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PSYCHOLOGY IN THE WORLD EMERGENCY

PSYCHOLOGY IN THE WORLD EMERGENCY

JOHN C. FLANAGAN

IN introducing this series of papers, *Psychology in the World Emergency,* it seems appropriate to review the history of psychology in relation to military and political affairs. Early in the century psychology and psychological procedures won a place for themselves in our schools, in our mental institutions, and in our courts, but it was not until World War I that psychology was given any place in governmental affairs. The personnel system developed under the guidance of the Committee on Classification of Personnel in the Army provided a remarkable demonstration of the value of job analyses, classification tests, trade tests, and qualification cards in improving the effectiveness of the use of men in the services in times of emergency.

During World War I research was also initiated on problems of selecting personnel for aviation. Although a number of interesting tests were developed during this period, none of them was adequately evaluated.

In spite of these very significant demonstrations of the importance of tests and systematic personnel procedures in the effective use of men in the services, the tweny-year period following World War I saw very little use of such procedures in the Armed Services. At the outbreak of war in Europe in 1939 the War Department had no trained personnel technicians and no functioning system of personnel procedures for selection and classification. The Navy had been using for

several years a battery of six tests given to recruits arriving at Naval Training Stations, but had no adequate program for follow-up and use of the test results which were recorded on the men's service records. As part of the preparation for the national emergency, psychologists were called in to many military and other government agencies to assist with the development of personnel procedures. During World War II about 1250 psychologists were employed full time in psychological work related to the national emergency.

The first World War had clearly demonstrated the importance of a general classification test of the type of Army Alpha. It had also shown the desirability of job analyses and trade tests. The psychologists' work in World War II will be remembered for the precise follow-up studies validating tests on large numbers of individuals using complex batteries of tests and for the extension of psychologists' work into many other areas such as the measurement of attitudes and adjustments, research on training problems, and research in the field of clinical psychology. Because the war lasted longer and demobilization was not so rapid, the studies done by psychologists in World War II were much more extensive and have been more fully reported than for World War I. Another important difference is that after this War each of the services retained a substantial number of psychologists to continue the research and development work begun during the War.

CURRENT STATUS OF THE FIELD OF PSYCHOLOGY

Another consideration which seems important to review at this time is the current status of psychology

as a field of knowledge. The systematic study of psychology is not much more than 75 years old. Because of the great complexity of the problems in psychology as compared with physics and chemistry and the difficulty of isolating single variables in order to do controlled experiments the accuracy of prediction in the field of psychology is ordinarily much less than in the physical sciences. Therefore to an even greater extent than in the physical sciences the primary function of psychologists in the services is in connection with research and development work. As the research psychologists prepare a larger and larger number of procedures and principles the need for trained personnel to apply these in the practical day-to-day functioning of the government organizations will increase.

One of the important problems of any organization is selection of its personnel. Probably this field is the one in which psychology has made the greatest advances. We can predict with considerable accuracy the chances for success which a man will have in certain specific jobs. However, this precision refers only to a small number of jobs at the present time. There is not sufficient data available for an understanding of just what psychological functions are involved in a particular job component which is observed to be important in a specific job. Tests are available for only a small number of the many job components required for many of the new highly skilled and complex jobs. More control and flexibility is needed in our selection procedures so that as jobs change it will not be necessary to duplicate the entire validation program before making changes in the selection procedures. As a more

adequate understanding of job components becomes available it will be possible to make large improvements in job design. Job design is important because if a job is set up so as to include only one or two important aptitudes or skills the number of persons having especially high ability in this area will be much greater than if it demands a large number of skills.

Closely related to the problems of selection are the problems of training. In this field the most important problems are those of what shall be taught and how it shall be taught. Although considerable progress has been made by psychologists in both of these areas the present techniques can be considered at best as first approximations. More research on the identification of difficulties and the problems of learning is necessary if our training programs are to be truly efficient.

Psychologists have developed greatly improved techniques for measuring proficiency in important job skills. They have made considerable progress on the problems in evaluating on-the-job performance. However, much research on basic problems in these areas still needs to be done.

One of the fields in which the most rapid advances have been made in the past ten years is that of human engineering. The expensive, costly equipment now being designed is to a much greater extent than formerly being systematically related to the capacities and limitations of the men who are going to operate it. Of course, the work done in the past ten years has not provided more than a general framework for the use of engineers and designers. Many fundamental problems still remain to be solved.

Similarly, in the field of internal and external stresses physiological and clinical psychologists have developed much information which is useful and effective. However, no one would claim that more than a promising beginning has been made on the problems of the effects of fatigue, drugs, unusual conditions, and emotional stress in impairing the performance of individuals on specific jobs.

Possibly the field with the greatest promise for future applications is that of social psychology. In military and political affairs there are few problems so important as the selecting and training of leaders, the organizing and managing of teams, the predicting and influencing of attitudes, and the group processes of weighing and evaluating evidence to decide on a course of action. The past twenty years have seen much progress on these problems. However, they appear to be somewhat more difficult and complex than those in some of the other fields of psychology. If man is to achieve the degree of control in human relations which is rapidly being gained over the environmental forces of nature the psychologists must increase our knowledge in this area by a great deal.

CURRENT PSYCHOLOGICAL PROGRAMS SPONSORED BY THE DEPARTMENT OF DEFENSE

Although the recent expansion of the work in the psychological units in the Department of Defense has resulted in a number of changes and therefore data rapidly become out of date on the organizational structure and functions of the various units, it seems worth while to outline briefly the scope and nature of current

psychological organizations in the Department of Defense.

The units concerned with psychological research in

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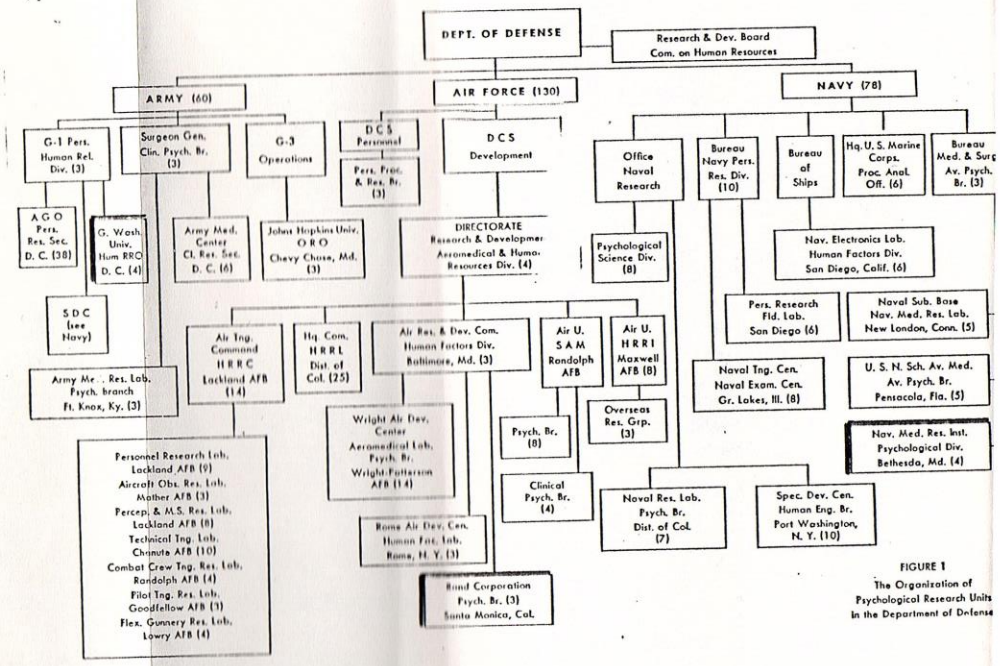

FIGURE 1
The Organization of Psychological Research Units in the Department of Defense

psychological organizations in the Department of Defense.

The units concerned with psychological research in the Department of Defense are shown in Figure 1. In addition to the names and general organizational relationships of these units the number in parenthesis indicates the psychologists listed in the 1951 Yearbook of the American Psychological Association as assigned to these units who are associates or fellows of the American Psychological Association. There are undoubtedly more people doing psychological research work at the professional level in these units than are shown in the attached chart. It also certainly does not reflect changes made in the past six months. As a rough indication of the relative sizes of various units, the chart can probably be accepted as being fairly trustworthy as of the summer of 1951. Units are shown for which there were at least three psychologists listed in the 1951 directory.

It is clear from this chart that the Air Force has the largest number of in-service personnel working on psychological research, the Navy has the second largest number, and the Army has the smallest. In the Air Force all of the psychologists shown on the chart except for the few in the Personnel Procedures and Research Branch in Personnel in the Air Force Headquarters operate as a part of one centrally integrated program. In both the Army and the Navy the various units tend to operate more independently of each other. One possible exception to this is that in the Navy the Office of Naval Research, which carries on almost all of its activities through contract research with universities and

other research laboratories, does all of the contracting for Navy research requirements. In both the Army and the Air Force the various units do their own arranging for contracts, except that in the Air Force the Headquarters of the Air Force delegates all of the research negotiations to field units. The Army has integrated and greatly expanded its in-service program in recent months. The papers which follow mine will give you some idea of the research programs carried on by a number of these groups.

CURRENT NEEDS FOR PSYCHOLOGICAL RESEARCH

Perhaps as a setting for the papers which follow, it might be well to examine briefly some of the principal problems confronting the military services which could be contributed to by psychological research. Probably one of the most important problems facing the services is the selection of men for top level military management positions. Other requirements in selection or aptitude research include selection procedures for both maintenance personnel and operators of new types of electronic and mechanical equipment.

Closely related to selection problems are those of training. There are many indications that training can be more efficient. This is important at the present time because if three months' training time can be saved each year for 1,000,000 men, the effective manpower pool for operating purposes will be increased by 250,000 man years. The other important aspect of training is insuring that the trained personnel will meet high standards of proficiency. Another need is becoming more urgent as a result of the complexity

and expense of some of the new equipment for which personnel are trained. This is the requirement for evaluation and efficient procedures for using the new complex training devices which are being developed to provide training for these new types of equipment.

It is immediately apparent that if adequate evaluation is to be made of the new training methods and new selection procedures, valid and precise measures of proficiency must be available. Training cannot consist primarily of a time serving process, and the opinions and impressions of the training instructors cannot be relied on to provide valid, reliable, and objective measures of the levels of proficiency that personnel have attained. Objective, reliable, and valid proficiency measures, therefore, become the cornerstone of effective research on selection and training problems.

It is impossible to prepare valid proficiency measures without a detailed definition of the job which specifies standards in objective terms and indicates the relative importance of the various job components. For this reason much time has been spent recently on job-analysis techniques and criteria of effective performance. Probably the most difficult task still confronting psychologists is to develop and install satisfactory procedures for getting valid measures of the on-the-job performance.

The proficiency measure is designed to answer the question, "*Can* this individual perform this task?" but the real criterion of effective job performance is, "*Does* this individual perform this task?"

