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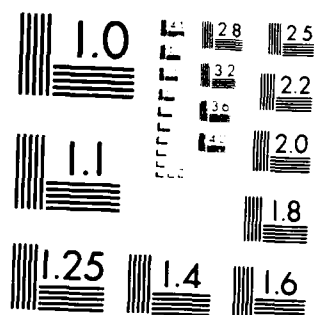
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Technical Report NAVTRAEQUIPCEN 81-C-0105-1

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SIMULATOR DESIGN FEATURES FOR CARRIER LANDING:
II. IN-SIMULATOR TRANSFER OF TRAINING

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pilots who had no prior carrier-landing experience: 16 recent graduates of Air Force T-38 training, and 16 highly experienced Navy P-3 pilots.

Display and simulator factors investigated were field of view (160 x 80 vs. 48 x 36 degrees), scene detail (day, solid-surface vs. night, point-light) and platform motion (six-degrees-of-freedom vs. no motion). Two training factors were included: descent-rate cuing (presence or absence of an extra element on the Fresnel lens display that provided information on glideslope descent-rate error), and approach type (training on straight-in approaches vs. circling approaches). Turbulance was included as a factor and pilot type (Navy P-3 vs. Air Force T-38) was also included as a factor to control this source of subject variability. After training under a certain factor-level combination, students were tested on the day, wide field of view, circling task with motion and without descent-rate cuing.

Results showed some temporary transfer advantages for the wide field of view and high scene detail conditions. Training on straight-in approaches resulted in transfer performance that was better than that produced by training on circling approaches. There was no motion or FLOLS rate cuing effects on the transfer task. Display and simulator transfer effects did not differ between the two pilot groups despite large differences in mean group performances.

As a result of these findings, it was suggested that a simulator-to-field transfer study be conducted with field of view, scene detail and approach type as factors. Such a study, using pilots from the target population of undergraduate Naval aviators, would provide the necessary information to make final simulator design decisions for the carrier-landing task.

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The entire VTRS technical staff deserves special mention once again for providing the support necessary to conduct multifactor experiments. Dr. Gavan Lintern and Daniel Sheppard wrote the briefing material for the experiment and acted as instructors during the training phase of the experiment. Dr. Lintern also conducted the pre-experimental instructional briefings. Data analysis was capably performed by Brian Nelson. Dr. Stanley Roscoe provided major editing assistance for this report and Dr. Charles Simon provided consulting for the experimental design. And finally, a special word of thanks goes out to the pilots volunteering for this experiment and the military personnel who cooperated in coordinating and scheduling pilot participation. Specifically, special thanks to Air Force Captains Kruse and Villalobos and the volunteer pilots from the 14th Flying Training Wing, Columbus AFB, Columbus, MS; and Navy Lt. Mike Harper and the volunteer pilots from Patrol Wing 11, NAS Jacksonville, FL.

SECTION 1

INTRODUCTION

The Visual Technology Research Simulator (VTRS) at the Naval Training Equipment Center (NTEC), Orlando, Florida, is designed for research on flight simulator requirements for training and skill maintenance. The VTRS consists of a fully instrumented Navy T-2C jet trainer cockpit, a six-degrees-of-freedom synergistic motion platform and a wide-angle visual system that can project computer-generated (CIG) images onto a spherical screen. The visual system is capable of displaying images via target and background projectors subtending 50 degrees above and 30 degrees below the pilot's eye level and can display 160 degrees of horizontal field (Collier and Chambers, 1978).

The current effort at VTRS involves research to define simulator requirements for the carrier landing task. Because of the large cost implications, there is a need to investigate a large number of visual and other simulator features. A research program was planned around the holistic experimental philosophy and paradigm proposed by Simon (1973; 1977a) which stresses the importance of studying as many potentially important factors as possible within a single experiment. The research program involves three major phases. The first phase consisted of performance experiments in which the effect of various simulator components on experienced pilots in the simulator was examined (Westra et al., 1982). The second phase, described in this report, consisted of a quasi-transfer experiment in which the simulator was both the training and the criterion device. Phase three will eventually include a simulator-to-field transfer experiment involving actual flight tests.

The information obtained from phase one experiments was directly relevant to the design of simulators for experienced pilot skill maintenance and transition training. The information from the phase two experiment reported here is directly relevant to the design of simulators for undergraduate pilot training. The experiment involved pilots with no carrier landing experience who were trained in the simulator under various conditions representing levels of several simulator equipment and training factors. The pilots were then tested in the simulator in its high fidelity configuration.

Three simulator equipment factors were studied in this experiment. A wide field of view was tested against a narrow field of view representative of carrier landing trainers in current use. A daytime scene was compared to a night carrier landing scene and cockpit motion was compared to the absence of motion cues. Two training factors were also manipulated during the training period. Pilots performed their training trials with either a conventional Fresnel Lens Optical Landing System (FLOLS) or with a FLOLS with added descent rate information. Approach type was also varied involving either a modified straight-in approach to the carrier or a circling approach during training. Two levels of turbulence were also used during the training trials, with pilots practicing under either calm or gusty wind conditions.

SECTION II

EXPERIMENTAL PLAN

An in-simulator transfer of training paradigm was used to study the effects of six factors plus a pilot type factor on carrier landing training. Students were trained in the simulator under various conditions representing combinations of factor levels, and then tested under a standard simulator configuration that represented maximum realism. A total of 32 pilots with no prior carrier landing experience were involved in the experiment. Sixteen were recent graduates of Air Force T-38 training and 16 were experienced Navy P-3C pilots.

FLIGHT TASK

The experimental task was a simulated daytime carrier approach and landing on the deck of the aircraft carrier Forrester with a T-2C jet. The simulated carrier was moving at 20 knots with a zero effective crosswind over the landing deck and 25 knots relative wind down the deck. The circling approach and landing is depicted in Figure 1. The task for this experiment was restricted to include only the final turn of the full circling maneuver as well as the final approach and landing.

VISUAL CUES. A successful carrier approach and landing involves the use of a family of visual cues external to the cockpit. The principal cues come from a visual landing aid called the Fresnel Lens Optical Landing System (FLOLS) for vertical glideslope control; the carrier deck runway, centerline and dropline markings for lineup control; and the sky, horizon, and seascape for general aircraft attitude control. Other cues necessary to the operation of the aircraft, possibly including motion, are also involved in carrier approaches and landings.

FLOLS. The FLOLS is the single most important source of information for the carrier landing task. It provides glideslope displacement information to the pilot during an approach. Physically located forward of the LSO platform and to the port side of the landing deck (see Figure 1), the FLOLS consists of five Fresnel lenses vertically arranged between two horizontal light arrays known as datum bars. The array of Fresnel lenses provides an image which appears to the pilot as a single sphere of light known as the "meatball." This meatball is visible to the pilot within a wedge of space 0.75 degree above and below the projected glideslope of 3.5 degrees and plus or minus 25 degrees horizontally from the center of the wedge projected parallel to the landing deck.

The pilot judges his angular glideslope deviation from the distance the meatball appears to be above or below the datum bars. A meatball that appears centered vertically between the datum bars indicates to the pilot that he is on the proper glidepath. Figure 2 gives a view of the FLOLS and its projection aft of the carrier and the perceived relationship of the meatball to the datum bars. Golovscenko (1975) provides more detail on FLOLS geometry.

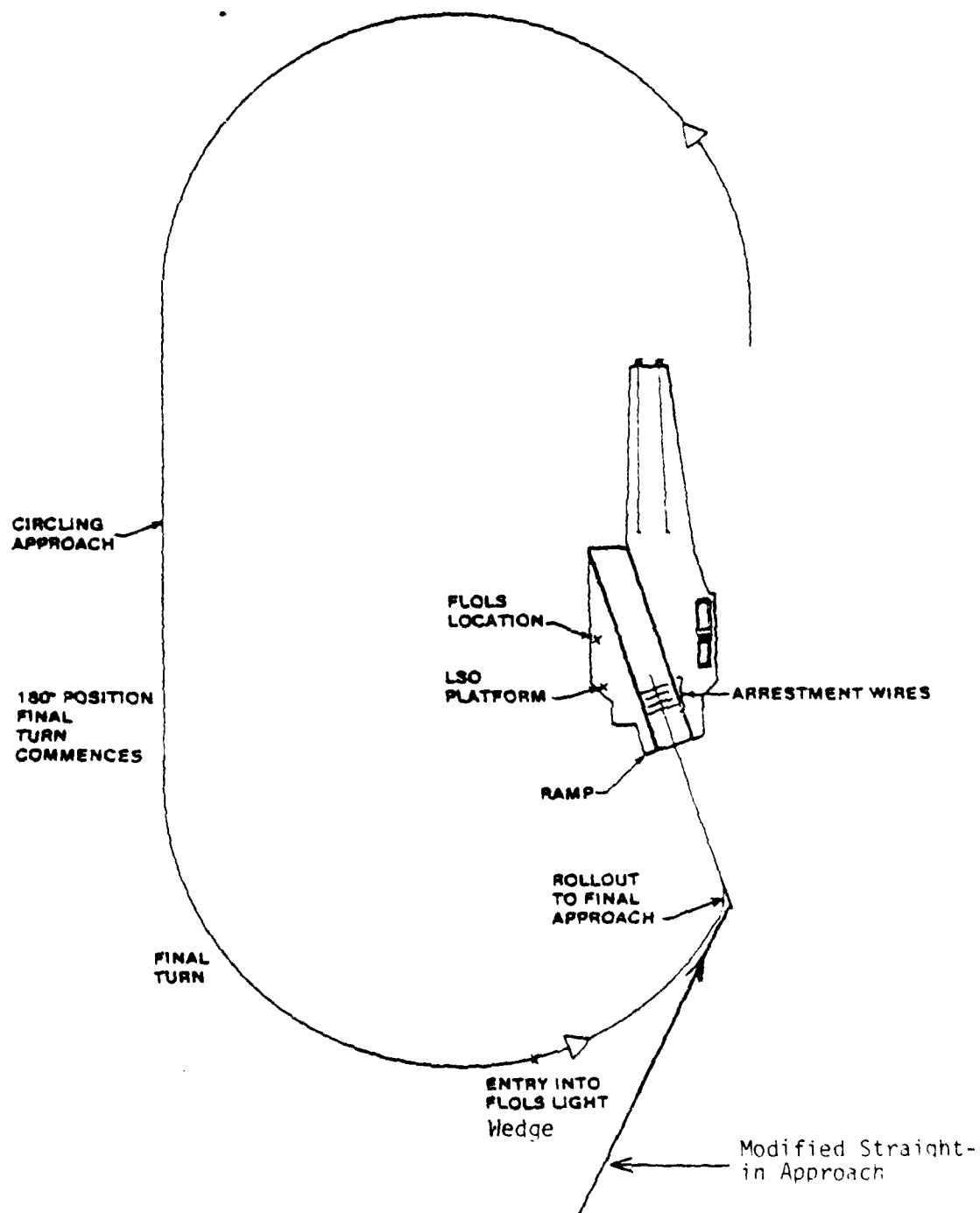


Figure 1. Overhead View of Typical Daytime Circling Carrier Landing Pattern and Night Straight-in Approaches. (not drawn to scale)

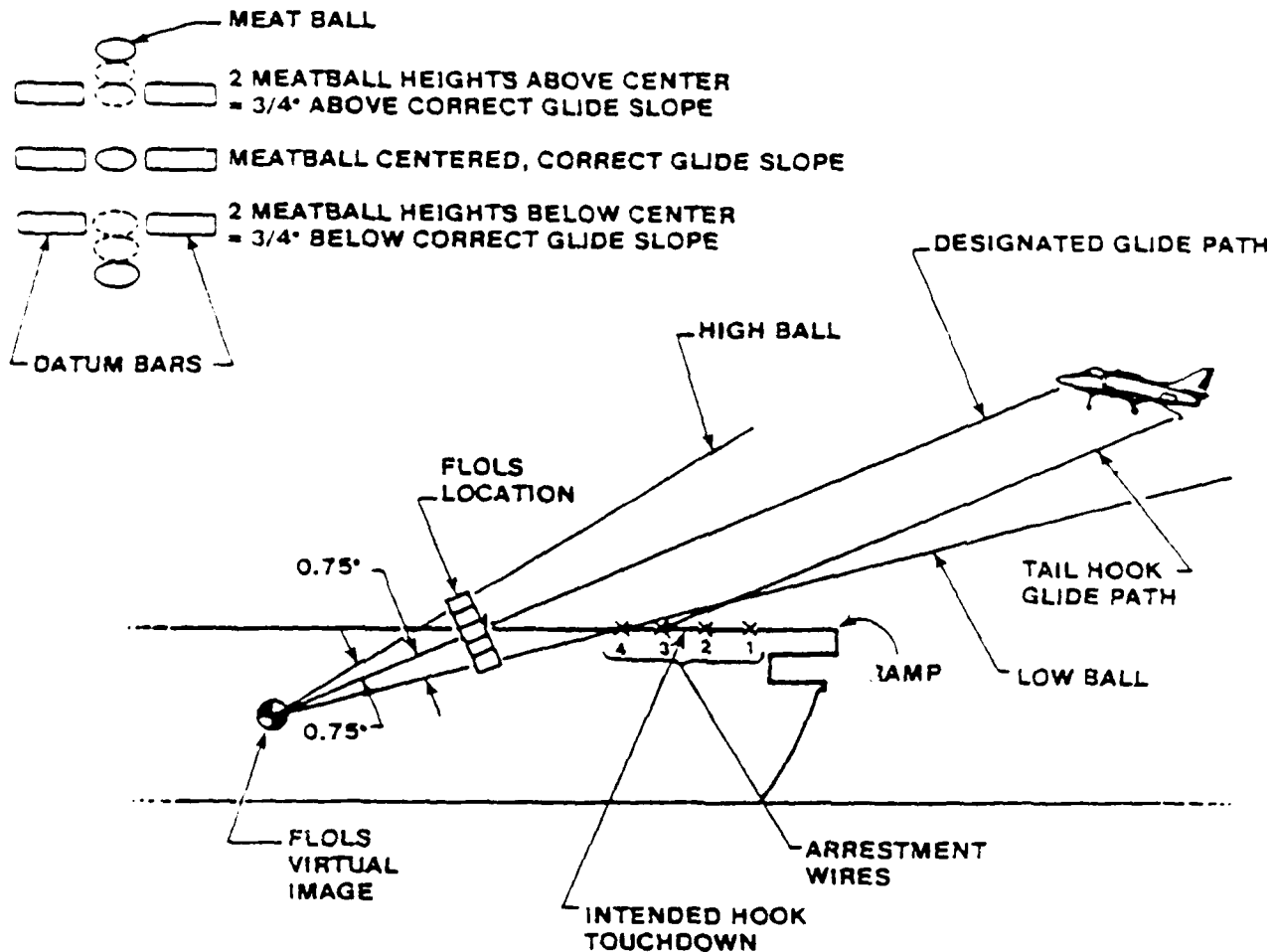


Figure 2. Carrier Approach Geometry Depicting FLOLS Projection of Glideslope Deviation Information. Adapted from Golovcsenko (1976)

The simulated FLOLS display was a TV projection of a CIG (General Electric, 1979) FLOLS data base defined by 96 edges. The two datum bars that normally consist of six lights each were represented by two solid bars. At long range the FLOLS was magnified two times normal size, gradually shrinking to 1.5 times at the ramp. The magnification was required to compensate for limited TV resolution so that the pilots could discriminate meatball position for glideslope guidance at a range similar to that in the real world. This FLOLS magnification technique is used regularly on Navy carrier landing trainers. For example, the TA-4J trainer (Device 2B35) uses magnifications as large as 7X.

FACTORS AND LEVELS

A large number of factors potentially affecting the carrier landing cues were tentatively selected as candidates for study. These were pared down by a panel of engineers and psychologists into a set of factors that were investigated earlier in performance experiments (Westra et al., 1982). Decisions regarding factors to be included in this experiment were based partly on results from the performance studies. Other considerations were the potential cost impact of factors and the potential training effects and interactions involving factors falling in the category of training aid or type.

"High" and "low" factor settings were chosen to bracket the reasonable range of interest. For the equipment factors, the high levels were generally set at the highest attainable while the low levels were chosen to be the most degraded form of the factor likely to be employed operationally. A summary of the factors involved in this experiment are given in Table 1. Other factor levels also bracket the range of interest but do not necessarily conform to "high" and "low" definitions.

FIELD OF VIEW. The high-level field of view was a 160-degree horizontal by 80-degree vertical display (Singer-Link, 1977) which is costly and is representative of that currently available for carrier landing training only on multi-task trainers such as the 2B35 and the F-14 Wide-Angle Visual System (WAVS). The low-level field of view was plus or minus 24 degrees horizontally by -27 degrees to +9 degrees vertically which is representative of the lower cost Night Carrier Landing Trainers (NCLTs) used for F-4, F-14, A-6, A-7, and S-3 transition training.

SCENE DETAIL. The high level of scene detail was represented by a daytime solid-model CIG (General Electric, 1979) carrier whose surfaces were defined by 985 edges. The daytime scene included a background with a uniform blue below a well-defined horizon. Brightness levels were approximately 2.80, 0.50, and 0.16 fL for the ship, sea and sky, respectively. This level of detail was approximately representative of that available from daytime CIG systems costing several millions of dollars, such as the 2B35 trainer, although displayed at higher resolution than available in the 2B35.

TABLE 1. SUMMARY OF EXPERIMENTAL FACTORS AND LEVELS

<u>FACTORS</u>	<u>LEVEL SETTINGS</u>	
	<u>"low"</u>	<u>"high"</u>
Field of View	-27 degrees to 9 degrees vertical, plus or minus 24 degrees horizontal	* -30 degrees to 50 degrees vertical, plus or minus 20 degrees horizontal
Scene Detail	Night: point-light ship	* Day: solid surface ship
Motion	Fixed base	* Six-degrees-of freedom
Approach Type	* Circling approach	Modified straight-in approach
FLOLS rate cuing	* Conventional FLOLS display	FLOLS display with "command" rate cuing
Turbulence ¹	Close to maximum flyable	No turbulence
Pilot type	Air Force T-38	Navy P-3C

*Indicates setting for the transfer test configuration.

¹Turbulence was set at half the "low" level setting for the transfer test.

The low level of scene detail was represented by an image of a night point-light CIG carrier consisting of 137 lights. It contained all deck outline, runway, centerline and drop lights. The background was dark with no visible horizon. This display is representative of a night CIG system costing less than a million dollars and used on several Navy NCLTs. Because the scene detail factor incorporates elements of brightness, seascape and ship detail, it was not fully crossed with field of view and approach type factors, although the experimental design treated them as fully crossed. Specifically, there was no practical difference between the wide and narrow field of view under the night scene with straight-in approaches.

