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1314 kinnear road
columbus, ohio
43212

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OPTICAL FLOW AND TEXTURE VARIABLES USEFUL IN SIMULATING SELF MOTION

Dean H. Owen
Department of Psychology

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acceleration, deceleration, and descent. Assessment of individual differences in sensitivity to these optical variables has begun and constraints on degrees of freedom in choosing variables for factorial experimental designs were determined.

Optical analysis of 256 Boeing 747 simulator landings was initiated to explore the applicability of our approach to flight situations and to guide future judgment and interactive experiments. Implications of optical analysis for aviation safety are also reported.

Lastly, a review of performance measurement in research on visual control of flight is presented. The review will guide our development of optical variables and invariants as measures of performance, under the assumption that pilots make control adjusgments in order to control what they perceive.

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THE OHIO STATE UNIVERSITY
COLUMBUS, OH 43210

Dr. Dean H. Owen

Controlling Office: USAF Office of Scientific Research/NL
Bolling Air Force Base, DC 20332

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ABSTRACT

The project is concerned with (1) mathematically isolating optical flow and texture variables as candidates for visual information useful in guiding flight maneuvers and (2) assessing the functional utility of these variables in judgment experiments and in fully interactive simulation environments. The major contribution of the reported year's effort was the development of a technique for holding optical variables invariant throughout self-motion events. The method was used to factorially study fractional rates of change as information for acceleration, deceleration, and loss in altitude. Assessment of individual differences in sensitivity to these optical variables was initiated, and the constraints on degrees of freedom in choosing variables for factorial experimental designs were determined.

Optical analysis of 256 Boeing 747 simulator landings has begun to explore the applicability of our approach to flight situations. Studies of this kind will be used to guide future judgment and interactive experiments. Implications of optical analysis for aviation safety are also reported.

Lastly, a review of performance measurement in research on visual control of flight is presented. The review will guide our development of optical variables and invariants as measures of performance, under the assumption that pilots make control adjustments in order to control what they perceive.

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INTRODUCTION

Overview of Progress

One of the two experiments presented in the previous final report is in print (Owen, Warren, Jensen, Mangold, & Hettinger, 1981), and the other is in press (Owen, Warren, & Mangold). A third paper (Warren & Owen, in press), is presented in Appendix A. It lays out the problems we have encountered in designing experiments on self-motion perception and presents some solutions we have developed. A fourth paper by Owen and Warren on relations between optical variables and mishaps will appear in the proceedings of a conference, and is presented in Appendix E.

The first M.A. thesis on the project was completed by Larry Hettinger (see Appendix B). Noteworthy was the lack of any effect of global optical density over a wide range of variation. The experiment was designed as a preliminary to several studies which will explore candidates for information specifying loss in altitude and compare eye-height-scaled versus ground-texture-scaled metrics for self-motion perception.

We have begun examining individual differences to determine their range and distribution and to assess the extent to which group means are representative. Results to date are presented in Appendix C. A major part of our experimental effort this year was devoted to the comparison of flow-rate and edge-rate determinants of perceived self speed, and individual differences may be one of the most important outcomes of this study. As shown in Appendix D, the influences of fractional increases in flow rate and edge rate are essentially additive when observers are required to distinguish acceleration and constant speed. Performance of some individuals is more related to flow rate, that of others more to edge rate. Perceived changes in speed with changes in edge rate or texture density are illusory and occur in actual flight situations, so these findings will receive more of our attention in the future.

Our approach has direct implications for flight safety and some of these are detailed, with examples, in Appendix E. The problems described will be explored by Ildiko Pallos in her M.A. thesis research on changes in sensitivity following adaptation to prolonged exposure to various flow rates. The complementary effects of edge rate change in compensating for adaptation will also be studied, with interactive as well as passive judgment task conditions.

We have begun a review of the performance literature relevant to our projected interactive studies. To test our general assumption that a pilot makes control adjustments in order control what he sees, we need to understand the relationship among control adjustments; their effects on aircraft attitude, path, and speed; and the optical transformations and invariants produced by the pilot's actions. Appendix F represents the current state of the performance review. It will be updated as we find more relevant articles and technical reports.

The final sections of the introduction show our progress in two major areas, (1) the continued study of visual information for detecting deceleration and (2) the analysis of Boeing 747 simulator landings in terms of optical flow parameters and their relation to performance measures. A listing of accomplishments related to the project follows directly.

Hardware development. For most of the period since the programmable scene generator was completed, we have not been able to conduct descent experiments because the scene changed in steps rather than smoothly. The analog board between the PDP 11/34 computer and the scene generator has been isolated with its own power supply and ground, and a scaling circuit has been added producing an acceptable scene transformation. This is considered a temporary measure until a new analog board can be designed and constructed.

An interrupt rack has been constructed to serve as a general purpose interface between the PDP 11/34 and the GAT-1 simulator, the subject response box used in judgment studies, a joystick, or the second projection TV we will need for studies of peripheral versus central vision. The first subject response box has been completed and is in use for automatic recording of the judgment made by the subject and of the reaction time.

A graphics board and CRT tube have been retrofitted in our new terminal so that we can now plot data from our own experiments or from outside sources. Figures can be photographed directly or data of enduring interest can be transferred to the Computer Center's electrostatic plotter for hard copy. The bootstrap terminator and expansion backplane for the PDP 11/34 are installed, and the new video projection screen and tape recorder are in use for testing subjects.

Dave Park estimated that about 50% of his time is spent on maintenance and repair and about 50% on new design, construction, and installation.

Software development. A new program has been written by Joe Schluter for custom scene texture generation including exponentially spaced edges, and the flight path and speed generation program has been rewritten to meet new and more general requirements. A library file has been developed for the approximately 1300 subroutines in the system.

As a result of a disk failure which wiped out two disk directories and cost us over two weeks down time, a system for recoverying lost files on disks or for recovering files after a disk crash has been written. A micro program disassembler was developed to aid in debugging the scene generator. In order to use the new graphics system for our special needs, Joe wrote a plot package to display path, speed, error, and optical variables from flight maneuvers.

Dave Park wrote a program for automated recording of subject's responses and reaction times in judgment experiments. This allows us to transfer data directly to the Computer Center's main computer for analysis via canned programs.

An Investigation of Optical Information for Detecting Loss in Speed

In an earlier experiment (Owen, Warren, Jensen, Mangold, & Hettinger, 1981), we demonstrated that fractional loss in speed ($\ddot{x}/\dot{x}$, where $\ddot{x}$ = deceleration and $\dot{x}$ = speed) was the useful optical information for detecting loss in speed when deceleration was a constant. In that case, fractional loss accelerated and became more easily detected as the event sequence proceeded. It is possible, however, to hold fractional loss constant throughout an event sequence by reducing deceleration at the same rate that speed is reduced.

In a Master's thesis experiment now being conducted by Shirley Tobias, four determinations are being made: (1) whether fractional loss is more detectable when it increases throughout an event than when it is invariant during a trial, (2) whether performance is the same when fractional loss (either varying or invariant) is the same regardless of the particular values of $\dot{x}$ and $\ddot{x}$, (3) whether attention to fractional loss is independent of global optical flow rate ($\dot{x}/z$, where z = eyeheight), global optical deceleration ($\ddot{x}/z$), and global optical texture density (z/g, where g = surface texture size), and (4) whether the ability to distinguish constant speed from deceleration is affected by initiating the loss in speed already in progress versus preceding loss with a brief period of constant speed. The fourth issue is of interest for several reasons. In all our experiments we have had an error rate of about 20% in the constant conditions. Why would constant speed appear as acceleration or deceleration, or constant altitude appear as descent? One possibility is that the contrast of change with no change has a different effect than the contrast of change with ongoing change.

Both kinds of conditions have ecological validity, since one class represents breaking out of a cloud, where the others represent flying with a variable constant and then having a change imposed. If sensitivity to change is different under these conditions, the effect will be investigated parametrically. Earlier results would have to be reinterpreted, and the design of all future studies would be affected.

Tables 1, 2, and 3 show time series for the types of events to be displayed. (Primes are used in place of the dot notation in the text; $\dot{x}/g$ and $\ddot{x}/g$ denote speed and deceleration scaled in ground texture units, respectively.) Comparing columns for what varies and what is invariant should make differences among the three kinds of conditions apparent.

Table 1

Values of Environmental and Optical Variables at Time T for Two Constant Speed Conditions at an Altitude of 70 m

GROUND TEXTURE SIZE IS CONSTANT AT 23.333 METERS HERE.

T	X´	X´´	X´´/X´(%)	X´/Z	X´´/Z	X´/G	X´´/G	Z/G
0	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
1	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
2	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
3	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
4	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
5	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
6	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
7	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
8	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
9	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00
10	42.000	0.000	0.00	0.60	0.0000	1.8000	0.0000	3.00

GROUND TEXTURE SIZE IS CONSTANT AT 35 METERS HERE.

T	X´	X´´	X´´/X´(%)	X´/Z	X´´/Z	X´/G	X´´/G	Z/G
0	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
1	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
2	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
3	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
4	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
5	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
6	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
7	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
8	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
9	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00
10	63.000	0.000	0.00	0.90	0.0000	1.8000	0.0000	2.00

Table 2

Values of Environmental and Optical Variables at Time T for Two Constant Deceleration Conditions at an Altitude of 70 m

GROUND TEXTURE SIZE IS CONSTANT AT 23.333 METERS HERE.

T	X′	X′′	X′′/X′(%)	X′/Z	X′′/Z	X′/G	X′′/G	Z/G
0	42.000	-3.780	-9.00	0.60	-0.0540	1.8000	-0.1620	3.00
1	38.220	-3.780	-9.89	0.55	-0.0540	1.6380	-0.1620	3.00
2	34.440	-3.780	-10.98	0.49	-0.0540	1.4760	-0.1620	3.00
3	30.660	-3.780	-12.33	0.44	-0.0540	1.3140	-0.1620	3.00
4	26.880	-3.780	-14.06	0.38	-0.0540	1.1520	-0.1620	3.00
5	23.100	-3.780	-16.36	0.33	-0.0540	0.9900	-0.1620	3.00
6	19.320	-3.780	-19.57	0.28	-0.0540	0.8280	-0.1620	3.00
7	15.540	-3.780	-24.32	0.22	-0.0540	0.6660	-0.1620	3.00
8	11.760	-3.780	-32.14	0.17	-0.0540	0.5040	-0.1620	3.00
9	7.980	-3.780	-47.37	0.11	-0.0540	0.3420	-0.1620	3.00
10	4.200	-3.780	-90.00	0.06	-0.0540	0.1800	-0.1620	3.00

GROUND TEXTURE SIZE IS CONSTANT AT 35 METERS HERE.

T	X′	X′′	X′′/X′(%)	X′/Z	X′′/Z	X′/G	X′′/G	Z/G
0	63.000	-8.505	-13.50	0.90	-0.1215	1.8000	-0.2430	2.00
1	54.495	-8.505	-15.61	0.78	-0.1215	1.5570	-0.2430	2.00
2	45.990	-8.505	-18.49	0.66	-0.1215	1.3140	-0.2430	2.00
3	37.485	-8.505	-22.69	0.54	-0.1215	1.0710	-0.2430	2.00
4	28.980	-8.505	-29.35	0.41	-0.1215	0.8280	-0.2430	2.00
5	20.475	-8.505	-41.54	0.29	-0.1215	0.5850	-0.2430	2.00
6	11.970	-8.505	-71.05	0.17	-0.1215	0.3420	-0.2430	2.00
7	3.465	-8.505	-245.45	0.05	-0.1215	0.0990	-0.2430	2.00

Table 3

Values of Environmental and Optical Variables at Time T for Two Constant Fractional Loss in Speed Conditions at an Altitude of 70 m

GROUND TEXTURE SIZE IS CONSTANT AT 23.333 METERS HERE.

T	X′	X′′	X′′/X′(%)	X′/Z	X′′/Z	X′/G	X′′/G	Z/G
0	42.000	−3.780	−9.00	0.60	−0.0540	1.8000	−0.1620	3.00
1	38.220	−3.440	−9.00	0.55	−0.0491	1.6380	−0.1474	3.00
2	35.120	−3.161	−9.00	0.50	−0.0452	1.5052	−0.1355	3.00
3	32.518	−2.927	−9.00	0.46	−0.0418	1.3936	−0.1254	3.00
4	30.294	−2.726	−9.00	0.43	−0.0389	1.2983	−0.1168	3.00
5	28.368	−2.553	−9.00	0.41	−0.0365	1.2158	−0.1094	3.00
6	26.681	−2.401	−9.00	0.38	−0.0343	1.1435	−0.1029	3.00
7	25.191	−2.267	−9.00	0.36	−0.0324	1.0796	−0.0972	3.00
8	23.863	−2.148	−9.00	0.34	−0.0307	1.0227	−0.0920	3.00
9	22.671	−2.040	−9.00	0.32	−0.0291	0.9716	−0.0874	3.00
10	21.596	−1.944	−9.00	0.31	−0.0278	0.9256	−0.0833	3.00

GROUND TEXTURE SIZE IS CONSTANT AT 35 METERS HERE.

T	X′	X′′	X′′/X′(%)	X′/Z	X′′/Z	X′/G	X′′/G	Z/G
0	63.000	−8.505	−13.50	0.90	−0.1215	1.8000	−0.2430	2.00
1	54.495	−7.357	−13.50	0.78	−0.1051	1.5570	−0.2102	2.00
2	48.286	−6.519	−13.50	0.69	−0.0931	1.3796	−0.1862	2.00
3	43.444	−5.865	−13.50	0.62	−0.0838	1.2413	−0.1676	2.00
4	39.540	−5.338	−13.50	0.56	−0.0763	1.1297	−0.1525	2.00
5	36.310	−4.902	−13.50	0.52	−0.0700	1.0374	−0.1401	2.00
6	33.589	−4.534	−13.50	0.48	−0.0648	0.9597	−0.1296	2.00
7	31.259	−4.220	−13.50	0.45	−0.0603	0.8931	−0.1206	2.00
8	29.241	−3.947	−13.50	0.42	−0.0564	0.8354	−0.1128	2.00
9	27.473	−3.709	−13.50	0.39	−0.0530	0.7849	−0.1060	2.00
10	25.912	−3.498	−13.50	0.37	−0.0500	0.7403	−0.0999	2.00

Optical Flow Analysis of Boeing 747 Simulator Landings

Purpose. The most basic assumption underlying our approach is that when a pilot makes a control adjustment, he is indicating dissatisfaction with the current perceptual conditions and is attempting to produce a more desirable state of affairs. That is, he behaves in ways necessary to control his perception. Optical analysis should allow us to determine both what he detected that he was displeased with and what he produced in its place.

Our short-term goal is to work from judgment experiments to situations where the optical effects of control actions by a pilot flying the simulator serve as perceptual reports. Optical analysis of data from a precision simulation system will allow us to learn what to look for in our own interactive data. The Boeing 747 data provide an ideal starting place, because changes take place so slowly. Our long-term goal is to be able to deal with data recorded during performance of actual flight maneuvers, and simulator landings will give us a feel for the complexities of the problem. We plan to use what we learn about the relationships among optical variables, pilot control actions, and aircraft attitude, speed, and path variables to guide the conduct of basic theoretical studies designed to isolate optical information useful in guiding flight.

The raw data. Through the generosity of Conrad Kraft of the Boeing Aerospace Company, we have acquired a copy of the raw data from Experiment 2 of the Kraft, Anderson, and Elworth (1980) study (AFOSR contract number F49620-79-C-0030). They used the Redifon Boeing 747 simulator fitted with a General Electric Compuscene computer generated imagery system. Experiment 2 factorially contrasted narrow versus wide fields of view and simple versus complex ground surface textures. The narrow field was limited to the forward display extending 20 deg to either side of the straight-ahead viewing centerline. The wide field included the forward display plus oblique and side displays for a total of 114 deg in front and to the left of the Captain's position. All displays extended 30 deg vertically.

The simple surface consisted of a blue-black 300 x 10,000-ft runway on a tan desert ground with blue sky above the horizon. The runway had no markings. The complex surface contained the details normally available in the Moses Lake, Washington, data base used for flight crew training, including rows of diamond shaped fields on either side of the runway. This artificial texture was added to give pilots more information when they were close to the ground (See Figure 1). The runway and sky were the same as in the simple surface condition.

Sixteen Air Force Military Airlift Command pilots each made four approaches in each of the four conditions, for a total of 256 landings. All were current in the C-141 military air transport, but had no prior experience in the 747. All approaches were straight in, beginning 4.7 nautical miles from runway threshold at about 1350 ft altitude with the aircraft trimmed for a 2.5-deg path angle. The landing gear was down and flaps were at full 30-deg throughout the approach. Dependence on visually guided flight was ensured by removal or occlusion of all instrumentation except the airspeed indicator. The simulator motion base was active during all trials.

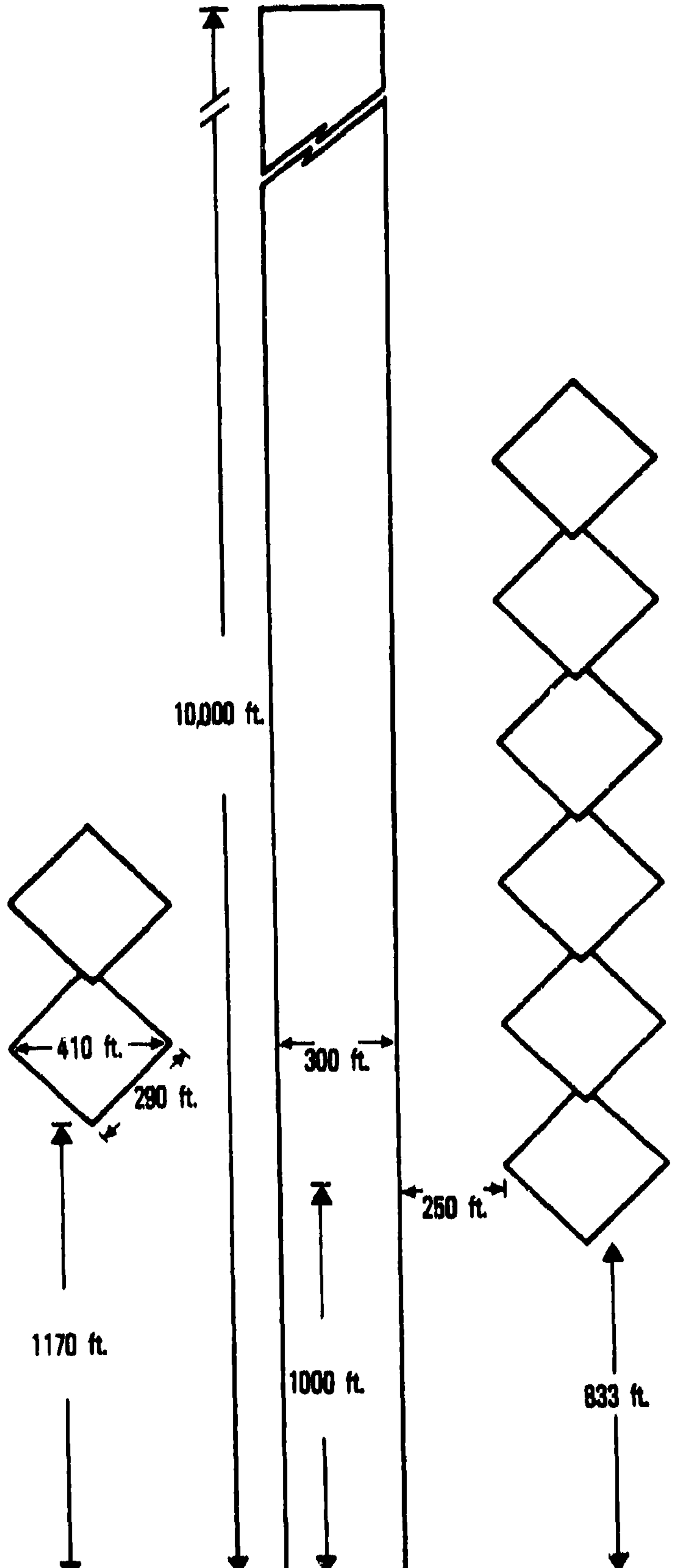

Figure 1. Topography of a section of the Moses Lake ground surface showing the runway and the artificial diamond shaped texture nearest the runway threshold.

The pilot was instructed to proceed straight in to a minimum-descent-rate touchdown at 1000 ft beyond the runway threshold. Among other variables, x, z, $\dot{s}$, and $\dot{z}$ were recorded every 450 msec. We are using these variables to compute and plot, over distance to instructed touchdown and over time, the pilot's eyeheight (z), path speed ($\dot{s}$), path speed acceleration ($\ddot{s}$), climb (sink) rate ($\dot{z}$), climb (sink) acceleration ($\ddot{z}$), instantaneous path slope ($\dot{z}/\dot{x}$), global optical flow rate ($\dot{s}/z$), fractional loss in altitude ($\dot{z}/z$), and fractional loss in speed ($\ddot{s}/\dot{s}$). Pilot control actions, such as power lever angle, and system variables, such as angle of attack, pitch, and roll will be related to optical variables and to computations of flight path error (vertical, lateral, and circular). Most of these variables are shown in Figures 2 through 5, using the first landing in the experiment as an example. Examples of eyeheight, flow rate, and fractional loss in altitude, all plotted over distance for three approaches, can be seen in Figures E-1, -2, and -3 of Appendix E.

When an aircraft is properly trimmed and the controls are not moved, the path of craft (and the pilot's eye) will be linear. A linear segment can be used as an indication that the pilot has achieved a desired set of conditions. Presumably, he will remain on the same path until he perceives that the path is undesirable. He may, for example, see that he is undershooting (or overshooting) the instructed touchdown point and adjust the power lever angle to reduce (or steepen) the path slope.

Figure 2 shows six linear path segments found by using a straight edge. (The second segment actually consists of three linear subsegments,

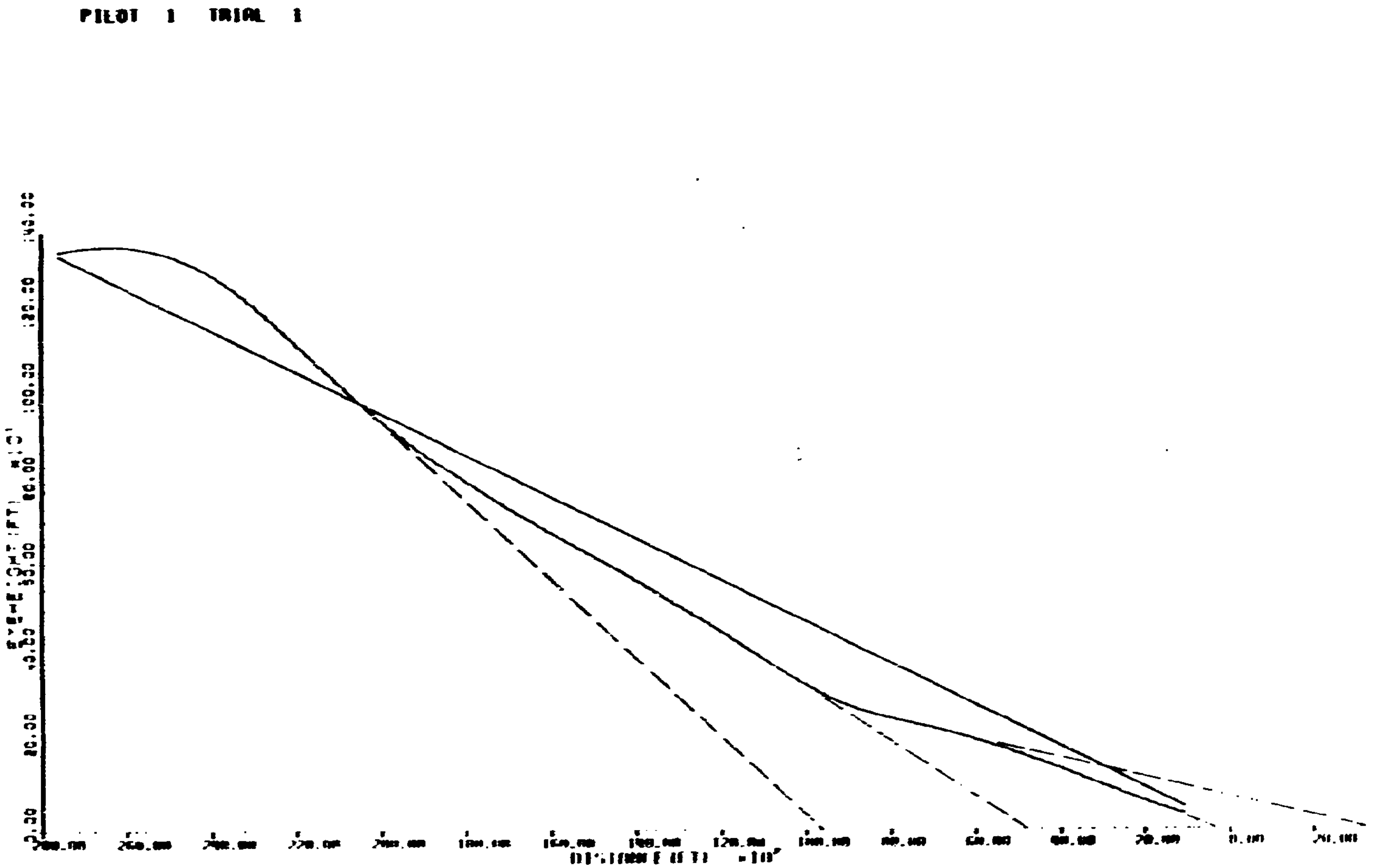

Figure 2. The line with curved segments shows the path of the eye during approach. The straight line is the instructed path. Dashed lines are extensions of linear segments to the points of potential impact.

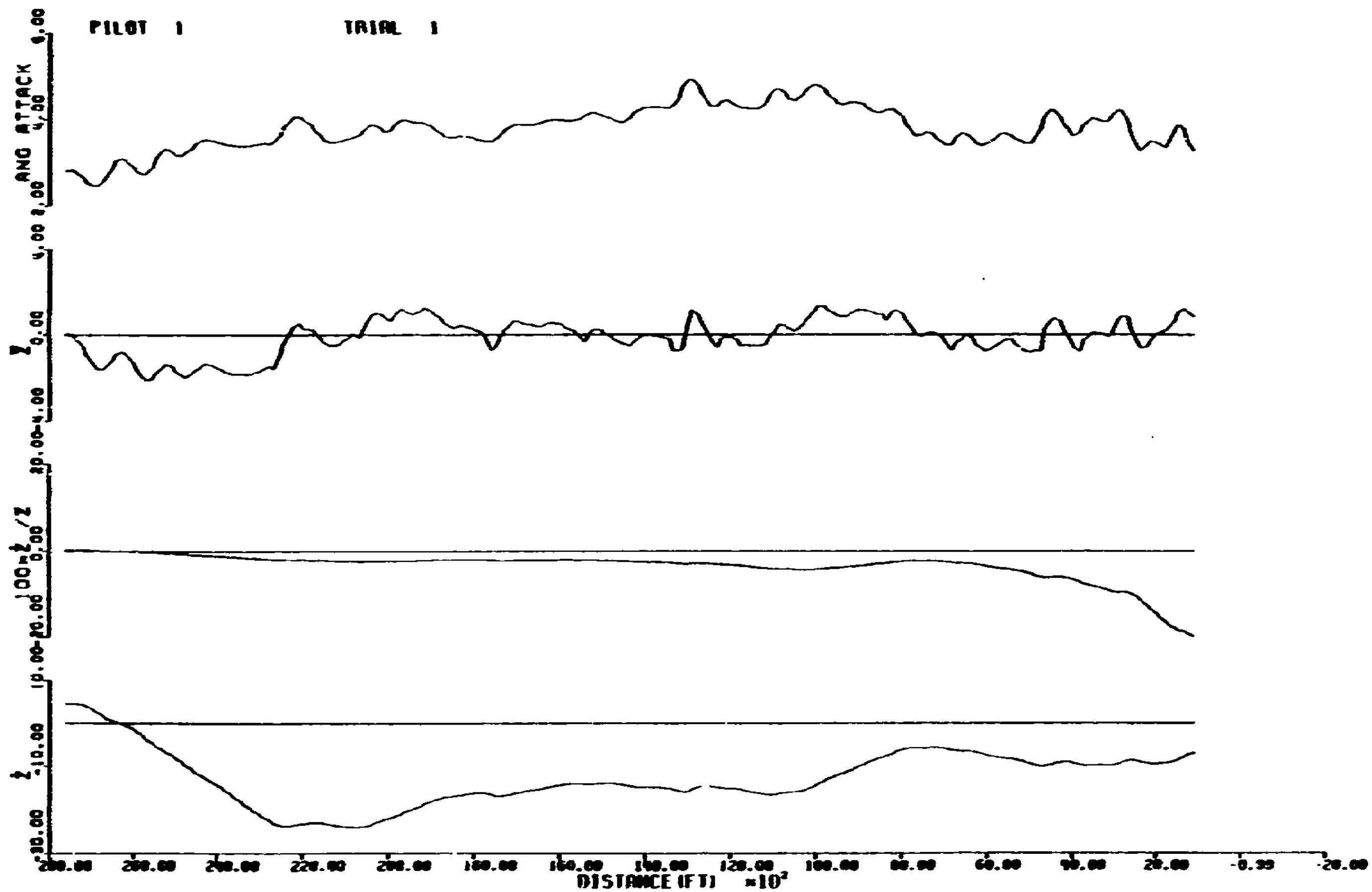

Figure 3. Angle of attack in deg, vertical acceleration ($\ddot{z}$) in ft/sec^2, fractional loss in altitude ($\dot{z}/z$) in percent/sec, and vertical speed ($\dot{z}$) in ft/sec.

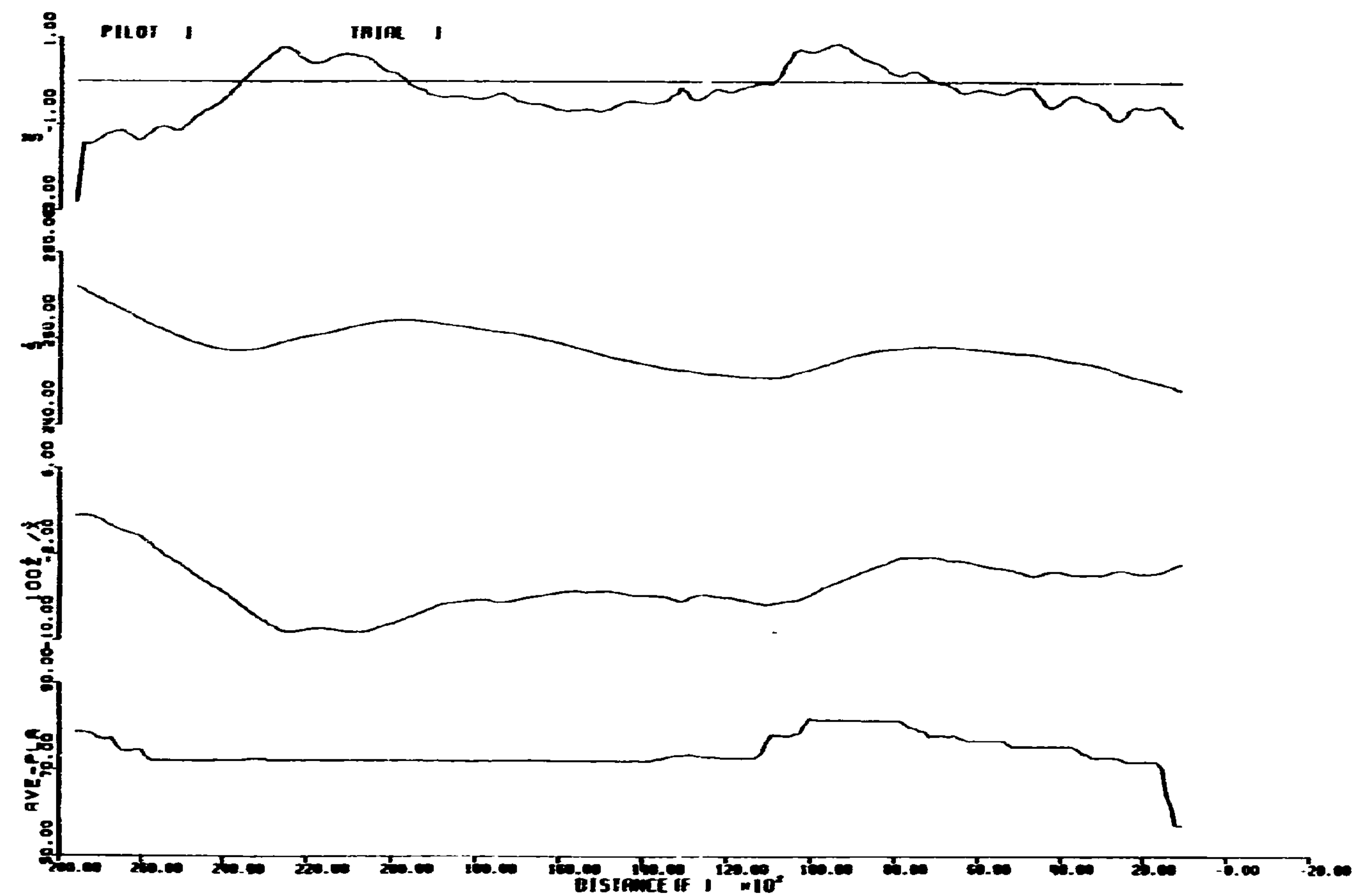

Figure 4. Acceleration along the flight path ($\ddot{s}$) in ft/sec, path speed ($\dot{s}$) in ft/sec, path slope ($\dot{z}/\dot{x}$) in percent, average power lever angle (AVE-PLA) in deg (the mean of the four hand throttle positions).

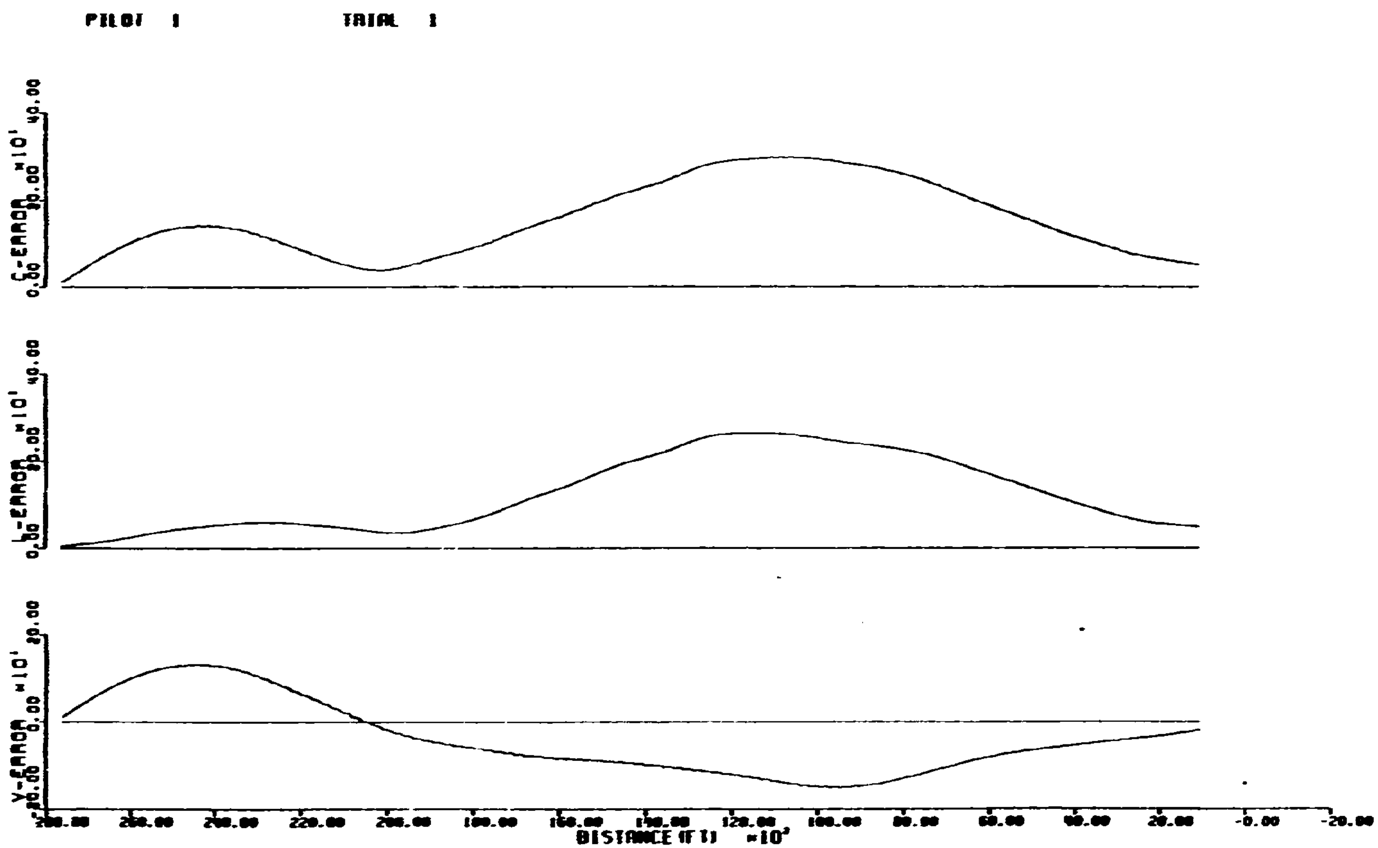

Figure 5. Root-mean-square circular error, lateral error, and vertical error over distance to instructed touchdown point.

of which only the last is extended.) Extensions of the segments show the point of potential impact which is also the point of optical expansion. Error in ground distance from the instructed expansion point can be computed, as well as time to collision if the path is not changed.

Efforts are now underway to use a technique developed by Pavlidis (1976) to isolate the segments by computer. When completed, the segments will be separately analyzed for duration, distance, and optical variables. An example of a segment invariant ($\dot{z}/\dot{x}$) is shown in Figure 6 over 450-msec iterations. The horizontal lines show the invariant values of path slope over (conservatively short) durations.

At the simplest level, the number of segments can be used as a dependent variable to compare scene and event conditions for adequacy of information, test for improvement with practice, and examine individual differences. If perceptually useful information is in fact eyeheight scaled, the segments should be longer in duration at higher altitudes where optical changes are smaller in magnitude.

Finally, each segment isolated will be subjected to analysis in terms of path, speed, and attitude variables; optical flow variables; pilot control adjustments; and system variables, in order to survey their relationships. Special attention will be given to the last linear segment, flare, and the time sample just before touchdown. A pilot who flies an ideal approach into the ground without flaring may have a low root-mean-square error, and a pilot who deviates radically from ideal during most of the approach may produce an ideal touchdown. Therefore the most weight in evaluating effects of real-world and experimental treatment conditions must be given to the critical phase of the required maneuver.

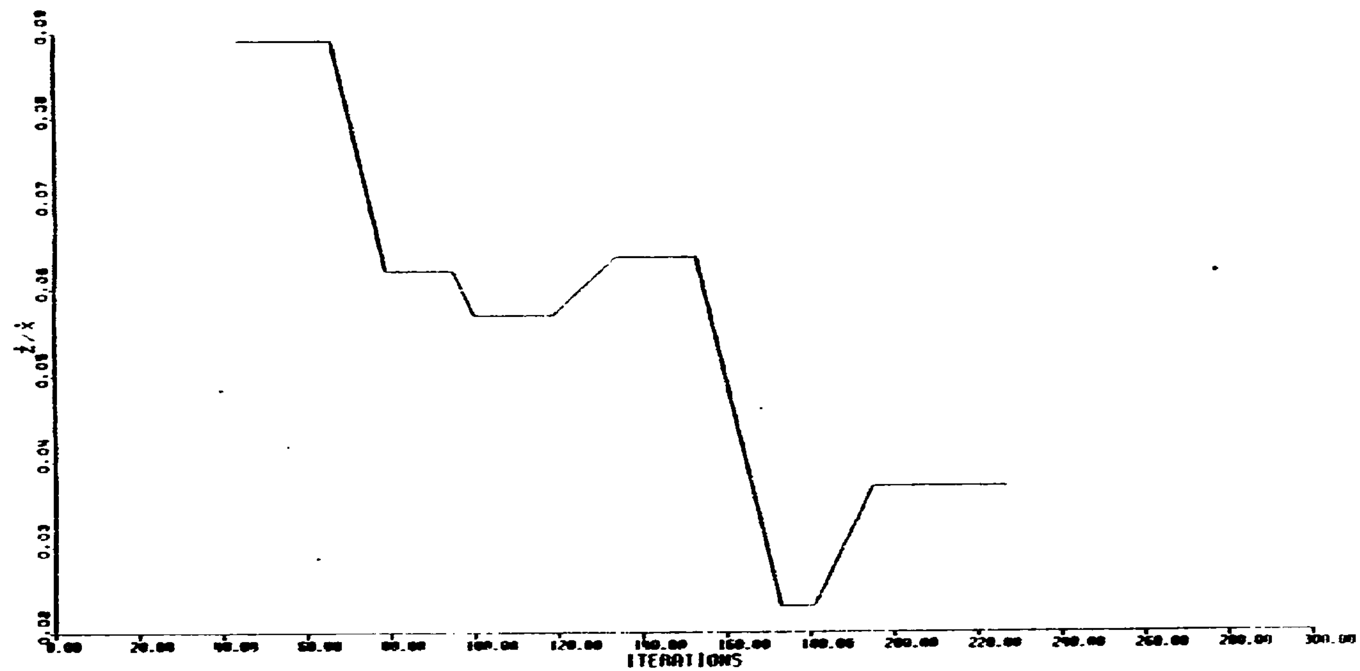

Figure 6. Horizontal lines showing the duration (in 450-msec iterations) over which path slope ($\dot{z}/\dot{x}$) is invariant. Compare with the linear segments of Figure 2.

ACCOMPLISHMENTS RELATED TO THE PROJECT

Publications

Owen, D. H., Warren, R., Jensen, R. S., Mangold, S. J., & Hettinger, L. J. Optical information for detecting loss in one's own forward speed. Acta Psychologica, 1981, 48, 203-213.

Warren, R., & Owen, D. H. Functional optical invariants: A new methodology for aviation research. Journal of Aviation, Space, and Environmental Medicine, in press.

Owen, D. H., Warren, R., & Mangold, S. J. Optical information for detecting loss in one's own altitude. Perception & Psychophysics, in press.

Proceedings papers

Owen, D. H., & Jensen, R. S. Optical texture variables useful in simulating flight maneuvers. Proceedings of the 1980 Air Force Office of Scientific Research Review of Sponsored Basic Research, 1980, 1, 11-12.

Owen, D. H., Warren, R., Jensen, R. S., Mangold, S. J., Alexander, G., & Hettinger, L. J. Transformational realism: An interactive evaluation of optical information necessary for the visual simulation of flight. Proceedings of the Image Generation/Display Conference II, 1981, 2, 385-400.

Warren, R., & Owen, D. H. Functional optical invariants: A new methodology for aviation research. Proceedings of the First Symposium on Aviation Psychology, 1981, 1, 192-204.

Mangold, S. J., Owen, D. H., & Warren, R. Fractional rates of change as functional optical invariants. Proceedings of the First Symposium on Aviation Psychology, 1981, 1, 205-215.

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M.A. thesis

Hettinger, L. J. Detection of descent in the absence of optical flow deceleration. M.A. Thesis, The Ohio State University, 1981.

Presentations

Owen, D. H., Jensen, R. S., Warren, R., & Mangold, S. J. Optical texture variables useful in simulating flight maneuvers. Presented at the Air Force Office of Scientific Research Review of Basic Research in Flight and Technical Training, United States Air Force Academy, Colorado Springs, Colorado, March 25-27, 1980.

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Warren, R. Identification, isolation and interaction of specific optical flow field parameters for specific perceptual tasks in the guidance of one's own motion. Paper presented at the NATO Symposium on the Study of Motion Perception: Recent Developments and Applications, Veldhoven, The Netherlands, August 24-29, 1980.

Owen, D. H., Warren, R., Jensen, R. S., Mangold, S. J., & Alexander, G. Optical flow information for the perception of loss in one's altitude and forward speed. Presented at the meeting of the Psychonomic Society, St. Lous, Mo., November 13-15, 1980.

Warren, R., & Owen, D. H. Functional optical invariants: A new methodology for aviation research. Presented at the Symposium on Aviation Psychology, Columbus, Ohio, April 20-22, 1981.

Mangold, S. J., Owen, D. H., & Warren, R. Fractional rates of change as functional optical invariants. Presented at the Symposium on Aviation Psychology, Columbus, Ohio, April 20-22, 1981.

