick and Holliday (5) and Sekuler (6) suggest that the perception of looming motion does *not* involve explicit coding of relative motion. It does not matter if it is contracting or looming, the system is sensitive to both. They proposed that the response of the neurons is a direct result of simple pooling of 2-D unidirectional detectors. Despite these apparent contradictions, all this implies that the neurological system for perceiving these changes is specialized or optimized for this task, regardless of how it accomplishes it.

But what information from the visual world do we use to discern looming? How do we decide if we are about to collide with the vehicle in front of us or if we are going to stop in time? When a driver is making a judgment about the distance, relative velocity or deceleration of an LV or other object in front of him, there are many factors he could be utilizing to make such judgments. While binocular cues such as retinal disparity have been studied extensively, these are only effective when viewing objects less than about 10 meters away. Beyond this distance the visual system must rely exclusively on monocular cues. Since the general LV Braking Scenario encompasses intervehicle separations in excess of 10 meters, it is reasonable to assume that monocular cues are the predominant ones used by the FV driver. For this reason, we will in this survey restrict our attention to monocular cues.

There are many possible monocular cues:

- First Order Cues:
 - Velocity of self and LV/object
 - Image edge speed
 - Rate of angular expansion ($d\theta/dt$)
 - Texture dilation
 - Relative velocity
 - Time to contact/collision (τ or TTC)
- Second Order Cues:
 - Acceleration or deceleration of self and object
 - Relative acceleration/deceleration
 - Time derivative of τ ($d\tau/dt$ or tau-dot)

When estimating velocity we need some way of measuring how far or how much the object (or part therein) has moved/expanded over a fixed amount of time. To make this estimation, we could be using many different parts of the object or even parts of the background scene, such as:

- Width, height and area increases of object;
- Proportional increase in the amount of visual field occupied;
- Decrease in distance to object from fixed object;
- Reduction in exposed pavement or space between self and object;
- Increase in angular size.

Most of these are not complex measurements and we are actually fairly accurate at scaling the relative velocity of automobiles under certain conditions. But an individual's capacity to estimate the actual difference in velocities between two vehicles has been shown to be fairly low, especially at low relative velocities [Hoffman, (7)], and that accuracy improves as the distance between the vehicles decreases and when the gap is closing as opposed to widening [Olson, et al. 1961 as cited by Hoffman (7)]. Using film clips made from a video camera in a moving car following another car, Hoffman and Mortimer (8) showed that subjects viewing the films were able to accurately scale the relative velocity between their own and a lead vehicle but only when the subtended angular velocity of the lead vehicle exceeded a threshold of about 0.003 rad/s (See Task 3, Experiment 2, however, for an investigation of this threshold assertion) and the relative velocities were high. Denton (1963) [as cited by Hoffman (7)], found that the accuracy of estimating the actual velocity of a car decreases at high speeds. This implies that there is a visual resolution limit on the perception of angular velocity, if the change is too small, our visual systems cannot correctly interpret the velocity of the object. In support of this, Häkkinen (1963) [as cited by Hoffman (7)], using films clips made by a stationary observer looking at moving vehicles, found that most people were good at estimating the distances between the two vehicles, but not the relative velocities. Braunstein and Laugherty (1964) [as cited by Hoffman (7)] found that detection time increased as the gap between the vehicles increased and decreased as the lead vehicle's acceleration increased.

Swanston and Gogel (9) found that the relationship between perceived size and distance is not linear. The velocity across the retina of any part or edge in the visual field does not always have a good correlation to the actual velocity of the object, as it is very susceptible to influences from the physical distance to object, aperture size, illumination, expected or familiar size [Brown (10)], visual angle [Epstein and Landauer (11)] and previous expansions or contractions of the object and fluctuations of the texture within the object [Whitaker, et al, (12)]. This was found even though there is evidence that we are able to use the retinal size of an object to recover its size and distance and that this judgment is unique for each combination of sizes and distances [Landauer and Epstein (13)].

Andersen, et al (14), using a computer generated simulated driving environment on a monitor (medium fidelity simulator) found that acceleration and deceleration are hard to separate from other factors and can be easily confounded with tau-dot (described later). He found that collision detection varied as a function of the size of the obstacles, observer speed, and edge rate, as opposed to Yilmaz and Warren (4), who found no effect of these factors.

Time to contact (TTC or τ) is a compellingly simple cue, it is simply an evaluation of how long it will take before the observer contacts the approaching surface. TTC can be found from

$$TTC = \frac{\theta}{d\theta/dt},$$

where θ is the angle subtended by the object and $d\theta/dt$ its rate of change.

In 1985 Hersch Leibowitz (15) authored the conjecture that the large size of trains fools us into thinking that they are moving more slowly than is actually the case. If so, the same might be true for our approach toward

vehicles from the rear. Either the headway to a given vehicle gets harder to estimate as we get closer, or larger vehicles are harder to gauge at a given distance, or both. (The geometry is the same in either case.) Suppose that the first glance at an object involved a computation of TTC, as given above. The question becomes whether any piece of the computation might be affected by angular subtense θ .

TTC can also be found through the optical expansion of a single point on a surface. If $Z(t)$ is the distance of the observer to a point on a surface and he is approaching it at velocity $V(t)$, then TTC represents the ratio of those two measurements. This ratio is inversely proportional to the rate of expansion $v(t)$ of the surface's optical projection $r(t)$ on the retina. Thus

$$TTC(t) = \frac{\theta(t)}{d\theta/dt} = \frac{Z(t)}{V(t)} = \frac{r(t)}{v(t)} \quad [\text{Lee (1976), as cited by Kim, et al (16)}]$$

There is a great deal of evidence that we use some sort of ratio of first order cues to estimate TTC [Lee (1976, 1980), Bootsma and van Weiringen (1981), Lee and Reddish (1981), Todd (1981), Lee, et al.(1983), Schiff and Oldak (1990), Savelsbergh, et al. (1991), Kaiser and Mowafy (1993), Schiff and Detwiler (1979), Regan and Hamstra (1993), and Regan, et al (1995), all cited by Yilmaz and Warren (4)].

Swanton and Gogel (9) found that if an object seemed larger than it really was then it was seen to be moving more in depth and faster than it really was and TTC judgments were inaccurate. If people have learned through experience that this is the case, they might use this prior information and overestimate TTC when the object is large. Regan and Vincent (17) found that the starting size of the object made a difference in how the observer perceived TTC. With small objects, accuracy in judgment of TTC may be dependent on binocular information—Gray and Regan (18) found that subjects could not discriminate trial to trial changes in TTC for small monocularly viewed objects.

Vincent and Regan (1997) and Beverley and Regan (1983) [as cited by Whitaker, et al (12)] found that mismatches between texture expansion rate and changing size led to consistent errors in TTC and motion in depth estimates, biased in the direction implied by the texture expansion. Pierce, et. al (19) found that if the rate of optic flow was increased in relation to the observer's motion, estimated TTC decreased and the observer reported feeling like they were moving faster, even with all other cues (such as ground texture) removed.

Mourant, et al (1969) and Land and Lee (1994) [as cited by Lappe and Hoffmann (20)] found that when driving along a straight path, drivers tended to focus their gaze at the focus of expansion for the optical flow field. This would imply that people prefer to use their fovea to judge optic flow patterns of expansion and contraction and are most accurate when they do so. This is supported by Regan and Vincent's (17) finding. Using squares displayed on a computer monitor, they found that peripheral and foveal vision are different in their ability to be interpreted independently of other changing factors of the image such as rate of expansion. With peripheral vision, varying the rate of expansion caused illusory variations in the TTC. So if the object or its edge lie in the periphery of the visual field, estimates of TTC can be confounded by other information. Czermak (1857) [as cited by Brown (10)] found that motion in periphery is phenomenally slower than in fovea.

Some believe that the time derivative of τ is the key to proper control of deceleration. Consider a surface that is $Z(t)$ away from an observer traveling towards it at instantaneous velocity of $V(t)$ and with a constant deceleration of D . D is adequate only if at that deceleration the observer can come to a complete stop within $Z(t)$. That is

$$\frac{V(t)^2}{2D} \leq Z(t).$$

Rearranging gives

$$\frac{Z(t) D}{V^2(t)} \geq .5,$$

and since

$$\tau(t) = \frac{Z(t)}{V(t)},$$

$$\frac{d\tau}{dt} = 1 - \frac{Z(t) D}{V(t)^2},$$

then

$$\frac{d\tau}{dt} \geq -0.5.$$

This dimensionless number implies that if $d(\tau)/dt \geq -0.5$ a “soft” or no collision results, and if $d(\tau)/dt \leq -0.5$, a “hard” collision results. A number of researchers have found experimental support for this mathematical prediction using squares displayed on computer screens (pre-simulator) [Kim, et al (16), Yilmaz and Warren(4)]. However, this has been questioned recently by Andersen, et al (14), who found tau-dot too difficult to separate from constant deceleration, which appeared to be a better predictor of collision.

Despite all this research it is still not entirely clear which cues are of primary importance in detecting and avoiding impending collisions. Perhaps we are using neither velocity nor TTC nor any of the second order cues and are relying entirely on counting the seconds between the time the LV passes an object and the time we pass the same object. Different people may use different cues, or use different cues depending on the task demanded of them. Many of the experimental results may only hold true when the observer is not moving, which might lead to different strategies during driving than when judging moving objects, and possibly there is a difference between the self motion cues while driving and while walking [Lappe and Hoffmann (20)].

There are some consistent results from the research, however. Second order cues seem to be good predictors of collision events. Also, if we could somehow call attention to the changing parameters of an object, so that they are perceived more accurately from further away, perhaps we could increase the accuracy of the first order cues. Schiff and Detwiler (21) found that changes in two dimensional angular size correlated better with TTC judgments than three dimensional cues such as distance/velocity. (These also correlate with the TTC judgments, but the lack of observer motion may make these inapplicable to the driving task.) This could lead to a device that calls attention to a two dimensional cue such as the changing width of the vehicle image (observers seem to be more sensitive to changing width than changing height from an experiment performed by Cohn, Toyofuku and Nguyen). Lights could be added to the sides of the vehicle [Mortimer (1971b) as cited by Hoffman (7)] emphasizing the increasing angular size and perhaps lowering the angular expansion rate threshold asserted by Hoffman (7).

Perhaps it is the environment that needs assistance, such as painted lines on the road. If no cues of actual distance are supplied estimations of speeds of motion in depth were found to be proportional to the rate of change of the size of the object [Regan (1)]. This supports the idea of some learned or hardwired assumptions about the position of objects in the world. This is consistent with Gogel's (1969) and Gogel and Tietz's (1973) [as cited by Swanston and Gogel (9)] finding that people tend to assume objects are roughly 2-3 meters away in the absence of other cues. Using this assumption could lead to many mis-estimations of velocity and of course, distance. It is not certain that adding ground texture helps at all. Kaufman (1974) [as cited by Regan (1)] and Yilmaz and Warren (4) found that observers were quicker to make judgments when they had fixed objects or a ground texture to compare the motion of the target object to. However Schiff and Detwiler (21) found evidence that adding ground texture and distance information did not assist the accuracy of the judgment of TTC, so even if they react faster it might not mean they have improved their accuracy.

What if the object itself was textured? Beverley and Regan (22) concluded that adding texture to a stimulus that is originally un-textured reduced its effectiveness of motion-in-depth simulation, unless its

expansion/contraction was exactly matched to that of the object. To avoid this affect, precautions must be made to maintain consistency between the two. When texture and size of object were locked in the manner characteristic of everyday objects, the motion-in-depth system was stimulated effectively. When texture grain remained constant w/ object size increasing, the motion-in-depth system was only weakly stimulated. This would imply that our visual system is very sensitive to inconsistencies in texture changes. If texture were exaggerated, then perhaps people would perceive the looming of an object better (in the case of driving, an overestimation of deceleration could help prevent collisions).

