R E P O R T R E S U M E S

ED 017 040 EA 000 945

KNOWLEDGE PRODUCTION AND UTILIZATION IN CONTEMPORARY ORGANIZATIONS.

BY- CARTER, LAUNOR F.

PUB DATE OCT 67

EDRS PRICE MF-$0.25 HC-$1.44 34P.

DESCRIPTORS- *EDUCATIONAL RESEARCH, *RESEARCH UTILIZATION, *EDUCATIONAL PRACTICE, ORGANIZATIONS (GROUPS), CHANGE AGENTS, DEVELOPMENT, FEDERAL AID, MILITARY SCIENCE, TECHNOLOGICAL ADVANCEMENT, DIFFUSION, VOCATIONAL REHABILITATION, EDUCATIONAL INNOVATION, *INFORMATION SYSTEMS, LIBRARIES, SOCIAL PROBLEMS, PROBLEM SOLVING, EDUCATIONAL ENGINEERING, RESEARCH AND DEVELOPMENT CENTERS,

THE UTILIZATION OF KNOWLEDGE IS ONE OF THE IMPORTANT INGREDIENTS IN COPING WITH CONTEMPORARY PROBLEMS. THIS PAPER DISCUSSES THE QUESTION OF PRIORITIES IN RESEARCH AND DEVELOPMENT AND ITS PLACE IN THE NATIONAL SCENE, DESCRIBES THREE STUDIES DEALING WITH THE PROBLEM OF COORDINATING RESEARCH, DEVELOPMENT, AND USE (PROJECT HINDSIGHT, THE TACOMA GOODWILL INDUSTRIES PROJECT, AND A TRAVELING SEMINAR AND CONFERENCE FOR THE IMPLEMENTATION OF EDUCATION INNOVATION), AND CONSIDERS VARIOUS ASPECTS OF INFORMATION TRANSFER AS A NATIONAL PROBLEM. DIRECTIONS FOR INITIATING A PROPER ROLE OF KNOWLEDGE DEVELOPMENT IN OUR CULTURE INCLUDE--(1) THE SOLUTION MUST BE SOUGHT WITHIN THE CONTEXT OF THE PROBLEM, (2) THE SOLUTION TO CONTEMPORARY SOCIAL PROBLEMS WILL BE COMPLEX AND MANY-FACETED, (3) CERTAIN CRITICAL CONDITIONS ARE ESSENTIAL FOR THE SUCCESSFUL ATTACK ON MAJOR PROBLEMS, (4) THE CONCEPT OF ASSESSMENT IS FUNDAMENTAL TO SOLVING SIGNIFICANT PROBLEMS, (5) A NEW PROFESSION OF SOCIAL ENGINEERING OR EDUCATIONAL ENGINEERING NEEDS TO BE DEVELOPED, (6) SIMPLE SOLUTIONS AND INSTANT EXPERTS ARE COUNTER-PRODUCTIVE, AND (7) A SPECIAL PROBLEM EXISTS BECAUSE OF THE NATURE OF THE GATEKEEPER IN CONTEMPORARY PROBLEM AREAS. THIS DOCUMENT IS A DRAFT OF A PAPER PRESENTED AT THE UCEA CAREER DEVELOPMENT SEMINAR, CO-SPONSORED BY THE UNIVERSITY COUNCIL FOR EDUCATIONAL ADMINISTRATION AND THE UNIVERSITY OF OREGON (17TH, PORTLAND, OREGON, OCTOBER 22-25, 1967). (HW)

DRAFT

KNOWLEDGE PRODUCTION AND UTILIZATION IN CONTEMPORARY ORGANIZATIONS

Launor F. Carter

System Development Corporation

When Dr. Carlson asked me to participate in this seminar sponsored by the University of Oregon and the University Council for Educational Administration, I was particularly pleased because the general problem of knowledge production and utilization is one of the major concerns facing the intellectual community. Not only is this a matter of investigation and discussion in education but the topic is pervasive throughout the scientific and technical world. It is particularly gratifying to be able to participate in a seminar devoted to a subject which involves so many people from education. In the long run, it is through improvements in the way we utilize new knowledge that a more fruitful culture will be developed. I have no doubt that those at institutions of higher education will play a most prominent role in bringing about this rationalization in knowledge utilization.

