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AFOSR-TR- 89-1636

# THE HUMAN OPERATOR IN ADVANCED AEROSPACE SYSTEMS

A Planning Report Prepared for the
Director of Life Sciences
Air Force Office of Scientific Research
Bolling Air Force Base
Washington, DC

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December 15, 1981

THE HUMAN OPERATOR IN ADVANCED AEROSPACE SYSTEMS

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## FOREWORD

This report was prepared under United States Air Force contract F49620-78-C-0075, with Wayne State University, Detroit, MI, and a second contract F49620-79-C-0090, with Stevens, Scheidler, Stevens, Vossler, Inc. (SSSV), Dayton, OH. Both contracts were entitled, "Evaluation of Research in the Life Sciences". The second contract was completed under a subcontract from SSSV to General Physics Corporation. The principal investigator was aided by part-time faculty member, Dr. Charles E. Hutchinson and Dr. Frederick A. Muckler, consultant.

The report represents approximately five man-months of effort -- three by Dr. Hutchinson and two by the principal investigator. Dr. Hutchinson was responsible for all chapters except the one on human factors engineering, the one on management of research, and corresponding parts of the Executive Summary. Dr. Christensen wrote the chapters on human factors engineering and on management of research. Dr. Muckler co-authored the chapter on management of research and contributed significantly to the entire report.

We were indebted to several individuals who gave freely of their time to discuss the nature of the research areas to which it is anticipated that AFOSR/NL will give special attention in the near future. Special mention should be made of Dr. A. J. Cacioppo, Chief Scientist of the Air Force's Foreign Technology Division; Dr. Bryce O. Hartman, USAF School of Aviation Medicine; and Dr. Earl A. Alluisi, at time of interview, Professor of Psychology at Old Dominion University and currently Chief Scientist at the Air Force's Human Resources Laboratory. Responsibility for the contents, however, is solely the responsibility of the authors. Ms. Cathy Gaddy assisted with the editorial work.

No attempt has been made by the authors to force complete agreement on all issues. Thus, occasionally, some overlap, or even contradictions, may appear from chapter to chapter. It was felt by the Principal Investigator that complete candidness should be the watchword for both writers; there are already too many committee reports that almost invariably express a "unaminity of opinion" -- which usually means that one or a few committee members were successful in persuading the others that theirs was the opinion that the committee should promote. This is not that kind of report.

Finally, we acknowledge the wholehearted administrative and scientific support and encouragement of Dr. Alfred R. Fregly, Program Manager, Life Sciences Directorate, and sympathetic monitor throughout the effort.

Julien M. Christensen, Ph.D.
Principal Investigator

THE OPERATOR IN ADVANCED AEROSPACE SYSTEMS

# EXECUTIVE SUMMARY

Julien M. Christensen, Ph.D.

Charles E. Hutchinson, Ph.D.

## EXECUTIVE SUMMARY

### PURPOSE OF THE REPORT

The purpose of this report is to provide the Director of Life Sciences, AFOSR, with the results of an effort to elucidate the roles of the human operators in advanced aerospace systems, to consider the implications of such roles for manning, training, human factors engineering, and operator performance, and to provide recommendations regarding directions for future research in the behavioral and social sciences.

The requirement for improved long-range planning for basic research in the field of human performance, as it relates to the advanced systems that will enter the Air Force inventory in the next ten years, was emphasized by Colonel Henry R. Taylor at the DDR&E program review at AFOSR on July 6, 1976. In response to his recommendation, a paper was prepared in the Life Sciences Directorate (AFOSR/NL) that acknowledged the need for a longer-range view in planning the Air Force research program and proposed a planning effort to accomplish this goal. The report is an attempt to provide the type of guidance that, if implemented, might close the gap noted in the 1976 review.

## THE NATURE OF WARFARE IN THE YEAR 2000 PLUS.

Military planning is carried out in the context of assumptions with regard to the situations for which military force should provide an effective defense or solution. We have provided assumptions that may serve as the basis for considering the human factors requirements important to the future development of air power. These assumptions are not free of the biases of the authors, and should be made the object of serious study aimed at developing better and more serviceable understandings about possible world futures and the role of military power in achieving national objectives.

In anticipating the nature of warfare in the years beyond the turn of the century, we have postulated that there will be a continuation and improvement of most of the systems that are presently in the military inventory of the U. S. There will be improvements in precision guided missiles (PGM's) so that weapons can be directed to selected targets with minimal damage to peripheral non-military structures and personnel. We anticipate that defensive weapons will be pursued to the point where some, if not most, offensive weaponry may become of doubtful value. We expect that satellites will have a greatly enlarged role in the surveillance of enemy activities and in the control of the order of battle. We believe that there is a significant opportunity for the improvement of military command and control through the enhancement of commander training and improved human factors engineering.

MANPOWER ISSUES -- PERSPECTIVE, THE MANPOWER BIND AND THE ALL-VOLUNTEER FORCE

In these sections we have tried to indicate the serious nature of manpower limitations for the Air Force, as compared to the difficulties faced by the less technologically dependent services in the U. S. and foreign countries. Under present circumstances, and in the forseeable future, the Air Force should be less influenced by the shortage of enlistments in the regular military force. The critical need of the Air Force is not basic airmen. These people, men and women, should be available in close to required numbers. The manpower bind is the recruitment of an adequate number of career-oriented people, qualified for professional status, who will elect the Air Force as their first career choice. It is predicted that during the next twenty-five years the Air Force will suffer a serious shortage of people with appropriate professional quality and aspirations.

We see a special problem with regard to the Air National Guard and Air Force Reserve units. These organizations are now supplied with well-motivated and highly competent people who have had training superior to that of many of the people entering active duty as regulars. The new acquisitions will in time become as good as the Vietnam veterans, but by that time the veterans will be leaving the Guard and Reserve units. The replenishment of these organizations is of very great importance because one of the ways that the regular Air Force has been reduced in size is through the transfer of front-line missions to the Guard and the reserve. If these organizations lack qualified and sufficient manpower to perform their missions, there will be no regular organizations capable of picking up essential responsibilities. It is important that their effectiveness be maintained.

Science, technology, engineering, and medical support services will share the same shortages of people as those projected for rated and unrated operational officer personnel. We note that recent program guidance in the area of the behavioral and social sciences has regularly downgraded the importance of research aimed at anticipating and resolving manpower issues.

THE OPERAT .N ADVANCED AEROSPACE SYSTEMS: IMPLICATIONS FOR TRAINING

New aircraft which will enter the inventories of the United States and foreign air forces will incorporate advanced concepts in aeronautical design and avionics. Avionics systems have grown to the point where they generally account for one-third of the cost of an Air Force weapon system, with propulsion, air frame, and weaponry accounting for the remainder. And these costs may well escalate. Airborne systems will have new characteristics of flight, new levels of maneuverability and will be capable of generating "G"-forces that could make an on-board operator either a serious constraint on system performance or a part of the man-machine system that might become inoperative at critical points in the mission profile.

When fully developed, the new systems should prove easier to control because the avionics systems will perform the manual and psychomotor functions of flying. In addition, improved integration of information on prime targets and on possible alternatives will be routinely available. The operators' roles with respect to those advanced systems should be at least as challenging as the tasks now performed by the combat pilot. Xenophobia may induce a certain reluctance on the part of rated officers to encourage rapid movement of the Air Force into the age of pilotless, but operator-managed, airborne weapon systems. However, the requirement for team work and effective decision making in real time will create even more challenging jobs for those airmen who have the capacity to perform complex operations rapidly in stressful situations.

Several research thrusts are identified with respect to areas where important gaps in knowledge exist in the behavioral sciences. Among the needed research tasks, the following are discussed: (1) future Air Force strategies and mission responsibilities must be identified, (2) profiles of future aerospace missions must be specified, and (3) operator career job and task functions must be described.

With respect to a needed computer-based taxonomy of aerospace operator functions, the following opportunities should be exploited: (1) an aerospace operator task information data bank must be developed, and (2) a systematic method of translating operator functions into training task specifications must be developed. Research attention should be directed toward the allocation of Air Force training responsibilities between institutional agencies such as the Air Training Command and the operational agencies which in the future must share a larger part of the responsibility for training operators on the job in much the same fashion that transition and currency training now involve operational commands. The need for training on the job is enmeshed in the need for continuous practice and innovations in team and crew performance that take full advantage of the flexibility that will be built into future weapons systems.

Key advances in training technology are required to meet the education and training requirements for the aerospace systems of the year 2000 and thereafter.

THE OPERATOR IN ADVANCED AEROSPACE SYSTEMS: IMPLICATIONS FOR HUMAN FACTORS ENGINEERING

Because of the diversity of its operations, it is virtually impossible to think of a human performance area for which Air Force support could not be justified. However, priorities must be established, and it is the intention of this chapter to help in this regard.

It is posited that the computer will have an increasingly profound effect on military systems design. In order to assure maximum man-machine compatibility and effectiveness in computer-based systems, it is essential that more be learned about man's perceptual/cognitive capabilities -- information retrieval and processing, multi-channel processing, aids to decision-making, etc. In fact, a careful analysis of the requirements for the establishment of an optimal relationship between man and computer might well form the core of a very effective 6.1 program.

The neuropsychological area is another area currently showing considerable promise. Research in this area should not only help assure greater man-computer-systems effectiveness, but may also yield clues that will lead to the design of more effective computers. We cite the work of O'Donnell in EP, and of Klopf at the level of the neuron, as two areas that we feel are particularly worthy of support.

We encourage more joint research with carefully chosen operations research specialists. We feel that this may be an avenue that will lead to better mathematical description of selected aspects of man's behavior in complex systems. This, in turn, would enhance compatibility with the other sciences that contribute to systems development.

A careful review of programs dealing with space operations is needed to assure that forseeable future Air Force requirements are being met. We suggest the areas of multi-stress effects and small team operations as two that should be assessed for adequacy of current support.

We strongly favor directed reviews of significant areas and the development in selected areas of what Bray, 20 years ago, termed "technologies of behavior." Such a program would yield not only immediate benefits but also would serve as a valid means of identifying areas that are most in need of basic research support.

In line with our plea for greater creativity in the human factors area, we recommend that small amounts of virtually unrestricted funds be awarded each year for a minimum number of years to a few carefully selected scientists of proven creative capability. We feel, too, that the program would benefit considerably from year-long periods of residency and acquaintanceship by outstanding foreign ergonomists. After examining several committee reports, it appears that something akin to a "party line" is developing. We may, subconsciously, even be a part of it. A fresh look by foreign authorities could be quite beneficial.

Special attention must be given to the development of better tools and methods in the human factors area. Investigators should give at least as much attention to the sampling of situations as to the sampling of subject populations. Models must be developed to replace the linear process models that repeatedly have demonstrated their inadequacies with respect to the phenomena of the behavioral and biological sciences.

Finally, our universities are turning out a generous supply of fine young Ph.D.'s, many of whom may have trouble finding positions suitable to their training and talents. We feel that AFOSR, specifically, and the Air Force, generally, would be richly rewarded were it to amplify plans that would provide appropriate opportunities for this wealth of talent and source of future Air Force creativity.

We develop and try to support the thesis that, in general, there is much more danger of overmanaging than undermanaging basic research programs. Relinquishing tight control, however, does not relieve the manager of significant responsibility; it simply changes the nature of that responsibility.

After selecting the areas that are most in need of research, the manager must make the decision as to who will conduct the research. This is, without doubt, the most important decision that he makes. After selecting the principal investigator (PI), the manager has an obligation to relieve the PI of as much administrative detail as possible, to provide sustained support as long as necessary, and to assure that the PI obtains appropriate rewards in addition to money.

Finally, basic research money is such a scarce commodity that it should never be diverted to other purposes. To use it, for example, to support nothing but a series of applied problems in need of immediate solution is a perversion. The great breakthroughs of the future in human factors will very likely come from some basic researcher in the area and it is not unlikely that the breakthrough will be in an area other than that for which he had contracted to work. Garner good investigators in appropriate areas and give them encouragement, but don't over-direct them.

## IMPLICATIONS OF FOREIGN HUMAN FACTORS RESEARCH

The information for this section was acquired during an interview with Dr. Anthony J. Cacioppo, Chief Scientist of the Foreign Technology Division, AFSC. Dr. Cacioppo was able to provide the perspective of an outstanding behavioral scientist who also has the responsibility for technical direction of an important research and analysis agency with competence in those scientific and engineering fields that relate to national security.

Air Force research requirements in the behavioral and social sciences are broader than the discipline of psychology. The knowledge needed by the Air Force in preparing for the next twenty-five years will be produced through inter-disciplinary efforts that use the expertise, theories and methods of psychology, sociology, operations research, mathematics, engineering, physics, chemistry, physiology, and related disciplines to attack the important human factors problems that relate to the development and use of advanced aerospace systems.

The most pressing problems facing the Air Force that merit vigorous research attention are ranked in order of priority as follows:

1. The information processing capacities of the human operator.
2. Human decision-making in the context of advanced military systems and operations.
3. Research in the social sciences in terms of development of more realistic scenarios for training commanders in the exercise of management functions in realistic military engagements.
4. Expanded research in the behavioral and social sciences and, in the opinion of the Chief Scientist, FTD, research aimed at the improvement of productivity of Air Force personnel and organizations.

# I. THE ROLE OF PLANNING IN HUMAN PROGRESS

CHARLES E. HUTCHINSON, PH.D.

# THE ROLE OF PLANNING IN HUMAN PROGRESS

> Things are not well in the world today in the most direct and simple sense of the word. Hunger and death threaten the majority of men. That is why the first goal of human progress must be to put an end to those dangers, and any other approach would be unforgivable snobbism. Yet I am not prone to insist on the technological and material side of progress. I am certain that the 'super goal' of human institutions, and that includes progress, is not only to protect those born on earth from excessive suffering and early death but also to preserve in mankind all that is human: the joy of spontaneous work with knowing hands and a knowing mind, the joy of mutual help and of good relations with people and nature, and the joy of learning and art. I do not believe the contradictions in these goals to be insurmountable. Even now, citizens of the more developed, industrialized countries have more opportunities for a normal healthy life than their contemporaries in the more backward and hungry countries have. And in any case, progress that will save people from hunger and disease cannot contradict the source of active good, that which is most humane in man.
>
> I believe that mankind will find a rational solution to the complex problem of realizing the grand, necessary, and inevitable goals of progress without losing the humaneness of humanity and the naturalness of nature.[1]

The goal of military planning is to assure that the military establishment is prepared at all times to support national policy objectives which, at the highest common denominator, should be compatible with the end of human progress stated by Sakharov. Military plans that confound the accomplishment of national political, social, and cultural objectives can be a source of disonnance or a blueprint for disaster. It is with recognition of the obligation of the military planner to facilitate the achievement of national goals on the broadest range of human values that we have undertaken the present task: To consider the role of the human operator in advanced aerospace systems, and to propose research that will contribute to the accomplishment of the aerospace mission as one approach to safeguarding the future so that the humane objectives can be achieved. The degree to which the

[1]Sakharov, 1974.

United States realizes its national objectives during the next twenty-five years will depend to a considerable extent on the achievement of a stable world environment that does not threaten the social, economic, and political interests of the nation. The responsibility for maintaining the conditions necessary for the orderly development of national institutions is an awesome task but that is what military planning and programs are all about.

## II. THE WORLD OF 2000 PLUS

CHARLES E. HUTCHINSON, PH.D.

The Nature of Warfare in the Year 2000 Plus

Manpower Issues - Perspectives

The Manpower Bind and the All-Volunteer Force

THE NATURE OF WARFARE IN THE YEAR 2000 PLUS

One can anticipate the retention and refinement of present military systems for destroying enemy capacity for attack and destruction of population clusters. There may be advances in Precision Guided Missiles (PGMs) to a point where operations can be directed toward military objectives with minimal loss of life for enemy and friendly populations. There is an expectation that geo-technological warfare methods will be available for impairing an enemy's subsistence, impeding his mobility on the ground or in the air.

An expanded role for satellites can be anticipated. They should continue to provide optical and electronic surveillance, and expanded surface communications capability. But a major extension should be in the form of battlefield and theater command, control, and communication centers. The role of the satellite command post can be appreciated when it is realized that a battlefield will be of such a level of lethality from heat, radiation, and blast that no one would enter this arena with the expectation of leaving alive.

The weapons of the future will make it possible to plan for "area" defense. The development of systems that can create a curtain of anti-missile missiles having a kill potential against all incoming weapons may be realized. Available weapon systems coupled with effective all weather warning and operational guidance centers make it possible to launch PGMs with assurance that incoming weapons will be destroyed outside the defense perimeter. This development makes is possible to forecast the creation of "safe" areas.

The possession of weapon systems that are continental in target coverage provides the base for exerting leverage in dealing with the leaders of an enemy nation. The possibility exists of holding a nation hostage by means of an ultimatum or threat to eliminate selected elements of the enemies' war-making potential, the industrial base, agricultural production, or people. Such a threat or ultimatum might depend on a combination of weapons such as chemical, devices to paralyze enemy electronic communications, PGMs, and methods of geo-technological warfare, as well as the traditional inter-continental weapons.

It is anticipated that each of the super powers will pursue the technological advances that will give them elements of a system that might be used in combination as a "hostage" weapon. The creation and exploitation of a combined weapon confrontation with an enemy would be dependent upon confidence that the system would function effectively if the enemy were to challenge the threat or ultimatum. If a preponderance of force were possessed by one super power, it is conceivable that it might selectively disarm or cause its opponent to dismantle its offensive or defensive arms. One must entertain the possibility that both super powers may move toward the acquisition of a "hostage" weapon system so closely in step that neither nation is willing to accept the risks entailed in using arms that may severaly damage the enemy while at the same time altering the world environment or creating damage to the aggressor that would be too serious to accept.

This does not leave us precisely where we are at present. When our arsenal includes point defense, area defense, and/or perimeter defense weapons we might (and our adversaries might) feel less reluctance to run the risk of an all-out weapons exchange; but there would be the restraint of not wishing to launch one's best weapons if there were doubt with respect to their efficacy. This situation might encourage an attempt at major power collusion to administer the rest of the world in a manner to avoid confrontation and to maintain spheres of influence in a less competitive manner than presently prevails.

Whatever one's expectation of the nature of the major power confrontation in the future, and of the weapons acquired to sustain the rivalry, there are some hard requirements for the United States in trying to maintain a credible defense in the world we have briefly sketched.

The human factors elements are fairly clear. The operator in aerospace systems will not be in the theater of operation; the battlefield will be of such a level of lethality that a human being could not survive the experience and he would become useless before other elements of his weapon. His contribution as a passenger in an automatically programmed and enemy weapon-seeking vehicle would be negative. He would be out of commission before he found out what he could do to improve the situation. The human operator will be located elsewhere than in the vehicle(s) that he directs or supports. He will need a different set of skills than those taught in flight training, whether by simulator or flight trainer. The characteristics used in selecting personnel will also be different. To be a successful pilot one must have both psychomotor aptitudes and intellectual or cognitive skills. In the future, emphasis will swing away from the psychomotor abilities. Cognitive skills will predominate in terms of intellectual traits. However, other talents are urgently required. High sustained motivation to perform routine tasks will be essential. Mackworth pioneered work in the field of vigilance, and effort in this field has been capped by an exceptionally thorough review by the Office of Naval Research ("Vigilance" - Edited by Robert Mackie, 1976). The problem with the application of the research on watch-keeping is that one finds no correlation with intelligence, and none was required when the watch-keeper had a simple stimulus-response task to perform. We propose that watch-keeping in the future will require some complex and evaluative responses; if not, the system will be automated. Not only will there be a need for high intellectual (officer) personnel engaged in continuous watch-keeping, but they must be selected from those most able to work effectively at any time of the day or night. We can depend on our adversary to launch his attack at a time most likely to catch our personnel "napping". The ability to wake effectively from a sleep state is not evenly divided in the population and does not correlate with test intelligence. Therefore, the study of circadian rhythms is of great importance, particularly as they correlate with other intellectual characteristics. The study of bio-rhythms, the longer range cycles of human personality and performance, does not appear to be a very promising avenue for research. Studies of athletes indicate no correlation between their bio-rhythmic state and their achievements.

Studies that are of high priority would include:

Circadian rhythms

Work-sleep scheduling

Personality type and acceptance of solitary or isolated team performance

Sustained motivation

Recruitment of high-intelligence operational (officer) personnel

Retention of officer personnel in occupations that involve low-stimulus environments

Extension of watch-keeping studies to include cognitive and motivational components of aerospace occupation.