COCKPIT MOTION. A six-degrees-of-freedom, 48-inch synergistic motion platform (Browder and Butrimas, 1981) was fully operational for the high level and was stationary for the low level of this factor. This platform is similar to those on the Navy's 27 T-2C Instrument Trainers (Device 2F101) used in Undergraduate Pilot Training (UPT) except that VTRS computation rates are higher for reduced cuing lag. While it is representative of many older platforms on existing trainers, it does not have the low noise and improved response of new platforms. An attempt was made to fine-tune the operational platform for optimum responsiveness for the carrier landing task by setting gains at 7.5, 2.0, and 1.2 for lateral, vertical, and pitch cuing, respectively. Roll, thrust, and yaw cuing gains were left at system supplied 1.0 gains.

APPROACH TYPE. Pilots performed their training trials with either straight-in approaches or circling approaches. For the circling approach the aircraft was positioned abeam the LSO platform at 5700 feet from the ship and at 600 feet of altitude in the approach configuration (full flaps, speed brake out, hook and wheels down, and 15 units angle of attack). Fuel was fixed at 3200 pounds to give a gross aircraft weight of 10,000 pounds. A trial consisted of the final turn, approach, and landing. The straight-in approach was started with the aircraft 11,990 feet behind the ship and 4150 feet to the left of the runway centerline. The initial altitude was 400 feet with the aircraft in the approach configuration. Figure 1 depicts the two approach types. The aircraft was trimmed for straight-and-level flight in both initial conditions.

The straight-in approach was defined specifically to provide an approach with task requirements similar to the circling approach but with the ship in view at all times under the narrow field of view condition. Thus the aircraft was set to the left of the runway centerline and headed at 18 degrees to the right of the ship heading. Pilots were instructed to fly this modified straight-in approach straight-and-level until approaching the runway centerline, then execute a turn and roll out on the runway centerline.

If these instructions were followed, a pilot would roll out for final approach at about 3/4-mile from the ship which is similar to the roll out point from a typical circling approach. Further, if the pilot remained at 400 feet of altitude during the initial part of the straight-in approach, the transition to glideslope descent would take place just prior to initiating the turn, necessitating the establishment and adjustment of a proper descent rate through the latter part of the turn as well as final approach as is the case

with circling approaches. The initial distance from the ship on the straight-in approach was adjusted such that flight times for typical circling and straight-in approaches were equal.

The approach-type factor was included in the experiment because of the evident possibility that the daytime mission can be adequately taught with (modified) straight-in approaches. This simplified procedure was supported by results reported by Collyer et al. (1980), and by preliminary observation. If this procedure should prove effective, it could have implications for the field of view question since a wide field of view is not required to keep the ship in sight during straight-in approaches.

FLOLS RATE CUING. The conventional version of the FLOLS display (see Figure 2) defined one level of this factor. The other level involved the use of vertical bars displayed with the FLOLS which presented glideslope rate of change information to the pilots. This information was presented to the pilots in "command" fashion, that is, the bar height could be interpreted directly in terms of action required to change to the desired vertical velocity. The height of the bars indicated deviation from correct vertical velocity relative to current glideslope displacement and nulled bars indicated correct vertical velocity for that glideslope position. Further information on the rate bars is given in the briefing material presented in Appendix A. The FLOLS rate cuing factor was included in the experiment on the basis of VTRS work (Kaul, Collyer, and Lintern, 1980; Lintern, Kaul, and Sheppard, 1981) which indicated a large improvement in glideslope control with rate cuing displays for experienced carrier landing pilots. The latter reference includes a description of the algorithms used to define the command rate bar operation.

TURBULENCE. Turbulence was included to allow examination of simulator factor effects under two difficulty levels. The two levels, no turbulence and the highest amount of turbulence under which operations would continue at sea, represent the range of expected real-world turbulence. The main effect of turbulence was also of some interest in this experiment as a training variable. Turbulence was generated in the form of pseudo-random "winds" composed of various sinusoidal frequencies and amplitudes acting on the longitudinal, lateral, and vertical aircraft axis. The RMS values of the winds used were 3.00, 1.25, and 3.00 ft/sec for the longitudinal, lateral, and vertical dimensions, respectively. These values produced a fairly large amount of turbulence judged by experienced carrier pilots to be near the limits for safe operation. Frequency and amplitude values used are given by Jewel et al. (1981).

PILOT TYPE. Pilots were selected from two populations without carrier landing experience. One group consisted of recent graduates of Air Force T-38 training, and the other group comprised experienced Navy P-3C pilots. The pilot group factor was included explicitly so that the expected large source of subject variance resulting from differences between the groups could be directly estimated and removed from the results.

FACTORS HELD CONSTANT. A number of factors investigated earlier (Westra et al., 1982) were held constant in this training experiment. Both CIG ship and FLOLS images were used throughout the experiment. The TV line rate was 1025 and engine computations were done at 30 Hz. Visual lag was at a system best of 100 msec, and the e-seat was off.

TRANSFER CONFIGURATION

After completing their training session, all pilots transferred to an in-simulator maximum realism version of the daytime circling carrier landing task. The initial position for the circling approach was the same as that used for circling approaches during training. A wide field of view was present along with the daytime scene, a turn on, conventional FLOLS, and an intermediate level of turbulence. RMS values for the "winds" used during training were halved for the transfer test. This level of turbulence was judged to be somewhere between small and moderate.

PILOTS

Pilots were volunteers from two populations with no carrier landing experience. One group of 16 pilots was made up of recent graduates of Air Force T-38 training. All the pilots in this group had approximately 200 hours of total flight time with about 100 hours of flight time in the T-38 in the six months prior to the experiment. The other group comprised 16 experienced Navy P-3C pilots. This group averaged 1800 hours of total flight time but varied considerably in overall experience with a standard deviation of 529 and a range of 600 to 2630 flight hours. Experience in the six months preceding the experiment averaged 329 hours, all in the P-3C, with a standard deviation of 105 and a range of 50 to 370 flight hours.

PERFORMANCE MEASURES

All of the summary measures collected and described by Westra et al., (1982) were also collected for the present experiment. However, only several basic measures that summarize final approach and landing outcome were used in analysis presented here. Percent time on target summary measures for the final approach were used rather than the more statistically desirable RMS error scores to deal with the problem of numerous "outlier" trials by the novice pilots. The time on target measure effectively limits a score and the problem of extensive data editing for outliers is reduced. An accuracy score for touchdown performance in the longitudinal dimension was used to assess landing performance. This score was essentially the inverse of absolute error from the desired touchdown point with limits for dealing with missed landings.

APPROACH SCORES. Percent time on target (TOT) summary scores were calculated for the segment 3000 ft. to the ramp. In the case of circling approaches, measurement was not started until roll out had occurred so that the measurement segment was less than 3000 ft. for some circling approaches with tight turns. The roll out point was defined as the point at which the aircraft heading had crossed the ship's heading and aircraft bank was within four degrees of level. Measures of three variables describing aircraft

position relative to the defined optimum flight path were used in computing the TOT scores:

- Vertical deviations in degrees from the specified glideslope;
- Horizontal deviations in degrees from the center of the landing deck (lineup); and
- Angular deviations, in units from the optimum angle of attack.

Based on recommendations by Navy Landing Signal Officers (LSOs), tolerance bands describing operationally desired performance were set at plus or minus 0.3 degree, plus or minus 1.0 degree, and plus or minus 1.0 unit for glideslope, lineup, and angle of attack, respectively. The tolerance for glideslope represents approximately plus or minus one "meatball" of deviation of the FLOLS display. The percent TOT scores are referred to as glideslope, lineup, and angle of attack tracking scores in the discussions that follow.

TOUCHDOWN WIRE ACCURACY SCORE. A score reflecting touchdown wire (longitudinal) accuracy was created for assessing touchdown quality. This score is similar to Britton's (1973) Landing Performance Score (LPS), but it is based on absolute deviation from the desired touchdown point rather than wire catch per se. This gives the score better statistical properties and more appropriately assesses quality for number one wire traps and bolters. With the LPS, all one-wire catches get the same score while a considerably different wire accuracy score can be achieved depending on whether the touchdown is well short of the one-wire or very near the one-wire.

The algorithm used to create the wire accuracy score is depicted in Figure 3. The score is essentially based on the wires with the distance

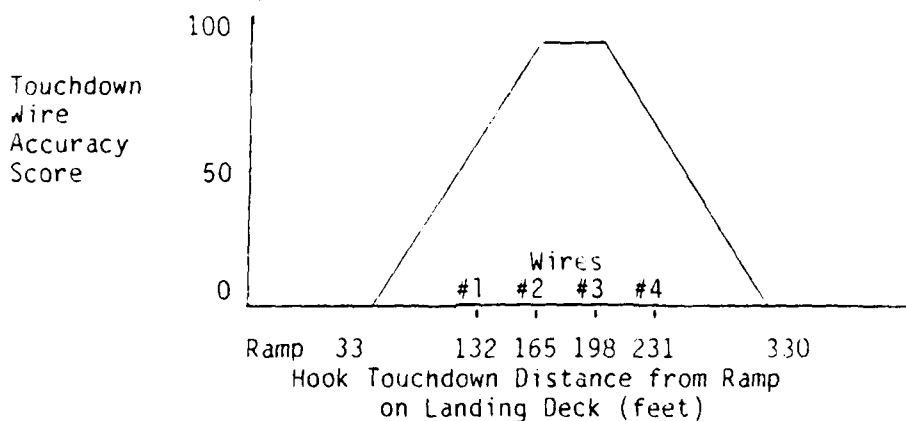


Figure 3. Scoring Algorithm for Touchdown Longitudinal Accuracy

between the wires used as a "unit of measure." A three wire catch results in a "perfect" score of 100 while a touchdown three wire-distance-units or more past the four wire or three wire distance units or more behind the one wire results in a score of zero. Beyond these limits a landing is in unsafe, near-crash conditions, and would almost certainly have been waved off had an LSO been in control of the flight. Touchdowns more than one wire-distance-unit behind the one wire or more than one wire-distance-unit beyond the four wire would also often have been waved off at sea. However, differential scoring in the "marginal" landing areas was helpful for assessment purposes in this experiment in which no waveoffs were given, and pilots were instructed to attempt a landing unless a crash were imminent.

COVARIATE TASK

Data were collected from all subjects on an ATARI video game called Air Combat Maneuvering (ACM) which is commercially available (Combat Tape CX2601, Game No. 24, difficulty "b", right controller). ACM is a game involving two moving aircraft displayed on a television screen. One of the aircraft is controlled by the subject with a control stick to turn right or left and to speed up or slow down. The other aircraft is the target which tracks on a constant heading after the initial heading is randomly selected following each "hit." The subject also controls a trigger for a missile on his aircraft which is aligned with the aircraft's longitudinal axis and fired at the target aircraft. The missile heading can also be controlled by the subject to a limited degree after it leaves the aircraft. A hit is scored when the target aircraft is struck by a missile from the subject's aircraft. A subject's score for one game is the total number of hits during a 2.25-minute trial period.

It was hypothesized that scores on ACM would predict carrier approach glideslope tracking ability since the game involves reaction time, aiming, and tracking skills that are basic to carrier landing success. Kennedy, Bittner and Jones (1981) reported a strong relationship between terminal performance on ACM and terminal performance on a compensatory tracking task ($r=.78$). Further, other research with this game suggested that it had a number of characteristics desirable in a covariate (Jones, Kennedy and Bittner, 1981). The game is easy to administer, low in cost, easily transportable, and is high in intrinsic reward value. But more importantly, performance stabilizes early (correlations over time are constant after a short acquisition period) with high "task definition" (correlations among stabilized trials are high, $r = .9$).

PROCEDURES

Each pilot's experiment sequence consisted of 40 training trials and 16 transfer trials. Before flying any trials, pilots were given approximately 1.5 hours of instruction in carrier landing procedures in the form of a briefing and an instructional videotape. They then flew two three-minute familiarization flights in the simulator before commencing with the experimental training trials. Instructional feedback was given after each training trial by a VTRS staff member and an automatic LSO (to be described)

was used throughout the experiment to give calls during the flights. Instructional feedback was not given during the transfer test trials.

BRIEFING. Pilots were given a pamphlet containing basic information on carrier landing procedures as well as any additional information necessary for a pilot's particular training condition. For example, pilots who were assigned to a training condition with rate bars on the FLOLS received additional information regarding the function and use of the rate bars. The complete set of briefing material is given in Appendix A. Pilots retained their briefing material throughout the experiment and were encouraged to refer to the material at any time while not engaged in flying.

The preliminary briefing itself consisted of a verbal "walk through" of the pamphlet with emphasis on the key procedures. This briefing was conducted by a member of the VTRS research staff. Members of the VTRS research staff authored the briefing pamphlet with assistance from several naval aviators and LSOs. Material for the pamphlet was drawn from Navy carrier landing instruction manuals and documents including Landing Signal Officer (1975), FCLP Pattern (1977), Carrier Qualification Procedures (1977), and Flight Training Instruction (1979).

INSTRUCTIONAL VIDEOTAPE. Following the verbal briefing, pilots were shown a portion of a Navy film describing key carrier landing procedures which is also shown to undergraduate naval aviators during their training. This film entitled "Carrier Landing Mishaps" is available through the Navy film library.

FAMILIARIZATION FLIGHTS. A briefing on the T-2C cockpit and controls was given prior to the first flight. Pilots were then given two three-minute familiarization flights prior to their experimental training trials. Initial aircraft positions were set in accordance with the training condition to which the individual pilots had been assigned. However, all familiarization flights were flown with no turbulence, no motion, and a wide field of view.

Pilots were instructed to fly to the carrier from their initial positions, attempt to locate the FLOLS, start a descent, and then fly over the carrier without attempting to land. They were then instructed to make a left turn and circle for another approach after flying over the carrier and to continue for three minutes. Pilots were told to "exercise" the controls and to try to familiarize themselves with the cockpit and controls as much as possible during the preliminary flights. If a crash occurred, the simulator was reset to the appropriate initial position, and elapsed time was subtracted from remaining time so that all pilots had exactly six minutes of familiarization flight.

AUTOMATIC LSO. An automatic LSO, giving calls via a VOTRAX voice generation system, was used for all trials in the experiment. The automatic LSO software was developed from a modified version of the model described by Borden and McCauley (1978). A restricted set of calls from the complete software program was used to avoid overloading the trainees with information. All calls pertaining to lineup were eliminated from the LSO repertoire. Only a set of basic glideslope and angle of attack calls was retained, and although angle

of attack calls were available, they were rarely given because of low priority in the program logic. Implicit in this use of the automatic LSO was an emphasis on glideslope control during the experiment.

Additionally, no waveoffs were given, and pilots were instructed to try to land on every approach, aborting only if a crash were imminent. If a pilot exceeded the automatic LSO's waveoff criterion limits during an approach, the LSO simply shut off until such time as the pilot got back within waveoff limits, unless the approach were within 2000 feet of the ramp, in which case the LSO stayed off.

INSTRUCTIONAL FEEDBACK. Instructional feedback was given after every training trial. This feedback was given by two members of the VTRS staff familiar with the carrier landing task. The "instructors" attempted to give feedback that was as uniform as possible, and instructors rotated every 10 or 20 trials such that any instructor effect was balanced across pilots. The feedback instruction philosophy was to identify the major problem or error that occurred during a trial and then advise as to the proper error correction procedure.

The instructors received preliminary training from a Navy LSO and drew heavily from the Navy publications referenced earlier for descriptions of error correction procedures. Emphasis was placed on glideslope correction procedures more or less "by the book" except that use of the throttle for glideslope tracking was heavily emphasized to overcome pilot tendencies to track the glideslope with elevator (nose) control. References to lineup and angle of attack problems were rarely made except in a few cases in which severe problems were noted.

Pilots flying circling approaches during training were also given feedback on turn parameters including bank angle control, angle of attack control, overshoot, undershoot, long in the turn, short in the turn, and altitude control. Pilots flying modified straight-in training approaches were given feedback regarding their control during the initial straight-and-level portion and their roll out for lineup. Instructors were aided considerably in their ability to give feedback by a CRT graphical display that provided plots of glideslope deviation, lineup deviation, angle of attack deviation, vertical velocity, throttle position and aircraft pitch for the final approach.

SCHEDULING. Pilots performed the experiment in pairs, starting on either a Monday or a Wednesday. Pairs starting on Monday flew 20 training trials each Monday, 20 on Tuesday, and then 16 transfer trials on Wednesday. Pairs starting on Wednesday flew 10 training trials on Wednesday, 20 training trials on Thursday, and 10 training trials and 16 transfer trials on Friday. A pilot performed ten consecutive training trials in a single session and then alternated with the other pilot in his pair until the day's schedule was complete. A single training session took about 45 minutes to complete. Transfer trials were flown in similar alternating sessions except that there were eight trials per session rather than ten.

Data on the ATARI Air Combat Maneuvering (ACM) video game were collected in sessions of ten trials each from a pilot when the other pilot in his pair was flying the simulator. The first 16 pilots in the experiment performed 50 2.25-minute ACM trials while the second 16 performed 30 trials after evidence from the first 16 pilots indicated 30 trials were sufficient for covariate use of the data.

EXPERIMENTAL DESIGN

A transfer of training paradigm was superimposed on the basic experimental design which was an adaptation of National Bureau of Standards (1957) Plan 4.7.16. Each pilot performed 40 training trials under one of the conditions of the basic design followed by 16 transfer trials in the simulator on the high fidelity transfer configuration. The basic design was $1/4$ of a fully crossed 2^7 design resulting in 32 experimental conditions. The defining generators and generalized contrast that define the experimental conditions and alias structure (see Box and Hunter, 1961; Davies, 1967; Simon, 1973; 1977b) are: $I = ABCEG = ABDF = CDEFG$, where the factors field of view, motion, pilot type, scene detail, turbulence, FLOLS rate cuing, and approach type are associated with labels A, B, C, D, E, F, and G, respectively.

All main effects are confounded only with three-way or higher interactions as are 15 of the 21 two-way interactions. The other six two-way interactions are confounded in strings of two each and the remaining six estimable terms in the basic design represent strings of three-way interactions. As the basic design was repeated across trials, trial effects are also fully estimable. The two-way interactions judged least likely to be important *a priori* were assigned to the strings of confounded two-factor interactions. Implicit in the use of this fractional factorial design is the assumption that three-way and higher-order interactions will generally be negligible. Each of the estimable effects is also confounded with the subject effects defined by the groupings involved in a contrast, and for this reason a covariate task was included in the experiment.

SECTION III

RESULTS

Figures 4 through 7 graphically describe the experimental results for main effects. The numbers in parentheses above a block of trials for the effects represent the results of an analysis of variance for that block of trials. The numbers are the ranks of the sizes of the effects (within the 31 estimable effects) of the basic design. Examination of Figure 4, for example, indicates that in the first block of training trials turbulence had the largest effect, approach type ranked second, motion ranked tenth, etc. An effect can rank high by chance alone, of course, and such things as stability over time and effect size must also be taken into account when judging the meaningfulness and reliability of an effect. On the other hand, if an effect size does not rank very high, it probably is neither meaningful nor real since it cannot be differentiated from noise.