This brings us to the last important problem area, the question of attitudes, morale, leadership, and man-

agement. Morale, of course, is the tendency for the individual to do the things which he can do. The problems of leadership and management are to motivate the individual to do those things which he can do. Another important related problem is to design the jobs so that they can be efficiently handled.

Along with right attitudes for our own personnel is the problem of changing the attitudes and ideas of the enemy's personnel. This is important at all levels from that of the foot soldier to the top diplomats.

PROBLEMS IN ORGANIZATION AND POLICY

A fundamental decision which in-service psychologists have had to make is what proportion of their efforts should go to research as compared with service. The psychologists in the various services have made different decisions on this problem, but in the main they have chosen to lay much the larger amount of emphasis on research. The principal exception to this is the clinical psychology program in the Army and even in this program there is substantial emphasis on research. A closely related problem is what share of the efforts will go to the problems of implementation of research findings and who shall be responsible for this implementation. I am sure that a number of the papers which follow will touch on these important points.

Someone must decide what research shall be done. Since World War II the Research and Development Board in the Office of the Secretary of Defense has taken on the functions of budget review and program guidance for all of the services. The Committee on Human Resources of this Board therefore exercises a

very important review and co-ordinating function. The present chairman of the Committee on Human Resources is Dr. Frank A. Geldard, chairman of the department of psychology at the University of Virginia. Dr. Geldard and the members of the panels established by this Committee are psychologists with extensive experience in psychological research for the military services. These panels hold relatively frequent meetings throughout the year to assist the Department of Defense in planning and co-ordinating the research program in the Human Resources area. Assisting these panels is the full-time executive director for the committee, Dr. Aaron B. Nadel.

Although the panels of the Committee on Human Resources do take the responsibility for channeling research and development funds into broad areas where their surveys suggest there is a special need for research, the in-service psychologists still have a large responsibility with respect to the specific problems which are to be attacked. There are three fundamental policies which may be followed in selecting research problems:

1. The research psychologist can request the operating military personnel to specify research requirements. At worst such a policy might lead to a series of specific research problems aimed at limited local decisions regarding problems of procedures. At best it provides real practical problems which confront the Military and for which they need answers which will aid them in formulating policy and procedures.

2. The second method consists of asking the research psychologist to specify the area in which he feels he can make

the greatest contribution at the present time, because of his training, experience, and special competence. At worst such a policy might lead to undertaking a pure research problem which would never have military application. At best it could be a source of major advances in theory and understanding with very important long-range significance for the military services.

3. The third policy on the selection of problems consists of programmed research. This consists of a systematically planned research program aimed at those problems most needing solution and for which research techniques either are available or appear practicable of development. At worst such a program might become an end in itself having no effect on military operations or policies ignoring both the present needs of the Military and the interests and competencies of the research psychologists. At best it would provide a balanced program of research on immediate and long-range problems with the research so designed as to contribute in a maximum way to the long range objectives of the research program. Although the research would be oriented toward important military problems it would be planned in such a way that the results could be generalized to similar problems in other fields and also this planning would include the testing of broad hypotheses and theories with important implications for understanding a wide range of problems in this area.

In the papers that follow you may find it interesting to observe the extent to which each of these three policies for developing a research service has been influential in the work of the in-service psychological groups represented here.

A major problem in organization and policy is where and by whom this research and development work is

to be done. Should it be done in the services by military and civil service personnel or out of the services by universities and research organizations? All of the services have adopted a compromise position on this issue, but it will be noted that the amount of emphasis on in-service as compared with contract research varies from group to group. Although all of the speakers at this Conference are civilians and the majority of the psychologists in the services are also civilians, there is a substantial number of psychologists in uniform in each of the three services. One of the problems still being worked on by the services is the most effective organization of research units containing both civil service and military personnel. A related problem is concerned with the auspices under which the research psychologist shall work. The practice in the services at the present time is quite varied. Some research psychological organizations are set up to report directly to the commanding general of the command; others work under the deputy for personnel, many are associated with medical groups, and some are under the sponsorship of the more general research and development programs of the services. Although there certainly is no unanimity on the point, it appears that most psychologists feel their research programs would be placed most appropriately in the general research and development program of the specific service.

THE RESPONSIBILITIES OF RESEARCH PSYCHOLOGISTS IN THE WORLD EMERGENCY

Considering both the in-service and contract research programs of the Department of Defense it is

clear that the effort of research psychologists on problems related to the world emergency constitute a substantial fraction of all research psychology in this country at the present time. Not only are the military services taking the lead on problems of personnel selection and classification but they are also sponsoring the major studies in the fields of learning, training devices, proficiency measurement, attitudes, leadership, management problems, operating procedures, systems analysis, and job design.

The work being done in connection with the defense program on these problems is not applicable only to these defense problems; it has broad implications for the development of nearly all phases of psychology as a science.

The opportunity given psychologists by these government-sponsored programs is unparalleled. Along with this opportunity goes a responsibility both to the profession and to the country to see that the advances in psychology are commensurate with the level of effort being made. Some of the country's best research psychologists are participating in this effort. The papers that follow should provide an opportunity not only to learn what has been accomplished in these programs but also how this research is related to the more general problems of the profession.

Finally, from these papers you may be able to discover how you as an individual can co-ordinate your own research efforts with those in the services so as to make maximum progress toward the common goal of a more adequate understanding of the fundamental problems of human behavior.

RESEARCH ON MILITARY LEADERSHIP

FILLMORE H. SANFORD

I. Introduction

THIS paper is an adaptation of one originally prepared for the Working Group on Human Behavior Under Conditions of Military Service, an organization of the Research and Development Board of the Department of Defense. Parts of the paper were later presented at a research conference on October 4, 1951, sponsored by the Panel on Human Relations and Morale of the Committee on Human Resources of the Research and Development Board.

I want to introduce my paper by making some general statements about the importance of studying leadership. This introduction, which like many such may be more accurately described as a "misplaced interruption," is occasioned by the belief that such general statements, though characterized by both looseness of form and piety of air, can help locate the specific topic in a broad context.

I think a case can be made that a people's orientation to leadership and authority is a cardinal factor in determining the form and flavor of the social institutions evolved to serve that people. Our own democratic institutions are reflections, in large measure, of the basic American attitudes toward authority and of related American readinesses to respond to certain sorts of leadership. The future of our institutions will depend in significant degree upon the ways our attitudes to-

ward authority develop—or regress—and upon the sort of social mechanisms we invent to implement these attitudes. If we wish to preserve and advance what we know as democracy—or if we merely wish to understand democratic society—we need to understand leadership phenomena. Further, we will need to establish some sort of intelligent control over this very crucial social process if social science is to contribute to the advancement of human and humanistic values.

At a less high-flown level we can make an additional case that the study of leadership has significant consequences for the general effectiveness of a society in advancing any of its goals. A vast proportion of human effort is effort expended in group settings. The success of these efforts depends on such things as effective division of labor, effective organization, effective communication, effective group structure. The activities of the leader, whether appointed or chosen, whether formal or informal, bear directly on each of these aspects of group functioning and hence on the over-all effectiveness of the group.

A society as well as each of its component organizations constantly faces the need to use human effort effectively. In times of national crisis this need has more apparent urgency and the problems of selecting and training leaders become more acute. At other times, when the society is less frantically interested in tangible productivity, there still remains the problem of organizing group action in such a way that human desires can be advanced. In our society certain sorts of leaders are effective and others ineffective in advancing group goals—whether these goals are material pro-

ductivity or softer purposes involving the advancement of the individual's security, maturity, and integrity. If we find out enough about leaders and leadership we can eventually insure that groups are better at achieving whatever it is they are constituted to achieve—whether greater production of guns for defense or greater production of leisure for living.

A third point worth mentioning is that the study of leadership has a significant potentiality for contributing to our general understanding of many events the social scientist concerns himself with. Anyone who has an interest in leadership phenomena, and who has suffered the inevitable confusion such an interest brings in its wake, is faced with the seemingly necessary conclusion that leadership events are not separable, except by the veriest of fiat, from the more general and inclusive phenomena of group functioning. There seems to be no reason, in the nature of things, why we cannot create a sound science of groups. There seems, in the nature of things, good reason for believing that such a science, when we make it, will give man unprecedented control over his social environment. And few will doubt that leadership phenomena are crucial phenomena for such a science and that their understanding will contribute mightily to the advancement of this science.

Q.E.D. Leadership is important. It is important for any social organization, military or otherwise. And both in the context of the cosmic things dealt with above and in the context of the present writer-reader situation it is time we got down to brass tacks in wrestling with it.

This paper focuses on military leadership. The form of the paper, however, is dictated by the conviction that military leadership is not different, except in relatively phenotypical ways, from any other sort of leadership, and by the belief that meaningful statements about leadership, when and if they are made, will contribute significantly to the effectiveness of military and all other groups in our society. The paper first talks about historical procedures for selecting, appraising, and training leaders. It then deals with some of the central and as yet unsolved problems confronted by the psychologist who wishes to make good and useful declarative sentences about military leadership. The paper then moves to a brief summary of research on the general psychology of leadership and goes on to deal with the rudiments of a conceptual scheme suggested by specific research findings, and potentially inclusive of them.

II. Applied Research on Military Leadership

In treating past research on military leadership the present paper will divide this general subject into three parts: (a) Selection of Military Leaders (b) Appraisal of Military Officers, and (c) The Training of Military Leaders.

The problem of a criterion is still the paramount problem in research on military or any other sort of leadership.

D. The Training of Leaders

The training of military leaders, like the training of doctors or lawyers or philosophers or linguists, has proceeded without any really scientific evidence regarding its effectiveness. Such evidence, of course, is very difficult to uncover. Particularly is it difficult when we are in possession of no satisfactory criteria. But with enough intelligence, enough time, and enough money there is no inexorable reason why we cannot discover the most effective sorts of training for the production of the most effective sorts of leaders.

Research approaches to the effects of leadership training are rare. The Human Resources Research

Center of the Air Force initiated a "practical problems course" in Officer Candidate School designed to go beyond the usual book-learning methods, but there is as yet no evidence bearing on the effectiveness of the course in producing improved leadership performance. The course has been installed as part of the OCS curriculum on the basis of its face validity, and grades in the course help determine the acceptance or rejection of an officer candidate. The Infantry School at Fort Benning has also recently installed a leadership training course.

Both at the U. S. Naval Academy and at the U. S. Military Academy since World War II there have been curricular experiments with various sorts of training in leadership. At Annapolis a course in Naval Leadership is a regular part of the curriculum. One section of this course is devoted to a study of psychology and is based on a specially prepared text called "Psychology for Naval Leaders." This section of the course has been subjected to a research examination designed to find out what changes, if any, were produced in the midshipmen who were exposed to it.[25] A sample of 100 midshipmen who spent eight classroom hours and an unknown number of "homework" hours on the psychology section of the course were given a variety of psychological tests before and after this brief exposure to psychology and their scores compared with those of a control group of 100 midshipmen who were not exposed to the course. The results show statistically significant changes on the part of the midshipmen who took the course. The study ended with the following general conclusions:

1. After taking the course midshipmen held ideas and opinions about human behavior which correspond more closely with those recognized as scientifically correct.