Owen, D. H., Warren, R., Jensen, R. S., Mangold, S. J., Alexander, G., & Hettinger, L. J. Transformational realism: An interactive evaluation of optical information necessary for the visual simulation of flight. Presented at the Image Generation/Display Conference II, Scottsdale, Arizona, June 10-12, 1981.

Owen, D. H., & Warren, R. Simulation of functional optical information. Invited symposium presentation at the meeting of the American Psychological Association, Los Angeles, August 24-28, 1981.

Warren, R., Owen, D. H., & Hettinger, L. J. Effects of exponential texture spacing and speed on perceived egospeed. Presented at the meeting of the Psychonomic Society, Philadelphia, November 12-14, 1981.

Presentations, continued

Owen, D. H., & Warren, R. Perceptually relevant metrics for the margin of aviation safety: A consideration of global optical flow and texture variables. Presented at the Conference on Vision as a Factor in Military Aircraft Mishaps, San Antonio, Texas, February 23-25, 1982.

Colloquia (Owen, D. H.)

"Visual information for the perception of one's own motion." Oberlin College, April 11, 1980.

"Visual information for the perception of one's own motion." The University of Konstanz, West Germany, July 4, 1980.

"Toward an interactive approach to perception." University of California, Santa Barbara, August 28, 1981.

"Isolation of visual information useful in simulating and guiding flight." Boeing Aerospace Company, Seattle, Washington, January 11, 1982.

Other relevant activities

Owen, D. H. Participated in the conference on "Quantifying Visual Scene Parameters" sponsored by the Life Sciences Directorate, Air Force Office of Scientific Research. At Virginia Polytechnic Institute and State University, Blacksburg, Virginia, July 11-12, 1978.

Owen, D. H. Organized a workshop on "Flight Simulation and Self-Motion Perception" sponsored by the Departments of Psychology and Aviation, Ohio State University, February 1, 1979. Researchers from five universities and Wright-Patterson Air Force Base presented papers.

Owen, D. H. Moderator for the Vision and Visual Perception session, 1981 Symposium on Aviation Psychology, Columbus, Ohio, April 21, 1981.

Owen, D. H., & Warren, R. Invited participants, First International Conference on Event Perception, University of Connecticut, Storrs, Connecticut, June 7-12, 1981.

APPENDIX A

FUNCTIONAL OPTICAL INVARIANTS:

A NEW METHODOLOGY FOR AVIATION RESEARCH

Rik Warren and Dean H. Owen

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The Ohio State University

ABSTRACT

The application of Gibson's (1979) "ecological approach to visual perception" to aviation psychology entails the use of information rich visual displays that must adequately and unambiguously enable a pilot to perform flight maneuvers. Optical information often takes the form of invariant properties of a changing optic array and functional invariants are defined as psychologically effective optical invariants. Their effectiveness is determined by empirical test but standard experimental paradigms are shown to be inappropriate for testing the effectiveness of information in rich displays due to the presence of inherent and unavoidable confounding factors that are here termed "secondary independent variables" in contradistinction to the "primary independent variables" manipulated by the experimenter. Recommendations for a new methodology and statistical treatment are offered and the implications for aviation psychology are discussed.

INTRODUCTION

The concept of functional optical invariants and the new methodology they entail were developed to meet certain difficulties we encountered in our attempt to apply J. J. Gibson's (1979) "ecological approach to visual perception" to fundamental problems of aviation psychology. Specifically, we are attempting to determine and describe the necessary and sufficient optical conditions that induce a perception of egomotion (self-motion). A knowledge of the necessary and sufficient optical bases for the perception of egomotion is needed to optimally design visual flight simulators and simulator training programs. Optimization is psychologically and economically important since underdesign results in poorer simulation training than possible and overdesign results in overly expensive training.

Ecological Optics and Optical Invariants

Since the concept of functional optical invariants is an extension of Gibson's (1979) theory, his ecological approach will be briefly reviewed. "Ecological optics" is the study of the information available in light and its origins trace back to Gibson's (1947) research on pilot selection and training in World War II. The principles of ecological optics that are relevant here are:

1. The light coming to a moving point of observation is structured owing to the structure of the environment and the observer's travel.

2. The optical structure is constantly changing, again owing to the observer's travel and also to events in the environment.

3. Over the changing structure or transformations of optical structure, there remain properties (often higher-order relationships) that do not change and are thus invariant over the transformation.

4. These optical invariants are claimed to be, or to form the bases of, the univocal information used by active perceivers to survive in and to exploit their environment.

Examples of change of optical structure. A common type of change of optical structure is the total change in the optical location or direction of points in the environment that corresponds to a displacement of the point of observation (Gibson, Olum, & Rosenblatt, 1955). Another example of change of optical structure is the change in optical size and optical density of environmental features due to a change in altitude.

Examples of optical invariants. During rectilinear egomotion. the optical position of the horizon is invariant over the otherwise total flow transformation. Also, the optical position of the ground point toward which a plane is heading is invariant if the path slope is constant. Since path slope (if there is no wind) is the ratio of the descent rate to the forward velocity, this means that the optical position of the aim point is furthur invariant over changes of descent and forward velocities as long as these change proportionately. Changes in these velocities do result in a change in the global optical flow rate (Warren, Note 1).

This example of path slope as a ratio of two rates of change underscores a common finding of ecological optics: often optical invariants emerge as rates of change during changes and especially as ratios of rates of change of environmental variables.

It is important to note that whether or not an optical invariant is indeed mathematically capable of specifying its source is a question for geometry; whether or not a particular optical invariant is actually used by an observer is a question for psychology. Hence, ecological optics is not itself a theory of perception, but a propaeduetic for one.

Perception and Functional Optical Invariants

Perception is defined as the pickup of information available in light. However, the existence of potentially available information does not force perceiving since, for example, an observer may not be attending or not yet have developed sufficient pickup skills (E. J. Gibson, 1969). Thus, optical invariants fall into two functional equivalence classes: those that are not utilized and are thus perceptually ineffective, and those that are indeed picked up and are thus perceptually effective.

Definition: Functional optical invariant. A functional optical invariant is an optical invariant that is perceptually effective (Owen, Warren, Jensen, Mangold, & Hettinger, in press). The term "functional" carries two implications: that of being used or utilized and also that of utility or practical, survival value.

The implication of being used means that the ultimate determination of whether or not something is a functional optical invariant is by empirical testing. This in turn implies that an adequate research methodology must be available.

The implication of utility means that the problems selected for study are motivated by practical concerns. This in turn implies that the research methodology be sensitive to the requirements of ecological validity.

Ecological functionalism and direct perception. The emphasis on ecological validity is a hallmark of the ecological approach and there are currently two active branches of development: One branch emphasizes the epistemological implications of the ecological approach and is associated with the term "direct perception" (e.g., Shaw & Bransford, 1977); another branch emphasizes the empirical implications and is termed "ecological functionalism" (Owen et al., in press).

This paper is on ecological functionalism and is concerned with the problem of how to study sensitivity to optical invariants. If standard experimental paradigms were adequate for testing candidates for functional optical invariant status, then this paper would be unnecessary. Unfortunately, standard experimental paradigms used today make assumptions that are inappropriate for perceptual research in aviation.

Assumptions of Standard Experimental Paradigms

The standard experimental paradigms we are refering to attempt to assess the effects on a performance dependent variable of systematic manipulation of two or more independent variables (IV) in a balanced, orthogonal factorial design. In practice, several assumptions are made in applying these paradigms to research problems. One class of assumptions may be termed "technical" and is not of interest here. These include the assumptions of random assignment and homoscadacticity. The second class of assumptions is concerned with the adequacy of the selection and evaluation of the IVs and are necessarily problem or context sensitive. In discussing these assumptions, the specific context is that of perceptual factors in aviation. The assumptions commonly made in current research are:

Assumption 1. It is assumed that the IVs generally selected are indeed the most relevant or germane for perception and action. Most relevant is used synonymously with directly relevant in a causal chain sense. For example, a common variable in the study of the perception of egospeed is actual speed of travel. The selection criterion apparently used is that of face validity albeit intuitively or tacitly applied.

Assumption 2. In any experiment, the total variation in the dependent variable may be partitioned into that due to: (a) the effects of the IVs selected and their interactions, (b) other systematic effects of either identified or unidentified sources, (c) individual differences, and (d) random error. Often, the sources of systematic effects may be intercorrelated so that advanced techniques such as multiple regression and correlation are required to evaluate the contribution of redundant factors, and hence interpretation is difficult (Cohen & Cohen, 1975).

But, it is assumed that the variation due to "other systematic effects" may be reduced to zero by means of a well designed and executed balanced orthogonal design. By well designed and executed is meant that the effects of all non-experimental factors (either identified or not) are made irrelevant by such means as elimination, use of a single level if elimination is not feasable, randomization, or counterbalancing so that their effects are self-cancelling and/or equal to zero. In essence, a well designed experiment is assumed to control for or be free of confounding factors. Technically, a confounding factor is a non- experimental or non-manipulated factor which has a non-zero coefficient of multiple determination ($\underline{R}^2$) or curvilinear determination (η^2) with some IV or interaction of IVs of interest. It is further assumed that the presence of a confounding factor indicates a poor experiment.

Assumption 3. The third assumption is that data analysis is complete once an analysis of variance or regression analysis has rendered a verdict on the main effects and interactions. (Post hoc tests, trend analyses, and regression equations are included in the above analyses.) The main point here is that although the discovery of an interaction may lead to joy if it was predicted, or anguish if it was unexpected and "must be explained", it is assumed that no further explication as to just exactly how the variables combine is required. An interaction is defined as an effect beyond the mere addition of the effects of main factors, and there is no presumption that the exact mathematical nature of the non-additivity must be explicated. More serious is the assumption that main effects are terminal findings especially if no significant interaction is found.

Ecological Critique of Standard Methodology

As reasonable as the above assumptions are, they are not immune to criticism. One obvious critique of most experiments from the ecological viewpoint is the lack of ecological validity of the tasks and situations commonly used. But ecological validity does not concern us here since it is orthogonal to the procedural assumptions at issue.

Critique of Assumption 1

Perception exists for the purpose of acting in and on the environment. Hence it is reasonable to vary environmental conditions to determine their effect on perception and performance. But perception as the pickup of environmental information contained in light is perforce constrained by the available information. We cannot see a very real tree in front of us in the dark. Hence it is also reasonable -- and we argue, more reasonable -- to systematically vary the information contained in the light and let the ego - environment states corresponding to that information vary freely rather than the other way around as is now the practice. There would be no problem as to which to deliberately vary and which to let vary freely if simple or low-order optical and environmental structures were in one-to-one correspondence, but that they are not always so has been plaguing the study of perception since Euclid.

An example in which there is lack of correspondence between simple or low-order optical and environmental states is common in aviation: Two planes may be traveling at the same ground speed, but if one is flying very low, both the optical flow rate and the corresponding experience of egospeed will be fast, whereas if one is flying very high, both the optical flow rate and the corresponding experience of egospeed will be slow (Warren, Note 1). Hence, a study that systematically varied egospeed, but not optical flow rate, could miss the dependence of perceived egospeed on altitude. A study that included altitude as a second orthogonal factor might find a significant interaction between egospeed and altitude, but unless it went beyond the environmental factors to the relevant optical factor, it could not explain the interaction. There are two lessons to be learned from this example: One is concerned with the number of factors to include in an experiment and is discussed in the next section. The other lesson is that the finding of a functional relationship between an environmental condition and perception is not enough, for we must also learn what the information "linking" the two is. Unfortunately, the optical conditions, especially the optical invariants, tend to be ignored.

Critique of Assumption 2

The second assumption of the standard approach may be characterized as implying that the factors chosen for an orthogonal design may be so chosen and so presented as to avoid the effects of any confounding factors either by elimination or deliberate control of all possible confounds. Our point here is that this situation, however desirable for elegance of design and ease of interpretaion, is in general inherently unattainable in experiments utilizing scenes of sufficient ecological validity to be of interest in aviation research. In general, there will exist at least one, and often many, identifiable factors, in addition to the specified set of orthogonal experimental factors, which will stand in a non-orthogonal relationship to them. In other words, there will always exist confounding factors whose effects cannot be controlled or eliminated by the experimenter, because the factors are inherently tied given the environmental constraints.

Where the inherent confounding exists, the very notion of confounding must be reinterpreted. We will attempt a reinterpretation and try to specify the conditions under which aviation research leads to non-standard analysis.

The reason for the inherent confounding of experimental factors is that each experimental factor (excluding non-visual factors such as replications and flying experience) corresponds to some characteristic or descriptive parameter of the visual scene, whereas the number of degrees of freedom available for distribution among the scene parameters is smaller than the number of scene parameters that must assume values. One consequence of the shortage of degrees of freedom is that an experimenter may manipulate or specify the values of only a small subset of scene parameters; the values of all the other unavoidably co-existing scene parameters are then forced or determined once the values of the initial subset are assigned. The experimenter's problems are further exacerbated since there is not complete latitude in choosing which combination of scene parameters may be assigned to the degree of freedom consuming subset. This may be best explained by identifying the scene parameters and their interrelationships:

Scene parameter degrees of freedom. A complete description of an egomotion scene includes a specification of the environment and the orientation of the "window" through which an observer views the world. In addition, the following must be specified:

1. The path slope. The specification of the path slope consumes one degree of freedom.

2. Speed of travel. Speed of travel may refer to the path speed or to its components, descent rate and forward velocity. But assignment of values to these three parameters is constrained since they are related by the Pythagorean theorem: Path speed is the square root of the sum of the squares of descent rate and forward velocity. Another constraint is that descent rate and forward velocity are functionally related by the prior selection of a path slope since path slope is equal to the ratio of descent rate to forward velocity. These two constraints mean that there is only one degree of freedom for selecting among the three parameters of path speed, forward velocity and descent rate.

3. Initial position. The initial position of an observer in an egomotion scene consumes one additional degree of freedom. Position is fixed once one of the three position paramters of path distance to the touchdown point, ground distance to the touchdown point, or initial altitude is assigned a value. This is because path distance, on a rectilinear path, is related to the ground distance and the altitude by the Pythagorean theorem: Path distance is the square root of the sum of the squares of the ground distance and the altitude. Another constraint comes from the prior selection of path slope since path slope, in rectilinear travel, is equal to the ratio of the altitude to the ground distance.

4. Initial acceleration. The acceleration aspect of travel also permits one degree of freedom for its determination in a manner entirely analogous to the cases of initial postion and initial speed. The three parameters of path acceleration, forward acceleration, and downward acceleration are determined once the value of one is chosen.

5. Ground texture size. Computer generated displays often use ground texture that is regular or stochasticaly so. The determination of the (average) texture unit size also consumes one degree of freedom.

Summary of degrees of freedom. The 11 scene parameters just descibed permit only five degrees of freedom for their selection.

Further restrictions. An experimenter is further constrained in that the five degrees of freedom may not be distributed freely. This is because certain combinations of variables are mathematically related and that relation cannot be broken. For example, since path slope is the ratio of descent rate to forward velocity, no experiment may orthogonally vary all three factors. This can be very frustrating to the researcher who wishes to determine the effects of these factors on flying performance. Another example is provided by the problem of determining the relative effects of the various variables that might affect the perception of change in altitude: No ecologically valid set of egomotion displays may simultaneously orthogonally combine the factors of descent rate scaled in meters, in eyeheights, in ground texture units, and the ratio of descent rate to forward velocity, since there are only three degrees of freedom available for these variables. But the expermenter's quandary is further deepened because the honorable techniques of setting one factor to a constant value or eliminating it are not applicable. All four factors must coexist, and due to their functional dependencies, one will always vary outside of the experimenter's control.

"Primary" and "secondary" independent variables. In an experiment, the factors that an experimenter chooses to manipulate are generally refered to as IVs and are here further specified as "primary" IVs. The factors that exist as a consequence of the mathematical relationships among the primary IVs are also true IVs in spite of the fact that they are not orthogonal to the primary IVs and that they assume their values as a function of their relationship with variables controlled directly by the experimenter. Thus, primary IVs correspond to the subset of scene parameters to which the experimenter has chosen to allocate the available degrees of freedom. The secondary IVs then correspond to the scene parameters not manipulated by the experimenter.

What is a primary IV in one experiment may become secondary in another experiment. For example, in one experiment, an experimenter may orthogonally cross descent rate and forward velocity as primary variables. Path slope is then determined by the ratio of descent rate to forward velocity and is a legitimate experimental factor although the experimenter did not assign its values directly. In another experiment, the experimenter might choose to orthogonally cross descent rate with path slope, letting forward velocity vary as neeeded. In this second experiment, path slope has become a primary IV and forward velocity a secondary IV. No member of a mathematically related set of factors is inherently primary or secondary despite the appearance of the equations specifying the relationship. Any equation may be rewritten so that any variable appears as a function of the others.

It is important to note that the choice of primary and secondary IVs refers only to activity by an experimonter and not to activity by a parceiver or perceptual system. The experimenter's activity is to affect the availabiliy of optical information by manipulating directly the levels and ranges of the primary scene parameters and indirectly the levels of the secondary scene parameters. The perceiver's or perceptual system's activity is to pick up and utilize information from the optic array. A perceiver also may act to bring an event and its information into being as in the case of making a landing approach. But, which optical invariants are functional optical invariants for a given perceptual system is determined, in part, by the information extraction (not merely transducing) characteristics of that system and not by what the experimenter does. The lesson here is that the information that a perceiver uses may not always be the information that an experimenter was primarily manipulating. Analysis of the performance data as a function of the secondary IVs may roveal the effectiveness of these sources in contrast to the possibly less useful (or unused) primary IVs. This possibility has implications for the tenability of the third assumption of the standard paradigm.

Critique of the Third Assumption

The ecological critique of the third assumption is simply that it is not sufficient to just report that an interaction exists between two or more variables. In a simple experiment in which all confounding effects are eliminated and especially when the the experimenter has no theoretical expectation of a mathematical relationship between two variables, it may be reasonable not to pursue an analysis beyond the determination of the regression equation for the variables and their interaction. This is because there is no reason to "create" a new variable to enter into the regression equation. But in the complex visual scenes of the type encountered in aviation research, there do exist secondary IVs as a consequence of the mathematical relationships among the primary or main IVs in a standard orthogonal design. The mathematical relationship often takes the form of a decidedly non-additive "interaction" of the primary factors such as their product or ratio. Thus, it might be possible to specify the exact form of how the factors interact. This is preferable to merely concluding that "some" interaction exists.

Toward a New Methodology

The traditional experimental method, with its insistence on controlling and excluding confounding factors, is too powerful a research tool to dismiss lightly. But the visual scenes used in aviation research do seem to preclude the total elimination of "confounding" factors, and we have seen that sometimes these so-called confounding factors are very much of interest. We would very much like to orthogonally cross certain sets of factors but unfortunately are logically prevented from doing so as in the case of descent rate, forward velocity, and path angle or in the case of the four scaling variations of descent rate, viz., descent rate scaled in meters, altitude, and ground units per second plus the descent rate as a fraction of forward velocity. Thus, experimental research in aviation psychology requires some modification of standard methodology. The following list is intended as a first attempt at grappling with the problems posed by aviation research.

Recommendation 1

Since the visual system extracts information from light, it is reasonable to include optical variables and not just environmental variables in the set of primary IVs. For example, global optical flow rate can be included in the primary set in lieu of or crossed with path speed.

Recommendation 2

Since there is good theoretical reason to expect much, if not all, optical information to take the form of optical invariants, especially invariant ratios, it is important to include several levels of the optical invariant in question and also to form each level of the invariant using different combinations of absolute environmental values. The inclusion of several levels of an optical invariant permits assessment of whether or not the optical invariant is a functional optical invariant. Three levels within a range optimized by preliminary experimentation will typically reveal the form of the functional relationship. For an optical invariant to be a functional optical invariant, performance must vary as the optical invariant is set to different values. For example, does ability to detect the point on the ground toward which one is flying vary as the the angular separation between the focus of expansion and the horizon, an optical invariant under rectilinear egomotion, is set to different values? The forming of each level of the invariant from several combinations of absolute environmental values is for the purpose of enabling the invariant to exist independent of particular absolute levels of the component variables. An invariant can exist over the change or transformation within an event and also between events whose absolute values differ. For example, Table 1 shows that a path slope of .10 is common to three different flight paths having, in arbitrary units, descent rate / forward velocity pairings of 1/10, 2/20, and 4/40 respectively. If only one combination of absolute values were used, it would not be possible to attribute the results to the ratio or to the absolute values.

Table 1

Path slope as a function of
descent rate and forward velocity.
(arbitrary velocity units)

		Descent Rate		
		1	2	4
Forward Velocity	10	.10	.20	.40
	20	.05	.10	.20
	40	.025	.05	.10

Recommendation 3

Make all known secondary IVs explicit. Generally, experimenters report only the primary IVs that they used in an experiment and these are generally environmental rather than optical variables. But the secondary IVs are nevertheless present. Sometimes it is possible from the experimental report to determine some of the secondary IVs, but this is not always possible and poses unnecessary problems for readers. More frustrating is the all too common problem that, whether or not the secondary IVs are reported, the results, such as means, for these variables are impossible to compute from results summarized over levels of a variable. (A table of means for each cell in the design would solve this problem.) Results for the secondary IVs might actually be more impressive than those for the primary IVs and thus should be reported.

Recommendation 4

Recommendation 4 follows immediately from Recommendation 3: the statistical analyses should be extended to include the secondary IVs. Since the secondary IVs are generally non-orthogonal to the primary set, this means that multiple regression and stepwise multiple regression would be appropriate. Since multiple regression can be cumbersome, it would be useful to have a simple way to evaluate the secondary IVs taken one at a time. The following techniques are presented only as working suggestions, and since the statistical procedures need further evaluation, the results obtained should also only be treated as suggestive.

One technique to simply assess a secondary IV is to ignore all other variables and perform a one-way analysis of variance on the data. The number of levels of the secondary IV will be determined by the number and spacing of levels of the primary IVs "interacting" to produce the secondary IV. The nature of the combinations is such that the data for each level of the secondary IV represent a pooling of the data from one or more of the "primary" data cells produced by the orthogonal crossing of the primary IVs. The number of primary data cells that are pooled into one level of the secondary IV will, in general, not be equal, and hence the number of data points per level of the secondary IV will also not be equal. For example, assume Table 1 represents the design of a simple experiment with descent rate and forward velocity as primary IVs. In addition to a standard analysis, the data may also be analyzed for the effects of path slope as a secondary IV. Notice that this particular spacing of the three levels each of the primary IVs yeilds five levels of path slope. In particular, a path slope of .10 is formed by three different crossings of the primary variables whereas a path slope of .40 results from only one crossing. Assuming equal numbers of data points per primary cell, then there are three times as many data points at the .10 level of path slope as there are at the .40 level since the data for the .10 level come from the pooling of three primary cells whereas the data for the .40 level come from only one primary cell.

There are two reasons for arguing that a one-way analysis of variance is appropriate for the assessment of a secondary IV. One reason is that one-way ANOVA is well suited for and unambiguous with respect to the unequal "*n*" problem that arises from the pooling of different numbers of primary cells to yield the levels of the secondary IVs. The problem of unequal "*n*" within the context of complex ANOVA is, of course, notorious. Another reason for suggesting the one-way ANOVA is that the ratio of the between-groups sum of squares to the total sum of squares is equal to the coefficient of curvilinear determination (η^2) and the coefficient of multiple determination (R^2). This ratio indicates the proportion of variance accounted for by all the statistical information in the secondary IV and thus is an index of the total strength of the variable.

However, extreme caution must be used in interpreting the R^2 produced by the above method. Its strength of using all the statistical information in the secondary IV is also its weakness. The R^2 so obtained is that R^2 obtained by a regression line fitted perfectly through the means of the secondary IV. That regression equation is a power polynomial of degree equal to the number of levels of the variable less one. The problem with such a regression line is that it accounts for too much: every kink and outlyer in the means is fitted. A "true" functional relationship, on the other hand, generally implies a smooth trend line through the means.

The determination of a smooth trend line depends on the particular data. Although mathematical curve fitting and trend analysis procedures are left to other sources, we emphasize that a visual inspection of the graph of means is the best first step, and that the trend equation need not be a power polynomial: Power or logarithmic functions are often more common and interpretable with respect to psychological theory. Whatever the trend equation, the proportion of variance accounted for by that equation is given by the ratio of the sum of squares due to trend to the total sum of squares (SStrend / SStotal). This ratio, asssuming a judiciously chosen trend line, will give a more reasonable estimate of the proportion of variance accounted for by the true relationship between the performance measure and the secondary IV, all other factors excluded.

In particular, if the means appear to have a logarithmic trend (which implies that constant increments in performance correspond to constant proportionate increases in the secondary IV), then the "honest" proportion of variance accounted for by the secondary IV is directly provided by the coefficient of linear determination (r^2) between the performance measure and the logarithm of the secondary IV.

Recommendation 5

In a two-factor balanced equal-"n" orthogonal design, the between cell sum of squares (SScell) is equal to the sum of the sums of squares for each factor and their interaction: SScell = SSa + SSb + SSab. In this sense, the SScell exhausts all the statistical information available in the primary IVs. This statement is also true for designs involving more than two factors with appropriate inclusion of all relevant main effects and interactions. The SScell may be formed for all the Primary IVs or for just a select subset. A subset of the primary IVs might be selected when, for example, an optical invariant can be formed using only some of the primary IVs in an experiment. The ratio SScell/SStotal is the total proportion of variance in the performance measure accounted for by all the statistical information in the primary IVs and their interactions. We suggest that this ratio can then serve as a reference or benchmark level against which the strength of any secondary IV may be compared.

An index of how well a particular secondary IV (SIV) accounts for the data as compared to the (relevant) primary IVs is given by:

(SSsiv/SStotal)/(SScell/SStotal) = (SSsiv/SScell)

But, as was just argued (in Recommendation 4 and letting the one-way ANOVA SSbetween there equal the SSsiv here), SSsiv is too strong a measure and can be artificially be made equal to SScell by any artificial function that results in as many levels of the SIV as there are primary cells. A more "honest" procedure is to use the proportion of variance accounted for by a smooth regression line through the means of the SIV, viz., SStrend/SStotal. An index of how well the smoothed SIV function compares to the primary variables is given by:

(SStrend/SStotal) / (SScell/SStotal) = (SStrend/SScell)

As a special case, if a logarithmic trend is manifest, the r^2 for the log of the SIV may be used directly:

r^2/(SScell/SStotal)

Notice that no SIV, however defined, can account for more variance than that accounted for by the primary IVs from which it is formed. But the SIV does represent a legitimate alternative interpretation of the data and may account for more variance than any single primary IV or interaction.

The above technique needs further study. For example, the proportion of variance accounted for by the SIV, either from the one-way ANOVA or the trend analysis, is obtained from a set of data with unequal "*n*"s for the SIV levels. Whether or not this instance of unequal "*n*" affects the analysis in any material way remains to be determined. Another area to be investigated is the use of *SS*cell for comparison purposes. In an unequal "*n*" design, it is not generally true that the *SS*cell equals the sum of the sums of squares of the main effects plus their interactions. What an experimenter should do in such a situation is not yet totally clear. Hence, the above procedures are offered as tentative suggestions, but nevertheless some method must be developed to enable assessment of the effects of the SIVs. The suggested procedures do show promise. They are easy to use and to interpret and there is reason to believe that if they are not precisely on target, they are not far off. At the very least, they serve a heuristic purpose in choosing primary IVs for subsequent experiments.

Raw data vs. means. So far the discussion has assumed that the entire data set was being analyzed. The variance not due to cells, (*SS*total - *SS*cell)/(*SS*total), includes the effects of "pure error", individual differences, practice, etc. It can be argued that it is unfair to expect a theory to account for such variance when evaluating a model (Cohen & Cohen, 1975, p. 249). A simple way to exclude practice and observer effects is to perform a regression analysis on only the means of the variables under study. For example, the r^2 between the means of a performance measure and the log of the SIV indicates how well a logarithmic function fits the means, all practice and observer effects excluded. Such an r^2 , by itself, can be comparable, if not identical, with the ratio r^2/(*SS*cells/*SS*total) defined earlier for the entire data set. The r^2 obtained using only the means will, of course, have many fewer degrees of freedom associated with it than the r^2 for the entire raw data set and this may affect the significance level.

Conclusions

The main point of this paper is that the visual displays encountered in aviation psychology research unavoidably make available optical information in addition to the information they are designed to present. Hence, experiments designed to assess the utilization of different sources of information in aviation are subject to alternate interpretaion. The experimenter is then faced with the problem of determining which of several (partially) redundant sources of information is actually responsible for a pilot's performance. These problems will become more evident and more formidable when the experimenter turns control of the environmental and optical variables over to the pilot in fully interactive flight situations, simulated or actual.

Although no solution yet exists, some statistical procedures are tentatively proposed to determine the relative strength of each factor. Whatever the fate of these particular proposals, some assessment procedure must be found that is applicable to experimental research in aviation. Paradoxically, the situation of the aviation experimenter is more akin to that of the non-experimental field researcher and hence, the multiple regression techniques developed for many-factor non-experimental data may prove useful.

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APPENDIX B

DETECTION OF DESCENT IN THE ABSENCE OF OPTICAL FLOW ACCELERATION

Lawrence J. Hettinger, Rik Warren, and Dean H. Owen

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In James Gibson's discussions of properties of the optical flow pattern during aircraft landings (Gibson, 1958a, 1958b, Gibson, Olum & Rosenblatt, 1955), he maintained that the ability to execute a proper landing necessarily involved picking up two related types of visual information: (1) the optical magnification of textural elements and objects on the ground surface, and (2) the acceleration of the flow of optical texture elements in the optic array.

> Approach to a solid surface is specified by a centrifugal flow of the texture of the optic array. Approach to an object is specified by a magnification of the closed contour in the array corresponding to the edges of the object. A uniform rate of approach is accompanied by an accelerated rate of magnification (Gibson, 1958a, p. 188).

In a previous study (Owen, Warren, & Mangold, in press) it was observed that along with optical flow magnification (decrease in density) and optical flow acceleration as sources of information for descent, there existed at least a third source, optical splay. Optical splay is defined as the perspectival angle formed by an environmental feature, the "straight ahead" point on the horizon, and the vertical line below that point (Warren, 1980). As a pilot descends along a path slope, the angle or splay between texture discontinuities increases.

In an ideal fixed-wing landing approach, one in which the pilot approaches the surface of the ground by descending on a linear path slope at a fixed path speed, all three of these sources of information (optical flow acceleration, decrease in density, and increase in optical splay) are perfectly correlated with one another. Owen, Warren, and Mangold (in press) found that all three optical variables shared the same fractional rate of change across time in simulations of constant descent rate. One way to assess the functional utility of these three sources of optical information is to adopt an accretion/deletion paradigm in which one or more sources of information are selectively added to or removed from a scene (Owen & Warren, 1981). For example, in the case of optical splay, the use of only horizontal texture will effectively remove any splay information. Systematic variations in performance which correspond to the presence or absence of an optical variable should provide evidence of its functional utility.

It is clear, however, that it is not always possible to completely remove one source of optical information in a scene without thereby influencing the other variables with which it is correlated (Warren & Owen, in press). This complicates the task of assessing the functional utility of one particular source of information when performance is simultaneously affected by other variables whose characteristics may also be altered by removal of the variable of interest.

In the current study we chose to negate optical flow acceleration for the purpose of assessing an observer's sensitivity to descent based on fewer sources of information. Warren (1980) derived equations to specify global optical flow rate (GOFR) mathematically. In the case of a linear path slope ($\dot{z}/\dot{x} = k$), GOFR may be mathematically represented as the ratio of speed along the path slope ($\dot{s}$) to altitude (z). In the case of level flight at a constant forward velocity GOFR is a constant. However, in the case of descent the increase in GOFR is specified by the increasing value of the ratio $\dot{s}/z$ as altitude decreases. Therefore, in order to negate GOFR as information for descent it is necessary to make $\dot{s}/z$ a constant ($\dot{s}/z = k$). It was found by Warren (1980) that the necessary constraints on path speed and altitude in the case of deceleration along the path slope could be expressed in the fashion $\dot{s} = zk$, that is, deceleration along the path slope must be proportional to the loss in altitude in order to produce a constant GOFR. Jagacinski (personal communication) showed that one way to achieve a constant flow rate, is to exponentially decrease path speed on a linear path slope.

The distinction between an ideal fixed-wing landing approach and its concomitant flow rate, such as that described by Gibson (1958a), and the special type of "modified" approach we are interested in investigating is summarized in the following table:

Landing Approach	Descent Rate	Path Speed	Flow Rate
Fixed wing	Constant	Constant	Accelerating
Modified	Exponentially decreasing	Exponentially decreasing	Constant

Although our interest in this area is primarily theoretical, the condition of deceleration along the path slope is a typical landing approach for rotary-wing aircraft (Armstrong, Hofmann, Sanders, Stone, & Bowen, 1975) and is not an unusual approach for Vertical/Short Take-off and Landing (V/STOL) aircraft (Hennessy, Sullivan, & Cooles, 1980). "... Rotary-wing aircraft do not execute final approaches at fixed velocities as do fixed-wing aircraft, but rather reduce airspeed during this maneuver such that a near zero velocity is achieved at touchdown or at hover" (Armstrong et al., 1975, p. 2).

In this study the following three independent variables were orthogonally crossed: (1) Global optical flow rate was chosen in order to investigate its effects when constant throughout a trial. (2) Path slope was added to the design for the purpose of determining whether constant flow rate effects are independent of path slope. (3) Global optical texture density was included to assess whether the density of optical discontinuties has any effect on sensitivity to loss in altitude. If only flow rate is important, then varying texture density uniformly should have no effect. If "edge rate" (the rate at which edges of surface texture elements cross the field of view) is important, then varying texture density should have an effect. In either case the results are likely to have implications for the designers of flight simulation scenes. If varying texture density shows no effects on performance then designers may decide to invest less of their resources in design considerations of this type.

One advantage of the current design over that of Owen, Warren, and Mangold (in press) is that fractional descent rate ($\dot{z}_t/z_t = k$) becomes a constant rather than increasing so that its value is the same at trial initiation and at reaction time. This variable therefore becomes a within-event rather than a between-event invariant. In the latter case it is difficult to state with certainty what level of the variable observers are sensitive to, while in the former the value of the variable remains invariant throughout a particular trial.

Method

Apparatus and scenes. A special purpose computer developed to generate real-time transformations in a video projector display was used to produce 10-second sequences representing self motion over a flat surface comprised of square texture blocks. The ground surface simulated consisted of a rectilinear island 30.72 km long. Block size was varied by assigning adjacent blocks the same color so that there was no separating edge. Island width was a function of texture block width, since the number of vertical edges was fixed at 20. Three texture block sizes were used: 4.5 meters long by 4.5 meters wide, 18 meters long by 18 meters wide, and 72 meters long by 72 meters wide. The corresponding island widths were 85.5, 342, and 1368 meters respectively. Calibration of the ground surface simulation was carried out by means of a previously constructed template.

Texture blocks were filled in four colors: red, green, light blue, and dark blue. The colors were randomly assigned with the constraint that a color could not be repeated in the length dimension (beyond what was necessary to produce the appropriate texture lengths) while a color could be repeated only once in the width dimension. The non-textured area surrounding the island was black and the sky a bluish-gray.

The screen was 1.5 meters wide and 1.125 meters in height, resulting in a field of view 34.3 deg by 26.1 deg when viewed from 2.43 m. The horizon represented in the visual scene was positioned at

1.956 meters from the floor, which approximates the height of the observer's eye. Consequently, the horizon was .5625 meters from the top of the screen. Presentation of the experimental scenes was under the control of a Digital Equipment Corporation PDP-11/34 computer. The observer sat in a Singer-Link General Aviation Trainer-1 flight simulator with the motion base deactivated.

Design. It was determined from previous experimentation that a multiplier of two for adjacent levels of variables produced a satisfactory range of error rates. However, in the case of path slope this was not done in order to keep the observer's task at an appropriate level of difficulty (see Table 1). The following values for the primary independent variables were chosen to approximate those from previous experiments (Owen, Warren, Jensen, Mangold, & Hettinger, 1981). The subscript "o" indicates the initial value of a variable which changes over time; the subscript "t" indicates the value of a variable at time t. Eyeheight is denoted by h.

1. Initial altitude (z_o): 72 m.
2. Global flow rate ($\dot{s}_t/z_t = k$): .25, .5, and 1 h/s.
3. Initial global texture density (z_o/g): 1, 4, and 16 g/h.
4. Path slope (tan = $\dot{z}_t/\dot{x}_t = k$): .02, .04, and .06.

The value of the following secondary independent variables were determined as a direct function of the values of the primary independent variables (see Warren & Owen, in press).

1. Initial path speed ($\dot{s}_o$): 18, 36, and 72 m/s.
2. Ground texture size (g): 4.5, 18, and 72 m.
3. Initial path speed scaled in ground units ($\dot{s}_o/g$): .25, .5, 1, 2, 4, 8 and 16 g/s.

4. Initial forward velocity ($\dot{x}_o = \dot{s} \cos\alpha$): 17.9677, 17.9856, 17.9964, 35.9354, 35.9712, 35.9928, 71.8707, 71.9425, and 71.9856 m/s.

5. Initial descent rate ($\dot{z}_o = \dot{x} \tan\alpha$): .36, .72, 1.08, 1.44, 2.16, 2.88, and 4.32 m/s.

6. Fractional descent rate ($\dot{z}_t/z_t = k$): 0.05, 0.1, 0.15, 0.2, 0.3, 0.4, and 0.6 %/s.

Table 1 illustrates the full factorial combination of primary and secondary variables. Equipment constraints required a high starting altitude, so a single value was used. One altitude (z_o) value was crossed with three values of global flow rate ($\dot{s}_t/z_t = k$), initial global texture density (z_o/g), and path slope ($\dot{z}_t/\dot{x}_t = k$). The values of the six secondary independent variables are determined by the values of the four primary independent variables. Setting $\dot{z} = 0$ produced nine unique level scenes/events which were repeated three times for a total of 27 level scenes per block of trials. The 27 descent and 27 level trials were combined to form one block of 54 trials.

Procedure. The experimenter said, "Ready," then initiated the trial by means of the computer terminal. The observer was instructed to indicate whether the event displayed represented descent or level movement over the surface by pressing one of two appropriately designated buttons, either of which simultaneously stopped a millisecond timer and specified the observer's decision. The observer was unaware that time to respond was being recorded, but was encouraged to respond during the 10-second scene duration. Following the button press, the observer rated his confidence in the choice by means of a

Table 1

Inventory of Displayed Events and Mean Performance

Variables[a]

Event	1	2	3	4	5	6	7	8	9	10	11	12	13
Number	$\dot{z}_t/\dot{x}_t=k$	$\dot{s}_t/z_t=k$	z_o/g	g	$\dot{s}_o$	$\dot{x}_o$	z_o	$\dot{s}_o/g$	$\dot{z}_o/g$	$\dot{s}_t/x_t=k$	$\dot{z}_t/z_t=k$	ZERR	$\overline{RT}_c$
1	.02	.25	1	72.0	18	17.9964	72	.25	.005	1.0002	.005	76.8	7.699
2	.02	.25	4	18.0	18	17.9964	72	1.00	.02	1.0002	.005	58.9	7.970
3	.02	.25	16	4.5	18	17.9964	72	4.00	.08	1.0002	.005	60.7	6.288
4	.02	.50	1	72.0	36	35.9928	72	.50	.01	1.0002	.010	51.8	7.211
5	.02	.50	4	18.0	36	35.9928	72	2.00	.04	1.0002	.010	39.3	6.414
6	.02	.50	16	4.5	36	35.9928	72	8.00	.16	1.0002	.010	37.5	6.998
7	.02	1.00	1	72.0	72	71.9856	72	1.00	.02	1.0002	.020	5.4	5.056
8	.02	1.00	4	18.0	72	71.9856	72	4.00	.08	1.0002	.020	32.1	5.378
9	.02	1.00	16	4.5	72	71.9856	72	16.00	.32	1.0002	.020	23.2	5.331
10	.04	.25	1	72.0	18	17.9856	72	.25	.01	1.0008	.010	64.3	6.604
11	.04	.25	4	18.0	18	17.9856	72	1.00	.04	1.0008	.010	48.2	7.140
12	.04	.25	16	4.5	18	17.9856	72	4.00	.16	1.0008	.010	55.4	7.414
13	.04	.50	1	72.0	36	35.9712	72	.50	.02	1.0008	.020	17.9	5.136
14	.04	.50	4	18.0	36	35.9712	72	2.00	.08	1.0008	.020	10.7	5.985

Table 1, continued

Event	1	2	3	4	5	6	7	8	9	10	11	12	;3
Number	$\dot{z}_t/\dot{x}_t=k$	$\dot{s}_t/z_t=k$	z_o/g	g	$\dot{s}_o$	$\dot{x}_o$	z_o	$\dot{s}_o/g$	$\dot{z}_o/g$	$\dot{s}_t/x_t=k$	$\dot{z}_t/z_t=k$	ZERR	$\overline{RT}_c$
15	.04	.50	16	4.5	36	35.9712	72	8.00	.32	1.0008	.020	14.3	6.026
16	.04	1.00	1	72.0	72	71.9425	72	1.00	.04	1.0008	.040	1.3	3.881
17	.04	1.00	4	18.0	72	71.9425	72	4.00	.16	1.0008	.040	5.4	4.037
18	.04	1.00	16	4.5	72	71.9425	72	16.00	.64	1.0008	.040	1.8	4.254
19	.06	.25	1	72.0	18	17.9677	72	.25	.015	1.0018	.015	30.4	6.954
20	.06	.25	4	18.0	18	17.9677	72	1.00	.06	1.0018	.015	17.9	6.166
21	.06	.25	16	4.5	18	17.9677	72	4.00	.24	1.0018	.015	23.2	7.378
22	.06	.50	1	72.0	36	35.9354	72	.50	.03	1.0018	.030	14.3	5.581
23	.06	.50	4	18.0	36	35.9354	72	2.00	.12	1.0018	.030	1.8	5.372
24	.06	.50	16	4.5	36	35.9354	72	8.00	.48	1.0018	.030	1.8	5.460
25	.06	1.00	1	72.0	72	71.9425	72	1.00	.06	1.0008	.060	0.0	3.395
26	.06	1.00	4	18.0	72	71.9425	72	4.00	.24	1.0008	.060	0.0	3.277
27	.06	1.00	16	4.5	72	71.9425	72	16.00	.96	1.0008	.060	1.8	3.736

[a]Variables

1. z_o = Initial Altitude.

2. $\dot{z}_t/\dot{x}_t=k$ = Path Slope.

Table 1, continued

3. $\dot{s}_t / z_t = k$ = Global Optical Flow Rate.

4. z_o / g = Global Optical Density.

5. $\dot{s}_o$ = Initial Path Speed.

6. g = Ground Texture Size.

7. $\dot{s}_o / g$ = Path Speed Scaled in Ground Units.

8. $\dot{x}_o$ = Initial Forward Velocity.

9. $\dot{s}_t / \dot{x}_t = k$ = Path Speed as a Fraction of Forward Velocity.

10. $\dot{z}_t / z_t = k$ = Fractional Loss in Altitude.

11. $\dot{z}_o / g$ = Descent Rate Scaled in Ground Units.

12. %ERR = Percent Error.

13. $\overline{RT}_c$ = Mean Reaction Time (Correct Only).

__Note:__ A dot over a symbol indicates a derivative with respect to time. The subscript indicates the value of a variable at the initiation of an event (e.g., z_o) or at any time in the event (e.g., z_t). Ratios which remain constant throughout the event are indicated as equal to k.

three-point scale. "Three" represented very certain; "two," fairly certain; and "one," guessing. No feedback concerning performance was provided during the testing.

The 54 trials were presented in the same sequence for all observers. Trials were randomly assigned in the sequence with the constraint that no more than four level or four descent trials would occur sequentially. Each sequence took approximately 18 minutes to complete and each observer was given a 5 minute break between repetitions of the sequence. Each scene was displayed for 10 seconds with an average intertrial interval of 10 seconds. All testing was conducted in a darkened room.

Observers. Twenty-eight undergraduate students served as observers as partial fulfillment of a course requirement. All observers were male and claimed no prior experience in flight simulators, and all reported normal vision.

Results

The following summary scores were computed for each observer for each of the 27 cells in the experimental design: proportion errors; mean reaction time for all trials (correct plus error) and also for error-free trials only. Since proportion error scores and error-free reaction times come from entirely different trials, these two dependent variables were chosen for detailed presentation.

In a previous study (Owen, Warren, Jensen, Mangold, & Hettinger, 1981) it was noted that in experiments of this type a sufficiently large number of observations will generally provide statistical significance in the conventional sense for most of the independent variable effects. Therefore, in order to merit discussion in this paper, an independent variable must account for at least 1.5% of the variance in a dependent variable.