Tables 2 through 5 present analysis of variance summaries of transfer test trials for performance scores shown in Figure 4 through 7. These analyses estimate effects averaged across all 16 transfer trials, represented by two blocks of eight trial means. Main effects were tested separately as were the field of view by approach type and scene detail by approach type interactions which were considered important on a priori grounds. Graphical results for these two interactions are given in Figures 8 and 9. Analysis of variance summaries for training trials are presented in Appendix B and are generally not discussed. Results for the covariate task and the implications of these results are presented and discussed separately.

The basic "residual" term for an analysis of variance was created from the sum of the two- and three-way string terms. The sum of two-way interactions not involving pilot type and the sum of the two-way interactions involving pilot type were tested against the basic residual term. If these effects were not significant, they were combined with the basic residual term to form a residual estimate against which all other effects were tested. Thus, for example, the F-ratios shown for the single-degree-of-freedom effects in Table 2 have a 22-degree-of-freedom denominator whose sums of squares is from all the indicated sources. There is an exception to this in Table 5 for angle of attack tracking scores in which the combined two-factor interaction term not involving pilots was significant. In this case there were two fairly large interactions involving turbulence, but because of problems in the interpretation of turbulence effects, the whole term was added to the residual.

The entire transfer-of-training design may be thought of as a special case of a repeated measures design (Winer, 1971, ch. 7) with observations repeated on a trials factor. In this sense, the residual estimate represents an estimate of subjects within groups (between subjects) error. Trial blocks were tested against a term comprised of all estimable three-way and higher interactions involving blocks which is then an estimate of error within subjects. Two-factor interactions involving blocks were tested omnibus fashion against this same term. Estimable two-factor interactions involving trials were also examined individually, but in the absence of strong evidence

TOUCHDOWN ACCURACY SCORE (SEE FIGURE 3)

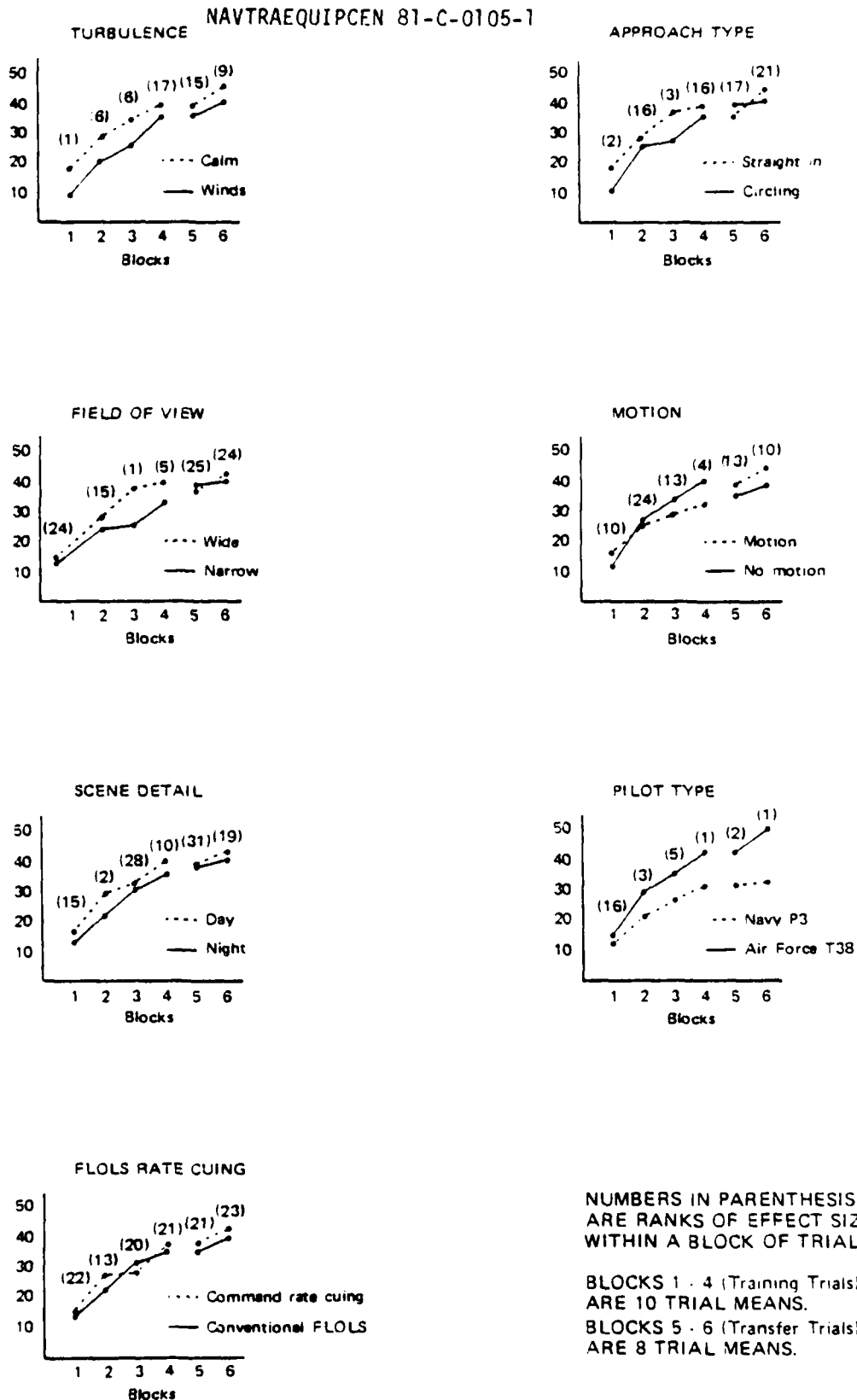
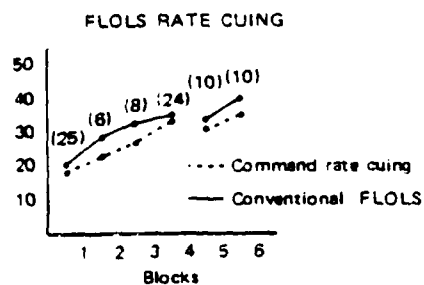
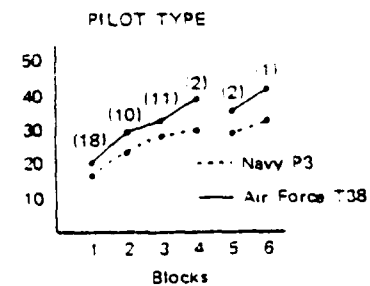
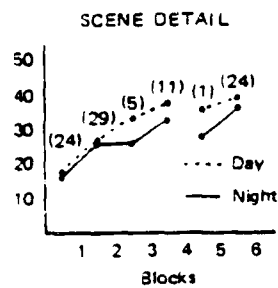
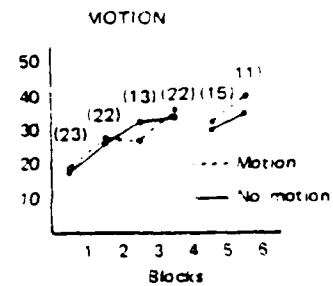
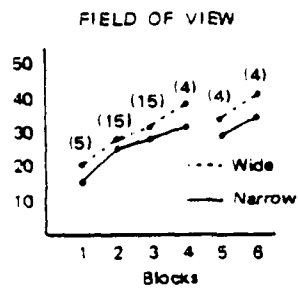
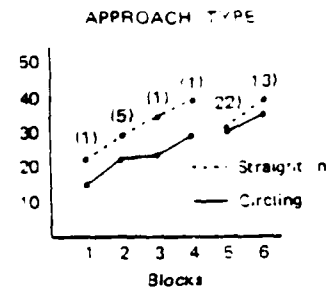
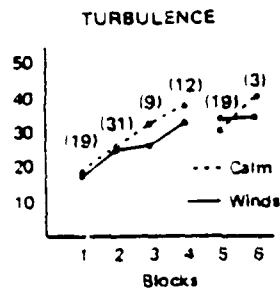


Figure 4. Main Effects for Touchdown Wire Accuracy Score.

PERCENT OF TIME WITHIN OPERATIONALLY DESIRED CRITERION



NUMBERS IN PARENTHESIS
ARE RANKS OF EFFECT SIZES
WITHIN A BLOCK OF TRIALS.

BLOCKS 1 - 4 (Training Trials)
ARE 10 TRIAL MEANS.

BLOCKS 5 - 6 (Transfer Trials)
ARE 8 TRIAL MEANS.

Figure 5. Main Effects for Glideslope Tracking Score:
Percent Time $\pm 0.3^\circ$ of Desired for 3000 Feet to Ramp.

PERCENT OF TIME WITHIN OPERATIONALLY DESIRED CRITERION

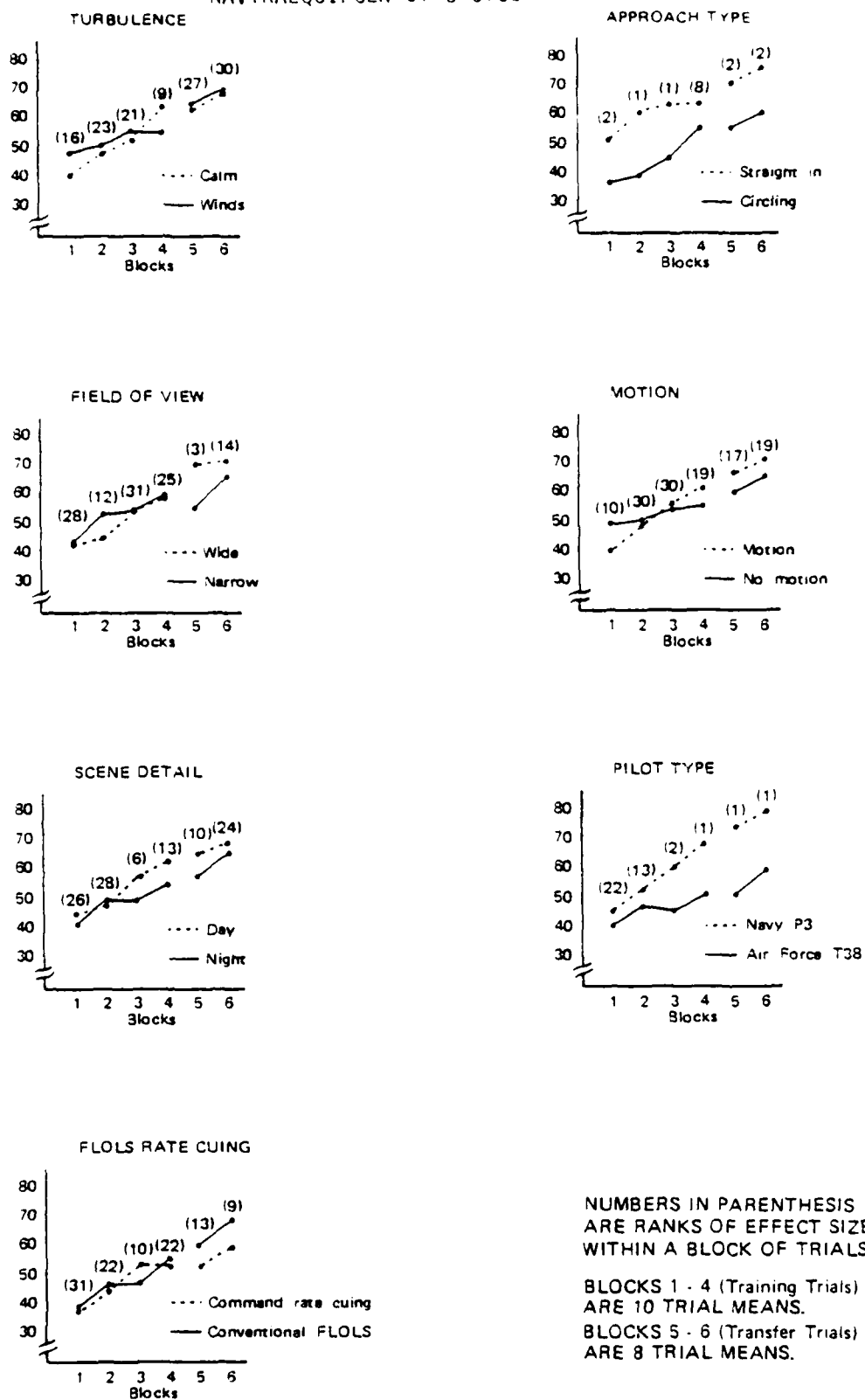
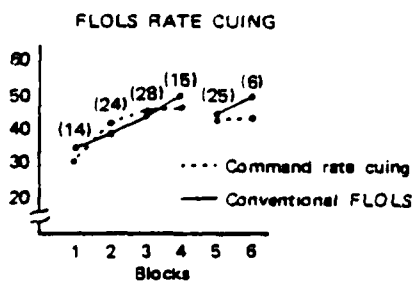
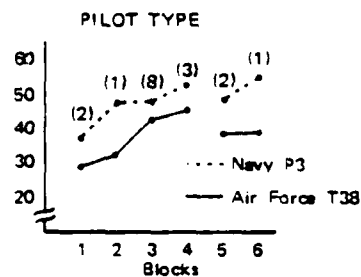
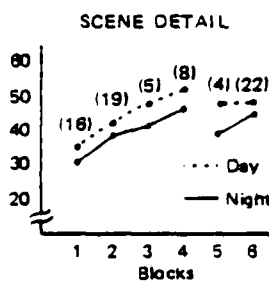
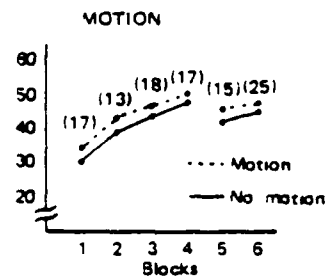
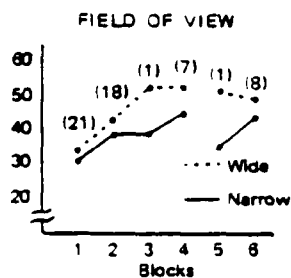
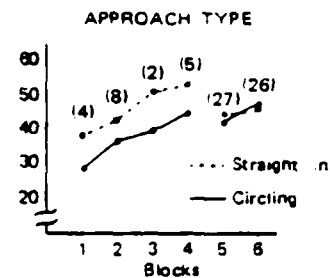
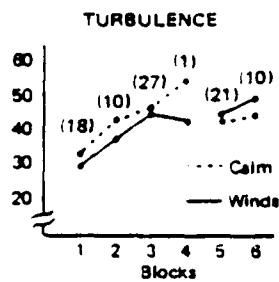


Figure 6. Main Effects for Lineup Tracking Score:
Percent Time $\pm 1.0^\circ$ of Desired for 3000 Feet to Ramp.

PERCENT OF TIME WITHIN OPERATIONALLY DESIRED CRITERION



NUMBERS IN PARENTHESIS
ARE RANKS OF EFFECT SIZES
WITHIN A BLOCK OF TRIALS.

BLOCKS 1 - 4 (Training Trials)
ARE 10 TRIAL MEANS.

BLOCKS 5 - 6 (Transfer Trials)
ARE 8 TRIAL MEANS.

Figure 7. Main Effects for Angle of Attack Tracking Score:
Percent Time ± 1.0 un. of Desired for 3000 Feet to Ramp.

NAVTRAEQUIPCEN 81-C-0105-1

TABLE 2. ANALYSIS OF VARIANCE FOR TOUCHDOWN
WIRE ACCURACY TRANSFER SCORES

Source of Variance	LEVELS		df	Mean		F
	High	Low		Difference ¹		
Field of View	Wide	Nar	1	0.4	(-) ²	0.01
Scene Detail	Day	Night	1	1.6	(-)	0.13
Motion	On	Off	1	4.8	(2.5)	1.60
Approach Type	St. In	Circ	1	0.0	(-)	0.00
FLOLS Rate Cue	Cuing	No Cue	1	2.3	(-)	0.36
Turbulence	Calm	Winds	1	5.0	(2.7)	1.75
Pilot Type	Nav P-3C	AF T38	1	-13.3	(19.1)	12.18**
FOV x App. Type			1		(1.0)	0.61
S.DTL x App. Type			1		(-)	0.43
2-Factor Int (No Pil) ³			7)Re-		(7.0)	0.44
2-Factor Int (Pil) ⁴			6)sid-		(7.4)	0.55
2+3 Way Strings			9)ual		(20.2)	
Blocks (8 Trials)			1	4.9	(2.6)	1.84
2-Factor Int (Blocks)			7		(2.4)	0.24
3-Factor Int (Blocks)			24		(33.8)	
(within subject residual)						
Grand Mean				39.5		
Std. Err. Difference				3.8		
Std. Deviation				10.8		

¹Mean of observations taken under high level minus mean of observations taken under low level of factor.

²Values in parentheses are percent variance accounted for in the experiment. Percents less than 1.0 are shown by a dash (-).

³Two-factor interactions not involving pilot type.

⁴Two-factor interactions involving pilot type.

*p < .05

**p < .01

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TABLE 3. ANALYSIS OF VARIANCE FOR GLIDESLOPE TRACKING TRANSFER SCORES

Source of Variance	LEVELS		df	Mean		F
	High	Low		Difference ¹		
Field of View	Wide	Nar	1	6.3	(8.4) ²	6.42*
Scene Detail	Day	Night	1	4.6	(4.6)	3.51
Motion	On	Off	1	3.2	(2.2)	1.68
Approach Type	St. In	Circ	1	2.5	(1.4)	1.07
FLOLS Rate Cue	Cuing	No Cue	1	-3.9	(3.3)	2.52
Turbulence	Calm	Winds	1	2.8	(1.6)	1.22
Pilot Type	Nav P-3C	AF T38	1	-7.6	(12.3)	9.40**
FOV x App. Type			1		(-)	0.46
S.DTL x App. Type			1		(-)	0.46
2-Factor Int (No Pil) ³			7)Re-		(9.7)	1.13
2-Factor Int (Pil) ⁴			6)sid-		(8.1)	1.10
2+3 Way Strings			9)ual		(11.0)	
Blocks (8 Trials)			1	5.9	(7.5)	8.42**
2-Factor Int (Blocks)			7		(7.4)	1.18
3-Factor Int (Blocks)			24		(21.4)	
(within subject residual)						
Grand Mean				34.8		
Std. Err. Difference				2.3		
Std. Deviation				6.5		

¹Mean of observations taken under high level minus mean of observations taken under low level of factor.

²Values in parentheses are percent variance accounted for in the experiment. Percents less than 1.0 are shown by a dash (-).

³Two-factor interactions not involving pilot type.

⁴Two-factor interactions involving pilot type.