I. Perspective on Priorities in Research and Development

Throughout the intellectual and government community, there is a very active debate regarding the priorities in allocating this country's resources. We are supporting research and advanced development at a rate of somewhat over 15 billion dollars a year. Of this amount something in the order of 2 billion is devoted to the area of basic research. The national space budget is around 5 billion and the sums being spent by the National Institutes of Health are slightly over 1 billion dollars. While these figures are large, they should be placed in the prospective of a gross national product in the order of 800 billion dollars. The amount spent on basic science in the United States is approximately one-third of one percent of the gross national product. It is argued that this is a relatively insignificant figure and that certainly a country of our wealth can afford these expenditures in the development of new knowledge. Yet, when these small pieces are added together, they become large and significant. More and more Congress and the Executive Branch of government are asking about the proper priorities in our national expenditures in the knowledge production area.

In a most interesting letter to the editor of Science, Professor Weisskopf of the Department of Physics at MIT says:

> "The troubles of today are, to a large extent, caused by our insufficient efforts to create a society in which more people can partake in a life which is worthwhile, interesting and significant. These efforts would become senseless if we begin to sacrifice some of the most active parts of our cultural life. In these difficult days, we must, more than ever, continue to support all that is positive and valuable in our civilization." (1)

This fine statement could be the prelude to the support of almost any worthwhile effort. Interestingly, it is stated in support of an increase in the amount of funds being devoted to basic science and particularly, to the relevancy of the development of the new two hundred billion electron volt excelerator now being authorized by Congress. It will not do for us to merely cite the proportionately small cost, or the contributions to our cultural heritage, in trying to determine relative priorities in national spending. Rather, we will have to examine the various components of our total national economic budget and make a number of firm, hard and rational judgments regarding the relative amount that will be spent in the various components of the scientific and technical world. Indeed, various members of Congress have been highly critical of the Office of Science and Technology and the National Science Foundation for not having any well-stated plan with regard to priority of national spending in science and technology. NSF at the last appropriation hearing, was chided for this fact and their Office of Planning and Policy Studies has recently undertaken a serious examination of the priority problem.

But it is not just a question of the relative priorities in supporting basic and applied sciences. In addition, there is a question of the balance between the amount of money that should be spent in support of basic work, applied work and the utilization of the knowledge that is being developed. More and more we are seeing concern over the extent to which basic research has become a closed system in which new results lead to further questions, which then demand renewed or increased attention, which in turn completes the cycle of an expanding demand for research support. The promised practical utilization of results tends not to appear with the rapidity or clarity that members of the public have come to expect. It is pointed out that for a number of years the space program has been supported at the five billion dollar level. Medical research has been supported at a one billion dollar level. Atomic energy development has been supported at a several billion dollar level. What has been the result of this support when evaluated in terms of practical utilization?

This topic has been the subject of much discussion and many seminars. With greater frequency the question is being asked as to whether the rate of investment and size of investment can be justified in terms of national priorities as judged by the administration, by Congress and the public at large. Particularly germane to this point is a recent discussion in Science (2) which says:

> "Last June Lyndon Johnson wondered aloud about the payoff the public is getting from the government investment in basic biomedical research (Science, 8 July), and, since scientists are among the more insecurity-ridden wards of the federal treasury, a shrewd salesman might have prospered by offering mourning bands for lab coats. By late August, the biomedical gloom was such that NIH called in some 300 of its advisors from throughout the country to take home the message that the Administration is not disenchanted with basic research. But panic in the scientific enterprise, especially in time of tight budgets, is easier to inspire than to quell, and apparently the NIH meeting was not altogether soothing. Sensing this, Senator Fred R. Harris (D-Okla.), chairman of the Senate Government Operations Subcommittee on Government Research, decided to call a sort of summit conference on biomedical research policies. . . . If any themes emerged from among the 29 papers that were presented during the conference, they were these:

1) Federal policymakers recognize the value as well as the peculiar vulnerabilities of basic research, and they want to protect it from severe budgetary fluctuations and demands for rapid payoff.

2) However, this rationale for federal support of biomedical research is the prevention and alleviation of suffering, and, therefore, greater attention and resources must be devoted to efforts that directly help the sick.

3) Since resources cannot be obtained for investigating or exploiting every reasonable possibility in research and treatment, choices will have to be made, and these choices may involve decisions to support applied research efforts at the expense, in terms of manpower, facilities, and money, of basic research."