Primary optical variables. As Figures 1 and 2 show, proportion error and correct reaction time decreased significantly with increases in global optical flow rate ($\dot{s}_t/z_t = k$). This variable accounted for 9.4% and 2.8% of the variance in the proportion error and correct reaction time data, respectively, in both the one-way and fully crossed analyses of variance.

As shown in Figure 1, the effect of optical flow rate is maximized at the highest value of path slope, and vice versa. This significant interaction between flow rate and path slope accounted for 1.8% of the variance in the proportion error data. A steeper path slope matched with a more rapid flow rate results in fewer errors. As

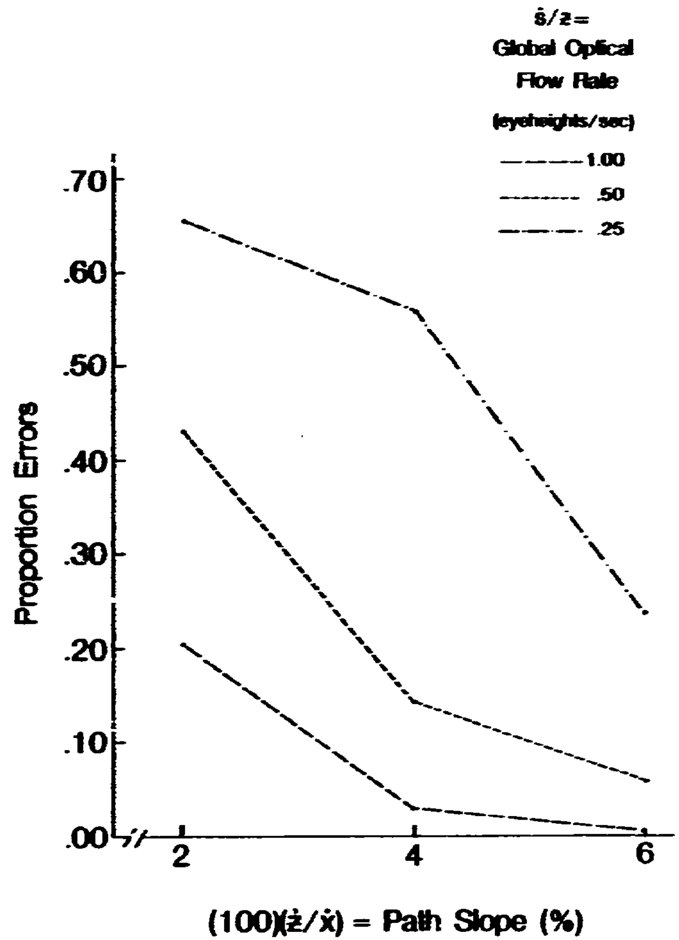

Figure 1. Proportion errors for the three levels of path slope crossed with the three levels of global optical flow rate.

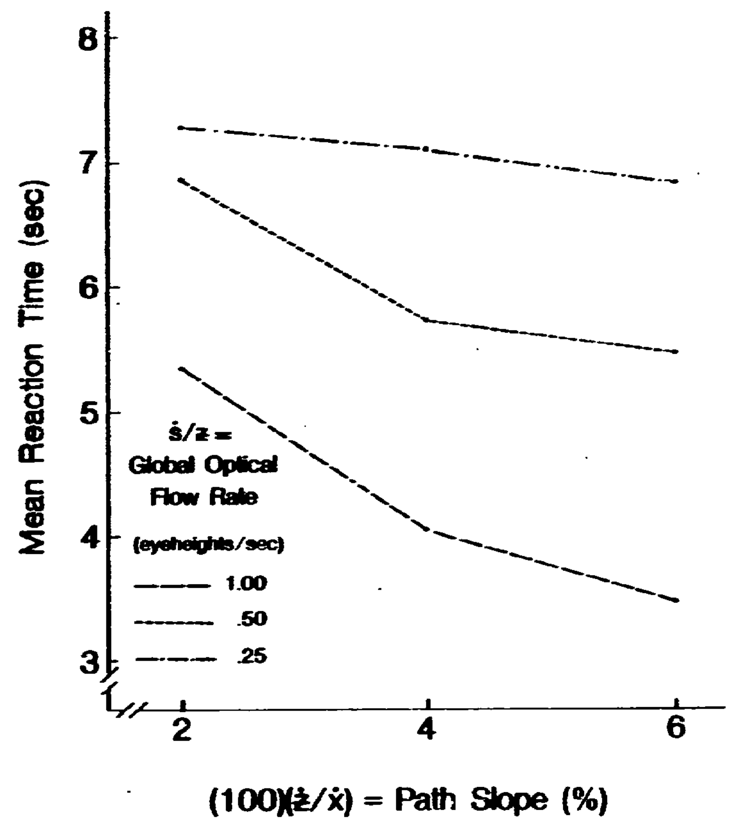

Figure 2. Mean correct reaction time for the three levels of path slope crossed with the three levels of global optical flow rate.

Table 2

One-Way Analysis of Variance Summary Tables

for Correct Reaction Time

Source	DF	SS	R^2%	F	p<F
Fractional Descent Rate (%)					
$\dot{z}_t/z_t=k$	6	1,787,533,982	22.9	55.35	.0001
Error	1115	6,001,623,456	-	-	-
Total	1121	7,789,157,438	-	-	-
Path Speed as a Fraction of Forward Velocity (%)					
$\dot{s}_t/\dot{x}_t=k$	2	218,861,603	2.81	16.18	.0001
Error	1119	7,570,295,835	-	-	-
Total	1121	7,789,157,438	-	-	-
Path Speed Scaled in Ground Units (g/sec)					
$\dot{s}_o/g$	6	469,114,393	6.0	11.91	.0001
Error	1115	7,320,043,045	-	-	-
Total	1121	7,789,157,438	-	-	-
Descent Rate Scaled in Ground Units (g/sec)					
$\dot{z}_o/g$	14	616,199,403	7.9	6.79	.0001
Error	1107	7,172,958,053	-	-	-
Total	1121	7,789,157,438	-	-	-

Table 3

One-Way Analysis of Variance Summary Tables

for Proportion Error

Source	DF	SS	$R^2\%$	F	p<F
		Fractional Descent Rate (%)			
$\underline{\dot{z}}_t/\underline{z}_t=\underline{k}$	6	73.726	25.5	85.74	.0001
Error	1505	215.679	-	-	-
Total	1511	289.405	-	-	-
		Path Speed as a Fraction of Forward Velocity (%)			
$\underline{\dot{s}}_t/\underline{\dot{x}}_t=\underline{k}$	2	27.155	9.4	78.12	.0001
Error	1509	262.250	-	-	-
Total	1511	289.405	-	-	-
		Path Speed Scaled in Ground Units (g/sec)			
$\underline{\dot{s}}_o/\underline{g}$	6	24.580	8.5	23.28	.0001
Error	1505	264.824	-	-	-
Total	1511	289.405	-	-	-
		Descent Rate Scaled in Ground Units (g/sec)			
$\underline{\dot{z}}_o/\underline{g}$	14	48.366	16.7	21.46	.0001
Error	1497	241.039	-	-	-
Total	1511	289.405	-	-	-

Table 4

Analysis of Variance Summary Tables

for the Primary Independent Variables

Source	DF	SS	$R^2\%$	F	p<F
		Proportion Errors			
$\dot{z}_t/\dot{x}_t=k$	2	27.155	7.4	96.87	.0001
z_o/g	2	0.869	0.3	3.10	.0453
$\dot{a}_t/z_t=k$	2	43.000	14.9	153.39	.0001
$\dot{z}_t/\dot{x}_t*z_o/g$	4	0.274	0.1	0.49	.7443
$\dot{z}_t/\dot{x}_t*\dot{a}_t/z_t$	4	5.310	1.8	9.47	.0001
$z_o/g*\dot{a}_t/z_t$	4	3.345	1.2	5.97	.0001
$\dot{z}_t/\dot{x}_t*z_o/g*\dot{a}_t/z_t$	8	1.310	0.5	1.17	.3150
Pooled Error	1485	208.143	71.9	-	-
Total	1511	289.408	100.0	-	-
		Correct Reaction Time			
$\dot{z}_t/\dot{x}_t=k$	2	218,861,603	2.8	20.46	.0001
z_o/g	2	26,259,582	0.3	2.45	.0864
$\dot{a}_t/z_t=k$	2	1,528,608,507	19.6	142.87	.0001
$\dot{z}_t/\dot{x}_t*z_o/g$	4	39,727,287	0.5	1.86	.1158
$\dot{z}_t/\dot{x}_t*\dot{a}_t/z_t$	4	46,879,251	0.6	2.19	.0680
$z_o/g*\dot{a}_t/z_t$	4	1,697,300	0.2	0.08	.9887
$\dot{z}_t/\dot{x}_t*z_o/g*\dot{a}_t/z_t$	8	69,157,917	0.8	1.62	.1158
Pooled Error	1095	5,857,965,989	75.2	-	-
Total	1121	7,789,157,438	100.0	-	-

Note. All main effects and interactions are tested using a pooled error term.

Figure 2 shows, this pattern is also present in the correct reaction time data, although the interaction in this case accounts for just .6% of the variance in the data.

Figures 3 and 4 show that the third primary independent variable, global optical density ($\underline{z}_o/\underline{g}$), exhibited little systematic influence on proportion error or correct reaction time performance. Optical density accounted for just .3% of the variance in both the proportion error and correct reaction time data.

Secondary optical variables. One-way analyses of variance indicated that fractional descent rate ($\dot{\underline{z}}_t/\underline{z}_t = \underline{k}$) accounted for 25.5% and 22.9% of the variance in the proportion error and correct reaction time data, respectively. As Figures 5 and 6 show, both proportion error and correct reaction time decreased with increase in fractional descent rate.

Descent rate scaled in ground units ($\dot{\underline{z}}_o/\underline{g}$) accounted for 16.7% and 7.9% of the variance in the proportion error and correct reaction time data, respectively. Proportion error and correct reaction time decreased significantly with increases in the levels of this variable (see Figure 7).

The ratio of path speed to forward velocity ($\dot{\underline{z}}_t/\dot{\underline{x}}_t = \underline{k}$) accounted for 9.4% and 2.8% of the variance in the proportion error and correct reaction time data, respectively. This variable is directly related to path slope ($\dot{\underline{z}}_t/\dot{\underline{x}}_t = \underline{k} = \tan\alpha$), representing a mathematical transformation of the path slope parameters. Figure 8 shows a general tendency towards decrease in proportion errors and correct reaction times with increases in the level of this variable.

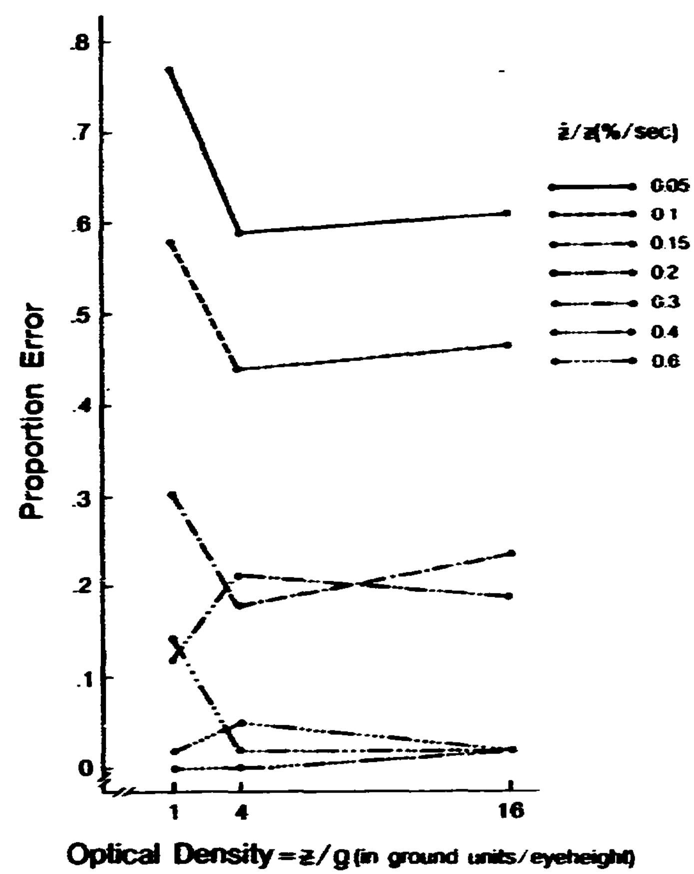

Figure 3. Proportion errors for the three levels of optical density crossed with the seven levels of relative rate of change in optical density and splay angle.

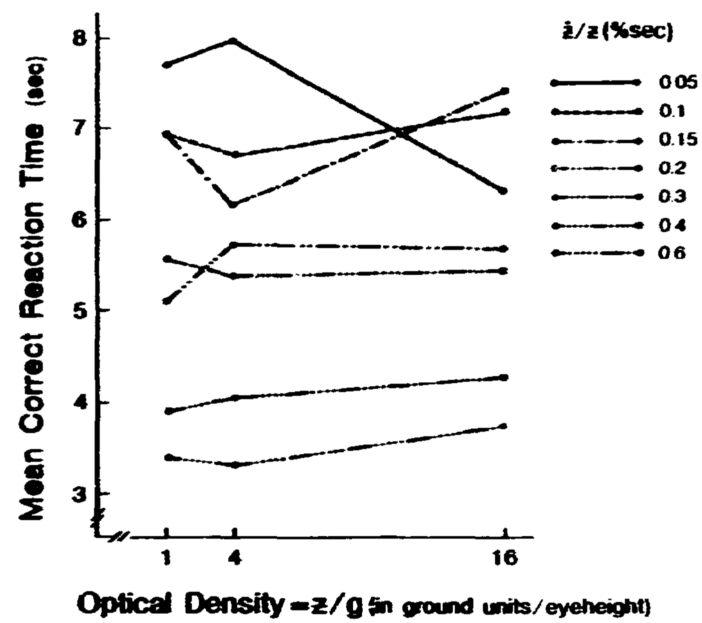

Figure 4. Mean correct reaction time for the three levels of optical density crossed with the seven levels of relative rate of change in optical density and splay angle.

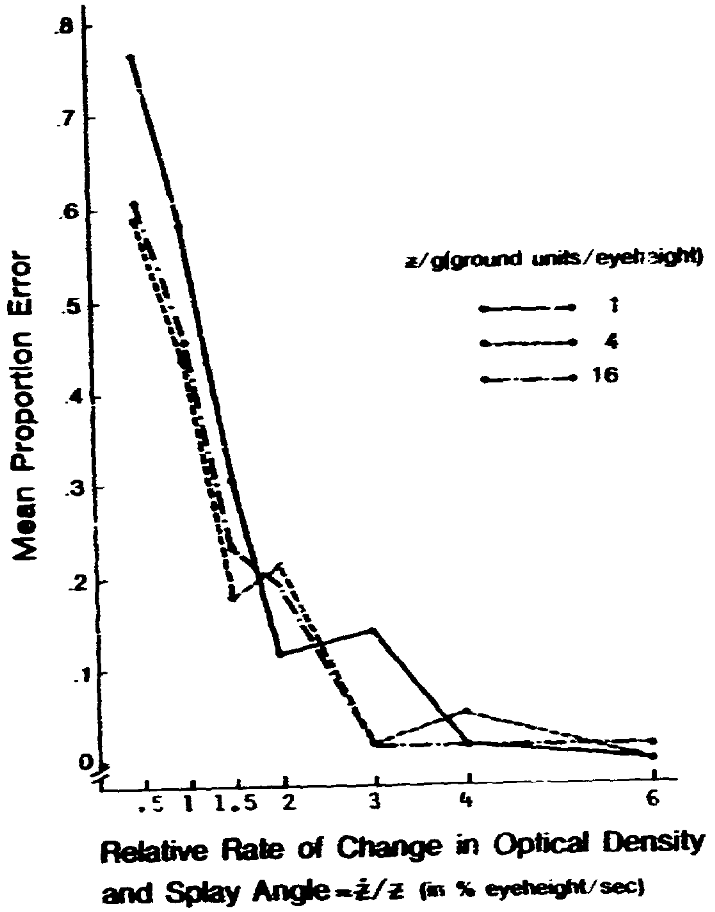

Figure 5. Proportion errors for the seven levels of relative rate of change in optical density and splay angle crossed with the three levels of optical density.

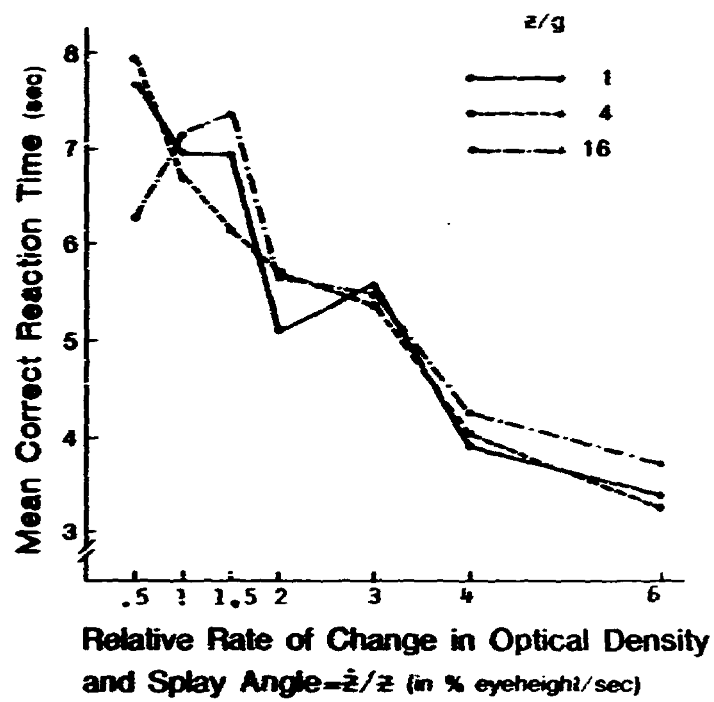

Figure 6. Mean correct reaction time for the seven levels of relative rate of change in optical density and splay angle crossed with the three levels of optical density.

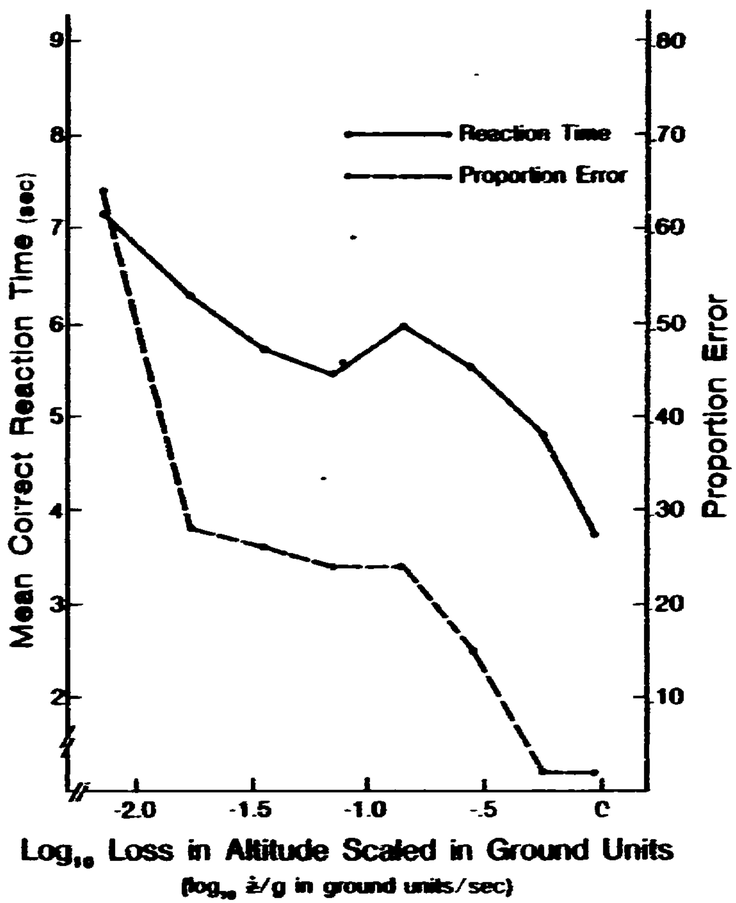

Figure 7. Proportion error and mean correct reaction time for the logarithm of the eight levels of loss in altitude scaled in ground units.

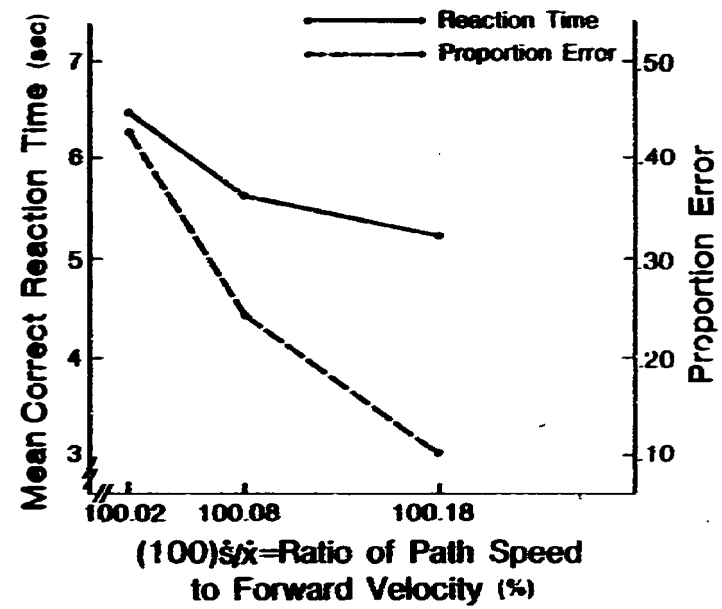

Figure 8. Proportion error and mean correct reaction time for the three levels of the ratio of path speed to forward velocity.

Path speed scaled in ground units ($\dot{s}_0/g$) accounted for 8.5% and 6.0% of the variance in the proportion error and correct reaction time data, respectively. Figure 9 shows a general trend toward decrease in both proportion error and correct reaction time scores with increases in the value of path speed scaled in ground units.

Multiple regression analyses. In an orthogonal experimental design, none of the primary independent variables correlate with one another, by definition. However, because many of the primary and secondary optical variables correlate to a greater or lesser extent with one another in this experimental design (and in actual flight), a stepwise multiple regression analysis was conducted in order to assess the unique contribution of each optical variable. These analyses indicated that fractional descent rate ($\dot{z}_t/z_t = k$) accounted for the greatest variance: 17.5% and 20.7% of the variance in the proportion error and correct reaction time data, respectively. Global flow rate ($\dot{s}_0/z$) accounted for an additional 4.7% of the proportion error data and 2.0% of the correct reactio time data. No other variable achieved the 1.5% criterion for discussion (see Tables 5 and 6).

Owen et al. (1981) found that converting the values of optical variables to a log scale produced functions which approximated those from Fechnerian psychophysical scaling, that is, equal ratio increments in stimulation produce equal interval increments in performance. For this reason, all optical variables in this study were converted to a $\log_{10}$ scale and were analyzed once again by stepwise multiple regression. Under this transformation $\log_{10}$ fractional descent rate ($\dot{z}_t/z_t = k$) accounted for 23.6% of the variance in the proportion error

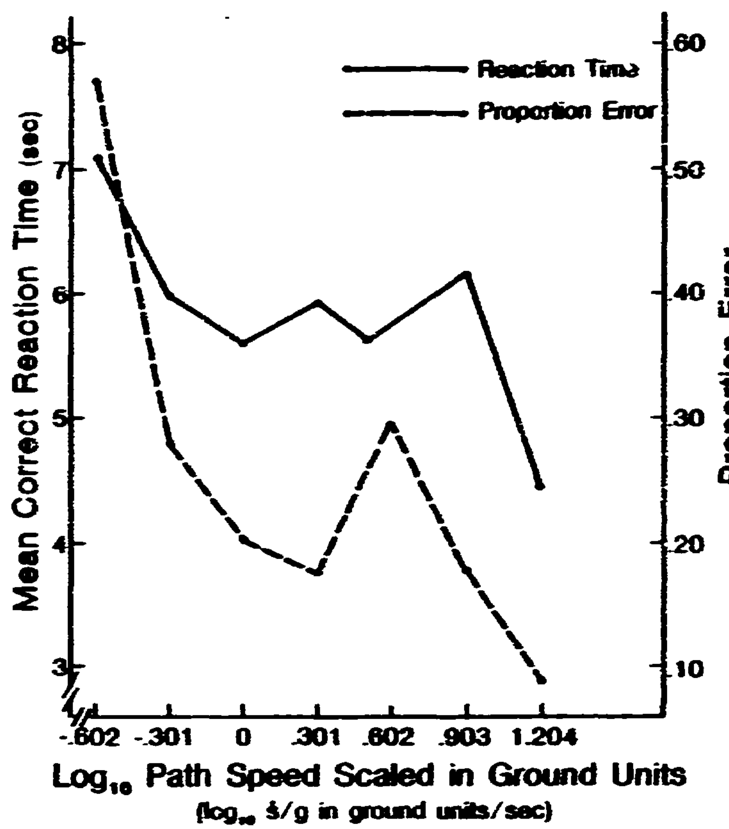

Figure 9. Proportion error and mean correct reaction time for the logarithm of the seven levels of path speed scaled in ground units.

data. No other variable achieved the 1.5% criterion. Log_{10} fractional descent rate accounted for 20.7% of the variance in the correct reaction time data, and $\log_{10}$ path slope ($\dot{z}_t/\dot{x}_t = k$) accounted for an additional 1.5% (see Tables 5, 6). Log_{10} $\dot{z}_t/z_t$ accounts for more variance than $\dot{z}_t/z_t$ because the former represents a smooth curve through the data (providing a better fit), while the latter represents a linear function.

Table 5

Stepwise Multiple Regression Analyses Summary Table

for Proportion Error

Global Optical Variables

Step	Source	DF	SS	R^2%	F	p<F
1	$\dot{z}_t/z_t=k$	1	50.741	17.53	321.03	.0001
	Error	1510	238.664	-	-	-
	Total	1511	289.405	-	-	-
2	$\dot{z}_t/\dot{x}_t=k$	2	52.505	18.14	167.22	.0001
	Error	1509	236.900	-	-	-
	Total	1511	289.405	-	-	-
3	$\dot{s}_t/z_t=k$	3	65.979	22.80	148.44	.0001
	Error	1508	223.426	-	-	-
	Total	1511	289.405	-	-	-
4	z_o/g	4	66.266	22.90	111.88	.0001
	Error	1507	223.139	-	-	-
	Total	1511	289.405	-	-	-
	Log$_{10}$ of Global Optical Variables					
1	Log $\dot{z}_t/z_t=k$	1	68.280	23.60	466.26	.0001
	Error	1510	221.125	-	-	-
	Total	1511	289.405	-	-	-
2	Log z_o/g	2	68.851	23.79	235.54	.0001
	Error	1509	220.554	-	-	-
	Total	1511	289.405	-	-	-

Note Each successive step contains the variables from the preceding step(s). All variables not included in the summary table failed to reach the .05 significance level for inclusion in the model.

Table 6

Stepwise Multiple Regression Analyses Summary Table

for Correct Only Reaction Time

Global Optical Variables

Step	Source	DF	SS	R^2%	F	p<F
1	$\dot{z}_t/z_t$=k	1	1,611,785,447	20.69	292.23	.0001
	Error	1120	6,177,371,991	-	-	-
	Total	1121	7,789,157,438	-	-	-
2	$\dot{s}_t/z_t$=k	2	1,765,905,430	22.67	164.03	.0001
	Error	1119	6,023,252,007	-	-	-
	Total	1121	7,789,157,438	-	-	-
3	$\dot{z}_o/g$	3	1,781,836,541	22.88	110.54	.0001
	Error	1118	6,007,320,897	-	-	-
	Total	1121	7,789,157,438	-	-	-
4	$\dot{z}_t/\dot{x}_t$=k	4	1,784,726,672	22.91	83.00	.0001
	Error	1117	6,004,430,766	-	-	-
	Total	1121	7,789,157,672	-	-	-
	Log_{10} Global Optical Variables					
1	Log $\dot{z}_t/z_t$=k	1	1,608,830,046	20.65	291.55	.0001
	Error	1120	6,180,327,391	-	-	-
	Total	1121	7,789,157,672	-	-	-
2	Log $\dot{z}_t/\dot{x}_t$=k	2	1,732,090,297	22.24	160.00	.0001
	Error	1119	6,057,067,141	-	-	-
	Total	1121	7,789,157,438	-	-	-

Note. Each successive step contains the variables from the preceding step(s). All variables not included in the summary table failed to reach the .05 significance level for inclusion in the model.

Discussion

The results of this study, when compared with those of Owen, Warren, and Mangold (in press), indicate that at comparable levels of fractional descent rate ($\dot{z}_t/z_t = k$) observers make fewer errors and take longer to respond (compare Table 1 and Table 7). The fact that error rates were higher in the previous study appears to be counter-intuitive in the sense that the removal of information (acceleration of optical flow rate) should normally not facilitate the performance of a task. On the other hand, the longer reaction times in the current study may indicate that observers were taking longer to search for descent information and, as a result, were more accurate. Very low levels of fractional descent rate led to very high error rates. The implication of this for future studies is to vary fractional descent rate within ranges where observer's performance is more accurate.

Optical flow acceleration ($\dot{s}_t/z_t = k$) accounted for the most variance of all the primary independent variables, and its effect was largely independent of any other variable. The only exception to this was a significant interaction with path slope ($\dot{z}_t/\dot{x}_t = k$) in the proportion error data. However, as the multiple regression analyses indicate (see Tables 5, 6), optical flow rate did not account for as much variance in the data as did fractional descent rate.

On the whole, detection of descent appears to be both faster and more accurate the greater the optical flow rate. However, performance was probably better at the higher values of flow rate because of the

Table 7

Inventory of Display Events and Mean Performance from the Owen, Warren, and Mangold Experiment

Event Number	1 $\dot{x}$	2 $\dot{z}$	3 z_o	4 $\frac{\dot{z}}{z_o}$	5 $\frac{\dot{x}}{z_o}$	6 $\frac{\dot{z}}{\dot{x}}$	7 $\left(\frac{\dot{z}}{z_o}\right)\left(\frac{\dot{x}}{z_o}\right)$	8 %ERR	9 $\overline{RT}_{c+e}$	10 $\overline{con}$
1.	18	1.25	20	6.25	.900	.069	5.63	5	2.750	2.90
2.	18	1.25	40	3.13	.450	.069	1.41	20	3.902	2.20
3.	18	1.25	80	1.56	.225	.069	.35	25	5.248	2.00
4.	18	2.50	20	12.50	.900	.139	11.25	5	1.868	3.00
5.	18	2.50	40	6.25	.450	.139	2.81	0	2.775	2.75
6.	18	2.50	80	3.13	.225	.139	.70	10	3.895	2.65
7.	18	5.00	20	25.00	.900	.278	22.50	5	1.876	2.95
8.	18	5.00	40	12.50	.450	.278	5.63	5	1.954	2.90
9.	18	5.00	80	6.25	.225	.278	1.41	5	2.389	2.90
10.	36	1.25	20	6.25	1.800	.035	11.25	10	3.298	2.75
11.	36	1.25	40	3.13	.900	.035	2.81	25	5.033	2.15
12.	36	1.25	80	1.56	.450	.035	.70	25	5.113	2.05
13.	36	2.50	20	12.50	1.800	.069	22.50	5	1.811	2.95
14.	36	2.50	40	6.25	.900	.069	5.63	15	3.268	2.75
15.	36	2.50	80	3.13	.450	.069	1.41	20	3.782	2.35
16.	36	5.00	20	25.00	1.800	.139	45.00	10	1.275	3.00
17.	36	5.00	40	12.50	.900	.139	11.25	0	1.624	2.95
18.	36	5.00	80	6.25	.450	.139	2.81	0	3.297	2.30
19.	72	1.25	20	6.25	3.600	.017	22.50	5	3.581	2.55
20.	72	1.25	40	3.13	1.800	.017	5.63	25	4.287	2.15

Variables[a]

Table 7, continued

Event Number	1 $\dot{x}$	2 $\dot{z}$	3 z_o	4 $\frac{\dot{z}}{z_o}$	5 $\frac{\dot{x}}{z_o}$	6 $\frac{\dot{z}}{\dot{x}}$	7 $\left(\frac{\dot{z}}{z_o}\right)\left(\frac{\dot{x}}{z_o}\right)$	8 %ERR	9 $\overline{RT}_{c+e}$	10 $\overline{con}$
21.	72	1.25	80	1.56	.900	.017	1.41	45	4.772	2.05
22.	72	2.50	20	12.50	3.600	.035	45.00	0	2.073	2.95
23.	72	2.50	40	6.25	1.800	.035	11.25	10	3.675	2.40
24.	72	2.50	80	3.13	.900	.035	2.81	10	4.015	2.35
25.	72	5.00	20	25.00	3.600	.069	90.00	5	1.105	3.00
26.	72	5.00	40	12.50	1.800	.069	22.50	10	2.128	2.90
27.	72	5.00	80	6.25	.900	.069	5.63	15	3.525	2.75

Variables[a]

[a]Variable	Description
1	$\dot{x}$ = Forward velocity.
2	$\dot{z}$ = Descent rate.
3	z_o = Initial altitude.
4	$\dot{z}/z_o$ = Initial fractional rate of change in global optical flow, density, and splay angle (in %/sec).
5	$\dot{x}/z_o$ = Initial optical flow rate (in the special case of level flight).
6	$\dot{z}/\dot{x}$ = Path slope (in %).
7	$(\dot{z}/z_o)(\dot{x}/z_o) \cong$ Initial global optical flow acceleration.
8	%ERR = Percent error.
9	$\overline{RT}_{c+e}$ = Mean reaction time (correct plus error).
10	$\overline{con}$ = Mean confidence rating.

(A dot over a symbol indicates a derivative with respect to time. The subscript indicates the value of a variable at the initiation of an event (t_o). All other values are constant throughout the event.)

fact that other sorts of optical information for descent, such as optical splay and density, were changing more rapidly under these conditions.

Another interesting result of this study is the fact that a 3-fold variation in initial optical texture density ($\underline{z}_0/\underline{g}$) appeared to have such a negligible effect. Denton (1980) found that exponentially decreasing the distance between painted lines on a road surface had a significant effect on driver's perception of egospeed, perhaps causing them to perceive their forward speed as increasing when their actual speed was not. The fact that Denton did not include a control condition in his design (equal spacing of pointed lines) makes it difficult to argue that his results are conclusive. Buckland, Monroe, and Mehrer (1977, 1979) found that varying texture density at the approach end of a runway had a significant effect on reducing sink rate at touchdown. However, it may be the case that in the present study ground texture density had little influence on performance because impending contact with the ground surface was not a certainty. In the Denton and Buckland et al. studies the close proximity between the observer and the ground surface made attending to the characteristics of optical density more crucial. The fact that fractional descent rate was so highly significantly indicates that the <u>initial</u> density of the ground surface is much less important than the relative rate of change in density across time.

The fact that initial optical texture density had no significant effect on performance under the conditions of this experiment indicates that edge rate information may be of little use in the detection of descent. Given a large enough number of findings of this nature, designers of simulator scenes would not have to overly concern themselves with details of surface texture density. However, as previously noted, density information may be more informative given impending contact with the ground surface. Edge rate information may also be of more use when the observer must judge his forward speed rather than his attitudinal relation to the surface of the earth.

The results also point to the need to further investigate the information for descent specified by fractional descent rate. Although not a primary independent variable in this study, this variable accounted for more variance in performance than any other. This is probably due to the fact that fractional descent rate is specified by the relative rate of change in optical density and splay angle. By using as a criterion demonstration of a functional optical invariant, Owen et al. (1981) found that the relative rate of change in fractional descent rate was apparently the crucial factor in observers' performance. The greater the relative rate of change, the more accurate performance tends to be. The advantage of the current design was in producing within-event values of fractional descent rate which were invariant. We are therefore able to conclude with a greater degree of confidence that changes in the level of the controlled variable have the observed effect on performance.

The possibility exists that fractional descent rate is specified by more sources of optical information than we have so far isolated. The further deletion of optical information specifying fractional descent rate may identify any other sorts of functional optical information which may exist. Perhaps a more logical experimental design would include fractional descent rate as a primary, rather than a secondary independent variable. The fact that descent was consistently detected by the observers despite constant optical flow rate indicates that the critical visual information is indexed by other specifiers of fractional descent rate.

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APPENDIX C

INDIVIDUAL DIFFERENCES IN SENSITIVITY TO GLOBAL OPTICAL FLOW VARIABLES

Dean H. Owen and Lawrence J. Hettinger

APPENDIX C

INDIVIDUAL DIFFERENCES IN SENSITIVITY TO GLOBAL OPTICAL FLOW VARIABLES

Dean H. Owen Lawrence J. Hettinger

The Ohio State University

Variation among subjects and interactions of subjects with the factors of major interest often account for a major proportion of the total variance in an experiment. These sources of variation are typically considered a necessary evil since they serve as "error" terms in an analysis of variance. A researcher is often ecstatic when differences of theoretical and/or practical interest reach some traditional level of statistical significance, even though the variable accounts for only a small proportion of the total variance.

In the greater scheme of understanding perception, behavior, and their relationships, variation in subjects cannot be treated in the same category with error of measurement. It forms, rather, an important set of phenomena to be explained in their own right. More to the point, it is the stuff of individual differences in skill and changes in skill: the factors that result in one person being better than another at some task or one person improving faster and/or reaching a higher asymptotic level of skill. It is the major focus of interest when individuals are to be selected for training or for more difficult or responsible tasks. It should be a major consideration when decisions are made to remove an individual from a skilled position.

This paper represents an initial attempt to explore individual differences in sensitivity to global optical flow variables that we have isolated in our studies of the perception of one's own motion. These optical variables are presumed to be useful in guiding locomotion, and later research will be concerned with individual differences in *producing* optical variations and invariants rather than simply *reacting* to them.

Individual differences in detection of loss in speed. In an earlier experiment (Owen, Warren, Jensen, Mangold, & Hettinger, 1981), we demonstrated that in distinguishing deceleration from constant speed observers are sensitive to visual information specifying fractional loss in speed. When fractional loss is greater, error rates are lower and time taken to detect deceleration is shorter. In addition, we discovered that fractional loss is a *functional invariant*, that is, when the ratio of deceleration ($\ddot{x}$) to forward velocity ($\dot{x}$) is a constant (regardless of the particular values of $\ddot{x}$ and $\dot{x}$), performance is the same. We have termed this ratio $(x/\dot{x})$ *global optical flow damping*. Global optical flow deceleration ($\ddot{x}/z$, where z = altitude or eyeheight) did not show this relationship, leading us to believe that flow deceleration plays a subordinate role, that is, being more or less detectable depending on the flow rate ($\dot{x}/z$) on which it is superposed $((\ddot{x}/z)/(\dot{x}/z) = (\ddot{x}/\dot{x}))$.

The four subjects shown in Figure C-1 were selected from the total of 42 subjects to reveal the broad range of individual differences in time to correctly detect deceleration. Errors were not frequent enough to produce orderly curves, but Table C-1 shows that individual error rates varied from 5% to 41%. Table C-1 also reveals a division of subjects into two groups: those who made errors (N=11) when flow damping had its highest value ($\ddot{x}/\dot{x}_0$ = .34) versus those who did not (N=31). These two groups show no difference in mean error rates over the other damping levels, indicating that about a quarter of the subjects were confused by the fact that

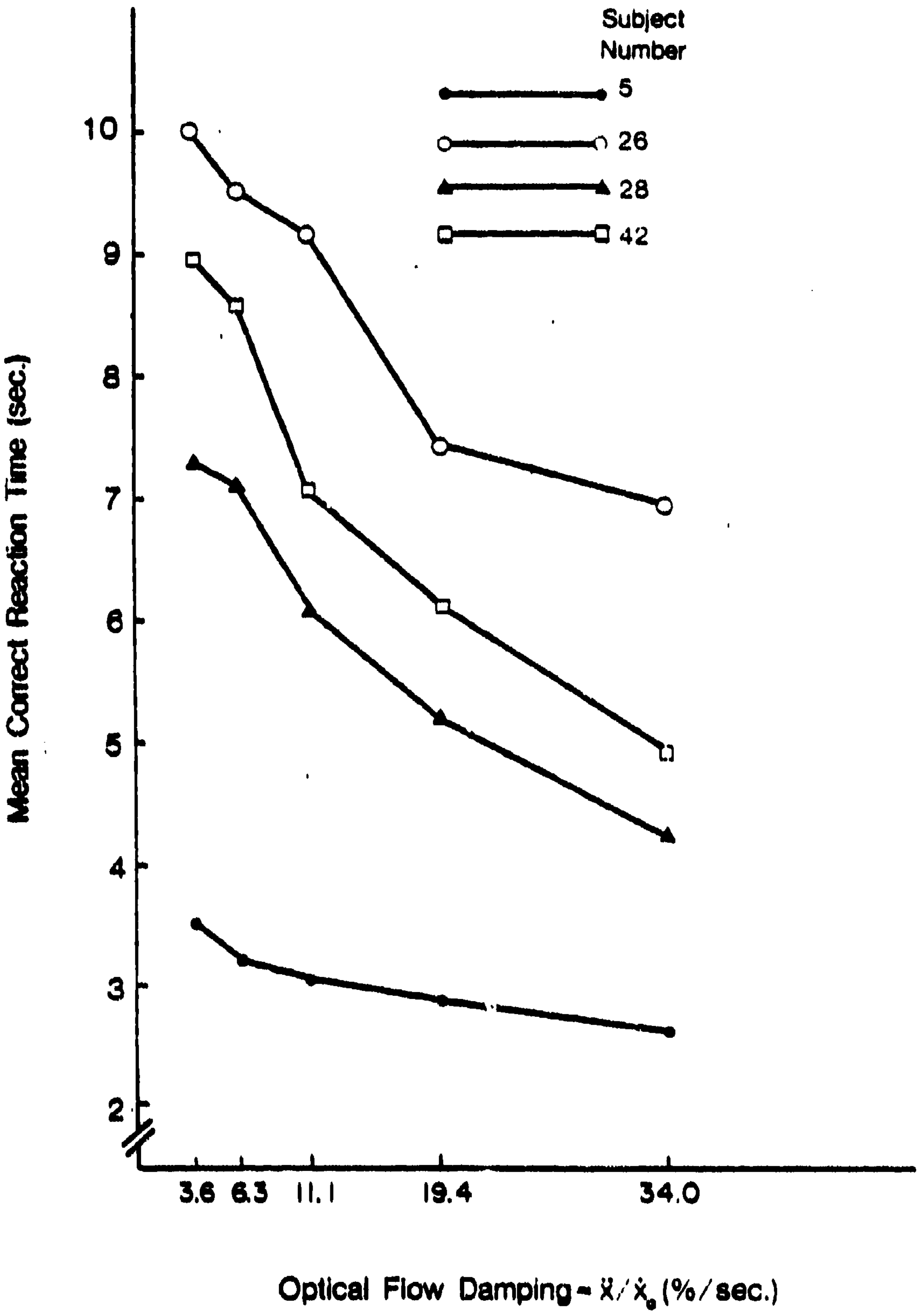

Figure C-1. Mean reaction times, from trials on which the subjects correctly detected deceleration, as a function of initial fractional loss in speed.

Table C-1

Proportion Errors
for Individual Subjects at Each Level of Fractional Loss in Speed

Subject Number	Initial Fractional Loss in Speed = $100\,\ddot{x}/\dot{x}_o$ (in %/sec)					
	3.6	6.3	11.1	19.4	.34	Mean
1	.22	.33	.11	.06	.11	.16
2	.44	.00	.00	.00	.00	.05
3	.56	.17	.00	.00	.00	.10
4	.67	.56	.07	.00	.00	.22
5	.78	.11	.15	.06	.22	.20
6	.67	.28	.07	.00	.00	.16
7	.22	.17	.04	.00	.00	.07
8	.78	.44	.15	.00	.00	.23
9	.56	.17	.07	.00	.00	.12
10	.22	.39	.33	.06	.00	.23
11	.78	.44	.37	.06	.00	.32
12	.78	.22	.04	.06	.11	.17
13	.78	.22	.04	.00	.00	.15
14	.56	.39	.30	.00	.00	.25
15	.56	.28	.19	.00	.00	.19
16	.33	.11	.04	.00	.00	.07
17	.78	.61	.30	.06	.00	.33
18	.89	.33	.00	.00	.00	.17
19	.56	.06	.07	.00	.00	.10
20	.89	.44	.48	.06	.00	.37
21	.78	.00	.04	.00	.11	.11
22	.44	.28	.07	.00	.00	.14
23	.56	.11	.07	.06	.00	.12
24	.89	.22	.11	.00	.00	.19
25	1.00	.61	.11	.00	.00	.28
26	.78	.67	.04	.06	.33	.30
27	.78	.33	.07	.00	.00	.19
28	.11	.00	.07	.00	.11	.05
29	.78	.17	.15	.00	.00	.17
30	.56	.17	.07	.00	.00	.12
31	.33	.06	.04	.00	.00	.06
32	.67	.17	.00	.00	.00	.11
33	.89	.61	.15	.06	.11	.31
34	.67	.22	.04	.06	.00	.15
35	.78	.72	.33	.22	.00	.41
36	.78	.17	.07	.00	.00	.15
37	.56	.44	.15	.06	.11	.23
38	.67	.22	.00	.00	.33	.16
39	.89	.11	.00	.00	.00	.12
40	.78	.33	.22	.00	.00	.23
41	.33	.22	.11	.00	.00	.12
42	.78	.22	.26	.00	.11	.23
Mean	.64	.28	.12	.02	.04	.18

motion decelerated to a halt part way through the 10-sec trial.