It is possible that driving is a task that our visual system is not as well designed for. Beverley and Regan (23) found that if we have changing size filters they are not sensitive to pairs of edges separated by more than 1.5 m. Conversely, the detection of collisions might require a larger area over which to pool local velocity information if an accurate estimation is to be made [Saidpour and Andersen (24)].

The multiple situations under which subjects made errors of estimation suggests several possible explanations:

1. They are not focusing on the correct visual cues to estimate size, velocity and/or distance;
2. They are not able to accurately process the information;
3. The cues they use are particularly prone to error and influence from other factors;
4. Rather than relying on a few cues, many are used and their information is combined in some compromising fashion.

The first explanation is not likely to be true given the proficiency with which people drive and walk and move around in the world without collision or serious damage. Given the limitations of resolution of the human visual system, it is likely that they are not able to process information as well or as quickly as a computer or robot navigation system might. Most objects in the real world tend to behave fairly consistently, there is rarely a discrepancy between texture expansion and size change and so on. So although the cues may be prone to error when manipulated in the lab, people's reliance on them is possibly due to their reliability in the real world. As for the last possible explanation, there is evidence that we do pool information, not just from various distance and velocity cues but from other cues as well, such as sound, road "feel", knowledge of the driving area, etc.

Nearly all of these experiments have focused on testing parameters in isolation or in very careful combination with other cues. A very clever method called "classification images" devised by Beard and Ahumada (25) and followed up by Gold, et al (26) and Murray, et al (27), tackles the question from the opposite direction. This method involves adding "noise" to the edges of contours and shapes in order to discriminate the specific areas people tend to focus on. The experiment uses 10,000 trials per image and calculated the errors resulting from noise in each pixel location. Results obtained by this method provide very specific information in regards to what area(s) people fixate on the most, without any previous bias or hypothesis of where in the image they might be looking.

Using this method in combination with the isolated or selected multiple cue experiments may yield a fuller picture. The former will isolate spatial areas to focus on and the latter will show how these interact in impoverished and extreme situations and also point to how they might be enhanced or altered to be more useful in detection of looming objects.

B. Laboratory Studies of Cues to Headway Change Judgment

Introduction

Based on our literature review, we chose to concentrate on the first order cues of angular expansion rate and texture dilation. There is broad consensus that these are fundamental to the collision detection and avoidance task, and there are still enough unresolved issues surrounding their function in this context that further study is warranted². In particular, evidence confirming or denying the Leibowitz Hypothesis and the (angular expansion rate) Threshold Assumption would have an immediate impact on our understanding of the visual system's capabilities in the LV Braking Scenario, which in turn would be of practical benefit in designing ACC to accommodate the related A-MT. These findings could also find immediate use in human driver models that may incorporate assumptions at variance with them.

Texture is ubiquitous in the everyday visual environment, and no less so in the LV Braking Scenario. Textural cues should therefore have a significant affect in detecting and avoiding collisions of this type. Studies of texture dilation in this context, though, are few and inconclusive. Here again additional research is warranted and offers immediate practical benefits.

Finally, we had an opportunity to extend our findings in the context of an actual on-road study, which we also report on here. In all, four laboratory studies were conducted:

1. A Test of Leibowitz Hypothesis;
2. A Test of the Threshold Assumption;
3. Making the Onset of FV Braking More Detectable;
4. The Effects of Texture Dilation.

These are reported on in the remainder of this section.

² Another reason to focus on first order cues is that TTC and $d\tau/dt$, which appear to have particular utility in the collision detection and avoidance task, may well be built up from the same mechanisms that detect angular expansion rate and texture dilation, making an understanding of these latter mechanisms all the more important.

Experiment 1: A Test of Leibowitz Hypothesis

Introduction

The purpose of this study was to test the Leibowitz Hypothesis (15), which conjectures that the large size of trains fools us into thinking that they are moving more slowly than is actually the case. The sensory problem is described schematically in Figure 1. For an object traveling towards the observer at a fixed rate, Time to

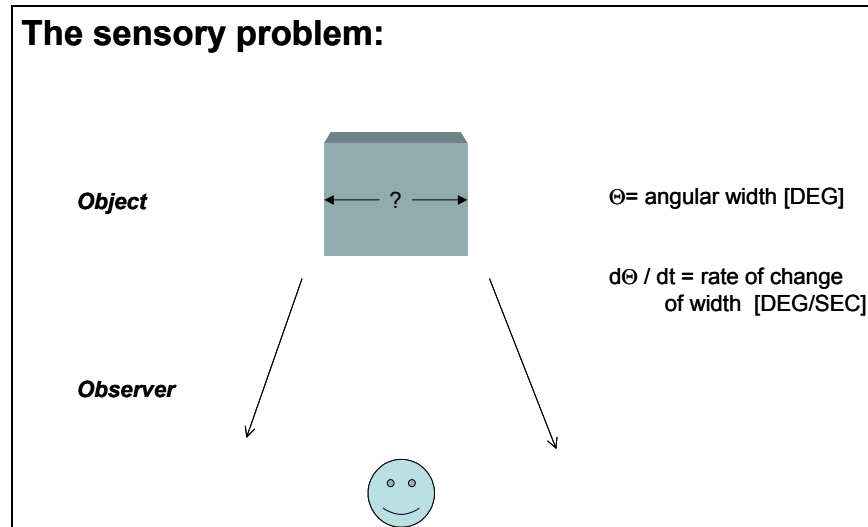


Figure 1: The Sensory Problem

Collision (TTC) is given by

$$TTC = \frac{\theta}{d\theta/dt}.$$

The question becomes whether any piece of the computation might be affected by angular subtense θ .

Methodology and Procedure

We simulated key elements of the visual scene comprising a looming object. A gray square presented on a computer monitor served this purpose. Its size was a two-dimensional indicator of its 'distance' from the observer, although the actual distance was fixed at 1.5 M. The gray stimulus (pixel luminance value of 150 with range of 0-255) was presented on top of a background, whose particulars are discussed below. This stimulus was displayed on a CTX color monitor, with a display resolution of 1024 x 768 pixels. The goal was to create a two-dimensional version of a "looming" effect that would be caused by an approaching object, one whose depth was diminishing. This was approximated by using five separate successive images, each, after the first, overlapping the prior. Each successive overlapping image expanded in size, in accordance with the monocular cue that was being isolated, and was shown for a duration of 13.3 milliseconds

Four different conditions were tested, for each of three different square starting sizes (3.7, 5.3, and 7.6 degrees of visual angle to a side):

1. The nature of the change (width expansion only vs. height expansion only);
2. The amount by which the square expanded (amount proportional to initial square size vs. a fixed amount for all square sizes);
3. Lighting Conditions ("day" vs. "night");
4. Point of Fixation (fixation on the center of the square vs. fixation on an active edge).

Subjects were six college students ranging in age from 18-26, two of whom were female. Of the six, half required and thus wore their corrective lenses. Subjects were seated 1.5 meters away from the computer screen and used a keyboard to input their responses. All were given written instruction before the start of every experiment and were initially allowed to practice five trials in order to familiarize themselves with the procedure. In the case of the night condition, subjects were allowed to adapt to the 'dark' for approximately ten minutes. The subjects were then instructed to focus on the fixation point and to press a button to initialize each trial. Once the button was pressed, a pair of simulated red alerting lights turned on to inform the subject that the looming was about to begin. At a random time after the ignition of the alerting lights (time ranging from 0-5 seconds) the looming would commence. The subjects were instructed to press a button at the earliest time possible, after detecting the looming effect. The proportional change tests consisted of 100 trials/observer and 5 subjects, whereas the fixed change tests and those conducted with fixation at an edge employed 50 trials/observer and 6 subjects. While practice effects are well-known in reaction time studies [Boff and Lincoln (28)], we think our design was conservative with respect to this possibility. Target size in our tests always ascended from small to large in sequential runs in a given set of conditions.

Results

Our purpose in this study was to examine the Leibowitz hypothesis that the speed of an object approaching an observer is misestimated in the direction of lower speed for larger objects. We chose not estimated speed but rather time to react (RT) as a measure of the accuracy of response of the observer, as the latter is a more objective criterion of response fidelity. We simulated the looming object scenario in its simplest form: an expanding gray square viewed on a computer monitor. We measured reaction times for five subjects (six in some cases) for each of the four viewing conditions described previously.

Overall Effect of Initial Square Size

The first comparison is the most critical as regards the Leibowitz hypothesis, and is the first that we know of to actually place his hypothesis at risk. We combined reaction times across observers, cues and conditions. The mean reaction times for each of the three starting sizes are shown in Figure 2. Error bars in this and in later figures indicate twice the standard error. Observers required longer to process the signal for the largest sizes. The smallest target (3.7 deg on a side) was seen to 'approach' (actually to expand) nearly 8 msec sooner than the medium target and 10 msec sooner than the largest target. A one-way ANOVA reveals that this result is significant at better than the 0.01 level and this is the first result of which we are aware to be consistent with the Leibowitz hypothesis. The reader will note that in all tests, there is actually larger area added to the square for larger starting sizes, so that, on simple target conspicuity grounds, one would expect lower reaction times for larger starting size (28), which is the opposite of what was found.

If Leibowitz' hypothesis is correct, then the mean reaction time for each subject, over all tests, should increase monotonically with initial square size. Thus each set of three reaction times for each test condition should be so ordered. The probability that this would happen by chance is 1/6. The binomial test can then be used to appraise the ability of our data to reject the Leibowitz' hypothesis. The results of such an analysis are shown in Table 2. Here we see that in five of the six cases, the actual number of "successes" (instances in which reaction time increased monotonically with initial square size) was greater than would predicted by chance (Binomial Test: significant at the .02 level).

Nature of Change

Some prior modeling work that has explored time to collision estimates by human observers has assumed that such observers employ width exclusively. We wanted to see if another cue can be used. Of the three cues tested, the best proved to be width, by a small margin. The finding of superior RT for width correlates with our earlier test of detectability of transient size increase [Toyofuku et al (29)] in which width also proved to be a superior cue. The difference, however, is slight. Figure 3 shows average reaction time for width and height at each of the starting sizes employed. Monotonicity of RT with starting size for height is apparent, although a small departure is seen for width.

| Test Subject | # Tests | Actual “Successes” | Predicted Successes | Significance Level |
|--------------|---------|--------------------|---------------------|--------------------|
| A | 25 | 12 | 4.2 | 0.00005 |
| B | 25 | 10 | 4.2 | 0.001 |
| C | 25 | 9 | 4.2 | 0.005 |
| D | 25 | 8 | 4.2 | 0.015 |
| E | 25 | 10 | 4.2 | 0.001 |
| F | 19 | 5 | 3.8 | 0.080 |

Table 2: Nonparametric Test of the Leibowitz Hypothesis

Amount by which Square Expanded

In real viewing situations a fixed range rate leads to a size change proportional to starting size. Thus if one target is twice the size as another and both approach an observer at the same rate, then the absolute angular size change of the larger will be twice that of the smaller. Some of our tests employed this sort of proportional change. But we wanted to see if a fixed change, irrespective of starting size, had the same effect. Figure 4 shows a comparison of RT obtained with proportional and with fixed size changes. There is roughly a 2 msec advantage for the proportional change across starting sizes which is too small to be of consequence.

Lighting Condition

Lighting levels encountered in ordinary driving cover a very wide range. We studied two fairly closely spaced levels within that range in order to see what effect adaptation level would have on RT. Figure 5 displays our results. Daytime conditions allowed an average of 10 msec quicker reaction. But the Leibowitz effect persists. RT increase monotonically with starting size. One can appreciate that this is a conservative test because there is more signal area in the case of larger starting size, and increased target area is known to favorably improve RT (28).