MANPOWER ISSUES - PERSPECTIVE

Secretary of Defense James R. Schlessinger told the Subcommittee on Manpower and Personnel of the Committee on Armed Services of the United States Senate (August 13, 1974) that, "In recent years there has been a substantial downward adjustment of manpower. Our military forces are now some 40 percent below the Fiscal Year 1968 manpower level, which was the Vietnam high. We have reduced by 1,386,000 men. In addition - and this is a fact not widely appreciated - we are 525,000 men below the 1964 pre-war level. The military manpower of the United States has shrunk by some 20 percent as compared to pre-war strengths."

Focusing on the cost of maintaining an adequate defense, the Secretary said, "We should be careful to analyze whether . . . the forces we possess are enough before further shrinkages are driven by budgetary considerations."[1]

While the Army and Navy were experiencing difficulties in meeting manpower goals, the Air Force was close to its authorized strength. But costs were not reflecting the reductions in the size of the force. In 1968, Air Force Secretary McLucas reported that manpower costs were 34 percent of the Air Force budget. By 1974 they had risen to 46 percent.[2] Subsequently personnel costs have gone up another ten percent, and no halt in the inflation of manpower costs can be expected. Secretary McLucas described actions that had been taken to control personnel costs. Between 1968 and 1974, manpower was cut over 900,000 to a force level of 627,000 uniformed personnel for the 1976 fiscal year. The civilian force was also cut 20 percent from 1968 to 1974 with an end strength of 287,000 which included 13,000 military positions that had been civilianized. As a means to reducing costs, missions were transferred from the regular Air Force to reserve units which were increased in size as part of the Vietnam winddown.

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[1]Hearing before the Subcommittee on Manpower and Personnel of the Committee of the Armed Services, U.S. Senate Second Session, August 13-14, 1974, p. 5.

[2]Secretary James McLucas testimony to the Subcommittee on Manpower and Personnel, 93rd Congress, August 13-14, 1974, p. 141.

While force size was being reduced, operational capabilities were maintained or increased with regard to strategic, tactical, and air lift capabilities. General Jones, Air Force Chief of Staff, speaking to the same audience (Subcommittee on Manpower and Personnel of the U. S. Senate) said that the major operational deficiency was the air lift capability to move Army divisions and their equipment rapidly enough to support units pre-positioned in Europe. Aircraft under consideration such as the AMST (Advanced Medium Short Take Off Vehicle) and medium-range STOL (Short Take Off and Landing Vehicle) were recommended because of their ability to move equipment of any size needed to the front lines as opposed to the C-5 which requires large air fields. Subsequently, development of both craft was halted.

In 1975 the number of Air Force people was approximately as follows:

| | |
|---|---|
| Active Duty | 627,000 |
| Civilian | 287,000 |
| Air National Guard | 92,000 |
| Air Force Reserve | 50,000 |
| TOTAL | 1,056,000 |

Each of these components was near its assigned strength except for the Air Force Reserve, which was 3,000 short of its manning goal. At the same time that the Air Force was meeting its numerical objectives there was satisfaction with the quality of accessions. Ninety-three percent of Air Force enlisted recruits in 1974 were high school graduates. Operational Readiness Inspections (ORIs) for the Air National Guard and the Air Reserve indicated that overall they were in the satisfactory range and the deficiencies that were found were of a type that could be eliminated in an emergency call-up.

The consensus in regard to the performance of the Air National Guard and the Air Force Reserve personnel in Vietnam was positive, although some units were disgruntled by extensive re-training after mobilization. While the performance of the Guard and Reserve as units was acceptable, some evidence suggests that, man for man, in the piloting phases of operations, Guard and Reserve personnel may even be superior. The reason for this situation, if it exists, would be the presence of motivated and operationally experienced personnel in the back-up units, while the regular units have a larger infusion of trainees who are graduates of the undergraduate pilot training program and are a cross-section group in contrast with the combat and self-selected pilots in the Reserve and Guard. If there is any superiority of the reserves it should be transitory as the Vietnam veterans are replaced by less experienced pilots.

The picture we get of the Air Force in 1976 is of a competent military force in regard to strategic, tactical and air lift capability, backed up by potentially top rank personnel and units in the Guard and Reserve. The Air Force probably is unique as compared to the other military services in regard to the quality and operational readiness of Reserve and Guard units. The Air Force survived the early years of the All-Volunteer Force with personnel recruitment problems under control, but with the companion problem of rising costs for people and equipment of such a magnitude that the effort to maintain adequate personnel could threaten the acquisition of the systems needed to keep the Air Force in balance with its principal rival. Unlike the Army and Navy, the Air Force is coping well in terms of quality and quantity of recruits under the conditions created by the abandonment of the Selective Service System (SSS). The incentive of draft avoidance which operated under the SSS was replaced by the threat of joblessness at a time (June, 1978) when the nationwide unemployment rate for all teenagers was 14.2 percent with 38.4 percent of all black teenagers unemployed. Without public service jobs for unemployed and unskilled youth, the unemployment rate would have been significantly higher. A condition of full-employment (unemployment rate of 4.5 percent) would increase the difficulties of the Army and Navy and might force the Air Force to relax its standards to meet its manning needs.

# THE MANPOWER BIND AND THE ALL VOLUNTEER FORCE

Looking at the human factors problems that must be faced during the next quarter of a century and beyond, a single problem overshadows all of the rest. This is the difficulty of attracting enough people with the required learning ability and educational preparation in appropriate disciplines to operate and to manage the kinds of weapons that will be a part of the aerospace inventory. This problem is unrecognized outside of a limited intellectual circle made up of military planners and academic authorities. Our present and past experience has led to the acceptance of a strategic military manpower posture that puts reliance on a limited regular force supported by a strong mobilization potential to meet special threats. The surge capability to meet unanticipated dangers is to be provided by the National Guard and Reserves and for a more protracted confrontation by conscription which is not presently authorized.

A review of the current status of Air Force manpower led to a feeling of satisfaction on all major fronts. Entrants to the officer corps, enlisted personnel accessions (93% high school graduates), and the Air National Guard and the Air Force Reserve staffed by experienced veterans of the Vietnam War are great national security assets. While this situation should be a source of pride and satisfaction, it is not justification for complacency, because some of these conditions are transitory. It is clear that Vietnam veterans have a limited period of remaining utility on the one hand, and, on the other hand, that the requirement for comparably qualified professionals will change remarkably in the next twenty-five years. We do not challenge the statement "It is the business of the Air Force to Fly and Fight, and don't ever forget it." But one should not lose sight of those key members of the flying and fighting team who performed their indispensable functions at the time General Ryan spoke in subterranean missile control stations, inside Cheyenne Mountain, or at early working sites.

In the future, the occupations of professional Air Force combat officers will shift very strongly toward the highly professional, non-heroic duties that are similar to those in Air Defense Control Centers, in missile units of the Strategic Air Command and in high technology assignments in the Systems Command.

Morris Janowitz has pointed out that three roles are played by the modern military officer: he is a battlefield leader, daring ace or performer in some other heroic role; he is also a manager (the American military is one of the most efficient bureaucracies in the world); but third, and most critical for future aerospace operations, the military officer is a professional scientist/engineer whose occupation involves him in the rapid acquisition of high technology devices and their incorporation into a continuously ready military force. It is our opinion that General Ryan, in stressing the heroic aspects of Air Force performance, was reinforcing the image of an Air Force career that was attractive to precisely the kind of persons needed to man the Air Force in World War II, Korea, and Vietnam, and it is the kind of Air Force that is still flourishing in the Strategic, Tactical, and Logistic Commands.

The heroic mold of the flyer will survive in some form, but the occupational image of the professional combat airman is eroding. It took a world energy crisis to introduce simulation as the preferred mode of providing training and currency experience for pilots. The substitution of ground-based training for flying is only one aspect of the diminishing heroic role of the Air Force pilot.

A second limitation on the pilot role is inherent in the occupations of the missile control officer, the RPV manager, and other non-flying combat tasks.

A third and more subtle change in the pilot role is involved in the evolution of the pilotage job itself as a key element of a flying weapon or of a weapon control system. There is concern in the Flight Dynamics Laboratory, AFWAL, about the role that is appropriate for the human component in an airborne man/machine system. The current generation of advanced flight vehicles such as Air Force Technology Integration (AFTI) and the F-16, incorporates control-configured variable geometry as a basic characteristic. The changes in aerospace technology are of such a nature that "stick and rudder control" cannot be performed by the human operator without the intervention of automated sensors, mini-processors, and direct feedback servo-systems. The B-1 has four levels of redundancy in its automatic control systems. When the fourth system fails, one does not go to manual override; one hits the silk or something harder.

What this says is that the role of the flyer is changing so radically that the thrill of flight control will be transformed into the satisfaction of being a component in a pre-programmed, but highly maneuverable and marginally stable, weapon system. There is not yet a good job title for the airborne professional. Such terms as flight manager, flight director, or flight controller used to describe the new role for the human component are evidence of the changing occupational image.

Other winds of change are blowing. Contests are taking place among the system designers in regard to what controls of currently planned vehicles should be automated and what should be assigned to the human operator. There may come a point where tasks more appropriate for automation will be given to the human operator to justify his presence in the vehicle and to attract the kind of professional person who will be competent to perform the flight director funtions that are not susceptible to cost effective automation. With secure, long-range communications there would be no compelling reason, other than cost, to locate the human oprator of a weapon system as a passenger in the vehicle of which he was a significant control element. Therefore, as we look toward the year 2000, we predict a continuing decline of the heroic aspects in the career of professional combat airmen, and a significant increase in those facets of the career that demand science/engineering professionalism, and that may be performed in isolated, remote, underground, airborne, or orbiting control centers manned by one or two people or a crew of limited size. Because of the demand for readiness, the duty tours and hours of the air combat person will become more like those of the missile control officer and, in some cases, like sea duty or Naval personnel with similarities to the life style of the submariner.

The critical issue in regard to developing new generations of flying weapon systems is to provide the new and succeeding generations of professional combat officers prepared to "fly and fight" in systems over which they have little or no direct "stick and rudder" control.

At this date we are still selecting and training people with qualifications for jobs demanding high psychomotor performance. Advanced research in the human factors field will be moving toward the facilitation of recruitment and training of professional personnel who possess qualifications for exceptional cognitive performance, high motivation to serve over extended periods in work environments that are naturally unstimulating, and in which vigilance and quick arousal are more important than the ability to control a high performance vehicle.

The Air Force has had relevant experience in regard to manning the control stations that are an essential part of the operation of Titan and Minute-Man missile systems. The AF ROTC has served as a good source of missile officers; those who had majored in science or mathematics were well qualified for these positions and accepted assignments in the Strategic Air Command with alacrity. These officers were advancement-oriented, whether they were science majors or graduates in management or business administration. A preponderant number pursued correspondence and extension courses in their professional disciplines. For a number of reasons, a large number of the officers with high learning potential found the conditions of military life in a missile unit to be unacceptable, and many refused to serve beyond their obligated tour-of-duty.

In looking for an analog of the augmented future need for professionally trained military personnel in full-time careers, we invite attention to the services' problems in staffing their medical programs. The Office of Navy Surgeon General has reported that the Bureau of Medicine and Surgery is staffed by a cohort of physicians of whom five percent are foreign born. A larger group (7.89 percent of Navy doctors), are graduates of foreign medical schools or are naturalized U.S. citizens. These figures may not pose an insoluable management problem at this time; however, a serious staff difficulty is apparent when it is reported by the B.M.S. Public Affairs Office that current accessions to the Naval Medical Corps (physicians) include 56% who are graduates of, or partially educated at, foreign medical schools, or they are naturalized citizens of the United States. The Surgeon General of the Navy reports that this situation creates problems since some medical assignments are not available to non-citizens. But the real message of this situation is that a military service with a medical school of its own and a recognized Naval Medical Center cannot staff its functions in the job marketplace, and is eagerly seeking to close facilities to gain more efficient use of its professional staff.

Inquiries directed to the Office of the Air Surgeon General indicate a pressing manning problem is chronic with regard to recruiting physicians for the Air Force. It seems evident that the grossly understaffed medical services of the Navy and Air Force are omens that forecast an unacceptable shortage of qualified manpower for the non-heroic job of air combat in the era of 1990-2000.

The Selective Service System functioned through World War II, but was permitted to lapse in 1946. The draft was reinstated in 1948, but the service requirements were low enough to permit a draft holiday from Jaunary 1949 to June 1950. Low military manpower needs and the minimal draft quotas reinforced the idea that volunteerism with more competitive compensation could meet the military manpower requirements. Attitudes adversive to conscription were amplified during the Vietnam War. There were defects in the Selective Service System that should be corrected if this system is reinstated in an effort to solve the military manpower problem.

There are a number of alternatives to volunteerism and the Selective Service System. In the past the Army and Navy have opted for the Universal Military Training for male youth who met minimum physical, mental, and emotional standards. After basic military training and an abbreviated tour most of the UMT conscripts would return to civilian status, automatic transfer to reserves, mobilization assignments, and refresher training for a period of years. Retirement costs of this system would be less than those of the current volunteer force but training costs might be excessive. Many youth conscripted under such a system would have low motivation for performance and the requirement for high quality career professionals might remain an unresolved problem. A system that is limited to males only might be discriminatory in the eyes of the public or the courts.

Another alternative would involve National Service for all youth, male and female. There need be no exemptions for students or for conscientious objectors since those who did not desire military service could elect public service in conservation, public health, sanitation, and so forth. There would need to be no added burden for retirement over present systems because everyone would participate and there would be no comparative disadvantage for the military trainee if he left after completing his minimum service requirement. He would revert to civilian status in step with those of his peers who had selected other types of national public service. When compared with other public service opportunities, military training compares well. The training acquired in the military usually transfers readily to the civilian work force. The military has a higher technology component than most other public service employments. The military stresses discipline and good work habits and makes an effort to develop organizational morale, productivity, and leadership, all of which are attractive to the private sector.

Research accomplished by the Westinghouse Health Systems Division under sponsorship of AFOSR indicated that military service is a high priority interest of a very large number of high school graduates. However, the services do not get too much benefit from the favorable opinions of high school students. The reason for this anomaly is the fact that military service is a second or third priority choice after such choices as going to college or accepting a job in the local community.

It might be expected that a change to Universal National Service might be an expensive way to meet the military manpower requirement but this is more apparent than real. We are already paying for a system that costs almost as much as a program of Universal National Service. These costs include the costs of the All Volunteer Force and some of the costs of the Military Retirement System which might be reduced if fewer fringe benefits were needed to induce officer and enlisted volunteering. They also include almost the total cost of CETA (Comprehensive Employment and Training Act). The costs of activities like Vista and the Peace Corp, to the extent that they employ youth of military age, are already being paid. Some costs of Unemployment Compensation would be avoided, and some public assistance efforts could be reduced or redirected.

It is apparent that the All Volunteer Force and all of the expedient programs to deal with the problems of undertrained and underemployed youth are consuming resources that could go far toward paying the costs of an austere program of Universal National Service. It is suggested that a flexible program of this kind could be manipulated to make the option of military service attractive to young people when compared to a period of national service in another field. The concept of Universal National Service has the value of being democratic; it would reduce the problem of unemployment at a time when the four-day week is being considered, and it would be an experience that would build character, responsibility, good work habits, tolerance for different kinds of people, and it would provide an object lesson in good citizenship.

If all youth participated in national service tasks, the comparative disadvantage of a tour of military duty would be removed. While this alternative has apparent advantages in manning the enlisted ranks of the services, it would also provide a pool of high quality aspirants for professional level careers in the services. If one is going to join a military service to satisfy a national service obligation, why not take advantage of the opportunity for advanced education and employment in a high technology career? At least it would be a better inducement to choose a military career for the top quality educable citizen who is needed in adequate numbers to operate and support advanced aerospace systems of the next century. It would be a giant step toward providing equality of service in providing for the welfare and security of the nation. It would eliminate the feeling that the survival of the nation is the responsibility of the poor, the disadvantaged, the undereducated, and the minorities.

# III. THE HUMAN OPERATOR IN ADVANCED AEROSPACE SYSTEMS: IMPLICATIONS FOR TRAINING

Charles E. Hutchinson, Ph.D.

## THE HUMAN OPERATOR IN ADVANCED AEROSPACE SYSTEMS: IMPLICATIONS FOR TRAINING

### Advances in Aerospace Design

New aircraft are entering the inventories of U. S. and foreign air forces that incorporate advanced concepts in aeronautics and avionics. In the future there will be further innovations in the design of the airframe, the propulsion system, control mechanisms, and weapon delivery capabilities. The dominant manned aerospace weapons systems of the 1990's and on into the twenty-first century will comprise a new generation of combat flight vehicles. Control configuration of the vehicle will provide new dimensions of maneuverability. By the year 2000 it should be possible to achieve substantial advances in manned airborne capabilities by achieving six degrees of freedom in the control of flying motions: deceleration in G's may be enhanced by as much as 300%; lift may increase in G's by 30%; sideslip in G's may increase 30%; pitch, in degrees per second, should increase by 100%, and yaw, plus 30% in deg/sec; roll may be increased by 50% in deg/sec. The new capabilities will include the possibility to make turns without the requirement to bank the aircraft and to aim the aircraft at a target that is several degrees off the sustained flight path. The new aircraft will have a relaxed level of inherent stability that will be achieved by moving the center of gravity (CG) of the vehicle nearer to a neutral point in the configuration. Reducing the dynamic stability of the craft is essential in achieving new levels of maneuverability.

When fully integrated, the new systems should prove easier to fly than current high performance aircraft. The key to this anomaly is the fact that the new generation of flight vehicles will be unflyable by traditional modes of control. The pilot will be facilitated by "intelligent" control systems. Without such systems, he will be incapable of managing a marginally stable machine, and he would be overloaded to the point of being unable to make a contribution to the management of the offensive capabilities of the vehicle. Accordingly, the pilot will need systems that unburden him from normal flight tasks.

The United States is in the vanguard of efforts to apply the newest aeronautical and avionics technology to construction of advanced aerial weapons systems. Areas where serious technology gaps may be encountered relate to the development of adequate means for the acquisition and training of the human components that are required to maintain and operate these systems at a level of efficiency close to their designed capability. Research programs in manpower acquisition, and in training of operators and maintenance and support personnel, are urgently needed to close this gap. The U.S.A.F. must acquire the capability to attract the high quality professional personnel needed to manage and operate complex new weapon systems. Concurrently, it must develop methods for training the whole operation and support team in complex systems operations needed to forge flexible and modulated weapons from elements that must be integrated meticulously if they are to function as effective defensive and offensive arms. Adoption of new high technology weapon systems is the key advantage that will permit the U. S. to counterbalance the superiority of the Soviets in numbers of combatants and in throw-weight of missiles.

Current Aerospace Design and Crew Size

Consideration of the functions currently performed by pilots is an appropriate place to begin our consideration of the changing role of human operators in advanced aerospace systems. The incorporation of modern technology in the design of high performance aircraft has resulted in the addition of new sources of information, new displays, and new responsibilities for the pilot. The result has been to create temporary task overloading of the pilot with occasional catastrophic failures of pilot and vehicle. This situation has been the cause of a debate between exponents of a two-man combat crew as opposed to those who recommend a one-man vehicle. It is now evident that this debate will be settled in favor of the single place fighter aircraft by unburdening the pilot through the use of devices that aim to reduce the pilot workload to manageable size.

While it seems clear that a single pilot can, with the help of enhanced controls over flight and power systems, manage a combat vehicle, a review of plans for future aerospace systems reveals that there are no technical reasons for including an on-board flight manager on each combat aircraft. Pilots in some current aircraft may be overloaded at particular phases of a combat mission, as can be assumed on the basis of the belief that some U. S. pilots shot down in Vietnam were unable to evade or counter an impending attack because their full attention was focused on other functions. The penalties that are inherent in carrying a two-man crew are only fractionally reduced by limiting the on-board crew to a single operato-. The question with regard to the size of crew for a combat aircraft leads to consideration of the proper location for the cockpit of combat aircraft.

## Location of Pilot or Crew

The arguments in favor of locating the operator in the aircraft, or external to the vehicle as in the case of remotely piloted vehicles (RPVs), or, preferably, remotely managed vehicles (RMVs), hinge on the comparison between the benefits and costs of including a passenger in an aerial weapon system and the advantages and penalties associated with control by operators external to the vehicle. We are persuaded that in most tactical and strategic combat aircraft the added costs and reduced mission effectiveness associated with weapon system designs that include an on-board pilot will make it mandatory to locate the pilot outside the vehicle(s) that he controls.

We do not postulate an Air Force without pilots or operators involved in the on-board management of aerospace systems. Flying personnel will operate such vehicles as RADAR-IR-TV surveillance craft, cruise missile delivery ships, combat vehicle command stations, transport and refueling vehicles, space stations for command of operations, shuttle vehicles, surveillance satellite stations, and strategic force deployment and resupply aircraft. We are suggesting that the closest approximation of Vietnem type Air Force combat pilots will be found among Army forward air control (FAC) type pilots, Army, Navy and Marine Corps combat aircraft and helicopter pilots, Army close support, observation and command mobility aircraft, and aircraft carriers.