Figure C-2 shows a scatter-plot of the 42 subjects by their means on the two dependent variables. Although there is a suggestion of a speed/accuracy tradeoff over subjects, in general, there is a great deal of dispersion.

Individual differences in detection of loss in altitude. Two experiments have been conducted to date, one holding descent rate ($\dot{z}$) constant throughout a 10-sec trial (Owen, Warren, & Mangold, in press), the other holding fractional loss in altitude ($\dot{z}/z$) constant (Hettinger, 1981). When descent rate is constant, global optical flow accelerates throughout the event. Fractional loss in altitude also accelerates with constant descent rate, and the results indicated that this variable was a functional invariant when subjects were asked to distinguish descent from level self motion. Global optical flow acceleration ($(\dot{s}/z)(\dot{z}/z)$, where $\dot{s}$ = path speed) was not a functional invariant and appears instead to have a subordinate role. Flow acceleration is more or less detectable depending on the flow rate on which it is superposed. Under these conditions, fractional increase in flow rate is identical to fractional loss in altitude ($(\dot{s}/z)(\dot{z}/z)/(\dot{s}/z) = (\dot{z}/z)$).

Figure C-3 shows the mean time to correctly detect loss in altitude as a function of fractional descent rate for three of the 20 subjects in the Owen et al. (in press) experiment with constant descent rates. The subjects were again chosen to illustrate the broad range of individual differences.

Table C-2 shows the error rates for all 20 subjects, which range from 0% to 44%. There are four subjects who were confused by the fact

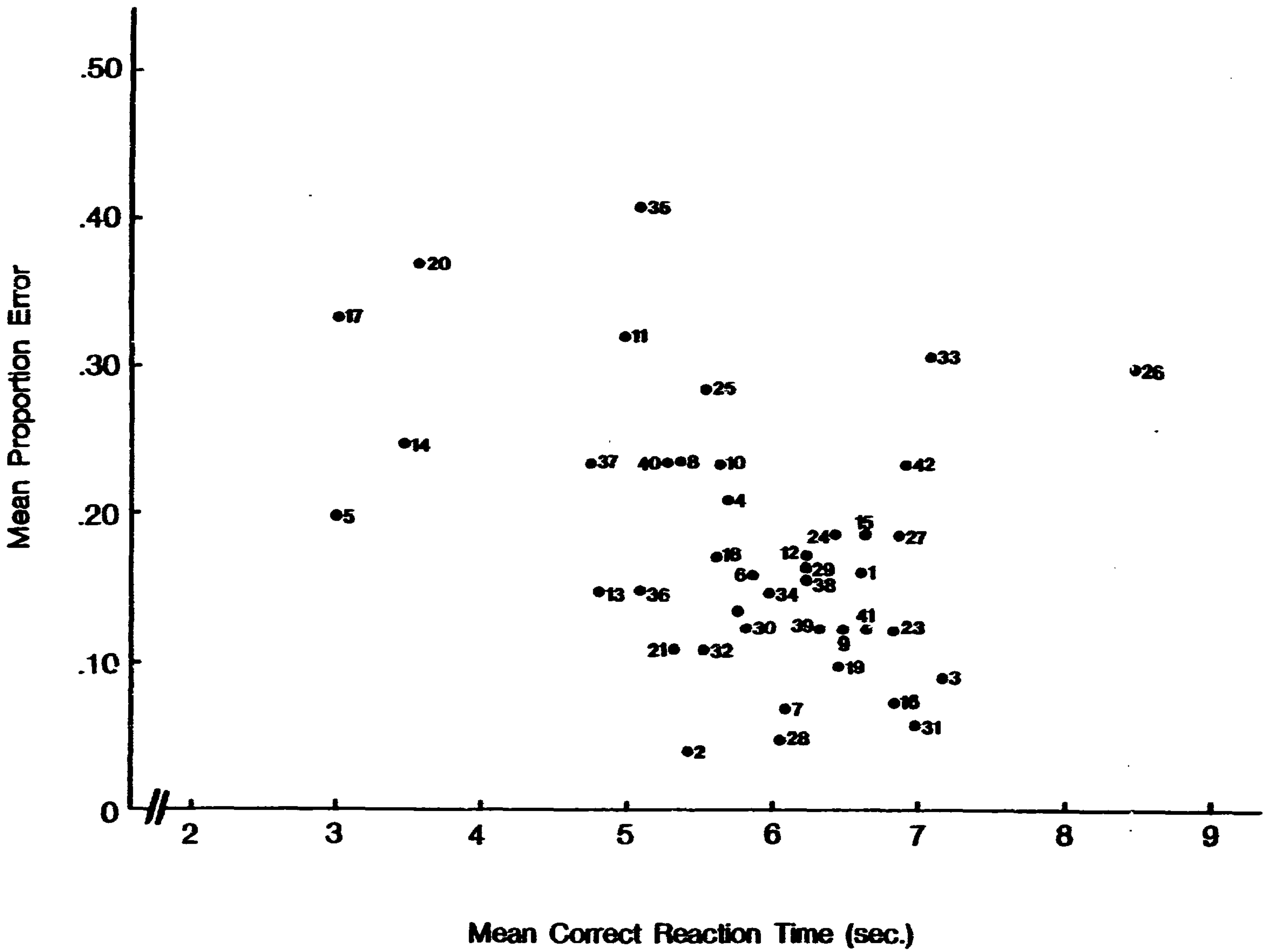

Figure C-2. Scatterplot of the 42 subjects in the Owen, Warren, Jensen, Mangold, and Hettinger (1981) experiment on detection of loss in speed. The subject numbers correspond to those in Figure C-1 and Table C-1.

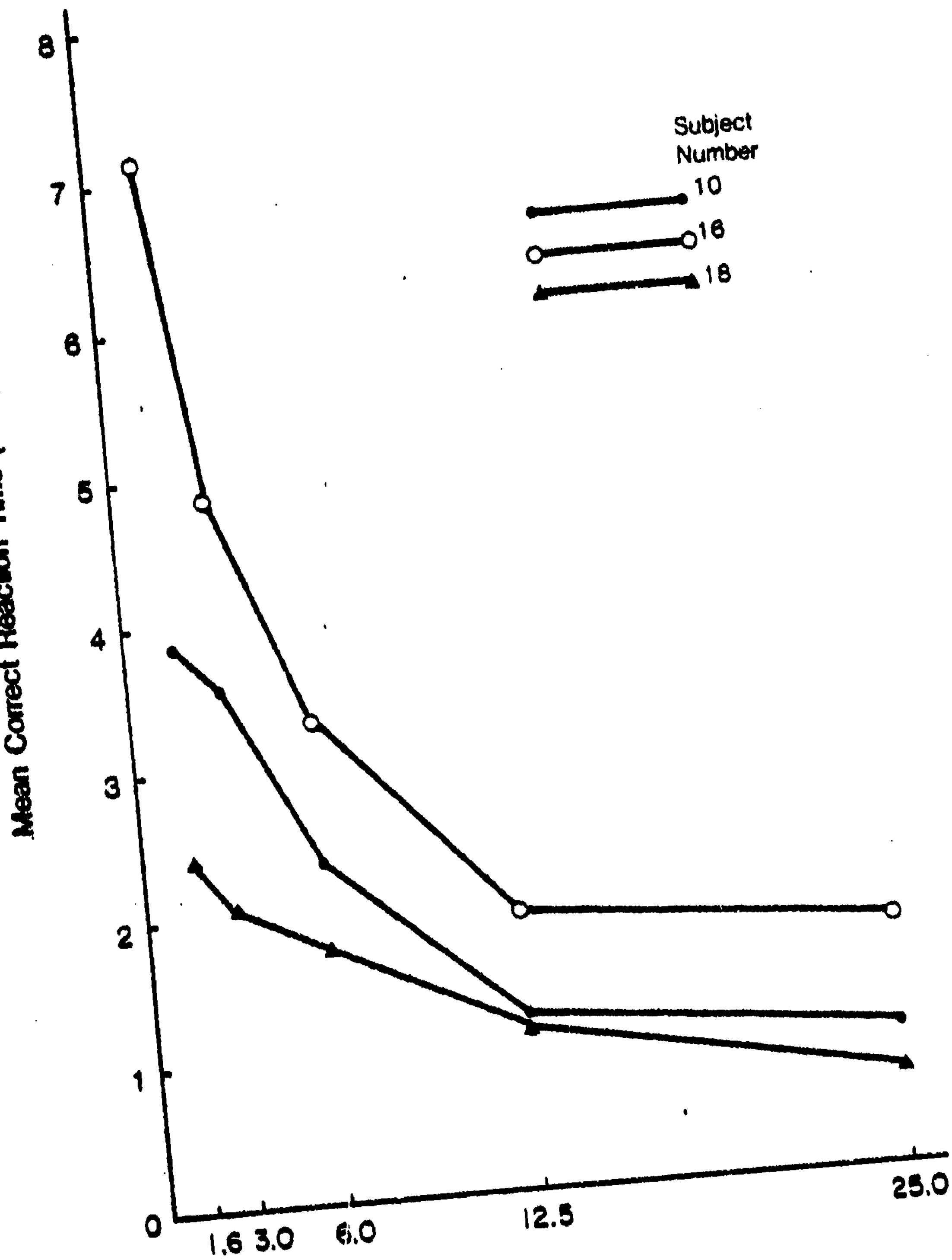

Figure C-3. Mean reaction times, from trials on which the subjects correctly detected descent, as a function of initial fractional loss in altitude.

Table C-2

Proportion Errors
for Individual Subjects at Each Level of Fractional Loss in Altitude

Subject Number	Initial Fractional Loss in Altitude = 100 $\dot{z}/z_o$ (in %/sec)					
	1.56	3.13	6.25	12.5	.25	Mean
1	.33	.17	.00	.00	.00	.07
2	.33	.33	.11	.00	.33	.19
3	.33	.33	.00	.00	.33	.15
4	.00	.17	.00	.00	.00	.04
5	.67	.50	.33	.00	.00	.30
6	.33	.50	.00	.00	.00	.15
7	.00	.00	.00	.17	.00	.04
8	.00	.00	.00	.00	.00	.00
9	1.00	.17	.00	.00	.00	.15
10	.33	.00	.11	.00	.00	.07
11	.67	.33	.00	.00	.00	.15
12	.33	.00	.11	.00	.00	.07
13	.00	.00	.11	.00	.00	.04
14	.00	.00	.00	.00	.00	.00
15	.33	.17	.00	.00	.00	.07
16	.33	.33	.00	.00	.00	.11
17	.67	.17	.44	.67	.33	.44
18	.00	.00	.11	.00	.00	.04
19	.00	.00	.00	.00	.00	.00
20	.67	.50	.11	.00	.33	.26
Mean	.32	.18	.07	.04	.07	.12

that trials for the highest rate of fractional loss were cut short when contact was made with the ground. Included in that group is Subject 17 who probably should have been excluded from the group analyses on the basis of erratic performance, a side benefit of examining individual differences.

Figure C-4 shows a scatterplot of the 20 subjects by their means on the two dependent variables. Other than the fact that no subject has both a long mean reaction time and a high mean error rate, there is little evidence for a speed/accuracy tradeoff. Three subjects in fact made no errors at all, and the deviation of Subject 17 is apparent.

When descent rate and path speed along a linear path are reduced at a rate which exactly holds flow rate constant, fractional loss in altitude remains constant throughout the event. In this case, global optical flow acceleration is eliminated as a source of information for descent. Under these conditions, a subject must use some other source of information for descent, such as increasing global optical (perspectival) splay or decrease global optical texture density (cf. Owen et al., in press).

Data from the Hettinger (1981) study are shown in Figures C-5 and 6 and in Table C-3. Because seven levels of fractional loss were used, there are fewer observations per point, and the individual profiles are less stable as a result. There is, however, a clear demonstration of individual differences in Figure C-5, and mean error rates ranged from 6% to 46%. Figure C-6 reveals a positive correlation between errors and reaction time, which is indicative simply of differences in skill. (This is the opposite of the negative correlation found for the deceleration-detection experiment.)

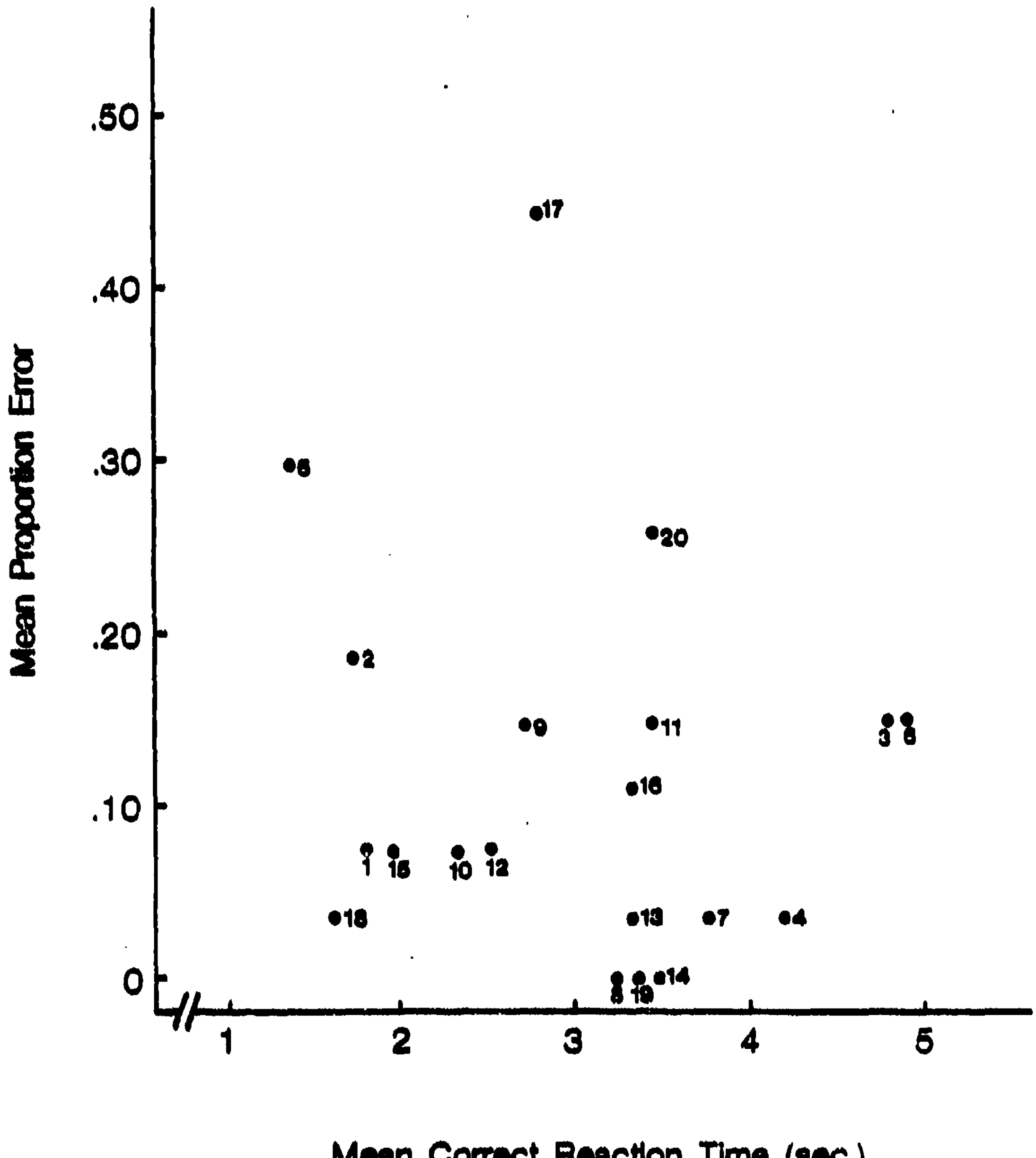

Figure C-4. Scatterplot of the 20 subjects in the Owen, Warren, and Mangold (in press) experiment on detection of loss in altitude. The subject numbers correspond to those in Figure C-3 and Table C-2.

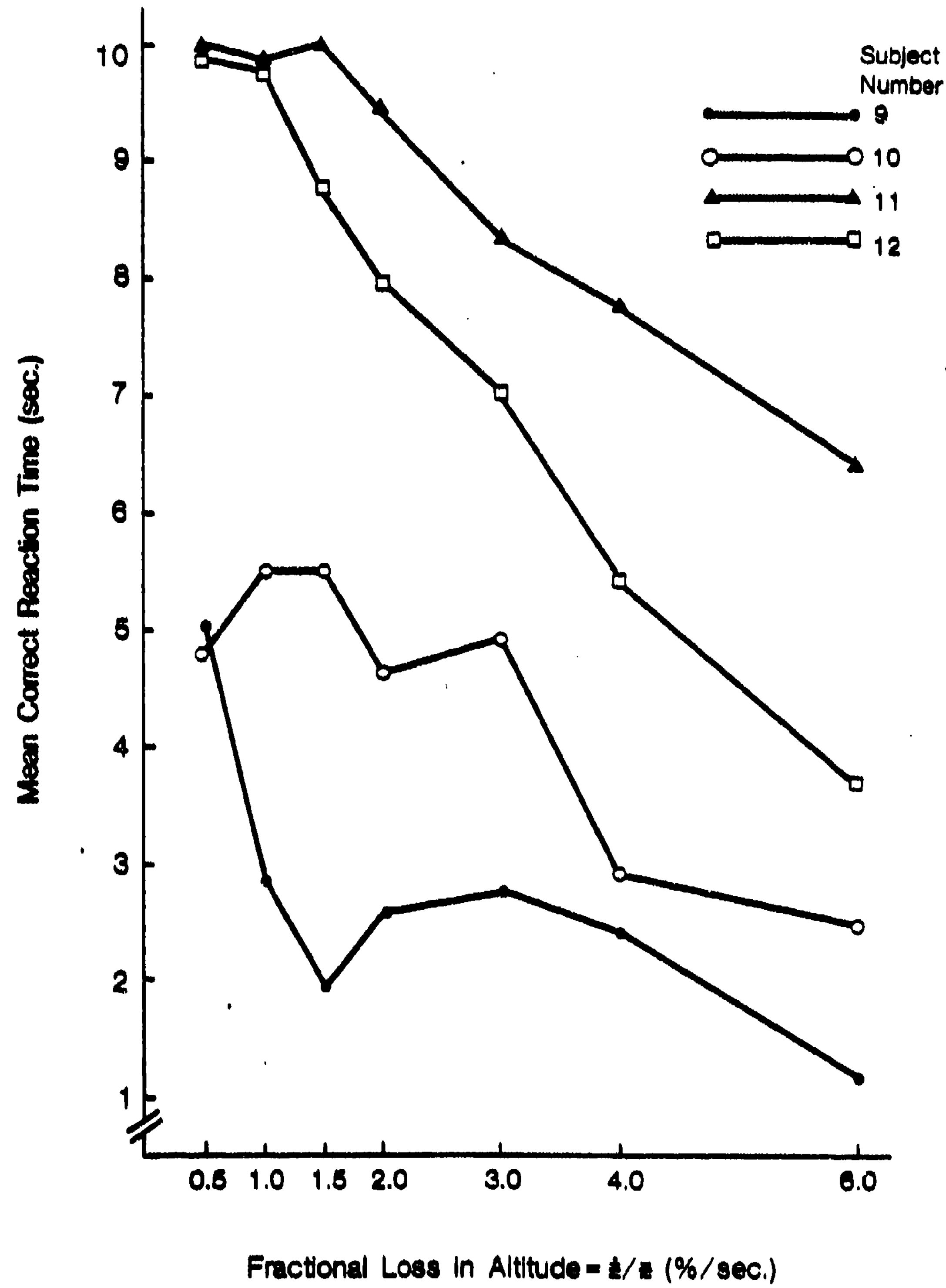

Figure C-5. Mean reaction times, from trials on which the subjects correctly detected descent, as a function of fractional loss in altitude.

Table C-3

Proportion Errors
for Individual Subjects at Each Level of Fractional Loss in Altitude

Subject Number	Fractional Loss in Altitude = $100\,\dot{z}/z$ (in %/sec)							
	.5	1	1.5	2	3	4	6	Mean
1	.17	.17	.00	.08	.00	.00	.00	.07
2	.83	.92	.33	.25	.00	.00	.00	.39
3	.17	.67	.33	.17	.00	.00	.00	.24
4	.50	.75	.17	.17	.00	.17	.00	.30
5	1.00	.83	.50	.00	.00	.00	.00	.35
6	.50	.17	.00	.00	.00	.00	.00	.09
7	1.00	.42	.00	.17	.17	.00	.00	.30
8	.50	.75	.67	.42	.00	.00	.00	.39
9	.17	.17	.00	.00	.00	.00	.00	.06
10	.50	.42	.17	.17	.00	.00	.00	.20
11	.83	.58	.17	.25	.00	.00	.00	.30
12	.67	.58	.17	.08	.00	.00	.00	.24
13	.83	.50	.00	.17	.00	.17	.00	.26
14	.83	.50	.50	.17	.17	.00	.00	.31
15	1.00	.67	.67	.33	.50	.00	.00	.46
16	.67	.50	.67	.42	.33	.17	.17	.43
17	.83	.58	.33	.58	.17	.17	.00	.43
18	1.00	.33	.00	.25	.00	.00	.00	.24
19	.67	.50	.17	.08	.00	.17	.00	.24
20	.33	.25	.00	.17	.00	.00	.00	.13
21	1.00	.58	.17	.00	.00	.00	.00	.26
22	.50	.33	.67	.08	.17	.00	.00	.24
23	.83	.50	.00	.25	.00	.00	.00	.26
24	.67	.83	.33	.17	.00	.00	.00	.33
25	.67	.33	.33	.00	.00	.00	.00	.19
26	.67	.50	.33	.17	.00	.00	.00	.26
27	.83	.50	.00	.08	.17	.00	.00	.24
28	.17	.00	.00	.17	.00	.00	.00	.06
Mean	.65	.49	.24	.17	.06	.03	.01	.26

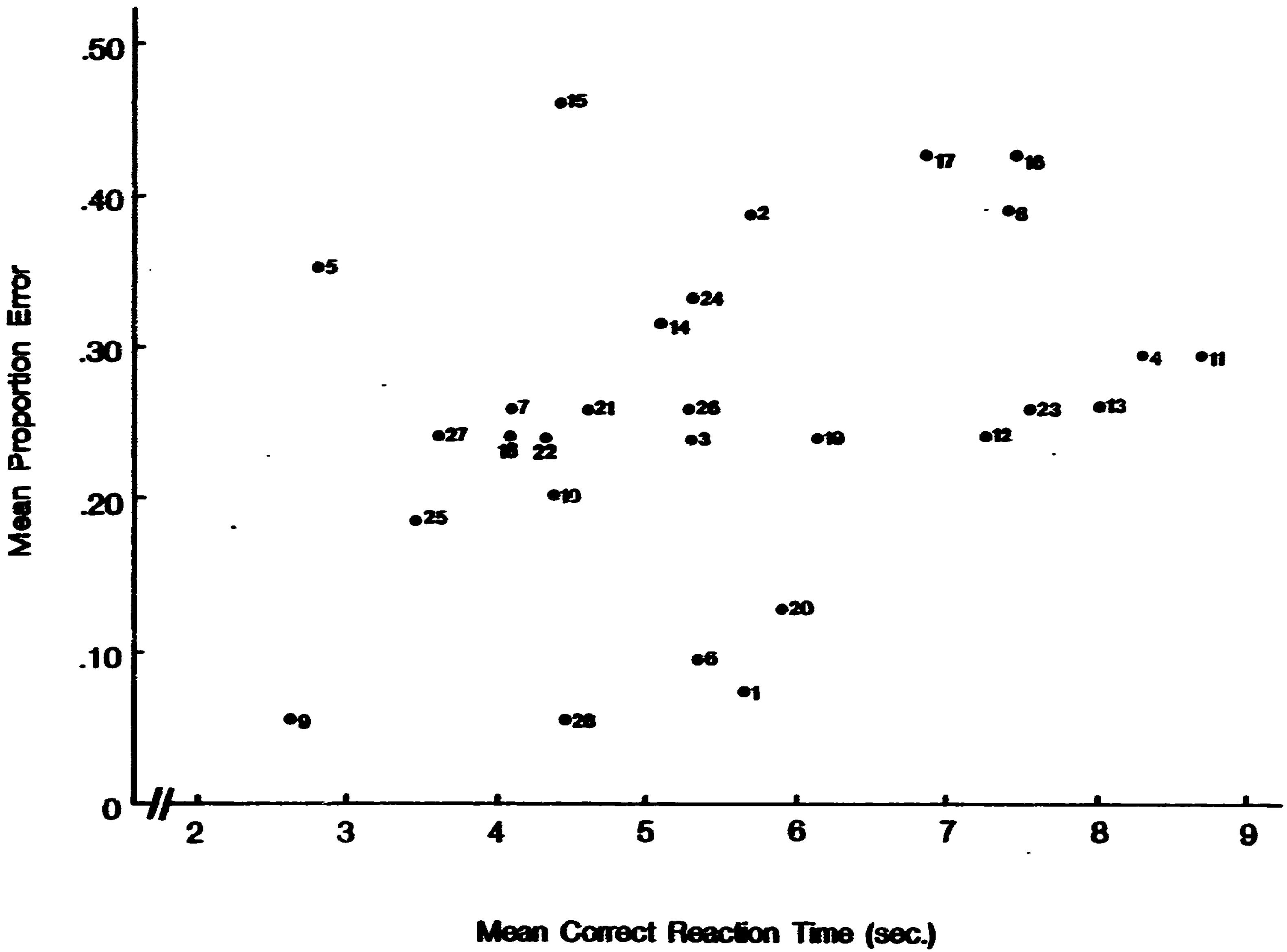

Figure C-6. Scatterplot of the 28 subjects in the Hettinger (1981) experiment on detection of loss in altitude. The subject numbers correspond to those in Figure C-5 and Table C-3.

Conclusions and implications. There is no question that individual differences in the three experiments are related to fractional rates of change. Whether these differences extend to interactions of subjects by fractional rates cannot be answered without considerably more replications to stabilize each individual's data.

When the relation between error rates and mean reaction times is considered over all three experiments, a suggestion of a pattern emerges. Since these points represent a sufficient number of observations to be considered stable, an attempt at interpretation is in order. In the first two experiments with constant rates of loss in speed or altitude, some events changed very rapidly and came to a halt long before the end of the 10-second trial duration. This may have induced time stress, resulting in a speed/accuracy tradeoff. The correlation over subjects between reaction time and error rate supported this interpretation. In the Owen et al. (1981) deceleration-detection experiment, $r = -.34$ ($p < .05$), and in the Owen et al. (in press) descent-detection experiment, $r = -.10$. These correlations correspond to the scatterplots in Figures C-2 and C-4, respectively.

In the Hettinger (1981) experiment, simultaneous reduction in all rates of change ($\dot{z}$, $\dot{x}$, $\dot{s}$) allowed the events to continue throughout the 10-second period. With no time stress, relative levels of skill are indexed by both dependent variables. The correlation between reaction time and error rate ($r = .23$) was appropriately positive (see Figure C-6). There is, of course, a great deal of dispersion in every case, suggesting that different subjects' results require different explanations. The average within-subject correlations between the two dependent variables for the three experiments were .27, .20, .11 respectively, showing that difficulty of detection is generally indexed by the two variables.

Denton (1976) isolated one explanation of interactions of subjects with optical variables. He selected two groups of 12 subjects each on the basis of whether they experienced a large versus almost no visual motion after-effect following prior exposure to a visual field of rectilinear motion. Given the task of holding a road scene constant at some speed, e.g., 70 mph, the high motion after-effect group showed the effect of adaptation to optical flow by increasing their speed. The low motion after-effect group showed no increase in speed for 11 of 12 observers.

Denton's results suggest that some observers adapt to optical flow, but others do not. Because pilots can compensate for adaptation to optical flow by increasing speed or decreasing altitude (cf. Owen & Warren, Appendix E), the implications for selection of pilots to engage in low-altitude flight are obvious. We will be giving this issue special attention in future experiments, as well as examining individual differences in all future studies (cf. Warren, Owen, & Hettinger, Appendix D).

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APPENDIX D

SEPARATION OF THE CONTRIBUTIONS OF OPTICAL FLOW RATE AND EDGE RATE TO THE PERCEPTION OF EGOSPEED ACCELERATION

Rik Warren, Dean H. Owen, and Lawrence J. Hettinger

APPENDIX D

SEPARATION OF THE CONTRIBUTIONS OF OPTICAL FLOW RATE AND EDGE RATE ON THE PERCEPTION OF EGOSPEED ACCELERATION

Rik Warren, Dean H. Owen, and Lawrence J. Hettinger

Consider two situations which result in illusory impressions of an increase in the speed of one's own motion (egomotion): (1) Travellers in a fixed wing aircraft during a landing approach may experience a marked impression of increasing speed. Yet, the aircraft's path speed and ground speed are essentially constant. (2) Driver's exiting high-speed roads using exit roads with stripes painted across them with exponentially decreasing spacing slowed down to 22.6% below the mean speed of those exiting over unstripped roads. This reduction in speed resulted in a two-thirds reduction in traffic accidents at the exits (Denton, 1980). The greater slowing was due, Denton argued, to drivers compensating for an illusion of acceleration induced by traveling at constant speed over the progressively closer spaced stripes.

This study is concerned with the question of what gives rise to the perception of acceleration of egospeed. An ecological optics analysis of the optical bases for the perception of egospeed and acceleration is presented. Two optical concommitants of egospeed, optical flow rate and edge rate, are defined and identified. Under the conditions of constant altitude and equispaced edges, flow rate and edge rate are linked. Since both are optically available to a visual system, it is thus not possible to determine which, if either, is the effective optical basis for the perception of egospeed. After a discussion of the two rates, two experiments are reported which break the normal linkage and permit an assessment of their separate effects.

Optical Bases for the Perception of Egospeed

Global optical flow rate. Warren (1982) has argued that the egomotion optical array flow pattern arising from travel over an endless plain has a characteristic global flow rate. Global optical flow rate ($\dot{s}/|z|$) is defined as the observer's speed scaled in eyeheights per second, and thus varies with actual egospeed and altitude, but is invariant with respect to the particular texture pattern on the flat surface. Here we are concerned only with the case of level travel and for this special case, the global flow rate differs from actual egospeed only by a scale factor. All further analyses assume level travel.

One optical concommitant of egospeed acceleration is an acceleration of the optical flow rate itself, which is equal to the rate of change of global optical flow: $d(\dot{s}/|z|)/dt = \ddot{s}/|z|$. Although this particular optical basis is mathematically sufficient to specify eogspeed acceleration, it is probably not psychologically effective. Owen, Warren, Jensen, Mangold, and Hettinger (1981) have argued that it is the relative rather than the absolute optic array properties that are psychologically effective. Hence, it is the fractional rate of change of an optical variable that serves as the functional invariant for percepetion. The fractional or relative global flow acceleration here is:

$$\ddot{s} / \dot{s} = (\ddot{s} / |z|) / (\dot{s} / |z|)$$

Edge rate. An egomotion flow pattern must also have a characteristic edge rate, since optical discontinuities (inhomogeneities) are necessary to define the flow. Environmentally, edge rate is defined as the number of reference ground texture edges traversed per second. Optically, edge rate is defined as the number of optical margins (corresponding to the ground edges) per second flowing past the optical locus corresponding to the "directly below." That edge rate is also available at other optical loci, such as a smudge mark on a windscreen, is left to intuition. A more formal discussion would be too lengthy and is not necessary here. However, two additional points must be made: First, edge rate depends critically on the choice of a reference ground texture element. The choice is arbitrary and is justified by perceptual utility: Cornfields per second may be useful to a jet pilot, furrows per second to a bird. Second, although texture elements are discrete units, the variable "number of edges" is considered continuous for ease of analysis.

Edge rate thus provides information for the observer's forward speed scaled in reference ground texture elements per second. It is invariant with change in altitude, but does vary with any change in the size of the ground testure elements.

Linkage of flow rate and edge rate. It follows from the above analysis that flow rate and edge rate each differ from ground speed only by a scale factor and hence are linked to each other under the condition of constant altitude coupled with a regularly spaced terrain: if ground speed is constant, both flow rate and edge rate are constant. This is illustrated in Figure D-1-a. The solid line represents the terrain and the tic marks represent equispaced edges. The dotted line represents the observer's path and the tic marks here represent the observer's position at various times, *t*. Note that speed is constant since the time tics are equidistant and the edge rate is one edge per second.

If ground speed accelerates, then both flow rate and edge rate accelerate. This is illustrated in Figure D-1-b for the case of exponentially increasing speed, but the logic is the same for any type of acceleration. The solid line again represents the ground and the accelerating speed is represented by the progressively greater spacing of observer position tics. Notice that one edge has been traversed in the first time interval, whereas several edges are traversed in the last time interval. Hence the number of edges traversed per second is accelerating.

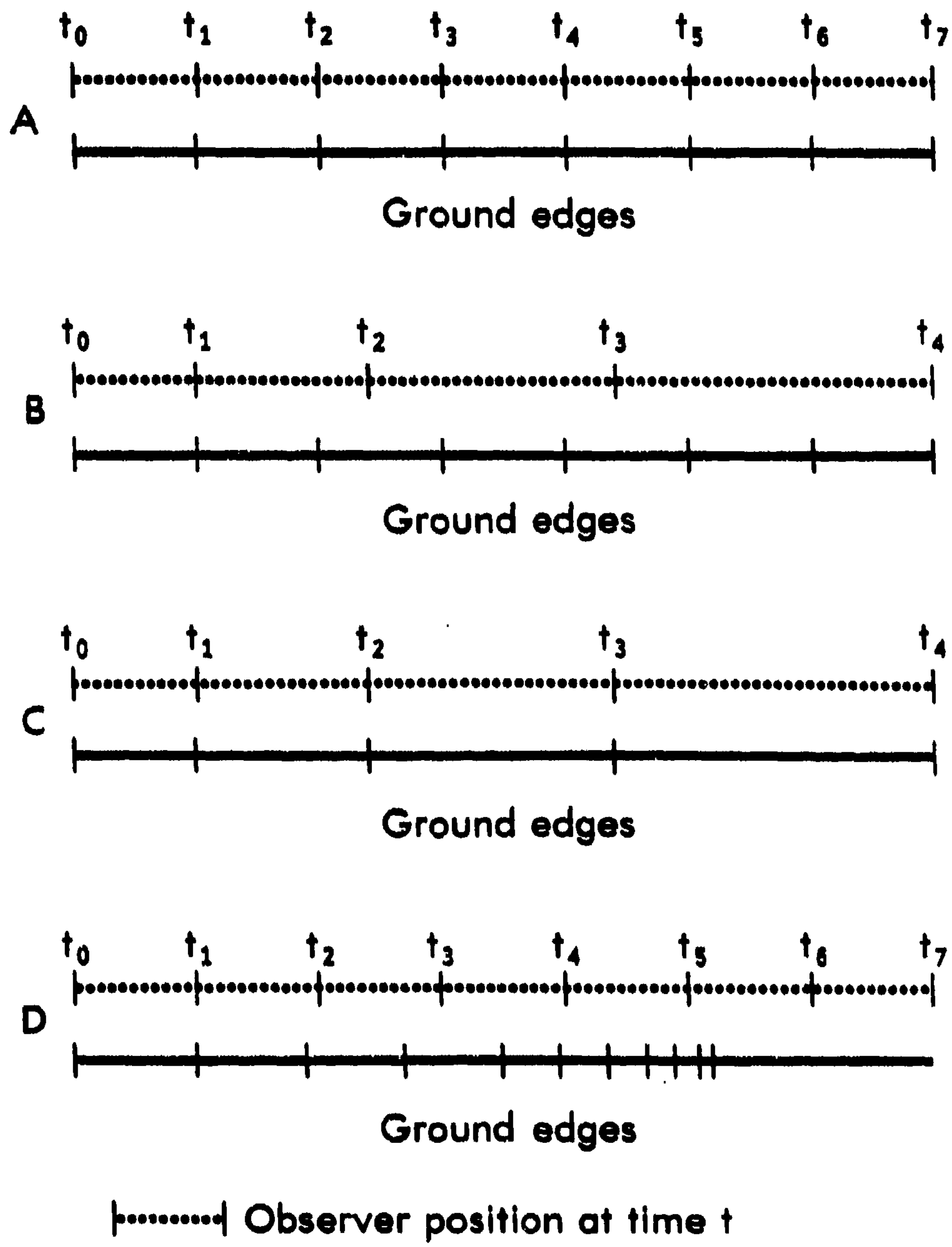

Figure 1. Observer position at time t and the corresponding ground texture edge spacing required to achieve four combinations at flow rate and edge rate acceleration.

Two Conflicting Perceptual Hypotheses

That two different sources of information are available in an optic array does not necessarily mean that they are physiologically/perceptually effective. Both, either, or neither may be effective, and if both are effective their relative effectiveness need not be equal. In order to assess the separate perceptual effects of flow rate and edge rate, it is necessary to break the normal linkage between them. One way is by varying altitude while keeping ground speed constant. This would keep edge rate constant while flow rate varied and in fact describes the optical conditions of a typical fixed-wing landing approach. The phenomenal acceleration that can be experienced during a constant-speed landing approach supports the hypothesis that flow rate more strongly influences perceived egospeed than edge rate (Warren, 1982). But Denton's research suggests the edge rate can dominate flow rate since his procedure results in displays with constant flow rate, but accelerating edge rate.

In order to test these conflicting hypotheses, we chose to break the normal linkage in such a way that either rate could be held constant while the other accelerated. Moreover, we wanted a method that did not entail a change of altitude. Constant altitude travel can be of any duration and speed without introducing possible complications due to the co-perception of impending or imminent landing or co-perception of change of altitude as such.

Breaking the linkage keeping altitude constant. Figure D-1-c illustrates a procedure for producing a constant edge rate although ground speed and flow rate accelerate. Simply structure the environment (in the forward dimension) so that an equal number of edges are traversed during each equal time interval no matter what the absolute distance covered during a particular time interval. Figure D-1-c illustrates an environmental edge spacing such that the observer crosses exactly one edge every unit time, although the absolute speed is ever accelerating. Recall that flow rate depends only on speed and altitude and is independent of the distribution of ground texture elements.

Figure D-1-d illustrates a procedure for producing an accelerating edge rate, although ground speed and flow rate are constant. Simply structure the environment (in the forward dimension) so that progressively more edges are crossed each successive equal time interval. The abosiute distance covered each equal time interval is to be constant since speed is to be constant. Figure D-1-d illustrates an exponentially decreasing edge spacing such that the observer crosses progressively more edges every unit time interval, although speed is held constant.

Basic design strategy. The basic experimental design is a 2x2 orthogonal crossing of flow rate and edge rate where either may be constant or exponentially increasing. The desired combination is achieved by manipulating the ground speed and forward spacing of edge lines.

Basic task. The basic task for observers was to view simulated egomotion displays from each of the four basic types and to indicate whether the simulated egomotion was constant or accelerating.

Although similar, our method and Denton's differ in the very important respect that our observers were passive viewers and necessarily viewed displays of constant flow rate. His drivers could control their speed and hence, as they slowed, the flow rate slowed also. Denton's experiments were designed to demonstrate an influence of pattern on perceived egospeed and that they did do. However, his design and controls do not permit an assessment of the reasons for the effect. For instance, his only control condition involved an unstriped road. Since no equispaced striped pattern was used, it is not possible to determine if his effects were due to the exponentially decreasing spacing of stripes or just the mere existence of stripes. Our experiments were specifically designed to enable a test of flow rate versus edge rate hypotheses to explain the perceptual effects, which necessarily entails provisions for all logically necessary comparisons and controls within the restriction of level egomotion.

Opposing predictions. The hypothesis that optical flow rate determines perceived acceleration and the hypothesis that optical edge rate does so both make the same predictions in the cases in which flow rate and edge rate are both constant or both accelerating. It is in the cases where only one rate is accelerating that they make opposite predictions. All predictions are presented in Table D-1.

Convention. The remaining analysis is in terms of ground speed only, because during level flight with zero wind velocity, path speed equals ground speed ($\dot{s} = \dot{x}$), and hence flow rate (in general, $\dot{s} / |z|$, or for the level case, $\dot{x} / |z|$)here differs from ground speed only by a constant scaling factor (namely, $1 / |z|$).

Table D-1

Predicting Percent Judgments of Acceleration by the Hypotheses that Perceived Acceleration is Based on Optical Flow Rate Versus Optical Edge Rate

Flow Rate Hypothesis:

		Flow Rate Constant	Flow Rate Accelerating
Edge Rate	Constant	0	100
	Accelerating	0	100

Edge Rate Hypothesis:

		Flow Rate Constant	Flow Rate Accelerating
Edge Rate	Constant	0	0
	Accelerating	100	100

EXPERIMENT 1

The predictions in Table D-1 are for an observer who operates perfectly at all non-zero "signal strengths" and makes no false alarms. The purpose of Experiment 1 was to allow for a more realistic possible variation in perceived acceleration as a function of degree of acceleration.

Method

Observers

Observers were 25 undergraduates, 13 males and 12 females, with no previous flight experience.

Apparatus

The simulated flight scenes were generated and displayed using the Ohio State University Aviation Psychology Laboratory's simulation facilities.

Scenes

General static view. All scenes depicted a flat rectilinearly textured plain. The view was that from an altitude of 72m through a window 34.2 deg wide by 26 deg high with the horizon in the middle. The rectangles were oriented so that their bases were shown parallel to the horizon. All rectangles had bases 72m wide with their edges aligned as in a checkerboard. Hence all lateral edges were equispaced and since altitude was constant, the lateral edges of the rectangles did not change their perspectival slope or splay with respect to the horizon during forward egomotion. The forward dimension of the ectangles depended on the particular experimental condition. Details are given in Table D-2.

General dynamic view. All scenes lasted 10 sec and simulated constant-altitude, rectilinear, forward egomotion. Hence the aim point and the focus of expansion were on the horizon in the middle of the "window." The particular flow rates and edge rates depended on the particular scene and are given in Table D-2.

Table D-2

Equations Prescribing the Ground Speed ($|\dot{x}_t|$), Edge Rate ($\dot{E}_t$), and Initial Edge Positioning (x_E) for Each of the Four Scene Classes

Class	Ground Speed	Edge Rate	Edge Position
1	k	k	$(\lvert\dot{x}_o\rvert / \dot{E}_o)E$
2	k	$\dot{E}_o r_{\dot{E}}^t$	$(\lvert\dot{x}_o\rvert / \log r_{\dot{E}}) \log [(E (\log r_{\dot{E}})/\dot{E}_o) + 1]$
3	$\lvert\dot{x}_o\rvert r_{\dot{x}}^t$	k	$(\lvert\dot{x}_o\rvert / \log r_{\dot{x}}) (r_{\dot{x}}^{E/\dot{E}} - 1)$
4	$\lvert\dot{x}_o\rvert r_{\dot{x}}^t$	$\dot{E}_o r_{\dot{E}}^t$	$(\lvert\dot{x}_o\rvert / \dot{E}_o)E$

Notes:

The k's are any arbitrary constants.

E = edge number 0, 1, 2, 3,...

The r's are constants of proportionality.

All logarithms are base e.

Scene classes and parameters. The 2x2 orthogonal crossing of flow rate and edge rate defines four classes of scenes. Table D-2 presents the general defining equations for the ground speed and edge rate ($\dot{E}$) changes in each class. In addition, the equation prescribing the positioning of forward texture edges is also presented. The derivation of these equations will be presented in Warren (in prep.) but their explanation is as follows: Edges are numbered according to the state of affairs at time zero or the onset of a scene. All scenes assume the observer is directly positioned over an edge ($E = 0$) at time zero. All other edges (E = edge number 1,2,3,...) are in front of the observer at time zero at forward distances symbolized by x_E. Although actual edges are numbered by integers, equations involving E assume that E is a continuous variable. The values of the flow and edge rates at time $\underline{t}$ are symbolized by $\dot{\underline{x}}_t$ and $\dot{\underline{E}}_t$. Lastly, the constant of proportionality in an exponential equation is symbolized by $\underline{r}_{\dot{x}}$ or $\underline{r}_{\dot{E}}$.