Technical industry, the government community and universities have taken clear note of this concern regarding the extent to which work in the basic sciences or in the applied sciences has resulted in payoff through the larger community. Many studies have investigated the way in which knowledge is transmitted from the scientific community to the technical community in the application of knowledge to new products and techniques. One of the conclusions that is slowly appearing is that outside of the particular military or space activity toward which applied studies are directed, there has been relatively little spinoff from the large sums being spent in applied military or space research. A recent study sponsored by NASA and done by the Denver Research Institute (3) was reported under the title "The Channels of Technical Acquisition in Commercial Firms and the NASA Dissemination Program." In this study 62 different firms in four industries dealing with the production of electric batteries, printing and reproduction, industrial controls, and medical electronics were studied to determine the extent to which NASA related technical developments had been available and influenced the production of products in these companies. Although there was considerable variation among the organizations studied, the major conclusion was that few, if any, of them are vigorously seeking to directly use the technical and scientific output of NASA or the other advanced technology developments being supported by the government. This study is not an isolated study. The problem is widely recognized and is forcing government support agencies to give more active concern to the dissemination of newly developed knowledge and techniques and also to examine the flow of knowledge and information in the cycle from research to development to use.

II. From Research to Development to Use — Revisited

At the February 1966 meeting of the American Education Research Association I participated in a symposium on the functions and operation of the then recently instituted program of Regional Laboratories. At that symposium I read a paper titled "From Research to Development to Use." (4) I was surprised at the wide interest shown in the paper and think it would be worthwhile to review here some of the points made in it. The original paper contains a description of three different major studies concerned with the steps leading up to the utilization of new developments. At the time the paper was written, the studies were just being completed and final reports are now available.

a. Project Hindsight

The first study has become known as Project Hindsight. (5,6) Since the Department of Defense spends about 1.5 billion dollars a year on basic research and exploratory development, it was interested in learning the extent to which this expenditure contributed to the development of new weapons systems and the value which could be placed on the improvements resulting from these developments. Some twenty different weapons systems were examined in detail to determine the various important events or specific technological development which allowed the design and production of the new weapon system. Once an event had been identified, a team of investigators visited the individuals who had been responsible for the perfection of the particular event and interviewed them intensively regarding the scientific or technical origin and the environment surrounding the development of the particular event. A summary of the data allows a number of important generalizations:

1. It was found that nine percent of the events could be classified as science events while 91 percent were classified as technology events. That is to say that in the development

of these various weapon systems, the new capabilities which allowed for the development, derived from technological studies and applications rather than from basic science itself. The authors are quick to point out that this result does not show that science is unimportant but rather it points to the time scale involved in the application of science. They say in their report:

"It is clear that, on the 50 year or more time scale, undirected science has been of immense value. Without basic physical science we could scarcely have had nuclear energy or the electrical industry or modern communications or the modern chemical industry. None of our science events could have occurred without the use of one or more of the great systematic theories -- classical mechanics, thermodynamics, electricity and magnetism, relativity and quantum mechanics. These theories also played an important role in many of the technology events. If, for example, we were to count the number of times that Newton's laws, Maxwell's equations, or Ohn's law were used in the systems we studied, the frequencies of occurance would be so high that they would completely overshadow any of the recent events we identified. But, however important science may be, we suspect its primary impact may be brought to bear not so much through the recent, random scraps of new knowledge, as it is through the organized "packed-down," thoroughly understood and carefully taught old science. This finding leads one to the almost inescapable conclusion that if a technical development is to take place and it is limited by current technology, then the way to solve the problem is to directly attack it in terms of the then known science and advanced technology rather than to hope that basic science will, in any short time period, provide the new knowledge required to lead to a successful system development.

2. Another important finding was that of the various technological events, about 95 percent were directly motivated and supported by the Department of Defense. That is to say that almost all of the events which were utilized in the development of these weapon systems were developed and refined as a direct result of the perceived need of the weapons system or similar weapons systems. Only very few events resulted from general technical developments or from technical developments outside of the weapon system area. This important finding seems to indicate that if a particular problem area is to be solved, the motivation and support must come from people working in that particular area rather than from the general hope that spinoff from other technological developments will have an important contribution to make.