*p < .05

**p < .01

TABLE 4. ANALYSIS OF VARIANCE FOR LINEUP
TRACKING TRANSFER SCORES

Source of Variance	LEVELS		df	Mean		F
	High	Low		Difference ¹		
Field of View	Wide	Nar	1	9.9	(5.0) ²	3.03
Scene Detail	Day	Night	1	5.4	(1.4)	0.88
Motion	On	Off	1	4.8	(1.2)	0.71
Approach Type	St. In	Circ	1	15.3	(11.7)	7.14*
FLOLS Rate Cue	Cuing	No Cue	1	-7.5	(2.8)	1.70
Turbulence	Calm	Winds	1	-1.4	(-)	0.05
Pilot Type	Nav P-3C	AF T38	1	21.5	(23.1)	14.08**
FOV x App. Type			1		(-)	0.14
S.DTL x App. Type			1		(1.1)	0.68
2-Factor Int (No Pil) ³			7)Re-		(9.8)	1.03
2-Factor Int (Pil) ⁴			6)sid-		(14.1)	1.72
2+3 Way Strings			9)ual		(12.2)	
Blocks (8 Trials)			1		(1.7)	2.96
2-Factor Int (Blocks)			7		(1.7)	0.42
3-Factor Int (Blocks)			24		(14.0)	
(within subject residual)						
Grand Mean				64.7		
Std. Err. Difference				5.6		
Std. Deviation				15.9		

¹Mean of observations taken under high level minus mean of observations taken under low level of factor.

²Values in parentheses are percent variance accounted for in the experiment. Percents less than 1.0 are shown by a dash (-).

³Two-factor interactions not involving pilot type.

⁴Two-factor interactions involving pilot type.

*p ≤ .05

**p ≤ .01

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TABLE 5. ANALYSIS OF VARIANCE FOR ANGLE OF ATTACK
TRACKING TRANSFER SCORES

<u>Source of Variance</u>	LEVELS		<u>df</u>	Mean		<u>F</u>
	<u>High</u>	<u>Low</u>		<u>Difference</u> ¹		
Field of View	Wide	Nar	1	10.8	(13.0) ²	8.09**
Scene Detail	Day	Night	1	5.4	(3.3)	2.04
Motion	On	Off	1	2.0	(-)	0.28
Approach Type	St. In	Circ	1	-0.1	(-)	0.00
FLOLS Rate Cue	Cuing	No Cue	1	-4.4	(2.2)	1.36
Turbulence	Calm	Winds	1	-3.3	(1.2)	0.76
Pilot Type	Nav P-3C	AF T38	1	13.0	(18.8)	11.75**
FOV x App. Type			1		(2.5)	1.57
S.DTL x App. Type			1		(-)	0.15
2-Factor Int (No Pil) ³			7)Re-		(20.2)	3.08
2-Factor Int (Pil) ⁴			6)sid-		(6.6)	1.18
2+3 Way Strings			9)ual		(8.4)	
Blocks (8 Trials)			1	2.9	(-)	1.39
2-Factor Int (Blocks)			7		(6.9)	1.53
3-Factor Int (Blocks)			24		(15.4)	
Grand Mean				45.1		
Std. Err. Difference				3.8		
Std. Deviation				10.8		

¹Mean of observations taken under high level minus mean of observations taken under low level of factor.

²Values in parentheses are percent variance accounted for in the experiment. Percents less than 1.0 are shown by a dash (-).

³Two-factor interactions not involving pilot type.

⁴Two-factor interactions involving pilot type.

*p < .05

**p < .01

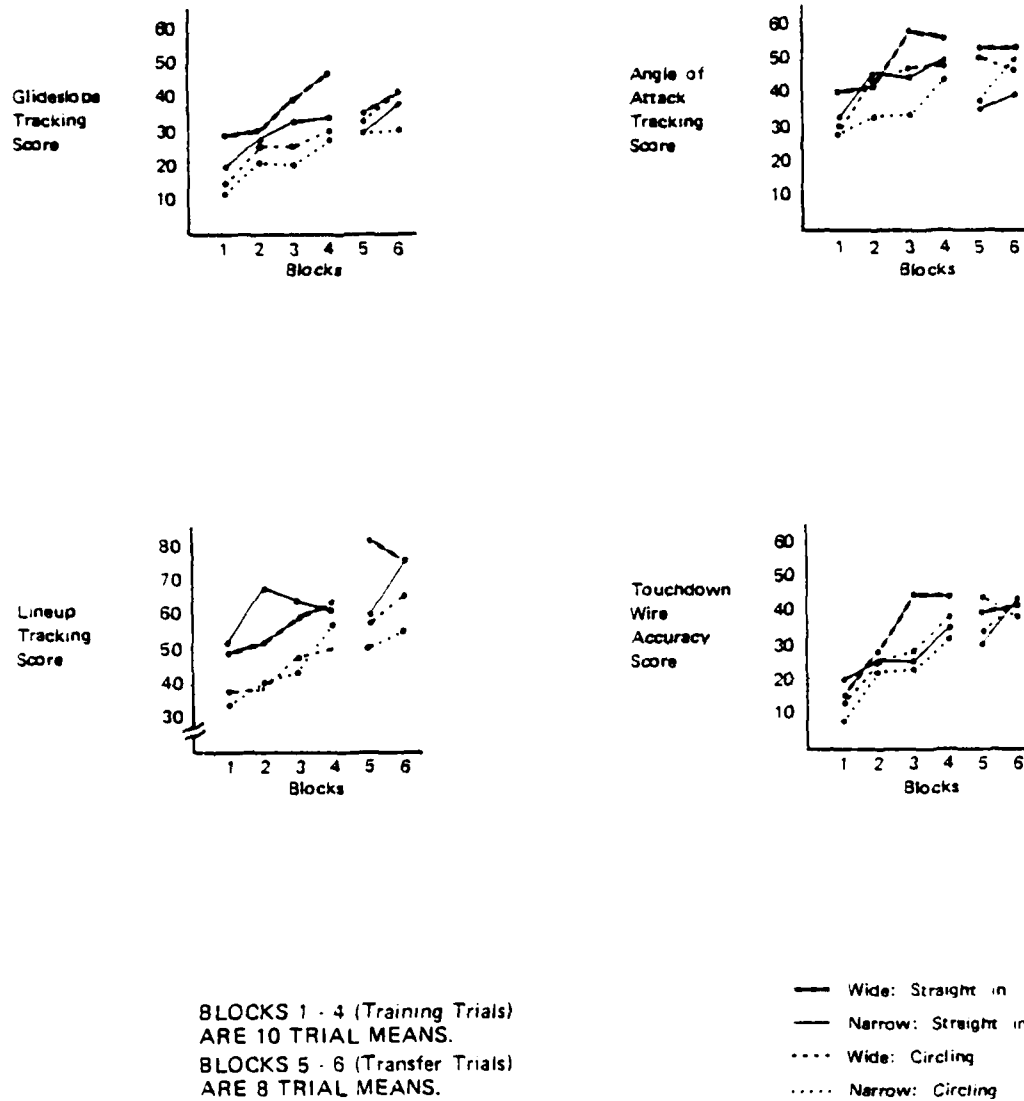


Figure 8. Results for Field of View by Approach Type.

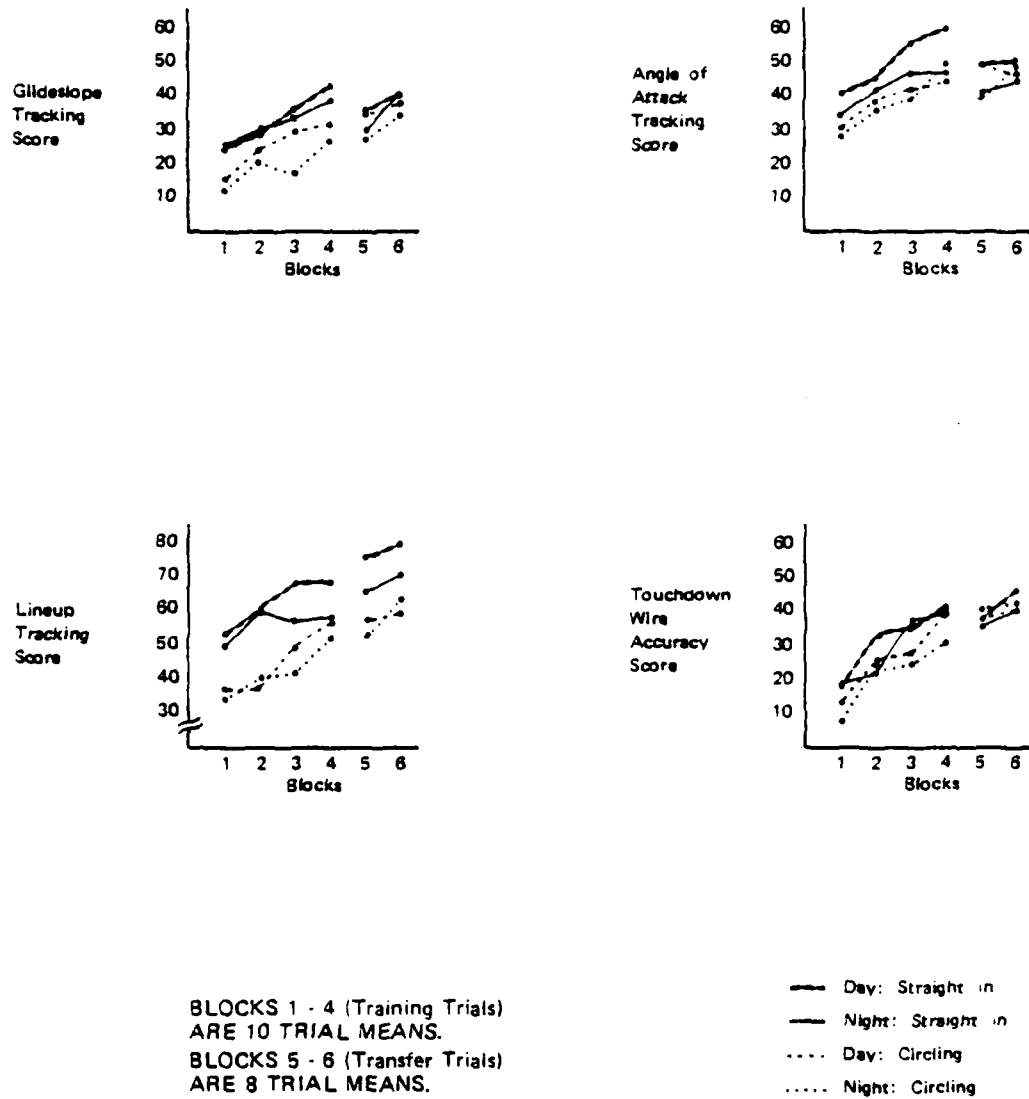


Figure 9. Results for Scene Detail by Approach Type.

for an effect, they were not partialled out of the combined terms and are not discussed.

OTHER INFORMATION. There are certain computations that can be performed to obtain supplementary information with which to interpret the data. The numbers required for these analyses are available in the tables.

The mean performance for high and low levels of any factor can be obtained by taking the grand mean shown at the bottom of each column and to it add (high level) or subtract (low level) half of the mean difference for that factor. The sign of the mean difference must be taken into consideration in this calculation since the mean of the low condition was always subtracted from the mean of the high condition to obtain the mean difference. A positive percent time-on-target mean difference indicates better performance with the factor's higher level.

Confidence limits for the mean differences can be roughly obtained by multiplying the standard error of the mean difference—"STD ERR DIFF" at the bottom of the table—by plus or minus two for the 95 percent level and plus or minus 2.6 for the 99 percent level. These values can be added to the mean differences of each factor to obtain the low and high limits within which the true mean difference is expected to lie. Standard deviations are based on subject within group error estimates and standard errors of differences are based on 16 subjects per group.

F-ratios for a particular effect can be calculated using the percentages in the table. The numerator of the ratio is the percent variance accounted for by an effect divided by its degrees-of-freedom, and the denominator is the residual percent variance accounted for divided by its degrees-of-freedom. Significance is indicated at .05 and .01 levels. For these results it is suggested that .01 is an appropriate level of significance providing some compensation for the multiple tests and measures involved. On the other hand, because of the small number of degrees-of-freedom available for "error" estimation, only large effects are likely to show up as significant at the .01 level.

INTERPRETATION STANDARDS. That an observed difference between two conditions is or is not statistically significant provides little information regarding the practical significance of the difference. Some outside "real-world" standards are needed to evaluate the data. The TOT scores are of some value themselves in this regard since they can be interpreted directly in terms of percent time within "real-world" defined levels of good performance. Scores of 100 for an approach represent "perfect" performance from an operational point of view, while scores of zero represent completely unacceptable performance. But this still gives no clues as to what constitutes a "meaningful" difference.

One standard against which to evaluate the magnitude of differences can be based on the values obtained by pilots with different degrees and/or kinds of experience. Based on data obtained from this experiment and other VTRS experiments (Westra et al., 1982), the following rough estimates of

performance for beginning, intermediate, and experienced levels of performance can be used:

	Beginner (Trials 11-20)	Intermediate (Trials 49-56)	Experienced
Glideslope TOT	26	38	59
Lineup TOT	49	68	N/A
Angle of Attack TOT	33	47	48
Wire Accuracy Score	26	42	66

The differences between scores representing these levels of ability would generally be considered "moderate" to "large" in size. Another guideline suggested by Cohen (1977) is to consider a difference that accounts for a true 5 percent of the variance in the population of factors and subjects (pilots) as an effect of "moderate" size. Based on estimates from data obtained by Westra et al. (1982), differences that correspond roughly to this size are 12.0, 10.5, and 17.0 for the glideslope TOT, AOA TOT, and wire accuracy score, respectively. Effect sizes corresponding to Cohen's estimate of small effects (1 percent variance accounted for in the population) are 5.4, 4.7, and 7.6 for the game scores.

GENERAL DISCUSSION OF RESULTS

The data indicate a few substantial, though transient, transfer effects for the simulator factors. Although there were no equipment factor transfer effects on touchdown performance (Figure 4 and Table 2), field of view and scene detail appear to have temporary effects on final approach tracking measures. However, these transfer effects have generally disappeared after eight transfer trials.

The pilot type effect during transfer was considerably larger than equipment factor effects (as it was during training), even though the direction of the effect depended on the dimension of performance measured. The Navy P-3C pilots performed much better on the lineup and angle of attack aspects of the final approach task, while the Air Force T-38 pilots performed better on final approach glideslope control and landing. The reversal of differences notwithstanding, this single-degree-of-freedom representation of pilot differences based on group type generally accounted for more variance than all simulator main effects combined. Despite the large pilot group effects, pilot groups by equipment factor two-way interactions were generally small. This indicates that equipment factor effects, such as they were, were consistent across groups.

Turbulence, which was included in the experiment primarily to allow examination of the other factors' transfer effects under two training difficulty levels, had little or no transfer main effect. Since there does not appear to be strong evidence of a turbulence training effect either, the

interpretation of this factor relative to its intended purpose is in question. Therefore, for analysis purposes, interactions involving turbulence were for the most part considered "error" and treated as such.

DISCUSSION OF INDIVIDUAL FACTORS

FIELD OF VIEW. Differences in the field of view during training had no effect on transfer landing performance. There did appear to be a transfer advantage for the wide field of view for final approach measures. Those pilots trained on the wide field of view averaged 6.3 percent more time within .3 degree of the desired glideslope. The effect continued through the second set of eight transfer trials and accounted for 8.4 percent of the variance in all transfer scores on this measure. However, the effect is not large, and significance at the .01 level was not reached. In addition, an analysis (not reported here) using the root mean square (RMS) glideslope error as the dependent measure failed to support the glideslope TOT finding. As RMS error and TOT are closely related, there is considerable question about the reliability of the effect. Further, since this tracking effect on final approach glideslope was not reflected in the touchdown wire accuracy score, it probably should not be considered anything more than a small effect. The field of view did appear to have substantial transient effects on final approach lineup and angle of attack transfer performance.

These effects seem to depend on approach type as illustrated in Figure 8. The transient lineup effect is associated with straight-in approaches, while a small but persistent transfer lineup advantage for the wide field of view is noted with circling approaches. For angle of attack tracking scores the nature of the transient effect depending on approach type is reversed. The transient effect is seen for circling training approaches but a persistent transfer advantage for the wide field of view is noted for straight-in training approaches. It should be noted that power to examine individual interaction cell means is weak in the experiment, and the interactions themselves were not significant. Therefore, results interpreted by approach type should be considered only suggestive at this point.

SCENE DETAIL. Scene detail had no transfer effect on the touchdown wire accuracy score. It did appear to have some transient effects on final approach performance. The daytime scene resulted in 8.0 percent more time within 0.3 degree of glideslope for the first block of transfer trials, but this effect was gone by the second block of transfer trials. Similar results can be seen for final approach angle of attack and lineup tracking scores but for the lineup measure, results depend on training approach type as shown in Figure 9. There is a sustained transfer advantage for the daytime scene for those trained with straight-in approaches. For those trained with circling approaches there is no transfer advantage, temporary or otherwise, for the daytime scene. The effects on lineup and angle of attack tracking scores should also be considered only suggestive at this point.

MOTION. Use of a motion platform during training provided no meaningful transfer advantage over the no-motion condition. This was true even though the physical simulation of motion was exactly the same in the transfer test as

in the training conditions. Considering this when attempting to generalize the motion result to the aircraft leads to the strong notion that simulator cockpit motion will provide no transfer benefit. In the real task scenario motion cues must be less than identical with the simulated motion cues because of the obvious physical limitations of a fixed based motion platform. Even allowing for the possibility that subjective similarity has been or could be achieved, there seems little possibility for a positive motion transfer effect for the carrier landing task.

FLOLS RATE CUING. The FLOLS display with the "command" rate information did not provide a transfer advantage over conventional FLOLS training conditions. Somewhat surprisingly, there was no performance advantage for the rate display during training either. Previous research suggested that there was a considerable performance advantage in glideslope control for experienced pilots using FLOLS displays with added rate information (Lintern et al., 1981; Kaul et al., 1980). Apparently, these findings do not apply to inexperienced pilots who are in the early stages of learning the task.

APPROACH TYPE. Training with circling approaches provided no transfer advantage compared to training with modified straight-in approaches, even though the transfer task involved a circling approach. In fact, for the lineup tracking score, training with straight-in approaches resulted in better transfer performance than training with circling approaches. This effect was actually fairly large in magnitude, with straight-in training resulting in 15.3 percent more time within 1.0 degree of the desired lineup during final approach on the transfer test.

The fact that training with circling approaches resulted in no advantage on the transfer task is probably in part due to the interaction of this factor with task difficulty. Approach type can be thought of as a training factor with part-task and whole-task training levels. Since the task is a very difficult one, it might be expected that the task could be learned more quickly with a part-task approach. The results of this experiment generally support this interpretation.

This result could be used to argue that the field of view issue is now moot because straight-in approach training resulted in as good or better transfer than circling approach training. This argument would be unjustified, however, for two reasons. First, Figure 8 indicates field of view transfer differences on lineup and angle of attack tracking for straight-in approaches as discussed earlier. Second, the approach type matter is still a training issue. Although results from this study support the use of straight-in approaches during early training, it is not known whether circling approaches--with or without a wide field of view--might be profitably introduced during later training. (Recall that this experiment involved training only to an intermediate level of ability.)