2. After taking the course midshipmen showed a greater tendency when confronted with written leadership problems to approve solutions which involved positive action based on consideration of human variables. They tended more often to reject solutions which were Ego-defensive, dictatorial, inconsiderate, or indefinite.

3. After taking the course midshipmen were able to consider more critically the evidence necessary to reach conclusions from given information. Their scores on a Test of Logical Reasoning improved.

4. After taking the course students tended to express less reactionary attitudes toward social problems.

There is no solid evidence that the course makes midshipmen better leaders. There is evidence, however, that the course does produce changes which many people would regard desirable. Though the Annapolis research runs directly into the problem of a criterion it does suggest that it is possible to train people in the solution of the human problems the military officer encounters every day. We may not be able soon to demonstrate that any given course of training produces better or worse leaders, but we can experimentally examine courses and experiences for their effect on specific behaviors which are regarded, on the face of things, as generally desirable. And there probably is much useful progress to be made in arranging that leaders and leaders-to-be have supervised direct experi-

ence with the problems and situations a military leader encounters.

The potential fruitfulness of such procedures as psychodrama, for example, has not been thoroughly explored in the light of military significance.

E. Research on Noncommissioned Leadership

The plethora of problems in the area of noncommissioned leadership have been relatively untouched by research efforts. We have tended to proceed on the assumption that military leadership inheres only in military officers. This is a poor assumption if we are interested in the effectiveness of the whole military organization, for many of the leadership functions in any military establishment are either formally or informally fulfilled by noncommissioned men. If one looks about a bit in the military it is easy to get the impression that both the selection and training of noncommissioned leaders represent problems into which research exploration can yield valuable results. Often it seems to be that promotion to positions of responsibility below the commissioned level is based almost entirely on a man's competence in the performance of a technical job. In the selection of any leader for any sort of situation it is very illogical to infer from technical proficiency to proficiency in assuming responsibility and in supervising the activities of other human beings.

There are currently two research projects in progress that may give us ideas for improving noncommissioned leadership. One of these, sponsored by HRRC of the Air Force and being conducted by the

Institute for Social Research, is exploring systematically into the behavior and functions of noncom leaders in the Air Force and is searching for personality variables which bear on proficiency of functioning. The other project, sponsored by the Army's Adjutant General's Office and carried out by the Institute for Research in Human Relations, is devoted to the study of small groups (squads in reconnaisance platoons) and aims to test certain hypotheses about leader-follower relations as they bear on the measured proficiency of squads. There are going projects also, both in the Strategic Air Command and the Air Training Command of the Air Force, devoted to the intimate study of small groups. These projects can be expected to reveal a good deal about the role of the noncom leader in small military groups and about the sort of person who can play that role.

F. Research on Administrative Function

Under the direction of Carroll Shartle[26] the Personnel Research Board at Ohio State University has been conducting extensive studies in the leadership area with a focus on what may be described as the problem of administrative or executive function. The procedure has been to investigate the actual behavior of executives in a variety of organizations, including military organizations, and to classify these behaviors under general functional headings. By using such a classification, it is possible to describe with considerable accuracy the pattern of activity characterizing any single executive or group of executives.

By this sort of approach it is possible to find the work

pattern that characterizes the individual executive. Then if we know the executive pattern demanded by a given organization, we can fit the individual to the demands of a job. If a given executive seems naturally to gravitate to public relations activity, we probably do not want to place him in an executive position demanding detailed technical supervision—or *vice versa*. A further utility in this approach is the possible selection of an administrative team for an organization. If the chief executive has one pattern of activity, it may be wise to select his subordinates so that they can supplement rather than compete with him. An organization that has been going successfully under an executive with one pattern of activity may deserve a subsequent executive with essentially the same pattern.

A further facet of the Ohio State studies is the analysis of organizational structure. Through an adaptation of the sociometric technique it is possible to determine for any organization *who actually spends how much time with whom for the purpose of getting work done*. A pattern of informal or actual working structure, developed in this way, can then be compared with the official organizational chart. Often, very great discrepancies occur. The charts of informal working relationships can be useful to the staff in understanding itself. And if the informal pattern of relationships deviates too far from the formal pattern, efficiency will probably suffer, for functions and titles are out of tune with one another.

Such analyses could be of considerable utility to military organizations. They could yield clearer pictures of what behavior characterizes military executive

and of what behaviors the various types of military organizations demand. A knowledge of the organization plus a knowledge of work patterns of individual officers could be of great value in guiding the placement of top-level officers.

G. *Research in Conference Leadership*

An ONR research project directed by Harold Guetzkow at the University of Michigan[13] has studied intensively the various psychological factors involved in productive conferences. Since such a large proportion of the time of higher ranking military officers is spent in conferences, it is clear that any increase in the speed and effectiveness of conferences will be of tremendous value. One factor in the success of any conference is the leadership skill of the leader. It is very reasonable to expect that on the basis of this sort of research we will be able both to select and train conference leaders.

SUMMARY STATEMENT ON APPLIED RESEARCH IN LEADERSHIP

There does not seem to be any reason inherent in the nature of things why scientific psychological and personnel methods cannot eventually produce highly successful procedures for selecting and training military leaders—procedures as successful and as *demonstrably* successful as current procedures for selecting aviators or machinists. At the moment, however, our knowledge of leadership is simply not sufficiently complete to enable us to put into operation tomorrow many demonstrably sound procedures for either the selection, training, evaluation, or assignment of military officers.

Research efforts have taught us a great deal that is of practical value. We know how to procure reliable ratings, for example. Much progress can be made immediately by installing reliable rating procedures in dozens of places in the military where ratings now are little better than feminine intuition and are subject to all the ills (as well as to the brilliant insights) that characterize intuition. Our scientific understanding of leadership increases every year. And this understanding is being communicated to military people who profitably use it in making decisions about leaders. But much of our scientific knowledge is essentially negative. We know that many common-sense statements about leadership are either plainly untrue or considerably distorted. Such negative knowledge is very important—even essential—in the history of a research problem. The really positive knowledge, which cannot often be created until after we accumulate negative knowledge, has just begun to come in. It may be five years or twenty before we are really able to make precise and maximally practical statements about military leadership. But that such statements will eventually be made is beyond doubt. And when they are made they will be worth—both to our society and our military—whatever time, money, and effort they entail.

III. The Basic Psychology of Leadership

We have dealt so far with the attempts to approach with practical directness some of the problems of military leadership. Though it is difficult, in this area of research, to draw a clear line between the applied and the basic approach, the present plan now is to look a

some of the research developments that have no immediate bearing on military practicality but which may guide future research and may lead, in three or five years, to significant practical applications.

In treating the "basic psychology of leadership" the procedure will be to summarize with relative brevity past research and then to present a "way of thinking" about leadership problems which may lead to basically significant hypotheses and which may lead eventually to useful applications.

A. The Search for Leadership Traits

It is a fairly safe estimate that nine-tenths of all the research on leadership, and ten-tenths of all expert and inspirational writing on the subject, have been concerned directly and almost exclusively with the characteristics of the leader. Stogdill's[27] recent review of leadership research cites 124 separate research papers, almost all completed since 1933, dealing expressly with the traits and alleged traits of leaders. The general aim of this trait search is to find, by means of observation or rating scales or psychological tests, those traits which leaders have but which their followers do not. It is easy to see why this search for the leader's traits has been carried on for so long on so many fronts. In the first place, we have the tools and techniques for dealing with the characteristics of individuals. American psychology traditionally has been interested in the individual and his doings. In the second place, we have tended to look at leadership as a function only of the leader rather than of a social relation between leader and follower. And in the third place, it would be so

very handy, for many purposes, if we could isolate leadership traits. If we can find out what the traits of the leader really are, then we can select very directly from among our candidates for leadership those who have the requisite traits, and perhaps we can train our chosen leaders to develop the necessary traits in a higher degree.

But accomplishments of this sort of research are not something to get excited about. Let's take a few examples of the results of trait research. It is certainly a reasonable hypothesis, on the face of things, that leaders will be older than their followers. You can test this hypothesis by selecting a number of top executives and comparing their age with the age of not-so-top executives. Or you can compare with respect to age a large number of college leaders with nonleaders. Stogdill summarizes 18 separate studies of this sort. In six studies, leaders are younger than their followers. In ten, leaders are older. In two, there is no difference in age. The correlations between leadership and age in these 18 studies varies from −.32 to +.67. There is clearly no general tendency for all leaders in all kinds of groups to be older than their followers.

Take another trait. Many people would expect that leaders will be more *dominant* than their followers. Stodgill cites eleven studies in which this hypothesis is borne out. But four investigators present evidence that dominant people are rejected as leaders.

Try emotional control. These are eleven studies in which leaders were found to be more stable and emotionally controlled than their followers. But five studies find leaders less well controlled than their fol-

lowers and three find no difference with respect to this variable.

All this is probably not as bad as it sounds. Each of the 124 researches Stogdill cites was done in a different situation. It cannot now be maintained, convenient though it would be, that there are basic traits possessed by all leaders in all situations. But it should not really surprise anybody that outstanding executives are, on the average, 12.2 years older than lesser executives and that student council members are younger than the average for the school population. Such facts, among other things, simply point out that leadership is complicated. And they suggest it is not to be very successfully dealt with on the basis of simple, currently measurable traits of leaders.

But we should not sell traits short. There is good evidence that some traits, ill-defined and fuzzy though they are, seem to characterize a wide variety of leaders in a wide variety of situations. For example, *verbal fluency* is a factor differentiating between followers and leaders in a large number of situations. And something called "insight" is another widely found characteristic of leaders. And leaders generally tend to be more intelligent than their followers, though if the would-be leader is *too much* brighter than his fellows he will not be followed.

Throughout these researches for traits the tendency has been to work with traits that are well defined and relatively amenable to measurement. It may well be that if we set up more genotypical hypotheses about leadership and seek to define a different sort of trait, we will find some personal characteristics common to

many leaders in many situations. And maybe we can measure such characteristics, can invent ways of selecting those people possessing them. At the moment, however, the status of knowledge of leadership traits is not conducive to optimism.

The studies of Carter and Nixon,[6] under the sponsorship of the Office of Naval Research, will reveal the sort of problems that arise when we experimentally examine for the existence of a generalized leadership ability. These investigators brought each of 100 high school boys into the laboratory and watched carefully, without the boys' knowing it, while each one worked in each of three leaderless group situations. Each boy was observed, scored, and phonographically recorded as he assumed or failed to assume leadership in doing an intellectual task, a clerical task, and a mechanical task. From these observations it was possible to obtain reliable indications of actual on-the-job leadership. Also, for each boy the investigators obtained (a) an extensive record of leadership activities at school, (b) ratings on leadership by teachers and supervisors, and (c) ratings through a nominating technique by the boy's contemporaries.