Specific scene parameters. Classes 2, 3, and 4 each contain 27 unique scenes formed by a factorial combination of 3 levels of initial ground speed ($\dot{\underline{x}}_0$ = 72, 108, 162 m/s): 3 levels of initial edge rate ($\dot{\underline{E}}_0$ = .4, .6, and .9 edges/second): and 3 levels of the constant of proportionality ($\underline{r}_{\dot{x}}$ and/or $\underline{r}_{\dot{E}}$ = 1.03, 1.045, and 1.067; for each scene of Class 4, $\underline{r}_{\dot{x}} = \underline{r}_{\dot{E}}$). Class 1 contains 27 scenes comprising three replications of the nine unique scenes formed by a factorial combination of the three levels of ground speed and three levels of edge rate given above. Thus a scene block consisted of a total of 108 scenes of which 90 were unique. In any particular scene, the flow rate and the edge rate increased by 3.0, 4.5, or 6.75% of the value one second earlier.

Procedure

The experiment was an entirely within-observer design. Two randomizations of the 108-scene block were prepared with the constraint that no more than four consecutive scenes were of the same class. Each observer was individually tested and received both blocks. Half of the observers received one ordering of the blocks, the other half received the other order. Thus, there were 216 trials per observer. Presentation and data collection were computer automated at a rate of three scenes/min which allowed for 10 sec of data recording and rest between each 10-sec scene. At this rate, total testing time for the three blocks was 72 min per observer. In addition, each observer received eight practice trials, two representing each scene class but with parameter values differing from the experimental scenes.

An individual trial consisted of a ready signal followed by a 10-sec viewing of a scene. Observers were instructed to indicate by pressing a button whether the scene represented constant speed or not and to give a confidence rating of "very", "moderately", or "slightly" confident. Observers were instructed to respond anytime they were ready, but if they had not responded by the time the 10-sec scene was over, they were asked for a judgment. Reaction time from scene onset to the button press was recorded surreptitiously.

Summary of "expanded" design

The expanded design is an entirely within-observer design with six fully crossed independent factors: 25 observers by 2 blocks (of 108 trials/block) by 2 flow acceleration states (constant, acceleration) by 2 edge rate acceleration states (constant, accelerating) by 3 initial flow rates (1, 1.5, or 2.25 eyeheights/sec corresponding to speeds of 72, 108, or 162 m/sec) by 3 initial edge rates (.4, .6, or .9 edges/sec) by 3 rates of acceleration (3, 4.5, or 6.75% if some acceleration else 3 replications, if acceleration is zero).

The last "R-factor" can also be interpreted as relative or fractional rate of change in classes 2, 3 and 4 instead of rate of acceleration. Fractional or relative rate of change of speed is the ratio of acceleration to speed:

$$\ddot{x}_t/\dot{x}_t = (\dot{x}_o\, r^t \log r)/(\dot{x}_o\, r^t) = \log r$$

This equation follows from Table 2 and by assuming $\ddot{x}$ and $\dot{x}$ are both positive. The fractional edge rate is found similarly. The fractional rates used here are thus log 1.03, log 1.045, and log 1.0675 (all base e) or 2.96%, 4.40%, and 6.53%. the remaining discussion refers to the rates or degree of acceleration as 3%, 4.5%, and 6.75%.

Results and Discussion

The results for the judgments of acceleration versus constant speed are presented first. These judgments are deemed correct of incorrect depending on their agreement with ground speed. The results for confidence rating data are presented next. The confidence rating data are similar to the judgment data, but have a "finer grain" since six categories of response are possible. The reaction time results are not presented due to their incompleteness, noisiness, and redundancy with the other results.

Judgments of Acceleration

The mean percent judgments of acceleration for the basic 2 x 2 crossing of flow acceleration versus edge rate acceleration are presented in Table D-3. Both main effects are significant at beyond the p = .00000+ level due to the power inherent in the design. Edge rate acceleration accounts for twice as much of the total variance as flow acceleration (7.60% versus 3.75%) and is the single most potent factor in the study, including observers and observer interactions. See Table D-4 for an ANOVA summary.

Table D-3

Percent Judgments of Acceleration in Experiment 1
as a Function of Flow Rate Constant or Accelerating
Versus Edge Rate Constant or Accelerating

		Flow Rate		
		Constant	Accelerating	
Edge Rate	Constant	21.1	38.3	29.7
	Accelerating	46.4	67.5	57.0
		33.8	52.7	

Note: N = 1350 per cell

Table D-4

ANOVA Summary Table for Judgments of Acceleration in Experiment 1

Source	SS	df	F	p	% Var
Observers	93.5	24			7.05
Blocks	8.0	1	18.5	.00003	.60
Flow Accel. (XA)	49.7	1	78.46	.00000	3.75
Edge Accel. (EA)	100.9	1	129.48	.00000	7.60
Initial Flow Rate (XV)	95.7	2	82.55	.00000	7.22
Initial Edge Rate (EV)	28.3	2	58.58	.00000	2.14
R-Factor/(R)	26.1	2	83.04	.00000	1.97
EA by R	14.3	2	43.39	.00000	1.08
Total	1,326.	5399			100.

Note: There are 127 possible sources of variance for this design. Twenty-seven of the sources not listed here each accounted for between 1.0 and 2.6% of the total variance and all involved observer interactions.

As expected, judgments of acceleration are greatest (67.6%) when both flow rate and edge rate accelerate. The finding that the edge-only acceleration elicits a greater percentage of acceleration judgments (46.4% to 38.3%) than flow only acceleration supports the hypothesis that perceived acceleration is governed by edge rate acceleration. But the 38.3% judgments of acceleration produced by flow only acceleration indicates that flow acceleration is not entirely ineffectual, especially since this result is considerably above the 21.1% pure false alarm rate (judgments of acceleration when no acceleration is present). The term "pure false alarm rate" is used to underscore that judgments of acceleration in the edge-only acceleration condition are also a type of false alarm since egospeed is constant. The pure false alarm rate and the 32.4% miss rate (100 - 67.6%) when both sources of information were present attests to the difficulty of the task. Difficulty may be due to the viewing time permitted (10 sec), to the particular acceleration rates used, or to the initial rates used.

Overall, the mean percent judgments of acceleration for the 0, 3, 4.5 and 6.75% acceleration rates were 21.1, 40.5, 48.0 and 63.9% (N = 1,350 per mean). Although Table D-4 indicates the main effect of the R-factor to be significant at the p = .00000+ level and to account for about 2% of the total variance, these values are actually underestimates of the effect of acceleration rate. The R-factor in the ANOVA has only three levels which correspond to the 3, 4.5, and 6.75% acceleration rates if at least one of edge rate or flow rate is accelerating. If both edge and flow rate are constant, then this factor is to be interpreted as three replications. The three R-factor means (N = 1800) used to determine the R-factor sum of squares are less variable (and smaller) than the 3, 4.5 and 6.75% means (N = 1350) since the relatively homogeneous data of the three 0% replications are incorporated in them. The complication does not affect the sums of squares, significances, or interpretation of ANOVA sources not involving the R-factor. The effect on sources and interactions involving the R-factor is to overestimate error terms and underestimate the impact of the R-factor when interpreted as rate of acceleration. The conservativeness of the ANOVA may be overcome in graphs or tables which distinguish between zero and non-zero acceleration rates.

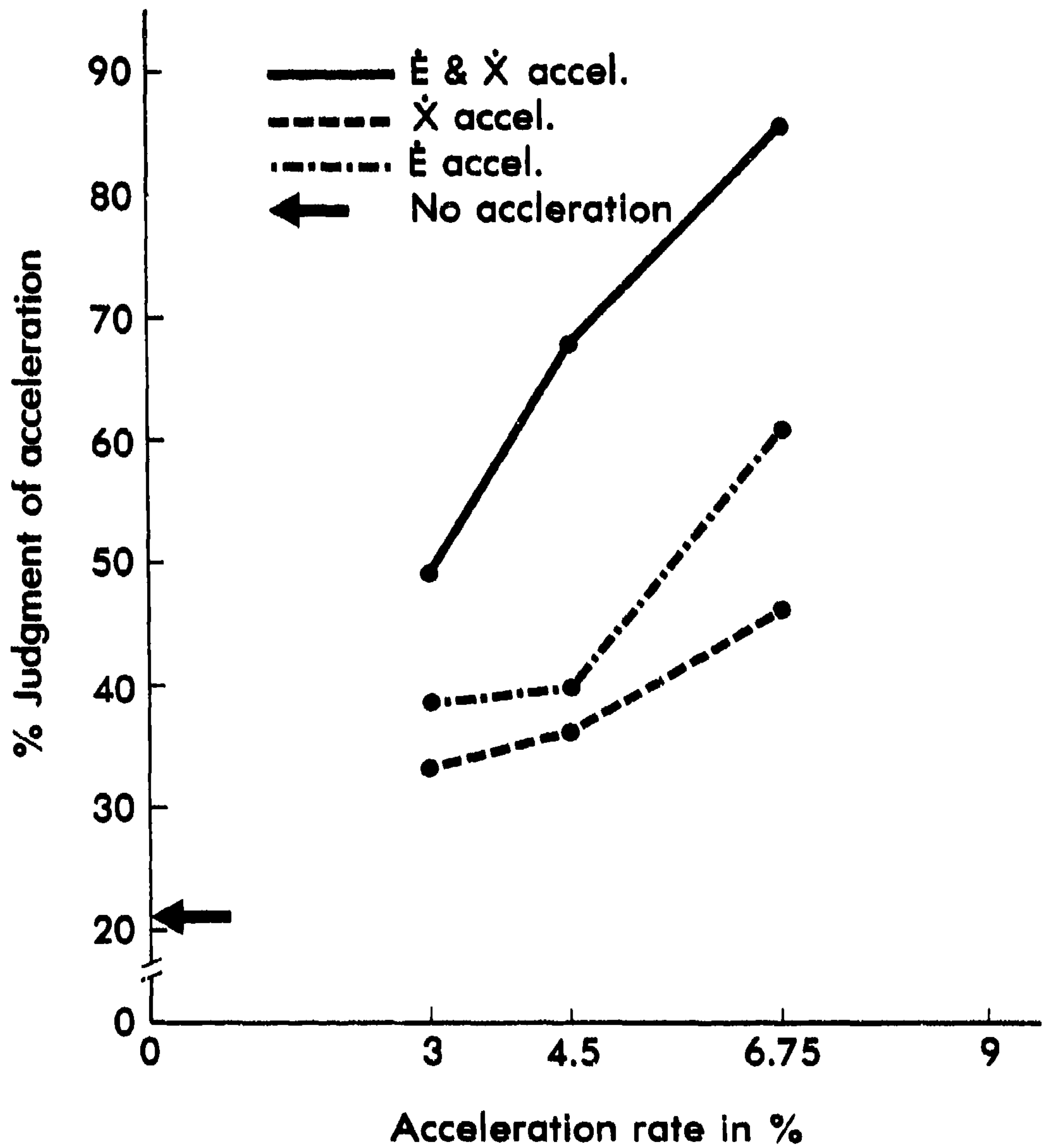

Figure 2. Percent judgments of egospeed acceleration as a function of acceleration rate and type of acceleration information in Experiment 1 (N = 1350 for the no-acceleration point, N = 450 all other points, data from 25 observers).

The mean percent judgments of acceleration as a function of the degree of acceleration for each of the acceleration information conditions are presented in Figure D-2. For each condition, the percentage of judgments of acceleration increases the higher the rate of acceleration. For a particular acceleration degree, the greatest percentage of acceleration judgments results from the conditions in which both flow rate and edge rate accelerate. The least percentage is always for the flow acceleration only condition and the intermediate percentage is always for the edge rate acceleration only condition. The judgments for flow only acceleration are always above the false alarm rate. The false alarm rate of 21.1% and the fact that the highest hit rate (for the case of both rates accelerating by 6.75%) was only 85.6% suggest that the growth rates were relatively low, at least in the context of a 10-sec exposure. None of the curves appears to be near an asymptote.

Table D-4 indicates that initial edge rate significantly accounts for 2.14% of the variance and initial flow rate accounts for 7.22% of the variance. It is not clear why these factors should be so potent. One speculation is that faster displays are more vivid and that some observers may confuse speed and vividness with acceleration. Another speculation is that all displays do technically accelerate in the sense that at time zero the speed "accelerates" from zero (a blank screen) to a greater value and this is more blatant the faster the initial flow or edge rate. Future experiments will check this possibility by showing a lead-in of constant speed travel for a short period before acceleration begins. In fact, Denton used a similar procedure.

Due to the inherent power and large N of the study, many of the 127 sources of ANOVA variance achieve statistical significance. However, no other factors account for more than 2.5% of the variance in an R^2 data descriptive sense. In a predictive sense, the percent variance would be even less.

Individual Differences. In conclusion, the main finding is that edge rate acceleration information dominates flow acceleration information in this experiment. But this conclusion is based on averages. However, 10 of 25 subjects gave a greater number of acceleration judgments to flow-only acceleration displays than to edge-only acceleration displays. This suggests that individual differences may be important and that some people may be edge dominant while others are flow dominant.

Confidence Ratings

Each judgment was accompanied by a rating of "slightly," "moderately," or "very" confident that the scene represented acceleration or constant speed. Being very confident that egospeed is constant is interpretable as being least confident that egospeed is accelerating. Thus, ratings were transformed into a 6-point scale in which "6" represents the most confidence for acceleration and "1" the least. Since judgments of constancy versus acceleration are essentially a 2-point rating scale, this 6-point scale enables a "finer grain" analysis of the judgments. The ANOVA design and analyses are parallel to that for the 2-point judgments.

The confidence results are largely confirmatory of the judgment results, but less noisy due to their finer grain.

Table D-5 indicates that edge-only acceleration information again dominates flow-only acceleration information (average ratings of 3.33 versus 3.02), but that flow-only acceleration ratings are higher than for displays with no acceleration information (3.02 versus 2.35). In general, observers were not totally certain that fully constant displays were constant or that fully accelerating displays were accelerating. Table D-6 indicates, that, overall, edge rate acceleration information accounts for about twice as much variance as flow acceleration information (8.87% versus 4.56%).

Table D-5

Average 6-Point Confidence Ratings of Acceleration as a Function of Flow Acceleration vs. Edge Acceleration for Experiment 1

		Flow Rate Constant	Flow Rate Accelerating	
Edge Rate	Constant	2.35	3.02	2.69
	Accelerating	3.33	4.22	3.78
		2.84	3.62	

Note: N = 1350 per cell. "6" indicates most confidence that a scene represents acceleration and "1" the least.

Table D-6

ANOVA Summary Table for the 6-Point Confidence Ratings in Acceleration in Experiment 1

Source	SS	df	F	p	% Var
Observers	1,589.0	24			8.80
Blocks	135.1	1	15.65	.0006	.75
Flow Acceleration	822.9	1	91.80	.0000	4.56
Edge Acceleration	1,602.8	1	115.37	.0000	8.87
Initial Speed	1,674.5	2	95.84	.0000	9.27
Initial Edge Rate	479.9	2	77.85	.0000	2.56
R-Factor	447.6	2	121.86	.0000	2.48
EA by R	235.5	2	46.47	.0000	1.30
Total	18,061.4	5399			100.

Note: There are 127 possible sources of variance for this design. Eighteen sources not listed here accounted for between 1.0 and 2.3% of the variance and all involved observer interactions.

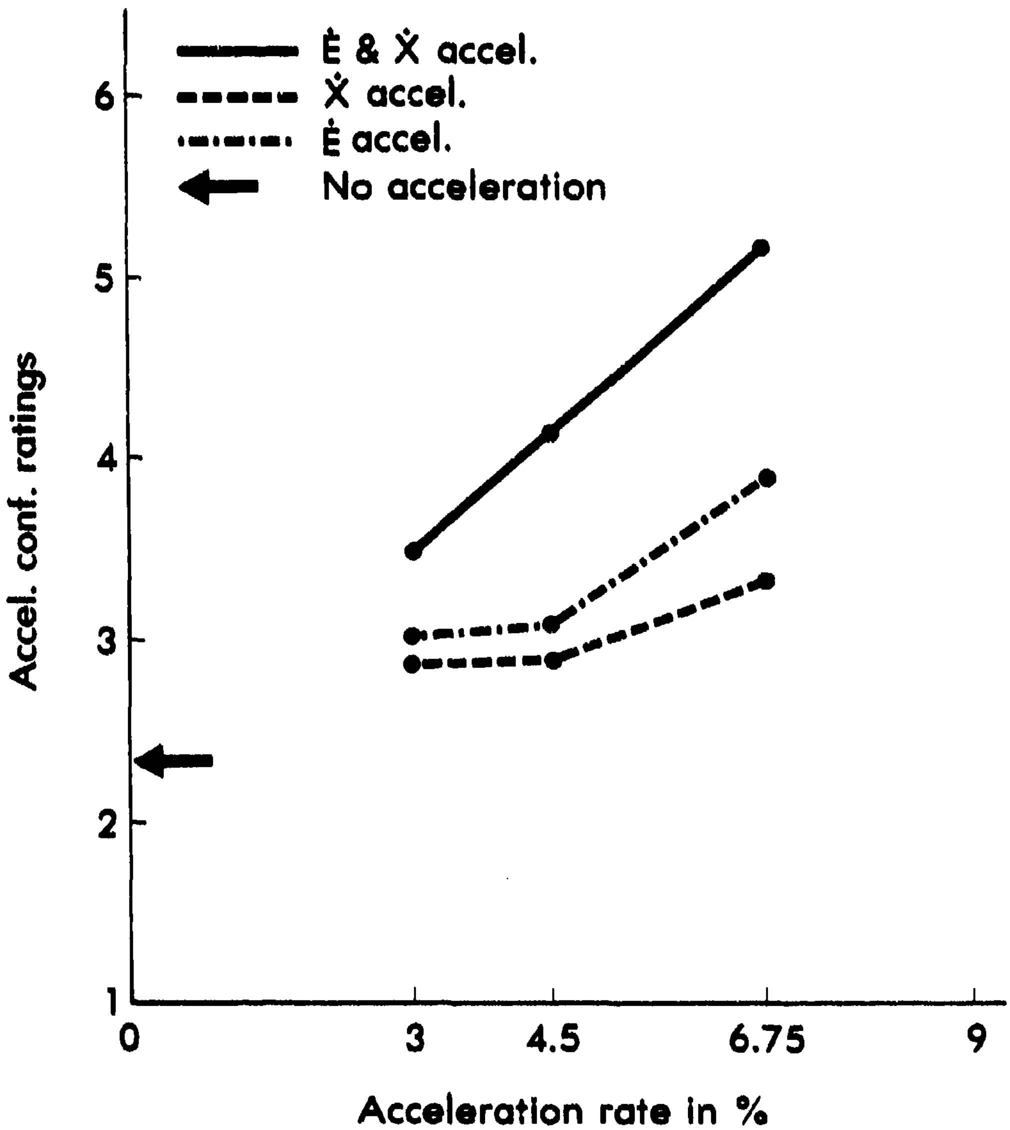

Figure 3. Mean ratings of egospeed acceleration as a function of acceleration rate and type of acceleration information in Experiment 1 (N = 1350 for the no-acceleration point, N = 450 all other points, data from 25 observers).

Overall, the mean confidence ratings for the 0, 3, 4.5, and 6.75% acceleration rates were 2.35, 3.13, 3.37, and 4.07 (N = 1,350 per mean). the R-factor accounts for 2.48% of the variance which means that degree of acceleration accounts for greater than 2.48% of the variance.

The average ratings of acceleration as a function of degree of acceleration for each of the acceleration information conditions are shown in Figure D-3. Figure D-3 closely parallel Figure D-2. On a case-by-case basis, edge rate acceleration information dominates flow acceleration information. No curves in Figure D-3 have reached an asymptote indicating that the maximum degree of acceleration is low for the task.

Table D-6 shows a similar pattern to Table D-4. The main difference is that initial speed or flow rate emerges as the most potent factor (9.3% of the variance) in determining ratings of acceleration. Again, this effect may be somewhat artifactual for the reasons already discussed.

Edge rate acceleration information is again shown to be dominant on average over flow acceleration information. But, the same 10 observers again show a preference for flow-only acceleration information over edge-only acceleration information. Thus, the same pattern of individual differences occurs for both judgments and ratings.

EXPERIMENT 2

In Experiment 1, none of the curves for either judgments or ratings of acceleration as a function of degree of acceleration appear to have reached asymptote (Figures D-2 and D-3). In particular, in the most favorable acceleration information condition, the mean judgment of acceleration was just 85.6%. This, together with the finding of a high false alarm rate (21.2% judgments of acceleration in the no-acceleration information condition) suggests that the task was rather difficult.

The purpose of Experiment 2 was to investigate the effect of a higher range of acceleration rates on judgments and ratings of egospeed acceleration. It was expected that judgments and ratings of acceleration would increase with greater degrees of acceleration. It was speculated that the false alarm rate might decrease because of the greater overall difference between accelerating and constant displays.

Method

Observers

Thirteen new observers (10 males and 3 females) participated.

Procedure and Design

The procedure and design were identical to those of Experiment 1.

Displays

The displays were the same as in Experiment 1 except for:

Acceleration rates. The range of acceleration rates was increased to 4, 6, and 9% corresponding to r values of 1.04, 1.06, and 1.09 for the equations in Table D-2.

Initial speed, flow rate, and altitude. In Experiment 1, the initial speeds were 72, 108, and 162 m/sec and the simulated altitude was 72 m. Thus the initial flow rates were 1, 1.5, and 2.25 eyeheights/sec since flow rate is given by speed/altitude. In Experiment 2, the initial speeds were raised to 80, 120, and 180 m/sec, altitude to 80 m, and lateral spacing to 80-m intervals. Geometrically, the coordinated increases in these environmental parameters produces static and dynamic perspectival views identical to those in the first experiment. In particular, the initial flow rates were again 1, 1.5, and 2.25 eyeheights/sec. The reason for increasing the simulated altitude was to decrease digital noise effects on the displays.

<u>Exposure duration</u>. The duration of all the displays was reduced from 10 sec to 6.5 sec. This was necessitated by equipment limitations. Due to the use of a digital computer, simulated speed cannot increase smoothly as in real travel. Rather, digitally simulated speed increases in a stepwise fashion. The greatest speed we could simulate with a reasonably small step size was 320 m/sec else acceleration would be jerky. Solving the equation $\dot{x}_{final} = \dot{x}_{initial}(r^t)$ for an initial speed of 162 m/sec, r=1.09, and t=6.5 sec yields a final speed of 315.2 m/sec. Thus 315.2 m/sec is the fastest speed simulated in the experiment and does not exceed the 320 m/sec limit.

Results and Discussion

Judgments of Acceleration

The mean percent judgments of acceleration for the basic 2x2 crossing of flow acceleration versus edge rate acceleration are presented in Table D-7.

Table D-7

Percent Judgments of Acceleration as a Function of Flow Acceleration Versus Edge Rate Acceleration in Experiment 1

		Flow Rate Constant	Flow Rate Accelerating	
Edge Rate	Constant	20.1	48.4	34.3
	Accelerating	34.2	61.8	48.0
		27.1	55.1	

Note: N = 702 per cell

The main effect of flow acceleration accounts for over four times as much of the total variance as the main effect of edge rate acceleration (8.09% versus 1.95%) and is the single most potent factor in this experiment including observers and observers interactions. See Table D-8 for an ANOVA summary and significance levels. This result is the exact reversal of that in Experiment 1. In Experiment 1 edge rate acceleration dominated.

Table D-8

ANOVA Summary Table for the Judgments of Acceleration in Experiment 2

Source	SS	df	F	p	% Var
Observers	20.4	12			3.00
Blocks	1.2	1	12.49	.0041	.18
Flow Accel. (XA)	55.0	1	58.14	.0000	8.09
Edge Accel.	13.3	1	37.55	.0001	1.95
Initial Flow Rate	46.2	2	20.48	.0000	6.79
Initial Edge Rate	6.8	2	23.12	.0000	1.00
R-Factor	20.1	2	43.06	.0000	2.96
XA by R	7.7	2	32.29	.0000	1.13
Total	679.9	2807			100.

Note: There are 127 possible sources of variance in this design. Twenty-three sources not listed here accounted for between 1.00 and 3.99 percent of the variance and all involved observer interactions.

Flow-only acceleration clearly is superior to edge-only acceleration in eliciting judgments of acceleration (48.4% vs. 34.2%). That both sources are used in normal situations is evidenced by the finding that the percentage for both sources present (61.8%) is greater than for only flow acceleration present (48.4%) or for only edge acceleration present (34.2%) and these both are greater than for no acceleration information present (20.1%). When both flow rate and edge rate accelerate, the percentage of acceleration judgments is lower than the comparable case in Experiment 1 (61.8% here versus 67.6%). This finding is contrary to expectation since the overall acceleration rates are greater here than in Experiment 1. The pure false alarm rate here (20.1%) is only marginally lower than the false alarm rate (21.1%) in Experiment 1. The lowering is in the right direction but the magnitude is not impressive.

The two main findings of this experiment are (1) the reversal of the dominance of edge-only acceleration versus flow-only acceleration found in Experiment 1, and (2) the finding that overall judgments of acceleration did not increase in comparison to Experiment 1. Both of these findings are clearly evident in Figure D-4. The overall mean percent judgments of acceleration for the 0, 4, 6, and 9% acceleration rates were 20.1, 34.0, 47.6, and 62.8%. Compared to the 21.1, 40.5, 48.0 and 63.9% judgments of acceleration for the 0, 3, 4.5, and 6.75% acceleration rates, these findings are unexpectedly lower. In particular increasing the degree range upward did not bring any curve closer to asymptote. The most favorable case (both sources accelerating at 9%) in this experiment was less effective than the most favorable case (both sources accelerating at 6.75%) in Experiment 1 (80.3% judgments of acceleration versus 85.6%).

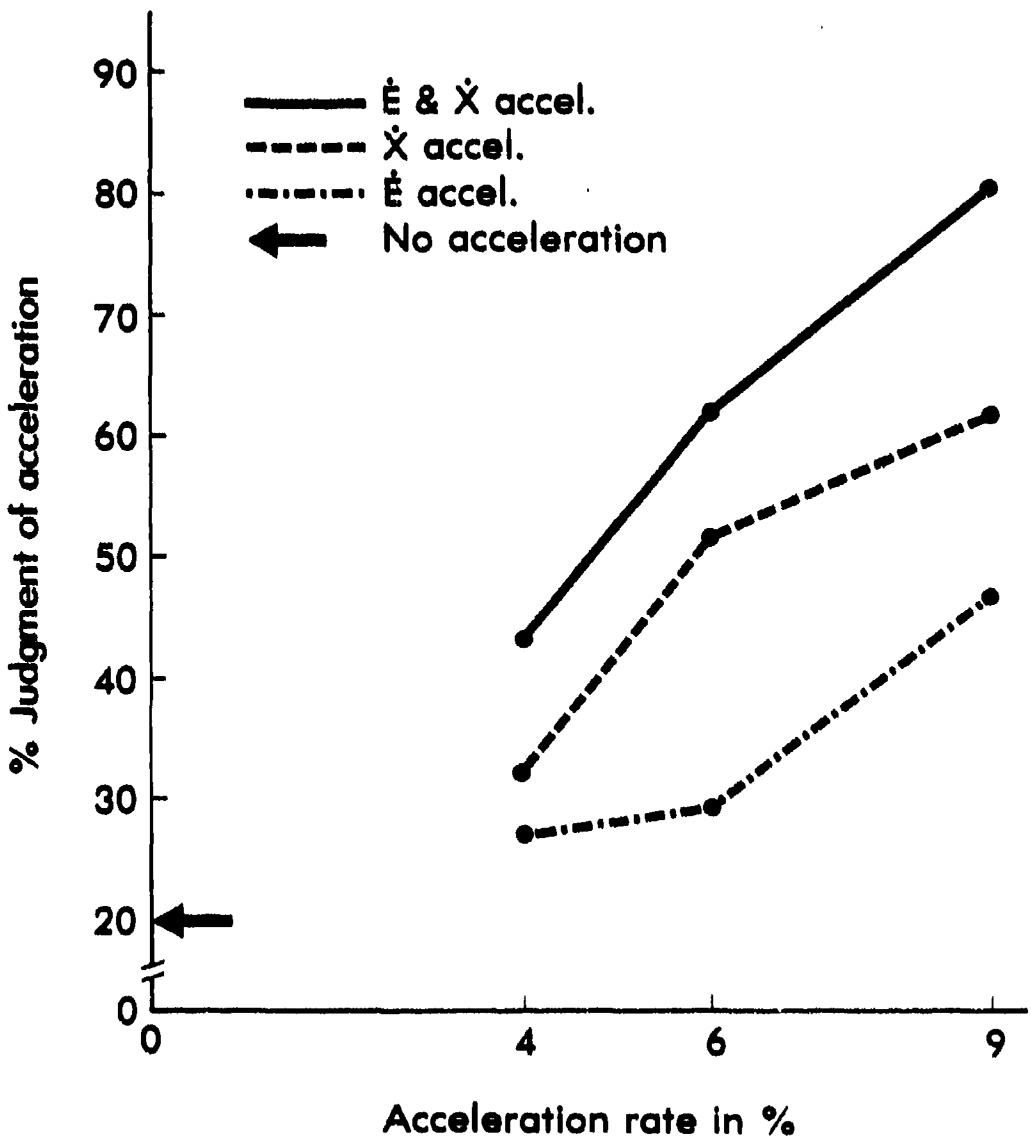

Figure 4. Percent judgments of egospeed acceleration as a function of acceleration rate and type of acceleration information in Experiment 2 (N = 702 for the no-acceleration point, N = 234 all other points, data from 13 observers).

The results of the two experiments are not always in opposition. Table D-8 indicates that initial flow rate again accounts for a relatively large proportion of the variance (6.79% here and 7.22% in Experiment 1) and that initial edge rate again contributes to a lesser degree (1.00% here and 2.14% in Experiment 2). The speculations about the reason for these results in Experiment 1 apply equally well here.

Individual Differences. Two of the 13 observers showed edge dominance instead of the flow dominance exhibited by the group as a whole. The lack of unanimity is less here (2 of 13 observers in a minority) than in Experiment 1 (10 of 25 observers in a minority) but the importance of considering individual differences is still indicated.

Confidence Ratings of Acceleration

Ratings of acceleration were determined the same way as in Experiment 1. This measure provides a finer grain (6 levels) index of performance than judgment of acceleration (2 levels). The results are essentially parallel to those for the judgments of acceleration although a bit less noisy. This may be seen by comparing Table D-7 with Table D-9, Table D-8 with Table D-10, and Figure D-4 with Figure D-5.

The same two observers showed a reversal of the group tendency to give higher ratings of acceleration to the flow acceleration conditions instead of to the edge rate acceleration conditions.

Table D-9

Average 6-Point Confidence Ratings of Acceleration in Experiment 2 as a Function of Flow Acceleration vs. Edge Rate Acceleration

		Flow Rate	
		Constant	Acceleration
Edge Rate	Constant	2.21	3.37
	Accelerating	2.80	3.97
		2.50	3.67

Note: N = 702 per cell. "6" indicates most confidence that a scene represents acceleration and "1" the least.

Table D-10

ANOVA Summary Table for the 6-Point Confidence Ratings of Acceleration in Experiment 2

Source	SS	df	F	p	% Var
Observers	376.5	12			3.92
Blocks	39.0	1	41.77	.0000	.41
Flow Accel. (XA)	954.3	1	54.11	.0000	9.92
Edge Accel.	247.1	1	37.32	.0001	2.57
Initial Flow Rate	993.6	2	31.53	.0000	10.33
Initial Edge Rate	133.6	2	30.08	.0000	1.39
R-Factor	342.8	2	60.57	.0000	3.56
XA by R	149.0	2	46.48	.0000	1.55
Total	9,616.0	2807			100.

Note: There are 127 possible sources of variance in this design. Eighteen sources not listed here accounted for between 1.00 and 3.93 percent of the variance and all involved observer interactions.

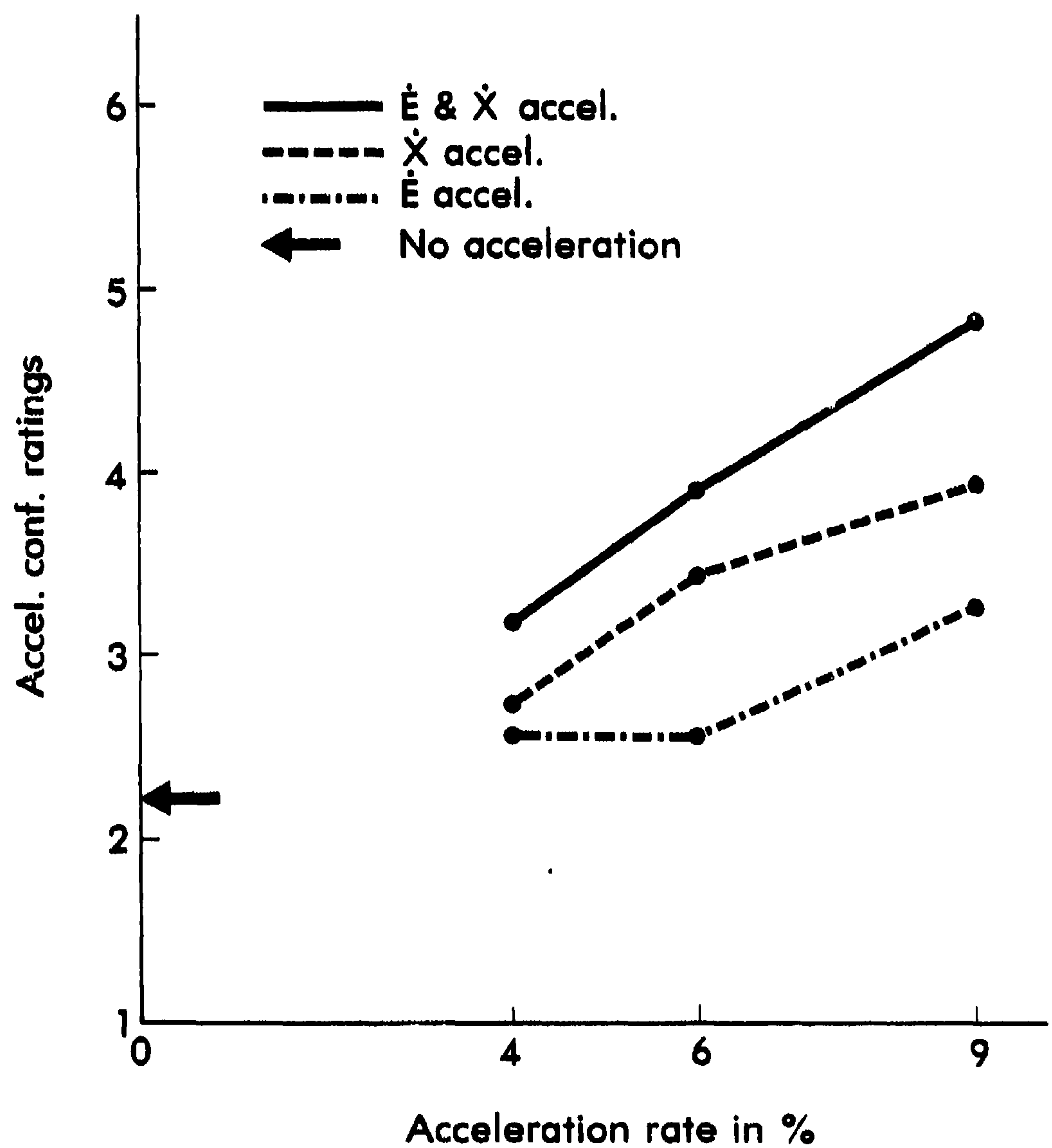

Figure 5. Mean ratings of egospeed acceleration as a function of acceleration rate and type of acceleration information in Experiment 2 (N = 702 for the no-acceleration point, N = 234 all other points, data from 13 observers).

GENERAL DISCUSSION

Experiment 1 showed that edge rate acceleration dominates over flow acceleration in eliciting judgments and ratings of egospeed acceleration. No prediction had been made as to which information would even be used as the experiment was designed to test two contrary predictions based on the work of Denton (1980) and Warren (1982). Denton's work suggested that edge rate information governs the perception of egospeed and Warren's analysis that flow rate does. Experiment 2 was designed as confirmatory to Experiment 1 and hence definite predictions were made. Hence, the finding of the reversal of the dominance of the two information sources and the general lack of increase in acceleration judgments were unexpected.

The most likely reason for the lack of a general increase in judgments and ratings of acceleration in Experiment 2 is the fact that exposure duration was shorter (6.5 sec) in Experiment 2 than in Experiment 1 (10 sec). Whereas the greater acceleration rates in Experiment 2 were intended to make the task easier, the shorter display duration apparently acted to make the task more difficult and hence offset the facilitating effect of the greater degree of acceleration.

Although this explanation is plausible in retrospect, it was not obvious beforehand. The two experiments are actually unusual in that they permit considerably greater viewing times than is typical. In general, many perceptual experiments today measure their presentations in milliseconds.

We chose to use exposure durations considerably above these to add "ecological validity" to the observer's task. Modern ecologically oriented theorists have strongly argued for the importance of permitting observers adequate time to extract information about the environment using their own exploratory and attention strategies (Gibson, 1979). Our displays were deliberately unnatural for an information content purpose; we did not want the information extraction task to be constrained. In particular, we assumed that both the 6.5- and 10-sec exposures were both more than adequate for the pickup task and that any differences in performance in this time range would be due solely to the information available and not due to the time allowed for pickup. That very long viewing times may be necessary in egomotion and aviation situations is suggested by Langewiesche's (1944) observation that "...it actually takes something like 4 or 5 seconds of patient observation ... to get a picture of what is happening" (p. 286). He was specifically speaking about landing an airplane, but a generalization to other tasks is reasonable.

The finding of an exposure time versus acceleration rate tradeoff is interesting and will be pursued in a third experiment. The key feature of the third experiment will be that exposure time as well as rate of acceleration will be independently manipulated.

The exposure time vs. acceleration rate tradeoff is a plausible explanation for the lack of difference in the judgments and ratings of acceleration in the two experiments. However, how such a tradeoff affects the use or relative dominance of flow-only or edge-rate-only acceleration information is not clear. One speculation is that flow rate information is relatively quickly picked up due to the nature of the physiological mechanisms in the retina. Physiologically, this quickness is possible since flow rate is globally (panoptically) defined and is a measure of overall dynamic change in the optic array, and by extension, the retina. The entire retina is implicated in its pickup. Flow rate is theoretically available instantly or near instantly since it is related to a scaling factor in a set of angular velocity vectors which are defined instantaneously.

On the other hand, edge rate and edge rate acceleration are locally defined environmentally, optically, and by extension, retinally. Assuming a fixated retina, a <u>count</u> of edges passing a local reference point (in the environment or the retina) per unit time might have to be made. Since the system must necessarily "wait" for another edge to "pass by" before it can make a "count," edge rate and its acceleration must necessarily involve the passage of considerable time for detection. Thus, global flow rate may be picked up given a short exposure, but local edge rate information may dominate given a long exposure. This hypothesis remains to be tested.

It is important not to be distractd by the apparent dominance reversal and to overlook a major finding of both experiments. In both experiments, both flow-only acceleration and edge-rate-only acceleration individually elicited more judgments and ratings of acceleration than displays with both rates constant and less than displays with both rates accelerating. This indicates that each individual source of information is indeed used by the visual system to some degree and that neither is sufficient by itself. The relative importance of each remains to be determined, but the perceptual utility of each is strongly supported.

The significance of this conclusion is especially interesting for the flow rate factor since flow rate is a global scaling index for activity defined over an entire optic array. The physiological importance is that pickup of global flow rate information is probably not accomplished by any local (small field) mechanism. Mechanisms for the pickup of global optical information need to be investigated.

Lastly, the finding of individual differences in flow versus edge dominance needs to be further investigated. We speculate that there may be individual differences in strategies of information pickup. The identification of such strategies, and of the individuals who tend to use them, may be of use in pilot selection and training.

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APPENDIX E

PERCEPTUALLY RELEVANT METRICS FOR THE MARGIN OF AVIATION SAFETY: A CONSIDERATION OF GLOBAL OPTICAL FLOW AND TEXTURE VARIABLES

Dean H. Owen and Rik Warren

APPENDIX E

PERCEPTUALLY RELEVANT METRICS FOR THE MARGIN OF AVIATION SAFETY: A CONSIDERATION OF GLOBAL OPTICAL FLOW AND TEXTURE VARIABLES

Dean H. Owen   Rik Warren

Department of Psychology and Aviation Psychology Laboratory

The Ohio State University

Columbus, Ohio

A margin of safety exists between a person and an environmental source of injury, i.e., between self and a danger (Gibson, 1964). The margin of safety involves both the closeness of danger in space and the imminence of danger in time. By maintaining a margin of safety in an interactive situation, a person can control the danger. The last possible response point in terms of distance and time is specified in environmental coordinates and metrics such as feet or meters and seconds. Specific values of these variables are determined by the ability to avoid impact, which in the case of aviation is a joint function of the skills of the pilot and the performance characteristics of the aircraft. A point in space, a line from one point to another, and time are not visible, however. Only the layout of surfaces and the relation between the self and surfaces are perceivable. The assumption from Gibson's (1961, 1979) ecological optics is that there is information in the light specific to the closeness and imminence of danger, and that it is of a higher order than environmental variables. What are these perceptually relevant variables, what are their metrics, and what research paradigms are appropriate to study them? The paradigmatic issue will be considered first, followed by a discussion of some variables and several applications.

Choice of paradigms. All perceptual-behavioral studies make use of either the reactive or the interactive paradigm. In the reactive paradigm, the experimenter controls the parameters of the test situation as independent variables. Measures of accuracy and efficiency are dependent variables. In the interactive paradigm, as in the world outside the laboratory, the person being tested controls the stimulation. As a result, the parameters of stimulation can be used as dependent variables, that is, as measures of performance.

In reactive studies, the experimenter initiates a test trial and the trial terminates when a response is made. Precise control of stimulation is maintained, idealized situations can be studied, and sensitivity to isolated variables can be assessed. But only half the perception-action cycle is studied. The other half, during which the person's actions affect what is subsequently perceived so that feedback is obtained about the appropriateness of the action, is left unstudied.

In interactive studies, the individual repeatedly loops through the perception-action cycle until the task is successfully completed or until some constraint is reached. One class of constraints includes environmental values which exceed the design characteristics of the aircraft, leading to, for example, stall or excessive vertical velocity at touchdown. A second class includes optical (that is, informational) constraints. An example is absence of optical flow acceleration when path speed is reduced at the same rate as altitude is lost. Another case is that of little or no optical texture due to a lack of surface texture, as during flight over a dry lake bed, calm water, or terrain with no lights at night (see Kraft, 1969). A third class involves constraints on the perceiver. Examples include values of optical variables below a detection threshold or above the resolving power of the visual system, as in the case of blur. All three kinds of constraint can result in a mishap, and all kinds may interact with each other as well as with the control skills of the pilot.

A researcher can take advantage of the best features of both paradigms by (1) conducting ecological surveys to determine the kinds and ranges of variables encountered in performing a task, (2) studying sensitivity to the variables in reactive laboratory experiments, and (3) assessing the usefulness of variables and training to attend to them in interactive experiments. (See Warren & Owen, in press, for a discussion of constraints involved in designing experiments on self-motion perception in aviation.) Surveys can be conducted during actual flight or recordings of performance can be made during maneuvers carried out in a simulator. In turn, transfer of training can be assessed in a simulator or in actual flight.

Mishaps and misperception. Accidents occur for two reasons that involve perception and action: (1) misperception of the danger or failure to perceive the danger altogether, or (2) inappropriate reaction in a dangerous situation or failure to act at all (Gibson, 1964). Since events take time to unfold, even a pilot who is very sensitive to the information specifying an event and highly experienced at controlling an aircraft must show some patience. If he is impatient, overconfident, or under stress, he may act in a way that has frequently been successful in the past, rather than on the basis of information which is becoming available. Acting on the basis of a response bias instead of current information is obviously risky. A skilled pilot, having experienced most of the situations the flight environment has to offer and having developed a repertoire of highly automated control reactions, may be able to turn control of the aircraft over to the environment. In stressful situations there is no time for processing, reasoning, judging, or interpreting to intervene between perceiving and acting appropriately. Such mediating activities may in fact interfere with performance in addition to reducing the margin of safety.

Misperception or failure in perceiving may occur (1) because the person did not know where to look, how long to look, or what to look for, or (2) because there is inadequate information or information that specifies more than one state of environmental affairs. The ecological approach defines perception in terms of the reciprocal relation between the perceiver and the surrounding environment, not as something that occurs in the nervous system. Accordingly, the explanation of misperception may be (1) primarily environmental, as in cases where there is little surface texture to provide for optical stimulation, or (2) primarily the fault of the individual for not producing adequate kinds and levels of stimulation or not attending to what is available. What might these variables of stimulation be?