Point of Fixation

One might think that the foregoing results could have been due to the distance from fixation (28) of the active area (the edge of the target). Figure 6 displays the results of this comparison. As in the case of RT as a function of cue, we see a slight departure from strict monotonicity with starting size. We found a very slight advantage in RT for central (as opposed to edge) fixation.

Discussion

When Leibowitz advanced the argument that the large size of the train (compared to other moving objects with which we are familiar) influenced ones estimate of its speed, he did so in the context of collisions at rail crossings. While systematic evidence for or against this hypothesis have not to our knowledge been presented, anecdotal evidence can be found. Typically, this presents in the form of a train engineer’s or bystander’s observation, that the driver of a vehicle that was later struck by the train was observed to look at the oncoming train, then to proceed across the tracks. It should be clear that if this is the problem in microcosm, Leibowitz could as easily have suggested that drivers overestimate their own speed capabilities. Nonetheless he dwelled on an explicit sensory deficiency and the task facing researchers in the field of rail crossing collision mitigation is to find a means of testing the hypothesis.

We have chosen an indirect laboratory test that rests on the implicit underlying concept on which Leibowitz seems to have based his hypothesis. The Leibowitz prediction for our situation would be that an incremental change in angular size would be more poorly seen (detected) in a large object than for a smaller object. Because of the known correlation between detectability and speed of response (28), we would extrapolate the

Leibowitz hypothesis to predict a slower response for a larger target. That is the prediction that our data cannot reject.

It is perhaps premature to consider countermeasures given the minimal understanding thus far achieved. But if Leibowitz is eventually proven to be correct, one might begin to think about ways of altering the look of a large vehicle to counteract the effect of size, an effect that now has more than theoretical underpinning. Leibowitz states: “it would be interesting to determine whether perceived velocity of large objects could be increased...by means of special markings or lights on locomotives.” More data will be needed to be sure that Leibowitz was right, but in the meantime it may be useful to begin to think about the visual attributes that one wants to achieve in order to lessen or eliminate the effect that now bears his name.

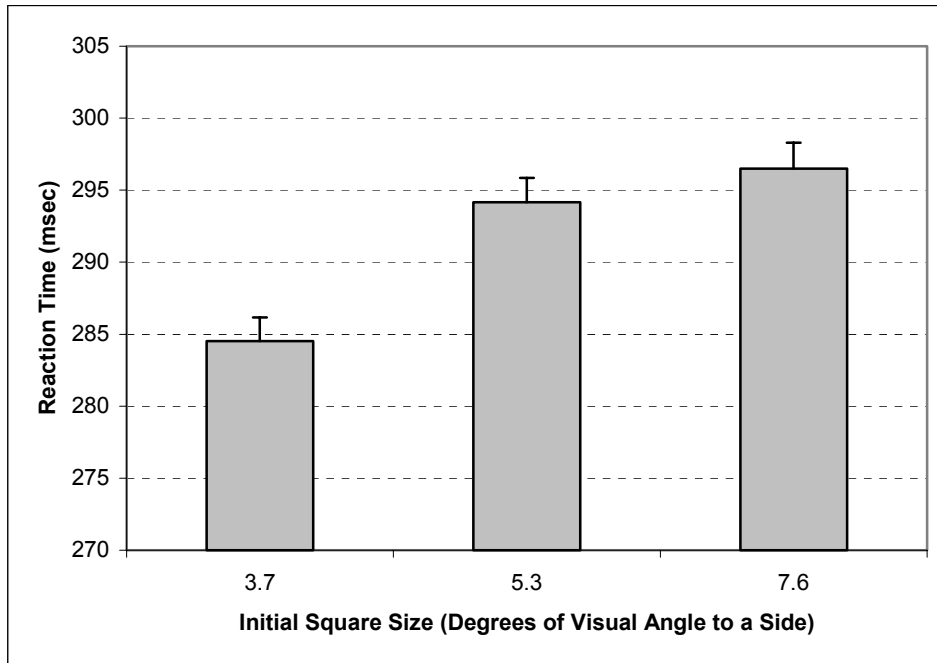


Figure 2: Overall Effect of Initial Square Size

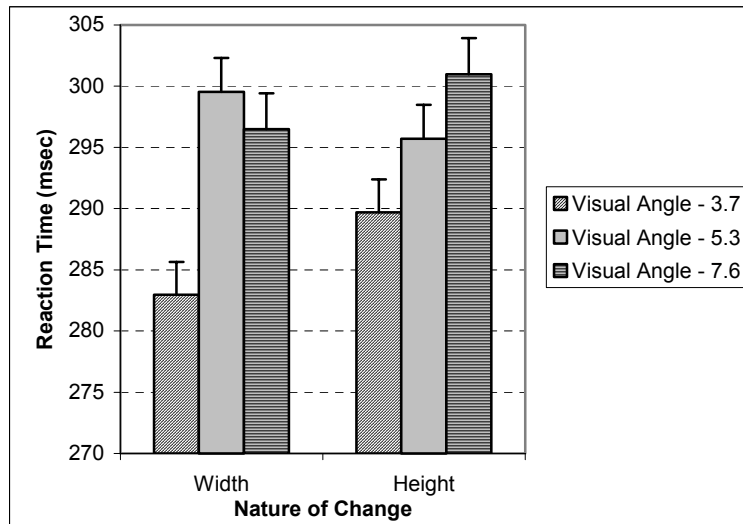


Figure 3: Overall Mean Reaction Time vs. Nature of Change and Initial Square Size

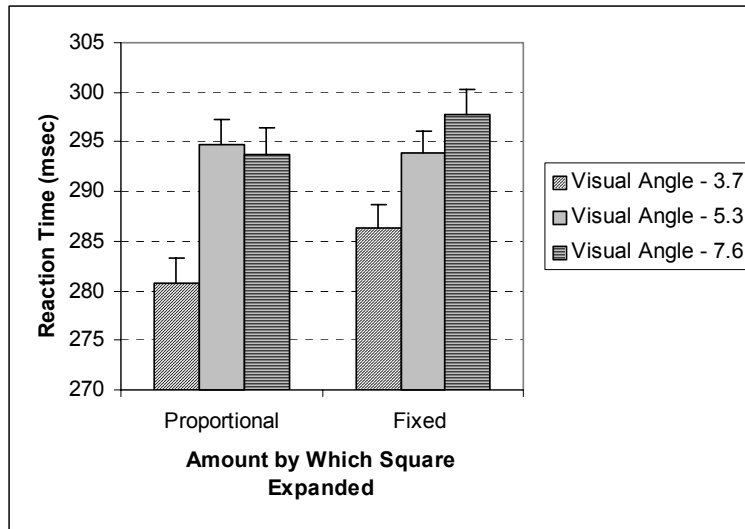


Figure 4: Overall Mean Reaction Time vs. Amount by Which Square Expanded and Initial Square Size

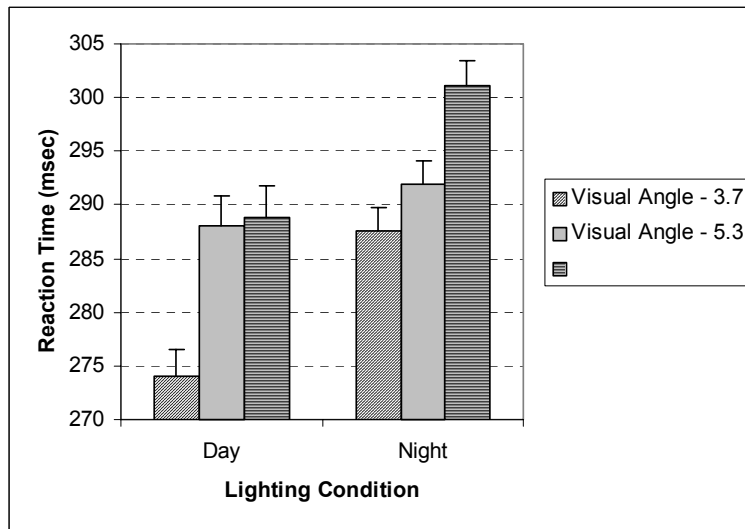


Figure 5: Overall Mean Reaction Time vs. Lighting Condition and Initial Square Size

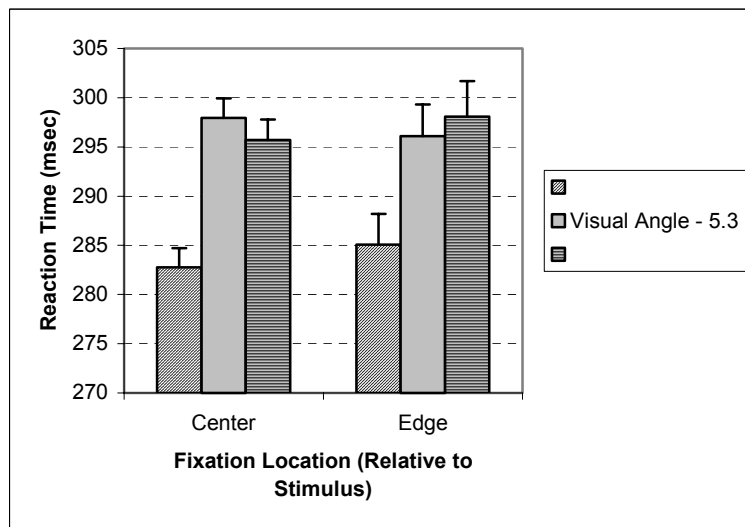


Figure 6: Overall Mean Reaction Time vs. Point of Fixation and Initial Square Size

Experiment 2: A Test of the Threshold Assumption

Introduction

It is generally accepted that humans detect and avoid impending collisions through the perception and regulation of the LV's angular width θ and its rate of change $d\theta/dt$. In their studies, Hoffman (7) and Hoffman and Mortimer (8) assert the existence of a just-noticeable "threshold" in the perception of $d\theta/dt$, of approximately 0.003 rad/sec. In this study we conducted two experiments to test the existence of a threshold for detecting $d\theta/dt$. These experiments are based on the methods of Tanner and Swets (30), who investigated the existence of a "classical" threshold associated with the detection of light signals. (As we explain later, all visual detection tasks ultimately reduce to this.) The implications that these results have for human driver models are far reaching: they suggest the need for a fundamentally different kind of model whose output lends insight not only into the manner in which humans perform this driving task, but also the ways in which their performance can go awry.

Experiment 2a: Methodology and Procedure

Three college students, all in their early 20's and having (corrected) normal vision, participated (They are identified as TS1, TS2, and TS3 in the experimental results tables that follow.) The stimuli were generated by a personal computer and consisted of four white squares presented sequentially against a gray background, as shown in Figure 7. Each of the squares was randomly displaced vertically and horizontally from the center of the screen by a small amount (exaggerated in the figure) to prevent the observer from establishing any fixed points of reference. The time increment during which each square was presented was the same: 500 msec. One of the squares, chosen at random, expanded by a small amount on all sides. This expansion took place 200 msec after the presentation of the square, and occurred over a single screen refresh cycle (1/72 or 1/75 sec).