The problem here bears immediately on the existence of leadership traits. Does the boy who leads in the intellectual task also lead in other tasks? Is leadership general, or does it vary with the situation? Further, does the boy who has the traits leading to his *nomination* as a leader also have the traits leading to *performance* as a leader? The Carter-Nixon research can not give final answers to such questions, but the results are suggestive of the general relation between trait

and leadership performance. The boys who were observed to assume leadership in the intellectual situation also tended to assume leadership in the clerical situation. The correlation between leadership scores in the two situations was .64. But when put to work on a mechanical task, the intellectual and clerical leaders were very often displaced by others. The correlations here were +.40 between intellectual and mechanical leadership, +.30 between clerical and mechanical leadership. It is clear, then, that leadership in these tasks is not very general. Who will lead whom depends on the situation as well as on the traits of the people involved.

Further results from this study show that the boys whom their supervisors rated high for one type of leadership are rated high for all types. Supervisors appear to fall into the well-known halo error. The boys' contemporaries, however, seem more discriminating. To a much greater degree they tend to pick different boys for different sorts of leadership jobs. But neither supervisors' ratings, nor nominations by contemporaries, nor leadership activities in school, though they all correlate with one another, correlate well with the scores on actual performance. We can say, roughly, that the boys who have the traits necessary to *impress* others with their leadership potential do not necessarily have the traits to *perform* as leaders in actual situations.

All this is somewhat discouraging. The many-sided search for leadership traits has not paid off very richly. But the research cannot be counted useless. At the very least, we now have empirical evidence to guide us away

from the easy and erroneous assumption that we can construct a general list of leadership traits. And it remains true that misses and near misses, if carefully observed, are often necessary for a truer setting of the sights. From a practical point of view, however, even near misses are disappointing.

From all these studies of the leader we can conclude, with reasonable certainty, that:

(a) there are either no general leadership traits or, if they do exist, they are not to be described in any of our familiar psychological or common-sense terms,

(b) in a specific situation, leaders do have traits which set them apart from followers, but *what* traits set *what* leaders apart from *what* followers will vary from situation to situation.

B. *Research on the Situation as a Factor in Leadership*

One logical conclusion from all the studies on leadership traits is that the behavior of leaders—and presumably the traits that are invented to lie behind behavior—varies widely from one leadership situation to another. We have already seen that the individuals who are judged leaders in OCS are not often judged good leaders in combat. And Naval officers who are given good efficiency ratings at sea are not always (nor even often, when the r is .10) given high efficiency ratings ashore. Some of the discrepancy between ratings in one situation and those in another is undoubtedly due to the raters. But it is clearly conceivable that the men *are* good leaders in the OCS or shore situation but *are not* good leaders in combat or at sea. As the situation changes, the demands on the leader change. If the leader cannot meet the changed demands, his proficiency as a leader will obviously suffer. It makes almost immediate sense that we should not expect the officer who excels in combat leadership to have the sort of keen insights and subtle abilities demanded by an intricate administrative job. And the industrial executive who succeeds in guiding his company through a wild and rapid expansion is probably not the one to be at the helm during a period of calm solidification of success. The behavior of leaders obviously changes as the situation changes. The demands on the leader also change as the situation changes.

All this means that it may be profitable to throw a research light on the situations in which leadership occurs.

It is clear that we now have few really adequate ways of dealing with situations—with groups—as entities. When we set out to describe a group, about all we can say is that it is large or small, that it is primary or secondary, that it has a certain name, or that it serves a certain alleged purpose. Or we can talk about military groups, church groups, young people's groups, college groups, political groups, or family groups. But such descriptions are neither precise nor complete. They are roughly equivalent to describing a man as a small, friendly, blonde colonel in the Air Force. Such accounts are all right as far as they go, but they do not help much if we are interested in studying the intimate and subtle details of a leader's behavior as it relates to the group he is leading.

If we are going to get anywhere in studying the situa-

tion as it affects leadership, we need to discover or invent new ways of describing social groups. One interesting approach to this problem has been undertaken by Hemphill.[16] This research project undertook the relatively ambitious job of finding basic dimensions that can be used in the precise and systematic description of groups. The research was based on the notion that it would be possible to take any group, give it a score on each of a number of dimensions, and come out with something of a "profile," like the profile on the psychograph of an individual who has taken a battery of psychological tests. If we could do something like this for a group, then we might really get somewhere in predicting what sort of behavior a leader will find adaptive in what sort of group.

The Hemphill research on dimensions has not yet paid off in any very practical way. It has run into some bothersome methodological bugs and is, all in all, a very difficult sort of research to handle. We will not take the time here to give an account of how data were gotten from 500 assorted groups and then ground up in IBM equipment. We will simply list the dimensions that were tried out and illustrate how they can perhaps help in getting at leadership problems.

The 15 dimensions which were defined and applied to the descriptive analysis of 500 groups are listed below:

1. *Size* of the group.
2. *Vicidity* or the degree to which a group functions as a unit (togetherness).
3. *Homogeneity* of group members with respect to age, sex, background, etc.

4. *Flexibility* of group relation.
5. *Stability* of the group with respect to frequency of major changes.
6. *Permeability* of the group to new members.
7. *Polarization* of the group with respect to its goals.
8. *Autonomy* of the group with respect to other groups.
9. *Intimacy* among group members.
10. *Control* or the degree to which the group regulates member behavior.
11. *Participation* of members in the group's activities.
12. *Potency* or importance of the group for members.
13. *Hedonic Tone* or the degree of satisfaction derived from group membership.
14. *Position* of a member within the group's status hierarchies.
15. *Dependence* of members upon the group.

Any given group, supposedly, can be given a more or less precise score on each of these dimensions. These scores will constitute something of a profile for that group. Such a profile should be considerably superior to the description of a group as "a bedraggled group in a P.O.W. camp" or a "large, low-morale group in the Army." Such a dimensional description may help enormously in dealing with leadership. It is not inconceivable, for example, that we can find "types" of profiles and that we will eventually be able to select or train our leaders so that their behaviors would "fit" the sort of group they are called on to lead.

These are all worthy ambitions and they still appear to be realizable. But the millenium is a long way off. Hemphill used these 15 dimensions in describing the 500 groups on which data were gathered, then made some progress in relating the group dimensions to leaders' behavior. The very definition of the dimensions, as a matter of fact, leads to the setting up of testable hypotheses about leadership. Take the dimension of *dependence* for example. Groups obviously vary with respect to the degree in which the followers must depend on the leader for the satisfaction of their needs. In one group the leader has the power of decision over hiring, firing, promotion, or even over life and death. In another group the leader may in fact be dependent on the followers; if they don't like him they may eliminate him. What effect would you expect this variable to have on the behavior of the leader? In order to lead well in a group where dependence is great what must the leader do? You can set up a number of reasonable and testable hypotheses about leadership and its relation to dependence. For example, where dependence is high, the leader's perceived fairness in administering rewards and punishments is likely to become very crucial. Where dependence is high, the leader probably needs to be very clear about stating rules and regulations, but where dependence is low this is not likely to matter much.

The dozens of hypotheses that spring from this dimensional thinking have not yet been thoroughly examined. But as an example of what happens when such analysis is made, Hemphill and Westie[16] have studied in some detail the relation between the leader's be-

havior and the size of the group. To make such analysis the procedure was to separate the 500 groups on which data were available into "larger" and "smaller" categories, then see what specific behaviors on the part of the leader were observed by group members as occurring more or less frequently in the two groups of groups. Each group member who reported on a group was asked to check, for example, the frequency with which the leader "demonstrated physical courage." Each reporting member was also asked to judge whether this item of behavior applied to the group he observed. Such an analysis reveals that a large number of leader-behaviors occur more often in large groups and are more often applicable to large groups than to small groups. The following list gives examples of behaviors that apply to and occur in large groups more often than small groups.

Leader demonstrated physical courage.
Leader demonstrated "moral" courage.
Leader made rules and regulations clear.
Leader knew his job.
He allowed no exceptions to the rules.
He made people enthusiastic.
He co-ordinated different jobs.
He wisely delegated authority.
He could give orders pleasantly.

These and other data add up to the conclusion that large groups make more and different demands on the leader than do small groups. In large groups a larger portion of the leader's total behavior seems critical for his role as a leader. The leader of a small group is, in a

way, a freer individual. Generally speaking, the leader in a large group plays the role of impersonal direction coupled with a firm and impartial enforcement of rules and regulations. In smaller groups the leader plays a more personal role. He can make exceptions to rules, listen to others, treat each member as an individual.

The attempts to deal with the group as an entity—to be described and measured much as we describe and measure a human individual or an amoeba or a molecule—may lead us to pay dirt in leadership research. The work of Hemphill and similar efforts on the part of Cattell and others[7, 8] deserve close consideration. It is very conceivable that a dimensional approach to military leadership situations would yield immediately valuable insights having a bearing on both the selection and training of military leaders.

C. *Studies of the Follower*

The need to study the leader and the need to study the situation are both obvious. But what about the follower? Of course, when we look at the situation we are also looking, in at least an indirect way, at the followers. But perhaps a direct look at the led will help us make sense out of leadership. After all, it is the follower who accepts or rejects leadership, who often judges whether leadership is good, who works or loafs for the leader. We may well ask questions about the factors in the follower which bear on the sort of relation established between him and the leader. What about something we can call the "readiness for leadership" in the typical enlistee or draftee? What attitudes or traits or ideas does he have which prepare him to

accept or reject various sorts of leadership? What sort of followers adapt most easily to military leadership?

There has been no research designed to get at such problems. But there is a Navy-sponsored project now going on at the Institute for Research in Human Relations, at Philadelphia, which promises to turn up some significant things about followship.

This project, through the use of field survey and other techniques, has delineated certain tentative personality traits, certain attitudes and certain "ideological" factors in followers and has examined the relation of these things to the "readiness for leadership." We need not here go deeply into the theory underlying the study but the questions the study should at least illuminate include such as the following: "Are there discoverable traits of the follower which move him to accept or reject strong-man leadership? Does the personally insecure person seek out leadership and lean heavily upon it? What is the American attitude toward authority? Are we really, as the anthropologists tell us, an authority-rejecting people? What is the American 'ideology' of leadership, if any? Does the American individual have a set of standards by which he judges the adequacy of various sorts of leadership? What do the American people expect of their military leaders, and what do these expectations have to do with their reaction to a military leader when they meet up with one?" The answers to such questions can be expected to furnish useful knowledge about the background against which all leadership in America occurs, and will almost surely help define the general leader-follower relationship.

The data from this study[23, 24] show with reasonable clarity that factors in the follower do influence his attitudes about leaders and will, presumably, influence his choice of a leader, or his behavior in the presence of a leader. For example, the American people perceived Roosevelt primarily as (a) a man who warmly liked people, (b) a man who supported and "looked out for" the little man, and (c) a man of great personal strength. It is fairly safe to say that the American people had certain "needs" and that they perceived FDR as the man who met these needs, who solved their problems. Perhaps we can describe these needs as (a) a need for approval from above, (b) a need for material support, and (c) a need for a strong father-like figure to reassure them in time of stress. Such conceptualizations leave much to be desired, but it is clear that followers will follow a leader who meets their needs, who solves their problems. And thinking in terms of needs of followers may give us new insights into leadership.