Penalties are too great and benefits too meager in designing high performance combat aircraft that include an on-board pilot for each vehicle. It is already clear that the single operator of a modern combat vehicle with the assistance of computer mediated information systems and computer and electronic actuated response systems is more effective than the two-man combat plane ever was. It is this transition that gives us confidence that shortly we will witness the era of combat aircraft managed by operators who perform their pilotage functions from other locations. The combat craft "cockpit" will be located in an airborne, spaceborne, or ground based command post with resultant gains in systems performance, readiness and cost avoidance.

## Man-Machine Task Allocation

In speculating with regard to the tasks that the combat aircraft operator will perform, it seems fairly clear that the demands in terms of aptitudes and skills required of the operator will be rather similar no matter where he is located. Advanced vehicles will be equipped with variable cycle jet turbines that will permit subsonic flight and supersonic dashes as required in different phases of a mission. Aircraft will be maneuverable to an extent that will make the vehicle an inappropriate environment for a human operator from the standpoint of exposure to G-forces. High maneuverability will be required to permit aiming the craft at targets that are several degrees off line to the flight path of the vehicles. Evasive tactics will include the ability to move laterally, vertically and to turn without banking. It will also be feasible for aircraft to achieve rapid negative accelerations while in a stable flight routine.

The present state-of-the-art in aircraft design provides flight control systems that are digital "fly-by-wire" mechanisms. However, the controls over power are still actuated by hydro-mechanical linkages, although the transition to electronic engine and thrust control systems has begun. We can be comfortable in predicting that in a very short time both flight control and power modulation will be part of the avionics packages of advanced aircraft, both combat and air transport. The remaining bar to the adoption of digital electronic power controls is reliability, but this obstacle has several potential solutions such as increased redundancy, fault correcting systems, and substitution of surviving sub-systems in unconventional arrangements for failing sub-systems. But the real solution to problems of system failure is through the development of components that have extended or indefinite periods of serviceability before failure. Fault tolerant designs, self-diagnosing and self-correcting systems, substitutions, redundancy and extended component and system time before failure are lines of attack that promise the achievement of reliable automated and semi-automated flight vehicles well before the year 2000.

Anticipated advances in aircraft performance will include the ability to build aircraft that can function as short take-off and landing (STOL) craft. In an emergency, the same craft might function as a vertical take-off and landing (VTOL) vehicle. It is expected that a STOL airplane will require an airstrip no more than one hundred yards in length. It is conceivable that some VTOL and STOL vehicles will be piloted aircraft engaged in ground support combat functions or in air transport activities involving the delivery of equipment, personnel or supplies. However, the development of these types of combat support aircraft should not be considered as a major qualification with regard to the changing role of the aircraft operator. It should be clear that effective operations in remote locations at ground-hugging levels will require automated terrain avoidance systems that must use prior geodetic information programmed into the control system or some other form of automated control mechanism, because nap-of-the-earth pilotage technology cannot be extrapolated to the extent required to pilot an aircraft in proximity to the ground and still cope with the debilitating effects of turbulence and buffeting as well as improved anti-aircraft systems. If the pilots of these types of aircraft are on board they will nevertheless be forced to rely on automated controls and information systems that will be comparable to the "cockpit" displays and control mechanisms available to the operator of an RMV.

The partition of tasks between the human operator and machine components is not considered a problem on the basis of general principles. Assign any function to automated control systems that can be performed by automata without seriously increasing cost or complexity. Other things being equal, the more tasks assigned to the avionics systems the better, because this will free the human operator to do those things in which he excels. The operator should be unburdened as much as possible to permit him to perform high level intellectual functions. Psycho-motor performance, which was fundamental in flying a conventional aircraft, may fall out completely. In commanding RPV's from an airborne control station the flight-rated operator may suffer negative cross-feed from his flight training due to confusion induced by his involvement in the flight motions of the airborne command post not under his control and the flight directions he should provide to vehicles that are under his control. There is no research evidence bearing on the negative transfer of flying skills to the RPV task, but observations have been made that indicate that some persons who lack piloting skills may perform RPV tasks as effectively as professional pilots.

One of the most demanding tasks of a conventional pilot is visual scanning of the information presented on the display panel. He must update his information at intervals and interpret the meaning of information provided by panel instruments, cathode ray tubes, and out-of-the-window perceptions. He is involved in making fine discriminations in target and terrain features that may at this time be too difficult for automated target recognition systems. The operator is called upon to select the appropriate attack maneuver in view of hostile craft, air defenses and environmental factors that influence the completion of the mission. On-board computers can intervene between the pilot and the accomplishment of many of these tasks with a consequent reduction in the human workload.

In discussing the partitioning of tasks between man and machine we have departed, momemtarily, from our assumption that there are no absolute requirements for the inclusion of on-board operators in advanced aerospace combat systems. We have included the discussion of the changing role of the pilot in contemporary aircraft to establish the fact that the transition to new systems of flight controls has already mandated a shift in the division of labor between operators and machines in current and next generation combat vehicles. The full fruition of the trends in the assignment of tasks to people and to automata will arrive when it is feasible to remove the man from the vehicle and to relocate the controller at some other nexus in the system.

## Current and Future Research Needs

Research planners of the Aerospace Medical Division of AFSC are fully aware of the limitations that human exposure to electromagnetic radiation poses for the realization of the full potential of advanced aeronautical systems. High energy transmitters and phased array radar antennae are specifically implicated. Many of the devices recognized as potential sources of hazard are found in systems that are being developed, or used, for offensive or defensive aerial surveillance and communication. While the systems that are subject to restrictions because of hazards to human operators are critical to the development of effective offensive and defensive weapon systems that would serve as command stations for RPV or RMV management, they are equally essential for the $C^3$ functions needed for use with piloted attack forces. The degree to which these problems can be resolved should not be a factor in the decision to use remotely managed attack vehicles or piloted combat vehicles.

Biomedical research is progressing on a number of tasks that will contribute to the adaptability of the pilot or aircrew to the environments encountered in manned combat aircraft as well as to flight crews involved in occupations aboard aerial command posts, in various space and satellite surveillance and $C^3$ centers, and in ground stations. We refer to present and proposed research efforts that would provide protection for flight personnel from the independent and combined effects of hazards such as thermal, acceleration, altitude, chemical and biological risks to health and performance. These tasks comprise an inclusive program of research to provide aircrew protection and to counter the adverse effects of hazards that have a particular impact on the combat pilot. The results achieved in these research efforts will be useful to U. S. and allied air forces during the years of transition to the newer modes of combat aircraft control that should be available in the 1990's. There is additional justification for support of these research efforts that have a direct bearing on the welfare of the combat pilot because they will provide essential technology during the transition period and vital information in support of allied air forces and of other services that may be less concerned with the detrimental effects of these hazards. There can be little doubt that these programs provide significant fall-out for advanced aerospace programs and for civilian programs to deal with the same sources of occupational risk. Research directed toward the protection of human operators from the effects of G-forces will have particular relevance to the design and use of manned combat vehicles. But in the longer run this technology will have limited applicability to combat systems design, because the human tolerance to physical force environments is so limiting to vehicle performance that designers of advanced combat vehicles will exclude the human operator as a passenger in the most highly maneuverable combat aircraft.

It is our assumption that to the degree possible the working place for the aircraft operations commander for the year 2000 should be a modified shirt-sleeves environment. Cumbersome and fault-prone protective clothing and devices should be removed from the individual and incorporated into the system. Extra-vehicular operational or escape systems should involve the use of clothing, tethers, survival equipment, capsules and other devices that can be used when required, but otherwise stored. There is an urgent requirement for additional studies of human performance in spatially constrained work and living space under environmental conditions that are in some respects analagous to those experienced by American and Soviet space crews.

We believe that there is a large, unmet requirement for research that is not now being accomplished and that has not been identified in current research plans. Tasks involving the performance of military personnel in space systems and environments are needed. The suggested effort will not be detailed because the capability to plan and implement the program should reside in AMD and AFHRL units. The following are some broad areas for fundamental research:

Proposed Initiatives:

I Human Factors contributions to the design and operation of military space vehicles and bases.

II Optimization of man/machine decision making capabilities applied to military space systems.

III Health, safety and toxicological screening of potentially hazardous compounds and environmental conditions that can be anticipated in manned space operations.

IV Simulation of inclusive and integrated space systems structures and operational dynamics. The objective of this program is to develop global system simulation capabilities that can be used for systems design, job and task analyses, for the specification of training routines and eventually to serve as a test bed for training exercises. This is one of the most ambitious research and development efforts that could be proposed and should be approached after a meticulous planning effort that provides a detailed map of the coordinated projects required to arrive at the task objective while providing useful milepost accomplishments during the life of the effort.[1]

## Conclusion: Job/Task Analytic Groundwork and Future Trends

There are some complex problems to be faced by those who wish to initiate research or to develop a detailed research plan to create the training technology necessary to provide human operators and managers for future aerospace systems. In this report we will not solve the knotty problem that starts with the need to develop provisional job and task analyses of the functions to be performed by future aerospace operators. The development of job and task taxonomies and the preparation of occuptional analyses at the level of tasks, functions, jobs, skills and aptitude clusters, and commonalities within and between occupations and functional families of aerospace jobs is required. There may be reluctance to define jobs and tasks

[1]This effort is of sufficient importance and scope to justify the development of a SYSTEMS SIMULATION RESEARCH CENTER (SSRC). If such a facility existed there are other priority tasks that would be appropriate for its research attention.

that relate to systems that have not been fully conceptualized, but it is essential to make a timely start on the human factors element in future systems, lest the training and manning functions become the "Achilles Heel" of future systems. Furthermore, it is clear that system planners and designers cannot perform their functions if the human factor is an unknown quantity.

The development of job and task descriptions is the unavoidable first step in Instructional System Development (ISD). ISD is the mandated method for developing curricula and training materials for courses of instruction where the desired student outcomes in skills and knowledge are known in advance. It may be more challenging to develop projective job and task analyses for families and generations of systems that have not been built, but it is essential to specify the potential human contribution to systems before unrealistic commitments are made. We believe that it is possible at the outset to make an effective beginning on this task by pooling the talents of professional in-service personnel and civilians drawn from the research community who have special qualifications in the areas of theories and methods in occupational analyses and special knowledge of jobs and tasks in the area of complex systems performance. Improvement of the quality and applicability of manning and training forecasts will be a significant part of the research program proposed in the following section. It should be apparent that it is not possible to develop recommendations for a program of research on training technology that ignores the contribution of research planners and performers in the field of human engineering. It is therefore our strong recommendation that planning and research in these fields continue to be cooperative and coordinated efforts for the appropriate AMD and AFHRL units.

I RESEARCH THRUSTS AND FIELDS OF KNOWLEDGE TO BE EXPLORED TO DEVELOP A TRAINING TECHNOLOGY FOR THE YEAR 2000

A. Systems Performance Models for the Derivation of Mission, Job and Task Requirement:

1. Future Air Force strategies and mission responsibilities must be identified through analysis of the threats and vulnerabilities of the U.S. and principal adversaries. The comparative strengths of adversaries and coalitions should be assessed in terms of future economic, industrial, technological, and social and political stability. These projections must extend the range of current forecasts and explicate alternative world futures that may be influenced by U.S. and Air Force interventions.

   The need for improved technology in these areas is related to the fact that very large investments are being made in systems that may never be used, and in systems that may never be tested prior to use. The danger of buiding inappropriate systems, and systems that do not by their availability help to create the conditions favorable to the economic, technological, and political welfare of the U.S. should be minimized through the development of a broad scientific base for planning that includes both encyclopedic knowledge and improved methodologies for anticipation of future states of the U.S. and the world environment, including people, resources, consumption patterns, and potential for friendly or hostile interactions.

2. Profiles of future aerospace missions must be generated and described with sufficient detail to permit the forecasting of the magnitude and quality of the performance required of human operators in future aerospace systems. The urgent need to acquire the capability to simulate future Air Force manpower and training requirements will necessitate the development of simulation methods, and will incorporate the ability to provide full mission operation and support simulations. The recommended models will provide details with regard to operational environments, man-machine

components, support system requirements, human operator functions in all phases of development, acquisition, and use of the systems, and explicit information regarding the life-cycle costs and benefits associated with contemplated weapons as these costs might be compared with alternative investment strategies.

3. Job and task operator functions must be described before the training curriculum can be developed. Air Force Manual AFM 50-2 Instructional System Development describes the approved method for ISD that must be followed in preparing an instructional unit. Block One mandates the analysis of system requirements; Block Two directs the definition of education and training requirements. In practice there is a major break between the description of system requirements and job and task delineation. This hiatus will be all the more difficult to bridge when dealing with future missions and systems, but an approach to this problem is fundamental to efforts to describe the training research requirements for Year 2000.

B. Computer Based Taxonomy and Modeling of Aerospace Operator Functions

1. An aerospace operator task information data bank is needed. Current practices involving the performance of task analyses for each new system are wasteful and inefficient. There is a need to design and develop a prototype model, and evaluate the performance of a computer-based task information data bank. This information should cross reference data with regard to generic classes of operator tasks because it is assumed that there are very large areas of commonality between the task functions to be performed by operators in markedly different aerospace systems. The training task information system should include categories of data relating to skills, aptitudes, physical, emotional and experiential prerequisites for entry into training based on the evaluation of performers in analogous task assignments. This program should include, in addition to task definitions, standards of desired task performance and criteria for assignment of operator effectiveness.

2. A systematic method for translating operator functions into training task specifications must be developed. Task operator functions can be inferred from system requirements, but the derivation of task training requirements and operator aptitude and skill requirements is not a readily performed personnel activity, and research on this aspect of IDS is indicated.

3. Human operator modeling technology is needed. "Advancements in computerized modeling technology and increased knowledge of human sensory, cognitive and motor capabilities could enable the development of a comprehensive . . . interactive model of . . . important human functions. Visual functions, including detection and identification, discrete and continuous manual control and information processing functions will be modeled. Dynamic anthropometric capabilities including force and stamina and the physiological effects of acceleration, thermal imbalance, altitude, toxic agents, and combined mission stresses will also be modeled. A considerably advanced version of COMBIMAN (Computerized Biomechanical Man Model) would result. This would include not only such cases as aircraft in flight, but also the simulated flight of aircraft using training simulators." The description of the applications of this model to flight personnel in aircraft and simulators was prepared as a joint contribution by AMD/AFHRL to the "Looking Forward 20 Years - Aeronautics Panel" (1978). It gives evidence of some immediate concerns of AMD and AFHRL, but does not create a constraint on the utility of the modeling technology that would emerge from the effort described. It is apparent that the role of the operator in multi-man operations and the functions of human operators in highly automated systems may require little in the way of manual control or visual-motor coordinated dexterity, but the ability to model these functions will not be unimportant in modeling total operator performance.

4. Preparation of team and crew operations models mandates the development of the enabling technology. The methodologies that were designed to perform the functions required to satisfy this need have in the past been referred to as Man-Machine Systems Modeling Methods. In advanced aerospace systems the operator will function most often as a member of a functional team that shares information and responsibilities in systems control or modulation of system performance. The need to expand the scope of man-machine systems analysis beyond the bounds of tradiational psychology is evident. The fields of systems sciences, operations research, econometric modeling and computer technology are critically involved.

   At some point in the development of advanced aerospace systems, designers will build into the total weapon concept the potential to use the system itself as a mission defining mechanism and training base in order to provide team and crew experience, readiness evaluations and estimates of systems effectiveness without the requirement to discharge or detonate the lethal potential of the weapon.

C. Characteristics of the Learner that are Relevant to Selection, Assignment, Training, and Appraisal in Aerospace Operator Occupations:

1. Student aptitude and skill clusters essential to successful operator performance should be identified as an essential part of efforts to recruit and assign personnel to functions and tasks where they will have a high probability of successful performance, and as a means to reducing student attrition in costly training programs. Aerospace operators will be required to perform many sub-task routines in order to exploit their presence in advanced systems. These tasks may require the operator to possess perceptual skills (target identification, spatial orientation); psychomotor skills (pursuit tracking); short and long-term memory (procedural performance and emergency routines); and cognitive or intellectual functions (mission planning, threat assessment, time sharing on multiple

tasks, decision-making, risk-taking, and communication facility). Research is needed with regard to the selection criteria and instructional methods that are most effective with regard to each of the skill or aptitude clusters as they relate to the responsibilities of the operator in different phases of the aerospace mission.

2. A refined technology for human appraisal is needed. Methods are needed for use at recruiting sites and at frequent stages in the career progression of enlisted and officer personnel that permit the assessment of the traits of the applicant, student, and operator that bear on their performance on the job. The required methods will include the ability to assess aptitudes and future potential of people at the entering level, techniques for the measurement of progress in training, means for evaluating performance on the job, and diagnostic assessments of worker characteristics and organizational situations that might be modified to enhance organizational productivity. The improved military personnel technology should, to the extent feasible, provide immediate feedback to the applicant, student, and operator with regard to the assessments made and the behavior modifications that might contribute to career advancement and organizational productivity

3. Methods for assuring student motivation and morale and productivity of individuals and organizations are needed. Considerable efforts have been exerted toward the development of methods for the improvement of individual and organizational performance in context of military and industrial organizations. In the face of this experience one might be persuaded to rely on traditional remedies of exhortation and experimentation with the available techniques for Organizational Development (OD). The penalties paid for low interest in job performance and low productivity cannot be accepted in an era when military systems will increasingly depend for their efficient operation on the effective performance of the human components of systems. Embedded in the problem of motivation and

productivity is the question of human reliability. The potential exists for sabotage and human operator and support personnel-induced failures of weapon systems, as does the problem of entrance of subversive persons and representatives of hostile organizations into Air Force employment. There are established methods for screening personnel and for monitoring the behavior of workers in military occupations. There remains the requirement to improve and extend the use of non-intrusive methods for assessing the operational effectiveness and motivation and competence of operators performing in remote and inaccessible locations, where their inability or unwillingness to perform might result in compromise of a military mission or operation.

D. Reorganization of Training and Improvement of Instructional Methods:

1. Research on the allocation of education and training responsibilities between institutional agencies such as the Air Training Command and operational organizations is needed. The Air Force is accustomed to providing training and education in an institutional environment specifically designed for pedagogical functions. However, it is well recognized that some of the most important training functions must be carried out in operational units using the equipment provided for operational use. Consideration needs to be directed to the partitioning of training between institutional instruction and training that can be more effectively acquired on the job. Initial training, on-the-job acquisition of skills and knowledge, currency training, and evaluation of organizational readiness are parts of a training continuum and deserve research to develop methods and devices useful in assuring timely and cost-effective instruction for new and established members of operational organizations. The potential advantages of transferring the primary responsibility for the training and integration of new personnel into operational units should not be overlooked. As we move into the era of advanced

aerospace systems there will be an increasing need to develop methods for training and performance evaluation that do not involve the use of the actual weapon systems nor the delivery and detonation of armaments. Simulation models that provide experience in the full range of system functions are needed. The advantages of locating these capabilities with operational units provide compelling reasons for transferring functional training to operational commands.

2. A broad spectrum of problems relating to instructional methods, pedagogical devices, and learner qualities should receive research attention. Basic research on motivation of learners, learning styles, team and crew instruction, reinforcement in the training situation, application of adaptive training procedures to aerospace occupations, development of diagnostic measures of student performance appropriate for use in computerized simulations of systems, low cost task trainers and simulation devices, use of operational equipment for training, the role of instructors, supervisors and team members in the facilitation of learning, adaptation of student paced instruction to aerospace environments, and the automated assessment of individual and unit performance are candidate fields for research efforts.

E. Bio-medical Aspects of Selection for Training, and Space Physiology Elements in Training for Military Space Occupations:

1. There is a requirement for research on the bio-medical factors in aerospace operations as elements in military training. Research and development conducted by AMD provided a major share of the technology applied to the physical selection and training of U.S. astronauts. Continuing investigation of the physiological factors influencing human performance in space, survival systems, and life support systems together with associated training, and physical conditioning of military operators in space occupations is proposed.

2. Research is recommended on the potential of training to mitigate the effects of disorientation due to the presence or absence of motion cues in dynamic environments. The utility of ground-based devices that are capable of producing illusions of flight motion and approximations of weightlessness should be studied. Motion sensations triggered by either visual or vestibular cues should be investigated from the standpoint of the effect of altered, spurious, and disorienting motion sensations that could have an effect on performance by RPV operators and external flight directors in command stations. The degree to which performance may be improved through training or familiarization should be studied. (Dowd, 1974)

## F. Summary and Recommendations

It is recommended that the Air Force laboratories now engaged in the refinement of human operator performance and pilot training with regard to piloted aircraft devote their basic research resources to the identification of research problems and development of research equipment and methods appropriate for training operators for the next generation of aerospace systems beyond those currently managed. We are pleased to note that the Aerospace Medical Research Laboratory (AMRL/HE) has already taken steps in this direction with the development of the RPV simulation facility. Because of the high competence of personnel in Air Force training laboratories and their familiarity with the fields of research that are most important in improving training effectiveness, we will avoid needless repetition of recognized deficiencies in training methods and improvement in instructional materials and devices. Rather, we will briefly underline some points that may depart somewhat from current Air Force research planning strategies.