Global optical variables and invariants. When the eye approaches a surface (Gibson, Note 1) or a surface approaches the eye (Schiff, 1965), the flow pattern of optical discontinuities in the optic array is specific to the event. Warren (Note 2) has mathematically decomposed the flow pattern into components to which perceived self motion may correspond. For our purposes here it is sufficient to note that global optical flow rate varies with distance of the eye from a surface. Perceived self speed correlates with flow rate as evidenced by the experience that one is moving very rapidly in a plane close to the ground, but very slowly in a plane at high altitude. The rate at which surface texture edges with stochastically regular spacing are traversed and occluded during forward motion is the same in both cases, so that perceived speed does not appear to have a ground-speed metric like edges per second. One's own speed, instead, appears to be scaled in altitude units[1] which denote the height of the eye above the ground surface:

$$\text{Global optical flow rate} = \dot{s}/z \quad \text{(in eyeheights/sec)} \tag{1}$$

Table 1 shows some representative global optical flow rates to give the reader a feeling for speed calibrated in the eyeheight metric. Comparisons of events having identical flow rates reveal that rates encountered in flight are well within the range of those experienced during walking, running, and driving. Flight, however, emphasizes the two-dimensional nature of the problem, since flow rate varies with changes in altitude as well as speed. And the consequences of impact correspond to environmental speed rather than flow rate.

Are accidents related to optical variables? Three suggestive examples will be explored.

Taxi speed and eyeheight.[2] When commercial airline pilots first made the transfer to the new Boeing 747, they were instructed that 15 knots was a safe speed for a 90-deg turn while taxiing, as it had been with the 707 and 727 aircraft. A number of pilots attempted turns at 20 to 25 knots, damaging the nose gear and leaving the runway or taxiway in the process. Eventually pilots were instructed to use an instrument to determine actual taxi speed (Boeing Company, Note 3).

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[1]The following notation system will be used:

- g = ground texture unit size
- x = ground distance to the instructed touchdown point
- z = altitude = eyeheight (h)
- $\dot{e}$ = edge rate (surface texture edges traversed per second)
- $\dot{s}$ = path speed
- $\dot{x}$ = ground speed
- $\dot{z}$ = climb rate (descent rate when values are negative)
- $\ddot{x}$ = acceleration in ground speed

[2]Related by Captain Harry W. Orlady, a 10-year Boeing 747 pilot.

Table 1

Global Optical Flow Rates

Event	Speed (in ft/sec)	Eyeheight (h) (in ft)	Flow Rate (in h/sec)
Brisk walk	5.5	5.5	1.00
9.65-sec 100-yd dash	31.1	5.5	5.65
Car at 34.7 mph	50.9	4.5	11.30
Car at 69.4 mph	101.8	4.5	22.60
Plane at Mach 1[a] (770 mph)	1,130	50	22.60
"		100	11.30
"		200	5.65
"		1,130	1.00
"		11,300	0.10

[a]The speed of sound has not been adjusted for change in altitude in these examples.

Why the excessive taxi speed? As shown in Table 2, optical flow analysis provides an explanation. Prior experience of these pilots was with aircraft in which they had lower taxiing eyeheights, and consequently higher flow rates. Boeing 707 and 727 pilots, for example, have a taxiing eyeheight of 13 ft. To produce a flow rate equivalent to that produced by taxiing at 15 knots in a 707 or 727, a 747 pilot would have to achieve a speed of about 34 knots. The evidence indicates that they were well on their way to a speed of 2 eyeheights/sec when accidents occurred. Flow rates identical to the safe speed of 15 knots are shown for driving and walking to indicate how slow the 747 pilot might feel that he is moving.

It appears, then, that transfer from one eyeheight to another can result in problems, supporting the notion that flow rate is the information for speed rather than edge rate, which remained the same.

Table 2

An Optical Flow Analysis of Taxi Speeds

Event	Speed (in knots = mi/hr = ft/sec)			Eyeheight (h) (in ft)	Flow Rate (in h/sec)
Boeing 707 or 727	15.0	17.3	25.3	13.0	1.95
Boeing 747	33.5	39.2	56.5	29.0	1.95
Boeing 747	15.0	17.3	25.3	29.0	.87
Car	2.3	2.7	3.9	4.5	.87
Walking speed	2.8	3.3	4.8	5.5	.87

Inadequate visual information for landing. From flight path and speed data, it is possible to recover some of the information available to the pilot that performed the recorded maneuver — that available in the global optical flow pattern. The examples of flow analyses which follow are based on data from a study by Kraft, Anderson, and Elworth (1980) using the Redifon Boeing 747 simulator fitted with a General Electric Compuscene computer generated imagery system.[3] Experiment 2 factorially contrasted narrow versus wide fields of view and simple versus complex ground surface textures. The narrow field was limited to the forward display extending 20 deg to either side of the straight-ahead viewing centerline. The wide field included the forward display plus oblique and side displays for a total of 114 deg in front and to the left of the Captain's position. All displays extended 30 deg vertically.

The simple surface consisted of a blue-black 300 x 10,000-ft runway on a tan desert ground with blue sky above the horizon. The runway had no markings. The complex surface contained the details normally available in the Moses Lake, Washington, data base used for flight crew training, including rows of diamond shaped fields on either side of the runway. This artificial texture was added to give pilots more information when they were close to the ground. The runway and sky were the same as in the simple surface condition.

Sixteen Air Force Military Airlift Command pilots each made four landings in each of the four conditions. All were current in the C-141 military air transport, but had no prior experience in the 747. All approaches were straight in, beginning 4.7 nautical miles from runway threshold at about 1350 ft altitude with the aircraft trimmed for a 2.5-deg path angle. The landing gear was down and flaps were at full 30-deg throughout the approach. Dependence on visually guided flight was ensured by removal or occlusion of all instrumentation except the airspeed indicator. The simulator motion base was active during all trials.

The pilot was instructed to proceed straight in to a minimum-descent-rate touchdown at 1000 ft beyond the runway threshold. Among other variables, x, z, s, and z were recorded every 450 msec. These variables were used to compute and plot the flight path, optical flow rate, flow acceleration, and fractional loss in altitude.

Flow rate undergoes an explosive increase as the ground is approached at a constant or nearly constant speed. Therefore, the magnitude of optical flow acceleration is potentially a source of information about closeness to the ground.

$$\text{Global optical flow acceleration} = (\dot{s}/z)(\dot{z}/z) \qquad (2)$$

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[3]Appreciation is extended to Conrad L. Kraft of The Boeing Company for his generosity with both the original data and his time.

Results of an experiment contrasting several candidates for information used in detecting loss in altitude indicated flow acceleration and flow rate have functions in event perception analogous to figure and ground in static object perception (Owen, Warren, & Mangold, in press). A given flow acceleration is more or less detectable depending on the flow rate on which it is superposed. As a consequence, the functional information for detection of loss in altitude is the fractional increase in flow rate, which is identical to fractional loss in altitude.

$$\text{Fractional increase in flow rate} = (\dot{s}/z)(\dot{z}/z)/(\dot{s}/z) = \dot{z}/z \quad (3)$$

Six mishaps were identified in the Kraft et al. (1980) data by using Boeing Company criterial values for vertical velocity ($\dot{z}$) for the time sample just before touchdown. Acceptable operational values range from -1.8 to -2.4 ft/sec, with -10 ft/sec as a "maximum" value, and -15 ft/sec as a "disaster." All mishaps were in conditions with the desert ground surface, three with the wide field of view and three with the narrow field of view. Observed values for these landings are presented in Table 3, averaged over the last linear path segment before flare and as recorded just prior to touchdown.

For comparison, data from three safe landings are also presented. One is an ideal, by-the-book landing (Boeing Company, Note 3). The second is by Pilot C, who had difficulty choosing a path appropriate to achieve the instructed touchdown point during an approach over the desert surface. The third safe approach, made by Pilot E over the Moses Lake surface, was chosen for comparison with the second landing by the same pilot having an excessive sink rate. Field of view was 40 deg for both his safe and unsafe touchdowns.

Of the pilots with excessive sink rates at touchdown, only Pilot A made contact short of the runway (by 1448 ft) and had a high airspeed. The latter produced a high flow rate at touchdown, but in general flow rate was not highly correlated with sink rate. Pilot A initiated flare just before touchdown, and Pilot B had not begun to flare. Both were too low, perhaps because they were still trying to determine their altitude relative to runway optical size during the first four landings. By the fifth through eighth landings, Pilots D and E were flaring at reasonable altitudes, and Pilots A and B produced no excessive sink rates.

Using vertical velocity to define a mishap guarantees, of course, that fractional loss will be very high at touchdown. Nevertheless, Owen et al. (in press) found that even nonpilots could detect a fractional loss of 12.5% on 96% of the trials in a condition relatively rich in optical texture. Lack of surface texture surrounding the runway apparently makes the imminence of collision with the ground difficult to detect. Are the global optical flow variables produced by the pilot diagnostic of whether visual information is adequate or inadequate?

Two examples of eyeheight-scaled variables are illustrated using three of the 747 approaches. In Figures 1, 2, and 3, the pilot's eyeheight is shown as a function of distance, followed by global optical flow rate ($\dot{s}/z$), a correlate of apparent speed (Warren, Note 2), and fractional loss in altitude ($\dot{z}/z$), a correlate of sensitivity to loss in altitude (Owen et al., in press). All three variables are plotted against distance from the instructed touchdown point, 1000 ft beyond the runway threshold.

Table 3

Data from Kraft et al. (1980) Comparing Unsafe with Safe Approaches and Touchdowns

		Last Linear Path Segment						At Touchdown				
Pilot	Landing	$100(\Delta z/\Delta x)$[a] (%)	Mean $\dot{x}$ (ft/sec)	Mean $\dot{z}$ (ft/sec)	x_{TL}[b] (ft)	z_{TL} (ft)	$(\dot{z}/z)_{TL}$ (%/sec)	x_{TD}[c] (ft)	$\dot{x}_{TD}$[d] (ft/sec)	$\dot{z}_{TD}$ (ft/sec)	$100(\dot{z}/z)_{TD}$ (%/sec)	$(\dot{s}/z)_{TD}$ (h/sec)
		Boeing Ideal Values						Boeing Ideal Values				
		-4.37	243	-10.6	1300	72	-17.7	0	243	-2.0	-4.8	5.79
		Desert Surface, 114 deg Field of View						Desert Surface, 114 deg Field of View				
A	4th	-4.91	257	-13.1	2550	34	-41.2	2448	257	-12.6	-41.2	8.39
B	1st	-6.08	239	-12.5	350	35	-44.1	350	238	-15.2	-44.1	6.90
B	3rd	-7.51	226	-15.7	49	38	-46.5	49	223	-17.8	-46.5	5.82
		Desert Surface, 40 deg Field of View						Desert Surface, 40 deg Field of View				
C	5th	-4.07	246	-8.7	383	32	-27.7	383	245	-8.9	-27.7	7.64
D	6th	-6.79	238	-16.9	800	60	-24.1	520	233	-13.8	-34.2	5.83
E	6th	-4.62	244	-12.3	-100	70	-15.6	-577	231	-13.4	-34.3	5.95
E	7th	-8.22	240	-19.9	900	65	-32.4	653	234	-19.2	-46.5	6.68
		Moses Lake Surface, 40 deg Field of View						Moses Lake Surface, 40 deg Field of View				
E	14th	-5.43	240	-14.7	1350	109	-13.0	-204	230	-4.3	-9.7	5.14

[a] $100(\Delta z/\Delta x)$ = path slope.

[b] Subscript TL denotes termination of last linear segment and initiation of flare, if flare occurred.

[c] Positive x_{TD} denotes undershoot, a negative value indicates overshoot, and zero is 1000 ft from runway threshold.

[d] Time-varying variables were computed during the last 450 msec before touchdown.

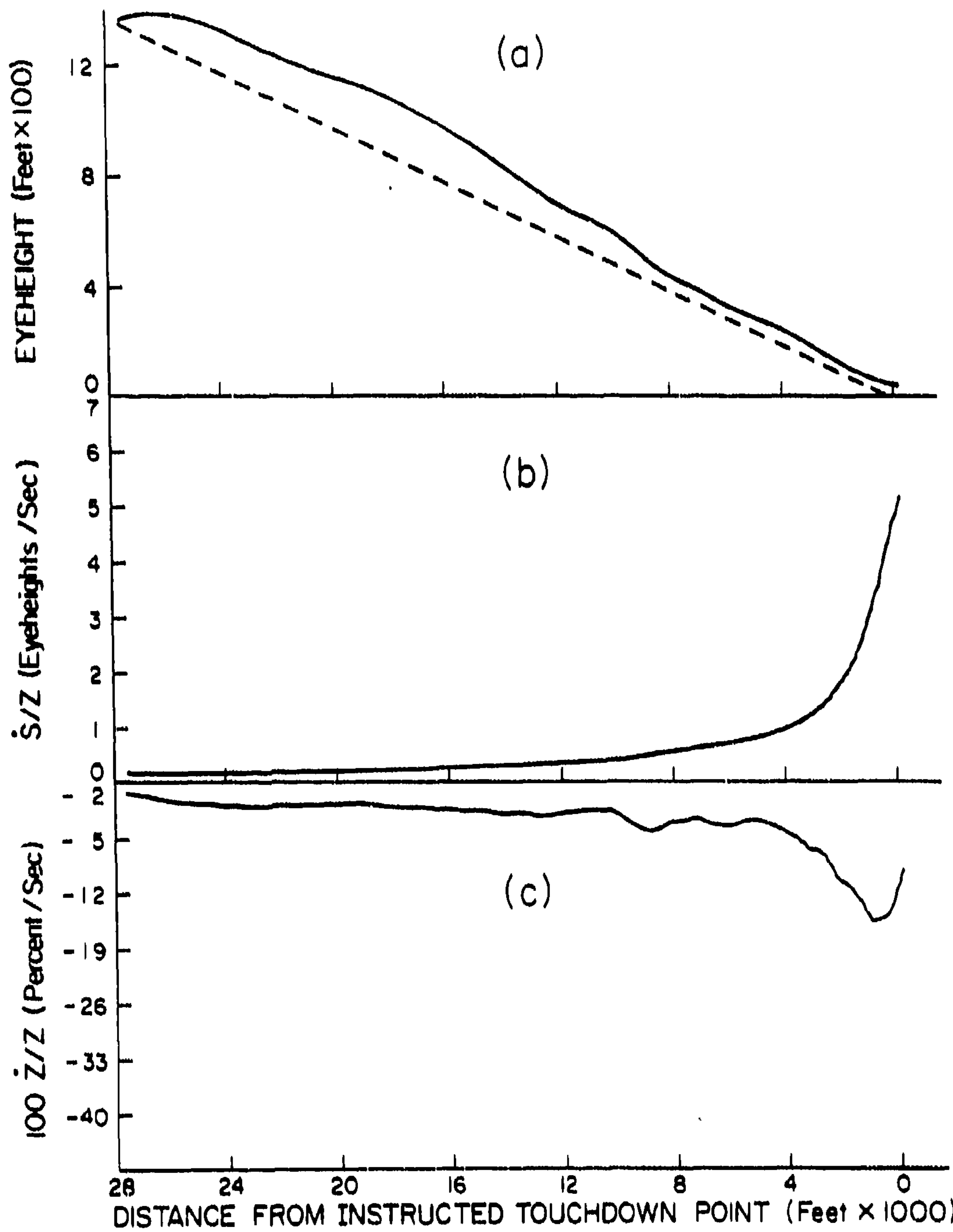

Figure 1. Eyeheight (a), global optical flow rate (b), and fractional loss in altitude (c) for Pilot E, Moses Lake ground surface, 40 deg field of view.

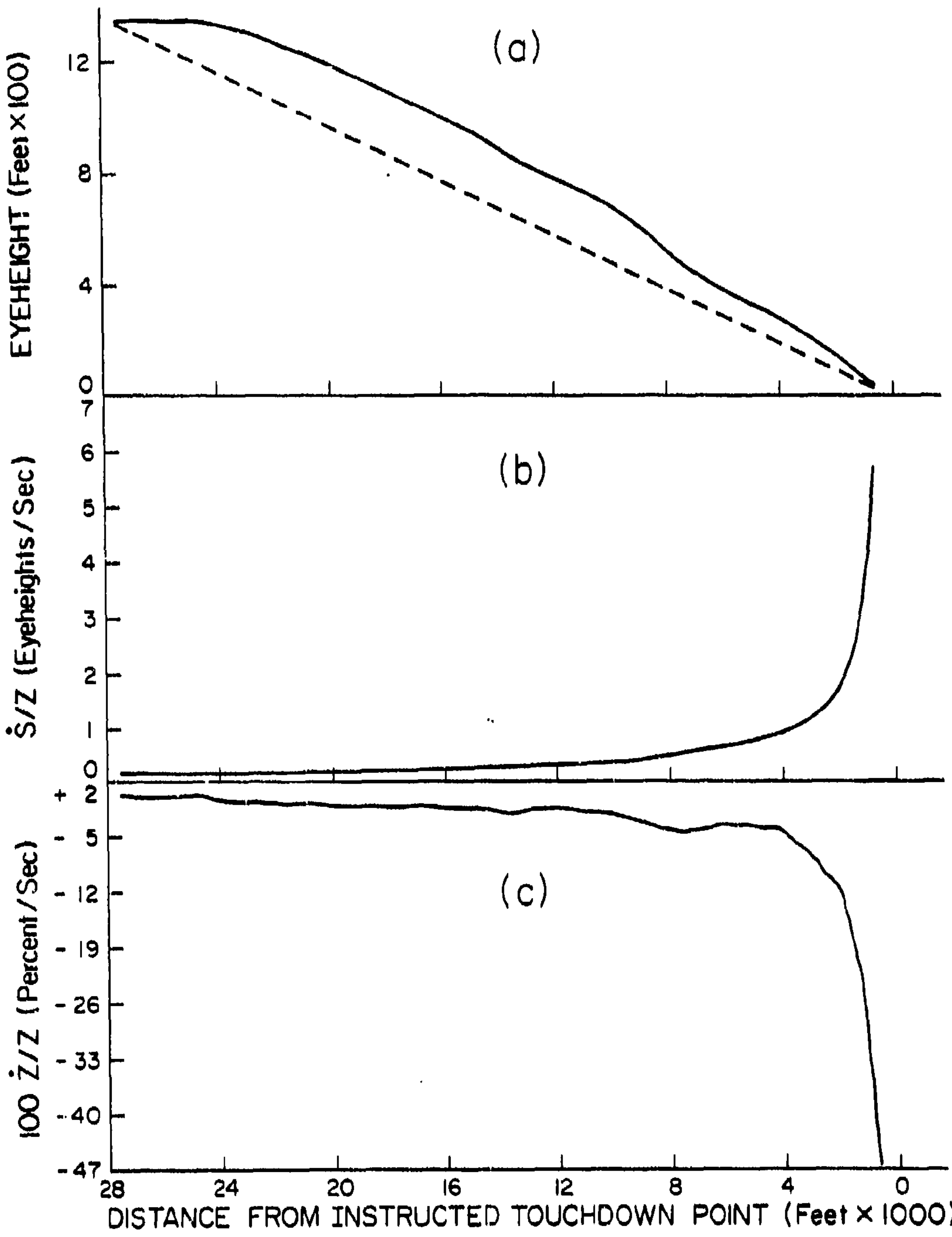

Figure 2. Eyeheight (a), global optical flow rate (b), and fractional loss in altitude (c) for Pilot E, desert ground surface, 40 deg field of view.

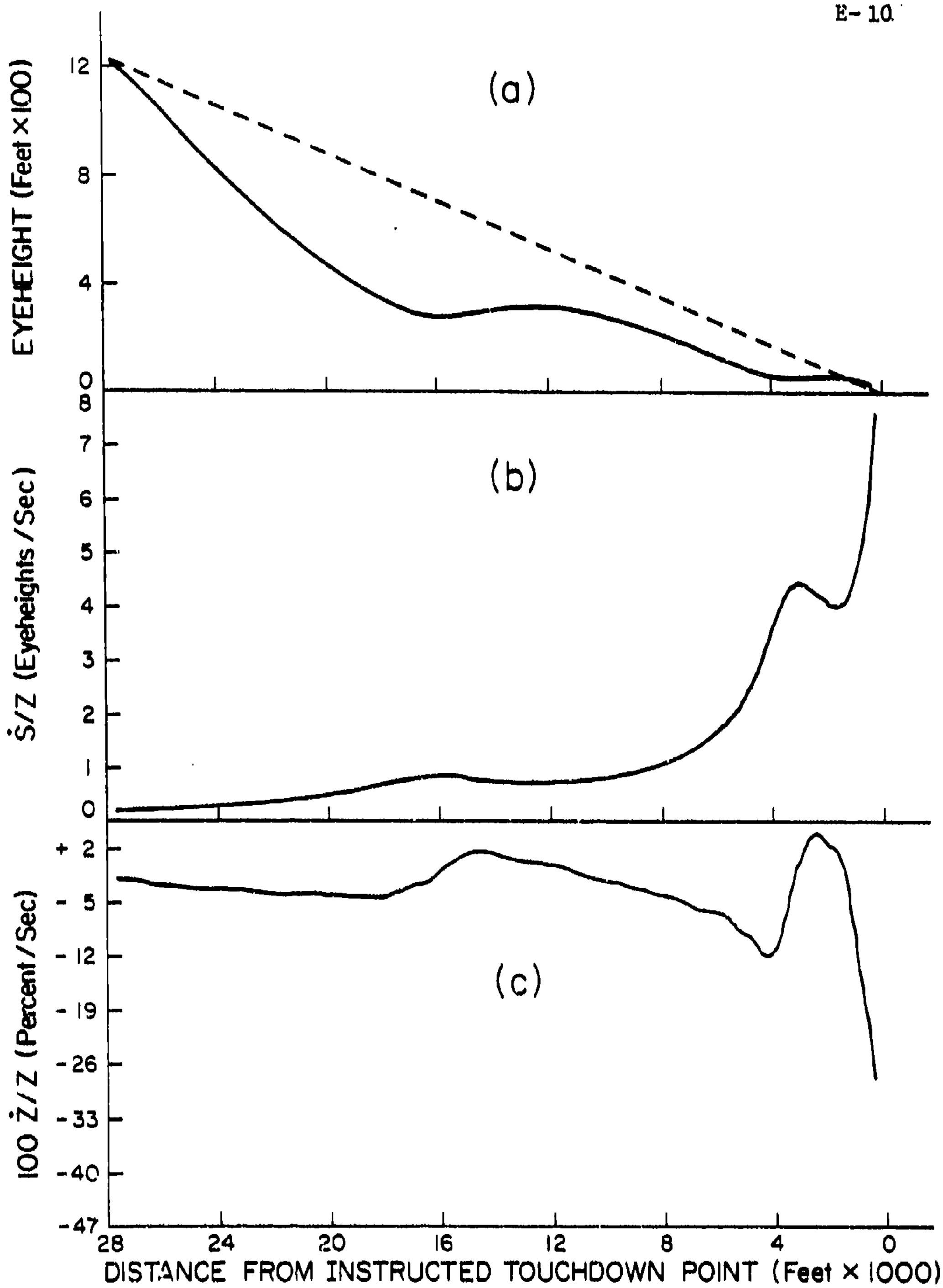

Figure 3. Eyeheight (a), global optical flow rate (b), and fractional loss in altitude (c) for Pilot C, desert ground surface, 40 deg field of view.

Two of the approaches were made by the same pilot (E), both with the narrow field of view. The first (see Figure 1-a) was over the Moses Lake ground surface, and was chosen because it matched the second closely except for flare and touchdown. Vertical velocity at touchdown was in the "safe" range, at -4.33 ft/sec. The second approach (see Figure 2-a) was made over the desert ground surface with very little flare before touchdown at -19.18 ft/sec, a value well into the "disaster" range.

As shown in Figures 1-b and 2-b, the major difference in the flow rates is in the rate of increase just prior to touchdown. Optical acceleration is greater, more "explosive," in the approach with the higher vertical velocity. The approaches differ more radically in fractional loss in altitude (compare Figures 1-c and 2-c). Over Moses Lake, a slower, negative increase is followed by a reduction in the relative rate of loss, whereas over the desert, the function shows an explosive negative increase. This difference suggests that the information for detecting rate of loss in altitude is more salient when there is more surface texture.

The third approach illustrates a strategy taken by Pilot C during his first approach with the 40-deg field of view over the desert surface. Figure 3-a shows a path that was too steep at first, followed by two more appropriate path-slope segments each preceded by increases in altitude. The three successively shorter and less steep path segments suggest the increasing salience of eyeheight scaled information as one approaches the ground. The more cautious final approach is revealed in Figure 3-b, where optical acceleration was delayed as the pilot "felt out" his altitude before touching down with a vertical velocity of -8.9 ft/sec. Fractional loss in altitude indicates the same careful pattern of behavior (see Figure 3-c), showing how a pilot can use fractional changes in optical variables in an exploratory fashion.

It seems clear that the pilots maintained a reasonable airspeed and, with practice, initiated flare at a reasonable altitude. What they appear to be misperceiving is their vertical velocity and/or their path slope. If path speed is nearly constant, then vertical velocity and path slope are confounded. The greater the vertical velocity, the steeper the path slope.

Why is vertical velocity, or more likely, fractional loss in altitude, so difficult to detect over the desert surface? Since the only difference in the two ground surfaces was texture density, the explanation undoubtedly involves optical texture density. In developing an experiment on sensitivity to loss in altitude (Owen et al., in press), it was discovered that at least three sources of information for descent covary when descent rate is constant: (1) fractional change in optical flow rate, (2) fractional change in optical density, and (3) optical (perspectival) splay rate. During approaches to the desert runway there are fewer optical discontinuities to convey flow rate and splay change, and there is only one ground texture unit — the runway — to undergo optical magnification.

Brown (1976) reported that descent rates at touchdown are typically higher in the simulator than in the aircraft, sometimes by as much as a factor of two. Even halving the sink rates for the desert conditions results in unacceptably high values, however. The simulated desert surface was designed to lack texture elements, of course, but many real-world areas surrounding runways are nearly as texture deficient.

In an analysis of inadequate visual information as a factor in aircraft accidents, Kraft (1969) emphasized the difficulty of judging sink rate over water or unlighted terrain at night. More work is obviously necessary to isolate the necessary optical information for controlling descent rate, but the data available suggest that in places where surface texture is sparse or absent, sufficient artificial texture in the form of buoys, painted areas, and lights should be provided. Where such texturing is not possible, education of pilots concerning the problem may be the only alternative.

Adaptation to optical flow compensated for by increases in optical density. A driver on a freeway attempting to maintain a constant speed will often find his speed steadily increasing. Pilots report a similar effect while attempting to fly at constant altitude and speed. Since adaptation to prolonged stimulation of a particular kind is a pervasive perceptual phenomenon, it is reasonable to assume that the driver or pilot is increasing environmental speed to hold apparent speed constant.

Denton (1976) studied the effect in a driving simulator with a visual scene generator. He selected two groups of 12 observers each on the basis of whether they experienced a large versus almost no visual motion after-effect following prior exposure to a visual field of rectilinear motion. The initial velocity of the road scene was set at some velocity, e.g., 70 mph, then the observer was given complete control of the display speed and told to hold it constant via a hand speed control. The group having a large motion after-effect increased the speed rapidly at first, then leveled off. In the 70-mph condition, the curve reached asymptote after about 75 sec at a velocity of 83 mph. The slower the initial speed, the lower the asymptotic speed and the sooner it was reached. The low motion after-effect group showed no increase in speed for 11 of 12 observers.

Having demonstrated that observers increase their actual speed to maintain a constant perceived speed, Denton (1980) explored the relation of the phenomenon to driving accidents. In Great Britain, accidents frequently occur at the approaches to traffic circles following periods of high-speed driving. Denton studied the effect of increasing edge rate by exponentially reducing the spacing between horizontal stripes across the road surface. In a simulator experiment, the observers experienced a randomly textured road surface at a constant speed for a half mile, then were asked to halve their speed using the hand control. With rapidly reduced spacing, the produced speed was less than half the prior speed, as compared to a control condition with continued random texture which resulted in a produced speed of greater than half.

Exponentially decreasing spacing was then applied before a traffic circle on a dual-lane motorway. Speed was reduced by 28.6% between 9:00 and 11:00 A.M. and by 18.5% between 6:00 and 8:00 P.M. This difference may reflect the differential sensitivity of central and peripheral vision during day and night conditions (Liebowitz & Owens, 1977). The stripes and their spacing, being mostly in the central field, may not be as detectable at night. Stripes installed at 37 other sites throughout Great Britain resulted in an accident rate decrease of nearly two thirds.

Thus, it appears that progressively increasing edge rate (optical density) over time can produce an effect of increased apparent self speed. Driver cancellation of this effect produces lower speeds, thereby increasing the margin of safety and reducing accidents. Experiments in our laboratory contrasting edge rate and flow rate as determinants of perceived acceleration versus constant speed indicate that the two effects are additive, but that some observers are more sensitive to edge rate and some more sensitive to flow rate (Warren, Owen, & Hettinger, Note 4).

If a pilot attempting to fly at constant altitude and speed adapts to flow rate, there are two ways to compensate: (1) He can increase actual speed and/or (2) lose altitude to hold apparent speed constant. Both changes will increase flow rate, and both decrease the margin of safety, especially during low-altitude flight. Whether adaptation influences sensitivity to subsequent change in speed (Owen, Warren, Jensen, Mangold, & Hettinger, 1981) or change in altitude (Owen et al., in press) are questions that require empirical attention.

A transition from flight over some particular texture size and density to stochastically larger or smaller texture may have similar short-term effects. For example, a transition to flying over smaller, more dense surface texture may lead a pilot who is attempting to hold optical density and edge rate constant to lose altitude and increase speed. Again, the effect would be to reduce the margin of safety.

Conclusions. The three examples of mishaps involving self-motion perception suggest that the metrics for the margin of safety are not arbitrarily environmental metrics. Rather they are the metrics of perceptually relevant, higher-order, relational variables. Candidates are eyeheight scaled variables and percent-per-second changes (cf. Lee, 1976).

The optical conditions prevailing before accidents can be surveyed, categorized, and mimicked in simulators so that inadequate sources of information can be isolated and then enriched as needed until adequate to enable maintenance of the necessary margin of safety — first in simulators and then in operational situations. Individual differences in pilots and within-individual consistency can be tested, training and transfer can be assessed, simulation systems can be evaluated and compared, and the informativeness of sensor displays can all be expressed in the optical metrics appropriate to self-motion perception.

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Requests for reprints should be addressed to Dean H. Owen, 404-C West 17th Avenue, Columbus, OH 43210.

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APPENDIX F

PERFORMANCE MEASURES AND THE VISUAL CONTROL OF FLIGHT

Susan J. Mangold

APPENDIX F

PERFORMANCE MEASURES AND THE VISUAL CONTROL OF FLIGHT

Susan J. Mangold
The Ohio State University

In the quest for solutions to problems in aviation a number of performance measures have been developed and used. Unfortunately, the rise in number of measures has not been matched by a corresponding interest in determining the effectiveness of each measure in assessing variations in performance due to experimental variables. Nor have there been many attempts to identify which measure(s) provides(s) most satisfactory choice for use with a particular issue and a particular task.

As a first attempt at resolving these difficulties, research which has used three classes of performance measures in investigations of visual variables is reviewed: (1) system measures, which reflect the combined output of the pilot and the airplane; (2) plant measures, which represent the altitude of the aircraft; and (3) pilot control adjustments, which measure the pilot's manipulation of the various airplane controls separate from the resulting effects on the airplane.

System and plant measures are probably the most popular classes of performance measures used to assess the effects of experimental manipulation influencing either the pilot or the aircraft. Execution of a task can be analyzed in terms of the aircraft's path or position relative to the ground surface, including its path along an axis parallel to the surface, lateral deviations from this path, and vertical changes in height or altitude. Beyond this, the aircraft's altitude, consisting of the three axes of yaw, pitch, and roll can be measured.

Control adjustment measures, on the other hand, have been used much less frequently. Probably the major difficulty hindering the use of these measures arises from uncertainty as to how to analyze the large quantity of data that accumulates even for short-duration tasks. In addition to considering the various approaches to data analysis, consideration must also be given to the question of whether these measures provide valuable information beyond that made available by system measures.

The present paper encompasses the research on visual or contact landings, and other visual maneuvers where the manipulated variables concern the effects of altering the nature of the information available outside the cockpit. At issue is the problem of identifying changes in performance accuracy resulting from variations in the amount and kind of visual information available to the pilot. Manipulated variables include scene complexity, day versus night landings, restricted visibility, and monocular versus binocular vision. Several experiments have been included which were examinations of variation in performance due to pilot learning and experience as well.

System Measures

The need for an evaluation of system measures arises from two issues. First, the vast number of measures available to the researcher emphasizes the necessity of developing criteria upon which a choice can be based. Such criteria conceivably could be derived from analysis of the conditions under which a measure has proved successful in the past at providing useful information about the variables of interest. A second argument for evaluating system measures is to specify the limits of their capability in reflecting variations in manipulated variables. The extent to which system measures are sensitive to pilot variables has been questioned (e.g., McCoy, 1963) because of the difficulty inherent in attempts to separate the performance of the human from that of the machine.

By definition, system measures quantify the performance of the overall system, to which the characteristics of both the human and the machine contribute. In the case of ground-referenced maneuvers, such as the visual landing task, problems can arise for two reasons. Temporal delays between pilot actions and airplane responses to these actions are inevitable. A further complication arises because of the airplane and control dynamics interposed between the pilot and the system output. Pilot response to a perceived event becomes apparent only after a time delay and only after translation into a system output. Inappropriate system performance could occur either because of failure to attend to the relevant information, because the pilot acted inappropriately relative to the system dynamics. System measures are meant to be used at the level of the system. The extent to which such measures can be successfully applied at the level of the pilot must be ascertained separately.

Some support for this contention can be found in several studies reviewed in this paper (e.g., Irish, Grunzke, Gray & Waters, 1977; Jensen, 1979). Both studies identified experimental variables to which control adjustment measures alone proved sensitive. Furthermore, Irish et al. suggest that system and control adjustment measures differ in terms of the category of experimental variables to which each class of measures is most sensitive. In their study, system measures best reflected environmental variables, such as reduced variability, whereas control adjustment measures were more sensitive to manipulations of the simulator itself, such as field of view (FOV).

To determine the conditions under which the various system measures are best used, at least six variables must be considered.

(1) The experience of the pilots who serve as experimental subjects. It is possible that some measures which adequately differentiate

experienced from low-time pilots would not be sensitive to differences within either group. For example, some studies (e.g., Miller, 1971) have shown that wileron-rudder coordination discriminates experienced from inexperienced pilots in the performance of certain maneuvers. However, Miller also found that the inexperienced group failed to demonstrate improvement on this measure through the course of the experiment, a result which suggests the measure is not as sensitive to performance differences at varying low levels of experience.

(2) Type of aircraft or simulator used in the experiment. Depending upon the type of maneuver under consideration certain, aircraft might not show differences on a specific measure under normal conditions of flight. Small propeller-driven aircraft, during high power or high angle of attack flight, may require the use of rudder to offset the affects of torgue. This phenomenon does not take place in non-propeller aircraft, including jets. Consequently, the rudder and yaw measures might reflect the effects of experimental variables (e.g, pilot experience) during maneuvers such as takeoff and slow flight in one type of aircraft, but not the other.

(3) Type of maneuver. Clearly, the maneuver under study will influence the selection of system measures. A preliminary narrowing of potential system measures could be made at this stage.

(4) Segment of the maneuver. Not only will the type of maneuver affect the selection of performance measures, but also the segment of the maneuver must be considered. During the landing task measures which are useful during the approach may be less effective at touchdown. Although some measures can clearly be rejected on the basis of the

maneuver or segment, empirical data might provide additional limitations on the use of specific measures.

(5) Information available to the pilot. Variations in the type of information available to the pilot, be it inside or outside the cockpit, might be expected to have specific consequences for certain measures. For example, night landings, with the corresponding reduction in certain types of visual information, might affect altitude control while failing to affect lateral position relative to the centerline of the runway. Consideration must be given also to the instruments to which the pilot has access in order to determine whether restrictions in one information source can be augmented through other sources.

(6) Location at which performance is recorded. It is common for researchers to select one or more specific spots during the maneuver, or maneuver segment, where performance will be recorded. Selection of the locations is usually based upon a logical analysis of the task in terms of where the experimental variables might be expected to have their greatest influence on performance. The problem can arise that the optimum locations were not selected, especially when the measure specifies an altitude or altitude velocity, such as pitch and roll angles and rates. Also, all of the measures tend to be recorded at the same locations and it may be the case that certain measures which change before related measures should be assessed earlier. High pitch angle during the landing phase may be followed later in the maneuver by large changes in glide path and altitude. This relationship can be observed when the three measured are recorded at the same location.

A useful approach to specifying the conditions under which one measure should be used in place of another involves analyzing each experiment reviewed in terms of the above factors where appropriate. Within each of the categories produced by these six factors the success of each measure employed can be evaluated. Useful information is provided in either the case in which a measure was satisfactory or when a measure failed to demonstrate sensitivity to the manipulated variables. It is hoped that principles governing the selection of a useful measure can be devised.

Complicating the specification of these conditions is the way in which the data from each measure are analyzed. Typically, actual performance is compared to some ideal or desired level of performance. Analyses of absolute error, signed error, and standard deviation, among others, are all possible. Significant differences obtained with one analysis do not guarantee that the other analyses will also prove significant. Finally, care must be taken that the researcher's notion of desired performance matches that of the subject-pilot. It is quite possible that deviations do occur, particularly in the case where the researcher expects more accurate performance than is demanded by the task. Armstrong (1970) warned his pilots against emphasizing lateral performance to the detriment of vertical performance. He was concerned that, because lateral deviations are more obvious to the pilot, this dimension would receive greater attention under experimental conditions, with the knowledge that performance is being recorded, than would occur in a normal flight.

Bearing the above considerations in mind the available literature on visual maneuvers using system measures is reviewed. Experiments included are classified first by performance measure, then by task, and finally by independent variable.

Altitude

One of the most commonly used system measures in aviation research is altitude, especially when the experimental task involves landing the aircraft. Its inclusion in so many experiments may stem from the clear relationship between maintenance of a required altitude and safety. This concern is supported by the observed difficulty many pilots have in judging altitude under reduced visual conditions. Accidents which occur during the landing phase often have as their cause the failure of the pilot to maintain a safe altitude, in spite of there being instruments in the cockpit registering the aircraft's dangerous descent below the glide slope.

Approach accidents are most likely to occur either at night or under low-visibility conditions when the pilot is tempted to look primarily outside of the cockpit despite serious deficiencies in the availability of critical visual information (Cotton, 1978; Kraft, 1969). Because of the association between reduced visual conditions and failure to maintain safe altitude, this measure tends to be used whenever the experimental manipulation involves modifying the availability of visual information.

A related interest in the altitude measure arises from the current concern with determining what information must be represented in the visual scene used in conjunction with flight simulators. In both cases, the assumption is that altitude is sufficiently sensitive to such visual manipulations as the field of view (FOV), scene complexity, and visibility as well as the pilot variables of learning and experience, to be of use in research. A preliminary evaluation of this assumption is possible by exploring the results obtained in experiments which have used the altitude measure.

For the landing task, the altitude measure is very similar to glideslope deviation. As used here, altitude analyses are based upon differences in height above the ground. Glideslope deviation, on the other hand, refers to deviations from an ideal glideslope to the runway. During the descent toward the runway, altitude will change while glideslope deviation will not if the pilot succeeds in remaining on the prescribed path. Experiments reviewed here are classified according to this distinction. Unfortunately, this distinction can be cloudy in some cases.

The Straight-in Approach. Two experiments have used the altitude measure to assess the effect of restricting the pilot's FOV and varying the visibility of the runway. Armstrong (1970) measured the landing performance of four highly experienced Royal Air Force pilots in a Varsity airplane as a function of two levels of FOV: (1) the unrestricted FOV normally found in the aircraft, which is approximately 258 degrees (4.5 radians) wide, and (2) a restricted (0.43-radian) FOV obtained by covering the side windows. The vertical dimension, unspecified by Armstrong, was identifcal for the two conditions. For the second variable, visibility, two levels were also used: Clear visibility and simulated fog, the latter provided by a perspex screen which was designed to approximate Category II conditions. Category II conditions refer to a runway visual range (horizontal distance measured from the approach end of the runway) of 400 to 800 m. Performance for day landings was measured at touchdown (except for altitude) and again at six ranges beyond the origin of the ILS glide path: 150, 300, 450, 600, 750, and 900 m. For night landings, the 750 and 900 m locations were not used and the data of only two pilots were analyzed. Armstrong does not specify which, if any, flight instruments were available to the pilot.

No significant main effect of FOV on altitude was obtained nor were the interactions of FOV with visibility, FOV with pilots, and the three-way interaction of FOV by visibility by pilots reliable at any range either during the day or at night. Apparently, peripheral information is not essential for effectively landing an airplane, even under conditions where visual information is significantly reduced by poor visibility, night conditions, or both. The visibility main effect was significant at the 300, 450, and 600 m locations at night and during the day; the effect was also significant at 150 m for day landings. However, Armstrong notes that visibility effects obtained for night landings were smaller than differences due to pilots, and suggests that these effects may not be of practical importance.

Probably the most consistent influence on landing performance arose from pilot differences. Reliable effects were obtained at all four night locations and at the five farthest locations during the day. The visibility-by-pilots interaction also proved significant at the 150 m location for the day landings, and at the 450 and 600 m locations at night. The difference in location between day and night landings may be due to the method used to simulate fog. The perspex screen had the effect of reducing the contrast ratio. At night, the runway lights may have been sufficiently bright to negate this diminished contrast. The consequence of this would be a screen that is less effective in simulating fog at night, especially as the airplane approached the runway lights. Although significant differences due to visibility were obtained at night with the altitude measure, no other performance measure reflected a main effect of this variable. Performance on day landings, on the other hand, was consistently affected by variation in visibility when assessed by several measures.

Irish, Grunzke, Gray, and Waters (1977) also tested the FOV and visibility manipulations by having three T-37 instructor pilots fly ground-controlled approaches in a T-37 simulator. Their study included manipulations of simulator motion, g-seat use, turbulence, and wind but these variables are not considered in this review. The usual flight instruments were available as was a Cognitronics voice generator which provided glideslope and centerline deviation information at distances less than 4.5 miles from the runway.

Two levels of FOV were used. In the unrestricted condition, all seven monochromatic channels of the CRT visual system were used, which provided +110 degrees to -40 degrees of vertical FOV and 150 degrees of horizontal. In the restricted condition, which used the FOV found in many currently used visual displays, the FOV was 36 degrees vertical and 48 degrees horizontal. The ceiling/visibility variable also had two levels, one of which was clear visibility and unlimited ceiling. Minimum visibility, the real-world minimums allowed for this maneuver, was .5 mile with a 200-foot ceiling. Altitude was monitored between eight miles and 4.5 miles out on final approach. At 4.5 miles, the aircraft intercepted the glideslope.

Complementing the findings of Armstrong (1970), Irish et al. failed to find an effect of FOV using deviation from criterion altitude as the performance measure. Unlike the Armstrong study, however, the visibility variable also failed to prove reliable. Because of the method used by Armstrong to simulate reduced visibilities this result must be accepted with caution, especially in light of the Irish et al. failure to find an effect. The visibility-by-FOV interaction for individual performance measures was not reported.

FOV and scene complexity were tested by Kraft, Anderson, and Elworth (1980) using a Redifon 747 flight simulator having six degrees of freedom motion and an attached General Electric Compuscene visual system. Unrestricted FOV, with a total of 114 degrees along the horizontal dimension, was available when both the left side and left forward-oblique windows were available. With the side windows unavailable the FOV was reduced to the 40-degree front view. In both cases the vertical FOV was 30 degrees. Two levels of scene complexity were also used. In the simple scene a blue/black runway with no markings was surrounded by a completely homogenous field, with no partitioning edges, representing sandy soil. The complex scene simulated the Moses Lake area of eastern Washington, having diamond-shaped fields, rivers, small lakes, and taxiways.

Sixteen Air Force Military Airlift Command pilots performed straight-in approaches to touch-and-go landings. Navigation aids which provide altitude and glide slope information were not available nor were altimeters and vertical-speed indicators provided. However, an airspeed indicator was present. Performance was analyzed at 12,000, 6000, and 3000 feet out from the glideslope origin, the point where the glideslope intercepts the runway (1840 feet beyond the runway threshold). Pilots were instructed to touch down at the 1000-foot point on the runway although this was not marked.