IV. Toward a Theory of Leadership

We are now in possession of many facts and insights concerning leadership, military and otherwise. Many of our facts are negative but none the less sound, and our insights are partial but still valuable. We also possess considerable knowledge about both individual motivation and group process, knowledge of direct relevance for the understanding of leadership phenomena. It does not appear too optimistic to hope that we can soon incorporate all existing facts into a systematic theory of leadership, a theory the formulation

of which would guide toward additive significance our separate research attempts and which, if formulated, would hasten the arrival of demonstrably useful applications.

I wish now to spend a few minutes stating some general ideas about one possible road toward theory and then to take an exploratory walk down this road.

The first general point is this: it now looks as if any comprehensive theory of leadership will have to find a way of dealing, in terms of one consistent set of rubrics, with the three delineable facets of the leadership phenomenon; (1) the leader and his psychological attributes, (2) the follower with his problems, attitudes, and needs and (3) the group situation in which followers and leaders relate with one another. To concentrate on any one of these three facets of the problem represents oversimplification of an intricate phenomenon. A focus on the leader alone will probably continue to yield positive but unexciting correlations. To concentrate on the follower alone will reveal relationships, but probably not very significant ones. A focus on the situation alone may carry us to a level of abstraction that obscures the dynamics of individual psychology and hence lessens the completeness of our understanding. A good theory must include, but somehow rise above, the facts we now have or may accumulate in all of these three limited areas of concern.

A second general idea I wish to express gets a little more down to earth and deals with one possible way of drawing a comprehensive theoretical picture at least crudely inclusive of what we now know about the leader, about the follower, and about the situation.

This general way of thinking involves the four following points:

1. There is a follower in every instance of leadership, a follower with certain problems, attitudes, expectancies, and needs.

2. In any group the motivational pattern of the single follower, and of the followers in aggregate, will depend on characteristics of specific situations. In one situation long-standing individual motivations will hold sway. In other situations motivations specifically and focally connected with an explicit group goal will be the salient motivations. For example, a hypothetical need for approval from above will be important in many situations but will give way in emergency situations to more specific and situationally determined patterns of motivation.

3. In any situation, the pattern of follower motivations will put demands on the leader, demands the leader must meet if followers are, both psychologically and physically, to stay in the group. In some situations, for example, the leader must be strong enough to meet salient dependency needs while in another he must be able to encourage and implement the followers' need for autonomy and responsible participation.

4. Whether or not the leader meets the demands upon him will depend both on his abilities and on some deep-lying personality attributes. If the group seeks a concrete goal, the leader is under pressure to give the technical assistance necessary for the reaching of that goal. If the group is in a state of insecurity and needs a strong leader upon whom to lean, the leader's basic orientation to his own authority must allow him

to assume toward the group a strong, father-like role.

If the situation is such that the follower's need for Ego-income is great, the leader must be able to deny directive authority and play a role in which nondirective procedures are paramount.

Here, then, is the bare outline of a way of thinking about leadership. The outline will need much filling-in and perhaps serious renovation before it becomes anything approximating a systematic theoretical picture. But in its present form it gives some promise of including a large number of known facts and it leads to some potentially profitable hypotheses. It may deserve some present elaboration.

Let's look for a moment at the things a follower brings with him into a leadership situation and then we can focus, for purposes of illustration, on one important follower need as it varies with the situation and as it makes demands on the leader.

Any follower who comes into a group brings his individual personality with him. He is, in large degree, a product of his social environment, a bearer of the motives and inclinations common among those who have been exposed to the same society he has made peace with. He also brings, of course, his own unique orientation to life.

Many of his existing needs and attitudes have a great deal to do with his readiness to respond to the leader of the group. He has, perhaps, a strong need for fatherly approval. Or he may have a need to lean dependently on a strong leader who will do his thinking for him. Or he may have a hidden desire to kick all authority in the teeth. Perhaps he brings with him a

learned bias against big men or men with red hair. Perhaps he carries a picture in his head of what a "real leader" is like. He brings his readiness to respond to various bearers of social status such as the wealthy or the educated. In the presence of any leader, all his learned ways of reacting to figures of authority come into play.

Of course the follower reacts to more than the leader in a group situation. The other members of the group are also potential sources of psychological income or of frustration. And the goal the group seeks is a very significant element in his motivational pattern. His general willingness to stay in the group, to contribute to it, is a function of his hypotheses about the psychological income to be derived from all these sources.

Now let's take one motivational element that seems crucial in many groups and examine it as it relates to changes in the situation and to the behavior of the leader. Our Philadelphia studies strongly suggested that followers in very many situations have a need for approval, for a feeling of belonging, of usefulness, of being respected and liked as individuals. Our data suggested that such a need leads to the seeking of leaders characterized by warmth and humanity, leaders who "like people." It is perhaps both adaptive and justifiable, at this juncture, to broaden this "need for approval" and to follow Likert[19] in referring to a more inclusive motivational syndrome that can be called Ego-needs. This syndrome has often been referred to in psychological literature without ever having been defined with optimal precision, but for present purposes we can use the term to refer to the individual's

desire to be recognized, to feel useful, to be approved, to feel integral and responsible. And we can set down some loose-jointed hypotheses about the way Ego-needs vary with changes in the situation and the way such variations give rise to changing demands on the leader.

Any American follower brings his Ego-needs with him when he comes into any group, whether the group is an infantry squad, a bomber crew, a PTA organization, or a road construction gang. Unless the over-all group situation satisfies these needs, there will be an increase in the individual's inclination to withdraw from the group or to pursue only passively the group's goals. The leader of the group, both in terms of the follower's perception and in terms of objective reality, has a good deal to do with the satisfaction of these needs. The strength or saliency of such needs will vary, but seldom are the needs completely dormant.

One very general hypothesis concerning the relation between the strength or saliency of Ego-needs on one hand, and the characteristics of the situation on the other, is as follows:

The need for Ego-satisfactions on the part of followers will increase as the potency of the group goal decreases. This hypothesis says, in effect, that a group of hungry men will follow a leader who can help them get food no matter how much of an S.O.B. is the leader. It also says that the West End Knit and Chat Club, having no psychologically potent goal to pursue, will soon disintegrate or will soon reorganize itself, if its leader blocks the followers' attempts to satisfy their Ego-needs. In military groups, where there is a life and death emergency, it does not matter if the leader is

poor at arranging smooth interpersonal relations. If he can get us out alive, he is acceptable and he will be followed. In many groups, the "popular" person, the sociometric hot-shot, is the one who is perceived as the best leader. He makes everybody feel like somebody. (At least this is one theory of popularity.) But if the group with a popular leader suddenly faces an emergency, the demands on the leader come in a different form and it is not enough that the leader be a "nice guy." The group goal becomes more potent and the nice guy is traded in for a new leader who can help solve the problem. The experiments of Carter and Nixon[6] illustrate that the leader who can win nominations from his followers and from his teachers is not necessarily the leader who is followed when a real group goal emerges.

We can further state two secondary hypotheses about the relation between Ego-needs and the group's relation to its goal.

1. *As the group goal becomes more clearly defined, there will be more emphasis, other things being equal, on the leader's ability to help the group reach that goal, and less emphasis on his ability to satisfy Ego-needs.*

2. *As progress toward the group goal becomes more visible, there will be increased emphasis on the leader's goal-relevant skills and less on his ability to satisfy Ego-needs.*

These hypotheses say that in such settings as bureaucratic organizations, where the goal is not clearly defined and progress toward it is not clearly visible, the "nice guy" leader can stay in office for years without having to demonstrate any technical ability. He needs

only certain skills in human relations. In a submarine on patrol, by contrast, where the goal is clearly defined and where every member of the group knows whether the hit is scored or whether the boat surfaces when it should, the skipper has a technical function. His ability to perform this function, in a functionally organized group, assumes great importance.

It is possible to set down a number of additional hypotheses about the variations of the strength and form of Ego-needs with variations in other dimensions of groups. The following will be illustrative:

1. Follower's Ego-needs decrease in strength as the polarization of the group increases. A group that is busy pursuing a goal will not take time out to worry about whether everybody is somebody. Perhaps a group cannot often get itself polarized unless Ego-needs are already satisfied, but once vigorous action is in progress the important thing is to reach the goal. All else is secondary.

2. As the size of the group increases, Ego-needs are less likely to be satisfied. This hypothesis raises the old question about the optimal size of a committee. The larger the group, the more difficult it is for every individual in it to be individually recognized. Ego-needs are likely to be better satisfied in small and informal groups, when interpersonal contact is maximal. Perhaps people will generally identify more strongly with small than with large groups. At least it seems clear that in large groups the leader cannot do the same things he does in small groups to satisfy Ego-needs. The data of Hemphill and Westie[16] show that he does not. In large groups the leader's *general appearance* of

warmth and humanity probably becomes more important, with respect to Ego-needs, than what he actually does in his interpersonal relations.

Followers' Ego-needs, in strength and manner of operation, vary as the characteristics of the group change, but these needs are present in a large variety of groups and they are often so important that they must be satisfied if the follower is to remain in and contribute even minimally to the group. Likert[19] has shown that industrial groups with "employee-centered" supervisors have higher productivity than similar groups with "company-centered" leaders. There is a good deal of evidence that military leaders who are "for their men" are the ones most enthusiastically followed. The superiority of democratic over autocratic groups in many situations is probably due to the fact that democratic procedures give followers more Ego-income. We cannot deny the importance of Ego-needs. And we cannot doubt that the leader in any group has a significant hand in determining whether or not followers feel wanted, approved, and recognized. This is one important way in which the leader determines the follower's psychological income, and hence the productivity of the group.

All this leads us now to ask questions about the leader's personality. What sort of leaders are able to meet the demands, in the various forms, for Ego-satisfactions? Such a question is probably not now answerable in terms of available conceptual or technical tools, and hence it is a very troublesome question. But wrestling with it may still be worth the trouble it entails.

It seems reasonable to believe that the leader who, other things being equal, can best satisfy the follower's need for acceptance and approval is the leader who genuinely likes people, who works on the general hypothesis that people are good and that the whole human enterprise is worth while.

It is not hard to believe that most of us, in our relations with people, act in consistent accordance with a learned general hypothesis about the goodness of human beings. Some people act in apparent consistency with an optimistic adience toward any human being that comes along. They exude an air of acceptance and approval. Some, at the other extreme, are at least initially abient to any other person and appear to be continually seeking evidence to document the belief expressed in Steig's well-known cartoon, that "people are no damn good." This abience may take the form of paranoid suspicion or of scorn, depending on whether the individual perceives himself as above or below his fellows. But whatever its form, such a general readiness to respond to people may have a good deal to do with an individual's performance when placed in a leadership role. If followers need to be liked, their relations with a leader who basically likes no one can be counted on to be mutually unsatisfactory and conducive to unproductive group morale.