PILOT TYPE. As expected, differences between pilot types were large. Air Force T-38 pilots had better touchdown success and glideslope performance but Navy P-3C pilots had better lineup and angle of attack performance. However, interactions of other factors with the pilot type factor were generally

small. Thus effects were fairly consistent across groups despite the large group differences, but this is not surprising since other effects were generally small.

Ideally, results of this experiment should be generalizable to the population of interest, undergraduate Naval aviators. Both pilot groups represent pilots who have flying skills but are learning a completely new task. The Air Force group is similar to Naval undergraduates in age and experience. But a serious generalizability question exists because both groups brought learned skills to the task that are likely to result in negative transfer.

Specifically, these pilots have learned to control vertical glideslope deviations with elevator (pitch) inputs. The carrier landing task requires vertical deviation control with throttle adjustments while maintaining a constant angle of attack with elevator inputs. Acquiring this new control strategy proved especially difficult for the Navy P-3C pilots, as reflected in the glideslope tracking scores. Their scores were consistently lower than Air Force T-38 pilot scores in this dimension of performance despite the great advantage in flight experience. But as there were no strong indications from the data that results for other experimental factors differed between the two groups despite the large group differences, it is reasonable to suggest that results will be similar for undergraduate Naval aviators.

SECTION IV

COVARIATE RESULTS AND DISCUSSION

BACKGROUND

The discussion to follow assumes that the reader has considerable statistical knowledge. It is of primary interest to those concerned with solutions to the problems of interpretability of data from between subjects, highly saturated experiments such as this one. The less technically-oriented reader is referred to Section V, in which conclusions are presented that adequately reflect the limitations discussed in this section.

A serious problem can exist when between subject highly saturated multifactor experimental designs employing one (or few) subjects per condition are used. The transfer of training design used in the experiment reported here, in which there was one subject in each of 32 experimental conditions is an example of such a design. In such a design every estimable effect is confounded with differences due to subjects represented by the particular groups that define a specific comparison. For example, the estimate of the motion effect in this experiment is composed of a true motion effect plus the true mean difference in level of performance between the 16 pilots in the no-motion and the 16 pilots in the motion conditions, plus other error. Thus the subject difference effect biases the estimate of the factor effect, and by an unknown amount.

Consideration of this problem requires some definition of terms and description of conditions that underlie the area of concern. First, it is assumed that large, predictable true differences between subjects exist in terms of their level of ability on the task at hand. (True subject differences in rate of learning ability also enter into the problem for transfer of training designs, but this will not be considered for the moment.) To the extent that subject differences do not exist or are very small relative to other factor effects, the problem does not exist.

If subject differences exist but these differences are known or can be estimated, the biasing problem is effectively eliminated. Thus, for example, a within subjects design does not have this confounding problem because subject differences can be estimated and "removed" from other sources of variance. Obviously, if a "perfect" covariate existed for a between subject design, the problem would also be eliminated. The identification of subjects' mean level of ability then represents a solution to the problem.

The term "highly saturated" here refers to a design in which there is an a priori interest in a large percentage of the possible orthogonal estimable effects. A typical few factor between subject design with more than a few subjects per condition is not saturated because only a few of the possible effect estimates in the orthogonal set defined by the design are examined. The rest are combined as part of some "error" estimate. Another way to think of this is that in the fully saturated one-subject-per-condition design, the pool of subjects is split into all possible groupings of an orthogonal set.

Then each grouping is associated with an effect and group differences are confounded with the effects with which they are associated.

This can be seen by referring to the eight condition design presented in Table 6 which shows each effect associated with one of its seven orthogonal groupings of subjects. (The particular groupings follow from an original random assignment of subjects to conditions.) In an unsaturated one-factor design with the same number of subjects, there would be an interest in only one of the subject groupings and its associated effect, say D in Table 6. All the other effects would be used to estimate an error term. Clearly, the bias problem also exists to some degree in a one-factor design with many subjects per condition. There is a possibility of getting all the "best" subjects in one group and all the "worst" subjects in another. Investigators generally trust that random assignment and the laws of probability will usually save them from such an outcome. If a more even split of subjects occurs, with roughly equal numbers of good and bad subjects in each group, the amount of bias will be small and probably trivial.

TABLE 6. FULLY SATURATED BETWEEN SUBJECTS DESIGN*

Condition	Subject	Effect							Mean
		A	B	C	D	E	F	G	
1	1	-	-	+	-	+	+	-	+
2	2	+	-	-	-	-	+	+	+
3	3	-	+	-	-	+	-	+	+
4	4	+	+	+	-	-	-	-	+
5	5	-	-	+	+	-	-	+	+
6	6	+	-	-	+	+	-	-	+
7	7	-	+	-	+	-	+	-	+
8	8	+	+	+	+	+	+	+	+

*All factors at two levels. "+" and "-" distinguish levels of the factors.

In fact, given a sufficient number of subjects, the probability of getting a very bad sampling break is small. For example, in a one-factor, two-level experiment with 32 subjects (16 per group), and assuming the subjects can be classified into 16 "good" and 16 "bad" performers based on mean level of ability, the probability of getting the worst sampling break is 3.0×10^{-9} . The probability of getting the next worst sampling break is 8.5×10^{-7} and the probability for next worst is 4.8×10^{-5} . On the other hand, probabilities for getting an even break (8 good, 8 bad subjects in each group) or a nearly even break (9 good and 7 bad in one group and 9 bad and 7 good in the other) are .276 and .435, respectively.

These same probabilities hold for an a priori consideration of one of the effects in a saturated one-subject-per-condition multifactor design. However, by implication of the design there is an intention to examine many of the effects. Because all of the possible orthogonal subject groupings are

involved in these effects, and thus all of the subject variability is involved in the confounding, it is virtually certain that some effects will involve "bad" subject sampling breaks. The high probability of getting at least a few heavily biased effects defines the crux of the problem.

Without knowledge of subjects' mean levels of ability, there is no way of knowing which effect estimates are heavily biased by subject differences. Since it is virtually certain that some estimates will be heavily biased, one would be faced with uninterpretable data in such a situation. An effect estimate could be large totally due to subject differences or an effect estimate could be small because a subject difference is in the opposite direction of the experimental effect with which it is confounded. The problem will always be serious when dealing with complicated motor control tasks for which subject differences typically account for 20 percent or more of performance variance.

It should be noted that the problem could also be partly described as one of multiple comparisons from the same set of data. In this sense, within subject variability for a within subjects design represents the same kind of problem. However, within subjects variability is generally assumed small relative to between subject variability, and it is typically much easier and cheaper to obtain data on many runs from the same subject than it is to obtain many subjects. Thus, the within subject source of error problem is considered categorically different from a practical point of view.

Because of the severity of the problem, it is mandatory that research on identification of subjects' level of ability be an integral part of any research program in which highly saturated between subject designs will be used. Further, basic research on the appropriate use of knowledge of between subject differences with these designs remains to be done. The problem is not insurmountable and there are several possibilities for reducing the degree of risk while sacrificing little of the great economy these designs have to offer. It should also be pointed out that this problem does not mitigate the economy of the holistic approach. For a given degree of subject variability, the arguments in favor of a multifactor approach remain the same. However, thorough exposition and statistical treatment of the problem remains.

EXPERIMENTAL SUBJECT VARIABILITY

Probably most significant in terms of controlling subject variability in this experiment was the inclusion of pilot type (Air Force T-38 vs. Navy P-3C) directly as an experimental factor. This type of subject variability control is best of all, of course, because the guesswork due to less than perfect covariates is done away with. Differences on the defined dimensions of subjects are estimated directly as part of the experiment. This subject factor almost completely accounts for flight experience differences also, accounting for 77 percent of the variance among pilots described by number of flight hours. This is, of course, primarily due to the fact that all Air Force pilots had little flight experience compared to the Navy pilots. Because flight experience is a strong covariate describing subject level of ability (for example, see Westra et al., 1982), there was reason to believe

that the pilot type factor would account for a large percentage of total predictable subject variability.

This is an important consideration bearing on the problem of subject effect biasing described earlier. If an effect represents a "best" vs. "worst" subjects comparison, it will account for most of the identifiable variance due to subjects in the entire experiment, depending on the distribution of subject levels of ability. For example, if the two groups represent homogeneous groups differing only in mean level of ability, this single effect will account for essentially all of the predictable subject differences. If subject mean levels of ability are ordered with equal intervals from worst to best, the "best" vs. "worst" contrast accounts for 75 percent of the identifiable subject differences (Simon, 1977b). In this case, two other effects can be identified which account for 19 percent and 5 percent of the remaining variability due to subjects. All other effects will have less than 1 percent overlap with subject differences.

Thus, if only one effect can be identified and associated with the largest single source of variability due to subjects, most of the remaining effect estimates are likely to be only trivially biased by subject differences. Further, given enough information to order subjects on their mean levels of ability, 28 effect estimates in a 32-condition design can be obtained which are essentially free of biases due to subject differences.

In the present study, there is considerable justification, both on a priori grounds and on post hoc grounds from an examination of effect sizes associated with the pilot type factor, for believing that the pilot type factor accounted for a large proportion of the variance due to subjects. To the extent that this is so, it is reasonable to assume that most of the other estimable effects in the experiment were only trivially biased by subject differences.

ATARI AIR COMBAT MANEUVERING RESULTS

For a covariate to be effective, several requirements must be met simultaneously. First, the measures on the task must have high test-retest reliability which implies strong capability to discriminate subject levels of ability. The reliability should remain high for a sufficient period of time over continued practice on the covariate task which implies stability of subject relative levels of performance. This is especially important when considering psychomotor tasks for which considerable learning will occur. Because subjects learn at different rates, stability may not occur until after considerable practice. Second, performance on the criterion task must have high test-retest reliability. The stability requirement is not as critical here since the goal is to predict subject levels of ability at the time of the experiment (or transfer in a transfer of training experiment). Still, without some reasonable stability at the time the key data are collected, the predictive capability of the covariate will probably be compromised.

Third, the relationship between the covariate task and criterion task must be strong. This strength of association is practically limited by the

reliability of the measures on covariate and criterion tasks. If the score on either has low reliability, there is no possibility for accurately estimating the subject's mean levels of ability from the data. Note that the ultimate goal of having the highest possible true relationship between covariate and criterion task performance at the desired time does not absolutely require stability of relative subject performance levels on the covariate task. Again though, unless a reasonable degree of stability exists, the practical strength of the relationship between covariate and criterion will probably be compromised. In any case, a prerequisite to the use of covariate data in the analysis of experimental results from saturated designs is an examination of the within and between task relationships.

The first 16 pilots in the present study performed 50 ACM simulator trials. Examination of the correlation matrix for these pilots' scores on five-trial means indicated stabilized performance essentially from the start. These correlations are shown in Table 7.

TABLE 7. INTERCORRELATIONS FOR FIVE-TRIAL MEANS FOR FIFTY TRIALS ON AIR COMBAT MANEUVERING (16 PILOTS)

Mean of Trials	<u>1-5</u>	<u>6-10</u>	<u>11-15</u>	<u>16-20</u>	<u>21-25</u>	<u>26-30</u>	<u>31-35</u>	<u>36-40</u>	<u>41-45</u>
6-10	.81								
11-15	.83	.83							
16-20	.90	.81	.88						
21-25	.81	.87	.82	.90					
26-30	.81	.79	.79	.89	.84				
31-35	.89	.90	.87	.89	.89	.87			
36-40	.88	.80	.76	.86	.81	.74	.83		
41-45	.73	.76	.80	.81	.81	.73	.85	.84	
46-50	.89	.87	.81	.85	.87	.81	.91	.85	.89

As this finding indicated that a small number of trials was required for stability, the remaining pilots were tested on 30 ACM trials. In subsequent analyses, each pilot's mean score for trials 26-30 was considered to be the covariate. The correlation matrix for all pilots' scores on five-trial means is given in Table 8. Clearly, the ACM task meets the requirements of stability and high reliability. The average correlation between blocks of five trials after the third block was = .85.

TABLE 8. INTERCORRELATIONS FOR FIVE-TRIAL MEANS FOR THIRTY TRIALS ON AIR COMBAT MANEUVERING (32 PILOTS)

Mean of Trials	<u>1-5</u>	<u>6-10</u>	<u>11-15</u>	<u>16-20</u>	<u>21-25</u>
6-10	.81				
11-15	.76	.77			
16-20	.89	.80	.83		
21-25	.79	.77	.79	.89	
26-30	.76	.65	.74	.87	.86

Scores on the last block of eight transfer trials for percent time within 0.3 degree on glideslope (3000 feet to the ramp), and the touchdown wire accuracy score were used to examine the relationship between ACM and simulator performance. Only this last block of eight transfer trials was considered reasonably "safe" to use in terms of independence from transfer effects. However, it must be kept in mind that it is not possible to determine how "safe" these transfer trials are because of possible subject effect confounding. Further, since there was a known pilot type effect through these transfer trials, the relationship within pilot type was also examined. The relative sparsity of usable information on the criterion task for covariate validation purposes does, of course, restrict the generalizability of the findings.

The correlations between mean scores on blocks of four of the last eight transfer trials are shown in Table 9, along with the correlations between ACM and glideslope and landing scores on the last eight transfer trials. As the table indicates, reliabilities for touchdown scores and glideslope tracking scores are poor. This could be in part due to the low number of trials used to obtain a pilot's mean score. In contrast, the high reliabilities given by Westra et al. (1982), were based on means from blocks (experiments, actually) of 32 trials from experienced pilots. The low reliabilities could also be due in part to lack of stabilization on the task. There was evidence suggesting poor stabilization for the Navy group in particular, at this point in training, even though it was the end of the training experiment. This suggests the training period might have been too short, among other things. Given the low reliabilities for scores on the criterion task, the relationships with the ACM covariate are generally as strong as could be expected.

EFFECT OF ACM AS A COVARIATE ON EXPERIMENTAL RESULTS

Rather than present an analysis of covariance with the available degree of justification for use of ACM as a covariate, the relationship of the covariate to the estimable terms in the design will be examined. Effect estimates from terms that share little or no variance with the covariate would not be affected by a covariate adjustment, i.e., these terms are "robust" to subject effects just like most terms in a multifactor design are "robust" to trend effects (Simon, 1977b). Examination of correlations between covariate and

TABLE 9. CORRELATIONS OF STABLE ACM SCORES WITH SIMULATED CARRIER-LANDING TRANSFER TASK SCORES

	<u>Air Force T-38 (N=16)</u>	<u>Navy P-3C (N=16)</u>	<u>All Pilots (N=32)</u>
Glideslope ¹ by ACM Correlation	.37	.25	.42*
Landing ² by ACM Correlation	.36	.48*	.51*
Glideslope Reliability ³	.32	.23	.32
Landing Reliability	.29	.20	.35*

¹Mean glideslope tracking scores for last eight transfer trials. Score was percent time within 0.3 degree of desired for 3000 ft. to ramp.

²Mean touchdown wire accuracy score for last eight transfer trials.

³Reliabilities are the correlations of the last four transfer trial means for each pilot vs. the preceding four transfer trial means.

*p ≤ .05

estimable terms identifies those terms associated with subject effects based on the covariate. Theoretically, assuming a covariate that perfectly predicts subject differences on the criterion task, this is all that it is necessary to know to design multifactor few-subjects-per-cell experiments. The terms correlated with the covariate would be left with no factor assignments and simply called subject effects in the analyses. These terms would then do the work of removing subject differences from the results.

The correlations between ACM means on trials 26-30 and the 31 estimable terms in the experimental design are shown in Table 10. Only three individual terms are associated with more than 6 percent of the total variance described by ACM as a covariate. These three effect estimates are the only ones that would be more than trivially affected by a covariate adjustment. In other words, under the assumption that ACM is a perfect descriptor of subject mean level of ability on the carrier landing task, all experimental effect estimates are essentially unbiased by subject differences (although there might be some debate about the triviality of 6 percent variance overlap) except for the three indicated terms.

One of the three terms associated with ACM is the pilot type factor with 14 percent variance overlap indicated. Since this term was already defined as

a subject effect, the effect estimate simply estimated this amount of subject difference. The fact that this particular term was defined as a subject effect is, of course, exactly what is desired. Other than this, what is noteworthy here is that ACM correctly predicts that Air Force pilots will have better glideslope tracking scores than Navy P-3C pilots, exactly opposite the effect direction predicted by flight experience. This lends additional support to the use of ACM as a covariate for this task.

The other two terms having more than trivial overlap with the covariate were defined as the motion factor and a three-way interaction string. The positive correlation of motion with ACM suggests that more of the "better" (based on the covariate) pilots were in the motion condition. Results for glideslope tracking and landing performance (Figures 4 and 5; Tables 2 and 3), without covariate adjustment, show a very small positive transfer benefit for motion training which cannot be differentiated from noise. The effect of a

TABLE 10. CORRELATIONS OF STABLE ACM SCORES WITH ESTIMABLE TERMS IN THE EXPERIMENTAL DESIGN

<u>Source of Variance</u>	<u>Correlation with ACM</u>	<u>Percent Overlap</u>
Field of View (A)	.00	0
Motion (B)	.35	12
Pilot Type (C)	-.37	14
Scene Detail (D)	-.21	4
Turbulence (E)	-.05	0
FLOLS Rate Cuing (F)	-.18	3
Approach Type (G)	-.09	1
BD + AF	.02	0
AB + DF	.12	1
BC	-.18	3
AD + BF	-.08	1
CD	-.03	0
DE	-.19	4
AC	.02	0
AE	.03	0
CE	-.11	1
BDC + ACF	-.05	0
BED + AEF	.01	0
EG	-.25	6
CG	.21	1
ADC + BCF	-.20	4
AED + BEF	.21	4
FG	.00	0
BG	.06	0
CF	.22	5
EF	.07	0
ADF + BFG	-.46	21
BDG + AFG	.24	6
DG	-.11	1

covariate adjustment then would be to decrease the size of the effect and perhaps even change the direction of the effect to a transfer advantage for the no-motion training conditions. In fact, in some covariance analyses that were run, the motion effect was reduced in magnitude but did not change direction. Since the motion effect was not significant to begin with, the impact of this is simply to emphasize the bottom line, no differential motion transfer effect.

The three-way interaction string shows the strongest individual relationship with the covariate (21 percent overlap). This effect was always treated as "error" in the reported analyses and thus was part of the term used as an estimate of the "subject within groups" variance described in classical tests (Winer, 1971, ch.7). Assuming that ACM is an appropriate covariate, the result of use of the effect in this way then was simply to make the F-tests more conservative than they would have been had this effect been labeled a subject effect and not included in the between subjects term.