1. There is an urgent need to identify the training requirements associated with future aerospace systems. These are systems that are characterized by the location of human operators at points in the system that may be remote from sources of target information and

detached from the vehicle, missile, or surveillance craft which may be engaged in mission accomplishment at a distant location and in a lethal environment.

2. There is an urgent requirement for improved aerospace systems training methods that are capable of providing realistic systems training within the context of a completely simulated environment and automated weapons systems response capability. The development of the recommended training technology would involve the incorporation of a broader range of skills and disciplines than those possessed by psychological and engineering professionals employed in traditional military training research laboratories.

## II RESEARCH ORGANIZATIONS NEEDED TO DEVELOP AN AEROSPACE OPERATOR TRAINING PROGRAM FOR THE YEAR 2000

A. The Air Force Needs an Agency Capable of Developing Long Range Studies of Future World Environments.

USAF/XO uses its resources and those of the RAND Air Force Project to provide answers to problems that are important in preparing military objectives studies, operational plans, and studies directed toward satisfying future capability requirements. These efforts should be extended in time, in depth, and in range of disciplines employed and in scope of information exploited in deriving estimates of alternative world futures. The responsibility of aerospace power in achieving national objectives must be projected further into the future. Research studies authorized by the USAF/RAND Committee are of short duration and are directed toward topics of high current interest to committee members who have a limited tenure in USAF headquarters. RAND research proposals that do not appeal to the advisory group are vetoed, with a resultant inhibition of sustained efforts by this organization to fulfill the need of the Air Force for basic research that could provide encyclopedic information from multidisciplinary efforts, and methodological and theoretical studies that would produce an improved technology for anticipating future states of the world in which the Air Force will be called to operate. Failure to anticipate future needs will put the Air Force in a reactive mode, and this is an unaffordable position for a force that must exploit systems that have a long development lead time.

B. The Air Force Needs an Agency to Perform Research Directed Toward the Development of a Full Systems Simulation Capability.

The agency created to perform this function should have direct access to personnel who are broadly educated and experienced in econometrics, operations research, computer technology, systems science, physics, chemistry, toxicology, bio-medical sciences, aeronautics, astronautics, astrophysics, propulsion, psychology, political science, anthropology, and philosophy. The personnel of the SYSTEMS SCIENCES RESEARCH CENTER (SSRC) should include a team of Air Force officers with training in appropriate scientific disciplines who would serve as members of research teams and task forces and also provide working level liaison with the Air Force.

1. The mission of the SSRC would include the performance of research, development of prototype simulation systems, and test and evaluation of model systems for transfer to, and use by, USAF Hq., major commands and subordinate units. The SSRC would retain ownership and control of models intended for global system forecasting for USAF exploitation, and would maintain and administer the performance of simulation models of systems that were intended for research use. The functions of the SSRC would combine fundamental research, development, and operational responsibilities. It would be essential to safeguard the research function from absorption by developmental and operational activities.

2. Research conducted by the SSRC would focus on the improvement of technology in selected fields of science that bear on such problems as: a) Mathematics, statistics, and operations research methods applicable to systems modeling and simulation. b) Information processing research applicable to the development of the theory of automata and robotized processes, to digital simulation techniques, and to computer language refinement and extension. c) Artificial intelligence research involving the improvement of methods for pattern recognition, processes for the solution of complex problems and development of trainable devices. d) Decision-making in functional systems including human data processing and decision behavior, man-machine decision processes, accomplishment of group tasks, and decision-making in structured organizations. The research functions of the SSRC would be performed by the Laboratory for Research in System Sciences (LRSS).

3. Developmental activities of the SSRC would focus on building and testing simulation models and evaluational prototypes of systems having high priority for future Air Force Plans and operations. The capability to develop and continuously refine modeling techniques that would be applied to the creation of systems simulations at many levels of complexity and generality is needed. Simulation methods would be applied to such functions and predictions as the following: global operations and support system; alternative systems for achieving desired missions capabilities; evaluation of life-cycle-costs and benefits of systems; evaluations of systems integrity and vulnerability; provision of a realistic test-bed for training personnel for system management responsibilities; develop, exercise and evaluate Command, Control, and Communication systems for limited and global operations; and forecast manning requirements and training tasks associated with planned new systems. The developmental activities of the SSRC would be performed by the Laboratory for Systems Modeling and Simulation (LSMS).

4. Communication of systems sciences technology and simulation methods to Air Force users would be accomplished by a specialized agency of the SSRC. Some of the specialized models and simulations could be located, operated, and serviced by user agencies, but there would be a continuing need for the SSRC to supervise, support, and upgrade simulation models wherever they existed in the Air Force. There is an urgent requirement for the development of several simulation models that could be of differing levels of complexity. Some of the candidate systems for the development of models are: $C^3$ model for development of requirements for training Air Force managers; a full mission simulation model of a planned or operational Air Force system designed for system coordination and currency training; computer modeling for systems engineering and design; simulation of a planned Air Force system to forecast personnel characteristics, manning requirements, job and task functions and training requirements.

C. The Air Force Needs a PERFORMANCE AND TRAINING RESEARCH CENTER that Administers a Comprehensive Program that would include Research and Development in the Following Areas:

Manpower Studies - Analyze the national military manpower pool potential, and the skill requirements of the Air Force; develop and administer a simulation model of the Air Force manpower and personnel system.

Personnel Studies - Conduct research on methods for the recruitment, accession, and assignment of personnel, with special emphasis on computer-based methods for assessment of aptitudes and interests of applicants at the point of recruitment.

Occupational Analysis - Develop and maintain the capability to analyze jobs and tasks in Air Force systems and translate these requirements into manning quotas.

Man-Machine and Systems Engineering - Perform the human engineering analyses of aerospace systems for the inclusion of human factors information in systems designs, for development of manning requirements for new systems, for consideration of trade-offs in systems design, for predicting life-cycle-costs of future systems in the design phase; maintain direct liaison with the SYSTEMS SCIENCES RESEARCH CENTER to insure that both programs are fully coordinated and that essential information is exchanged.

Training Research - Conduct research and developmental studies on technical training, flight training, simulation of systems for training, and training methods designed to train operators and support personnel for advanced aerospace systems.

Productivity and Assured Performance - Develop methods and instruments for the unobtrusive evaluation of individual, team, crew, and organization performance; conduct studies of productivity in a model O & S Wing; analyze leadership and management characteristics influencing productivity; account for the degree of influence that job, income and environmental factors have on productivity; assess the influence of external political, social, and economic factors on worker productivity; prepare an analysis of the impact that Integrated Computer Aided Manufacturing (ICAM) may have on worker productivity; and analyze the potential influence that personnel organizations in the military may have on the availability and productivity of Air Force workers.

# IV. THE OPERATOR IN ADVANCED AEROSPACE SYSTEMS:

# IMPLICATIONS FOR HUMAN FACTORS ENGINEERING

JULIEN M. CHRI.˜ENSEN, PH.D.

## INTRODUCTION AND A BIT OF HISTORY

The previous chapter, "Implications for Training", and this chapter, "Implications for Human Factors Engineering", were written by different authors with virtually no communication between them during the development of the materials. It is interesting how much agreement there is between the two with respect to which fundamental areas should receive attention (e.g., systems research, multi-stress research, motivation, etc.).

Our first impulse was to take every vestige of human engineering-related research out of the training chapter and vice versa for the human engineering chapter. Upon further reflection this impulse was rejected when it was realized how well any overlap serves to highlight their degree of communality at the basic research level and that their separation at such level is justifiable only for administrative purposes.

Though some of what was done by pioneers such as Taylor and the Gilbreths around the turn of the century might be considered human engineering, the specialty was more formally initiated near the end of, and immediately after, World War II. Men like Fitts, Taylor, McFarland, and Karlin in the United States and Shackel, Bartlett, Singleton, and Murrell in the United Kingdom deserve special recognition for their leadership roles in this new movement.

Confusion still exists as to what to call this relatively new field. In this chapter, the term _human factors_ will be used to describe both research and applications efforts that deal with man's performance in technological systems. The distinction between research and applications is preserved by using terms such as _engineering psychology_, _engineering anthropology_, etc. to describe the theoretical or scientific basis of human factors while _human factors engineering_ (or simply _human engineering_) is used to describe applications efforts. The first is properly the province of the scientist while the second is properly the province of the practitioner. In Europe, and increasingly in the rest of the world, including the United States, the term _Ergonomics_ (the study of work), coined considerable support by the military establishments in World War I (and again in World War II). Many medical advancements grew directly out of military experience. The incredible advancements in computers and microelectronics can be traced directly to interest and support by the military establishment. These are just a few of many possible examples.

Second, designers of military systems are continually faced with the problem of extracting the last vestige of capability from their components, whether those components are organic or inorganic. For these designers are quite aware that their potential adversaries are undoubtedly doing the same. This forces them frequently to make demands on the physical and mental capabilities of people that far exceed that which is customary in everyday living.

This second point has important implications for the type of research that OSR should support. Specifically, it is even more important that OSR-supported researchers demonstrate that they have achieved a representative sampling of those ecological variables that are important in their experiment as well as the usual (often assumed) representative sampling of experimental subjects. Findings from traditional psychological laboratory settings must always be examined critically before being applied to Air Force problems since the vast majority of such experiments were (and still are) conducted in a "constant" environment -- an environment that is almost certainly <u>not</u> representative of a military operational environment. (Mackie, 1980) In fact, the best that one can say for such research is that is <u>may</u> be representative of laboratory environments.

OSR should provide leadership in breaking out of the very limiting traditional and current practice of conducting behavioral experiments with environmental variables "held constant". This practice automatically assures neglect of the extremely important interactions involving environmental conditions. It is for this reason that we recommend elsewhere in this chapter that OSR vigorously support a program of research in "unusual environments". If properly conducted, it would serve as an "acid test" of the generality of behavioral principles, most of which were established in a laboratory or some other very restricted and unrepresentative environment. It is important that experimental work performed for OSR take full account of the environmental conditions under which those findings will be applied. Is this not the essence of human factors engineering -- study of behavior as it occurs in a technological environment?

Put another way, stimulus-response (S-R) psychology's inadequacies were recognized, giving us stimulus-organism-response (S-O-R) psychology. It is time to recognize the incompleteness of S-O-R psychology and give increased attention to the situation or environment in which the observed behavior is occuring.

Considered in a slightly different way, behavior, of course, is a function of heredity and environment. B=f(H,E). The terms "H" and "E" are clearly interactive and should be connected with a multiplicative sign; i.e., B=f(H x V). In the systems of a modern technological society, including military systems, the environment is determined to a large extent by engineers. (If one should doubt this, let him arise one morning asserting that he will not interact that day with any product designed by an engineer. That day, his activities will be restricted pretty much to sitting under a shade tree -- if there is one within walking distance. And even then a falling apple may remind him of an early scientist!) Engineers clearly are important modern social architects. Unfortunately, most of them are ill-prepared, by formal education at least, to assume this awesome responsibility.

Since engineers design systems, commercial as well as military, it is important that they be supplied with information from the behavioral and biological sciences that can be readily used in the design of systems and systems components. This information, insofar as possible, must be expressed in numbers, preferably on sophisticated scales (e.g., ratio scales), thus enabling design tradeoffs to be made more readily and with improved precision. Functional relationships must have criteria on the ordinate that are meaningful to engineers and must have abscissae that are amenable to engineering treatment (size, amount, degree, etc.). It is the abscissa that distinguishes engineering psychology and biology from the rest of experimental psychology and biology. And these functional relationships must reflect the effects of situational or ecological variables to be maximally useful to designers.

During its earliest years, human engineers were concerned primarily with the two interfaces shown in Figure 1. (Later, the concept of feedback had a profound effect -- especially in the area of manual control.) The input interface usually consisted of a dial or other instrument, while the output interface usually consisted of a knob or other control device. In fact, human engineers were often described as "knobs and dials engineers", probably due to the fact that that was where they tended to concentrate their initial research and application efforts. This channel of entry into equipment and systems design was reinforced by the fact that human engineers were usually brought into the design process at such a late stage that the only recommendations for improvement which could be considered economically and within the ever-present time constraints were those of the knobs and dials type. These were, and remain, important design considerations for how can an operator make the best possible decision unless he is receiving accurate, comprehensible information and how can he effect maximum performance without efficient controls? Numerous accidents resulting in extensive property loss, injuries, and death (the ultimate inefficiency) can be traced directly to inadequate design of controls and displays. The writer has kept a record of some of these, and even his small sample runs into needless costs of hundreds of millions of dollars and inexcusable and incalculable loss of the lives of some or our nation's most talented people -- pilots and other aircrew. The design characteristics of "knobs and dials" remain of critical importance.

With the advent of computers, man's interaction with this impressive and revolutionary technological advancement assumes first-order importance. This obvious requirement will be considered later.

Welford has summarized the history of ergonomics (human factors) much more succinctly and eloquently than could we. His thoughts regarding the success of ergonomics warrant repetition here:

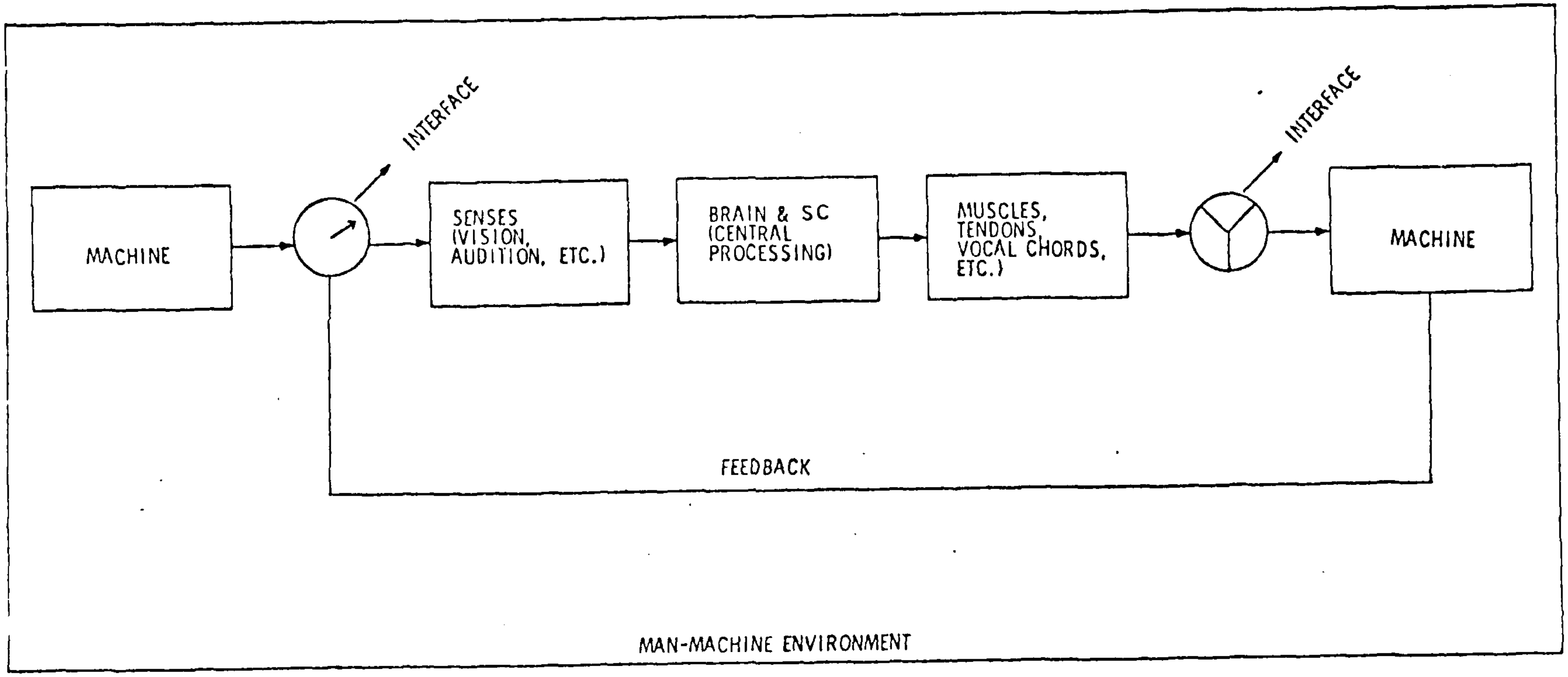


Figure 1. A MAN-MACHINE MODEL (NOTE "INTERFACES")

"(i) the concept of capacity and its counterpart limitation, as quantities that attach to many different facets of human potential;

(ii) the idea that such capacities can be specified not only as regards the amount that can be done at any one instant, but also as regards the amount that can be done in a given time;

(iii) recognition that such capacities concern not only physical, bodily mechanisms of muscles, joints, and cardiovascular system, but information processing and decision making by the central mechanisms of the brain;

(iv) the concept that human performance is more than making responses to stimuli, but rather involves a servo process developing in time and involving feedback, so that each action grows out of those that have gone before and influences those that follow;

(v) recognition of the need to express and quantify the capacities of individuals and the demands of tasks in the same terms;

(vi) understanding that capacities and the demands of tasks do not function in isolation but in complex systems involving both human and environmental factors, so that if the functions of man and machine are to be allocated efficiently, it is necessary to go beyond the study of particular capacities and demands to the behavior of the system as a whole; only thus can a full understanding of the significance of particular capacities and demands be obtained." (Welford, 1976)

The wisdom of Welford's remarks will become more evident as this report develops. His point "vi", for example, is recognition of the fact that every system is part of a larger system and, we believe, supports our previous plea for full consideration of ecological variables.

In the case of military systems, elements in the larger systems include significant social and political variables. These, as intimated in several of these chapters, will probably have greater and greater influence on the nature of future military systems. As never before, the military systems planner can ill-afford to ignore the parameters of the larger socio-political system of which his system is only one component.

Numerous committees, composed of outstanding scientists, have reviewed and written reports regarding research needs in the area of engineering psychology. It would be presumptuous of the present writer to think that he, with his many limitations, unknown (to him) biases, and only approximately two man-months devoted to the effort, can assay the field better than they. The only advantage, if it be one, is the fact that the writer spent over 30 years with the Air Force and time and again experienced the difficulties attendant to supplying design and systems engineers with the materials that they really needed if they were more effectively to integrate man into their designs.

No less an authority than Chapanis has observed that, in systems design, men are still being assigned those functions that machines do not do well (the "what's left over" syndrome) rather than some combination that might be more optimal from the systems viewpoint and certainly more optimal from the human's point of view. We certainly agree with Chapanis and, in searching for reasons, several possibilities suggest themselves. We know, for example, that design engineers generally approach problems in accordance with precepts gained during their years of formal education. And, regrettably, this education, as stated earlier, usually did not include any significant training in the human factors area. Many engineers have little idea of the fundamental characteristics of the people who operate and maintain their systems in the field.

Second, the data are often inadequate and require unusual skill and intuition in their application -- a skill that is so rare among engineers that the few truly great designers who have it are thought of as geniuses or artists, implying that they employ private capabilities that are unavailable to others. Perhaps more cooperative efforts among life scientists and designers would uncover ways to make what is now considered extraordinary, available to a greater number.

Third, it is an easier way to proceed -- at least easier for the moment but the flaws often become painfully evident when the system becomes operational and it turns out to be accident-prone, difficult to maintain, and so on.

Fourth, only recently has any emphasis been placed on developing tools that will allow human factors specialists to make a meaningful contribution at the requirements and conceptual stages of development. Yet, according to expert design and systems engineers interviewed by the writer, it is during those phases that between 70 and 90 percent of the critical design decisions are made. The engineering design phase is primarily a process of elaboration and detailing of decisions made well before that phase. Thus, it is of the utmost importance that (1) tools and data be developed that will enable significant human factors input to be made at the requirements/concepts stages, and (2) human factors researchers be ever sensitive for new biological/psychological concepts that may well "drive" the development of new systems. We will never out-man our potential adversaries; we must out-think them and out-create them.

In one sense, it is a great tribute to the human that he is so flexible and adaptive that he can assume responsibility for "what's left over" and still contribute positively to systems effectiveness. In a larger sense, however, such an approach is certain to produce systems that are less effective than they might have been.

Finally, truly great systems should contribute to the advancement of mankind (and we certainly include preservation of freedom and keeping the peace as advancements) and to the enrichment of the lives of those touched by those systems.