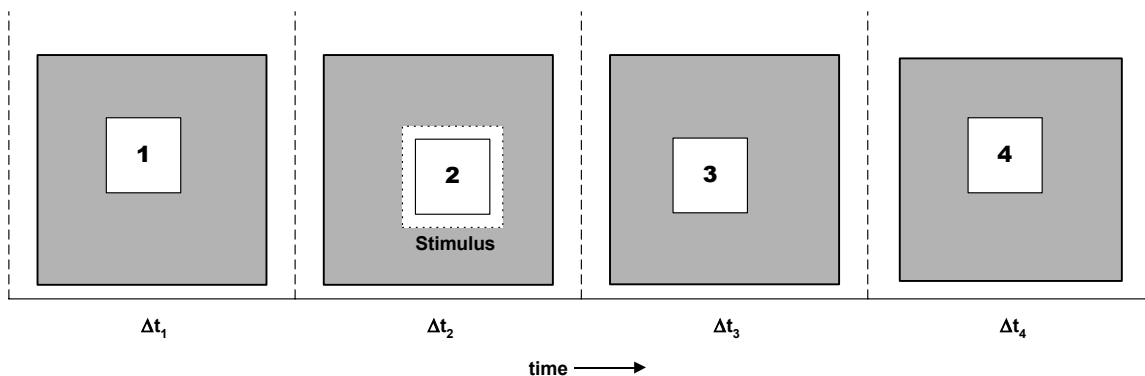


Figure: Stimulus Presentation

(The expanded square thus remained visible for the last 300 msec of the 500 msec time interval.) Test subjects were instructed to fix their gaze on the number at the center of each square as it appeared. The initial size of the squares, the amount by which it increased, and the distance between the test subject and the display were determined in pilot tests beforehand to achieve approximately a 75% detection rate on the first choice.

Each observer viewed the display in a darkened room, and took approximately 75 practice trials to become familiar with the stimulus display and test regimen. Five to eight 300-600 trial tests were then conducted over a three day period. At the end of each trial the test subject was given two chances via prompts to identify the square that had expanded via the keyboard. A tone sounded to indicate an incorrect choice. (There was no time limit placed on the subjects' response, but they typically responded within a couple of seconds of each prompt.) Test subjects were paid 4¢ for each correct first choice, and 2¢ for each correct second choice (when the first choice was incorrect). This ensured that they would choose correctly on the first choice if they could, but also encouraged them to try their best on their second choice in the event that the first was incorrect. At these detection rates and payouts the test subjects could earn approximately \$10 per 300 trial test. An experimenter

was present at all times to monitor the test. The correct choice and the responses for each trial were logged into a data file for later processing.

Experiment 2a: Results

We measured the ability to identify which of the four squares expanded. Performance was about 75% correct on the first choice. A second choice was also recorded. The results of these experiments are summarized in Table 3. The individual trials making up a given test constitute Bernoulli Trials, for which the Proportion Correct and the Standard Error are given by

$$P_i = \frac{n_{ci}}{n_i}, \quad S_i = \sqrt{\frac{P_i(1-P_i)}{n_i}},$$

where $i=1,2$ (see Table 3). [For a review of all the statistical methods employed in this study, see Spiegel, et al (31).] If it is true that our test subjects perform no better than chance ($\frac{1}{3}$) on their second choice, then in a series of tests like the ones conducted here their second choice performance should be above chance about half the time and at or below chance the other half (assuming that the probability distribution the second choice results actually correspond to has a median of $\frac{1}{3}$). The 14 tests performed here then constitute another set of Bernoulli Trials in which

| # | Test | 1 st Choice | | | | 2 nd Choice (When 1 st Choice Incorrect) | | | | r |
|----|---------|------------------------|-----------------------|-------|--------------------|--|----------|-------|-------|---------|
| | | # Trials n_1 | # Correct n_{c1} | P_1 | Std Error S_1 | $n_2=n_1-n_{c1}$ | n_{c2} | P_2 | S_2 | |
| 1 | TS1-001 | 300 | 220 | 0.733 | 0.026 | 80 | 33 | 0.413 | 0.055 | -0.0879 |
| 2 | TS1-003 | 300 | 188 | 0.627 | 0.028 | 112 | 45 | 0.402 | 0.046 | +0.2804 |
| 3 | TS1-005 | 300 | 225 | 0.750 | 0.025 | 75 | 26 | 0.347 | 0.055 | -0.3321 |
| 4 | TS1-006 | 300 | 270 | 0.900 | 0.017 | 30 | 17 | 0.567 | 0.091 | -0.1040 |
| 5 | TS1-007 | 600 | 492 | 0.820 | 0.016 | 108 | 58 | 0.537 | 0.048 | +0.0609 |
| 6 | TS2-001 | 300 | 217 | 0.723 | 0.026 | 83 | 27 | 0.325 | 0.051 | +0.0815 |
| 7 | TS2-002 | 300 | 207 | 0.690 | 0.027 | 93 | 27 | 0.290 | 0.047 | -0.2859 |
| 8 | TS2-004 | 300 | 213 | 0.710 | 0.026 | 87 | 44 | 0.506 | 0.054 | +0.1769 |
| 9 | TS2-005 | 300 | 201 | 0.670 | 0.027 | 99 | 45 | 0.455 | 0.050 | +0.3926 |
| 10 | TS2-006 | 300 | 265 | 0.883 | 0.019 | 35 | 14 | 0.400 | 0.083 | -0.5551 |
| 11 | TS2-007 | 300 | 253 | 0.843 | 0.021 | 47 | 20 | 0.426 | 0.072 | +0.0759 |
| 12 | TS3-002 | 300 | 208 | 0.693 | 0.027 | 92 | 41 | 0.446 | 0.052 | +0.5095 |
| 13 | TS3-003 | 300 | 255 | 0.850 | 0.021 | 45 | 25 | 0.556 | 0.074 | -0.1996 |
| 14 | TS3-007 | 600 | 420 | 0.700 | 0.019 | 180 | 81 | 0.450 | 0.037 | +0.0843 |

Table 3: Experiment 2a Results

$$\text{Probability}\{\text{Success}\} = \text{Probability}\left\{P_2 > \frac{1}{3}\right\} = \frac{1}{2},$$

$$\text{Probability}\{\text{Failure}\} = \text{Probability}\left\{P_2 \leq \frac{1}{3}\right\} = \frac{1}{2}.$$

By this reasoning, then, we would expect only 7 of the 14 tests to yield correct response rates in excess of $\frac{1}{3}$. We find, however, that 12 of the tests did. Since the underlying probability distribution for Bernoulli Trials is

the Binomial Distribution, the probability that we could encounter this result if indeed the true probability of success is $\frac{1}{2}$ is given by

$$\text{Probability}\{x \geq 12\} = \sum_{x=12}^n \frac{n!}{x!(n-x)!} p^x (1-p)^{n-x} = .0065.$$

where $n=14$ and $p=\frac{1}{2}$. Since this probability is less than 1%, we can reject with 99% confidence that these subjects are able to do no better than chance on their second choice in favor of the alternative, that their performance is better than chance.

One possible explanation for the results of Experiment 1 is that test subjects, being aware of the experimenter's interest in the 2nd choice detection rate, "saved" responses for some trials that they were sure of for the 2nd choice. This would inflate the 2nd choice detection rate but at the same time reduce the 1st choice detection rate, causing the two to be negatively correlated. To investigate this, the data for each test was subdivided into 10 non-overlapping 30-trial "windows", and p_1 and p_2 calculated for each. The first window thus contains trials 1-30, the second window trials 31-60, and so on to the tenth window, which contains trials 271-300. The correlation coefficient r measures the correlation between p_1 and p_2 , and is given by,

$$r = \frac{S_{p_1 p_2}}{S_{p_1} S_{p_2}},$$

where the covariance and variance terms are given by

$$S_{p_1 p_2} = \frac{\sum_{i=1}^{n_w} (p_{1_i} - \bar{p}_1)(p_{2_i} - \bar{p}_2)}{n_w}, \quad S_{p_1}^2 = \frac{\sum_{i=1}^{n_w} (p_{1_i} - \bar{p}_1)^2}{n_w}, \quad S_{p_2}^2 = \frac{\sum_{i=1}^{n_w} (p_{2_i} - \bar{p}_2)^2}{n_w},$$

and where n_w is the number of trial windows and \bar{p}_1 and \bar{p}_2 the overall detection rates of the two series. The far right column of Table 3 gives the correlation coefficient for all of the tests. We can again construct a test of the hypothesis that there is no correlation between p_1 and p_2 . Here, then, we would expect half the computed values of r to be non-negative and the other half to be negative. According to Table 3 this is almost exactly the result that we observe, leading us to conclude that test subjects were not saving correct responses for their second choice. Apparently, contrary to threshold theory, observers can do better than chance if given a second choice.

Experiment 2b: Introduction

In their investigation, Tanner and Swets hypothesized that the detection problem had nothing to do with even a varying threshold, but instead was one of distinguishing between two signals in the presence of noise. In their experiments the first signal consisted of a uniform light background of intensity I , and the second consisted of a circular target of intensity ΔI superimposed on this background. The noise to which they refer can arise from many sources: it is intrinsic to all visual stimuli, and "neurological noise" is present in every element of the visual nervous system. An efficient way in which to treat this noise is to aggregate it together from all its various sources and associate it with the visual stimuli, as indicated schematically in Figure 8a. A common assumption in this regard is that this "equivalent" noise is normally distributed and additive, so that both distributions have the same variance. Introduction of the target of strength $I + \Delta I$ thus has the effect of shifting the background distribution to the right by ΔI . The observer distinguishes between the two signals by selecting a criterion level, C , and assuming that neural activity corresponding to signal strengths less than C indicate background alone, while neural activity corresponding to signal strengths greater than C indicate background plus target (assuming that neural activity is a monotonically increasing function of signal strength). In so doing, the observer accepts that only a fraction of all the targets presented will be correctly identified (a "hit"), and that some background-only stimuli will be mistakenly included as well (a "false alarm"). This is indicated in Figure 8b. The actual level at which the observer sets the criterion will depend both on the benefit derived from a correct assessment of the signal and the cost incurred from an incorrect one. The observer can thus vary C (and be influenced to vary C), and in so doing obtain different proportions $P_{\text{Hit}}(C)$ and $P_{\text{FA}}(C)$. I and ΔI are under the

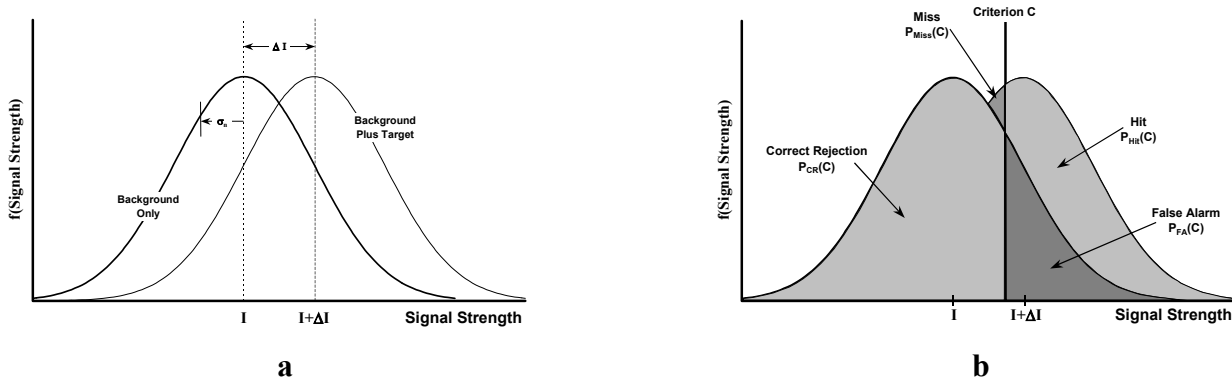


Figure 8: a) Presentation of Visual Stimuli in the Presence of Noise, b) Outcomes of the Detection Task

control of the experimenter and thus known. If in addition the variance σ_s can be determined, then it is an easy matter to compute $P_{FA}(C)$ and $P_{Hit}(C)$ for different C (while holding I and ΔI fixed). Plotting them against one another gives a single *Receiver Operating Characteristic (ROC)* curve, as shown in Figure 9a, thus allowing us to predict in advance the observer's performance for any C . (Repeating this calculation for different ΔI , leaving I constant, would yield a family of such curves.) Incorrect choices are thus not guesses made in the absence of any information, but rather the result of an informed use of existing (noisy) sensory information, in response to