Consistent with the experiments of Armstrong and Irish et al., no effect of FOV was obtained at any measurement location. However, a main effect of scene complexity occurred at the 6000-foot location. Also, a trial-by-scene-complexity interaction was found at both the 3000 and 6000-foot locations reflecting a monotonic increase in altitude over the four trials for the simple scene contrasted with an increase over the first two trials followed by a drop over the last two trials for the complex scene. There is no obvious explanation for this difference.

A separate analysis, using distance from the glideslope intercept as an additional independent variable, showed a main effect of distance (as expected) as well as the interactions of trials with scene complexity, trials with distance, and scene complexity with distance. The latter was due to performance with the complex scene displaying reduced variability at measurement locations farther out in comparison to the simple scene but Kraft et al. consider the effect to be small.

In all three cases FOV failed to significantly influence altitude when the task involves straight-in approaches. Since the task used in Armstrong, Irish, and Kraft experiments involved the straight-in approach the role of FOV in controlling altitude while landing aircraft from other approaches cannot be completely discounted. That altitude is sensitive to reduction in visual information is shown, however, by Armstrong's main effect of visibility and Kraft et al.'s main effect of scene complexity and the accompanying interactions of scene complexity with such variables as trials. The latter findings suggests that altitude can be used to assess learning effects at least in terms of how they modify performance with other independent variables.

Kraft et al. extended their investigation of visual display variables by manipulating the color of the fields surrounding the runway. The sandy-colored surface of their first two experiments was replaced by blue or red colors, which varied in saturation in order to differentiate the patterns in the complex scene. Fifteen of the 16 pilots used in the first experiment were assigned to three groups according to their chromostereopsis threshold (red advancing, neutral, blue advancing). Performance during the approach was recorded at four locations: 15,190 feet from touchdown (2.5 nm), 12,150 feet (2 nm), 6,076 feet (1 nm), and 3,038 feet (.5 nm).

A main effect of trials was obtained at the two furthest measurement locations while a trials-by-scene-complexity interaction occurred only at the furthest location. The main effect produced a U-shaped curve with altitude on the two middle trials being above the instructed altitude while altitude on the remaining two trials averaged below the instructed altitude. This curve was obtained primarily with the simple scene.

The failure to find an effect of color surround, especially at the closest measurement location, contradicts the hypothesis that altitude will differ because of the interaction of chromostereopsis groups which surround color and scene complexity. It was expected that the blue advancing group would see the blue/black runway as closer than the red surround, especially in the simple scene, while the red advancing group would see the red surround as nearer than the runway. Such an effect should have influenced altitude control.

These results do not completely coincide with those obtained in the Kraft et al. experiment cited earlier. While both experiments did demonstrate an effect of trials and a trials by scene-complexity interaction, these results were obtained at the closer locations in the first experiment (6000 and 3000 feet) and at the farthest locations in the second experiment (12,150 and 15,190 feet). The significance of this is not clear.

A second condition under which reduction of visual information is common is flying at night. Altitude was used in the first of two experiments performed by Kraft (1969) in order to detect variations in performance due to terrain slope, initial altitude, and lighting of the simulated city to which the pilot approached. Twelve Boeing instructors flew a commercial airline simulator toward a scale-model terrain board of a city positioned on a movable table. All approaches were performed under simulated night conditions with one eye covered. Monocular vision was required because of the stereoscopic visual information available in the simulator, but not in the real world owing to the distances involved. Manipulated variables included slope of terrain (flat or three-degree upward slope), initial altitude (16,000 and 10,000 feet), and distribution of city lights (airport only, airport plus distant half of the city, airport and full city). The pilots were instructed to meet specified criteria of altitude and airspeed at two locations, 10 miles out and 4.5 miles at which point the trial ended. Pilots were informed as to whether the simulated city rested on sloping terrain prior to the trial. No altimeter was provided.

Results are in the form of percent of variance accounted for by each variable. Kraft found that initial altitude had no significant effect on altitudes flown later in the trial. The majority of the variance in generated altitude (24.9%) was due to individual differences among pilots, a finding which Kraft found curious in light of the supposedly standardized training received by airline pilots and the emphasis placed on performing all approaches and landings in accordance with an established pattern.

This measure refers to deviations from the assigned altitudes at the 10-mile and 4.5-mile locations. Sixteen percent of the variance is due to city slope. Pilots tended to fly lower with sloping cities in comparison to flat cities (16%), in spite of having foreknowledge concerning terrain slope. Only 4.3% of the variance was credited to light distribution. Contrary to what was expected, additional lights tended to increase deviations from the desired path.

The effect of delay in computer updating of visual displays on performance of a helicopter approach and hovering task was investigated by Ricard, Parrish, Ashworth, and Wells (1981). Normal procedure for boarding a destroyer class ship involves flying to within four to five miles aft of the ship, at which point the pilot initiates a decelerating descent along the glideslope specified by a glideslope indicator. When near the ship, a high stationary hover at approximately the height of the hanger top is held until the aircraft is stabilized and has attained a forward speed comparable to that of the ship. This hover is maintained until the vertical motion of the ship deck diminishes sufficiently to allow boarding of the ship. The boarding phase involves the helicopter proceeding forward to the landing area and descending until at a height of about five to six feet above the deck. Any deck motion must be tracked throughout this low hover and subsequent landing.

For experimental purposes, the full landing was not performed. Instead, the task reached completion at the high hover stage, when the helicopter hovered 50 feet from the ship, lined up with the diagonal deck markings, at an approximate height of 20 feet above the flight deck. Because the simulated scene used a terrain board displaying a model of a DEG 1052 ship equipped with helipad, hangar, and appropriate deck markings, the concern was for possible damage to the optical probe of the TV system if a full landing was attempted. The ship model rested on a g-seat which could move the ship in three axes. In addition, a 15-knot forward ship motion was used in each condition.

Fourteen helicopter pilots, with experience in either SH-2 or SH-3 helicopters were tested in the Langley Visual Motion simulator. The simulator was modified to respond like a general-purpose two-seat transport helicopter similar to a Huey Cobra (AH-1) with a rate control stability augmentation system. Because of the narrow FOV, 45 degrees by 26 degrees in the horizontal and vertical dimensions respectively, a heads-up display (HUD) was employed to provide supplemental lateral, longitudinal, and vertical position information. Other instruments available to the pilot included altimeter, vertical speed indexer, turn and bank indicator, direction, and airspeed. Random turbulence to the helicopter simulator was used in all conditions.

Three variables were manipulated. Delay in the updating of the visual scene was either 66 msec or 128 msec, the latter being a value commonly found in computer-generated imagery. The ship, above which the helicopter was to hover, either was made to move so as to simulate a heaving deck or remained stationary, in order to determine the effect on performance of the high hover where deck motion is not tracked. The authors proposed that a heaving deck could encourage confusion as to whether the ship or the helicopter was shifting. Finally, three conditions of simulator motion were compared. The influence of six degrees of freedom motion from a motion platform was contrasted with g-seat motion and no motion.

Deviation from the desired hover point served as input to univariate analyses of variance for each measure. The variance due to replications and pilots was kept separate from the variance due to the other variables. No effect of replicates on the altitude measure was found, which suggests that the opportunity to practice each new condition prior to data recording was sufficient to prevent additional learning from taking place. Pilot differences, on the other hand, were significant, a finding which tends to consistently occur in all experiments that measure them. Ship motion (heaving deck) failed to reach significance nor did it interact with pilots, or with visual delay. This failure to find a main effect of ship motion contradicts the hypothesis that ship motion should complicate the hover task by fostering confusion as to the source of the motion. However, the lack of a ship motion-by-visual delay interaction was specific to the altitude measure.

Longer visual delay resulted in significantly higher error scores, as expected, but the delay variable also interacted with pilots. This interaction was due to the superior performance shown by two pilots with the long delay, a rather surprising result. The second-order interaction of pilots with ship motion and delay did not, however, prove reliable, indicating again the small role of ship motion in the high hover task.

Altitude was the only performance measure to display a ship-motion-by-simulator-motion interaction. This interaction was due to the failure to find differences in control of altitude as a function of motion except when the g-seat was used, in which case no ship motion resulted in better performance. It may be that confusion between the vertical ship motion and the information for sustained acceleration provided by the g-seat, and not by the motion platform, may be responsible for the difficulty in controlling altitude. The strength of this effect did not vary across pilots as shown by the failure to obtain a pilots-by-ship motion-by-simulator-motion interaction.

Of the remaining interactions, only the four-way interation of pilots, ship motion, simulator motion, and visual delay proved reliable but there is no apparent practical significance for this result. Finally, two additional pilots performed the task under the same conditions but without the HUD. Their performance did not differ from that of the original 14 pilots, signifying that these results are not dependent upon the use of this display.

Two experiments by Hill (Hill & Goebel, 1971/revised by Hill & Eddowes, 1974; Hill & Eddowes, 1974) were directed at identifying flight measures which are sensitive to variations in pilot proficiency. Both experiments used three groups of ten subjects each, selected on the basis of flight experience. Beginner pilots were those having less than ten flight hours while the intermediate subjects had between 25 and 50 hours. The advanced group had more than 100 hours as well as at least 20 hours in the previous six months. Subjects were tested in a GAT-1 simulator. An ILS landing approach to a simulated airport was one of the maneuvers in which altitude performance was assessed in the Hill and Eddowes experiment. One way analysis of variance were performed on each measure. Differences between means of the three groups reached statistical significance and the standard deviations were not reliably different. Correlations with other performance measures showed a significant altitude-elevator correlation, which is not surprising because of the role the elevators can have in controlling altitude during the descent.

For the straight-in approach to landing, 15 variables were tested using the altitude measure. Of these, four variables consistently failed to show an effect: FOV (Armstrong, 1970; Irish et al., 1977), initial altitude (Kraft, 1969), color surround and chromostereoscoping group (Kraft et al., 1980, Exp. 3). Of these, the latter seemed most likely to affect altitude control. Consequently, the lack of an effect for these variables is the most informative in terms of later selection of performance measures. One variable, pilot differences, consistently demonstrated differences in the Armstrong, Kraft et al. and Ricard et al. experiments while visibility produced a difference in one study (Armstrong), but not the other (Irish et al.). Practice effects across trials and scene complexity effects (in interactions) were found in both of the studies by Kraft et al., and the pilot-experience effect was reliable in the Hill and Eddowes study.

Runway Approaches Involving Turns. FOV and visibility were both tested in an experiment where the landing task involved a turn, a factor which might be especially important for the FOV variable. Irish et al. (1977) had their pilots perform a 360-degree turn prior to landing (for details about the pilots and simulator used, see p. 9 of this report). The maneuver began with the pilot flying down initial approach to the runway while maintaining constant airspeed and altitude. When approximately halfway down the runway the pilot was to "pitch out" by reducing power and initiate a steep 180-degree left turn, which was closely followed by a second 180-degree turn which brought the aircraft into position for completion of the maneuver by touching down on the runway. Airspeed, altitude, location of when to lower speedbrakes, flaps, and landing gear were all specified. Altitude was measured at the point where the pilot pitched out and again on downward. Neither location showed an effect of FOV while both locations showed a significant effect of visibility, with better performance occurring in the clear-visibility condition. The lack of a FOV is consistent with results obtained thus far. Both locations at which altitude performance was recorded occurred when the aircraft was flying straight ahead, although reduction of power during pitch out would require coordinated changes in elevator back pressures so as to avoid change in altitude. No differences in elevator power during this segment arose, suggesting that FOV restrictions are not a problem for this maneuver.

A two-mile descending 90-degree turn onto final approach was used by Kraft et al. (1980) as a further test of the variables of FOV and scene complexity (for additional details, see p. 10). Pilots were provided heading information at all times as well as initial altitude, location, and the ideal altitude at the end of the turn following a 2.5-degree glideslope. Line-up with the runway centerline was the final criterion. Performance was recorded at three points: 1.0 nm from the start (1.45 nm to the left of centerline and 6.4 nm out); 5.14 nm out from the touchdown point, which coincides with the point on the ideal 90-degree turn when the runway first comes into view in the non-sidewindow condition; and at the end of the instructed turn, 4.4 nm out from touchdown.

At the first measurement location, a significant main effect of trials was obtained because of the increase in altitudes flown which occurred over the first three trials followed by a leveling off on the final trial. The FOV by scene complexity interaction was significant at the final two locations, although a trend was apparent at the first location. This interaction was due to both the simple-scene, restricted-FOV and complex-scene, unrestricted-FOV trials being flown near the desired 2.5-degree glideslope, whereas the other two conditions were low. Restricting either the FOV or reducing scene complexity might have the consequence of encouraging the pilot to fly at more conservatively higher altitudes, although, as Kraft et al. note, this fails to explain why pilots in the unrestricted condition for both variables failed to fly lowest.

Collyer, Ricard, Anderson, Westra, and Perry (1980) tested the effect of limiting FOV on accuracy and efficiency in learning to land on a simulated aircraft carrier. Two approaches to the carrier were compared. The first required a circulating approach downwind of the carrier prior to rolling out on final approach while the second was simply a straight-in approach to the carrier. Two FOV conditions were included, the unrestricted having 300-degree horizontal and 150-degree vertical limits while the narrow dimensions were 48 degrees horizontal and 36 degrees vertical. Twenty-one T-38 instructors were tested in an A-10 simulator with the motion base and g-seat capabilities deactivated. Instruments available included an angle-of-attack indexer, altimeter, airspeed indicator, and Fresnel Lens, the latter providing information about the desired 3.5-degree glideslope.

The experiment, because it was designed as a quasi transfer study, involved two phases, both of which were conducted in the same simulator. Training took place under one of three conditions: wide FOV, circling approach; narrow FOV, circling approach; and narrow FOV, straight approach. The test for all three conditions consisted of a circling approach under wide FOV, the latter condition more closely simulating that found in the operational setting. Fifteen trials per phase were performed. Altitude variations between groups were assessed at five locations on the circle and at quarter-mile points on final approach. No differences in altitude flown were found in training scores for the two groups which performed the circling approach nor did the groups differ in control of altitudes during the test phase.

This failure to find an effect of FOV during the training phase is surprising in light of the expectation that maneuvers involving a circle would be sensitive to variations in this variable. However, because of the availability of supplemental information, in the form of altimetors and other instruments, in this experiment pilots could have made up for FOV restrictions. Give the finding of Kraft et al. that FOV in conjunction with scene complexity does influence control of altitude during turn approaches, the hypothesis that FOV is important when supplemental information is not available remains unchallenged. Because of the locations at which performance was measured, for the straight-in approach, Irish et al. did not provide a clear test of this variable.

As was the case with the experiments using the straight-in appraoch, trials did prove to have an effect, one which supports a learning trend in terms of control of Hidvae. In support of Armstrong, but contrary to the ground-controlled appraoch study of Irish et al., the latter authors did find an effect of visibility. Unfortunately, there are no clear differences between those studies which did find an effect and the study which did not.

Non-landing Maneuvers. The two experiments of Hill (see p. 15 for a discussion of methodology), which attempted to identify performance measures sensitive to pilot differences, tested the altitude measure in a variety of maneuvers other than the landing task. Only those maneuvers in which the altitude measure should not change during the course of the maneuver were evaluated in the first experiment. For example, altitude was not used during a descending turn.

The first task used by Hill and Goebel (1971/1974) involved maintaining a constant altitude and heading while being subjected to simulated rough air affecting the pitch and roll axes (pitch and roll tracking). No differences due to pilot experience were apparent in the means and standard deviations of the altitude scores, nor were the correlations between altitude and any of the other performance measures recorded (airspeed, climb, roll, pitch, and heading) significant.

Pilots were required to make power changes while still attempting to maintain constant altitude and heading in rough air in Hill and Goebel's second task. Power changes are frequently accompanied by altitude changes in airplanes of the type simulated and, consequently, the altitude measure might be expected to reflect differences in control of the vertical dimension as a function of pilot experience. In spite of this, however, none of the altitude variables reached significance.

Five maneuvers, performed sequentially, comprised the third task. From level flight, the pilot was to climb 1000 feet and level off, after which a 360-degree level right standard turn was to be performed while maintaining constant altitude and airspeed. Slow flight occurred next, with a 30 mph drop in airspeed required while holding altitude and heading constant. This was followed by a descending left standard 360-degree turn. The task was completed by a straight 2500-foot descent to the ground, maintaining a constant heading. Each segment of the task was to be completed as closely to the criterion time limit as possible, but performance was recorded only for the first 75% of the maneuver so as to avoid bias due to maneuvers finished too quickly. Presumably this ensures that the same quantity of data is available for each subject. Simulated rough air again occurred throughout. Means, standard deviations, and correlations with other performance measures were assessed only during the level right-hand turn and slow flight. Slow flight alone resulted in significant difference between means of the groups differing in experience. The slow flight maneuver requires the pilot to increase the pitch of the aircraft while producing coordinated changes in airspeed so as to avoid changes in altitude. The finding of pilot differences as reflected by control of altitude is thus a reasonable finding.

Hill and Eddowes (1974) expanded this approach to include six additional maneuvers as well as replicating those used in the first experiment. The later study also differed from that of Hill and Goebel because of the recording of performance on measures which change during the course of the maneuver. Results with the first maneuver, roll and pitch tracking, were very different from those obtained in the first experiment so far as altitude is concerned. Standard deviations between the groups were significantly with rudder (negative) correlation, aileron, northing from a reference station deviation (presumably a course deviation measure), and pitch. It should be noted that of the altitude correlated variables, only pitch had been included in the first experiment.

Similarly, in the second maneuver, roll and pitch tracking with power changes, standard deviations were reliably different and the altitude-rudder correlation was significant and negative. In the first experiment, altitude had provided not significant effects. The third maneuver, consisting of the five segments described previously, showed no effects on the 1000-foot climb, the level right 360-degree turn, and descent to the ground phase. For slow flight, which had previously shown an effect of pilot experience for the means, standard deviations were reliably different this time. In addition, four reliable correlations were obtained with power (negative correlation), aileron (negative correlation), pitch, and airspeed (negative correlation). The latter two alone were tested in the first experiment but the correlations were not significant. Means for the descending 360-degree left turn were also significantly different.

Why these differences between the two experiments appeared is not clear, as the methodologies used in both cases were the same. Although it is possible to devise post hoc explanations for each of these results, there is little in the way of predicting reults ahead of time that can be done. For example, the slow flight maneuver involves coordinating pitch with power changes in order to maintain the desired altitude. Consequently, it is not surprising to find correlations of altitude with power, pitch, and airspeed. Unfortunately, the correlations with pitch and airspeed appeared only in the second of Hill's two experiments (the power measure was not included in the first experiment). There is no obvious reason for the different findings. In addition, altitude also corrolated with aileron. The relationship between these two measures for the slow flight maneuver is not intuitively obvious, either.

The remaining six maneuvers were employed only in the Hill and Eddowes experiment. Roll tracking served as the fifth task and involved the same type of tracking as in the first task except that only the roll axis changed, thus producing a one-dimensional compensatory tracking task. Here, the standard deviation again were significant as was the altitude-roll correlation. The sixth task included roll, pitch, and yaw tracking, where all three axes had to be tracked. In addition to significant standard deviation differences, altitude correlated significantly and negatively with aileron.

Tasks seven and eight required roll tracking again, but in the former a reduced bandwidth command signal was used to drive the simulator while in the latter the amplitudes of the signal were reduced, which sould make tracking easier. In the seventh task, standard deviations were again significant as were the correlations of altitude with turn rate (negative correlation) and pitch. Neither the standard deviations nor means were significant in the eigth task but the three correlations of altitude with aileron, roll, and pitchdid prove reliable.

The last tasks involve subjects using information obtained from a ground reference system which plotted x and y coordinate information. Task nine required pilots to fly a half-standard turn (360 degrees rotated in four minutes) while in rough air. Two new variables, heading deviations from criterion and radius of the aircraft to the center of the circle. Only the altitude-airspeed correlation was reliable.

The final task consisted of altitude and position tracking. While maintaining the specified altitude and airspeed, the pilot had to sustain a constant heading. Position and altitude were varied in order to produce a two-dimensional tracking task. this task was considered to be more difficult than the other tracking tasks, with the exception of the sixth task, and significant standard deviations and four correlations were obtained. Altitude was found to correlate with power (negative correlation), elevator (negative correlation), airspeed, and climb rate.

The slow flight maneuver was used again in the Irish et al. experiment (see p. 9 for further details) where FOV was evaluated. A different design from that developed for the two landing tasks was used. A third level of FOV, that of no visual scene at all to simulate instrument flight, was added. Also, the visibility variable was not tested. No effect of POV was obtained with the altitude measure.

Summary. The most striking and consistent result of these studies is the failure to find an effect of FOV. This variable was tested in airplanes and in simulators, in conjunction with variables which further reduced available visual information (visibility and scene complexity), during straight-in approaches and approaches requiring a turn onto the runway or carrier, and also during slow flight. Only in one case, the FOV-by-scene-complexity interaction of Kraft et al.'s first experiment was there a FOV effect. Two factors seem to be involved: (1) Whether the maneuver requires a turn or other deviation from straight ahead. If a turn is required and if altitude is measured during the turn, FOV is more likely to have an effect. (2) The type of information available to the pilot in the performance of the turn. Collyer et al. (1980) failed to find an effect of FOV for the circling appraoch but their pilots had access to aircraft instruments. Kraft et al.'s FOV-by-scene-complexity interaction suggests that the type of information available in the visual scene is a major determinant of FOV effects. The implication is that simply restricting the FOV is not sufficient to degrade performance, even if a turn is required. There must also be a reduction in critical information outside the cockpit which is not replaced by cockpit instruments.

Scene complexity, as a main effect, was found only in the second experiment of Kraft et al. (1980) and only at one location. The variable seems to make its biggest contribution through interactions with other variables, especially FOV in the first experiment, trials in the second experiment, and trials (only at one location) again in the third experiment.

Altitude as a performance measure proved sensitive to most of the visual scene variables, including visual delay and ship motion (Ricard et al., 1981), terrain slope and city lights (Kraft, 1969). Notable exceptions were color surround and the related pilot variable of chromostereoscopic group, a finding which contradicted Kraft et al.'s hypothesis concerning the interaction of these variables. Visibility produced reliable effects in Armstrong's (1970) experiment using the straight-in approach and Irish et al.'s (1977) overhead pattern and landing, but not in the latter's straight-in approach task. The obvious difference between the two straight-in approach experiments concerns the locations at which performance was measured. Since Irish et al. did not measure altitude at locations closer than 4.5 miles while Armstrong's farthest location was 900 m, it may be that visibility effects are somewhat location specific. That is, at the closer locations pilots may be encouraged to rely more on information outside of the cockpit even though cockpit instruments may provide more accurate information. However, because Armstrong does not specify the instruments available to his pilots no firm conclusions can be drawn. This conclusion is also problematic because of Irish et al.'s visibility effects with the overhead pattern maneuver. At both locations where altitude was recorded the aircraft's altitude was well above the simulated ceiling, which would force the pilot to rely on instruments.

The three pilot variables of pilot differences, trials, and pilot experience each provided reliable altitude control differences in most of the experiments which used them. Learning or trial effects were obtained in each of the Kraft et al. experiments, although the experiments did not agree in terms of where in the maneuver the locus of learning effects would be found. The second Kraft et al. experiment, for example, found the trial effect at the closer locations while all of the trial main effects and interactions were found at the farthest locations in their third experiment.

The experiments of Armstrong, Kraft (1969 and 1980), and Richard et al. all provided consistent pilot differences in several different maneuvers and in a variety of aircraft. In fact, pilot differences are probably the strongest and most consistent effects found in any of these experiments. Less consistent, however, wer the effects due to pilot experience found in the two Hill experiments. In the four maneuvers which measured altitude in both experiments (counting task number three as two separate maneuvers), agreement was found on only one of them, the level turn where no significant results were obtained.

Most common, in Hill's 1974 experiment, were differences in the standard deviations of the groups, which were found in nearly every maneuver. Three maneuvers or segments of maneuvers failed to show any effects of the altitude measure: straight-ahead climb, level right, turn, and straight-ahead descent. It would appear that altitude effects are most likely to occur when the maneuver requires any kind of tracking, as in the simulated rough air cases. Significantly different standard deviations were always obtained for these maneuvers and also for slow flight where the pilot must compensate for deviations from the desired levels induced by changes in handling qualities at lower airspeeds. A significant difference between means was obtained only for the descending left turn maneuver. In comparison, Hill and Goebel found either no effect (in the tracking tasks and level turn) or the effect produced differences in means (slow flight).

In terms of the correlations with other measures no clear pattern is obvious. Correlations are found with all of the tracking tasks, but none of the measures is consistently correlated with altitude in all of the tasks nor does a measure correlate consistently when the same axis must be tracked in different maneuvers. Airspeed correlated with altitude in both of the ground-referenced maneuvers, but beyond that little can be said.

The final category of experimental variables, having to do with aircraft position relative to the visual scene, were for the most part significant. Distance from the runway in Kraft et al.'s (1980) second experiment, as a main effect and in interaction with other variables, produced reliable differences in altitude as did measurement location in Kraft's (1969) night approaches. The one variable that did not have an effect was initial altitude in the latter experiment.

Glideslope Deviation

The glideslope deviation measure assesses departures from a theoretically ideal glideslope, often that of the instrument landing system (ILS). As noted earlier, it resembles the altitude measure in that both record vertical position, but altitude uses the ground as its zero point while a theoretical glideslope serves as the reference for the glideslope measure. Both measures should provide a similar pattern of results and they are often used to investigate the same issues.

The terms, glideslope and glidepath, are normally used more or less interchangeably (e.g., Gentle & Reithmaier, 1980). For the purposes of this paper, however, "glideslope" will be used to denote the instructed approach; for example, the approach specified by the ILS in instrument landings and the FLOLS in carrier landings. "Glidepath" will refer to the actual approach made by the aircraft.

Straight-in Approach. Irish et al. (1977) recorded glideslope deviation between two locations on a ground-controlled approach, 4.5 miles and .2 miles out, in order to assess variation in performance due to FOV and visibility. (See p. 9 for methodological details.) Neither variable produced a significant main effect on performance, which may be due to the glideslope information provided by the cognitronics voice generator. These results coincide with those found for the altitude measure, although the latter were obtained earlier in the approach where supplemental glideslope information was not provided.

FOV again failed to reach significance at any of the three measurement locations in the Kraft et al. (1980, Exp. 2) experiment where deviation from a 2.5-degree electronic glideslope was assessed (for more details, see p. 10). Because a turn was not required in this maneuver no FOV effect would be expected. Scene complexity did prove reliable at three locations (not touchdown) and when distance from touchdown was used as an independent variable. Although scene complexity effects were found with the altitude measure, they usually were in the form of interactions with other variables. Here the effect was more straightforward, with larger deviations occurring for the simple scene. In the distance analysis, the main effect of distance was significant as was the scene complexity by distance interaction. This interaction was due to a decrease in glideslope deviation at closer distances which was greater with the simple scene, suggesting that pilots apparently were able to partially compensate for information not presented in the simple scene as the aircraft approached the runway. Generally steeper slopes were obtained with the simple scene. A similar interaction was found with the altitude measure. Also, the simple main effect of scene complexity at the 6,000-foot location occurred with both measures. Finally, vertical deviation from the glideslope of 2.7 degrees, which subjects were instructed to use, failed to provide significant differences at the 1000-foot target touchdown point.

Glideslope deviation was also used by Kraft et al. to assess the effects of color surround, chromostereoscopic groups, and scene complexity (discussed earlier on p. 12). At the three furthest measurement locations, the main effect of trials was significant. Also, the trials by scene complexity interaction proved reliable at the 2.5 NM location, similar results having been found with altitude as well. On the first and fourth trials pilots overshot the glideslope while on the remaining trials the glideslope was undershot. A similar pattern appeared for the simple scene whereas the complex scene showed an increase over the first two trials followed by a drop, with only the final trial displaying deviation below the glideslope. Although no effect of chromostereoscopic groups appeared in the analysis of variance, a separate t-test which compared the point at which actual descent path crossed the electronic glidepath showed that the blue advancing group tended to fly higher during the early part of the approach, thus crossing the electronic glideslope nearer the runway, especially when the runway surround was red. The red advancing group, on the other hand, deviated less from the glideslope and flew lower, crossing the glideslope earlier, when the runway surround was blue.

With the altitude measure, two points should be made. In both of Kraft et al.'s experiments which used the straight-in approach, an effect of trials was obtained. Glideslope deviation, however, did not display trial effects in the first experiment. Also, both experiments failed to provide significant effects, in the analysis of variance, of color surround and chromostereoscopic groups, contrary to what the authors anticipated.

Three experiments used glideslope deviation to evaluate landing performance of pilots at night. Kraft's (1969) second experiment (see p. 14 for discussions of the first experiment and the methodology used) varied the slope of the scale-model city while always placing the runway in a level position. A second variation was starting distance, either 20 or 34 miles out, both having the same starting altitude. Consequently the same glideslope cannot be used in both situations. Pilots were asked to attain specified airspeeds and altitudes at two locations, deviations from which provided the data for analysis. Airspeed and rate of climb indicators were provided.

Under these conditions, no effect of city slope on glidepath was obtained for the 20-mile starting distance condition. An analysis of variance for all of the data showed significant effects of city slope, distance out (the two measurement locations), pilots, and significant interactions of slope with starting distance, slope with pilots, and starting distance with pilots. The main effect of starting distance did not prove reliable. Altitude approaching the flat city, on the average, was higher in comparison to the sloping city.

A direct comparison of day versus night landings, using glideslope deviation, is provided by Brictson (1967) in a study of carrier landings. Carrier landings typically require supplemental glideslope deviation information and the display which was devised for this purpose is the Fresnel Lens Optical Landing System (FLOLS). The display consists of a "meatball" which represents the position of the aircraft relative to two horizontal bars. When the aircraft is on the glideslope the meatball remains level with the bars. Deviation from the glideslope is specified by displacement of the meatball relative to the bars.

The glideslope is usually 3.5 degrees and, if followed, will result in the aircraft tail hook touching the landing deck between the second and third (of four) arrestment wires, with the third wire actually stopping the aircraft. An approach below the glideslope will result in either the first or second wires stopping the aircraft, or a ramp strike where the aircraft collides with the carrier's stern. An approach above the glideslope can mean arrestment by the fourth wire, or the airacraft may bolt, that is, touch down beyond the wires necessitating a missed approach.

Aircraft speed, in Brictson's experiment, was "provided by an angle-of-attack indexer (AOA) located in the lower left portion of the windscreen" (p. 1220). Twenty-two fleet-experienced Navy pilots served as subjects and flew F4 Phantom jets. Glideslope deviation was recorded at .5 miles, .25 miles, .125 miles, and touchdown.

Brictson found that night landings were performed with significantly greater variability at .25 and .125 miles from touchdown, suggesting important difficulties in the control of altitude. At night, 23% of attempted landings were low (38% of these were bolters) while only 4% were low during the day. These values were obtained at the ramp location but roughly the same figures occurred at the other locations. In general, flights during the day showed a consistent tendency to fly above the glideslope, while approximately one-fourth of night approaches were low at each of the ranges. This is in spite of the availability of the Fresnel lens which provides reliable glideslope information.

As was the case with altitude, glideslope appears to be a reasonably useful measure for investigating the effects of reduced visual conditions which occur at night. Pilots tend to fly lower at night even when approaching a sloping surface where their altitude becomes dangerously low. In an extension of the Brictson study, Brictson, Ciarvilli, and Wulfeck (1969) found that pilots continued to fly below the safe glideslope during bad weather conditions which caused the deck of the carrier to pitch.

The FLOLS provides only zero-order (displacement) information. The consequence is a lag in the system before the effects of higher-order information, such as rate, over which the pilot has more direct control, is displayed. This may explain why Brictson's pilots appeared to ignore FLOLS information. Because of limitations in the current system, Kaul, Collyer, and Lintern (1980) evaluated two displays which do provide rate information. A first-order display was constructed by adding vertical bars or arrows to the inside ends of the bars currently found in the FLOLS. In the "rate" display, the direction of the arrows was determined by the direction of meatball movement, with the length of the arrows specifying rate of deviation from or toward the desired glideslope. If the meatball was above the horizontal bars and the arrows pointed upward this meant the aircraft was high and going still higher relative to the electronic glideslope. The arrows in the "command" display represented differences between actual and desired descent rates, meaning that the vertical bars would not appear if the aircraft was either on or returning to the glideslope. Descent rates which were too low produced arrows directed upward while too-high descent rates were indicated by downward pointing arrows. Consequently, both displays provided information about current status and also trends. Both displays were compared with the conventional FLOLS.

Eight carrier-qualified Navy pilots flew the Visual Technology Research Simulator (VTRS) which has a T-2C Navy jet trainer cockpit. No motion was used. The FOV was 160 degrees horizontal and 80 degrees vertical. An image of the Forrestal (CVA 59) with carrier wake, and the FLOLS, were computer generated for both day and night landings. The scene also included a horizon during day landings and no other features were present. At night only the lights on the deck could be seen.

The trial began at 9000 feet from the ramp, the simulated aircraft lined up with the centerline, on the glideslope, and appropriately configured. A crosswind was always used to force pilots to initiate control adjustments. Independent variables included turbulence, time of day (day or night), and display type. Root mean square (rms) error deviation from glideslope was recorded during four segments of the task: 6000-4500 from the ramp, 4500-3000, 3000-1500, and 1500-0.

During all four segments, the main effect of display type was significant, with superior performance occurring for the command display and poorest performance appearing for the conventional display. The pairwise comparison between these two displays was the only significant result and accounted for approximately ten percent of the variance in each segment. The command display was reliably better than the rate display in the two segments between 3000 and 5000 feet, with eta squares of six and eight percent, respectively. The rate display was statistically superior to the conventional display only in the farthest segment.

Means and standard deviations were determined at four locations: 4500, 3000, 2000, and 1000 feet from the ramp. Results from the means show that pilots flew above the glideslope using the rate display, and still higher with the conventional display, while approaches using the command display were slightly below the glideslope. Differences between conventional and command displays were significant at all locations whereas differences between conventional and rate displays were reliable at the 3000, 2000, and 1000-foot locations.

Glideslope tracking was significantly less variable, based upon standard deviations, for the command display at all four locations in comparison to the other displays. Rate performance was reliably less variable than the conventional display only at the farthest location, 4500 feet.

The main effect of time of day, for rms error, was reliable only at the 4500 to 6000 foot segment with smaller errors occurring during the day, but this accounted for only three percent of the variance. Only two interactions were significant: display type by time of day and display type by turbulence. However, both interactions individually accounted for less than one percent of the variance.

Two results of this study are especially noteworthy. First, although Kaul et al. did find an effect of time of day, thus confirming the Brictson experiments, this effect was not nearly as strong and consistent. However, the display type interaction with time of day suggests that time of day is a determinant of performance and the poorer performance normally found with night landings can be improved through the judicious selection of a landing display.

The second point of interest concerns the consistent effects of display type on performance. Clearly, some measure reflecting pilot control of the vertical dimension is required for experiments attempting to relate landing performance with variations in the type of information provided by displays. That this conclusion holds even in the case where both the task and display variables emphasize the lateral dimension will be shown by the results of Jensen (1979), discussed later in this paper.

The two experiments of Hill (Hill & Eddowes, 1974; Hill & Goebel, 1971) discussed earlier (see p. 15) both tested glideslope deviation in order to assess its reliability in discriminating skill in performing an ILS landing approach. Pilot experience was the sole variable of interest and in neither experiment was there a significant effect.

Deviation from glideslope provided results which generally mimic those obtained with altitude. No effect of FOV appeared in the two experiments which tested this variable (Irish et al., 1977; Kraft et al., 1980, Exp. 2), nor did Irish et al. find an effect of visibility. Given the conditions of the experiments these results are not surprising. Glideslope control was influenced by scene complexity, however. Maintaining the desired glideslope was more difficult with the simple scene, although by touchdown pilots were able to compensate, as shown by the lack of an effect of scene complexity at this location (Kraft et al., 1980, Exp. 2). And once again, color surround and chromostereoscopic group each failed to produce differences in performance, as was the case with altitude.

For night landings, Kraft (1969) demonstrated an effect of the visual variable, city slope, as well as measurement location, and interactions between these variables occurred as well. In comparison to day landings, Brictson (1967; 1969) showed that night landings tend to deviate below the glideslope and are significantly more variable. This effect of time of day was confirmed by Kaul et al. (1980) but was not nearly as strong.

Control of glidepath was found to be strongly influenced by the type of display used in performing carrier landings, in terms of rms error, means, and variability (Kaul et al., 1980). Consequently, for stright-in approaches, glideslope deviation appears to be a useful measure for studies investigating landing displays, especially those for carrier landings.

For the pilot variables, glideslope deviation was insensitive to pilot experience effects in both the Hill studies. This result contrasts with the Hill and Eddowes (1974) experiment which did find significant differences between means, and the reason for this conflicting result is not clear. Trial effects were obtained with both altitude and glideslope deviation in the third Kraft et al. experiment but no trial effect was found in the second experiment with the latter measure. Finally, pilot differences, assessed only in Kraft's (1969) study were obtained as were interactions of this variable with most of the other variables tested.

Approaches to the Runway from a Turn. Several experiments used glideslope deviation to evaluate performance in an approach and landing following a turn. Irish et al. (1977) had their pilots fly a 360-degree overhead pattern (previously described on p. 16) which was completed by landing on the runway. The variables of interest were FOV and visibility but neither produced significant results when glideslope was assessed beginning at approximately 2,000 feet from the runway. This failure to obtain an effect confirms the results Irish et al. obtained for the glideslope measure when the task was a ground-controlled approach. However, when altitude served as the performance measure on the same task a significant effect of visibility ws obtained. This difference is probably due to altitude having been measured only at pitchout and on the downwind leg, whereas glideslope deviation was measured on final approach. The aircraft was above the visibility ceiling at both altitude measurement locations but came below the ceiling at some point before or during final approach.

Collyer et al. (1980) varied FOV and type of approach made during the training phase of their transfer study in order to determine whether either variable affected accuracy in landing on a simulated aircraft carrier under the wide FOV condition following a circling approach (see p. 14 for details). Based upon glideslope deviation the only significant effect was that the narrow FOV, circling group displayed more variability in comparison to either the narrow FOV, straight-in group or the wide FOV, circling group.

Because the task involved a circle, FOV effects would be expected to have the greatest impact on lateral control. The greater variability in glideslope deviation displayed by the narrow FOV, circling group probably reflects the effect of increased difficulty in controlling the aircraft's lateral position. In attempting to cope with this dimension, pilots may have permitted control of the vertical dimension to deteriorate slightly. That the effect is not large is shown by the lack of significant differences in altitude found both during the circle and on final approach. No differences appeared during the test phase.

The accuracy of curved landing approaches made by pilots with displays differing in the type of information provided was compared by Jensen (1979). His task consisted of a 120-degree descending left turn, having a radius of 1.5 NM, followed by rollout onto a .5-mile final approach to the runway. The electronic glideslope was three degrees.

The basic display, a forward-looking contact analog, for the zero-order condition, consisted of a moving horizon, a runway outline with centerline and landing aimpoint, guidance poles representing the desired flight path, and a fixed airplane symbol. A predictor condition was devised by adding a moving airplane symbol, providing future position of the aircraft. This symbol was driven by either a first-, second-, or third-order computation algorithm. With this display, the goal is to place the predictor symbol (moving airplane) at the desired future position relative to the guidance poles.

For the pure quickened condition, the contact analog scene with guidance poles advanced, in accordance with the appropriate computational algorithm (first-, second-, or third-order), while the two airplane symbols remained stationary. This made the task one of compensatory tracking as opposed to the pursuit tracking required by the predictor display. In the control condition, a conventional ILS display having a moving horizon, fixed-airplane symbol, and scales presenting lateral and vertical deviation from the glideslope was used.

The predictor and quickening displays could be combined by having both the background and moving predictor airplane symbol advance toward each other, the proportion of distance covered by each subject to experimental manipulation. For example, the predictor symbol might move 33 percent of the distance and the quickened background would travel the remaining 67 percent.

An additional manipulation was that of frequency separation, where the airplane predictor symbol shifts immediately in the direction specified by the pilot's control input and the background represents the airplane's position and altitude, either actual or quickened. Both the predictor symbol and the background can be driven by first-, second-, or third-order algorithms.

Twelve of the 17 display configurations were obtained by crossing the three levels of algorithm (first-, second-, third-order) with four combinations of prediction and quickening (zero, 33, 67, 100 percent prediction). One display presented third-order, 100-percent prediction but the predictor symbol did not rotate in response to roll direction and rate. Three displays incorporated frequency-separated dynamics. Two were driven by third-order predictor algorithms, with one assigning two terms to the predictor symbol, the other all three. The third frequency-separated display used the second-order algorithm with one term applied to the predictor symbol. Finally, the conventional ILS display served as the seventeenth display.

Eighteen instrument-rated pilots performed four approaches with each display in a Link GAT-2 simulator having washout pitch and roll motion. Each approach was made under one of four wind-shear conditions. All displays were presented on a 13 by 18 cm CRT monitor. The flight task was divided, for measurement purposes, into three segments: (1) curved-path steering, which ended at 4115 m from the runway; (2) wind-shear recovery, which began at the point where the wind shear took place, 4115 meters out, and ended 1231 m from the runway; and, (3) runway delivery, begun at 1231 m and ending at the runway. Performance was recorded at 5000, 3000, and 500 m from the runway.

Reliable deviations from the glideslope, in the form of rms errors, due to display type were obtained at all three measurement locations but significant pairwise comparisons between displays were found only at the curved-path location. Glideslope deviation as a function of percent of prediction was smallest at intermediate levels, indicating that both prediction and quickening are best for vertical control. Jensen suggests that quickening aids in discriminating the orientation of the guidance poles relative to each other, thus providing useful information concerning the future state of the aircraft.

No effect of computation order was found at any location which may be due to having used too long a prediction span (eight seconds). The span probably should have been much shorter for this dimension. Number of prediction terms in the frequency-separation displays produced one reliable effect: the third-order algorithm in combination with one prediction term was reliably superior to the other two displays at the curved-path location. The failure to find stronger effects due to frequency-separation variables is not surprising since the concept of frequency separation was devised in order to cope with control reversals, a problem limited primarily to lateral flight control.

Display type interacted with wind shear at all three locations, with the predictor display being least affected. Finally, vertical error correlated slightly and negatively with the lateral measure, especially at runway delivery and slightly but negatively with airspeed at the curved-path location.

Jensen's results confirm the conclusion drawn earlier concerning display studies using the straight-in approach. Glideslope deviation does provide information of use to the designer of landing displays, especially in light of the results found with percent of prediction. Vertical control is superior with a display incorporating some quickening, which is not the case with lateral control. Consequently, some measure reflecting vertical performance is needed in any comparison of landing displays.

Glideslope has been used in several experiments which compared performance under monocular and binocular conditions. (These experiments are described in greater detail in the section of this paper on longitudinal deviation from touchdown since this was the central measure used in these experiments.) Lewis and Krier (1969) had pilots perform touch-and-go landings in a jet trainer with one eye covered and again with both eyes. One pilot received additional practice on the task in order to assess learning effects. Steeper approaches were obtained under the monocular condition even for the pilot who received additional practice, and there was little evidence of a learning effect.

Lewis, Blakely, Swaroop, Masters, and McMurty (1973) tested general aviation pilots in a Piper Cherokee airplane in order to determine the extent to which these results generalize. Unlike the first experiment, pilots did not fly steeper monocular approaches and no significant effect was found. However, Grosslight, Fletcher, Masterson, and Hagen (1978) replicated the Lewis et al. (1973) experiment with a few modifications which compensated for such biases in the original experiment as greater opportunity for practice with monocular landings prior to the experiment. General aviation pilots performed landings in a Beech Sport aircraft and, confirming Lewis and Krier, monocular landings were flown significantly higher and on a steeper glideslope. Thus it would appear the general finding of these experiments is that monocular approaches tend to be steeper.

Summary. In general, the results obtained with glideslope deviation closely approximate the findings of experiments which used altitude. FOV effects were again found only when a turn was required (Collyer et al., 1980). No visibility effect was obtained (Irish et al., 1977) for either straight-in approaches or the overhead pattern and landing, even though supplemental glideslope information was only provided for the straight-in approach. Reliable scene complexity effects were found in the second of Kraft et al.'s (1980) experiments and the magnitude of the effect was shown to depend upon distance from the runway. As was the case with altitude, Kraft et al. failed to find differences due to color surround and chromo-stereoscopic group.

Once again, most of the variables tested by Kraft (1969) affected night landing performance. These variables included variations in visual information, measurement location, and pilot differences. However, Hill failed to find an effect on glideslope control due to pilot experience, whereas altitude variations were found in the one Hill experiment which assessed them (Hill & Eddowes, 1974). Night carrier landings were found to deviate consistently below the glideslope in comparison to day landings and also were more variable, in spite of the presence of glideslope information provided by the FLOLS (Brictson, 1967; Brictson et al., 1969; Kaul et al., 1980).