As Einstein, physicist and humanitarian said, "It is not enough that you should understand about applied science in order that your work may increase man's blessings. Concern for man himself and his fate must always form the chief interest in all technical endeavors, concern for the great unsolved problems of the organization of labor and the distribution of goods — in order that the creations of our mind shall be a blessing and not a curse to Mankind. Never forget this in the midst of your diagrams and equations." (Einstein: 1931 Address at the California Institute of Technology)

Or, as John W. Gardner, former Secretary of Health, Education, and Welfare, stated, "We must learn to make technology serve man not only in the end product <u>but in the doing</u>." (emphasis added)

Weiner, the father of Cybernetics, has likened man's role in many "modern" manufacturing systems as akin to the transportation systems of yore that employed galley slaves, chained to their oars, as a source of power.

Hopefully, additional research and enlightened application will enable man to emerge from this period of technological slavery. <u>This is at least as important in military systems as in commercial systems</u>. Sustained superior performance during extended periods of non-hostility cannot be expected from operators and maintenance men who do not enjoy their jobs. The designers of military systems, exploiting as they do the very latest in technology, should be capable of providing some of the most attractive jobs in society. But in order for the designers to meet his responsibility the "life scientists" must provide them the data that they need to do the job properly. And no applications program of which we are aware ever went very far unless it was grounded in sound theory that resulted from careful, usually tedious, scientific inquiry. Most of this very rough road lies ahead of engineering psychology and biology. The development and administration of a solid 6.1 program at OSR can contribute enormously to the achievement of this goal that is so critical to the development of effective systems in these days of advanced technology, space exploration and operations, etc. For too long mankind has given little more than lip service to the study of man and mankind. This neglect is now being manifested in the messes into which we regularly get ourselves socially, economically, and yes, militarily.

Lest there be a misunderstanding, we are <u>not</u> proposing an easier role for people in systems--especially military systems. Indeed, we are proposing that the results of suitable research will enable designers to employ people in more challenging roles, at least from the cognitive point of view, and in more rewarding jobs. When needed, he can be asked to operate closer to his physiological and psychological thresholds.

Let us examine the nature of the challenge that faces the manager of a basic research program in the life sciences, particularly with respect to the problems attendant to the selection of what to support (which necessarily determines what will not be supported).

## THE CHALLENGE

The problems associated strictly with the management of basic research programs in human factors are addressed elsewhere in this report--the continued support and encouragement of basic research in a nation whose citizens don't understand it or at least don't encourage it; a Congress that probably accurately reflects the feelings of its constituency in this regard; even discouragement from officials within the Air Force itself, and so on. However, these are only a few of the challenges that face the 6.1 manager. He needs, for example, to be aware of the progress and direction being taken by his counterparts in other countries, such as the Soviet Union, where a very substantial human factors program has been underway for several decades.

So where is the challenge in this to the planner of basic research programs in human factors? The challenge, as stated previously, lies in developing, and helping to make available in usable form, human factors concepts and data that not only contribute positively to the ideas for systems to be developed by others but also occasionally <u>drive</u> the development of selected systems. As it is now, systems development is usually driven by some new, brilliant discovery in electronics, metallurgy, propulsion, and so on, while the most fantastic component of them all is left to tidy things up and take over what is left. It is difficult to imagine a more flagrant misuse of a nation's resources--components with proven capabilities in such attributes as flexibility, adaptiveness, judgement, learning, memory, foresight, planning, and on and on. But such design will never be achieved without concentrated attention, persistent inquiry, and positive response to the need for both better basic research and better technology with respect to human performance.

With the availability of the kind of information we have been talking about, the prediction of the British psychologist, Singleton, begins to appear feasible. Singleton recently stated, "There will also be greater attention to machines as specialized servants of the individual rather than individuals as components in massive man-machine systems." (Singleton, 1976) Hopefully, in the future, at least some systems engineers will intentionally start with people as the central component in their systems and then introduce hardware only as means of compensating for their limitations, extending their senses, freeing them from the odious, etc.

One very positive result would be to extend human capabilities beyond anything achieved before. We are trying not only to relieve the human of duties that are undesirable but also to lead the human to new and higher levels of achievement.

## SOME BASIC RESEARCH AREAS

As stated previously, basic research in human factors is needed in both content and method. Over two dozen candidate research areas were considered within the constraints of time and ability; a few of those that appeared most promising will now be examined.

### MAN-COMPUTER INTERACTION (INCLUDING REFERENCE TO PERCEPTUAL/COGNITIVE FUNCTIONS)

We live during the dawning of the "Age of Computers"; thus, we don't have the benefit of hindsight to assist us in interpreting its impact. We unhesitatingly predict, however, that computers will have a greater impact on man and mankind than any technological development to date. The reasons for this are rather evident and have been stated by many writers. Gregory writes, "By the year 2000 many economists expect the computer industry to be the single largest employer in the United States, outranking even the auto industry." (Gregory, 1979) Pittendrigh states, "The computer is the most significant of human inventions because it complements the human brain in precisely two ways which limit the brain - slowness and boredom . . . It has added speed to the complexity of the brain . . . and the capability to solve many problems which would never be attempted because of the tedium involved." Singleton declares, "We need a greater understanding of human perceptual and cognitive activities in order to make informed decisions about man-computer allocation of function." (Singleton, 1976). Fuller goes so far as to suggest that the computer will prove to be the savior of mankind! He states, "Extinction is a consequence of over-specialization . . . you outbreed general adaptability . . . (the computer) is going to take over specialization and save man." (Fuller, 1969). Berman, echoing a position that Licklider had advanced years before, stresses the need for operator-computer compatibility in complex command-control systems (Berman, 1978).

We feel, however, that we have just begun to learn how to integrate man and computers in the design of systems. As a result, we again find man in the position of assuming responsibility for what state-of-the-art equipment (computers in this case) is, as yet, unable to do. We consider this fact alone as sufficient reason for OSR to put major emphasis on man-computer research with particular emphasis in the perceptual/cognitive area. Otherwise, Licklider's vision of over 20 years ago of man-computer symbiosis will never be fully realized.

In order to gain a bit of insight as to the nature of functions that machines as of today perform comparatively well and the nature of the functions that people as of today perform comparatively well, the writer examined a recent version of the famous "Fitts Lists". (See Tables 1 and 2) (Lists, incidentally, which Fitts once told the writer that he wished he had never devised because they were so widely misinterpreted and had led to so much fruitless controversy!)

TABLE 1

SOME THINGS MACHINES DO COMPARATIVELY WELL

* Respond quickly to signals
* Sense energies outside human range
* Exert enormous power
* Relatively uniform performance
* Rapid transmission of signals
* Perform several acts simultaneously
* Expendable
* Perform precise, routine, repetitive operations
* Recall and process enormous amounts of data
* Monitor men or other machines
* Reason deductively
* No time-of-day effects

TABLE 2

SOME THINGS PEOPLE DO COMPARATIVELY WELL

| CODE | ITEM |
|---|---|
| S | * Detect signals in high noise fields |
| S | * Sensitive to a wide variety of stimuli |
| M | * Perform fine manipulations |
| M | * Relatively compact |
| M | * Perform when partially impaired |
| M | * Relatively maintenance-free |
| P/C | * Recognize objects under widely different conditions |
| P/C | * Perceive patterns |
| P/C | * Long-term memory |
| P/C | * Handle unexpected or low-probability events |
| P/C | * Reason inductively |
| P/C | * Profit from experience |
| P/C | * Exercise judgement |
| P/C | * Flexibility and improvisation |
| P/C | * Creativity |
| P/C | * Select and perform under conditions of overload |
| P/C | * Adapt to changing environmental conditions |
| P/C | * Appreciate and create beauty |
| P/C | * Express emotion |

CODE:

S - Sensory (2)

M - Motor (4)

P/C - Perceptual/Cognitive (13)

Examination of Table 1 suggests some things that machines do comparatively well. Table 2 shows some things that people do comparatively well and these are predominantly in the perceptual/cognitive realm. There is not the slightest doubt that in the future computers will make impressive inroads on those activities currently classified as "P/C" in Table 2. Table 2, in fact, suggests areas for future research of perceptual/cognitive dimensions in the man-computer area.

We hope that this will result in a man x computer table and not a man versus computer table—complementary and not antagonistic. Behavioral scientists should be very much involved in these efforts and should lend their understanding of perceptual/cognitive matters to the efforts of the designers of computers. A review of what is known in the perceptual/cognitive area from this point of view would, in our opinion, be extremely valuable.

Presumably the chief reason that we continue to use more computers in our military systems is to enable better decisions to be made. This, again, suggests that greater efforts are indicated in research in the cognitive area. Perhaps an acceptable solution to the man-computer problem will be reached without a deeper understanding of the nature of human information processing, judgement, cognition, etc. but an ideal solution will never be realized without a more thorough understanding of the so-called higher mental processes.

It if is not already obvious, we mean explicitly also to include an examination of directions that need to be taken in software development in this area of inquiry. We envisage development of different levels and types of software for different purposes--that, for example, will enable different users to consider their particular problems at different levels of abstraction and in different contexts. (Cuff, 1980) This is simply

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Cuff, R. N. On casual users. International Journal of Man-Machine Studies, 1980, 12 (2), 163-187.

recognition of the fact that the language of the systems designer is different from that of the operator, the maintainer, or the user. Each needs, and should have, a way of communicating with the computer that, although appropriate for him, might not even be understood by other specialists. Advances in our understanding and development of a vast range and variety of "softwares", coupled with advances in electronics, probably will effect more profound changes in airborne systems than any foreseeable changes in aerodynamic structure. Systems attributes such as flexibility and adaptiveness must be of prime importance in the development of the systems of a nation whose posture seems destined to be one of reactivity. Proper software, reflecting an advanced understanding of cognitive matters, can contribute to the achievement of this goal. (In referring to man-machine interaction in this context, we mean explicitly to include the managers of such systems. Eason has written well on the challenge that this presents to the designers of future man-computer systems. (Eason, 1977) We intend also to include "systems research" in this area--at least that part of systems research that deals with information processing and decision making.)

We favor also further elaboration and exploitation of Sternberg's work (See, for example, Sternberg, 1979). Sternberg addresses important issues of both the structure and content of cognitive abilities. Concepts of information-processing are handled nicely; in fact, "information-processing components" are at the heart of his theory. OSR should cooperate fully with ONR in their support of Sternberg.

A better understanding of mental abilities would mean not only that man could be used more effectively in systems as a prime source of cognitive capabilities but also that understanding would yield clues for future computer and softward development—might "drive" them, so to speak. We feel, further, that Sternberg's work might also provide a better understanding of the results of various types of activity analysis (task analysis, therblig analysis, and so on). Some of these practices would undoubtedly benefit from support of firmer theoretical structure—task analysis currently is very situation specific, therblig analysis slights interactive and contextual effects and so on--sound theory might clarify some of the many problems involved with the use of these tools. (These, incidentally, are examples of two applied tools that have grown to be used extensively without possessing adequate theoretical structure.)

Again, at the applied level, some of the problems attendant to simulator design and utilization undoubtedly would benefit significantly from a more thorough understanding of the fundamental nature of the perceptual/cognitive processes involved. Present simulator programs strive for, and achieve, considerable refinement in what might be termed hardware fidelity while achieving a largely unknown degree of psychological fidelity, with the greatest weakness being in the perceptual/cognitive area--coding, retrieval, processing, etc. Psychological fidelity is, or should be, what simulation is all about. (The practical rewards here are, of course, staggering. It has been estimated, for example, that simulator costs are approximately 10-15 percent of flying costs; the utilization rate of a simulator is eight to 10 times that of an airplane.) (Anon., 1973)

As implied previously, in our view man-computer interaction is the essence of systems research, at least when viewed from the human factors point of view. The excellent work that has been done at establishments such as SDC, AMRL and its contractors, and other institutions seems to have faltered. (For a magnificent summary and evaluation of this field, the entire profession is indebted to Parsons--probably the only person who could have written "Man-Machine Systems Experiments" (Parsons, 1972)).

We definitely recommend a sustained program in systems research from the human factors point of view with major emphasis on man-computer interaction and ecological variables. Whether this should be done in-house, under contract, or a combination of both, we do not know, but it must be adequately and consistently supported over an extended period of time. The alternative is the present program whose support waxes and wanes with the result that man-computer interactions are poorly understood resulting, certainly, in less effective computer utiization and perhaps even less than optimal computer design. And, consistent with statements made previously, systems research of this nature not only will provide opportunities to try to apply and to assess the adequacy of theories such as Sternberg's but also will yield important clues regarding where it might be most fruitful to apply future funding in the important area of mental abilities. We understand single variables reasonably well in the systems context; a full appreciation and exploitation of the nature of interaction awaits dedicated, sustained inquiry. As Gall has stated, "...the fundamental problem does not lie in any particular systems but rather in systems as such. Salvation, if it is attainable at all, even partially, is to be sought in a deeper understanding of the ways of systems, not simply in a criticism of the errors of a particular system." (Gall, 1977) (Incidentally, Gall also has a series of axioms, theorems, and laws that humorously, and often bitingly, illustrate many of the current problems with systems. For example, Axiom No. 1 states, "Systems in general work poorly or not at all." Corollary No. 3 states, "The bigger the system, the narrower and more specialized the interface with individuals." Another: "A complex system that works invariably evolves from a simple system that worked. A complex system designed from scratch never works and cannot be made to work. You have to start over, beginning with a working simple system." Gall is recommended reading!)

Pew and his associates are also among those who fully support programs to advance the state-of-the-art in systems modeling technology (Pew, et al, 1977). Their excellent report merits careful consideration.

Finally, as long ago as 1953 and again in 1956, the late Paul M. Fitts urged that more support be given to "information transmission and processing" and to "decision processes" (Fitts, ed, 1953; Fitts, 1956). A quarter of a century later we are only a little bit better off with an enormous amount remaining to be accomplished in, what we consider to be, the most critical of all the human factors areas. It was one of Fitts' students who stated that we have made mistakes in the past regarding the matching of input data with sensoria and mistakes matching output with the neuromuscular; we seem about to repeat these kinds of mistakes with respect to the relationship between higher mental abilities and computers (See also Pew, et al, 1977).

NEUROPSYCHOLOGY

Many times during the writer's career someone seemed on the verge of bridging the gap between neurophysiology and psychology. While interesting relationships were often established, the theories always fell far short of capturing the full flavor and richness of human behavior. However, significant advancements are occurring and at least two deserve careful consideration for support.

The first is the evoked cortical potential work. Interestingly, it is the power of the computer that has facilitated what appears to be a major breakthrough in this area by making possible the uncovering of signals that characteristically have an amplitude only 10 percent or so of that of the background EEG activity. Great improvements in recording, analysis, and interpretation have been made since the early superposition work of investigators such as Dawson (Dawson, 1947) until today researchers such as Defayolle in France and O'Donnell and his associates at AMRL are obtaining positive results with everything from fitting glasses to infants to an improved understanding of concepts such as mental load. (Defayolle, et al, 1971; O'Donnell, et al, 1975; Gomer, et al, 1976; Gomer, et al, no date). Motivational, attitudinal, and expectational factors have been shown to affect the EP. New insights into areas such as vigilance and task demands are being made. O'Donnell speaks confidently of a neuropsychological performance battery. The list is almost endless.

Should OSR support basic research in the EP area? Absolutely! How can OSR afford not to?

What specifically should be supported? We would favor investigations of reliability, variance, individual differences and, of course, generality of application. Fortunately, the Air Force has one of the world's acknowledged experts in this area--Colonel Robert O'Donnell. We would defer unhesitatingly to his recommendations.

The adaptive network concepts of Klopf of the Avionics Laboratory merit serious consideration for support. (Klopf, 1978) Klopf's ideas offer several attractive possibilities. First, it assumes a role for the basic neuron that requires more of it than a simple "on-off" response. (I recall from my first course in neuroanatomy wondering why the Creator made neurons so complex if all they had to do was say "yes" and "no".) Second, Klopf's interpretation of intelligence as an emergent phenomenon based on the fundamental goal-seeking properties of $10^{10}$ neurons and $10^{14}$ feedback loops seems consistent with the general nature of systems with a wealth of interactive possibilities. Third, the theory assigns significant responsibility to the midbrain and thalamic reticular formation (MTRF)--areas that many researchers have suggested as the focus for more responsibility in the system than has generally been acknowledged. Fourth, the theory is consistent with reinforcement theory--a well-established theory of human behavior. Fifth, if Klopf is correct, here is a bionic model that may prove of considerable value--may serve to "drive" hardware development and, through a more valid understanding of mental processes, assure greater compatibility between man and computer in complex Air Force systems, perhaps offering a viable and attractive alternative to artificial intelligence.

## TECHNOLOGIES OF BEHAVIOR--WITH EMPHASIS ON CLUES FOR BASIC RESEARCH

Two decades have transpired since Bray and his associates wrote their "Technology of Behavior" papers. (See Bray, ed, 1960 and Bray, 1962). Unfortunately, their prescience was appreciated by very few, although they perhaps did get ARPA more involved in human performance and at least one group in the human factors area, Eckstrand and his associates, grasped the idea and have done an admirable job of developing a technology of training.

We recommend that OSR become actively involved in encouraging the development of technologies of behavior, not necessarily supplying the funds but helping to decide which areas are to be treated and in what order. Their goal would be essentially as defined by Bray almost twenty years ago: "If decisions about people are to be improved, the managers of people should be assisted by a growing technology of human behavior, a technology which aspires to the level of content and effectiveness of the engineering technology on which the art of weapons management now rests." (Bray, ed, 1960). We would add only a comment emphasizing the importance of these technologies to systems designers, to those responsible for the design of simulators and other support components, and to those responsible for training and maintenance of proficiency. We are not sure exactly what form these technologies will take--this may require a bit of experimentation itself. Hopefully, the data from some, if not most, of the areas can be translated directly into terms already familiar to the engineer. In others, such as motivation, we may have to insist that the engineer learn a little of our language.

Such a program would have other substantial benefits. Some examples include: (1) Identification of specific areas that would benefit most from a thorough, integrative review. It is well to stop and "tidy up the areas", as Singleton puts it, once in a while. This, in our view, is as valid an activity for OSR as supporting the gathering of more data points. (2) Disclosure of gaps in the theoretical basis of areas that would be candidates for 6.1 support and would serve to confirm or refute the recommendations of individual scientists. (3) The actual translation of theory into data usable by designers would do much to disclose weaknesses in theory and available data. We suspect, incidentally, that this will confirm our previous assertions regarding the lack of consideration given to ecological variables in traditional human performance research--whether from experimental or engineering psychology. (4) Some areas might be found so wanting that consideration should be given to the establishment of centers or institutes to assure their adequate treatment, e.g., systems research. Bray recommended support of centers or institutes for man-machine systems (see our man x computer area above), intellectual skills (see our emphasis on perceptual/cognitive skills), team performance, organizational research and persuasion and motivation. (We haven't come very far in 20 years have we? Think where we might have been if support had been made available to implement Bray's recommendations!) (5) A fifth reason for the development of behavioral technologies is that they will facilitate early, continuous, effective participation by human factors specialists in the design process. Finally, it might help convince some engineering educators that they should include the behavioral technologies in their curricula.

Predicting what may result from these processes (reviews; development of behavioral technologies) is perhaps both hazardous and foolish. However, our acquaintanceship with human engineering applications problems suggests that at least the following areas will emerge as worthy of basic (6.1) and/or applied (6.2) research:

1. Scaling. The writer has been advocating more work on improved scaling for many years. The reason is simple. It would greatly facilitate designers' use of our data appositively with other parametric information if the data were expressed in terms of objective scales, preferably ratio scales. We also need clarification of the uses and limitations of ordinal and interval scales. The splendid work of Smith Stevens should be extended with special emphasis on the types of variables important to design engineers.

2. Sensitivity Analysis. Increased emphasis is needed in this area. For too long, too many human factors recommendations have been based almost entirely on statistically significant differences--differences, as we have inferred previously, that often might appear to an objective, outside observer to be of questionable practical significance or at least whose applicability to actual design problems is largely unknown. Functional relationships must be established with abscissa manipulable by designers. Examination of the nature of these relationships should be a requirement for any reviewer of areas as recommended above. Pew reports, for example, that a relatively crude model of scanning behavior is adequate since human performance is relatively insensitive to changes in scanning strategy. (Pew, et al, 1977). More information of this sort is needed.

3. Human Reliability. Swain at Sandia is known throughout the world for his pioneering work in the human reliability area . His work has found direct application in the areas of nuclear weapons and the nuclear power industry (Swain and Guttman, 1980). The work on human reliability of Swain and his associates deserves careful consideration by OSR, the Air Force and DOD in general. It is one human factors area in which DOD is not the leader and could learn from another agency. Askren and Regulinski have worked with continuous activities, although their research seems to have stopped.