Can we define and measure such a variable? There seems to be no real reason why we cannot. We could now probably do a fairly reliable job of rating our acquaintances on a continuum from like-people-in-general to dislike-people-in-general. If we could do this, and if we could also determine for any given situa-

tion, the strength of the follower's need for Ego-income, we could state and test some relatively neat hypothesis about what sort of leaders will win what sort of acceptance and promote what sort of group effort. At a very general level we can state the hypothesis that over a period of time and throughout a variety of situations the industrial or military leader who likes people, who is "people-centered," will, other things being equal, achieve better group productivity and better subjective group morale than will the leader who is possessed of a deep distrust of people.

This analysis of the follower's need for approval, its variations with changes in the situation, and the implications for the personality of the leader give an example of the sort of hypotheses growing out of the approach here advocated. Though presently our definitions are fuzzy and our concepts lacking in neatness, the approach may prove productively provocative to somebody and may lead to some solid experimental investigations.

We can make the same sort of analyses for other follower needs. Take the follower's need for strength from above, a need the Philadelphia study indicated to be important. Probably this need increases with the potency of the group goal and the general insecurity of followers. What sort of leaders or potential leaders have the ability to assume great responsibility for the welfare of others? Some people seem basically incapable of making decisions for others. They cannot play the role of a strong father. Maybe their need to be loved by their followers is too extreme to let them run that risk of disapproval that resides in the assumption

of responsibility. Other people *must* assume responsibility for others—they need power and a dominant role. Still others can assume power or they can leave it alone, as the occasion demands. A significant aspect of the leader's personality, this reasoning goes, is his attitude toward his own authority. Perhaps this attitude, too, can be incisively defined and its relation to group performance systematically studied.

The follower's hypothetical need for structure leads to further hypotheses. This need will vary from situation to situation but its presence anywhere leads to questions about the leader's *interest* in giving structure and about his ability to do it. Research results show that in many situations leaders are characterized by both more intelligence and more verbal fluency than are the followers. These facts may be tied together under the general heading of the ability of the leader first to see what the problem is and then to communicate it to his followers. In addition to the *ability* to see and to communicate structure, the leader must *want to* give structure. Some leaders (e. g., some teachers) appear more interested in letting the followers know that the *leader* knows about everything than in letting the followers see the problem for themselves. This sort of factor in the leader's personality might well be investigated further.

There are other follower needs we might think about with profit, but demands from followers are not the only source of pressure on the leader. Many leaders themselves have leaders. They work in an official hierarchy with official goals to achieve. In many instances the leader's success is judged in terms of the group's

performance in advancing a goal imposed from outside itself. This sort of arrangement raises fascinating problems in leadership and in morale. We will not take time here to do an essay on this problem, but it may be worth while to set down one general hypothesis about the leader's orientation to "official missions."

The general hypothesis says that individuals differ widely in the proclivity for accepting "missions from above." Some people are chronic "company men." They accept any goal that is handed down from authoritative sources. They pursue it vigorously and will do almost anything to make their followers pursue it. Others are unable to accept any mission from above; in any organization, they are constitutional outlaws. Still other individuals can accept some missions from above and can, with skill and rationality, persuade followers to pursue them. The leader's orientation to official missions is probably a consistent aspect of his personality. It probably can be defined and dealt with in relatively objective terms. It probably has a lot to do with morale and effectiveness of the groups he leads.

SUMMARY

In a number of ways, psychological research has contributed usefully to the solution of practical leadership problems. It seems fair to conclude that in the military and in other settings we can now select and train leaders better than we could 25 years ago. Through psychological research we can now select potential leaders who are in known possession of certain attributes (e. g. intelligence) widely believed to be necessary for successful leadership. We have invented ways

to increase the reliability of judgment about the effectiveness of leaders, thus eliminating a good deal of adventitiousness from the processes of selection and promotion. Our knowledge of group processes is increasing, and may yield valuable insights into problems of leadership. We still have not solved the problem of a criterion of effective leadership, but this problem is not necessarily insoluble. Good hints come to us from those who work on the characteristics of groups and on criteria of group effectiveness. Research now in progress is based on a keener insight than was the research of five years ago. We now at least know something about which alleys are blind.

We can, with a right good will, continue our efforts to understand leadership, for even though practical results are slow to come, the potential social benefits in even a minute improvement in leadership are indeed tremendous. Our chances of achieving such benefits, if our opportunity to do our research is not restricted, appear to be excellent.

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PROBLEMS IN THE ADMINISTRATION AND UTILIZATION OF CONTRACT RESEARCH

JOHN W. MACMILLAN

WHEN asked to participate in the sixth Current Trends program I was told that emphasis should be placed upon the utilization of contract research studies, and that little comment was needed regarding the administrative aspects per se. I replied that administrative problems are sometimes as important as those of utilization and that I would welcome an opportunity to relate some of the experiences and problems encountered during the five and a half years I have been directly concerned with the administration of a research program consisting entirely of contracts between the U.S. Government and universities, nonprofit organizations, and consulting groups. Permission to do this was granted, hence the title of this paper. You have only me to blame for this inclusion of administrative problems in a series which is supposed to report the development of psychology as science, with reviews of progress and method. My reasons for doing this are several.

First, the title of this conference, Psychology in the World Emergency, indicates logical consideration of the role psychology plays in the present cold-hot war. This role is largely determined by people in administrative positions.

Second, psychology as science, or if your prefer, psychological research, is to a considerable degree de-

pendent upon government funds for support, and this support is in turn dependent upon the psychologists and others responsible for the administration of research programs.

Third, research administrators are in a measure the determiners of directions in which psychological research is guided now and in the future.

Fourth, the support of research has assumed many of the characteristics of "big business," with many of the same administrative problems. It is thus a legitimate field for study. There now are at least seven government agencies which support continuing programs in social science research. Several full-time administrative positions exist in each of these, to say nothing of part-time employees and advisors. While the funds involved represent a very small fraction of the total research and development budget they still amount to quite a few millions of dollars, or "megabux," and the administration of this amount entails some interesting and provocative problems.

My discussion will concern the administration of contract research and its utilization, omitting reference to in-service programs of research except for comparison. I have drawn heavily upon personal experiences and those of my close associates, but I do not believe that these differ to any great extent from experience of administrative people in other agencies. Discussions and meetings with representatives of other groups, and the resultant familiarity with their programs, clearly shows that many problems are universal, and that they are handled in similar fashion in different agencies. Many of the problems are common to the

administration of contract research in all fields of science, physical and medical as well as psychological. Others are specific to the conduct of psychological projects. No attempt is made here to differentiate these problems, but the intent is to present an over-all view of psychological research administration.

Let us take a quick look at a bit of history. Immediately following the cessation of hostilities in August of 1945, hundreds of psychologists discarded the uniform and returned to civilian life; OSRD-NDRC went out of business, releasing many more from wartime research projects; and the military services cut their personnel staffs to ribbons. I cannot speak for the other services, but shortly after the war there was only one psychologist in Navy uniform (and he was called something else!) and one division which, during the war, had employed nearly two hundred people now had a staff of eight.

While this mass exodus was going on a government lush with victory and uncommitted funds was persuaded to make money available for research support. A few hardy souls accepted Civil Service positions to administer these funds, and the growth of research support into big business began. The Navy's Office of Research and Inventions was changed to the Office of Naval Research. The Joint Research and Development Board was established, later to drop the "Joint" when the Air Force was unified into a separate service by the National Security Act of 1947; the Human Resources Committee of the Research & Development Board was formed; advisory panels were organized by the services; and funds were allocated for both in-

service and contract programs. In 1948 and 1949 there were setbacks to the research program as a whole. An economy minded Congress reduced budgets and many programs suffered reductions of funds. This was only temporary, however, and the psychological program suffered little due to the efforts of the Human Resources Committee and other groups. The increased international tensions beginning in 1949 resulted in increased budgets which have continued until the current fiscal year.

Although the future is uncertain, the establishment of the National Science Foundation, the possibility of a settlement of the Korean war, and the political upheaval certain to result if we have a change of administration this year, will profoundly influence the budgets of the next few years. It is to be hoped, however, that the value of psychological research will be recognized and that budget cuts, if they occur, will be minimal.

You will remember that prior to 1942 there was practically no governmental support of psychological research. During the war large sums were expended, primarily on in-service programs under the direction of uniformed personnel, and in circumstances considerably different from those of the present. The large research program built up since the war has been administered by people who had, to coin a phrase, to fly by the seats of their pants, and sometimes found themselves in difficulties because the position of their heads interfered with gluteal proprioception. The problems confronting these people may be discussed in any number of ways. I have chosen to consider them in the form

of questions, because they are perpetually confronted with questions to which they must offer solutions.

It is in human relations research that the greatest difficulty is found, both in the conduct of the program and in utilization of results. This is a new field, methods are not stabilized, results are far from clear cut. It borrows from many disciplines of psychology as well as from sociology, anthropology, political science, psychiatry, and economics. There is as yet no solid body of theory of principles, because it is still in the data gathering stage of development, which must precede theory. Leadership, group organization, communication of ideas, and the effect of racial and cultural factors on behavior comprise its subject matter. These cannot be studied in vitro, but must be observed under natural conditions where the attendant variables are so numerous that reliable data can be obtained only through many observations. This can be called basic-applied research because under certain conditions it is possible to advance our knowledge of the determinants of human behavior while at the same time utilizing research data to improve human relationships. The potential pay-off value of this type of research is enormous. Even at its present primitive stage enough has been learned about certain functions of leadership, supervisory practices, and the communication of ideas to contribute materially to military efficiency if it were properly applied in training programs and operations. It is difficult to convince military leaders of this, however, since they feel that they have been getting along

pretty well with the status quo and are rightfully resistant to change without overwhelming evidence of the value of other practices.

Another avenue toward increasing utilization of psychological research is through the enlightenment of civilian groups. The ignorance of the general public of the potential value of psychology in a world emergency is frightening. Any decrease in this ignorance would extend the utilization of research immeasurably. Possibly a campaign organized by professional groups such as the APA, SSRC, SPSSI, and others would prove worthwhile. Certainly, some formalized informational program would pay dividends out of all proportion to the investment.

I have tried to convey to you something of the job of the research administrator, to describe in brief his problems and responsibilities, and to outline some of the major obstacles to more effective utilization of psychological research. These are not insurmountable, by any means, and the gains that have been made in the past half-dozen years are little short of phenomenal. Six years ago no one could have predicted the greatly expanded research program or the acceptance level it now occupies.

Psychologists are enjoying the novel experience of being in short supply, and the resulting financial and personal gratifications. I trust sincerely that these bene-

fits will continue, but for a moment I should like to go back six years to the conference on Military Contributions to Methodology in Applied Psychology, held at the University of Maryland in November of 1945. In his closing address Jack Jenkins reviewed the progress of psychology through Phase I, or the local loyalty period of school psychology, and Phase II, the development of a professional loyalty, transcending schools, which ended with World War II. He suggested that we were then entering a third phase—that of social loyalty and responsibility, where we have to ask if our results have social and statistical significance.

Undoubtedly, some of the research now in progress has great social significance. Included in this volume is Fill Sanford's contribution, for example.