It would appear that use of only one of the measures would be adequate unless there was reason to believe that either of two special cases might occur. In the first case, differences in mean altitude could occur but the experimental conditions might line up at approximately equal deviations from an ideal or instructed glidepath. In this case, the altitude measure would demonstrate significant effects while the glideslope deviation measure would not. The second case involves experimental conditions which do not differ significantly when compared with the glidepath. Thus, the altitude measure would provide no significant differences while glideslope deviation does.

Variables not assessed with the altitude measure included display type and monocular versus binocular landings. Regardless of whether the approach required a turn, display type did influence glideslope control. Two studies, Lewis and Krier (1969) and Grosslight et al. (1978), found that approaches performed under monocular vision tended to be steeper than binocular landings. The effect of practice on this finding is not clear.

The results of Collyer et al.'s experiment raise an issue which is probably relevant to all of the studies discussed in this paper. They concluded that the greater glideslope deviations displayed by the narrow FOV, circling group arose because their pilots were forced to expend effort on controlling the aircraft's lateral position. As a consequence of this, less effort was devoted to managing the aircraft's glidepath and larger deviations from the glideslope arose. It would seem that other experiments might suffer from this problem. The net effect of this is that any performance differences could be explained in two ways: as a direct result of the variable of interest or the indirect result of the demands imposed on the pilot in having to devote greater attention to control of a different aircraft dimension.

Vertical Velocity

This performance measure, which records speed of change in the vertical axis, appears under a variety of names including descent rate, sink rate, and climb rate. All of these labels designate the same measure, differing only perhaps in the direction of change. Sink rate typically is used primarily in reference to vertical downward velocity near the runway. Nonetheless, the terms can, in most cases, be used interchangeably.

The difficulties that tend to occur when attempting to judge altitude under reduced visual conditions have already been mentioned. A related difficulty concerns the control of descent rate. Accompanying the failure to maintain a safe altitude is the inability to detect high descent rates, as Kraft (1969) pointed out for night landings. Similarly, the problem of abnormally fast descent rates at touchdown in flight simulators compared with aircraft motivated experiments which attempt to isolate the cause of this problem (e.g., Armstrong, 1970). Consequently, descent rate tends to be used in experiments dealing with the same issues as the altitude measure.

Straight-in Approach. Armstrong (1970) used descent rate to assess the effects of reduced FOV and visibility (see p. 8 for details concerning methodology). For day landings, two main effects and two interactions each showed a significant effect at one location. Armstrong dismissed the significant main effect of FOV at 150 m, the visibility by pilot interaction at touchdown, and the three-way interaction of FOV by visibility by pilots also at touchdown as probably due to chance. The second main effect, visibility, was highly significant at the 150-m location. Both the FOV by visibility and FOV by pilot interactions was significant at all locations except 300 and 450 m, a pattern which complements the pilot differences found with the altitude measure.

For night landings, the main effect of visibility at 150 m, and the interaction of FOV with visibility at touchdown, FOV with pilots at 600 m, and FOV with both visibility and pilots at 600 m were significant. Because of the single locations at which reliable results were obtained for each of these main effects and interactions, it is possible that these effects are spurious. The FOV main effect interactions it is possibile that these effects are spurious. The FOV main effect was significant at 450 and 600 m but oddities with these results lead Armstrong to disqualify them as meaningless. At 450 m, performance on the unrestricted FOV was "abnormally good", while at 600 m, the restricted condition resulted in performance superior to the unrestricted condition.. The pilot main effect achieved significance at each location with the exception of 450 m, which is consistent with results obtained on the altitude measure. Finally, the visibility-pilot interaction produced no significant effects at night for any of the five locations.

It would appear that the only variable to which descent rate is sensitive is that of pilot differences. No other main effects or interactions demonstrated a reliable pattern of effects. Because of possible access to a vertical climb indicator, pilots might have been able to minimize the effects of FOV and visibility. Armstrong did not specify whether this instrument was, in fact, available.

However, it might be expected that, in keeping with the visibility effects displayed with the altitude measure, pilots would compensate for these altitude differences by means of descent rate if they were aware of them. Consequently, it appears that the vertical velocity results neither support nor oppose the visibility effect on control of altitude.

Descent rate proved to be a very sensitive measure in Kraft et al.'s investigation of FOV and scene complexity (see p. 19 for details). Both FOV and scene complexity were significant at the 12,000-foot measurement location while scene complexity was also significant at the 3000-foot location. Scene complexity interacted with trials at the middle locations of 6000 and 3000 feet, and with FOV at 3000 feet. When distance is used as an additional variable, main effects of distance and trials were obtained as were the interactions of trials with FOV, FOV with scene complexity, scene complexity with distance, and the three-way interaction of trials with scene complexity with distance.

Slowest descent is found in the unrestricted FOV, simple scene while the unrestricted FOV, complex scene and the restricted FOV, simple scenes had the fastest descent rate. Conservative decent rates are not surprising in the unrestricted FOV, simple scene condition because of pilot sensitivity to loss of important information. Why the restricted-FOV, simple-scene condition would produce fast descent rates is unclear unless the combined reductions of both variables left the pilot unaware that a rapid descent was occurring. However, since there is nothing in the simple scene to see, FOV restrictions would be expected to make little difference.

The scene complexity-distance interaction occurred because of initially faster descent rates which are then rapidly diminished with the simple scene in comparison to the complex scene. This result complements those of glideslope deviation in that deviations from the glideslope lessened as distance the runway decreased, especially for the simple scene. Together these results suggest that differences in scene complexity have a smaller effect on vertical performance as the aircraft approaches the runway. Trials interacted with FOV because little difference between the two groups was found on the first trial while substantial differences appeared on trials two and four, with the wide FOV displaying higher vertical velocities.

Vertical velocity was again used by Kraft et al. in their third experiment to evaluate scene complexity, color surround, and chromostereoscopic group (for methodological information, see p. 12). No effects reached significance at the furthest location, 2.5 nm out, but color surround interacted with scene complexity at the 2 nm point. This interaction was due to the faster descent rate displayed with the red surround, simple scene and the blue surround, complex scene in comparison to the other conditions. Kraft et al. suggest that these differences are due to pilots correcting for being at a higher altitude with the red surround, simple scene and the blue surround, complex scene, but comparatively lower in the other two conditions.

At the 1 nm location two interactions were significant: trials by scene complexity and trials by scene complexity by chromostereoscopic groups. The former occurred because of slower descent rates with the complex scene on early trials while no apparent pattern across trials appeared with the simple scene. The three-way interaction resulted from the blue advancing and neutral groups descending more rapidly with the simple scene and at a rate which the red advancing group matched only with the complex scene.

At the closest location, .5 NM, a trial by scene complexity interaction again occurred because of the slower rates obtained on the first trial with the simple scene. Faster rates were found on the remaining three trials. The neutral group, in comparison, consistently had the slowest descent rate and, at this location, demonstrate the greatest slowing of their descent. This is probably due to pilot corrections made in response to being below the glideslope at this point. With the complex scene descent rates progressively decreased across trials. Also, scene complexity again interacted with chromostereoscopic groups interaction, predicted by Kraft et al. was obtained. A significant four-way interaction of trials, scene complexity, color surround, and chromostereoscopic group occurred with the unadjusted touchdown (see p. 54 for details about the touchdown measures).

As was found with altitude and glideslope, trial effects occurred only at the two furthest locations in the third experiment while (observed for both altitude and descent rate) trial effects were primarily at the closest locations. Unlike Armstrong, Kraft et al. found vertical velocity to be sensitive to main effects and interactions of each of the variables tested.

Two studies by Brictson (1967; Brichtson et al. 1969) examined differences in aircraft carrier landing performance as a function of day versus night landings. The 1967 study, using experienced Navy pilots found a reliable difference in sink rate, with night landings displaying higher rates and harder landings. This confirms the glideslope and wire arrestment data which both demonstrated greater difficulty in controlling altitude at night compared with day landings. In an extension of the 1967 study, Brichtson et al. examined performance of combat ready pilots. Although they confirmed the findings of landing longer at night with greater altitude variability at night, no differences in sink rate were obtained.

Two displays which provide rate as well as displacement information were compared with the FLOLS in order to identify any differences in descent rate at touchdown (see p. for details concerning methodology). Kaul et al. found a significant main effect of display, as well as reliable pairwise differences between the command display and both the conventional FLOLS display and the rate display. Touchdown rate was highest with the conventional display and lowest with the command display. Each of the pairwise comparisons accounted for two percent of the variance. This finding complements the results obtained with the glideslope deviation measure which showed that pilots flew above the electronic glideslope with both the conventional and rate displays, and slightly below the glideslope with the command display. A shallow glidepath is frequently followed by a slower sink rate while steeper glidepaths often produce faster rates.

Time of day failed to differentially influence descent rate nor were there interactions of display type, time of day, and turbulence. With the glideslope measure there was an effect of time of day but it occurred only at the farthest measurement location. Apparently, pilots were unaffected by time of day at the closer locations and this is seen in the similarity in descent rates found in each condition.

Hill and his colleagues (Hill et al., 1971; Hill et al., 1974) used climb rate to assess pilot differences in performing an ILS approach. The methodology used in both experiments was identical, except that additional performance measures were included in the later experiment (see p. 15 for details). Both experiments failed to provide significant effects. These results coincide with those obtained with the first Hill et al. experiment using another measure presumed to be sensitive to altitude control, glideslope. The second experiment, however, found no main effect of glideslope, but altitude means differed reliably as did the pitch standard deviations. This suggests that variations in skill at controlling altitude due to pilot experience are occurring and pilots are not compensating for them by means of vertical velocity.

For the straight-in approach, vertical velocity provided significant effects of pilots (Armstrong); time of day (Brictston but not Kaul et al.); trials, FOV, and scene complexity (Kraft et al. Exp. 2); color surround, scene complexity, and chromostereoscopic groups (Kraft et al., Exp. 3) and display-type (Kaul et al.). Pilot differences due to experience, however, were not found in the two studies which investigated them.

In most of these experiments, significant differences obtained with glideslope deviation also appeared with vertical velocity. Moreover, the two measures frequently follow a consistent pattern: deviations above the glideslope were accompanied by higher vertical velocities and deviations below the glideslope were associated with lower vertical velocities. This relationship was found in several of the studies discussed earlier, including the scene-complexity-distance interaction found in Kraft et al.'s (1980) second experiment; the surround-color-by-scene-complexity interaction of their third experiment; and Kaul et al.'s (1980) main effect of display type.

This pattern is to be expected if one assumes that the pilot is aware of his or her position relative to the desired glideslope and is compensating for it by means of descent rate. A possible implication of this concerns the type of conclusion to be drawn whenever differences between groups occurs on these measures. If two groups differ in that one is above, the other below, the desired glideslope and if their vertical velocities follow the above pattern, the obvious conclusion is that they differ in terms of their ability to control the aircraft's vertical dimension. However, in light of the consistent relationship obtained between the two measures, such a conclusion may be hasty. Only in the situation where the vertical velocities exceed the design specifications of the aircraft can this conclusion be drawn unconditionally.

The above situation can be contrasted with the pattern found in Brichtson's (1967) study of time-of-day effects on carrier landings. He found that night landings tended to be flown below the glideslope and with greater variability in comparison to day landings. In addition, vertical velocities tended to be higher and touchdowns harder at night than during the day. This pattern does allow the conclusion that vertical control is deficient at night in comparison to day landings in that pilots are not modifying their descent rate so as to improve their position relative to the desired glidepath.

Runway Approaches Involving Turns. In assessing the effects of restricting the FOV and modifying scene complexity, Kraft et al. (1980) found a significant FOV by scene complexity interaction at the second measurement location, where the runway first comes into sight. No significant effects were obtained at the final measurement location, and performance on this variable was not assessed at the first location. This finding complements those obtained with the altitude measure in that both a FOV-by-scene complexity interaction at the middle location. The restricted FOV, complex condition and the unrestricted FOV, simple scenes displayed highest rates of descent and lowest altitudes, followed by the unrestricted FOV, complex condition, with the restricted FOV, simple scene condition producing the lowest, most conservative rate of descent, and the highest altitude. By the time the final measurement location has been reached, pilots have similar vertical velocities, but the FOV interaction with scene complexity is again found at the third location on the altitude measure.

In light of the earlier discussion concerning the relationship between glideslope deviation, or in this case altitude, these results suggest that control of the vertical dimension is affected by the combination of FOV and scene complexity variables, at the middle measurement location. The two combinations which produced the highest rate of descent also were somewhat low relative to the desired glideslope (although glideslope deviation per se was not assessed) while the conditions showing the lower vertical vèlocities were, according to the authors, close to the desired glideslope. This pair of results implies that pilots were much more conservative in their control of the aircraft's vertical status under the wide FOV, simple scene condition and the restricted FOV, complex scene. Having the runway in sight apparently encouraged the pilot to select similar descent rates regardless of variations in the two experimental variables. However, an explanation of why performance varied as a function of FOV in the simple scene remains unavailable.

Differences in sink rate were found in Lewis and Krier's (1969) study of variations in performance under monocular and binocular conditions. Unfortunately, the authors do not specify which condition produced the higher sink rate. Nonetheless, this result, combined with the finding of steeper approaches under monocular conditions suggests that altitude control is modified when only one eye is used even though differences in touchdown deviation from the target did not appear. As was noted in the discussion on the latter measure, pilots might be able to salvage a poor approach by altering the glidepath toward the end of the approach in order to bring about a touchdown on the target. Use of supplementary measures such as descent rate make it possible to identify differences in performance which otherwise might be overlooked.

Non-landing Maneuvers. Hill and Goebel (1971/1974) and Hill and Eddowes (1974) both used climb rate to assess differences in performance due to variation in pilot experience (see p. 18). Three maneuvers were used in both experiments, with rather different results. Hill and Goebel failed to find any significant effects or correlations on the two pitch and roll tracking studies. Similarly, there were no significant effects with the altitude measure as well. In the 1974 study, however, significant differences in means were found when power changes were required while tracking the two axes and when they were not, reliable differences in standard deviations occurred. A correlation of climb rate with rudder was found in the first maneuver while a negative climb rate-power correlation appeared in the second maneuver. Power is used to control altitude and thus increses in climb rate would be expected to result in decreases in power. The correlation between climb rate and rudder in the first Hill and Eddowes maneuver is unexpected yet is confirmed by the altitude results. The altitude measure correlated with three lateral measures, including rudder and ailerons, in the Hill and Eddowes experiment. Apparently, this correlation represents differences in coordinated control of the two dimensions disrupted in this maneuver. Consquently, each Hill experiment provided results with the vertical velocity measure which are consistent although not identical, with those obtained using the altitude measure. But the two experiments do not provide results which are consistent with each other based upon the same measure.

Performance on each of the five segments of the third maneuver also failed to be consistent in the two experiments. Hill and Goebel found significant differences between means on the climb segment, reliably different standard deviations on the level turn, and differences in means and a negative correlation with airspeed for slow flight. Significant means and standard deviation differences as well as negative climb-rate-airspeed correlation and positive climb-rate-pitch correlation were found on the straight-ahead descent. Hill and Eddowes, on the other hand, found only a negative correlation with role on the climb segment and a correlation with airspeed on the right turn. Significant differences in means as well as correlation with ailerons was found for slow flight with a climb rate-power correlation occurring on the descending left turn. No significant effects were found on the straight-ahead descent. Why these differences should occur is not clear.

There is some evidence for a pattern in the Hill and Goebel results. Those maneuvers which probably require a major change in power in conjunction with changes in pitch, such as climbs, descents, and slow flight, have in common the finding of significant differences in means on the vertical velocity measure. The 1000-foot climb, descending turn, 500-foot descent, and slow flight maneuvers all support this pattern, although the roll and pitch tracking task with power changes does not. Maneuvers demanding a turn, where changes in the lateral axis dominate, tend to display differences in standard deviations, as was the case with the level turn and the descending turn.

Unfortunately, this pattern fares less well in the Hill and Eddowes experiment. Of the five maneuvers which did provide significant main effects (including those maneuvers not used by Hill and Goebel), only three would be expected in accordance with the pattern: the differences in means found in slow flight, the differences in means on the roll and pitch tracking task with power changes, and the differences in means in the altitude and position tracking task. The latter involved maintaining a constant altitude and straight course while being subjected to severe side winds and vertical drafts.

There is even less evidence of a pattern in the correlations. Climb rate did correlate with two lateral measures, roll and aileron, on maneuvers requiring coordination between throttle and yoke in the vertical dimension, climb and slow flight. Most of the correlations were with airspeed, or the related measures of power and pitch. These correlations were not, however, found on similar maneuvers, in that they occurred on descent, turns, and descending turns. The six maneuvers used only by Hill and Eddowes fail to clarify the situation. Only two correlations were found and both involved vertical measures. Once again, the maneuvers were dissimilar, as one required a turn, the other maintenance of a constant altitude and course while experiencing wind directed to the vertical and lateral axes of the aircraft.

The remaining six maneuvers were included only in the 1974 study. Reliable differences between standard deviations were obtained for the roll, pitch, and yaw tracking task or for reduced bandwidth roll tracking. Significant differences in means occurred on the reduced amplitude roll tracking task and again on altitude and position tracking. Climb rate correlated with altitude on the latter maneuver. Finally, climb rate correlated with elevator on the left circular turn.

Summary. The vertical velocity measure seems to be most informative when used in conjunction with a measure of vertical height, such as altitude and glideslope deviation. In those experiments where differences in vertical height, due to some experimental variable, are accompanied by differences in descent rate, the direction of the differences can be informative in terms of inferring whether the pilot is aware of the aircraft's deviation from the desired vertical height and is altering the descent rate accordingly.

A number of experiments did not find any differences in vertical velocity even though the altitude or glideslope deviation measures did. For example, Armstrong (1970) failed to find visibility effects and interactions at most of the measurement locations used even though such differences were reliabibly obtained at virtually all locations with the altitude measure. The reason for this is not obvious. It could be that pilots find it easier to trim the aircraft for a given descent rate, in spite of the experimental manipulations, whereas control of altitude is more demanding and easily influenced by these manipulations. Pilots may be more familiar with descent rates required during a maneuver or may be told what descent rate to use in the experiment, and can relay on instruments or position of the nose relative to the horizon to achieve the desired rate. The altitude required at a specific measurement location needed to achieve the approach glidepath may be less apparent to the pilot and thus precludes dependence on an altimeter to attain the desired height, even if this instrument is available.

The relationship between vertical velocity and altitude is less obvious in non-landing maneuvers, which may explain the failure to find similar results with the two measures. Most of the Hill maneuvers required the maintenance of a constant altitude, which obviously means no climb rate. Although differences in standad deviations with the two measures might be expected to occur at the same time, such a pattern was the exception rather than the rule. Similarly, the two measures failed to correlate consistently, although correlations between related measures were common. Unfortunately, there is no obvious way to predict the measures which will correlate in a given maneuver.

Longitudinal Deviation from Target Touchdown

Longitudinal deviation from a marker specifying where the pilot is to attempt the touchdown has been a popular performance measure, especially in earlier experiments, because it does not require sophisticated technology to implement while still providing quantitative data. The measure has frequently served as the central dependent variable in a line of research dealing with the related issue of monocular versus binocular vision, restricted peripheral vision, and general manipulation of FOV.

Research concerned with these experimental variables have used this measure in conjunction with a number of other measures, which is a more effective use of the deviation measure because of a fundamental problem with it. As the pilot approaches the target and detects that either an overshoot or undershooe is occurring, the temptation is to radically alter the glidepath. The pilot might be able to salvage a poor approach, thus masking differences due to the manipulated variables. By supplementing this measure with other dependent variables such as altitude and glideslope the researcher can discern the extent to which the pilot has modified the earlier approach.

Straight-in Approach. Armstrong (1970) used touchdown deviation in order to detect differences in performance due to visibility, FOV, and pilot differences (for details, see p. 8). For day landings, a significant main effect of pilots and a reliable visibility by pilots interaction alone were obtained, while at night the main effect of pilots, the interactions of pilots by visibility, visibility by FOV, FOV by pilots, and the three-way FOV by visibility by pilots interaction were significant. Armstrong questions the reliability of the last three results because of the failure to find a pattern of significant results for these interactions.

The lack of an effect of FOV is consistent with the results using other measures which reflect pilot control of altitude, including altitude, descent rate, and pitch angle. Similarly, pilot differences, which were found for both day and night landings, were uniformly apparent with the other measures while the visibility main effect consistently failed to appear at touchdown.

Kraft et al. (1980, Exp. 3) calculated deviation from the touchdown point in two ways as part of their investigation into the effects of scene complexity, color surround, and chromostereoscopic group (for details, see p. 12). One touchdown analysis was based upon measuring deviation at 450 msec before touchdown and calculating deviation from the electronic glideslope intercept (at 1840 feet from the threshold). The second analysis took into account pitch angle, aircraft sensor position, extent of gear compression, and algebraic sign, and assessed deviation from the instructed 1000-foot touchdown goal to which the pilots were aiming. Note that this target was not marked on the runway.

Both analyses produced the same results; A significant main effect of color surround and a reliable chromostereoscopic group-color surround interaction. Pilots overshot the target in both color conditions, with the blue surround producing the greatest deviation as well as the highest variability. The interaction occurred because the red advancing group overshot more with the red surround color, and produced less variability with this color, while the blue advancing and neutral groups overshot more with the blue color. For the red color, the neutral group tended to undershoot the target touchdown point and their variability was the smallest of all conditions and groups. In general, surround color least influenced the neutral group for they also landed closest to the target under the blue color in comparison to the other chromostereoscopic groups.

Kraft et al.'s hypotheses concerning the interaction of color surround and chromostereoscopic group were supported. The chromostereoscopic groups performed about equally when the runway and surround were a matching blue. When the blue advancing group was presented with the red surround, the runway should have appeared above the surrounding red which should induce pilots to land shorter while the red advancing group should perceive the surround to be closer and land longer. Both of these hypotheses were supported.

The color-surround main effect and its interaction with chromostereoscopic group did not occur with the altitude and electronic-glideslope deviation measures. However, deviation from the calculated slideslope (derived from the instructed touchdown point) displayed the same results as the longitudinal-deviation measure. This supports the anticipated relationship between measures of vertical position and actual touchdown point. With the vertical velocity measure, the only significant result was the interaction between color surround, chromostereoscopic group, trials, and scene complexity.

Longitudinal deviation from the desired touchdown point on an aircraft carrier's deck was used in Kaul et al.'s (1980) investigation of display type and time of day (see p. for methodological details). Based upon mean values, no main effects and interactions were obtained. This contrasts with the mean glideslope errors which provided a reliable difference between the conventional and command displays at all measurement locations, including touchdown. The expectation is that glideslope differences would be accompanied by longitudinal deviations at touchdown. However, standard deviations were reliably different as a function of display type, with two significant pairwise comparisons, the exception being the conventional versus rate display comparison. The command display produced the smallest deviations and the conventional displays the largest.

This result is different from the order shown by the means. In the latter case, the order was reversed. The standard deviations did match the pattern displayed by longitudinal deviations. Similarly, glideslope rms errors for the last segment (1500-0 feet from touchdown) were smallest for the command display and largest for the conventional display, this difference being significant. No time of day effects were found with the longitudinal deviation measure, nor did turbulence interact with either variable. A turbulence interaction with display type was found for glideslope rms error, in this segment, however.

In general, the results with the glideslope and longitudinal deviation measures match, especially in terms of the direction of the differences. In spite of the failure to find differences with the means, the standard deviation scores confirm the patterns presented by both the glideslope rms error scores and the glideslope standard deviations. Deviation from the desired glideslope is accompanied by deviations from the desired touchdown location.

One of the earliest studies to manipulate FOV used a projective periscope in one condition and vision reducing goggles in a second condition. Roscoe (1948) devised a periscope protruding from the top of a modified Cessna T-50 to project that part of the external world directly in front of the aircraft onto a six-inch square ground glass. This display restricted peripheral vision and eliminated both binocular information and head movement parallax while providing a visual field of 10 degrees 40 minutes by 11 degrees 50 minutes for the horizontal and vertical dimensions respectively. The vision reducing goggles provided the same FOV without disturbing binocular information. Head movement parallax was restricted through the use of a metal screen on the windscreen.

Six flight instructors performed straight-in approaches with all of the aircraft instruments available. The aircraft was trimmed as required by the safety pilot and descent was controlled by power. To assess differences in precision of the landings, absolute distance (sign disregarded) from targets placed on either side of the runway to the actual touchdown spot was recorded. Performance in all three conditions differed significantly from each other, with the greatest accuracy displayed under the contact (control) condition while the least accurate landings occurred with the periscope. No practice effect was found within any condition but a general practice effect across all conditions was obtained.

Because of difficulties with the goggles due to pilots being able to shift the FOV by moving the body, Roscoe was reluctant to conclude that binocular cues are as important as his results suggest. Nonetheless, deviation of actual touchdown point from the target position did reliably demonstrate differences due to the experimental manipulations. Unfortunately, Roscoe did not use other measures, making it impossible to relate his results to performance on other aspects of the task, especially in terms of control of the vertical axis of the aircraft.

Traffic Pattern Approach. Roscoe, Hagler, and Dougherty (1968) extended the investigation into the effectiveness of the projection display by manipulating the magnification of the scene displayed on the screen. Three magnifications of the 30-degree outside angle were tested: 15 degrees (magnification of 2.00), 25 degrees (magnification of 1.20), and 35 degrees (magnification of .86). Six military pilots, having between 1000 and 5000 hours of flight time, performed traffic pattern approaches ending with a touchdown aimed at the runway markings 1500 feet from the end of the runway.

The pilots had available an altimeter, vertical speed indicator, directional gyro, turn-and-bank indicator, and an airspeed indicator, as well as the eight-inch square projection display. Mounted on the latter display were cord cross hairs which, during level flight, matched the horizontal hair with the horizon and the vertical hair with the centerline of the runway during takeoff and landings.

Manipulation of image magnification directly affected the location of touchdown with pilots overshooting with low magnification (.86) and undershooting with high magnification (2.00). Magnification of 1.20 resulted in a mean deviation of only 11 feet, which did not differ statistically from a zero constant error. Roscoe et al. suggested that the touchdown means were probably a simple linear function of image magnification for the ranges sampled. The contact condition did not differ statistically from the 1.20-degree magnification condition when compared on the last 10 trials, suggesting that a prctice effect was present. However, analysis of means of the three image magnification conditions and the control condition over blocks failed to find a significant improvement in mean deviation. The low and high magnification groups displayed somewhat greater variability relative to the intermediate magnification condition but this result failed to reach significance. When variability on only the last block of trials is analyzed, the three image conditions as well as the contact visibility control all failed to differ statistically. A practice effect with reduction in variability for all image conditions was obtained but not with the control. Roscoe et al. concluded that the main effects of image magnification and practice both affect the accuracy and variability of target landings as assessed by deviation from the target marker.

Roman, Perry, Carpenter, and Ausi (1957) also used absolute deviation to assess performance under restricted horizontal FOV conditions. Two experienced test pilots performed touch-and-go landings in a T-33A jet trainer. FOV was manipulated with transparent amber-colored plastic shields in congunction with blue goggles, the combination producing an opaque appearance. Seven levels of FOV were used, ranging from 5.7 to 360 degrees.

The results showed a failure to find a correlation between horizontal FOV and landing error for the seven FOVs. This is in spite of the fact that, with the 5.7-degree FOV, use of only one eye was possible. These results directly contradict the findings of the two Roscoe experiments even though the same performance measure was used. Obvious differences between the experiments include aircraft type and the manner in which FOV was restricted. Roscoe et al.'s reduction of target deviation by means of the 1.2 degree magnification suggests that the display itself is responsible for the different results.

Pfaffman (1948) tested the accuracy of monocular landings as well as restriction of peripheral vision by using modified Mach II flight goggles which, because of the thick construction across the bridge of the nose, reduce the binocular field. A second set was devised to reduce peripheral vision. In both cases the FOV was not markedly affected. Five Naval flight instructors performed standardized precision landings to a circle placed in the center of the field. Landings were scored as either in or out of the circle. A Navy N3N primary training biplane was used.

With unrestricted vision 11 out of 12 trials were successful as defined by landing in the target circle. Only seven of 13 trials were successful with the binocular-reduced goggles, the failures usually resulting from overshooting the circle. These failures were largely due to two of the five pilots. Restriction of peripheral vision did not affect success in landing on the circle.

Lewis and Krier (1969) suggest that the deficit in monocular landings may be due to Pfaffman's pilots having to land in a large field where linear perspective cues are not available. If they are correct this would explain why Pfaffman's findings contradict those of Roman et al. To test this Lewis et al. had 13 pilots (12 test pilots) perform touch-and-go landings in a T-33A jet trainer. One pilot was given additional experience with the task in order to test for learning effects. Target touchdown point was marked by a white line across the runway. They found a slight but non-significant trend toward better performance under monocular conditions as measured by longitudinal deviation from the target and significant differences among pilots for the first part of the experiment. This contrasts with the steeper approaches displayed under the monocular condition, as measured by glideslope deviation. The one pilot who participated in the second, extended practice phase (three flights over a three-week period) demonstrated slightly better but non-significant performance under binocular conditions, but this may have been due to unusual weather conditions. No learning effect was apparent.

Lewis, Blakely, Swaroop, Maters, and McMurty (1973) extended their investigation to general aviation pilots flying a Piper Cherokee airplane. For the 14 pilots having less than 100 flight hours, the average monocular performance was

24.7 feet better than binocular performance, a difference which proved highly significant. Twenty-four pilots had less than 200 hours flight time and their average monocular performance was superior to the binocular by 19.2 feet. For those five pilots having over 200 hours monocular performance was better by 29.9 feet, which again was statistically significant. Collapsing across experience monocular performance was 18.8 feet better than the binocular condition, which also was reliable. No effect of eye dominance was obtained. Surprisingly, low-time pilots performed more accurately than the experienced group, but this may have been due to differences in amount of recent experience. No differences in glideslope deviation were obtained, however. The expectation is that reliably different longitudinal deviations would follow different glidepaths.

In response to Lewis et al.'s finding of superior monocular performance, Grosslight, Fletcher, Materton, and Hagen (1978) identified three factors in the Lewis study which might have biased the results, including differences in motivation, unequal number of experimental landings under monocular versus binocular conditions, and subject awareness of prior experimental findings concerning this issue. Thirteen pilots, with an average of 123 hours of flying time, performed landings in a Beech Sport airplane. Care was taken to avoid the problems which cloud the Lewis results.

Mean absolute error in monocular versus binocular conditions did not significantly differ nor was there a difference on the last six landings although a practice effect was obtained. Monocular approaches were flown significantly higher and steeper while tending to overshoot. Binocular landings, on the other hand, were more likely to overshoot the target.

Longitudinal deviation from the target touchdown point has been shown to be a sensitive measure of several variables. Pilot differences were obtained by

Armstrong, including interactions with both variables of FOV and visibility, and Lewis and Krier. Differences between monocular and binocular conditions were obtained by Pfaffman, and Lewis et al. (1973) although both Lewis and Krier, and Roman et al. failed to obtain a reliable effect. Longitudinal deviation effectively provided results which supported Kraft et al.'s hypotheses concerning the effects of color surround and chromostereoscopic groups while FOV and magnification were reliable in the Roscoe experiments. Finally, practice effects arose in the Grosslight et al. and Lewis et al. (1973) experiments.

A different use of the distance from touchdown measure was used in the first experiment of Kraft et al. In assessing the effects of FOV and scene complexity, they measured distance from touchdown at the first measurement location. Significant main effects of FOV and scene complexity were obtained. With side windows unavailable pilots made tighter turns, presumably in order to view the runway earlier. Similarly, tighter turns were found with the simple scene, perhaps, as Kraft et al. suggest, because the simple scene is perceived as "more distant" or "less well defined." The restricted FOV condition, contrary to expectation, failed to produce more variable flight paths in comparison to the unrestricted FOV. The measure, when used in this way, mapped out the actual path flown when completing a turn toward the runway, and provides results which complement those obtained with the altitude and vertical velocity measures at the middle measurement location.

Finally, Collyer et al. (1980) used longitudinal deviation from the aircraft carrier during the circling approach to landing in order to assess differences in performance due to FOV (see p. for details). Data were obtained at five locations during the turn, and on training trials, pilots in the narrow FOV condition tended to produce tighter turns, as was the case in the first of the Kraft et al. experiments.

Data from the test phase showed that the narrow FOV, circling group still made the tightest turns while the wide FOV, circling approach made the widest turns. The former group also proved to be least variable. Comparisons of the final trials of the training phase with the early trials of the test phase produced only one unique result: On the first two trials flown with the wide FOV during the test phase, the group which had experienced the narrow FOV, circling approach during training turned more widely than the group which had the wide FOV during training.

This measure appears to provide a direct measure of an effect which was implied by the glideslope measure. During the training phase, the narrow FOV, circling group displayed more variability in comparison to the other two conditions. Collyer et al. proposed that this difference was due to the greater difficulty experienced by pilots in controlling the aircraft's position on the circular track. The longitudinal deviation measure seems to confirm this explanation. In the first Kraft et al. experiment, the main effects of FOV and scene complexity obtained at the first measurement location with the longitudinal deviations may be related to the interaction of these two variables found at the middle measurement location with both the altitude and vertical velocity measures. Whether these results are due to increased workload in the same way as was the case in the Collyer et al. experiment is unclear.

The majority of experiments which used both glideslope (or altitude) and longitudinal deviation measures provide support for the expected relationship between the two measures. Kraft et al. found support for their hypothesis concerning the interaction of color and chromostereoscopic group with both measures in their third experiment and also identified a likely relationship between tightness of the turn and altitude at the end of the turn as a function of scene complexity and FOV. Collyer et al. (1980) obtained a similar relationship between width of the circle and glideslope variability, also due to FOV. Finally, variations in glideslope deviation due to display type were later followed by the same pattern with longitudinal deviation variability in Kaul et al.'s experiment.

Less clear were the experiments on monocular versus binocular landings. Grosslight et al. (1978) did find that steeper glidepaths produced in the monocular condition were accompanied by overshooting the target. The two Lewis experiments (1969; 1973) did not display this relationship, however, in that significant differences were found with only one of the measures in each experiment.

Aircraft Carrier Measures

Because of their similarity to other measures, performance measures which are peculiar only to landing on aircraft carriers are included. These measures include wire arrestment, which records the wire that successfully stops the aircraft, and percent of unsuccessful landings (bolters). Both measures reflect the location at which the aircraft touches down, since bolters typically result from a failure to set down on the recommended landing area on the carrier. Consequently, results obtained with this measure supplement the longitudinal deviation from target measure.

Straight-in Approach. Two experiments, both of which were concerned with the variables of day versus night landings and pilot experience, were performed by Brictson and his colleagues (Brictson, 1967; Brictson et al., 1964). In the first study (see p. 30 for additional details), Brictson found that 40% of the aircraft landings at night were stopped by the number 4 wire in comparison to 18% during the day. Less than 5% hit the first wire at night compared to 17% during the day. As noted earlier, Brictson found that night approaches tend to be low, with higher vertical velocities and harder landings, in spite of there being glideslope information provided by the FLOLS. Taken together, these results strengthen the conclusion that pilots experience greater difficulty in controlling the aircraft along the vertical axis at night.

In the 1969 study, combat ready pilots were found to land shorter during the day (#1 and #2 wires) than at night (#3 and #4 wires) with almost twice as many bolters at night, thus confirming the results Brictson found with slightly less experienced pilots. Comparisons of experienced and less experienced pilots showed that day landings became less variable while night altitude variability tended to remain unchanged. Apparently, the problems associated with night landings are not resolved through additional practice.

Brictson et al. (1969) also investigated landing success when weather conditions produced a pitching deck. Comparisons of day versus night landings showed that 70% of attempted night landings were unsuccessful (bolters or wave-offs) compared to 18% of the day approaches. In that pilots continued to make low approaches at night in spite of the pitching deck, as shown by the glideslope measure, such a high failure rate is to be expected. It should be noted that this measure is biased by the judgment of the landing signal officer (LSO), who might tend to wave off borderline cases more often at night than during the day. This bias probably extends to other variables such as pilot experience.

Two empirical performance envelopes were devised based upon two standard deviations away in either direction from the mean of successful landing performances, one for the day and one for the night data. If the pilot wanders beyond the empirical performance criterion envelope during the day, successful recovery occurred in every case. At night, however, only 45% of the F4 and 55% of the A4 approaches recovered. These percentages were obtained from the data of experienced pilots. With inexperienced pilots, 38% of the day and 19% of the night approaches recovered. Note the potential role of LSO bias here also.

The effect of delay in the presentation of visual information on performance of simulated aircraft carrier approaches was assessed by Cooper, Harris, and Sharkey (1975). Twelve pilots, most of whom were carrier qualified from 2.5 to 25 years ago, flew a TRADEC F-4 flight simulator, equipped with four degrees of freedom motion and a visual monochromatic CRT display system providing a 19-degree square FOV. Two conditions were used: (1) The no-delay condition with the normal update lag of between 12.5 and 25 msec; and, (2) the delayed condition of between 112.5 and 125 msec.

Six tasks of varying difficulty were used. In each task, the simulated carrier moved at 35 knots, and the aircraft was always trimmed for the correct airspeed and glideslope. Tasks A and B were considered the least difficult. In the former, the aircraft was positioned to the right of the deck's centerline, necessitating a left turn to attain centerline lineup. A straight-in approach to the carrier served as tasks B and C. Task C was considered moderately difficult in comparison to task B because of the inclusion of light turbulence. The second moderately difficult maneuver, task D, involved the aircraft's initial position being left of the centerline, thus requiring a right turn in order to bring about deck lineup. Task E, considered to be most difficult, was similar to task D except that simulated heavy turbulence was included. Also considered most difficult was task F, which was the same as task A in that it also required a left turn, but differed because of the addition of severe turbulence.

In their first experiment, the number of trials needed to perform three successive aircraft arrestments (traps) was recorded. To be considered a trap, five conditions specifying altitude, descent rate, pitch, and roll requirements when entering the trap area had to be met. Note the similarity between the trials-to-criterion approach used here and Brictson's performance envelopes. Both entail the assessment of performance on several measures simultaneously. They differ only in that in Brictson's experiment the position of the aircraft relative to the empirical envelope is used to predict success in landing whereas Cooper et al. use their envelope as a requirement which the pilot must meet; the number of trials needed to satisfy this requirement serves as the dependent variable. The tasks were performed in accordance with their order of difficulty, beginning with the two least difficult tasks. For each pair of same-difficulty tasks, one task was assigned the no-delay condition and the second the delay condition.

The main effct of visual delay failed to prove reliable, nor did this variable interact significantly with task presentation order. Task presentation order did reliably affect performance but this effect was due to a reduction in trials-to-criterion with increasing task difficulty, meaning that performance changes due to practice dominated the task difficulty factor. Consequently, visual delays of 100 msec do not appear to hinder learning to perform simulated carrier landings when a trials-to-criterion measure based upon touchdown performance criteria is used.

Brictson, Burger and Wulfeck's (1973) Landing Performance Score scale (LPS) was used in Kaul et al.'s (1980) investigation of the effects of FLOLS modifications on aircraft carrier approaches and landings (see p.   for methodological details). The scale assigns a number between 1.0 and 6.0 to each of the possible landing outcomes (except ramp strikes), with the highest score given to aircraft arrestment by the third wire and the lowest to waveoffs. In effect, progressively lower scores are given as the aircraft deviates in either direction from the third wire.

For LPS means, the main effect of display type was significant, as was the pairwise comparison between the command and conventional displays, with the highest LPS score appearing for the command display. This result differs from that found with the longitudinal deviation measure in that, although not significant, performance with the conventional display was closest to the desired touchdown point, followed by the command display. Glideslope mean errors at the ramp measurement location, also demonstrated the superiority of the conventional display with largest mean deviations from the glideslope occurring with the command display. However, the LPS order of displays matches the bolter rate in that the lowest rate was obtained with the command display. This may explain why the LPS failed to provide the same display order.

Means for the time of day variable also differed reliably, but, surprisingly, the LPS was higher (indicating better performance) for night landings. Similarly, night landings were, on the average, closer to the desired touchdown point, according to the longitudinal deviation measure, although this result was not reliable.

LPS standard deviations, on the other hand, displayed only a main effect of time of day, with larger standard deviations occurring for day landings. This result, together with the superior night performance reflected in the LPS means, contradicts Brictson's (1967; 1969) conclusions concerning greater difficulties in approach and touchdown performance at night. However, glideslope rms error during the last measurement segment was lowest for day landings. In contrast to the longitudinal deviation measure, no standard deviation differences due to the main effect of display type were found, although the display orders matched, with the smallest standard deviations produced by the command display on both measures. This superiority of the command display is confirmed by both glideslope standard deviations and glideslope rms errors.

Although the differences were not reliable, the pattern of results obtained with the LPS measure matched those found with the longitudinal and glideslope deviation measures, the latter when assessed near or at the ramp. Two notable exceptions should again be pointed out. For the time-of-day variable, means and standard deviations for both the LPS and longitudinal deviations display superior night performance whereas glideslope rms error scores reflected superior day performance. The reason for this difference is unclear.

In addition, both longitudinal and glideslope deviation means reflect better vertical and touchdown control with the conventional display in contrast to the higher LPS means found with the command display. As noted earlier, both the LPS and bolter rates provided the same display order. This reflects the essential difference between the LPS and longitudinal deviation measures: LPS means also include unsuccessful landing attempts in the form of bolters and waveoffs. Consequently, the two measures should generally show a similar pattern of results, except in the extreme case where longitudinal deviations, substantial enough so as to not permit aircraft arrestment by any of the four wires, are common.

Carrier Approaches Involving Turns. Collyer et al. (1980), in their investigation of the effect of FOV on learning to land on aircraft carriers (see p. 14 for more information), used a modified version of Brictson, Burger, and Wulfeck's (1973) landing performance score rating which included a score of zero for ramp strikes. The experiment, being a quasi-transfer study, consisted of two phases, a training and test phase. Three groups, which differed in terms of the FOV and type of approach used (circling or straight-in) were all tested on the wide FOV, circling approach in order to assess transfer.

The analysis of variance for the scores obtained with the Brictson et al. scale found a significant difference of training conditions and a highly significant effect of trials. No differences, however, appeared for the test phase. Performance during the training phase was best for the narrow FOV, straight-in approach, followed by the wide FOV, circling approach, while poorest performance was found with the narrow FOV, circling group. Once again, the results obtained with the LPS complement those found with other measures since the narrow FOV, circling group displayed significantly more variability than the other groups on the glideslope measure during the training phase. Performance across training trials displayed a significant learning effect but the test trials did not show a continuation of this effect. Twenty-eight percent of approaches were successful during training while the corresponding figure for test trials was 87%.

A second carrier measure employed by Collyer et al. was time within combined tolerances (TWCT), which reflected success at maintaining glideslope, lineup, and angle of attack within acceptable tolerances defined by the LSO. Again, significant and large group differences occurred during the training phase and there was a learning effect reflected in improved performance over training trials. No differences were found during the testing phase. Collyer et al.'s use of TWCT exemplifies a third use of the performance envelope, where landing performance is scored in comparison to some ideal performance envelope.

Each component of the TWCT was also analyzed individually. With few exceptions, the same ranking of groups found with the analysis of variance during training remained. The narrow FOV, circling approach group performed worst on most components while the narrow FOV, straight-in approach was best. Also, no significant differences were found during the test phase. Groups were most different when measured by lineup while the learning effect was strongest for glideslope control. In addition, the LPS scores were correlated with TWCT data but the correlation was low suggesting that different parts of the task are tapped by each measure.

Summary. In spite of potential biases accompanying unsuccessful landings, all of the carrier measures were useful in detecting variations in performance due to such variables as time of day, weather (pitching deck), display type, practice, and training condition. With few exceptions, however, the results obtained with these measures matched those found with more conventional measures, such as glideslope and longitudinal deviation. The LPS scale offers the advantage of assigning a score to unsuccessful landing attempts and, for that reason, might be beneficial in carrier landing studies.

The performance envelope deserves additional consideration as a possible means of avoiding problems in interpreting the overall pattern produced by several measures. For example, in evaluating the results obtained with the vertical velocity and glideslope measures, there were many cases where the pilot modified the aircraft's vertical velocity in order to compensate for glideslope deviations. The implication is that the pilot is aware of this discrepancy but simply analyzing one of the measures will not show this. It may be that a performance envelope relating the two measures within the tolerances required for a safe and reasonably accurate landing is the solution, especially when there is no electronic glideslope which the pilot is instructed to follow. The latter situation is common in experiments evaluating landing performance under VFR conditions.

A second possible advantage of the performance envelope lies in the reduction in the number of statistical analyses required. One analysis might replace the several typically required when using such uni-dimensional measures as glideslope deviation, altitude, and vertical velocity.

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