There is a clear requirement for handling the human performance aspects of systems reliability analyses. Such analyses are even finding their way into products liability/products safety law suits; such suits are becoming of considerable concern to many aerospace contractors. It may not be too long before government employees engaged in human factors research will be called on to testify regarding the effects of particular design features on human (and, thus, systems) reliability. Valid information in the human reliability area would serve them well at that time.

4. Human Factors Data Bases. While he was with the National Bureau of Standards, Van Cott did yeoman's work in laying the foundation for a national repository of standardized, validated human factors data, including human reliability data. (Van Cott, 1978; Van Cott, 1979; Teichner, et al, 1979). The writer served as a consultant to NBS on this program (Christensen, 1978).

Participation in the development of human factors data bases would help OSR officials decide where to allocate their 6.1 money since attempting to develop a standard ergonomics reference data system ("SERDS, as Van Cott termed it) would disclose gaps and weaknesses in our technologies which, in turn, should suggest fruitful areas for 6.1 support.

5. Expert Opinion. The Delphi technique has lent respectability among engineers to expert opinion! It is a relatively crude technique. We very much favor the use of expert opinion, and, when properly gathered and treated, it can be scaled. We favor a paired comparisons approach since man's ability to make exceptionally acute discriminations using paired

comparison methods is well-known. We are not recommending further basic research in this area; however, a review and critique of available techniques might be useful.

6. Criteria. The criterion problem seems ever to be with us. Leuba placed it in perspective when he stated, "There are many ludicrous errors in quantification as it is practiced today, but none is quite as foolish as trying to quantify without a criterion. It is awkward enough to quantify the wrong thing when a criterion exists, but is a sham of the most unprofessional sort to quantify in the absence of a criterion. If a criterion does not exist, it must be created. It may not be inferred." (Leuba, 1964). Replogle has caused us to pause with his demonstration of lack of correlation betwen a long-favored mesure, time-on-target, and actual hits. The late Rains Wallace presented a beautiful discourse on the problems attendant to the development of adequate criteria. (Wallace, 1956) We can add little except to urge OSR to be ever-sensitive for fresh approaches to this continuing problem. Its importance for the success of programs such as SERDS is evident. In fact, it is essential for any progress. As Singleton says, "The classification of objectives cannot be avoided because it determines the criteria and without criteria measures we can make no progress." (Singleton, 1976)

7. Stressors. The state-of-the-art with respect to the effects of individual physical stressors is relatively good; the state-of-the-art with respect to the effects of combinations of physical stressors is very unsatisfactory in spite of efforts by scientists such as Grether, formerly of AMRL, to correct the situation. This must be corrected; but we don't know exactly what to recommend. Good human performance theory in the area of multi-stress, at least to our knowledge, simply does not exist. Perhaps this is an area that should be supported in a separate institute or center or perhaps, as one individual whom we interviewed suggested, even in a foreign country. Particular emphasis should be placed on multi-stressor effects on higher-order functions, since it is evident that, more and more, these are the reasons for having human beings in systems. Special attention should be devoted to those stressors attendant to long-term missions in space. This, again, is evidence for a more representative sampling of the environmental variables in the human factors area.

Multi-stressor research is not easy to do. Experimentally, it is among the most difficult of areas since it demands extremely careful definition and construction of stressor stimuli. Further, constraints on the use of human subjects in these types of experiments have been radically increasing. Follow-up after the experiment is essential to ensure no negative aftereffects (Cohen, 1980). Finally, these types of experiments are expensive, usually requiring elaborate equipment and controls. Just at a time when we need basic research data on the effects of multi-stressors, there are these many reasons why this kind of research is being discouraged. The only feasible alternative at this time appears to be complex task simulation with careful medical and psychological monitoring of the subjects. It is no longer a question of whether or not such experiments are needed, but whether they can be done at all.

8. Neglected Groups. Renewed dedication to the principles on which this nation was founded demands that attention be given to the capabilities of females, minority groups, the elderly, and the handicapped. Obtaining adequate human performance information on the first two groups should be easy to justify. It would probably be impossible to get Air Force support for the study of the elderly and the handicapped although our personal feelings are that they, too, should have the opportunity to be of service to our nation in times of emergency. We do them an injustice by not letting them participate actively in the defense of our country and since there are thousands of jobs "behind the lines" that the elderly and the physically handicapped can perform extremely well, we would like to see them given this opportunity. We need a national assessment of all human capabilities.

Industry's experience with the physically handicapped, for example, has been very rewarding. They are conscientious, dedicated employees, less prone to alcoholism, and so o. . Why doesn't the Air Force consider how it might use them? And what an opportunity for the life sciences!

9. Motivation. We believe that the area of motivation--the "will do" part of the "can do/will do" paradigm--is ready for the "technology treatment". Welford states, "The extension of ergonomics interest from ability to willingness is a logical one, and appears to be well within our grasp." (Welford, 1976) Again, the very process of trying to develop a technology of motivation should yield valuable clues as to where more basic research support might be needed. Among other things, it is almost certain to highlight the need for more information regarding personality variables. Virtually nothing has been done with regard to the interaction of various personality types and design of jobs, equipment, training equipment, and so on. Muckler (personal communication) asks the very cogent question, "Do different personality types benefit differently from simulator training?" We simply don't know.

10. Design for Safety. Major William Elliott, formerly Chief of Aviation Law at HQ., USAF, informed us that products liability suits against aerospace companies by Air Force personnel or their surviving relatives are becoming increasingly common. It would be extremely helpful if one were to put between covers human factors data and principles that relate specifically to design for safety. It might be termed a "human factors safety technology". As we implied earlier, it is surprising that an Air Force human factors specialist has never been called to testify as an expert witness in one of these lawsuits. Our limited experience as an expert witness leads us to suggest that the development of this technology would disclose that we know far too little about such areas as perception and interpretation of risk, the effect of perceived risk on performance, training to recognize and evaluate risk and, surprisingly, even how to design a truly effective warning sign or system. (See also our comments above on "Human Reliability".)

11. Excellence. This is a favorite of ours but one to which, as far as we know, the Air Force has given virtually no attention. We would like to see an intensive study of those who are superior at their jobs--pilots, radar operators, maintenance men, and so on--the Chuck Yeagers; the Neil Armstrongs. One is recurrently impressed with statements such as the one we

heard during one interview; towit, "Ninety percent of the kills are made by ten percent of the fighter pilots." Perhaps a study of the top ten percent in various lines of endeavor would yield clues regarding their skills and practices that, through selection, training, and design, would make them more generally available--in other words, can we change existent distributions of skills from normal (if that is what they really are) to distributions that are negatively skewed? A technology of excellence?!

12. Job Enrichment ("Quality of Work-Life"). Job enrichment is one of those very attractive sounding concepts that has not always lived up to its promise in industry. Sometimes it has been successful; sometimes its application has failed so miserably as almost to bankrupt the companies that tried it. We suspect the reasons are to be found in a lack of commitment by management, a lack of understanding of the concept, a lack of sustaining theory and perhaps a misapplication of those few principles that seem to be fairly well established. If and when the Air Force decides to try to "enrich" its jobs, we urge that a very careful review of successful programs and abandoned programs be made. In addition to "support of top management", which is always mentioned, there almost certainly are principles associated with such factors as age, skill level, personality-type, and so on that are significant determinants of success or failure in job enrichment programs.

13. Small Group (Team) Performance. More needs to be known about the characteristics of these sub-systems. For example, it will be a critical determinant of success in manned space vehicles. Enhanced individual productivity, based perhaps on successful application of a Technology of Motivation, may not always result in better team performance because of interactive effects. The same applies to teams as sub-systems in larger systems. Again, Singleton says it very well, "An over-productive individual can upset the productivity of the team, a highly productive team is not necessarily the most effective component in a larger system . . ." (Singleton, 1976), due, we are sure, to the interactive effects which very much need additional research attention. Attention to this area and to motivation will almost certainly lead OSR into the area of personality, an area that, regrettably in our opinion, has been ignored for too long by engineering psychologists.

14. Dynamic Anthropometry. Of basic importance to human engineering/ergonomics is dynamic anthropometry - that is, the dynamic physical capabilities of people in their work environments. Owing to the almost unlimited number of variables (i.e., tasks, work stations, environment, body-support configurations, personal requirements, etc.), obtaining the required information from actual measurements of subjects in a dynamic situation, real or simulated, is extremely difficult. To date, the most efficient approach to this problem has been by simulation of the body and its work stations. Programs such as AMRL's Combiman and ATBM (Articulated Total Body Model) and the work supported at Michigan State University are examples of research fundamental to realizing significant improvement in the fidelity with which the size, mass distribution, and mobility characteristics of the human body can be represented by three dimensional physical and/or mathematical analogues.

Dynamic anthropometry, in our opinion, represents a very significant improvement over traditional static anthropometry, which contributed so much in the past to the design of Air Force work stations and personal equipment. This area, which is fundamental to design for human use, must receive adequate support.

METHODS

As stated earlier, it is axiomatic that any branch of science makes progress pretty much as a function of its tools. (We have already made reference to the need for improved scaling.) OSR, in our view, should actively support consolidation of our knowledge regarding our present tools--their strengths and weaknesses--and, where indicated, should support the development of improved tools. Here, again, the "shopping list" is of almost infinite length and those responsible for the Life Sciences program are in the unenviable position of having to decide "where and how much". You can rest assured, however, that if you supply a new tool, someone will pick it up and use it. So it is important to supply good ones. As Colby's Second Law of the instruments states, "Give a small boy a hammer and suddenly everything needs pounding"--give a scientist a new tool and suddenly . . .!"

As a general (and serious) rule, however, we feel strongly that OSR must support the development of models and methods of analysis that treat the myriad of variables and their interactions, including their interactions over time, that characterize human behavior. Models that require the user to vary only one variable at a time are insufficient; even the complex analysis of variance and multiple regression models are inadequate. The understanding of human performance in all but the simplest situations is much too complex to yield satisfactorily to such treatment--"t" ratios, "F" ratios, etc. are certainly not the complete answer. Unfortunately, we don't know the answer. However, we remember several years ago being introduced to Brunswik's "lens" model. This would appear to be the sort of model we're talking about. The problem needs serious attention and by someone who thoroughly understands the mathematical/statistical problems involved.

Petrinovich (1979) has beautifully and precisely articulated the weaknesses in current methods. We strongly recommend that this article be carefully studied; we believe that it has extraordinary value for the development of improved OSR research programs in human performance and, by implication, selection of researchers to carry out those programs.

High priority should be given to method and tool development that might be needed in our most highly recommended research areas--man x computer and neuropsychology and related behavioral sciences. Others (some of which would find application in the previously mentioned areas) would include the following.

1. <u>Operations Research</u>. Perhaps it is unfair to OR scientists to list this among the methods. At any rate, we feel strongly that OSR should support increased cooperation between OR scientists and behavioral scientists. We have personally observed the remarkable skills that some OR people possess when it comes to modeling complex situations. The right psychologists working with the right OR scientists are virtually certain to produce impressive, worthwhile results. It has already happened to some extent. See Pew et all (1977) for an excellent account of the use of OR (and other) models. We feel that this area deserves considerably more support by OSR than it has received in the past.

The choice of the members of the research team is crucial here--even more so than the topic that the team chooses to address initially. The experimental or engineering psychologist must have a strong background in mathematics and in modeling; the OR scientist must have, among other virtues, an appreciation for variability--within and between individuals.

2. <u>Taxonomy of Methods; Standard Methods of Measurement</u>. These extremely important areas were brought to our attention by Dr. Bryce Hartman. Their importance is obvious. We need, as mentioned previously, to have available complete descriptions of our tools--how to use them, their virtues, their weaknesses, their applicability in different situations, etc. Standardization of methods in specific areas of inquiry would facilitate interpretation and validation of experimental results by making cross-comparisons easier and would greatly facilitate the work of those who might be asked to develop a technology in a specific area.

3. <u>Field Study Methods</u>. The program above should explicitly include field study methods--another area suggested by Dr. Hartman. We, as do the industrial engineers, prefer to "go to the floor" ("field" in our case) when a problem arises. But improved methods for use under various circumstances need to be known and made readily available to potential users.

4. <u>Small Sample Methods</u>. Warrick (personal communication) has advocated more work in this important area. We need, also, it seems to the writer, more attention by experimenters to the <u>content</u> of their experiments and perhaps less on the sophistication of their statistical models--a sophistication which usually makes demands on the experimenter regarding basic assumptions that are seldom, if ever, met. The work of Bradley (1960) regarding the effect of violating the assumptions of various statistical models is an example of what we are talking about.

5. <u>Cost-Benefit Models</u>. We have already mentioned the need for better sensitivity analysis of human factors data. This would contribute to cost-benefit analysis. We frankly don't know specifically what to recommend here. We are sure of two things, however; costs are much easier to assess

than benefits and the power of the MBA's, cost accountants, and economists make it mandatory that we at least try to figure out how to conduct more and better cost-benefit analyses in the human factors area.

6. Systems Application Models. Hopefully, the technologies referred to previously will make the job of the design engineer and human factors applications specialist easier. A tool that shows promise for such endeavors is that developed by Ostrofsky (Ostrofsky, 1977). OSR should support its extension and test with a wide variety of systems under a variety of circumstances so as to learn more about its generality and other features. It needs independent test if such has not already been done. Here, again, such an effort will disclose areas that need additional basic and applied research support.

## SUMMARY AND CONCLUSIONS

We cannot think of a single topic in the general area of human performance for which we could not justify OSR support. The systems of the Air Force and the management of the Air Force require about every skill that mankind has ever developed.

However, we render no service at all by recommending that everything be supported because then probably nothing gets supported adequately. We have intentionally virtually ignored the results of previous committees in this area; we don't even have a copy of the current contractual program since we felt that this would introduce a biasing factor with which it would be difficult to deal. What has been recommended is primarily the result of personal reflection on over 30 years of experience, an enormous amount of reading, and conversations with three highly respected peers--Alluisi, Hartman, and Muckler. (They, however, should not be blamed for the flaws in this chapter; that responsibility is totally the writer's).

We recommend that major emphasis be placed on a relatively few areas and that attempts be made to get other organizations to develop support for important areas that OSR cannot support. There are two general areas that we feel need immediate, sustained attention. The first is the man x computer area which, in our view, includes the perceptual/cognitive area. The second is the neuropsychological area. We hope that we have adequately supported our contention that these are of utmost significance.

Thirdly, we favor strong support, hopefully along with support from other 6.1 agencies and 6.2 programs, of the development of selected technologies of behavior. Considerable progress has been made in the training area; other areas are ready for treatment--motivation is an example of one that we suggest might be worthy of immediate attention. This exercise, developing technologies, will itself serve as a very effective tool for disclosing significant gaps in basic knowledge. We specifically include under human factors technology development, state-of-the-art reviews in candidate areas where suitable reviews from the technology development point of view do not already exist.

Fourthly, we favor support of joint OR-psychologist projects. The topic could well be drawn from the previously mentioned areas of great importance--small team performance, for example.

Fifthly, we strongly support the development of a "technology of human factors safety". The products safety movement that is so strong in the civilian sector has already had its effects felt in the military sector. As we find it necessary to maintain a high degree of operational capability in peace time, so will we find it to our advantage to make adequate provision for safe operation of those systems.

The many other areas are included only to bring them to the reader's attention. Circumstances unknown to the writer may demand that some of them receive immediate attention. Bear in mind, however, that as a matter of research policy, we tend to favor more support of a few very important areas rather than limited support of a relatively large number of areas.

We recommend also that a small amount of funds be set aside each year to support a few very carefully selected investigators who are acknowledged to be unusually creative, dedicated individuals. We would favor giving each of them a modest amount of money each year for, say, five years. This would be a clear recognition of what we feel is the single most important aspect of a basic research program--the selection of the principal investigator. We feel confident that at the end of the five-year period these might very well be considered some of OSR's most productive contracts.

Finally, we feel that an assessment of OSR's program by one or more foreign ergonomic experts would bring not only a slightly different point of view to the program but also might well inject a note of freshness and renewed vitality into the program. Several names come to mind, among whom Singleton, Welford, Broadbent, and Grandjean would be outstanding choices. Lomov and Zinchenko would be excellent; however, we doubt that their government would make them available!

# V. MANAGEMENT OF BASIC RESEARCH PROGRAMS

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## MANAGEMENT OF BASIC RESEARCH PROGRAMS

### INTRODUCTION

In this chapter the management of research programs will be considered -- particularly basic research programs. Some comments will be offered regarding the nature of basic research (what is it that is being "managed"?), the current climate for basic research, and what might be done to increase the productivity of basic research programs. We make frequent use of quotations; to quote Montagne, "I quote others only to express myself better."

The term "basic research" as used here generally refers to the systematic investigation of phenomena that has as its primary purpose the development of knowledge. We include also investigation of problems of measurement or methodological research in our definition of basic research, because without the development and continuous improvement of its tools, no scientific area can continue to make progress.

Some will say that theoretical and basic research is not the business of the Air Force. We cannot agree. The Air Force is a vast technologocal system and it needs the products of basic research to continue and improve its development.

We realize that we are members of a very small minority when we encourage substantial independence of basic research (6.1) from 6.2 and 6.3 activities. No less an authority that Arnoult and his distinguished team have stated, "The research (6.1) programs of the Services and ARPA should be much more closely tied with the exploratory and advanced development (6.2 and 6.3) programs of the in-house laboratories ..." (Arnoult, 1969). Marlin and Bloch have stated, "It is of the highest concern to the Department of Defense that research conducted in its behalf be applicable and usable." (Marlin and Bloch, 1977) It is not surprising that a statement such as this would come from a center for economic analysis. However, a somewhat less strident but similar position is taken by Biel and his committee and by Briggs and his committee (Biel, 1969; Briggs, 1969).

It is our contention that in the long run it is not in the best interests of the Air Force to tie 6.1 research too closely to 6.2 and 6.3. The goal of Air Force-sponsored basic research is to solve long-term and particularly difficult operational problems. The goal is not short-term needs as expressed in 6.2 and, specifically, 6.3 projects.

One major product of basic research is quantitative theory. As the physical sciences have shown, good theory solves many problems. The Life Sciences in particular need to develop better quantitative theory. Let's examine, for example, the position taken by Licklider who stated, "The problem is not simply that we need theory; we need mathematical structure with which to develop theory." Certainly, this is especially true in engineering psychology and engineering biology. Licklider's view on the level of sophistication involved here is well expressed when he states that the late, great mathematician, Von Neumann, believed that whole new areas of mathematics would have to be developed if the social sciences are to be put on an adequate quantitative basis. As expressed elsewhere in this report, we believe that a much closer working relationship between experimental psychologists and operations research experts (carefully selected for their understanding of such concepts as individual differences and other behavioral phenomena) could do much to exploit current mathematical techniques and yield specific clues as to where further mathematical development is needed.

Good theories are those that not only can account for established facts in a given area, but also can serve as the basis for effective applications. This is <u>not</u> a contradiction of the earlier statements regarding the perceived need for substantial independence of 6.1 programs from 6.2 and 6.3 programs. It is good to derive applications from basic research; however, the converse is not necessarily good because is places the 6.1 program in the role of a follower and not, as it should be, a leader. Besides, the applied problems that are in need of immediate solution will have been solved, one way or another, long before adequate theory can be generated to solve them. Certainly the opinions of those in 6.2 and 6.3 programs should be sought by the 6.1 manager but they must be considered along with the opinions of those deeply involved in theoretical and futuristic developments. The 6.1 program must not be driven by the 6.2/6.3 program or we will forever be solving short-term needs.

Theory, however, like so many good things, is not without its hazards. As Welford has stated, "We systematize the plethora of facts into theories of which we than become prisoners, unable to look beyond them to the full richness of the real world outside." (Welford, 1976) While we recognize that for short-term problems, tight planning and control are essential, for longer term problems, creativity is essential.

Bateson suggests that progress is best made by a varying process of strict and freer thought. He states "...whenever we pride ourselves upon finding a newer, stricter way of thought or exposition; whenever we start insisting too hard upon operationalism or symbolic logic or any of these very essential systems of analysis, we lose something of the ability to think new thoughts. And, equally, of course, whenever we rebel against the sterile rigidity of formal thought and exposition and let our ideas run wild, we likewise lose. As I see it, the advances in scientific thought come from a combination of loose and strict thinking, and this combination is the most precious tool of science." (Bateson).

It seems to us that the manager of a basic research program must determine in which stage the theories and knowledge of his major areas of interest currently reside and then decide whether, at the moment, each area needs a looser or stricter treatment. He should be aided in this difficult task by reviews and consolidations of what is currently known, performed by respected authorities in the field. We strongly support the development, where they do not already exist, of major, tough reviews of areas important to OSR. (It helps in science, as in other endeavors, to stop and catch one's breath once in a while.) Such reviews should not be restricted to basic or theoretical studies, as many applied studies have information of importance to the development of better theory. Kulik and his associates, for example, have developed a promising method, "meta-analysis", based on statistical analysis of a large collection of individual applied studies, for drawing out the reliable and generalizable principles that may be embedded therein (Kulik et al., 1979).