But I would like to leave you with the thought that maybe, and I really mean maybe, Psychology in a World Emergency is not completely fulfilling its responsibilities to Society in a World Emergency.

THE USES AND LIMITATIONS OF MATHEMATICAL MODELS, GAME THEORY, AND SYSTEMS ANALYSIS IN PLANNING AND PROBLEM SOLUTION

JOHN L. KENNEDY

[The preparation of this paper was greatly assisted by discussions with Allen Newell, W. C. Biel, and R. L. Chapman. They should, however, be relieved of responsibility for my interpretations of their thoughts.]

BEFORE launching into the formidable array of subject matter contained in the title of this paper, let me say, by way of introduction (and perhaps of apology) that, for the past year, I have been straying from my natural habitat, the fields of human engineering and physiological psychology, into close daily association with a group of mathematicians, philosophers, physicists, engineers, social scientists, and psychologists. The common problem of these specialists may be stated as follows: How can we deal with the complexity of real human affairs? What methods, preferably scientific and objective, can be utilized or developed for predicting the behavior of complex, interacting systems? This is the problem referred to in the title of the present paper.

The word "systems" will appear rather frequently from now on. Webster says that a system is "an assemblage of objects united by some form of regular interaction or interdependence; an organic or organized whole." Webster is here defining what we should recognize as a simple system since the key word in this defi-

nition is "regular." We wish to deal with more complex systems, such as assemblages of machines, assemblages of men and machines, assemblages of men and the ultimate complex system, individual human behavior, where the interactions between parts, although well-recognized, are anything but "regular." If this is too strong a statement I think most of us would agree that much of the regularity remains to be discovered.

Why study systems rather than components? Because the important questions we are asked to solve today are primarily questions involving complex interactions. In real life we are forced to solve "staff" problems as well as "line" problems. We are forced to plan, and thus to predict, in order to adapt successfully to the emergencies of life.

It has been pointed out many times that the scientist, when he is faced with complex systems, usually begins on his problem of prediction and control by breaking the system up into components of a convenient size and degree of complexity. He studies the components intensively and, many times, is piously content to leave the problem of interaction of components to some future time or to solution by wisdom. I am particularly sensitive to this criticism in relation to the field of human engineering, where, it seems to me, component thinking has dominated the efforts to establish it as a substantial scientific contribution to the real world of human affairs. I am reinforced in this conviction by the work we did at Tufts College in preparing the *Handbook of Human Engineering Data for Design Engineers.*[4] The Handbook answers many component questions having to do with the parts of the

human and his environment, but it is woefully weak in answering systems questions. Perhaps this is another way of saying that there is no central theoretical framework on which the data are hung—there was no mathematical model for spelling out the complex interactions everyone sees from common observation of individuals and groups.

Let us try out some systems questions on the human engineer to make the point possibly a little clearer. The aircraft designer wants to know, for example, the relation between the size of the dial of a particular aircraft instrument and the performance of the pilot-aircraft combination in executing a standard military mission. He is not much interested in learning that speed and accuracy of dial reading improve as dial size is increased because the performance criteria are *specific* to the component. He wants to be told that either this instrument component is critical to *system* performance so that he should maximize its space, or that its particular size, within wide tolerances, is not critical to the successful performance of the military task for which the plane is designed. Then he can go ahead with confidence in working out the optimal dial sizes in accordance with over-all space limitations of the instrument panel in a reasonable way. Actually, the human engineer makes that latter kind of recommendation but, it should be emphasized, he does most of it by using common sense, intuition, or wisdom, and not with the data from his science.

Component thinking may be illustrated in another more particularly human case. The National Research Council's Committee on Aviation Psychology[7] investi-

gated the contribution of the pilot's visual acuity to the task of learning to fly an aircraft. "Normal" visual acuity, as measured by the Snellen Chart, has for years been required of all aviation cadets. The Committee's studies demonstrated, however, that within a range of tolerance, visual acuity of trainees did not significantly affect training time or terminal skill.

Component thinking vs. systems thinking appears most clearly in psychology in the choice of the criterion measure for evaluating performance. At the risk of appearing to pour salt on old wounds of some of my colleagues on the program, we may cite as an example of component thinking the choice of school grades as a criterion measure for determining the effectiveness of selection tests. Our real interest is prediction of success on the job, even though it may be argued that success in school is a necessary antecedent to job success. It is possible that we are too frightened of negative results and this fright drives us to carve our systems up. Negation of an hypothesis about the relative importance of a component in a system has equal stature with affirmation. Thus, it probably saved the Western Electric Co. in the famous Hawthorne study a considerable amount of money to discover that illumination level of the work space was not a very important determiner of productivity.

You have recognized by this time, I am sure, that it is difficult to draw a line between components and systems. "Big fleas have little fleas," big systems have little systems, and so on, ad infinitum. In the real world a system is defined by the questions the scientist or expert is asked to solve, or that he asks himself. A com-

ponent solution is one that may be perfectly valid, but it does not answer the question. I shall argue, then, that to deal with the complex problems of human life we are forced to adopt broader systems criteria.

Let me remark, before going on to the next section, that this feeling of uneasiness with respect to component criterion measures is coming to be shared by many users of electronic devices. The electronic gadget is the example par excellence of the component measure—we build a resistor to, or so many ohms, without reference to the demands of the system in which it will become a component. Electronic devices have become complicated enough so that we are forced to re-evaluate how the components interact in determining the over-all goal of the system. This field of study in engineering has received the name "Machine System Reliability." In psychological terminology it is machine system validity that is being studied. The question asked is: "What do we have to do to the parts in order for the whole system to perform the over-all task we wish it to perform at some level of predictability?"

WISDOM AND COMMON SENSE

I did not include wisdom or common sense in the title of this address as a solution to the problem of complexity because it is such an obvious one.

We solve our everyday problems with this device in one form or another and hardly ever stop to examine the method unless we happen to be philosophers. If we feel that our own wisdom is inadequate, we consult the wisdom of someone else, the "expert." Now, I don't need to flog the idea that the solution to the problem

of complexity by the opinion of experts leaves much to be desired. If he is the right kind of expert, however, he can perform an important function in problem solution. He can bring to bear accumulated historical wisdom, i.e., how components interacted in relation to systems criteria in the past, either by reference to his own experience or through study of the great interacting systems of human affairs in the past. This kind of wisdom is an extremely scarce commodity. What we usually find is component wisdom or common sense, which results in emphasis on one aspect of a complex problem with the other equally important aspects ignored. I presume that this observation about the behavior of experts is responsible for man's attempts to generate other methodology, such as the scientific method, to assist him in mastering complexity. But here again we ask, "Which scientific method, systems methods, or component methods?" Again the answer is to be found in the choice of a performance criterion, which, in fact represents the level of aspiration of the scientist. Let us consider, now, some alternatives to the method of wisdom.

MATHEMATICAL MODELS

Mathematics, the queen of the sciences, also offers a traditional solution to our systems problems; traditional in the sense that mathematical methods have been successful in dealing with many complex interactions in the physical world, and it is quite natural to assume that they would perform equally well in dealing with the complexities of human interaction. Why is it, then, that social scientists are not more inclined

toward mathematical model building? Two general answers may be given to this question. The first has to do with the information or data about a complex interaction supplied to the mathematician. The second general answer is that the mathematics for handling interaction problems is still in an early stage of development.

Mathematics provides the optimal language for communicating complexity, but mathematics only operates with the information fed into it. If only component information is put in at one end, no information about the total system comes out at the other end. If we require the mathematician to provide solutions to systems questions without giving him the necessary data, he will make some assumptions on his own in order to wrap up his package. Then we find that we cannot stomach the assumptions, that the mathematical model is too simple, that our wisdom tells us that life really isn't that way, and we become suspicious of the whole mathematical model concept. I would argue that the fault, if there be one, in this controversy, lies mainly with the provider of data for mathematical treatment. We stick to our small components when the mathematician needs quantitative *system* information for his special brand of magic.

Here is an example which will illustrate the limitations under which the mathematician works. It is desirable, from the point of view of a tracking system designer, to describe and predict the performance of a man-machine target tracking system by means of mathematical formulae. P. M. Fitts[3] describes a mathematical model, proposed by Ragazzini, for the response

of the human operator in the tracking situation. It is the second-order linear differential equation:

$$\theta_{out} = a\frac{dE}{dt} + bE + c\int Edt \quad \text{delayed by .3 seconds, (the RT of the subject)}$$

where θ_{out} = the operator's response

E = the error or difference between actual position of the target and desired position of the target.

a, b, c = constants

$\frac{dE}{dt}$ = rate of change of error

$\int Edt$ = summation of error information over time t.

The model predicts that the movement the operator makes is a linear function of the rate at which the error is changing, the absolute magnitude of the error and the integral of past error.

Now, I believe that it is fair to say that this model presents only a partial picture of the variables that enter into human performance in tracking. The work of Ellson, Taylor, Tustin, and Hick has clarified in what ways the human individual differs from a continuous servomechanism. Among these may be listed learning, set from instructions, interest in the job, rapport with the experimenter, etc. So the constants in the equation are not constants. They turn out to be variables in time and hence are to be described by some sort of learning function. They are also variables with the complexity of the input, as the work of Ellson and Taylor has shown.

The important point to note in this example is that when we come to test the predictive efficiency of the equation describing the system it is found to be discouragingly low. Apparently the mathematical model for the man-machine system does not have enough complexity in it to describe the system successfully. This situation I would blame on the aspects of performance which were originally measured. We do not supply the mathematician with an adequate description of a system.

Let us now explore the mathematician's contributions to the problem of complexity. My mathematician colleagues tell me that there are two promising developments for dealing with man-machine systems, namely, (1) the theory of stochastic processes and (2) the theory of games.

A stochastic variable is one, the set of possible values of which is known, but it depends upon chance which of these values the variable assumes. Independent stochastic processes have yielded the classical theory of probability with the familiar models of the die, the deck of cards, and the urns filled with white balls and black balls. Independent stochastic processes are at the basis of the kind of statistics most psychologists use.

Dependent stochastic processes, where the set of possible values of the variable are known but the variable assumes a value dependent upon a previous value, look more interesting from the point of view of handling complexity. One may cite here such developments in dependent probability analysis as time series, information theory, and the study of communication.

Already, there have been several applications in the psychological literature. Let us consider one such ap-

plication which is directly related to human interactions. Bales[1] is interested in predicting the interactions which occur in a small group while solving or reaching a decision concerning a group problem. He describes a model for two-person interaction as follows: "Visualize two persons who have met to discuss a problem, and agree on a specific course of action. Let us say that they have a stock of experience, inclinations, impulses, etc., which give rise to certain 'suggestions' in their minds. Each of them also brings along a critical frame of reference—certain generalized preferences, prejudices, values, etc.—in terms of which he evaluates the suggestions that occur to him. Let us say that when persons interact with each other there are four general types of messages which they send to each other. We will call these 'suggestions,' 'positive reactions,' 'negative reactions,' and 'questions.' A suggestion by one typically leads to a positive or negative reaction or a question from the other. The reaction or suggestion received is a kind of 'feedback,' and exerts an influence on the future direction of the process. The alternating process of giving suggestions and reacting to them is a circular process of social interaction."