3. Another important finding of Project Hindsight has to do with the time distribution of events. The time from which the development of a

particular weapons system was initiated, to the time at which the several specific events became technologically feasible, shows a very wide time range. Of the 700 events studied, the range in time was from 20 years before the weapons system was started through 10 years after it was started. Most of the events took place in a period before the weapons system was started. On the average, the time period was around 5 years but even so, many of the events were not available at the time a decision was made to proceed with the overall system and had to be perfected in parallel with the system development. On the average, the medium delay between first application and discovery of a particular science event was 9 years and for the technology events, it was 5 years. The implication of this result seems to be that there is a very considerable lag between the time knowledge or a technique is developed and the time it is applied. Also, even though all the technology is not available at the time a particular system is started, the pressure of working on the system and having schedules to meet, tends to force the development of missing events so that by and large, a successful outcome is achieved.

I do not have time to review the many detailed results of Project Hindsight but I believe that this is one of the most important studies ever undertaken of the process by which knowledge is put to use. It is perfectly clear as a result of this study that an orderly process from research to development to use is largely a myth and that in fact, there is a great deal of crossing back and forth in terms of the development cycle involved, in terms of funding and in terms of the people involved.

b. <u>A Case Study of A Successful Development Project but Unsuccessful Diffusion of the Techniques Developed</u>

Edward Glaser's Human Interaction Research Institute (7) has completed an interesting study for the Vocational Rehabilitation Administration. In this study they examined the factors which seem to have inhibited a number of vocational rehabilitation agencies from adopting the techniques and methods of a successful demonstration by the Tacoma Goodwill Industries in a project titled "The Development of an Occupational Evaluation and Training Center for the Mentally Retarded." (VRA 308) The objective of the Tacoma Project was to demonstrate the feasibility of rehabilitating severely retarded young adults to a level of sustained employment. The population consisted of young adults between 16 and 30 who had measured IQ's between 50 and 75. In addition to vocational training, the workshop emphasized training in work habits and in the various attitudinal and performance characteristics which would make these people acceptable to employers. A team consisting of a psychiatrist, a psychologist, a nurse, a social worker, and a vocational specialist worked with the individuals trying to impart the necessary skills. As a result of this effort, 63 percent of the subjects were placed in jobs, with each person remaining on the job for a minimum of 3 months. Some of the individuals were retained in sheltered workshops but many were placed in competitive employment in janitorial, domestic, factory, and farm settings. Although the original project was sponsored by federal funds, the Tacoma Goodwill organization has been able to continue this work under local auspices. This study was completed in June of 1963, and the results were communicated through formal reports to VRA and distributed to a number of rehabilitation agencies. However, despite the successful demonstration by the Tacoma Goodwill Insustries, no other organization was known to have adopted the procedures used.

Glaser and his associates studied the efficiency of various methods of communicating the results of this study. As a first step, a questionnaire was sent to 40 widely separate VRA-sponsored occupational training centers for the mentally retarded inquiring whether or not they were aware of the study and its results. Since very few knew of the study, they were sent reports and a special brochure on the study. As a second communication step, a representative of the Tacoma workshop visited a selected sample of agencies in the California area to communicate the Tacoma results to them. As a third technique, a conference and demonstration for 33 representatives of workshops was held in the state of Washington. In addition to the representatives themselves, consultants from Human Interaction Research Institute, the VRA, Tacoma Goodwill, and the University of Washington participated in a discussion of the Tacoma Goodwill project. A fourth communication method involved direct psychological consultation to the management of various workshops. It was hypothesized that when an organization becomes involved in a self-examination of its goals, opportunities, ways of operating and its problems, it would tend to seek new ways to reach those goals. If a skillful psychological consultant were available to management, it seemed probable that the organization would be led to change more rapidly. To evaluate this hypothesis, a psychological consultant was made available for 15-day long visits over a period of six months to each of five workshops.

As reported by Glaser and his associates, the major results of this investigation were as follows:

1. If promising research or demonstration findings are reported in easily readable, brief and non-technical form, and are widely distributed to potential users, the chances of their having impact and being used will be increased over reporting by a formal report.

2. If potential users of the research or demonstration attend a conference where they can discuss the innovation and see it in operation by a site visit, use of the innovative research or demonstration is significantly facilitated, especially if there also is an opportunity for the conferees to tell each other about their own innovative programs or practices.

3. If rehabilitation workers who have heard about and seen an innovative demonstration elsewhere are later visited in their own agency by a member of the demonstration project staff, that added increment of face-to-face communication to one's own premises and with one's own working group further promotes the use of the innovation.