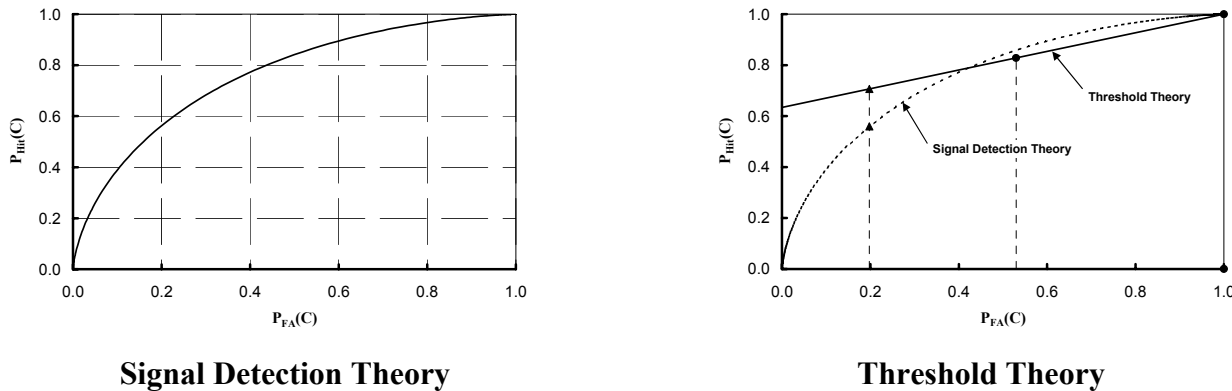


Figure 9: ROC Curves

a particular cost/benefit tradeoff. An easy way to influence this tradeoff, and hence the observer's selection of the criterion level, is to change the costs and benefits associated with false alarms and hits.

Threshold theory specifies a different relationship between P_{FA} and P_{Hit} [Green, Swets (32)], given by

$$P_{Hit} = P_{Hit}^t + P_{FA} (1 - P_{Hit}^t).$$

P_{Hit} here is the observed hit rate. P_{Hit}^t is the true hit rate, which depends only on I and ΔI (which are under the experimenter's control). And P_{FA} is the observed false alarm rate. Since P_{Hit}^t remains fixed for a given I and ΔI , the relationship between P_{FA} and P_{Hit} is linear. Experimentally obtaining one pair of points (P_{FA} , P_{Hit}) and observing that $P_{Hit}(P_{FA} = 1) = 1$ then allows us to plot this relationship. This is shown in Figure 9b, along with the previously derived ROC curve for comparison. [We note too that by extrapolating this plot to the abscissa, we can obtain P_{Hit}^t , since $P_{Hit}(P_{FA} = 0) = P_{Hit}^t$]. These considerations lead us to another way to test the threshold

theory: Perform the “rating scale” version [Green and Swets (32), described below] of the first experiment to obtain two pairs of points (P_{FA} , P_{Hit}). Construct a straight line passing through the rightmost pair and (1, 1), and plot it as in the Figure 9b (filled circles). If the leftmost pair (the filled triangle) falls on this line, it would lend support to the threshold theory. If it falls significantly below this line, however, it would contradict this theory and lend support to the signal detection theory. This was pursued in our second set of experiments.

Experiment 2b: Methodology and Procedure

The same subjects participated in this experiment as the first. The stimulus was generated by a personal computer and consisted of a single white square presented in the center of the display monitor, against a gray background. The square, which appeared for a total of 150 msec, expanded by a small amount on all sides or not, in a random fashion. If it expanded, it did so 75 msec after it first appeared, and the expansion took place over a single screen refresh cycle (1/72 sec), giving an angular expansion of approximately 0.05 rad/sec. The expanded square then remained visible for the last 75 msec of its presentation. Test subjects were instructed to fix their gaze at the center of the square as it appeared. At the end of each trial the test subject was asked to indicate whether or not the square had expanded in one of three ways:

- *Certain*
- *Possibly*
- *Certain It Didn't*

A tone sounded if the subject incorrectly chose *Certain* or *Certain It Didn't*. No tone sounded for *Possibly*. The subjects were paid +4¢ for a correct choice, +2¢ when they indicated *Possibly*, and penalized -3¢ for an incorrect choice.

The inclusion of the *Possibly* category is an efficient way to obtain two pairs of points (P_{FA} , P_{Hit}) in a single test. The *Possibly* responses represent those trials that the test subject was most ambivalent about. In the absence of this choice, he would have recorded these trials as either *Certain* or *Certain It Didn't*, depending upon the relative magnitudes of the reward and penalty offered. To see how two sets of points can be derived from this data, we first list in Table 4 all the possible stimulus-response outcomes for a trial:

| Stimulus | Response | Outcome |
|--|----------------|------------------------|
| Signal (Square Expanded) | Certain | Hit |
| Signal | Possibly | MaybeHit |
| Signal | Certain Didn't | Miss |
| NoSignal (Square Didn't Expand) | Certain | FalseAlarm |
| NoSignal | Possibly | MaybeFalseAlarm |
| NoSignal | Certain Didn't | CorrectReject |

Table 4: Possible Outcomes of Experiment 2b

The two pairs of data points are then given by

$$P_{Hit,1} = \frac{\# \text{ Hit}}{\# \text{ Signal}},$$

$$P_{Hit,2} = \frac{\# \text{ Hit} + \# \text{ MaybeHit}}{\# \text{ Signal}},$$

$$P_{FA,1} = \frac{\# \text{ FalseAlarm}}{\# \text{ NoSignal}},$$

$$P_{FA,2} = \frac{\# \text{ FalseAlarm} + \# \text{ MaybeFalseAlarm}}{\# \text{ NoSignal}}.$$

Furthermore, because all of the data is derived from a single sequence of test trials, this scheme has the added benefit of ensuring that the same psychophysical parameters (e.g., motivation, fatigue, and external conditions) apply to each of the computed pairs of points.

In order for this scheme to provide the desired information, the various test parameters and payments/rewards must be set properly to influence the appropriate utilization of the *Possibly Expanded* category. For this reason the test parameters cited above (the presentation times, initial size of squares, amount by which the square expanded, and the distance between the test subject and the display) and payments/penalties were adjusted from the stated nominal values to provide an adequate mix of responses for each test. This adjustment had to be performed at the beginning of each test because the performance of the test subjects could vary from test to test. (No adjustments were made during the course of a test.) The procedure was very similar to that of Experiment 1. After approximately 75 practice trials to become familiar with the stimulus display and test regimen, each test subject completed three to five 300 - 600 trial tests.

Experiment 2b: Results

We measured the ability to detect a randomly expanding square according to a three-category rating scale. The results of these experiments are summarized in Table 5. Figure 10 shows representative ROC curves calculated from the data for tests TS1-011 and TS2-014. The far right column indicates whether the pair $(P_{FA,1}, P_{Hit,1})$ (the filled triangle in Figure 9b) fell on/above (+) or below (-) the straight line constructed through $(P_{FA,2}, P_{Hit,2})$ and $(1, 1)$. Assuming that the threshold theory holds, the pair $(P_{FA,1}, P_{Hit,1})$ should be collinear with $(P_{FA,2}, P_{Hit,2})$ and $(1, 1)$. For these tests, then, we would expect $(P_{FA,1}, P_{Hit,1})$ to plot on or above this straight line half the time and below it the other half. Thus, we can again treat these tests as a set of Bernoulli Trials for which

| # | Test | # Trials | $P_{FA,1}^o$ | $P_{Hit,1}^o$ | $P_{FA,2}^o$ | $P_{Hit,2}^o$ | On/Above (+) Below (-) |
|----|---------|----------|--------------|---------------|--------------|---------------|---------------------------|
| 1 | TS1-009 | 300 | 7.43% | 69.08% | 16.89% | 80.92% | - |
| 2 | TS1-010 | 300 | 12.08% | 78.15% | 24.16% | 86.09% | - |
| 3 | TS1-011 | 300 | 20.27% | 58.55% | 29.73% | 70.39% | - |
| 4 | TS1-012 | 300 | 17.93% | 45.16% | 53.79% | 74.84% | - |
| 5 | TS2-011 | 300 | 3.52% | 79.11% | 11.97% | 86.71% | - |
| 6 | TS2-012 | 300 | 2.07% | 79.35% | 9.66% | 87.10% | - |
| 7 | TS2-013 | 600 | 10.25% | 72.66% | 18.94% | 77.34% | - |
| 8 | TS2-014 | 600 | 18.71% | 59.31% | 42.58% | 75.17% | - |
| 9 | TS2-015 | 400 | 11.52% | 73.68% | 29.32% | 83.25% | - |
| 10 | TS3-013 | 600 | 15.11% | 46.84% | 37.16% | 63.57% | - |
| 11 | TS3-014 | 600 | 17.28% | 60.87% | 39.20% | 71.24% | + |
| 12 | TS3-015 | 300 | 20.78% | 77.40% | 36.36% | 86.99% | - |

Table 5: Experiment 2b Results

$$\text{Probability}\{\text{Success}\} = \text{Probability}\left\{\left(P_{FA,1}, P_{Hit,1}\right) \text{ lies on or above the line}\right\} = \frac{1}{2},$$

$$\text{Probability}\{\text{Failure}\} = \text{Probability}\left\{\left(P_{FA,1}, P_{Hit,1}\right) \text{ lies below the line}\right\} = \frac{1}{2}.$$

According to Table 5, we encountered only one “success” among the 12 tests (TS3-014). Since the probability of getting 11 (or more) failures when the true probability of success is $\frac{1}{2}$ is

$$\text{Probability}\{x \geq 11\} = \sum_{x=11}^{12} \frac{12}{x!(12-x)!} (.5)^x (1-.5)^{12-x} = .0032,$$

We can with 99% confidence reject that $(P_{FA,1}, P_{Hit,1})$ is collinear with $(P_{FA,2}, P_{Hit,2})$ and $(1, 1)$, and accept instead that it lies below the line formed by these latter pairs of points. This result suggests that the data is better described by an ROC curve, consistent with a signal detection theory that includes underlying noise but no sensory threshold, than by the linear relationship derived from threshold theory.

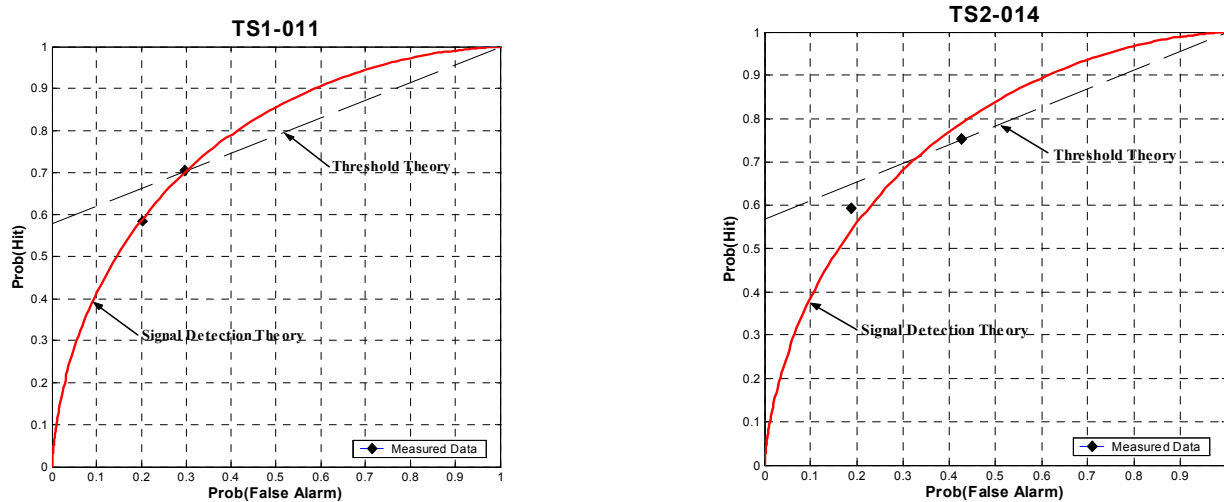


Figure 10: Representative ROC Curves

Discussion and Conclusions

This study tests the assertion of the existence of a threshold associated with the detection of expansion rate, $d\theta/dt$, and provides evidence contradicting it. Hoffman and Mortimer (8) asserted that the just-noticeable expansion rate is 0.003 rad/sec. This is a statement of static threshold theory. According to it rates above the threshold will always be detected. Below threshold, however, the observer sees nothing and resorts to guessing. “Threshold” thus takes the form of the step function presented in Figure 11. The expansion rates used in these experiments ($0.038 \leq \Delta\theta/\Delta t \leq 0.102$ rad/sec) are over an order of magnitude greater than Hoffman and Mortimer’s threshold and yet correspond to an approximately 75% detection rate. This highlights the problem for human driver models that incorporate the absolute threshold assumption. Though at expansion rates less than 0.003 rad/sec detection levels may very well be near zero, human drivers will continue to err significantly in detecting expansion rates quite a bit larger than 0.003 rad/sec. The models, however, will fail to pick this up, and will instead predict perfect detection at these higher levels.