SECTION V

SUMMARY AND CONCLUSIONS

An in-simulator transfer of training experiment was conducted with novice carrier-landing pilots as part of an effort to define design requirements for undergraduate carrier-landing simulators. Three simulator equipment factors were investigated (field of view, scene detail, and cockpit motion) along with two training factors (approach type and FLOLS rate cuing) and one environmental factor (turbulence). A pilot group factor defined by two populations with no carrier-landing experience (Air Force T-38 and Navy P-3C pilots) was also included in the experiment. Pilots performed 40 training trials with instructional feedback under conditions defined by various levels of the experimental factors and then performed 16 transfer test trials under a high-fidelity simulator configuration.

The main conclusion to be drawn from the results is that transfer effects due to equipment variables were fairly small from a practical point of view. Transfer landing quality was not affected by any factor other than pilot type and approach quality was generally only temporarily affected by equipment factors, i.e., effects had essentially disappeared after eight transfer trials. The pilot type effect was generally larger than all other effects combined and the only other factor that had a sizable, sustained transfer effect was a training factor, approach type.

The temporary effects that did occur, however, were not insubstantial. The wide field of view and high scene detail conditions generally resulted in better final approach performance during the first eight transfer trials. This suggests that, for an undergraduate carrier landing trainer, some training advantage could result from the use of a wide-angle high-detail visual system over the narrow-angle lower-detail alternative. Whether this advantage would result in cost savings sufficient to offset the added simulator procurement and life-cycle costs is questionable due to the short duration of the effect. Nevertheless the results are sufficiently suggestive to justify a simulator-to-field transfer of training experiment in which the variables of field of view, scene detail and approach type are manipulated.

This experiment, combined with those of Westra et al. (1982), have thus provided a strong basis for deciding which variables are most important for inclusion in a much more costly and difficult field transfer experiment. The other factors examined in these experiments are judged far less likely to be of operational significance and are not recommended for inclusion in such a field study.

INDIVIDUAL FACTORS

A brief summary of results follows with the factor effects listed in the order of overall impact on the transfer task.

Pilot Type had by far the largest effect during the transfer trials but the effect was dependent on the dimension of performance measured. Air Force T-38 pilots did better than Navy P-3C pilots on glideslope tracking and landing wire accuracy, but Navy P-3C pilots did much better at lineup tracking and angle of attack control. The straight-in approach type training resulted in better final approach lineup control on the transfer task despite the fact that the transfer task involved a circling approach. Pilots training with straight-in approaches had 15 percent more time within the lineup tolerance limit on the transfer task than pilots training with circling approaches. Also, the circling approach training showed no transfer advantage for other final approach scores or landing accuracy.

The wide field of view resulted in some advantage on the transfer task for final approach quality but not landing accuracy. There was a small effect on glideslope tracking and a temporary effect on lineup and angle of attack tracking. The overall effect on final approach quality after eight transfer trials was small at best.

Daytime scene detail training conditions had no transfer advantage over night scene detail training conditions on landing accuracy. There were transient effects on final approach glideslope and angle of attack performance favoring the daytime training scenes. There appeared to be a final approach lineup performance advantage on transfer with the daytime training scenes but only with straight-in training approaches.

The presence or absence of motion during training did not make a difference on transfer. The addition of rate cuing information to the FLOLS did not result in a transfer advantage compared to training with a conventional FLOLS.

CONSIDERATIONS

Several points should be kept in mind when considering these results. First, the serious problem of subject effect confounding was discussed in detail. Although a subject factor was included in the experiment that appeared to account for a large amount of subject variance, and a covariate task was employed that apparently showed some relationship to the criterion task, this problem was not fully resolved.

It should also be noted that in keeping with the "screening" nature of the design, factor levels chosen for study represent extremes of the operationally reasonable range of interest. It is not suggested that "low" fidelity be strictly defined by the low-level conditions of the experiment. Improvements to low-level conditions that can be made at little or no cost should always be considered, particularly if there is some justification from experimental data. For example, both field of view and scene detail had moderately large effects on roll variability during the training trials with the high-fidelity versions of these factors resulting in reduced roll variability. While these effects may not have been large enough to affect task outcome substantially, they do suggest that the "flyability" of the simulator in terms of workload is

affected by these factors. The effect on roll variability is almost certainly a function of the extent and visibility of the horizon, varying from a full horizon with the wide field of view daytime high-detail scene to none at all with low-detail scenes. This suggests that a low cost horizon for the low-detail scene should be considered even if task outcome transfer results show no difference.

Finally, there is a question of generalizability of results because of possible negative transfer of skills from the subject pilot populations to the task. A simulator-to-field study with undergraduate Naval aviators is needed to confirm the results obtained here. This study provides recommendations for such an experiment and at the same time depends on a field-transfer study for its own ultimate value. Confirmatory results could go a long way toward increasing confidence in in-simulator results and saving some of the enormous expense associated with field-transfer studies.

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APPENDIX A

BRIEFING

CARRIER LANDINGS IN THE
VISUAL TECHNOLOGY RESEARCH SIMULATOR

INTRODUCTION

Welcome to the Visual Technology Research Simulator (VTRS). This is a Naval Research Facility developed to study the use of simulators for teaching flight skills.

The VTRS simulates a T-2C aircraft and consists of a single seat cockpit, a ten foot radius spherical screen which surrounds the cockpit, and control computers which run the simulator. The cockpit controls and instruments operate just as they do in a real aircraft. A picture of an aircraft carrier is projected on the screen, and when the simulator is running, the scene will look just as it would if you were flying a real carrier approach.

Because this is a controlled experiment, we will be using a special sequence and schedule to instruct you in what you are to learn. This is to assure that each person in the experiment receives the same material in exactly the same manner. However be sure to ask for clarification on any points you do not understand.

We are teaching different people under different conditions. While we do not believe that knowledge about other conditions will affect your performance, we would like you to inhibit your curiosity about what others are doing until your experimental work is over. It is possible that viewing the displays at the control station could affect your performance, so we would like you to wait in the subject room if you arrive early for a session. Brief exposure to the control station displays or those found elsewhere in the VTRS building will not affect you, but please do not spend any substantial amount of time studying them.

We will tell you when you have finished the experiment and will be prepared to describe other conditions at that time, or to let you view the control station operation if you wish.

We appreciate your participation in this experiment and we hope that it will be a meaningful experience for you.

ABOUT THE EXPERIMENT

This study has been designed to tell us something about how simulation can be used to teach carrier landings. We are examining the training efficiency

of several different instructional techniques by teaching carrier landings under the different configurations and then testing landing ability in a simulator configuration that is as close to full fidelity as we can get. While the experiment will be conducted entirely in the simulator we intend to use the information gathered from it to help us design a study in which pilots will be taught first in the simulator and then tested in the aircraft.

Note that this experiment is aimed at testing the simulator, and is not a test of your ability. Nevertheless there will be differences between pilots and we need to account for these when we analyze the data. Differences will be minimized if everyone does their best. We would like you to concentrate on learning the task in the correct manner and as quickly as possible. We would also like you to do your best on every trial.

For a normal day carrier landing the pilot circles the carrier to fly a downwind leg parallel to and in the opposite direction to the carrier heading, and about one mile to port (left looking towards the bow). This is where you will start your circling approaches. The simulator will be flying straight and level in the landing configuration (wheels down, flaps down, hook down and brakes out) with 15 units Angle of Attack (AOA), 600 feet of altitude, 85 percent power, and on a heading of 180° . Start a left turn with 15° to 18° of bank when the tip tank is abeam the carrier ramp. Throttle back to establish a descent rate of approximately 400 fpm but maintain 15 units AOA. At the 90° position (see Figure A-1) you should be close to 400 feet of altitude. From that point continue the turn to roll out on the glideslope and on the extended centerline of the landing deck. You should be about 3000 feet from the carrier (20 to 25 seconds out to touchdown) at roll out. Continue the approach to touchdown.

For straight-in approach, the simulator will be initialized in the Landing configuration two miles from the ramp, 15 units Angle to Attack, 400 feet altitude and left of the center line. This is where you will start your straight-in approach.

Upon release you will fly the aircraft straight and level and maintain 400 feet altitude (approximately 86 percent power). Flying this configuration you will make a gradual cut into the extended center line of the landing deck. In addition, by maintaining a 400 feet altitude you will intercept the glideslope and a centered meatball at approximately 4500 feet from the ramp. When the meatball approaches centerball you are to reduce power to 83-84 percent and continue the approach to touchdown.

THE CARRIER APPROACH

Precise aircraft control is essential in a carrier approach. Vertical displacement errors at the ramp (threshold of the landing deck) of a few feet can be disastrous, as can descent rate, airspeed or attitude errors at touchdown. Thus, the pilot must maintain a precise glideslope (generally set at 3.5°) and maintain the correct descent rate, airspeed and attitude.

CARRIER LANDING PATTERN MIRROR APPROACH

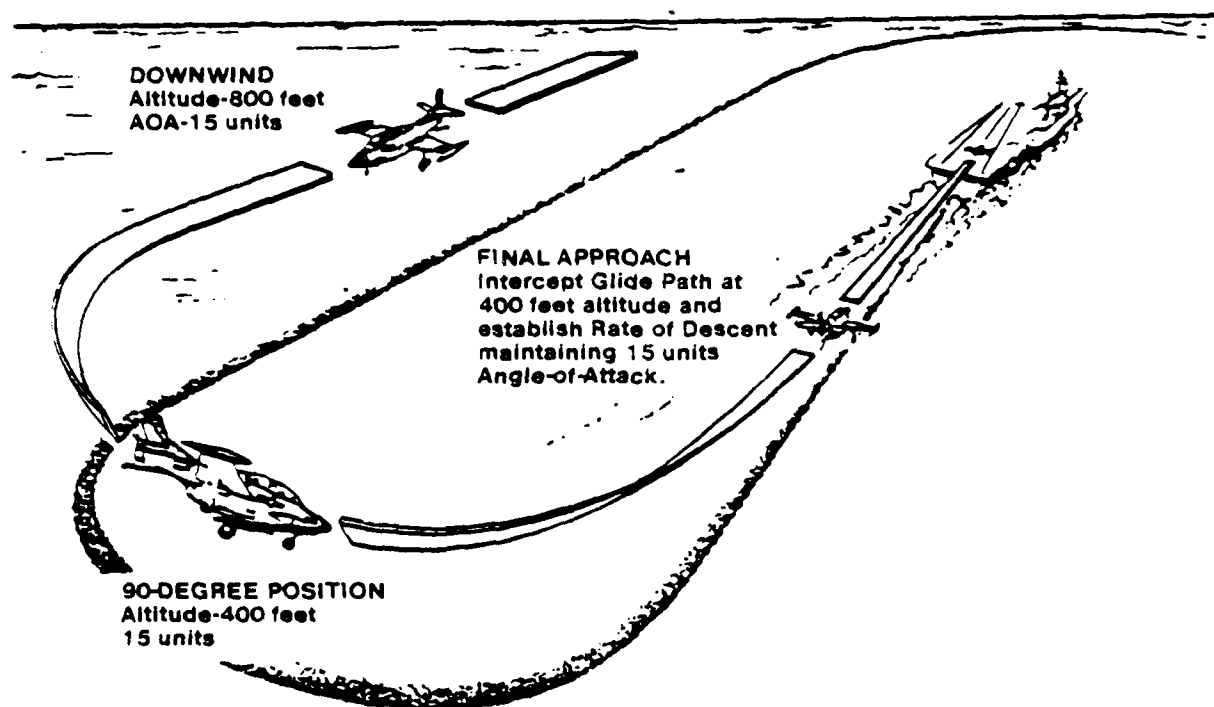


Figure A-1. Carrier Landing Pattern from the Downwind Leg

Conventional landings permit some deviations in these parameters but Navy carrier pilots must establish them early in the approach and maintain them to touchdown. Neither is it acceptable for a Navy pilot to fly a loose early approach with the aim of establishing better control near the carrier. The potentially disastrous consequences of errors makes the uncertainty associated with this type of behavior quite unacceptable. In this experiment you will learn some of the skills needed for carrier landings.

PARAMETERS FOR APPROACH CONTROL

In making an approach from the roll out position the carrier pilot must be concerned with:

- 1) current position in relation to the glideslope,
- 2) current descent rate--is it correct, if not is it taking him away from the glideslope
- 3) airspeed and pitch attitude--integrated into one instrument known as the Approach (Angle of Attack) Indexer, and
- 4) lineup.

GLIDESLOPE POSITION

Glideslope guidance is normally given by the Fresnel Lens Optical Landing System (FLOLS). We have simulated this system with two horizontal bars (to represent the datum bars) and a moving dot (referred to as the ball or the meatball). The system is illustrated in Figure A-2 and Figure A-3 a to e. A center ball indicates that the aircraft is on the glideslope (later discussion will note that correct aircraft attitude is necessary for that to be true). A high ball indicates that the aircraft is above glideslope, and a low ball that it is below glideslope. At two balls low the meatball starts to flash. Plus or minus two balls is the maximum effective range of the system. The ball will be lost off the top or the bottom at larger deviations from glideslope.

A real FLOLS projects cones of light from the ship as shown in Figure A-4. Thus the system is angular. Larger errors are required far from the ship to see meatball movement than are required near the ship. At 3/4 mile a 12 foot glideslope displacement is needed to move the ball off center while at the ramp, a one foot displacement will move the ball off center. The range of the FLOLS is approximately $\pm 3/4^\circ$ (precisely $\pm 47.5'$) or, if set for a 3.5° glideslope, from 2.75° to 4.25° (approximately).

CARRIER LANDINGS

In making a carrier landing the pilot attempts to follow the FLOLS center beam to the deck of the carrier. If he can maintain a center ball, and keeps the aircraft in the correct pitch attitude, a hook fixed to the tail of the aircraft (Figure 4) will follow a glide path that is parallel to, but lower than the center FLOLS beam. It is intended that the hook contact the deck

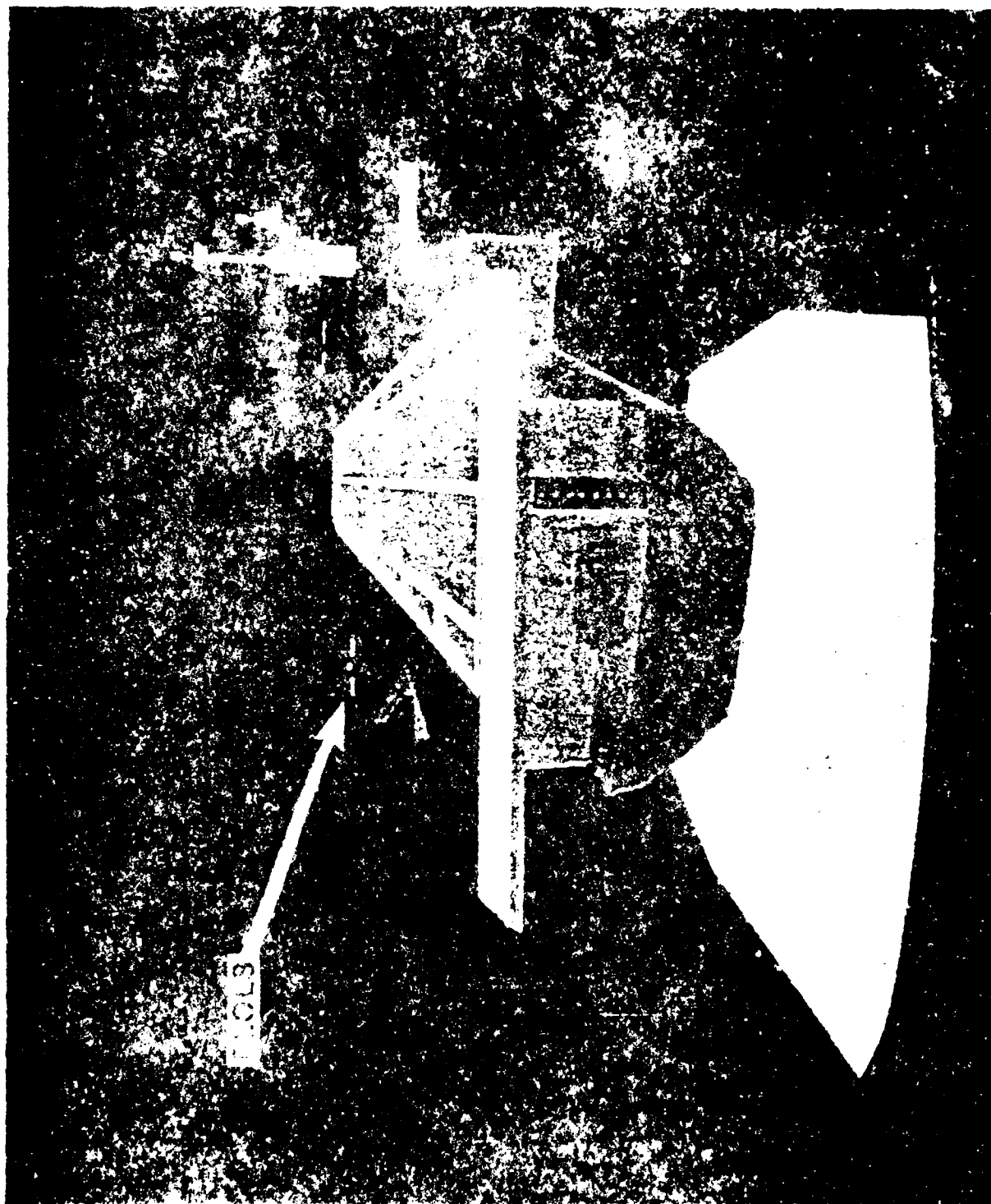


Figure 6-4. Aircraft Carrier from the Gladslope.
(Note: The Fresnel Lens Optical Landing System (FLOLS).)