4. Psychological consultation to management helps the organization change more rapidly and become more open to change.

c. A Traveling Seminar and Conference for the Implementation of Education Innovation

The System Development Corporation was interested in testing the feasibility of conducting traveling seminars and conferences as a technique for increasing education innovation. The U. S. Office of Education supported SDC in its traveling seminar program. This program has been described by Malcolm Richland under the title "Traveling Seminar and Conference for the Implementation of Educational Innovation." (8) While Mr. Richland authored the report, a large number of people at SDC were involved both in conducting the seminar and conference and in evaluating the results. The remainder of this section will be devoted to describing the way in which the program was conducted and some of the conclusions which can be drawn regarding its effectiveness. Much of the material is quoted or paraphrased from the report.

"The project had four major objectives as follows:

1. To conduct a survey of, and visitations to, school sites with outstanding innovations.

2. To implement and conduct a traveling seminar of some 120 educators to selected innovating school districts in four regions of the United States.

3. To conduct a conference on the problems of implementing tested innovations.

4. To perform research related to the testing of the field extension service concept in education.

"Principal activities of the project included a traveling seminar in which four groups of approximately 30 educators each, representing four regions of the United States, visited selected schools where significant innovations had been introduced and in operation for at least one year. Immediately following the seminar, a conference of tour participants was conducted at SDC on the dynamics of educational change; approximately one year later, on-site visitations to the participants' own schools were implemented.

The school visitation sites were analogous to the demonstration centers inherent in the field extension concept of the Department of Agriculture. Each tour was led by a well-known and respected educator ('outside change agent') who was accepted by his professional colleagues as being especially qualified to interpret the experimental foundations upon which a particular innovation was based, if such foundations were, in fact, offered by the innovator."

These four tour leaders were responsible for conducting the tour, were involved in the selection of the sites to be visited by the traveling seminar, and made all the arrangements for the visits to the schools, including advance briefings to the officials of the schools involved.

The schools selected for visitation were ones that showed evidence of successful implementation of various educational innovations. The emphasis was on new educational media, major changes in curriculum, innovative teaching methods, and new school organizational patterns involving the use of teachers' time and classroom space. The schools selected also represented different sizes and urban-rural characteristics in the geographic region. Each of the schools visited had at least one year's experience with the particular educational innovation involved. To give a feeling for the kinds of innovations observed, the eastern tour, visiti one school in Massachusetts and two in New York, was exposed to the following:

Continuous Progress Plan

Lay Personnel on Teaching Staff

New Vocational Training Plan for Culturally Disadvantaged Students

New Curriculum Materials

Auto-Instructional Devices for Individual Study

Flexible Scheduling

The tour participants formed a somewhat heterogeneous group. A number of studies have shown the importance of the school superintendent and the need for positive and effective leadership at this level. In addition, the representatives of the various formal echelons of education are important and their concurrence is often needed in effecting innovations. Therefore, the final composition of each tour group included 15 local administrators, 8 state education department officials, and 7 representatives from teacher training institutions. The tour itself lasted one week. Each group met on Monday of the week of May 11, 1964, were briefed by the tour leader, and then began the site visits. At the site they observed a particular innovation and discussed its advantages and problems with the teaching and administrative personnel. The team often met among themselves to discuss further the particular activity observed and then moved to the next site. The complete tour involved visiting at least three different schools in separate geographic locations.

Following the tour, the tour members came to Santa Monica for a conference on May 16 through May 19, 1964. This conference was attended by the tour leaders, the tour participants, and selected consultants and specialists from SDC. At the conference each of the tour directors gave a fairly extensive description of the innovations observed by each team, as well as a summarizing report of the problems associated with the innovations observed. In addition, there were various addresses by leaders in the field of education and people who had studied problems associated with the introduction of change within various organizations.

Although the participants in the seminar expressed great enthusiasm for the traveling seminar as a technique for observing innovations and for stimulating participants to try such innovations in their own school setting, a more careful evaluation of the results seemed desirable. This evaluation consisted of two parts. One was assessment of a large amount of anecdotal material, letters, discussions, etc. The easiest way to summarize this material, which is discussed at considerable length in the report, is to say that the participants seemed to be extremely pleased with the program, and expressed plans to attempt many innovations in their own school settings.