## CLIMATE

The climate for basic research has never been outstanding in our country. There are many reasons for this. First, we seem to resemble Romans more than Greeks -- we would rather build a road or conduit than study the fundamental nature of materials and methods that might eventually result in better roads and conduits. This is quite understandable. The great pioneers, agricultural and industrial, who settled and developed this country could hardly wait for better theories regarding how to proceed! (And yet, as only one example from many possible, we cannot help but observe the incredible improvements in quality and production that the application of science has brought about in agriculture--once it was recognized as an area for intensive basic research.) But a crisis is now at hand. As Anderson said almost a decade ago, "The United States must soon face the fact that it is living on research and development capital and the account is beginning to run low." And later, "...we must correct negative attitudes toward science and technology in the United States...". "To attack the value of this process is to discard our best tool in achieving whatever goals we might formulate for the nation." (Anderson, 1971) Unfortunately, as we shall see later, it is frequently under attack -- especially in the life sciences.

Second, the funding processes of our government do not favor and, in fact, often discourage the support of basic research. Funding is usually on a year-to-year basis. Effective basic research needs a longer, more stable funding base. Short-term funding virtually forces an investigator to produce something every few months that will assure continued funding for him and his staff.

Further, the accounting procedures of the government have forced him to spend an inordinate amount of time keeping records, corresponding, filling out forms, etc. Ableson, the editor of Science, has stated "...the government in the name of accountability required the universities to create vast bureaucracies which produce nothing while devouring hundreds of millions of potential research dollars annually. When a scientist notes that high grant proposals are inflated by as much as 90 percent overhead charge and then later has to deal with arrogant clerks, morale sinks." (Ableson, 1979) And even if the number of hours so spent is not excessive, such activities constitute a constant source of worry and demands on the scientist's attention that might be better spent in more creative ways. Add to the paperwork burden the time that the university professor spends preparing and delivering lectures, serving on committees, etc. and it is little wonder that he has very little time to concentrate intensively on research. Those engaged in basic research must be held accountable, but it must be a valid and efficient accountability.

Universities used to provide perhaps the best climate for basic research. As suggested previously, this is not necessarily true any more. Besides the factors mentioned above, an untenured university professor is under constant pressure to produce journal articles and/or texts. Otherwise, his chances of receiving tenure are virtually non-existent. (Again, note the pressures to neglect long-term research.) Many universities have attempted to solve this problem by establishing separate institutes, dedicated almost solely to research. Unfortunately, the scramble for money is probably more intense there than in the departments and the chances for tenure are definitely diminished. Too often, the scientists and engineers in university institutes are looked upon as second-class citizens.

At least some industrial laboratories (Bell Telephone and DuPont are prime examples) provide a climate that appears more suitable than that of most universities for long-term research. (One of them, incidentally -- Bell -- supported the first industrial human factors laboratory in the United States under the leadership of John Karlin and has been a strong supporter of human factors ever since. It perhaps is no coincidence that a NSF-supported study disclosed that the fastest growth rates in industry are being experienced by those industries that have put the greatest proportion of their resources into research and development. Over 70 percent of all scientists and engineers were employed in industries whose growth rate exceeded the average while less than seven percent were employed in industries with declining growth rates. (National Science Foundation Report 79-307, 1979).

The climate for science will not significantly improve, however, until the Congress senses that the nation wants it so. And the most immediate way to get the word to Congress is through paid representatives. (Hopefully, better general education will help solve the problem in the long run, but we can't wait for that solution.) We used to believe that no portion of our dues to scientific societies should be used for political purposes; this attitude is naive and self-defeating in this less than ideal democracy. Scientists must make their voices heard in Washington if the funds that are so desperately needed to carry out research on the great unsolved problems of our society are to be forthcoming.

A major problem may be that the nation does not understand the <u>time</u> needed for application of basic science. The application of basic research appears to "peak" between 20 and 30 years while applied research appears to peak in less than 10 years. Basic research is very much like the planting of a seedling--you do it for the next generation. This 20-year period, incidentally, is, according to Adams, probably the reason that Project Hindsight was such a triumph for <u>applied</u> research while TRACES was a triumph for <u>basic</u> research--Hindsight limited itself to 20 years and if one is trying to justify basic research, twenty years is probably no better than the <u>mean</u> period of time that must be examined. Of course, if Copley and McMasters (1978) and others are correct in their prediction that a major international war, involving the USA and USSR, is virtually inevitable within the next five years, then OSR should cease all basic research and devote its energies to forming cave-digging and food storage clubs.

Contrast the negative attitudes about science with some of the beautiful expressions of Thomas in his delightful article, "Notes of a Biology-Watcher". Thomas states, "The essential wildness of science as a manifestation of human behavior is not perceived. As we extract new things of value from it, we also keep discovering parts of the activity that seem in need of better control, more efficiency, less unpredictability. We'd like to pay less for it and get our money's worth on some more orderly, businesslike schedule. The Washington planners are trying to be helpful in this, and there are new programs for the centralized organization or science all over the place, especially in the biomedical field.

"It needs thinking about. There is an almost ungovernable, biologic mechanism at work in scientific behavior at its best, and this should not be overlooked.

"The difficulties are most conspicuous when the problems are very hard and complicated, and the facts not yet in. Solutions cannot be arrived at for problems of this sort until the science has been lifted through a preliminary, turbulent zone of outright astonishment. Therefore, what must be planned for in the laboratories engaged in the work is the totally unforseeable. If it is centrally organized, the system must be designed primarily for the elicitation of disbelief and the celebration of surprise." (Thomas, 1973).

While we agree with Birt and Kemmerling that human engineering should be shown to be cost-effective (Birt and Kemmerling, 1978), developing cost-effective indices for basic research is extremely difficult. One reason for this, as was mentioned above, is that the average lag time between discovery of a scientific principle and its application is between 20 and 30 years.

In the area of basic research it is much easier to identify costs than it is to demonstrate benefits--particularly when the evidence suggests that the benefits may not be realized in less than two to three decades. The scientist is doomed to defeat in such an arena. E.F. Schumacher put it rather well when he stated, "To undertake to measure the immeasurable is absurd and constitutes but an elaborate method of moving from preconceived notions to foregone conclusions; all one has to do to obtain the desired results is to impute suitable values to the immeasurable costs and benefits." A more productive activity would be to devise ways to cut the 20-to-30-year time lag from discovery to application and assure a quicker, smoother transition from basic laboratory to market place, without, incidentally, inducing unacceptable cultural shock.

What to do? It appears that in the current climate the scientist and the research manager must make every effort to show benefit from his research. To quote Sidman, "Good data are always separable, with respect to their scientific importance, from the purposes for which they were obtained." (Sidman, 1960). Or, as Alluisi has proposed, "...themes and directions should be chosen to achieve desired technological competence without losing the benefits of basic research." (Alluisi, 1970). Such a compromise, however, requires skills in our scientists that are uncommon.

## CREATIVITY

"History provides ample evidence that the society that loses its power and facility to innovate dies." -- Representative Emilio Q. Daddario in Science, 13 Dec 1968.

Volumes have been written on the topic of creativity; no one, in our opinion, has expressed the need for it more clearly or succinctly than former Congressman Daddario. Many indices suggest that creativity is on the decline in the United States. Ableson blames lack of innovation for such problems as our decreasing competitiveness in international markets, the sinking dollar, inflation, and unemployment "...many major companies are concentrating their R&D on improvements in existing processes and products." (Ableson, 1978).

What is the nature of this most desirable but alarmingly scarce commodity--creativity? (We are <u>not</u> implying that creativity need be a scarce commodity. In fact, we would argue that it is rampant in the human race, at least at birth, but is squelched by selfish or unaware parents, by a deadening educational system and by managers who too often have their eyes on their personal power and career progression rather than the nurturing of creativity in their assigned personnel.)

The creative person seems to be able to resolve the unresolvable--the apparent conflict--the rational and the irrational--the fact that a body can be falling and at rest at the same time! He seems to be able to break out of what Kubie terms the "neurotic way", i.e., unalterable, repetitive, insatiable patterns of behavior, and to enter the realms of flexibility and imaginativeness without losing his ability to apply tightly organized thought processes to the result (Kubie, 1965). He needs to be able to let previous perceptions and cognitions "percolate" and somehow to allow the products to surface occasionally for consideration and evaluation. Much of the process of creativity is undoubtedly carried on at a subconscious level; it probably goes on in all of us but its products are probably recognized only by the few who have learned to communicate with the levels of their brains that harbor the myriad interactive activities of those levels. It sounds mysterious and it is. We desparately need tools to examine these levels--perhaps evoked potentials (EP) offers such a possibility. Perhaps with such understanding we could raise the proportion of creative people in scientific and technical organizations from the current estimates of 5-10 percent (Maugh, 1974; Ranftl, 1978).

Conditions for creativity are described rather well by the Russian author, Sheinin, who states that for successful innovations one needs financial flexibility, receptiveness to new ideas, decentralized decision making, and speed in making necessary resources available. (Sheinin as reviewed by Amann, 1978). If the idea must wait several months or years for financial support, the creator will almost certainly have lost interest and gone on to something else.

In addition, creative people are human beings! They generally respond to such motivators as achievement, recognition, responsibility, growth, and chances for advancement. They are relatively independent, restless, willing to take risks, self-reliant, and responsive to challenging goals. They often resist being "typed" or forced to join a group or society and, unfortunately, society seems intent on integrating <u>everyone</u> into <u>some</u> group or association! They are generally not concerned about "immediate results; given minimum support and encouragement, they possess incredible patience with respect to the nurturing of their ideas. They do not thrive in an atmosphere of high anxiety -- "do it today--don't put it off". The manager's job, as it is with anyone, is to determine which motives dominate such an individual's behavior and then develop and follow a reinforcement schedule that will encourage the creative person in the attainment of his goals. (It's probably not quite that simple but it is about the best that we can do for now.)

In addition, an experienced and wise manager can sometimes help the creative person distinguish between <u>currently</u> good and bad ideas--the art of "finding problems that can be solved", as Maugh puts it (Maugh, 1974). If you will pardon a personal reference, my job in "managing" Colonel John Simons, one of the most creative individuals whom I have ever known, was to help him decide which of his many ideas the world, or specifically the Air Force, was ready for at that time. Or, as Oscar Wilde put it, "There are works which wait, and which one does not understand for a long time; the reason is that they bring answers to questions which have not yet been raised; for the question often arrives a terribly long time after the answer." (Do not misunderstand our position here, however. We believe that some answers <u>should</u> be provided for questions ("requirements") which the Air Force has not yet devised. For too long, the Life Sciences, as we have stated elsewhere, have followed a respectable three paces behind the other sciences in the Air Force. We need bold, new initiatives. Consider, for example, Flexman's and Simon's "Puff the Magic Dragon", for which there was no stated requirement. We have faith that many such initiatives are quite apt to spring from <u>basic</u> research conducted by carefully selected investigators. Further, we believe that this is entirely consistent with, and supportive of, the theme of General Slay's "Project Vanguard"--to <u>drive</u> the situation and not constantly be in a reactive position. One of our goals should be to have major systems concepts "driven" by concepts from the Life Sciences.)

There have been numerous attempts to develop tests for identifying creative individuals. In general, they attempt to measure the sorts of characteristics mentioned previously. Harris, for example, in his test of creativity in engineering, identified three factors: fluency, flexibility, and originality (Harris, 1960). These three factors receive major attention in very few colleges of engineering and/or science; they may, in fact, be suppressed.

Creativity is, or should be, a part of all human activities. For example, as the Honorable Brockway McMillan, then Assistant Secretary of the Air Force, stated at the First Congress of the Information Systems Sciences, "Engineering is a creative process, one whose characteristic elements scarcely have names; they are certainly not parts of the process usually packaged into courses and taught in the engineering curriculum. Yet until the engineer masters these characteristically creative steps he no more deserves the title 'engineer' than a plasterer deserves the title 'architect'." We mention this because, in our opinion, scientists engaged in basic research in the life sciences have a golden opportunity to contribute to the creativeness of the engineer, and, thus, to effective systems; creativity should be a fundamental research topic in the Life Sciences. Imagine what the engineer will be able to create when we supply him with significant amounts of usable data, principles, and theory regarding the most fantastic component available to him--man!

## MANAGEMENT

The internationally renowned Russian engineering psychologist, Boris Lomov, who is also a member of the USSR Academy of Sciences, states that what management needs is a better understanding of the manager! (Lomov in Azernikov, 1971). It does seem that relatively little attention is paid to him--we assume he is always highly motivated, etc. But why should he be? What are his rewards?

In our opinion, the real challenge in managing a basic research program is in the selection, in the broadest terms, of the areas to be investigated and, even more important, <u>the selection of the principal investigators</u>. Note that we haven't said a thing about a proposal. We recommend selection on the basis of performance--performance is a much better guide than promise. Where does this leave the inexperienced investigator--the new Ph.D.? Right where he should be--working for an outstanding principal investigator until he has the necessary experience and record of productivity to justify supporting him as an independent, principal investigator.

How does the basic research manager select the areas to be supported? We're not at all sure. We are sure, however, that it involves more than derivations from present and forecasted requirements. To quote the Navy's McLean, whose views merit consideration because of his unquestioned success, "I don't think I've ever seen a useful piece of equipment produced by the <u>normal</u> procedure--the idea that you first write a specification and then expect the laboratory to fulfill it . . .

"How do you program research? How do you make sure that the research people are working on projects the Navy will be interested in? Well, you select people who will do <u>good</u> research.

"If the guy who is doing the research is any good, he's going to be way ahead of anybody who could review his proposal. And if you've got to pass his proposal through several committees, each of whom understands it less than he did, you can almost be sure that no worthwhile research project will ever get funded." (McLean, no date)

We agree with McLean, especially with respect to the overdependence on stated or written requirements. However, what do we propose in its place? This, we hope, is evident in the chapter on "Implications for Human Factors." In a word, we believe that development of technologies of behavior will disclose serious gaps in the present state of knowledge in each area. These areas should then be examined carefully as candidates for basic research support. In addition, comprehensive reviews of relevant areas will prove essential.

After these two critical decisions are made, it then is primarily a matter of support, encouragement, and communications. Resist with all your might the temptation to over-manage basic research, which means, incidentally, that its proper conduct requires greater self-discipline on the part of the investigators than almost any other pursuit. How one manages this essential ingredient is unknown to these writers.

Such an attitude, of course, flies in the face of such management techniques as MBO, centralization, etc., and if followed, is apt to leave the 6.1 manager feeling that he isn't really doing his job. General Holzman described the sort of plan we favor with respect to the management of basic research. "A marked trend in the Air Force management of its basic research program has been toward decentralization. More and more reliance has been placed on the judgements of those closest to the research program itself. In research, as in no other activity, technical decisions must be made at the working level, and the Air Force has adopted this as a principle of management." (Holzman, 1962-63). Maintaining a posture of decentralization requires constant attention because of the forces mentioned earlier (accountability, MBO, etc.) that encourage centralization. Incidentally, the Holzman article is proof that at least some military men do understand basic research, what to expect from it, and how to nurture it. The Holzman article, although 17 years old, is still excellent reading for all of those who are engaged in the management of basic research.

Basic research is a very risky business. The manager must be willing to defend expenditures and people whose ideas he may not fully understand. It should be abundantly clear that a good manager of basic research is, in our opinion, a person in possession of considerable courage and of deep conviction regarding the ultimate value of relatively unrestricted inquiry.

He (the manager) will be called on the carpet to defend "crazy" ideas. At that time, he should recall what Charles Kettering said about crazy ideas-- "Whenever you look at a piece of work and you think that the fellow is crazy, then you want to pay some attention to that. One of you is likely to be and you had better find out which one it is. It makes an awful lot of difference."

A basic research manager, particularly in the areas of life sciences, is always tempted to support more areas than his budget can adequately support. This is because there are so many potentially fruitful areas that could stand additional attention. Callahan feels that, in general, DOD tries to keep too many things going--"There are strong indications that the Department of Defense tries to keep twice as many projects alive as can be reasonably funded at a full level of effort. The result is that many programs crawl at such a slow rate that they are obsolete well before they are developed..." (Callahan, 1978). In general, we favor more support of fewer areas. Coordination with other research agencies, governmental and non-governmental, should enable an equitable and profitable assumption of responsibility for all. However, unless budgets are increased, choosing what to support from among the many attractive alternatives will continue to be a painful process. It is emphasized that this sort of assignment is meant to apply only to basic research and not to applied research--the former, by definition, should not be directed toward specific, immediate systems applications; the latter should.

Most of us are not as familiar as we should be with the basic research being conducted in other countries. ONR has done an excellent job in this regard but it has usually meant having selected U.S. scientists spend a year or more working out of London. We recommend that OSR invite carefully selected foreign scientists to spend a year in the United States. For example, two great scientists who have thought both extensively and intensively about the problems in Ergonomics are Singleton of the U.K. and Welford, formerly of England but now in Australia. We believe that either would have a stimulating and salutary effect on the quality of basic research being supprted by OSR in the area of human factors.

Almost any program benefits from an occasional infusion of new blood. And a lot of new blood will be available in this nation within the next few years. Report after report has commented on the "glut" of new Ph.D.'s that is just now beginning to enter the market. The universities can't possibly absorb all of them, shackled as they are with a plethora of tenured professors, some of whom disclosed their last creative act when they received tenure. If it has not done so already, OSR should begin immediately to develop a plan to utilize the services of these fine youngsters. Assigning one or two to several of its experienced, creative principal investigators would be excellent. In addition, laboratories such as AMRL and SAM with their superb facilities and many fine scientists, could offer these young scientists attractive opportunities. The country already has an enormous financial investment in a new Ph.D. and he has an enormous investment of his personal time and effort. Let's not waste this treasure; we would think that our Air Force leaders and our Congress might respond to a plan to help these fine young men and women develop useful careers in the service of their country.

Further down the road the total supply of young scientists will probably diminish. The competition for bright, young, new professionals will be high. One the one hand, the universities - our primary source for basic research - are becoming less and less attractive to the young scientist. Pay is low; advancement is relatively slow; non-research demands are increasing, etc. All of these factors are leading to fewer positions and a radical decrease in the attractiveness of a career in basic research in the university setting. At the same time career possibilities in industry and in private practice are expanding. Indeed, there is consideration among faculty curriculum developers to revise the basic university courses more towards application for the new Ph.D. The question is: How attractive will basic research be to the bright young professional emerging from a doctoral program? The future of basic research in the United States may well be in question if we cannot attract the best talent to it.

The research manager must also avail himself of the advice and counsel of his peers. Dr. Hartman of USAF, SAM agrees with us that the effectiveness of "peer reviews" would be improved if there were an opportunity for greater in-depth exchanges, including interaction at the worker level. He also suggests that joint research between in-house and contractor personnel be given greater consideration. This, we feel, would have many advantages in instances where interests and capabilities are compatible. All contracts should allow for sufficient travel to permit trips to relevant meetings and frequent communications between the OSR technical monitor and the contractor PI.

## Maintaining Personal Skills

The manager of basic research programs is in a position to do great long-term service or considerable harm to basic research. He or she will be in a critical point to determine how funds and resources are used and, in turn, what research is done and what is not done. The judgements that must be made are highly technical; they require a thorough and current knowledge of the research state of the art, and an understanding of the long-term needs of the Air Force. The judgements are predictions: they allocate resources for future scientific and applications needs. All of this requires a great deal of personal scientific and technical competence in the manager.

Unfortunately, maintaining personal professional skills is not easy for the in-house research manager. Much of his time is spent in paperwork, bureaucratic planning, report preparation, and briefings. There does not appear to be adequate time available for the technical job he is supposed to be doing.

And there is a question as to whether any scientist can maintain his or her proficiency when that scientist stops doing any actual personal creative work. There is a point of view that the scientist will remain competent only so long as he continues to be personally productive.

One (probably unsatisfactory) solution to this problem is to insist that the research manager be allowed 50% of his or her time to do personal creative work. "Management" demands on time and effort being what they are, this is by no means easy to achieve. Upper management must insist that the personal research activity be maintained, and rewarded. The benefits are two-fold: productive output is obtained from the research manager and probably his judgements about research allocations will be improved. Finally, it might be worthwhile, if desired, to substitute continuing education graduate-level courses for the research manager instead of personal research. But all of this is not in addition to his managerial work but <u>instead</u> of it.

## SUMMARY AND CONCLUSIONS

We have built a picture of the basic research manager which requires a diversity of skills including such tasks as:

- State of the art assessments of his field
- Integration of results in his field
- Defense of his programs
- Development of system drivers
- Resource allocation decisions
- Conduct of appropriate peer reviews
- Program management and control

We wonder how many individuals can fully satisfy all of these roles.