The above quotation describes a dependent stochastic process since the values of the behavior of each subject are specified or categorized in advance, and which one at a particular time (suggestion, positive reaction, negative reaction, question) is chosen by the subject in accordance with a preference schedule which it is possible to describe in terms of probability. Thus, let us suppose that each subject makes a suggestion with $P(s) = .1$, makes a positive reaction with

$P(p) = .4$, a negative reaction with $P(n) = .3$, and questions with $P(q) = .2$.

Then, the two-person interactor may be diagrammed as follows:

(1) ⟶ ⟵ (2)

PREFERENCE SCHEDULE	$P(s) = .1$	$P(s) = .1$
	$P(p) = .4$	$P(p) = .4$
	$P(n) = .3$	$P(n) = .3$
	$P(q) = .2$	$P(q) = .2$

The process is started by, let us say an assertion by (1); (2) reacts by a negative reaction; (1) then reacts with a question; (2) with a positive reaction, and so on. The interaction model, even in the simple preference situation given above, is complex enough to require a computer to solve. When the possibility of learning is introduced as the effect of our own decisions and kinds of responses received from others on the valves for the preference schedule, the actual computation becomes quite difficult.

The other development toward a mathematics of complexity comes from a desire to predict the outcome of games involving a number of players operating according to a set of rules. The publication of von Neumann and Morgenstern's *Theory of Games and Economic Behavior* in 1943[6] has resulted in much pure mathematical work to extend the basic ideas to encompass more and more complexity. We shall not have time to give more than one example of the kind of computation required to describe and predict the outcome of a game, but let us illustrate with the simple game of "Her."

This is a so-called two-person zero-sum game. Each player puts up 50¢. From a shuffled deck of playing cards, the dealer gives a card to the receiver and one to himself, both face down. The man who ends up with the highest card wins, but draws go to the dealer. The ace is low in this game. The receiver may compel the dealer to exchange cards, but, if the dealer has a king, he is allowed to retain it. If the dealer is not content with the card he first obtained, or which he has been compelled to take from the receiver, he may draw a new card from the deck. If the drawn card is a king, however, he must play with his previous card.

The game, then, consists of three moves: The first move is a chance move, one card each being dealt, at random, to the receiver and dealer. The second move is a personal move by the receiver, who exchanges his card with the dealer or stays with the original card. The third move is a personal move by the dealer, who exchanges his card with a card from the deck or stays with the card he holds.

The game may be summarized by defining a strategy for the receiver to be a determination of change or stay for each of the 13 cards. One such strategy might be a simple alternation such as:

1	2	3	4	5	6	7	8	9	10	J	Q	K
C	S	C	S	C	S	C	S	C	S	C	S	C

Note that the strategy is a complete set of instructions, a so-called pure strategy. Each player has two^{13} such strategies. Most of them are poor and would never be played.

Now, there are two essential decisions which make

this game an example of a complex, interacting system. First, consider the plight of the receiver. If he decides to change, he relies on his estimate of the dealer's card. Simple probability theory handles the problem since the card was dealt at random. If he decides to stay, however, he must worry about the effect of his staying on the dealer's estimate of the value of his (the receiver's) card. But now consider the dealer's plight. He not only must worry about estimating the value of the receiver's card but also about being bluffed. So the problem is guess and second guess.

Game theory allows us to analyse the game completely. There is no good pure strategy, yet one can compute an optimal "mixed" strategy for either player, i.e., a technique of play which will maximize wins and minimize losses for the receiver and also maximize wins and minimize losses for the dealer. These strategies work regardless of what the opposing player chooses to do. They allow us to solve the problem of interaction in a rational and predictable way.

Now, what has all this got to do with the problem of complexity? It suggests, I believe, that the problem of predicting interactions between people, and between machines and people, may be attacked by new mathematical methods. At the present time, game theory has the following limitations:

(1) Large segments of human behavior are not rational. Game theory needs additional information about how to handle the concept of "value," for example.

(2) Game theory requires a very detailed description of all possible actions that can happen.

(3) Game theory is most successful in handling competitive behavior. The non zero-sum game or the game of co-operation does not lend itself to easy solution by game theoretical procedures.

As a way of studying complex human behavior, game theory suffers from having encompassed only part of the problem, namely, the rational competitive part.

Incidentally, if anyone is interested in the game theory solution to the game of "Her," here it is. The appropriate mixed strategy for the dealer is to hold when his card is 8 or over $\frac{3}{8}$ of the time, and to change 8 and under $\frac{5}{8}$ of the time. For the receiver, hold 7 and over $\frac{5}{8}$ of the time, and change 7 and under $\frac{3}{8}$ of the time. If both players actually play these strategies, the receiver can expect to average about 2 cents winnings on the hand. So if you ever indulge in practical "Her-ing," be sure to make it a fair game by passing the deal for each game, such as is done in cribbage. Actually, I do not recommend "Her" as a game. It is tedious and unprofitable unless you can find a "sucker."

In summary, then, of the contribution of mathematics to our problem we must be impressed with the contributions it is beginning to make in the social science field. Why it has not accomplished a revolution in the social sciences may be attributed to many reasons. I suspect that the developments in mathematics just described may be indications that the mathematicians are ready conceptually for the revolution to occur if, somehow or other, the appropriate kind of

information for mathematical treatment can be provided. I shall have a few things to say about this problem a little later, but let us now turn to systems analysis as the next method for dealing with complexity.

SYSTEMS ANALYSIS

It should be pointed out at the start that systems analysis is a practical combination of the method of mathematical models, the method of wisdom and the methods of common sense. A new idea has been added, however, namely, the idea of systems evaluations or the attempt to fit components into their proper perspective with the over-all goal of the system as the paramount consideration. Systems Analysis has close ties with Operations Research,[2] a scientific art-form that developed just before and during World War II. It shares with Operations Research the following description: "Operations Research deals with problems that are organization wide, and it deals with the *whole* of such problems. It may very likely concentrate study on certain parts, but it does this not because the parts are easy or attractive (which would often be the case in pure science), but because over-all examination has revealed that these parts are the critically important ones. Operations Research uses the widest possible set of tools. It may utilize such studies as would be made in time and motion analysis, but it also uses the insights of the statistician, the psychologist, the mathematician, the engineer, the physiologist, the specialist in probability theory, etc., etc. And it puts all these together, into one over-all and interrelated analysis." Systems Analysis differs from Operations Analysis

principally in time scale. Operations Research characteristically goes after immediate problems while Systems Analysis is most often interested in prediction of the behavior of systems that do not yet exist.

Systems Analysis deals with *quantitative* interrelationships between the important variables determining system performance. It relies heavily on the use of high-speed computers for playing out the consequences of interaction. We may quote from a cleared source[5] the following description of the process:

A bomber itself is a system of the elements (speed, altitude, etc.) that make up its character; it is also a component of the larger bombing-attack system consisting of bases, bombs, bomb-sights, electronic equipment, methods of employment, etc., and the men who operate them. A generalized aircraft study interrelates speed, altitude, range, payload, and weight. Such factors as the number of engines, wing area, and size of crew compartments are brought in as a basis for calculations of vulnerability and reflectivity (echoing area of aircraft with respect to radar impulses). If it is assumed that the bomb-sight has a certain probable aiming error, then the study of the damage a bomb can do to a target is figured in terms (among others) of this probable aiming error. Calculations of this kind are made as component studies by specialists.

When all this component material has been collected and appraised, a vast sorting process begins. Large numbers of weapon combinations and weapon designs are studied to permit selection of the preferred ones. Then comes the big job—the systematic combining of all the components under a cost limitation.

The attempt to find the best combination of characteristics at the same time, rather than the best individual

characteristics, is nothing more or less than scientific method applied to problems of decision.

Certainly we can agree that this somewhat journalistic description begins to sound like an approach to the problem of complexity. Many special methods find their place in this over-all technique but, Systems Analysis deals primarily with the prediction of *physical* characteristics of weapon systems. What happens when we go to the real-life situation of the marriage of man and machine in systems? How do the human components interact with the machine components? Which components of man-machine system should be man and which should be machine? When cost is introduced as a constraint, how far in the direction of mechanization of systems should we go? These latter questions depend for their answer on studies of man-machine interaction which have never been carried out. We shall consider some of the reasons why they have not been done in the next section, in which I wish to describe the final method to be considered, namely, a method with the unpronounceable name of Systems Snythesis.

SYSTEMS SYNTHESIS

In order to save myself from this tongue twister, I shall refer to it hereafter as S.S. It is the man-machine and man-man counterpart of systems analysis as described above for the prediction of preferred hardware combinations. Right now it has the status of a glint in the eye of a team of psychologists, mathematicians, and physicists, of which I am a member, who are attempting to work out a feasible procedure

for getting the basic data upon which S.S. can proceed.

S.S. turns out to be, first of all, a laboratory-computer-method, requiring rather extensive laboratory facilities, including the availability of high-speed computers. I am happy to say that we opened the doors of a Systems Research Laboratory to subjects two weeks ago and the first of a group of man-machine system problems is underway. The control of input is made possible through the use of IBM computers which allow us to present complex "real" problems to the system. The system itself is a low order abstraction of a military problem. It involves the interactions between a group of 28 people, associated machines and communications network working against a systems criterion. Since this is a classified military system, I cannot go into further details at this time.

Let us now return to mathematical models. In a sense, the Systems Research Laboratory is a large computing device for grinding out the interactions of machines and people. One of the primary goals of the research program, however, is to discover how to construct mathematical models for this interaction. The mathematician in S.S. becomes an integral part of the data-gathering team. He discovers by actual experience whether or not his assumptions about behavior are credible and consistent with reality. The psychologist, on the other hand, in joining this partnership agrees to attempt to identify and measure variables at a level of abstraction appropriate for mathematical synthesis.

von Neumann and Morgenstern[6] have stated the mathematical program:

The importance of the social phenomena, the wealth and multiplicity of their manifestations, and the complexity of their structure are at least equal to those of physics. It is therefore to be expected—or feared—that mathematical discoveries of a stature comparable to that of calculus will be needed to produce decisive success in this field.

We believe that there should be a concurrent program in the social sciences to provide an optimum climate for the new mathematics and that Systems Research looks promising to provide such a climate.

THE FUTURE OF SYSTEMS SYNTHESIS

One should not avoid the responsibility, in a Symposium on Current Trends, for some extrapolation of the trends into the future. The solution to the problem of complexity in the social sciences via mathematics has been a "current" trend for so many years now that possibly the best extrapolation would be to agree with the von Neumann-Morgenstern suggestion concerning the necessity for new mathematical techniques before real progress can be achieved. I cannot help but think that some optimum blend of observation, controlled experimentation, abstraction and generalization will eventually work. The great leveller of theory construction is the laboratory, where concepts can be put to operational test. If a laboratory can operate with systems rather than component criterion measures, I believe that we will achieve the optimum climate for solution to our pressing problems of complexity.