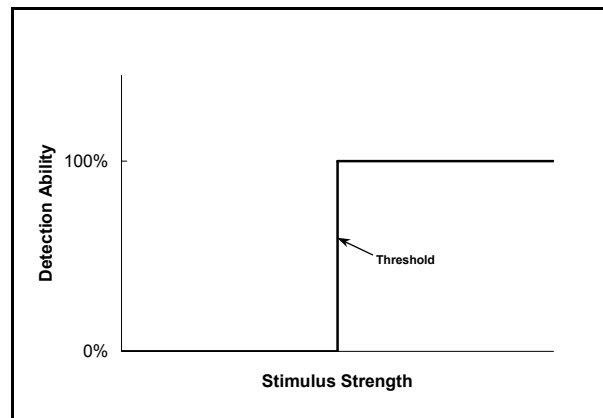


Figure 11: Static Threshold Detection Curve

We believe the experimental evidence is more convincingly accommodated by Tanner and Swets' signal detection model. In this view the observer distinguishes between two signals ($d\theta/dt=0$ vs. $d\theta/dt>0$) in the presence of noise (Figure 8), with reference to a criterion level that has been established to reflect to costs of incorrect assessments and the benefits of correct ones. The detection task thus does not involve guessing, but instead involves an informed tradeoff between hits and false alarms. The ROC curve (Figures 9 and 10) quantifies this tradeoff for particular values of expansion rate, noise, cost and benefit.

In this study we investigated one of the essential tasks of the human visual system—the detection of approaching objects—for the purpose of accurately describing and quantifying its underlying function. Our data firmly reject a threshold theory for the detection of angular expansion, but are well fit by a signal/noise model. We envision that these results, combined with those of future studies investigating related aspects of the visual system, will comprise a scientifically grounded model of human vision as it relates to the collision detection and avoidance task.

Experiment 3: Making the Onset of LV Braking More Detectable

Introduction

If the two-dimensional expansion of the LV's image in the FV driver's field of view is a cue to the onset of LV braking, how might we exploit this to make the onset of FV braking more detectable? The problem is of special interest to transit properties. While rear-end collisions account for 23% of all accident types in cars (33) the rate is 36.8% for buses (34), making this the most frequent type of accident for transit buses. It is interesting that most rear-end collisions with buses occur in urban areas, when the bus is stopped, when the road is straight and in the best ambient conditions imaginable (daylight, dry road, clear visibility) (34). For some reason drivers do not see the buses early enough to avoid a later collision. This research was in collaboration with a larger rear-end collision mitigation project sponsored by USDOT-FTA, that focused on finding a way to prevent these types of rear end collisions. The key feature of that project was detection by bus-mounted radar of a threat to the rear of the bus as LV with subsequent visual signaling to the driver of the FV. The visual signal (Figure 12) was an 8 cm x 150 cm structure that consisted of eight identical self-luminous segments, each equipped with a pair of automotive (halogen) incandescent lamps and an amber transmitting lens. It was designed to be mounted on the rear-end of a bus, roughly at the eye level of a following driver. It is herein termed a "light-bar"



Figure 12: Fully Illuminated Light-Bar

owing to its elongated rectangular shape. A brute force approach to improving the detectability of FV braking would be to ignite all of the segments simultaneously when a rear-end collision was deemed imminent. Using this as the standard, we compared this to an alternative design wherein we altered both the ignition sequence and intensity of the individual segments. The endpoint performance measure that we used for the performance comparison was time to react (RT).

Our design took advantage of experience gained in the study of in-vehicle warning signals. We learned that the movement of a visual stimulus is a key element in improving both its visibility and the speed of response that it can engender. Accordingly, we designed an 'optimized' light-bar to have a form of movement. The form that we picked was that of a looming target in view of our previously reported findings. While salience might not affect the reaction time tests of the present study in alerted, prepared observers, (and indeed was not examined at all) it might nonetheless have important utility later, on the road, when viewed by un-alerted, unprepared drivers. The looming quality of the 'optimized' light-bar was achieved by turning it on piecemeal. We arranged to ignite segments 4 and 5 (See Figure 7 for segment numbering scheme) initially. These were followed in order by 3 and 6, 2 and 7, and finally by 1 and 8, as shown in Figure 13. Thus, unlike the standard which was ignited all at once, our design ignited on the light-bar gradually.

The standard was from BuCom, Inc (Wauconda, IL). It used individual lamp modules from Tomar Electronics (Gilbert, AZ. -individual elements were designated as size=RECT-37; style=HNB – halogen spot, and color=amber). We chose to implement the required light control using LEDs instead of halogen (incandescent) lamps. That choice, in turn, added complexity to the testing. It would have been a poor experimental design to pit the Bucom/Tomar standard against the LED prototype because if differences had been found one would naturally have questioned whether any of the physical differences (slight size difference, spatial pattern of light intensity, color) might have been responsible.

Accordingly we developed a pair of tests: the first compared an LED-based standard against the 'optimal' light-bar. The second explored the influence of the sluggish turn-on time of incandescent lamps as simulated within the 'optimal' light-bar. The advantage, if any, that owes to the optimal design turn-on sequence alone could thus be separately measured. Equally, the advantage, if any, that owes to the use of LEDs alone could

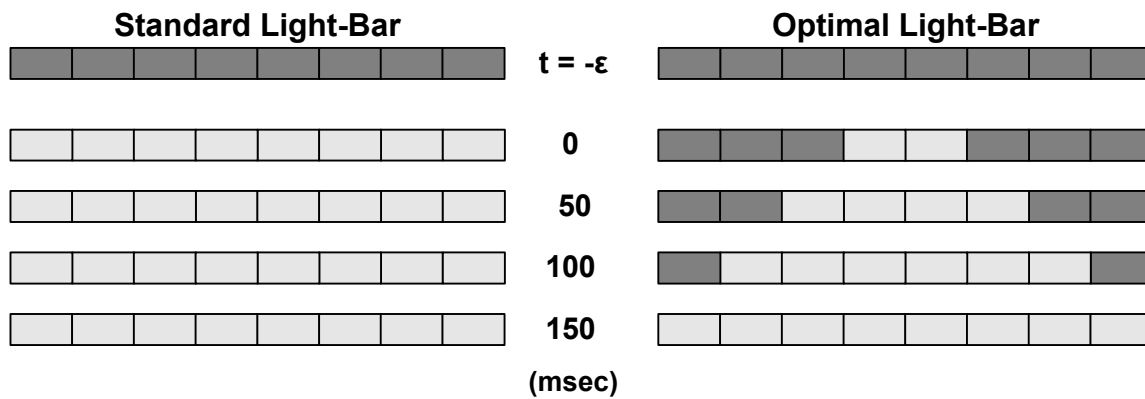


Figure 13: Time course of the turn on sequence for standard and test ('optimal') light-bars. Time proceeds from top to bottom. The appearance of the light-bars is shown schematically both just before turn-on ($t = -\epsilon$) and at and after turn-on at 50 msec intervals. The standard turns on fully at $t = 0$ and stays on. The test bar turns on piecemeal.

also be separately measured. Both are found as the difference in RT and are thus expressed in units of time and represent respectively, delays imposed by (1) the turn-on sequence of the standard and by (2) the incandescent lamps used for the standard. The sum of the two, also expressed in units of time, is the net average advantage of the proposed 'optimal' design. Other aspects of the experimental design were arranged to be best-case. That is, every effort was made to ensure that observers were alerted, undistracted, and prepared. [Prior studies suggest that worst-case conditions would much more strongly favor the 'optimal' design (35)]. Thus this was a conservative test.

Methodology and Procedure

We performed a reaction time test where subjects were told that we were testing the ability of human observers to see a warning signal and that our interest lay especially in how fast they could report seeing it. They were told that their job was to detect and report the onset of a light-bar signal as quickly as possible. They were told that ignition could occur, at random, anytime within a 15 second interval after the bus-bar was reset (turned-off from the prior trial). (The probability of ignition at a given time was a declining function of time to minimize anticipation effects). Fixation was controlled by instructions.

The subjects were instructed to push a microswitch button on a hand-held console as quickly as possible upon seeing the signal and were further advised to not push the button in the absence of a signal. Reaction time was quantified using a digital timer (Hewlett Packard 5315A Universal Timer) gated to the light-bar onset and to the subject's button push. When, on occasion, the subject 'missed' the response button (by withdrawing finger pressure too soon) he was instructed to inform the experimenter and the trial was voided. The subjects were also instructed that the testing procedures would last for 50 trials (first 25 trials—test mode; second 25 trials—standard mode). Testing the standard mode second lends a conservative bias to the method. Artes et al (36) have shown a 12 msec average speed-up in time to react to a near threshold target for trials run in a second test some 4 minutes after a first test.

The test parameters consisted of near (4.6m) and far (45.7m)³ viewing conditions, day vs. night illumination conditions, 100% light-bar intensity (1600 cd/m²) and 5% intensity (80 cd/m²), and specified fixation above the light-bar or free fixation. A separate test was employed to estimate the effect of the near instantaneous LED turn-on time vs. incandescent turn-on time. This test differed substantially from an earlier one reported by Sivak et al (37) Those authors estimated reaction time for incandescent vs. LED lamps. The latter proved to yield

³ The 45.7 m viewing distance was simulated by using a pair of x10 binoculars with inverted viewing. These resulted in a negligible light attenuation.