In the management of basic research programs, we believe that there is greater danger of overmanaging than of undermanaging. The prevailing climate in our nation is such that it is almost impossible to undermanage anything--poorly manage, yes; undermanage, never!

Relinquishing tight managerial control, however, does not relieve the manager of basic research of significant responsibility; in fact, it may be argued that it makes his job even more difficult. He must not, to paraphrase Emerson, "defer to the popular cry". He must, however, try to decide which <u>general</u> areas might be of the most significance to the Air Force a couple decades hence, determine which need what kind and amount of support, assure that unreasonable overlap does not take place with the programs of other basic research organizations, and, above all, select the most competent scientists available to conduct his programs. His obligations to these scientists, once selected, include relief from onerous administrative tasks, a pledge of sustained support as long as the program meets the tests of good fundamental research and is consistent with Air Force objectives and, finally, recognition and encouragement of good work. He must have enormous faith in his convictions and be prepared to defend his decisions to authorities who have a different set of managerial circumstances and criteria than he with which to deal.

The rewards may not be very great for this sort of activity--a footnote in a journal article, mention of one's name in the introduction to a paper given at a scientific meeting, etc. Additional extrinsic and intrinsic rewards can be added: better organizational recognition, performing his own technical work, etc., which might make the job considerably more attractive to outstanding personnel.

We would guess that the primary reinforcements for a basic research manager, as the job now exists, have to come from within--from a feeling that one has made a modest but enduring contribution to the welfare of the next generation. As we said previously, it's sort of like planting seedlings--you may not live long enough to enjoy the benefits.

# VI. IMPLICATIONS OF FOREIGN HUMAN FACTORS RESEARCH

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Preparation of this paper was stimulated by
an interview with the
Chief Scientist of the Foreign Technology Division (AFSC)
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# AIR FORCE NEEDS FOR RESEARCH IN THE BEHAVIORAL AND SOCIAL SCIENCES[1]

Air Force research requirements in the behavior and social sciences are broader than the discipline of psychology. The knowledge needed by the Air Force in preparing for the next twenty-five years will be developed only through carefully integrated inter-disciplinary efforts that use the expertise, theories, and methods of the physical sciences, the biological sciences, the behavioral sciences, and mathematics to successfully attack the human factors problems that relate to the development and use of advanced aero-space systems. It is absolutely necessary that we bring scientists together from the different disciplines if we are to have an effective research program.

There is a need to develop an in-service multi-disciplinary agency to attack behavioral problems that are currently neglected by Air Force human factors research programs. There is also a need for the assignment of small groups of at least two or three -- always more than one -- trained in the behavioral and social sciences to work in the engineering and product laboratories. Such an arrangement would help assure inclusion of human factors contributions during the critical stages of requirements and concepts.

Among the research needs facing the Air Force, the following are deemed to merit vigorous research attention. They are arranged in order of their priority.

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1 This chapter on the informational needs of the Air Force in the area of the behavioral and social sciences is based on an interview with Dr. Anthony J. Cacioppo, Chief Scientist of the Foreign Technology Division of the USAF Systems Command. Dr. Cacioppo is also a professionally qualified behavioral scientist and, as such, is uniquely qualified to render opinions in the areas of research in the behavioral and social sciences. The interview took place at Wright-Patterson Air Force Base on the 11th of April, 1979. Dr. Cacioppo's willingness to address this problem and to provide guidance is gratefully acknowledged. The writer, however, is solely responsible for the contents.

1. The most pressing problem to be dealt with is the information processing capacity of the human operator. There is a need for new approaches in this important area. Current research is characterized by the use of traditional research designs and the perpetuation of conventional laboratory procedures that have enjoyed only limited success in the past. To achieve more useful results, a multi-disciplinary approach is necessary in order to better understand human operator information processing functions and to permit extrapolation of findings to advanced systems design and operation. Some academic research is less useful than it might be because investigators are not aware of the role that human operators will play in the management of future Air Force systems. Efforts to close this informational gap should be encouraged.

2. Second in priority is the problem of decision-making in the context of advanced military system operations. Some of the knowledge required in the area of decision-making is available in the research literature and from laboratory demonstrations. As was the case with regard to information processing, however, there is a need to integrate what is known and use it as a foundation for additional investigations.

There is also a requirement for research on methods for the transfer of research results into real world application. We need also to learn how to train people to make better judgements and how to use information to arrive at better decisions. We need to be able to reduce the time required to reach acceptable decisions, nor can we expect to reach 100 percent correctness in decision making or wait interminably for correct judgement. How does one telescope the decision process? How does one evaluate judgements while managing in the context of a unique series of contingent events? How can the decision maker use feedback most effectively to check and, if necessary, correct his actions? How does one provide information to a decision maker in a form and at a rate needed to optimize performance? Are there analogs to the flight director instrument display that would inform the decision maker of the consequences of pursuing the course he has selected? Are there ways to provide the decision maker with a review of alternative actions that might remind the operator of unrecognized opportunities? Traditional theories in the behavioral sciences are inadequate to resolve these problems. A new thrust, based on inter-disciplinary approaches, is indicated.

3. The third priority for behavior and social science research relates to the urgent need for better scenarios for the training of commanders. Dr. Cacioppo stated that the Soviet military establishment has developed excellent decision making simulation techniques and training methods. Their accomplishments deserve respect because of their diligent pursuit of relevant scientific and technical information in the Western world in addition to their energetic support of research and development in their own country. Dr. Cacioppo noted that Russian human factors literature seldom cites Western sources in their papers because their own research provides an adequate basis for their research; it is not because they are unaware of what is going on in science outside the USSR. They are capable of exploiting advanced theories to support improvements in their own systems applications.

Acknowledgement of the superiority of Russian methods for the training of commanders is based primarily on the assumption that their order-of-battle and engagement scenarios are more realistic. Their training includes such aspects of effectiveness as motivation and ideological commitment. The quality claimed for Soviet C-3 training programs may be judged from the fact that the Russians feel that they have no problem directly applying the results of their war games to actual warfare. It is doubtful that U.S. war games and scenarios have achieved the same level of veridicality.

At a conference sponsored jointly by CNR and AFOSR, Major General Jasper Welch, Deputy Chief of Staff for Studies and Analysis, indicated that he was distressed because different officers selected different alternatives when making decisions in tactical war games. (Thrall, 1976) He emphasized the importance of providing U.S. officers with an understanding of the military/political milieu in which they are operating as an essential input for effective decision making in operational situations.

U.S. war games are the product of efforts by operations analysts and military strategists. To develop better scenarios, there is a need for participation by social and behavioral scientists who have an interest in empirical studies directed toward the development and validation of realistic gaming scenarios and a knowledge of methods for their employment in training.

It is clear that the Russians appreciate the importance of including parameters drawn from social and political contexts in order to develop maximally useful simulations. It is postulated that U.S. war games ignore significant aspects of the environment in which military operations occur, or they resort to assumptions in place of scientifically derived judgements with respect to human characteristics and situational variables. A broad systems approach is required in designing and operationally testing effective programs for training Air Force leaders who will manage systems that may have catastrophic consequences if they fail to achieve their design objectives.

Our experience in designing war-game scenarios may be influenced by the human reluctance to "think the unthinkable," according to Herman Kahn; or it may be easier to rely on seemingly reasonable assumptions in regard to human motivations and human failures in man-machine systems than to admit ignorance and seek the information required to fill the gaps in current U.S. procedures and facilities.

There is impressive evidence that human factors parameters are critical in war-making and systems functioning. In South East Asia, the attitudes of the U.S. public and a small group of civilian "experts" dictated the way in which the war was waged. At a nuclear power plant at Three Mile Island, human error in an impending crisis magnified the hazard implicit in the situation and put an entire region in jeopardy. Failure to train operators of military systems properly could produce results similar to the debacle at the Pennsylvania nuclear facility and with far more serious consequences.

Experience in Korea and in Vietnam lends emphasis to the point that the members of the American military establishment, the politicians and the public have participated in the reification of folklore into accepted political dogma and military doctrine to a degree that limits the use of knowledge and sound judgement in undertaking, pursuing and concluding military actions. Failure to consider the roles and obligations of military leaders and failure to specify legitimate standards of social and public performance of military leaders increase the possibility of unacceptable improvisations with resultant unpredictable consequences by U.S. military leaders in critical circumstances. It is the writer's opinion that Soviet leadership is more realistic and more militarily professional in its outlook. Further, its viewpoint is shared by many top civilian leaders who have significant experience in the military establishment. There is a more homogeneous basis for the derivation of military doctrine and national goals than is the case in the United States. But the difficulty in understanding the problems of national security strategies is not a valid reason for ignoring the subject. The United States is fairly effective in developing and producing military equipment, and the selection and training of operators has been satisfactory. However, we have been reluctant to consider the values and standards of performance that should characterize the behavior of military personnel. Dr. Samuel Stauffer of Harvard, at the conclusion of World War II, studied the role conflict that characterizes the careers of Air Force officers. They are faced with maintaining a record of achievement while doing what is required to survive and advance in their career, even when there may be a conflict between systems demands and personal integrity.

There are many other areas where our system of management places leaders in a difficult position. The multiple inspections and performance evaluations that characterize some combat organizations are justified by the serious implication of failure to perform at required levels of efficiency. However, there is no way, at present, to relate the utility of effectiveness reports to other Air Force objectives, such as the retention of superior personnel or the longer-range dimensions of operational effectiveness.

Failure to include human factors considerations in management as a suitable field for research is mirrored by the neglect of these same elements in the development of scenarios for war games. We feel strongly that factors such as motivation, vigilance, and human performance in general represent significant influences on unit readiness and performance potential in war-gaming situations and the gaps that exist in current knowledge of these matters is of such a magnitude as to make the utility of U.S. gaming exercises questionable, or at least candidates for significant improvement.

Another aspect of U.S. war games that could be improved relates to the probable behavior of our Russian adversaries. Air Force and DOD programs should incorporate accurate information with regard to Soviet military strategy and tactics. U.S. war-gaming protocols focus on the functions of the U.S. leader (airman) managing a single offensive weapon system in an offensive encounter while neglecting, or minimally representing, information about the battle environment in which he is operating.

The Air Force has sponsored translations and publication of authoritative Soviet documents that provide a rich source of information in regard to the principles that influence Russian military planning and operations. Information in these documents is important for studies of the training and indoctrination of Soviet military operators and managers. The Soviet Military Thought Series provides an invaluable introduction to the study of Russian concepts for the conduct of warfare and could provide data for the improvement of U.S. war gaming scenarios. While the Soviet translations provide the fundamental principles that guide Russian leaders, they also represent sources for the study of policy formation, improvisation, and deviation from structured patterns of operational strategies that may characterize the way in which Russian leaders conduct offensive and defensive operations.

As far back as 1963, the development of better U.S. war gaming programs through the support of social science research was recommended by Vincent McRae, Special Assistant in the Executive Office of the President, and an early contributor to war gaming programs in the Department of Defense (Pool, 1963). During the seventeen years subsequent to this report, no research of any consequence has been devoted to improving the realism of war-gaming exercises through the expedience of interdisciplinary research in relevant behavioral and social sciences. There is an urgent requirement for research on improvement of the quality of training of U.S. military leaders through the development of more realistic scenarios for the conduct of war games. Immediate implementation of research programs in this area of the national security disciplines is essential to restore competitive balance between U.S. leadership training programs and those of a Russian military establishment in programs that more nearly approximate reality by the inclusion of a significantly broader range of variables that could influence the outcome of military engagements.

4. Fourth and final area for expanded research in the behavioral and social sciences involves the conduct of investigations that are aimed at the improvement of productivity of military personnel and employees of military organizations. The requirement for enhanced performance relates to both people and organizations, and to both operational and support activities. Combat units spend their time in training, readiness, and stand-by postures that are indistinguishable from each other, but that are vastly different with respect to peace time versus war time conditions. Logistic and support organizations perform functions that are distinct in pace and content from peace to war time operations. Combat organizations produce a higher utility when they are not engaged in operations than when committed to combat.

Despite the higher utility of non-engaged military units, however, there are compelling reasons to explore ways to exploit military capabilities during peace time to satisfy objectives in the area of public security, while at the same time enhancing individual and unit effectiveness so as to compensate for past and anticipated reductions in force, mandated to control escalating personnel costs. There is a need to improve personnel effectiveness through the reduction of time spent in off-the-job training and orientation to military life because these reduce the time spent on actual job accomplishment. Reductions in training time result in an increased return on the investment in human capital resources needed to manage and operate the Air Force.

A number of research approaches should be pursued to enhance the return on investment in people. One approach would involve econometric modeling procedures to develop personnel planning protocols that would determine the costs and benefits associated with different recruitment and manning strategies. Recruitment of new personnel, retention and retraining of personnel after conclusion of a period of service, and transfer of previously trained personnel to secondary career fields are alternative modes of providing skilled personnel to operate the Air Force. Each has different costs and advantages as a means for continuous maintenance of a combat-ready force, even with an austere budget. Econometric research is needed to provide methods for costing various trade-offs that are possible between capital intensive and labor intensive methods for the accomplishment of important military functions. Studies are required that will identify the costs and advantages of different recruitment and retention policies that are recommended for DOD and Congressional consideration.

Behavioral scientists such as Herzberg, Blake and Mouton, Argyris, Likert, and others have conducted studies in industrial and military settings that have established the effects of motivators and demotivators in influencing the productivity of workers and supervisors. Pritchard performed studies at Scott Air Force Base that compared the effectiveness of extrinsic motivators and intrinsic motivators in achieving changes in the quality of airman performance. (Extrinsic motivators are tangible rewards such as pay increases, reduced time on the job, fringe benefits, etc. that result in a higher economic cost to the Air Force.) (Intrinsic motivators are intangible rewards for high performance that relate to satisfaction achieved through pride in individual or team effort.) Pritchard's work suggests that intrinsic motivators that are almost free of cost to the Air Force may be more effective than extrinsic motivators. The latter types of reward appear to focus attention on the reward structure and may actually reduce awareness that high performance is its own reward, especially if recognized and encouraged by management.

Still another approach that deserves exploration by the Air Force is the role of leadership on the part of commissioned and non-commissioned officers in achieving unit effectiveness. Earlier Air Force studies were directed toward the improvement of leadership effectiveness and the development of models that would be useful in career guidance and in the evaluation of personnel policies that may be dictated by cost considerations. However, there are no significant research programs that focus on the problems that cause a considerable number of people with high career motivation to resign from the Air Force because of what are perceived as the unacceptable personal costs of continuing in the service as compared to the perceived benefits of a civilian career.

Critical incident techniques have proved their value in the past as a method for assessing the significant components in a social situation. These methods merit further application as an approach to military management and leadership training.

The requirement to integrate people from different social, ethnic, economic and educational backgrounds into an efficient military organization poses a difficult task. The people who are now entering military service are different from those who now occupy highly responsible positions. Investigations that would reveal the values and interests of American youth are essential in order to develop a military structure that would, insofar as possible, accommodate the youth without sacrificing effectiveness. Manley and McNichols, of the faculty of the Air Force Institute of Technology, Department of Management Sciences, conducted studies during the mid-seventies which indicated that one-third of the Air Force enlisted ranks were favorably disposed to the idea of unionization as a means of improving the quality of life for military people. This finding is compatible with the conclusion of Moskos and others to the effect that military personnel in the United States are adopting an attitude that their service to Air Force is an occupation and not a military profession. This trend is highly significant in the creation of an effective military service. The development of a better understanding of the attitudes of youth and military service personnel is a prerequisite to the support of studies to enhance the effectiveness of military people.

Many Air Force organizations have initiated or are considering team development programs to improve the socialization of employees into a more effective mission-oriented functional group. Efforts of this type exploit what has been learned through past studies and experimental interventions in industrial, educational and military organizations. At this stage it is necessary to replenish the reservoir of knowledge with regard to organizational effectiveness so that future attempts to enhance the quality of individual and group performance may be carried out with greater assurance that the efforts will achieve desired objectives without producing unfavorable side effects. The ultimate goal of organizational development research should be the explication of management techniques that will accomplish through normal channels the team building functions now achieved by the intervention of OD consultants and others. The continuing need for organizational development training will provide an incentive to develop means for the improvement of the quality of life in the working environment of Air Force employees and will contribute to the reduction of management-worker conflicts that lower morale and impair effectiveness.

While outside consultation is an appropriate instrumentality for initiating programs of organizational development in Air Force units, it should be recognized that the Air Force has available several groups of behavioral science specialists who are competent to conduct research or to advise in regard to such requirements. The School of Management at AFIT, the Department of Behavioral Science and Leadership at the Air Force Academy, and the Department of Professional Development at the Air War College regularly employ faculty members who have professional competence in organizational development and leadership training.

Management information systems are used extensively in the United States. Almost anything that can be counted is deemed worthy of counting. Items such as cost per employee of office space, number of contracts or grants that have passed their reporting date without the receipt of the required report, etc., are examples of the kinds of information that, because of the lack of better measures and because of their ready availability, managers use in evaluating effectiveness. Calculations of the average grade level of different organizations, or the ratio of super-grades to employees supervised, are other items employed to estimate the efficiency of subordinate units. No one is happy with these measures of unknown validity, but they are used because of the lack of more meaningful information. Further, they do serve to goad subordinate commanders into additional efforts to conform to Command norms or to make special justification for the uniqueness of their units.

It is suggested that Air Force managers would perform better if they had better MIS systems. Better MIS systems will not become available without research dedicated to the proposition that there are kinds of communication, both upward and downward, that will contribute to the effectiveness of management. How to derive the content of an MIS system that contributes maximally to organizational effectiveness is an important research question.

In the current era of personnel shortages facing the three services, it is important to develop methods for the improved utilization of people. While the Air Force has not experienced the same degree of difficulty in recruiting at the enlisted level as the other services, it is already suffering serious personnel shortages in such professional fields as medicine, science, and engineering, where understaffing is approaching the fifty percent level. It is essential that the Air Force create a reputation as a good place to seek employment. Investigations focused on the career expectations of individuals qualified for Air Force service must be conducted if there is not to be a dangerous shortage of officers during the next twenty-five years.

To summarize the preceeding section:

We have identified four areas of research that, in the opinion of the Chief Scientist of the Foreign Technology Division (AFSC), have potential for productive human factors research. Past failures to address these multidisciplinary problems constitute a deficiency in the Air Force research program as it prepares to meet its responsibilities for the year 2000, and further, jeopardize efforts to maintain a credible defense against the Soviet potential for hostile actions. These then, in order of importance, are the research areas:

1. Research on human information processing capabilities.

2. Research on decision-making and enhancement of the human decision-making process through training.

3. Research on the improvement of training for combat commanders through the development of more realistic simulation programs that incorporate more representative scenarios of future military engagements.

4. Research on the enhancement of individual and organizational productivity through investigations on the fundamental motivational elements of individuals and groups.

A FOOTNOTE ON BEHAVIORAL SCIENCE RESEARCH IN THE SOVIET UNION

Research scientists in the Soviet Union are moving forward in most fields of the behavioral sciences that relate to military applications. They are making progress in the areas that relate to the performance of troops and commanders in anticipated types of military engagements. Only two fields can be identified where Soviet science or technology lags in research related to human performance. The first relates to investigations of learning where the inheritance from Pavlov's study of conditioned reflex not only continues to blight efforts directed at the derivation and experimental validation of different theories of learning, but also hampers the application of knowledge available from foreign sources. Availability of state-of-the-art computers is the other area in which there is a deficiency that has implications for the behavioral and social sciences. Soviet scientists are expending resources and effort to acquire necessary computer capability, but current differences between Russian and Western accomplishments is measurable in years.

Having acknowledged these deficiencies it is important to emphasize the quality and breadth of the Soviet scientific enterprise as it relates to the development and operation of modern weapons systems. They are particularly concerned about human factors in the development of their systems. They are able to build weapons that are cost-effective and well-suited to the intended missions. Their weapons technology is eminently practical. Mission requirements are realistically defined and desired performance characteristics are accurately specified. Soviet armaments are neither over-designed nor over-engineered. Their weapons include the components and subsystems necessary to assure total system integrity and operational effectiveness. Systems are functional; nothing superfluous is added. To Western eyes Russian weapons technology may appear to be relatively unsophisticated, but for the Soviets the payoff is that their weapons can do the jobs for which they were designed.

Science in the Soviet Union is well advanced in the fields of mathematical modeling, cybernetics, engineering psychology, operations research, decision-making, perception, neuropsychology, and human factors in equipment design and training. We have accounted for the lag that characterizes learning theory and computer sciences developments. Western scientists would profit from efforts to become more familiar with available Russian scientific literature in their areas of specialization.

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