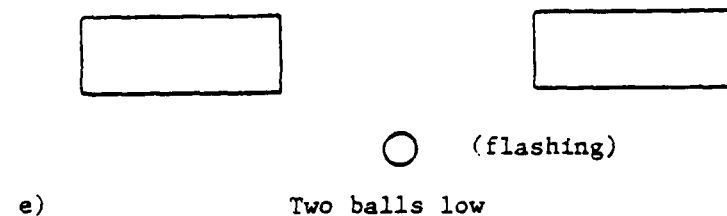
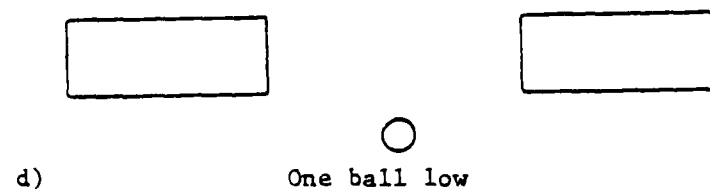
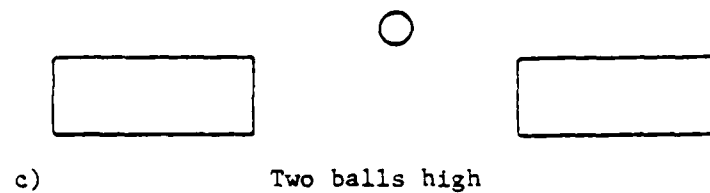
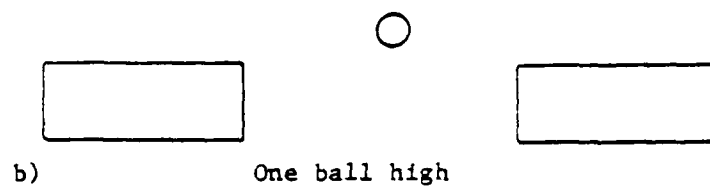
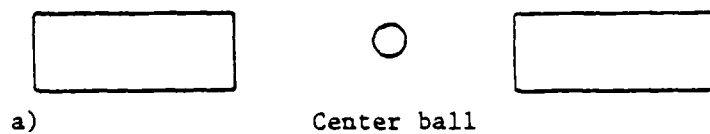


Figure A-3. The Fresnel Lens Optical Landing System

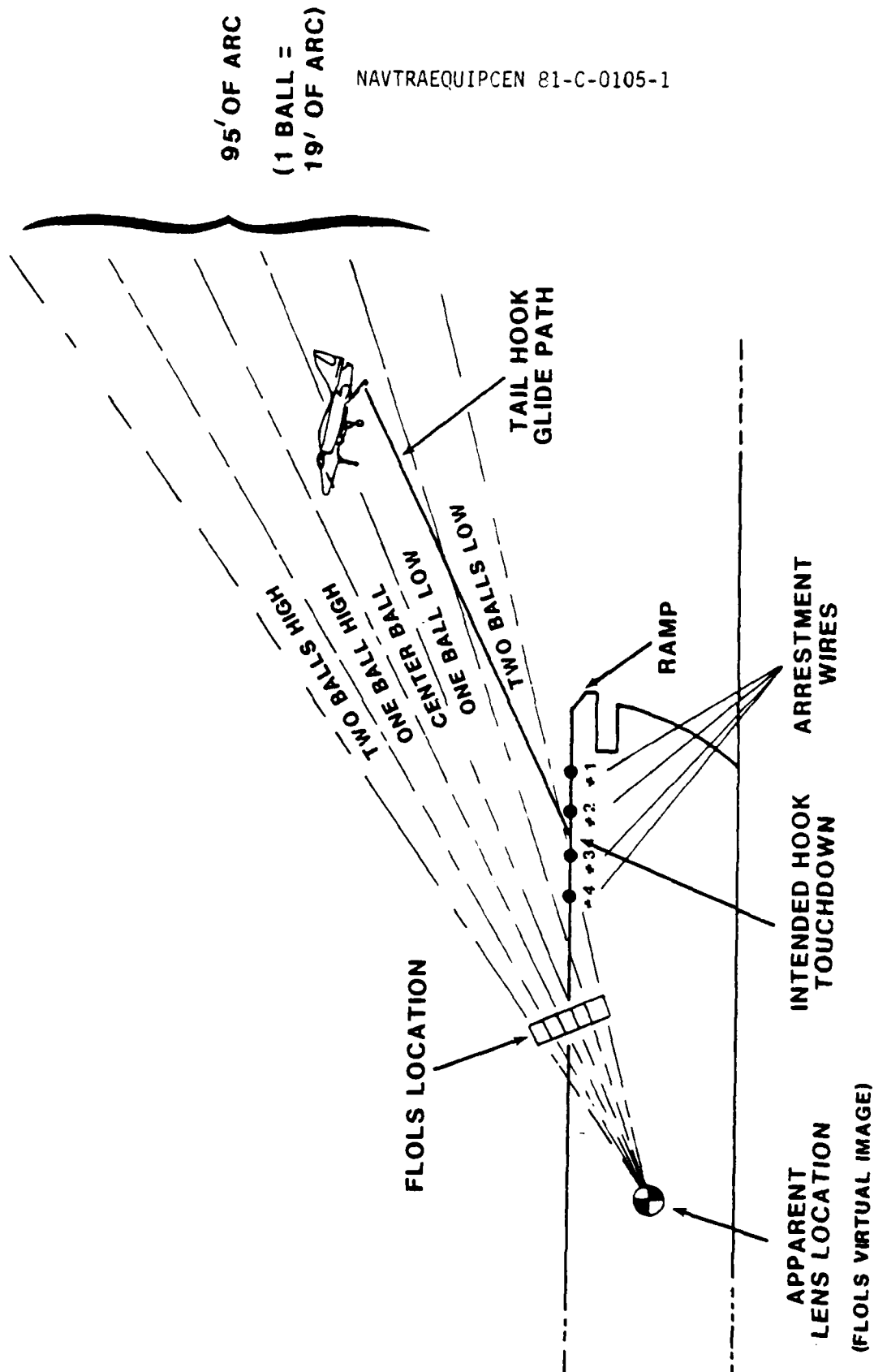


Figure A-4. Carrier Approach Schematic Depicting FLOLS Envelope, Tail Hook Glide Path, and Arrestment Wire Locations.

midway between the second and third of four cables stretched across the deck (these cables are known as arrestment wires). The hook travels forward from this point to snag the third wire, and so the aircraft is halted.

If the pilot is slightly low on the approach he may snag the first or second wires. If he is very low (actually an error of 10 feet may be enough) he may hit the ramp, thereby bringing disgrace and physical harm to himself, and severely damaging a multi-million dollar aircraft. If a pilot is slightly high on the approach he may snag the fourth wire. If higher (possibly only two feet higher than optimum he may miss the wires altogether and fly off the end of the carrier. A missed approach of this type is called a bolter. Fortunately bolters do no lasting damage (about 5 percent of approaches result in bolters), but they do detract from shipboard efficiency. Thus the ability to follow the glideslope contributes to a Navy pilot's health, happiness and self-esteem.

DESCENT RATE

The aircraft has a Vertical Speed Indicator (VSI) (Figure A-5), with hash marks shown at 200 fpm intervals (Figure A-6). The reference descent rate for the T-2 in the configuration that you will be flying is 515 fpm. That is, if the aircraft is on the glideslope and with the correct attitude and airspeed, it will stay on the glideslope if the reference descent rate is maintained.

If you are above glideslope you will need to establish a descent rate of up to 800 fpm, while if you are below glideslope you will need to establish a descent rate of as low as 200 fpm. These corrections will return you to the glideslope at an appropriate rate. Maximum, minimum, and optimum vertical speeds are indicated in Figure A-6.

Note that if you perceive an incorrect vertical speed, it will probably not be sufficient merely to correct back to the reference rate (515 fpm) even if you are on glideslope. By the time your correction has taken effect you will probably be off glideslope and will need to correct in a direction opposite to that which caused the error. The techniques for correcting glideslope errors are central to good carrier landings, and will be discussed in detail in a later section.

Descent rate information can also be obtained from the meatball. If you have a center ball, but see it moving, you can judge that your descent rate is incorrect. In addition, if you are high, you need to start the ball moving down, and if low, start it moving up. You can use the rate of ball movement to establish an appropriate corrective descent rate. This can be useful because it means that you do not have to look inside the cockpit at the VSI. Unfortunately, it is possible to discern movement of the meatball only when the aircraft is approximately 1500 feet from the ship. At greater distances the rate of movement is so low that it is below the threshold for the psychological process that interprets changes in position as rates of movement. Thus, you will have to rely on the VSI until you close on the carrier.

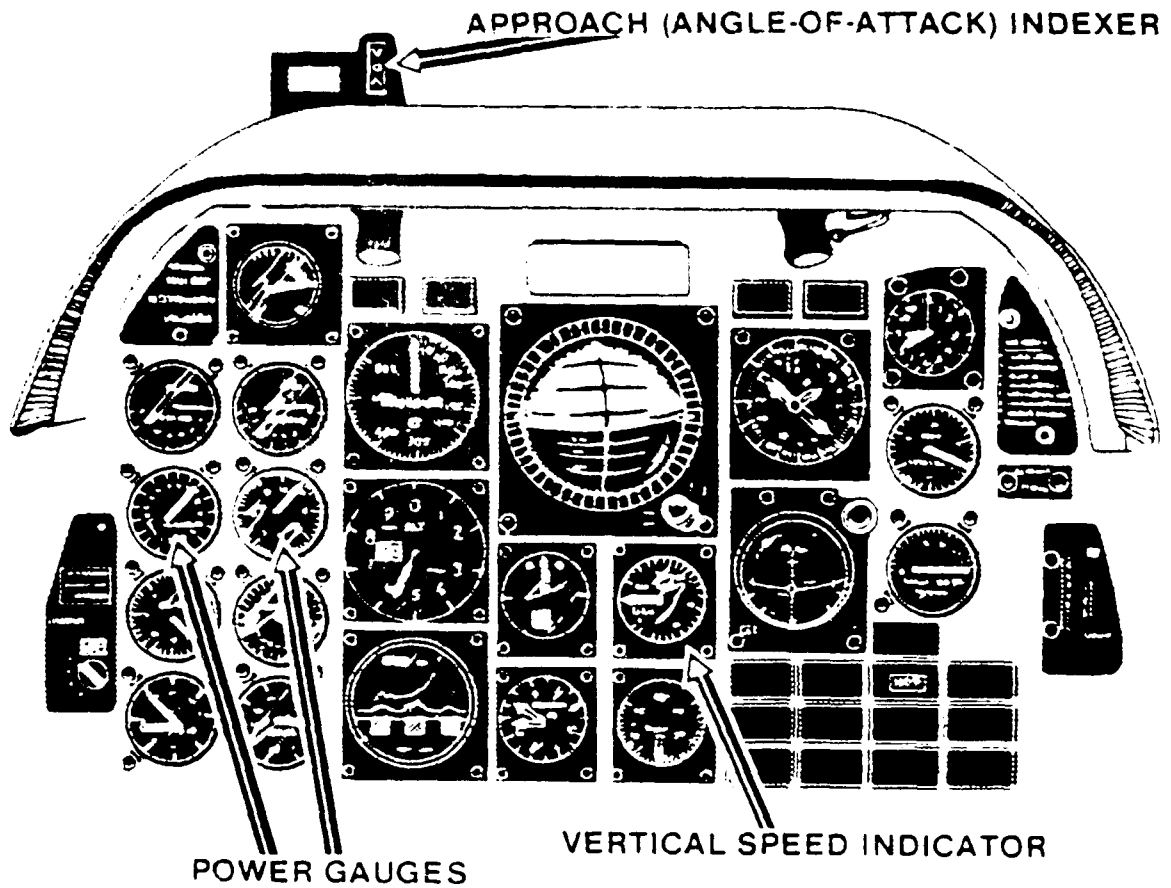


Figure A-5. T-20 Instrument Panel

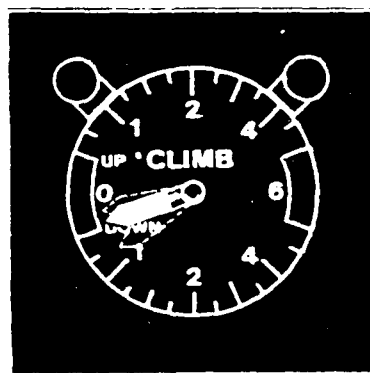


Diagram of T-20 VSI showing 200 fpm hash marks (needle at 480 fpm, and dotted needles at 200 and 800 fpm).

Figure A-6. Vertical Speed Indicator

To assist your rate judgments we have added some vertical arrows to the FLOLS as shown in Figure A-7. They are calibrated to indicate whether you should modify your vertical speed. A null indication while you have a center ball indicates that you are on glideslope and staying there. Arrows up or down indicate that, although you may now have a center ball, you will soon be high or low. If you are above or below glideslope, a null indication shows that you are returning to the glideslope at an appropriate rate. Down arrows mean you are descending too quickly. Up arrows indicate you are not descending quickly enough.

If you are high, up arrows indicate that you are not returning to the glideslope quickly enough, and could even be going further from it. You should descend more quickly. Down arrows indicate that you are returning to the glideslope too quickly and will probably overshoot. Reduce your descent rate.

For a low meatball the interpretations are just the opposite, down arrows indicate that you are not returning to the glideslope quickly enough and may even be flying further from it, while up arrows indicate you are approaching it too quickly and will overshoot.

The basic rule is to null the arrows wherever you are. Up arrows indicate you are not descending quickly enough. Down arrows mean you are descending too quickly.

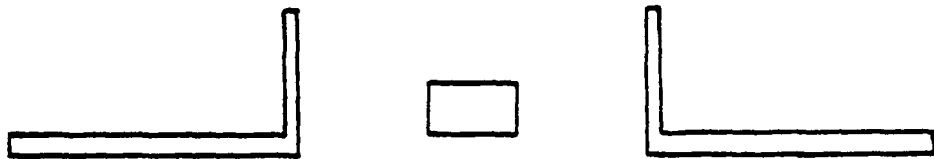
The arrows will be available during your initial training, but they will not be available in a later test session. Use them for guidance, but do not rely on them at the expense of the other rate information. Use the arrows to help you learn to use the other rate indications.

ANGLE OF ATTACK

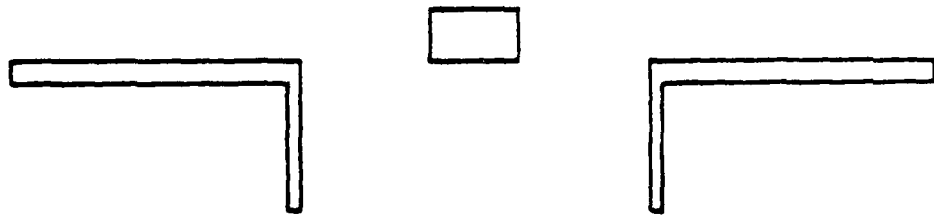
The FLOLS is a passive optical system, and the pilot sees a center ball when his eye is in the center beam. The center beam is set so that, at the correct aircraft attitude, the tail hook of the aircraft is proceeding on a glide path of its own, towards a point on the carrier deck midway between the second and third arrestment cable (Figure A-4). However, the hook is at the other end of the aircraft from the pilot's eye, and simple geometry would suggest that an incorrect pitch attitude will move the hook above or below its glide path even when the pilot's eye is on the correct FLOLS glideslope. In fact, the hook is the critical point of the aircraft for touchdown accuracy, not the eye of the pilot. The only means the pilot has of ensuring that the hook is in the correct position is by following the FLOLS beam with his eye, and flying the correct AOA (which will ensure correct pitch attitude).



- (a) One-ball high; returning to the reference glideslope at an appropriate rate.



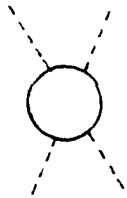
- (b) One-ball high; not returning to the glideslope quickly enough (may even be going higher).



- (c) One-ball high; returning to the glideslope too quickly and will probably fly through it.

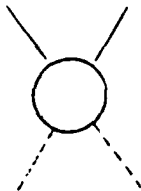
Figure A-7. Three Types of Indications from the Rate Arrows.

To monitor AOA the Navy pilot is provided an instrument called the Approach Indexer. You will find it above and to the left of the instrument panel (Figure A-5). It consists of an upper and lower chevron and a center circle (donut). It is possible for one chevron, or the donut, or a chevron-donut pair to be illuminated. The readings and their interpretations are shown in Figure A-8.

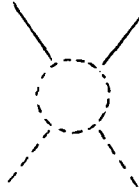


Correct AOA (15 Units)
i.e., on speed, correct attitude

14.5 to 15.5 units

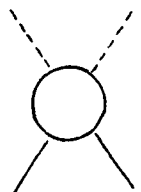


15.5 to 16.0

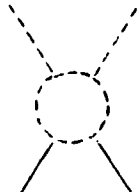


16+ units

High AOA (more than 15 Units)
i.e., slow, with high pitch attitude



14.0 to 14.5



14.0 units

Low AOA (less than 15 Units)
i.e., fast, with low pitch attitude

Figure A-8. Indications from the Approach (Angle of Attack) Indexer

A chevron-donut pair can generally be regarded as acceptable. This would allow a range of 15 ± 1 units. More extreme AOA errors should be corrected as is described in a later section of this reading.

LINEUP

In carrier landings the pilot lines up with the extended center line of the landing deck. Note that the landing deck is canted at 10.5° to the longitudinal axis of the ship. It is not, therefore, appropriate to use the carrier wake or the main deck for lineup. Lineup errors are corrected with small banking turns to the left or right. You will need to use fine control pressures in moving the stick to the left or right, and on the rudder pedals, to start these turns. In turning onto the center line, you should anticipate closing on it: that is, start your lineup turn before you reach it. If you start your lineup turn when you reach the center line, you will find yourself a long way past it by the time you are heading the simulator in the right direction.

At night you will need to use the drop lights at the stern of the carrier to assist you with line up. If you are lined up it will appear as a straight extension of the center line of the landing deck. If you are off center it will appear angled to the center line. It will, in fact, form a V with the center line, with the apex of the V pointing in the direction you must go to line up (Figure A-9).

ERROR CORRECTION: GLIDESLOPE AND AOA

Upon reaching the 90 degree position (about halfway through the turn) and acquiring the ball, the aircraft is on the glideslope. Due to ever-increasing closure rate on the touchdown point, the rate of descent required to maintain a centered ball from the 90 to wings level on final is an ever-increasing amount. Therefore, less power may be required from the 90 to the start of the final so as to maintain a centered ball, while the nose attitude is adjusted to maintain 15 units angle of attack. In addition, you will need to reduce power when you roll your wings level to compensate for the increased lift.

Always keep in mind your glideslope position (i.e., meatball position), your vertical speed (noted from the VSI and the rate of movement of the meatball), and your AOA. Try to determine a reference power level that will maintain you on the glideslope. The location of the left and right power gauges is shown in Figure A-5, while Figure A-10 shows approximate optimum, maximum, and minimum values. Also, note on Figure 10 that each minor hash mark (small gauge, top center) represents 1 percent of power and major hash marks represent 10 percent of power. A reference power of about 83 percent should work well. Lead corrections with power (except as noted in 2)c) below); changes of 2 percent to 4 percent should be sufficient. Certainly do not go above 90 percent or below 75 percent. Follow with small pitch changes to correct or maintain AOA. An 8.5° pitch up is correct; and corrections for AOA should not require pitch movements to below 7° or above 10° (the dot on the attitude indicator corresponds to 1°). Greater changes than that will indicate that you are overcontrolling in pitch.