166 msec faster reaction times on average. But inevitable differences in color, spatial distribution of light, and physical characteristics cannot be ruled out as the cause of some or all of the difference. In our test we controlled for such extraneous factors. The LED-populated bar was given the same gradual intensity rise as the incandescent lamps were measured to have. This was done through the use of time-varying pulse width modulation in the driving circuitry. In that test we set aside the roughly 40 msec dead-time of the incandescent lamps (as measured by a triggered Photo Research Spectra Pritchard Photometer) and instead started the LED-simulated incandescent at the point in time when intensity above ambient could first be measured. Hence, the 40 msec dead-time will appear in later discussion as an additional factor weighing in favor of the optimized light-bar. Table 6 summarizes the tests that were conducted. Subjects included six young adults with normal (corrected) vision plus three older adults (≥ 60 years). Protocols were approved by the Committee for the Protection of Human Subjects, the institution's IRB.

| Test | Light-Bar | Light-Bar Illumination | Viewing Conditions | Viewing Distance | Other |
|-------|----------------------------|------------------------|--------------------|------------------|--------------------|
| 1, 2 | Optimal, Standard | Full Intensity | Day | 4.6m and 45.7m | |
| 3, 4 | Optimal, Standard | 5% Intensity | Day | 4.6m and 45.7m | |
| 5, 6 | Optimal, Standard | 5% Intensity | Night | 4.6m and 45.7m | |
| 7, 8 | Optimal, Standard | 5% Intensity | Day | 4.6m and 45.7m | Free Fixation |
| 9, 10 | LED, Incandescent Settings | Full Intensity | Day | 4.6m and 45.7m | 250ms Turn-on Time |

Table 6: Summary of Tests Conducted

Results

Figure 14 shows the distributions of reaction times for both light-bars seen under daylight conditions at a

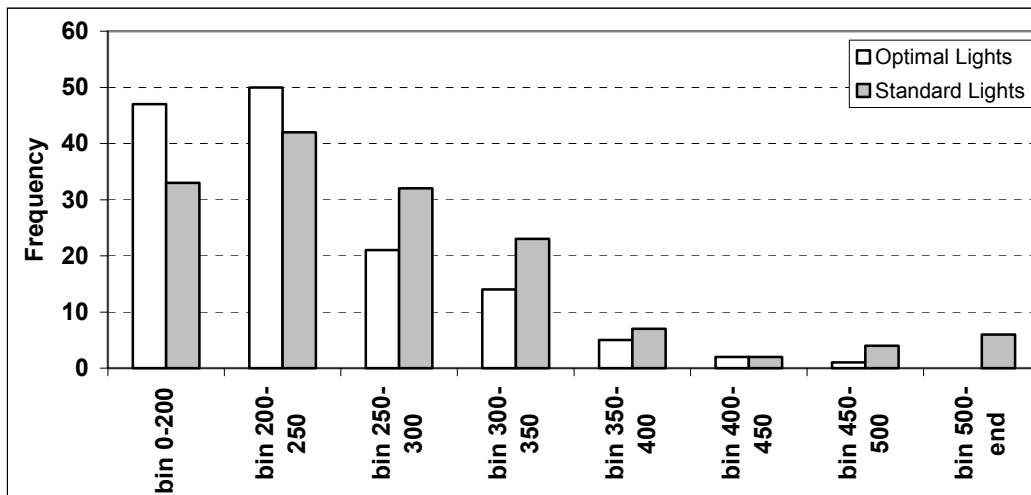


Figure 14: Reaction time distributions for standard and ‘optimized’ light-bars. Ordinate: raw frequency. Abscissa: reaction time bins in msec. Data for nine observers. 4.6 m viewing distance, day condition, 100% intensity, and fixation constrained to lie just above the light-bar. White bars: ‘optimized’ light-bar. Gray bars: standard light-bar.

distance of 4.6 m with fixation constrained to lie just above the light-bar. Data in each test were similar to those shown here, so that only summary data for the other tests are provided. The average RT for the standard light-

bar was 32.6 msec greater than that for the optimized light-bar. This general finding persisted across conditions of testing (day vs. night; full intensity or 5% intensity, old and young eyes, and fixation condition), as will be seen in Table 7. The turn-on sequence, which involved the delay of the ignition of some elements, was seen faster, across conditions, by a weighted average of 26.6 msec (minimum average difference was 10.1 msec and maximum was 54.3 msec) at the close distance. Variance of response was likewise consistently and significantly greater for the standard light-bar at the close distance. At the longer distance, the optimized light-

| Condition | Optimal | | | Standard | |
|-----------------------------|---------------------|----------|---------|----------|--------|
| | Trials ^a | Mean | SD | Mean | SD |
| 100%-Day@4.6m | 150 | 237.0*** | 60.7*** | 269.6 | 89.1 |
| 100%-Day @ 45.7m | 150 | 277.9*** | 68.3** | 302.9 | 85.1 |
| 5% -Day@4.6m | 225 | 282.9n | 79.3* | 293.0 | 93.6 |
| 5% - Day @45.7 m | 225 | 311.7n | 74.3n | 322.3 | 82.7 |
| 5% -Night@4.6m | 225 | 287.7*** | 78.1*** | 317.6 | 106.2 |
| 5% - Night @45.7m | 225 | 330.3n | 78.9n | 334.1 | 72.3 |
| 5% Day-Free Fixation @4.6m | 75 | 322.8*** | 68.3** | 377.1 | 85.1 |
| 5% Day-Free Fixation @45.7m | 75 | 401.9* | 128.7 | 442.4 | 170.9* |

Notes: ^a Trials at each condition at the rate of 25 per light-bar per observer; Statistical tests: means - t-test unequal variance and 1 tail; variance - F-test and one tail.
*** p < 0.001; ** p < 0.01; *p < 0.03; n - not significant

Table 7: Summary of Reaction Time Mean and Standard Deviation (msec) by Condition

bar showed a smaller average advantage (14.8 msec) with a hint of increased variance for the standard bar except for the free fixation condition wherein the variance difference was significant but in the opposite direction. Even in that condition, however, a plurality of instances of low reaction times for the optimized light-bar and much higher RTs for the standard light-bar suggest that the optimized bar will offer a substantial statistical advantage. The average difference obscures a more significant advantage of the ‘optimal’ light-bar. In 10 – 20% of instances, the optimal is seen more than 100 msec faster than the standard. In other words, the 26 msec advantage is not across the board and does not reflect a simple translation of the distribution of RTs.

Results for the comparison of gradual (incandescent-like) turn-on compared to the near-instantaneous LED turn-on were qualitatively as expected. Representative findings are shown in Figure 15. The rapid turn-on characteristic of LEDs allowed the light-bar to be seen faster by an average of 36 msec in close viewing. To this time difference one must add the 40 msec dead-time. This yields a 76 msec advantage for the LEDs over the incandescent lamps. This result is not as large as the one (166 msec) reported by Sivak et al (37), but is the first, as far as we can determine, that was obtained under conditions in which confounds due to color, light distribution, intensity and physical characteristics were eliminated. The sum of the two different sources of advantage for our ‘optimized’ light-bar, the LED advantage (76 msec), plus the piecemeal turn-on advantage (26.6 msec) gives a 102.6 msec net predicted average advantage for the ‘optimal’ design at a close distance. This result is uncontaminated by factors (shape, color, size, position, surrounding elements) that could in theory affect RT. The results were robust across changes in intensity (full or 5%), ambient lighting conditions (night vs. day), and age (young vs. old observers).

Average RT difference was most pronounced for the condition of free fixation (e.g. observers instructed to allow their gaze to wander around the entire scene. In that condition average RTs were also highest for both light-bars. The optimized bar exhibited 40 – 54 msec faster average RT than the standard in a condition likely to be more representative of normal driving. This latter difference begins to approach a difference (180 msec) that we observed when, in preliminary studies, signals with the same temporal parameters were simulated on a

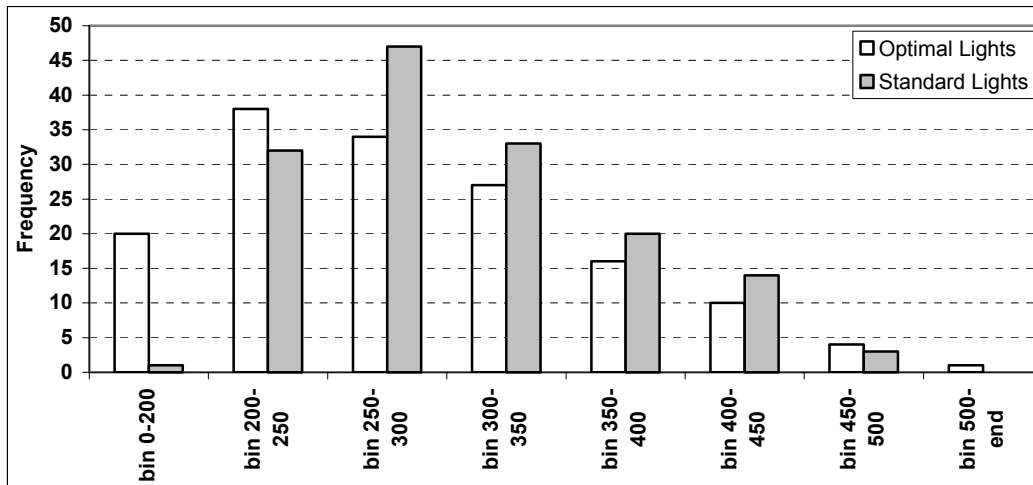


Figure 15: Raw reaction time frequencies for LED and simulated incandescent turn-on characteristics using intensity-modulated LED light-bar. White bars are for the LED device with sudden (< 0.01 msec) turn-on. Gray bars are for an LED light-bar that was constrained to turn-on with a time course that approximated that of the measured incandescent turn-on characteristic but without the 40 msec dead-time.

computer CRT and viewed by six of the same observers employed in the main study. The difference was least in the several distance viewing conditions, a condition for which one expects the signal to have least impact.

Discussion

Readers familiar with the field of rear signaling will understand that this study cannot begin to unravel the very complex issues that devolve from a multiplicity of signals on the rear of transit vehicles. On buses these include running lights, brake lights, turn signals, back-up lamps, license plate lights, emergency warning flashers, and in some cases, ‘decell’ lights. These signals exist in a milieu of other visual fare including advertisements, logos, decorative designs and hardware. Interactions between such signals, and other visual stimuli located nearby, expectations related to them, and the overall visual context of the signals, which could substantially affect signal visibility and meaning [Moore and Rumar (38)], were not explored in this study. In fact the only claim to relevance of our tests is that the light-bars tested were seen against a backdrop with the look (albeit diminished in size by 20%) of the actual bus upon which it might later be mounted. The soundness of the finding rests on the design of the study in which we compare two types of signaling with all other potentially confounding factors held constant, and in which the only endpoint factor under test is the speed with which low level vision is able to detect the onset of our visual signals.

The theory that sequential (in fact, delayed) turn-on of some of the warning signal elements would lead to faster signaling appears to be correct. The theory is based on the supposition that, whether due to its ‘looming’ nature, or simply to the apparent motion inherent in it, the optimized light-bar preferentially stimulates M-pathway neurons in the visual nervous system which are thought to be more sensitive and to signal more rapidly. Despite the delays inherent in waiting for later elements to come on (ranging from 50 – 150 msec), average reaction times were shorter for the ‘optimal’ signal configuration. We had used some preliminary simulation experiments to fix the inter-segment delay at 50 msec and our results showed only little in the way of an advantage for that value. Consequently, it may be possible to gain further improvements by speeding up the turn-on sequence, but this was not studied.

The smallest mean differences occurred at low light levels or at long viewing distance. This may mean that the nature of the signal, its geometry and intensity, need to be adjusted for the prevailing conditions. While the prototype light-bar would have been incapable of such flexibility, there is no reason why, in principle, one could not control the light-bar characteristics based upon the information gathered by the radar monitor that is used to trigger it into action.

An additional advantage is gained from using LED as opposed to incandescent lamps. While this is easily understood in a qualitative way based on physical arguments (the latter turns on gradually as its filament heats up) we believe this to be the first attempt to quantify the perceptual difference with other factors controlled. The LED advantage accrues from two separate factors. The first is a dead time in which the incandescent lamp produces no light at all. While this may vary from lamp to lamp, in our tests it amounted to 40 msec or more. The second LED advantage is due to the gradual turn on of the incandescent once light does start to be emitted from it. We measured the average time that it took for observers to see the simulated incandescent light and this amounted to an additional 36 msec for our observers.

The light-bar that we tested is now a candidate for installation on Ann Arbor Transit Authority (AATA) buses. The prediction is that it should lead to lessened stopping distance (100 msec at 30 mph amounts to 4.4 ft less distance traveled) or to lessened collision speed and consequently lessened damage. While collisions are rare for a given bus, near misses should be more common and more easily observed. It would be expected that near misses, by whatever measure, should be lessened in frequency with the use of a light-bar, and lessened even more using the bar that we have termed 'optimized'.