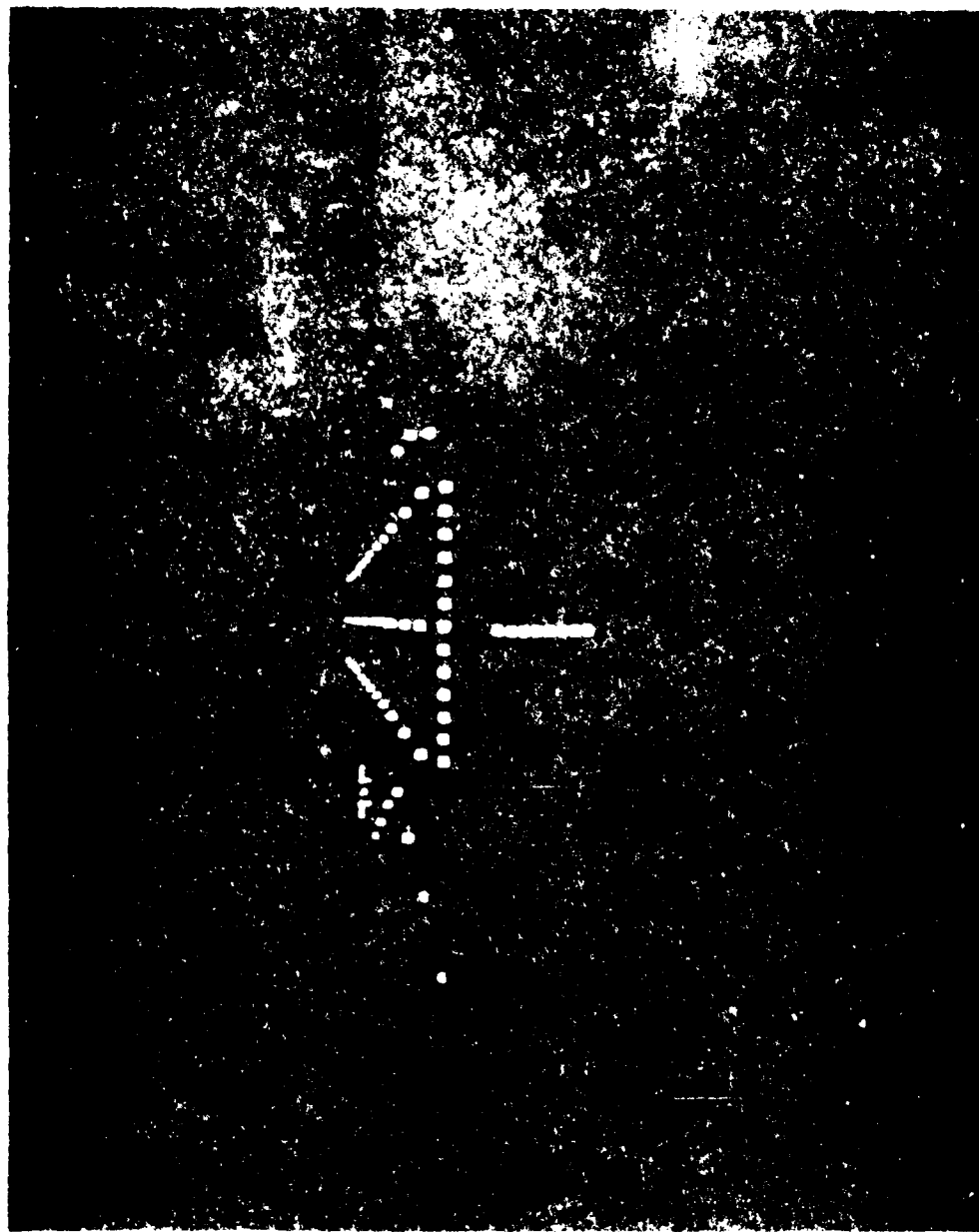


Figure A-9. Computer-Generated Image of the Right Carrier, with HOLS, showing an On-centerline view. A Centerline-dropline Configuration Such as - Indicates the Need to Fly to the Right While One Such as - Indicates the Need to Fly to the Left.

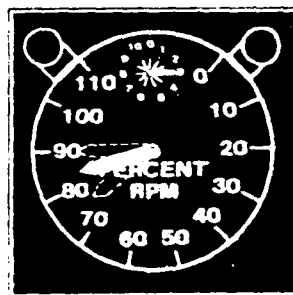


Diagram of T-2C power gauge, showing hash marks - note the needle at 83 percent, and dotted needles at 90 percent and 75 percent.

Figure A-10. Power Gauge (Both Left and Right Gauges are Identical).

Remember that corrections are almost always started with a power adjustment and AOA errors should generally be corrected before glideslope errors (except as noted in 2)c) below). The power adjustments for a correction will be made in three (and sometimes four) steps. First increase or decrease power to initiate the correction. Secondly, take out the correction as you approach the correct AOA or glideslope position. In taking out the correction go past your reference power to null any acceleration or unwanted velocity component that you have introduced in the first step. The third step: to return the power to its reference level, follows the second step almost immediately.

If you need to make a large power correction for a glideslope error, you may find it necessary to insert another power adjustment between the first and second steps. After the initial correction you should look for a target descent rate that will return you to the glideslope quickly enough, but not so quickly that you will not be able to stop on the glideslope. You may achieve the target descent rate before you near the glideslope. If so, you should take out some of your power correction (probably about half) so that you do not go past your target descent rate. Specific types of errors are discussed below.

1) AOA errors

If on glideslope and correct vertical speed,

- a) high AOA (slow): add power, smoothly push the stick forward (slightly) to correct AOA; as aircraft accelerates reduce power to slightly less than reference level, and then almost immediately adjust back to reference level.
- b) low AOA (fast): decrease power, smoothly pull stick (slightly) to correct AOA; as aircraft decelerates increase power to slightly higher than reference level and then almost immediately decrease power to reference level.

2) Glideslope errors

Note that if your AOA is correct and you add power to make a glideslope correction, you will need to pull the stick back slightly to maintain the correct AOA (because with the same stick pressure the extra surge of power will push the aircraft a little faster and tend to lower its attitude). If you decrease power you will need to push the stick forward slightly to maintain AOA.

- a) Going high: decrease power (if AOA is low the decrease in power will tend to correct the AOA error before it corrects the glideslope error; otherwise you need to push the stick forward). When you see that you have started back to the glideslope add about half the power you have taken out. As you near the glideslope add more power so that the power level is now slightly above the reference level. Almost immediately reduce power to the reference level.
- b) Going low: increase power to start the ball moving up (if AOA is high, the increase in power will tend to correct the AOA error, but let the ball start moving up before you ensure that AOA is closing on the correct value). When you see that you have started back to the glideslope, take out about half the power you have added. As you near the glideslope take out more power so that the power level is now slightly below the reference level. Almost immediately increase power to the reference level.
- c) Correcting for a low or a high in close (less than 1000 ft from touchdown): for a low add power to start the ball moving up. Stop the ball moving up by adjusting the pitch (this is the only time that pitch should lead power in making an adjustment). Use power to get back on speed.

If the ball is moving up in close or has stopped with a high indication in close (either as a result of an overcorrection from a low, a slightly low descent rate from farther out, or for some other reason), do not recenter.

A correction at this point can lead to an excessive descent rate at touchdown (correction for a high ball in close can produce a 5° glideslope). If the ball develops a rapid motion towards the bottom of the lens, apply enough power to stop the movement.

LANDING SIGNAL OFFICER

In real carrier approaches a Landing Signal Officer (LSO) is stationed to the side of the landing deck and advises the approaching pilot by radio on the suitability of his approach. He may, for example, advise the pilot that he is high or low, or to the left or right. He may give instructions, such as "POWER" to indicate that the pilot should add power. He may instruct the

pilot to discontinue his approach, and go around to set up for another approach by flashing two vertical light arrays on the FLOLS and calling "WAVEOFF".

The role of the LSO is also instructional, in that he will make a record of the pilot's performance, and use this in a debrief to point out errors, and to advise him on how to improve his approaches.

In this experiment we have a computerized LSO to give selected calls during the approach. The calls are listed in Table A-1, together with the type of error that will evoke the call, and the corrective action required.

The instructional role of the LSO will be filled by an experimenter who has been trained by an LSO for this task. He will comment on the significant features of your approach at the end of each trial, and will suggest ways to improve. These suggestions will not cover new material. Anything that should be explained to you already has been explained. The LSO - experimenter's comments will be taken from this briefing, and will serve to remind you of the material covered, and to orient you towards the errors that you are making and the appropriate corrective action. Common terminology that might be used during these instructions is shown in Table A-2.

SUMMARY

It requires care and effort to learn the control techniques for carrier landings. Navy pilots complete more than 100 approaches in a simulator or to a shore-based landing strip before they attempt a carrier landing. Our research indicates that even after hundreds of carrier landings pilots continue to improve their glideslope control. We will be measuring your performance throughout the trial, not just at the deck of the carrier. Follow the recommended procedures, and in particular try to set yourself on the glideslope, and with the correct AOA early in the approach. Your errors along the glideslope will be assessed. Avoid the temptation to correct by leading with pitch adjustments. Also avoid the temptation to trap a wire at all costs. If you are high as you approach the wires, accept it. A sudden dive for the deck at this point will downgrade your overall rating for that approach more than will a bolter. You should approach the task with care and perseverance. Review this lesson, and note the feedback during the trials. There is something to learn from even a bad performance.

TABLE A-1. LSO TRANSMISSIONS, THEIR MEANING,
AND REQUIRED CORRECTIVE ACTION

<u>TRANSMISSION</u>	<u>MEANING</u>	<u>REQUIRED RESPONSE</u>
"YOU'RE A LITTLE HIGH"	A/C is between .5 and 1.5 meatballs above glideslope.	Adjust altitude to a centered meatball immediately.
"YOU'RE HIGH"	A/C is 1.5 meatballs or more above glideslope.	Reduce power and adjust altitude to a centered meatball immediately.
"YOU'RE GOING HIGH"	A/C is less than .5 meatballs above glideslope and sink rate is less than 60 ft/min.	Reduce power and re-establish rate of descent.
"YOU'RE A LITTLE LOW"	A/C is between .5 and 1 meatball below glideslope.	Maintain current altitude until glideslope is intercepted.
"YOU'RE LOW"	A/C is more than 1 meatball below glideslope.	Add power and adjust altitude to a centered meatball immediately.
"YOU'RE GOING LOW"	A/C is less than .5 meatball below glideslope and sink rate is greater than 660 ft/min.	Add power and re-establish rate of descent.
"A LITTLE POWER"	A/C is between .5 and 1 meatball below glideslope.	Add 1 to 2 percent power to adjust altitude to a centered meatball immediately.
"POWER"	A/C is more than 1 meatball below glideslope	Add power to adjust altitude to a centered meatball.

or

A/C is in-close, more than .5 meatballs below glideslope and sink rate is greater than 480 ft/min.

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TABLE A-1. LSO TRANSMISSIONS, THEIR MEANING,
AND REQUIRED CORRECTIVE ACTION (cont'd)

<u>TRANSMISSION</u>	<u>MEANING</u>	<u>REQUIRED RESPONSE</u>
"MORE POWER"	Response to an initial "power command" was inappropriate.	Add more power.
"YOU'RE FAST"	Angle of Attack is less than 13 units and sink rate is between 210 and 390 ft/min.	Correct Airspeed/Angle of Attack Indication.
"YOU'RE SLOW"	Angle of Attack is greater than 16 units and sink rate is between 480 and 660 ft/min.	Correct Airspeed/Angle of Indication with power addition.
"FLY THE BALL"	A/C is "in-close" and more than 1 ball above glideslope.	Fly and use the meatball for rate/altitude information.
	or A/C is "in-close" and sink rate is less than 210 ft/min.	

TABLE A-2. COMMON TERMINOLOGY

1. The 180° position - A position on the downwin leg where the initial turn onto the base leg is commenced.
2. The 90° position - A position reached halfway along the 180° arc from the "180" to the landing line.
3. Final approach - That portion of the pattern flown from the sighting of the meatball to touchdown.
4. Groove - That portion of the final approach which coincides with the landing line. It commences upon rolling the wings level with the aircraft on the landing line and allows for approximately a 18-25 second straightway.
5. Cocked up - Flying too slowly or at too high an angle of attack, causing the use of excessive power to maintain altitude or rate of descent. This is a condition that exists when operating on the back side of the power curve.
6. Dive for the deck - Pushing the nose over and establishing an excessive rate of descent. This causes either a three-point landing (all gear hitting the deck at the same time) or possible nose wheel first.
7. Ramp - The after end of the flight deck or the downwind end of the platform of the runway.
8. Bolter - A touchdown onboard the carrier in which the arresting hook does not engage an arresting wire, usually caused by landing past the wire area or by the hook's skipping over the arresting wires.
9. Meatball - Terminology used to describe the mirror presentation of the source lights as seen by the pilot.
10. Clara - A term used to signify that the meatball has not been sighted.

APPENDIX B

ANALYSIS OF VARIANCE SUMMARIES
FOR TRAINING TRIALSTABLE B-1. ANALYSIS OF VARIANCE FOR TOUCHDOWN
WIRE ACCURACY TRAINING SCORES

<u>Source of Variance</u>	<u>LEVELS</u>		<u>df</u>	<u>Mean</u>		<u>F</u>
	<u>High</u>	<u>Low</u>		<u>Difference</u> ¹		
Field of View	Wide	Nar	1	5.8	(3.7) ²	4.0
Scene Detail	Day	Night	1	3.7	(1.5)	1.65
Motion	On	Off	1	-2.9	(-)	1.03
Approach Type	St. In	Circ	1	5.9	(3.8)	4.20
FLOLS Rate Cue	Cuing	No Cue	1	1.2	(-)	0.16
Turbulence	Calm	Winds	1	6.4	(4.5)	4.96*
Pilot Type	Nav P-3C AF T38		1	-6.9	(5.1)	5.63*
FOV x App. Type			1		(-)	0.04
S.DTL x App. Type			1		(-)	0.11
2-Factor Int (No Pil) ³			7)Re-		(5.5)	0.70
2-Factor Int (Pil) ⁴			6)sid-		(4.1)	0.61
2+3 Way Strings			9)ual		(10.2)	
Blocks (10 Trials)			3		(30.4)	32.32**
2-Factor Int (Blocks)			21		(7.5)	1.13
3-Factor Int (Blocks)			72		(22.6)	
Grand Mean				27.0		
Std. Err. Difference				2.9		
Std. Deviation				8.1		

¹Mean of observations taken under high level minus mean of observations taken under low level of factor.

²Values in parentheses are percent variance accounted for in the experiment. Percents less than 1.0 are shown by a dash (-).

³Two-factor interactions not involving pilot type.

⁴Two-factor interactions involving pilot type.

*p < .05

**p < .01

TABLE B-2. ANALYSIS OF VARIANCE FOR GLIDESLOPE TRACKING TRAINING SCORES

Source of Variance	LEVELS		df	Mean		F
	High	Low		Difference ¹		
Field of View	Wide	Nar	1	4.8	(3.1) ²	1.97
Scene Detail	Day	Night	1	3.6	(1.6)	1.03
Motion	On	Off	1	0.0	(-)	0.00
Approach Type	St. In	Circ	1	9.4	(11.9)	7.48*
FLOLS Rate Cue	Cuing	No Cue	1	-3.7	(1.8)	1.11
Turbulence	Calm	Winds	1	3.2	(1.3)	0.83
Pilot Type	Nav P-3C	AF T38	1	-5.2	(3.7)	2.30
FOV x App. Type			1		(-)	0.25
S.DTL x App. Type			1		(-)	0.41
2-Factor Int (No Pil) ³			7)Re-		(6.5)	0.42
2-Factor Int (Pil) ⁴			6)sid-		(8.6)	0.65
2+3 Way Strings			9)ual		(19.8)	
Blocks (10 Trials)			3		(17.7)	23.52**
2-Factor Int (Blocks)			21		(5.0)	0.96
3-Factor Int (Blocks)			72		(18.0)	
Grand Mean				27.1		
Std. Err. Difference				3.3		
Std. Deviation				9.4		

¹Mean of observations taken under level minus mean of observations taken under low level of factor.

²Values in parentheses are percent variance accounted for in the experiment. Percents less than 1.0 are shown by a dash (-).

³Two-factor interactions not involving pilot type.

⁴Two-factor interactions involving pilot type.

*p ≤ .05

**p ≤ .01

TABLE B-3. ANALYSIS OF VARIANCE FOR LINEUP TRACKING TRAINING SCORES

Source of Variance	LEVELS		df	Mean Difference ¹		F
	High	Low				
Field of View	Wide	Nar	1	-2.7	(-) ²	0.14
Scene Detail	Day	Night	1	4.4	(-)	0.35
Motion	On	Off	1	-0.8	(-)	0.01
Approach Type	St. In	Circ	1	15.3	(10.4)	4.23
FLOLS Rate Cue	Cuing	No Cue	1	0.0	(-)	0.00
Turbulence	Calm	Winds	1	-1.8	(-)	0.06
Pilot Type	Nav P-3C AF T38		1	10.9	(5.3)	2.15
FOV x App. Type			1		(-)	0.14
S.DTL x App. Type			1		(-)	0.07
2-Factor Int (No Pil) ³			7)Re-		(16.1)	0.79
2-Factor Int (Pil) ⁴			6)sid-		(11.7)	0.67
2+3 Way Strings			9)ual		(26.3)	
Blocks (10 Trials)			3		(5.2)	8.04**
2-Factor Int (Blocks)			21		(7.7)	1.71
3-Factor Int (Blocks)			72		(15.4)	
Grand Mean				51.0		
Std. Err. Difference				7.3		
Std. Deviation				20.6		

¹Mean of observations taken under high level minus mean of observations taken under low level of factor.

²Values in parentheses are percent variance accounted for in the experiment. Percents less than 1.0 are shown by a dash (-).

³Two-factor interactions not involving pilot type.

⁴Two-factor interactions involving pilot type.

*p < .05

**p < .01

TABLE B-4. ANALYSIS OF VARIANCE FOR ANGLE OF ATTACK TRACKING TRAINING SCORES

Source of Variance	LEVELS		df	Mean		F
	High	Low		Difference ¹		
Field of View	Wide	Nar	1	6.0	(4.2) ²	3.91
Scene Detail	Day	Night	1	4.5	(2.4)	2.21
Motion	On	Off	1	3.0	(1.0)	0.97
Approach Type	St. In	Circ	1	8.0	(7.2)	6.80*
FLOLS Rate Cue	Cuing	No Cue	1	-0.9	(-)	0.11
Turbulence	Calm	Winds	1	5.2	(3.0)	2.85
Pilot Type	Nav P-3C	AF T38	1	9.8	(10.9)	10.24**
FOV x App. Type			1		(-)	0.02
S.DTL x App. Type			1		(1.2)	1.17
2-Factor Int (No Pil) ³			7)Re-		(4.4)	0.52
2-Factor Int (Pil) ⁴			6)sid-		(8.1)	1.13
2+3 Way Strings			9)ual		(10.9)	
Blocks (10 Trials)			3		(17.0)	18.49**
2-Factor Int (Blocks)			21		(7.6)	1.18
3-Factor Int (Blocks)			72		(22.0)	
Grand Mean				41.8		
Std. Err. Difference				3.2		
Std. Deviation				8.9		

¹Mean of observations taken under high level minus mean of observations taken under low level of factor.

²Values in parentheses are percent variance accounted for in the experiment. Percents less than 1.0 are shown by a dash (-).

³Two-factor interactions not involving pilot type.

⁴Two-factor interactions involving pilot type.

*p < .05

**p < .01

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