Conclusion

An 'optimal' rear-end collision warning signal has been measured to supply an average of over 102 msec and, on occasional trials, considerably more than 200 msec faster warning to alerted, prepared human observers with normal vision. This speed-up is gained, paradoxically, by slowing the turn-on characteristic of the light-bar signal.

Experiment 4: The Effects of Texture Dilation

Introduction

The previously described experiments measured the speed and accuracy with which test subjects could detect a suddenly expanding object, based on the outward movement of its edges. This experiment investigates the cue of dilation of texture.

Methodology and Procedure

To isolate the texture expansion cue from the previously explored edge movement cue, we constructed a stimuli consisting of a random grayscale checkerboard with a “window” (0.48 deg visual angle), which kept the edges fixed while allowing the elements of the texture to expand. Three different texture “scales” were used to simulate coarse and fine texture (each square in checkerboard started as either 20x20, 30x30 or 40x40 pixels). Each square in the stimulus expanded one of two ways. In the unaltered and blur conditions, each square expanded by one pixel symmetrically on every side (total of 2 pixel increase in each dimension, a 20x20 square became 22x22 pixels). Vartrim and settrim (see below) increased asymmetrically by a ½ pixel in every side for a total increase of 1 pixel in each dimension (20x20 became 21x21). A one pixel increase was an increase of 0.031 minutes of visual angle or a total displacement of 1.457 min of visual angle for the outermost squares on the texture in the 20x20 case. Sample stimuli are shown in Figure 16.

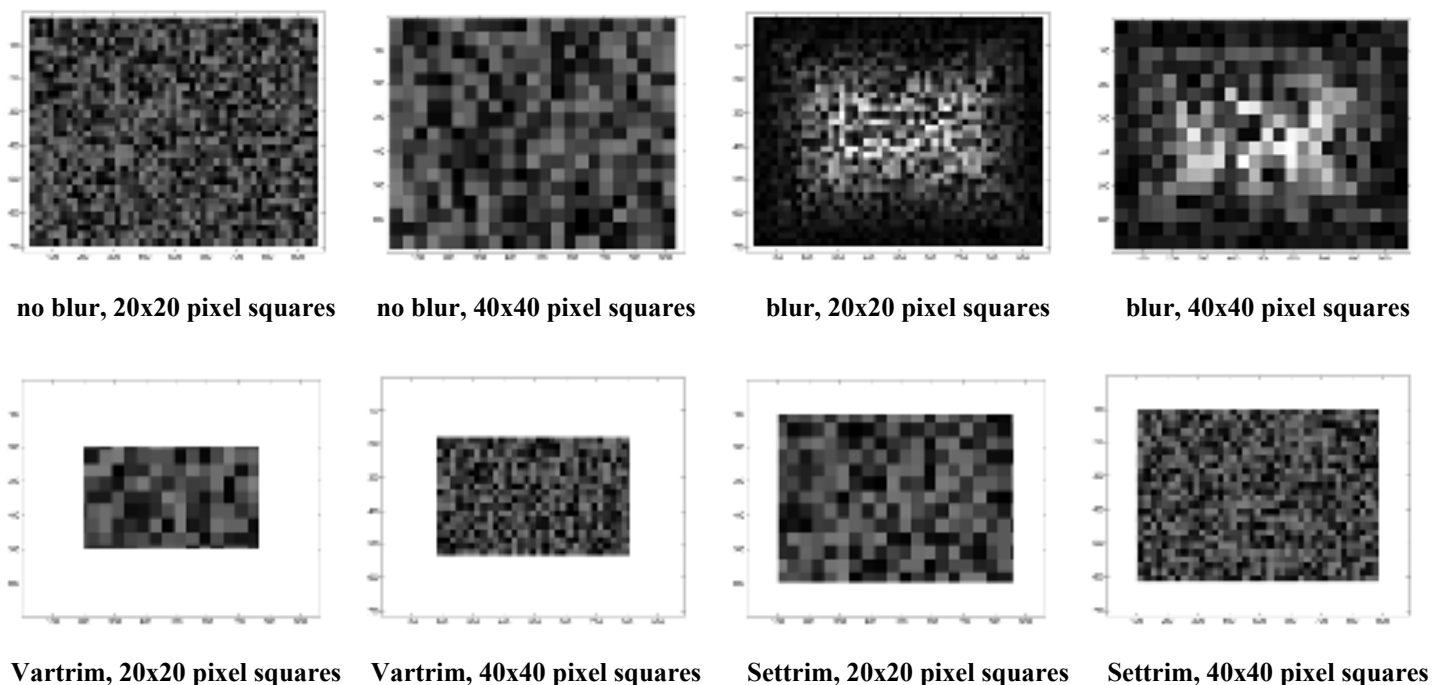


Figure 16: Sample Stimuli

The stimulus was manipulated four different ways. First the edges were contrast reduced linearly to induce a blurring of the edges (called “blur”). This was to further increase the emphasis on the texture only cue by removing any sharp edges that resulted from the windowing of the stimulus. Even though those edges did not move it is possible they were distracting. Trials run on this stimulus showed it to be harder than the unaltered stimulus. To explore whether this was due to the lack of edges or simply a reduction in available information the second condition was a trimming of the edges (called “vartrim”). This left just the part of the stimulus that had been unaltered (or unblurred) in the previous condition. The stimulus was also reduced to an asymmetrically smaller increase to see if that would reduce accuracy. Because this was manipulating two factors at once, a third condition (settrim) was created to compare the original symmetrically increasing stimulus with an asymmetrically increasing stimulus with a similar window size. Because the two error rates

were similar, the reduced accuracy in the vartrim condition is likely due to the reduced window size rather than just the reduced increase.

To stimulate the motion in depth feeling, the stimulus was presented very briefly, for a total of 0.4 sec. Randomly half of the time the stimulus would expand in the second half of the stimulus presentation (0.2 sec), and the subject’s task was to determine whether it had expanded or not. (The first few trial runs made it clear that the previously used 1.5m viewing distance was inadequate. The task was too easy. Viewing distance was increased to 20 m by having the subjects sit 2 m from the computer screen and use inverted x10 binoculars with chinrest.) Each experimental run consisted of 12 trials, and the test subject was alerted of incorrect choices via an auditory tone.

Results

Figure 17 shows the error rate in percent for the detection task, defined as

$$\text{Error Rate} = \frac{\% \text{ Hits} + \% \text{ False Alarms}}{2} \times 100\% .$$

The fine scale unaltered stimulus was the easiest for subjects. The trimmed and blurred conditions seemed the hardest of the four conditions, with the highest error rates. In the edge expansion only experiment, the percent error rates for a 1 pixel symmetrical expansion averaged 37.31% error. In this experiment, percent error calculations for a one pixel expansion detection fell to 14.2 %. Even the ½ pixel asymmetrical expansion was detected with similar error rates (14.9 %). It is possible that these results are due entirely to the additional edge information in the squares of the texture itself. Error rates of the hardest stimulus condition (coarse texture, small window) are similar to those of the edge-only experiment results. This implies that the texture has a certain threshold requirements in scale and size before it becomes useful, so the simple addition of more edges is not the only reason texture is more helpful, otherwise adding any texture edges at all should make the task easier.

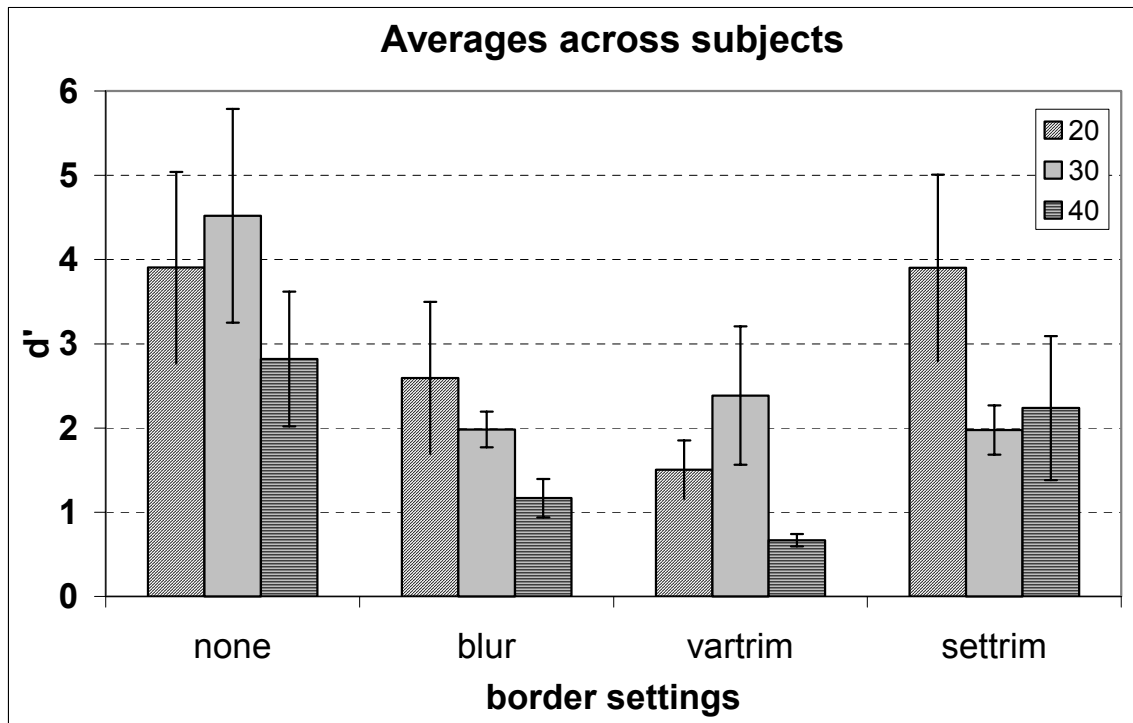


Figure 17: Overall Results – Dilation of Texture Experiments

A one pixel increase was an increase of 0.031 minutes of visual angle or a total displacement of 1.457 min of visual angle for the outermost squares on the texture in the 20x20 none case. In the hardest case, 40x40 coarse scale with the vartrim treatment, the largest displacement was 0.2 min of visual angle (because of the ½ pixel increase instead of 1 full pixel). The previous edge-only expansion used expansions of 0.24, 0.48 and 1.44 min of visual angle. Table 8 compares these situations in terms of percent error. This shows that the increase in

| Type of Expansion | Error |
|-------------------|-------------------------------|
| 20x20 none | 14.2% ± 2.4 percentage points |
| 6 pixel increase | 3.02% ± 0.9 percentage points |
| 40x40 vartrim | 38.8% ± 1.3 percentage points |
| 1 pixel increase | 37.3% ± 6.8 percentage points |

Table 8: Summary of Reaction Time Mean and Standard Deviation (msec) by Condition

efficiency cannot be due to the smaller visual angle subtended by the texture stimulus, or the 20x20 and 6 pixel increase error rates would be reversed or at least similar.

The sensitivity the subjects showed to the texture cue somewhat contradicted the results of Beverley and Regan (22), who concluded that the addition of texture to a stimulus that was originally untextured reduced its effectiveness as a simulation of motion-in-depth unless it was exactly matched. To avoid this affect, they advised precautions to keep the dynamic changes of the texture consistent with the dynamic changes in size. When the texture and size of an object were locked in the manner characteristic of everyday objects, the motion-in-depth system was stimulated effectively. When texture grain remained constant w/ object size increasing, the motion-in-depth system was only weakly stimulated. We did not find that the motion in depth system needed to have consistent edge and texture motion to be stimulated. The percept reported by subjects was of a texture moving in space, viewed through a rigid window.

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