The second effort was to undertake a formal evaluation of the effects of the program. In this evaluation, 46 of the 60 participating school districts were used as the experimental group and 57 comparable districts formed a control group. Prior to the initiation of the tours, the superintendents of schools in both the experimental and control groups had filled out a detailed questionnaire concerning the nature of educational innovations in their districts. Approximately a year later each superintendent was visited, and participated in a structured interview regarding the school district and its innovations. Following the interview, the questionnaire and interview material were assessed by SDC staff personnel, and degree of innovation was scaled on a 0 to 4 scale. Participating districts had a higher innovation score than did the nonparticipating districts. This change score was evaluated by analysis of covariance with the results being significant at past the .01 level of confidence.

The three studies reported in this section have each contained many findings and recommendations. Although they come from different fields (the military, the welfare field and education) their conclusions and results have a common core of implication for knowledge dissemination and the utilization of research. These broader implications will be considered in the final section of this paper.

III. Information Transfer as a National Problem

There has been increasing concern regarding the formal aspects of the information transfer problem. The results of basic and applied research and technological innovation are reported in numerous documents, journal articles, government reports, books, etc. The number of these and the difficulties in making them available for use has been increasing for a number of years. This has been recognized at the Federal level by a number of agencies. The National Science Foundation has established an Office of Science Information Services which has associated with it a Science Information Council of which I am a member. Also, the Federal Council for Science and Technology has established a committee known as COSATI, the Committee on Scientific and Technical Information. Finally, within the last year, the President has appointed a National Advisory Commission on Libraries.

Two years ago I was fortunate enough to head an SDC study team which had been commissioned by COSATI to undertake a study of the national problems in scientific and technical document handling. The results of the study have recently been reported in a book authored by those of us participating in the study. (9) In the short time available, I can do no more than to give a very quick synopsis of the book and hope that those interested in the total national scientific and technical document handling problem will be stimulated to read the total book. The first part of the book describes the present document handling system. There are chapters on document handling institutions, on the process of document flow and on document users. Another section is devoted to a statement of some of the fundamental problems in document handling and the formulation of basic propositions regarding Federal responsibility in this area. The next section develops various alternative approaches to solving the problems set forth in previous chapters and the final section evaluates the various alternatives and makes prognoses regarding future actions.

It is argued in the book that information is one of our most precious national resources. The information problem is much more than the local annoyances, inconveniences and dissatisfactions with document information systems. It is argued that a natural resource such as knowledge and information is something with which the Federal government must be vitally concerned and that it needs to guide the overall development and conservation of such an asset. In view of this perspective, the various problems currently facing the national document handling system are reviewed. Among the problems discussed are:

1. There is a need for the adoption of a fundamental statement of policy on the part of the Federal government. It is suggested

as a fundamental proposition that the Federal government has the responsibility to assure that there exists within the United States at least one accessible copy of each significant publication of the world-wide scientific and technical literature.

2. There is a great increase taking place in the number of users and user requirements. It has been estimated, for example, that there will be about a fifty percent increase in the number of acientists and technologists in the next five years. It is estimated that there will be four million scientists and technologists by 1970 at which time they will represent 4.7 percent of the total work force.

3. A serious problem is the fact that the number of documents is increasing very rapidly. The number of books, journals, etc. doubles almost every 15 years. For example, it is estimated that in 1961 there were 658,000 technical documents published and that by 1970 this number will grow to 1,143,000.

4. Another problem is that the present system for handling formal documents is in serious trouble in its effort to render quality service. There are a number of evidences of this difficulty. For instance, the Library of Congress is having to greatly increase its bibliographic service to libraries but even so, only 50 percent of the various card catalogues required are available to major research libraries. Some libraries have large backlogs of documents and books which they are unable to process into their collections. Although

libraries are desirous of giving service to all legitimate users, many of the nation's libraries are adopting restrictive policies regarding the services they render. The amount of trained manpower in the library field is far short of the demand and is not growing at as fast a rate as the growth of the general professional work force. Likewise, the budgetary situation for most research libraries is critical. Public libraries and school libraries are curtailing services and stinting on staff because they cannot raise the money to maintain a desirable level of service.

5. Libraries have been very slow to adopt modern technology and computer techniques.

6. At present, the system of document handling institutions is composed of many independent units within the government, at universities, in professional societies as private efforts and in industry. Each of these units has tended to go their separate way in terms of local plans and resources. The need for an integrated long-range plan has only very recently been recognized and hopefully will be one of the outcomes of the recommendations of the National Advisory Commission on Libraries.

In view of the many problems just summarized, the study team reviewed the various plans which had previously been proposed for national document handling systems. Three new major organizational concepts were developed and evaluated at considerable length. One of these involved establishing within the Executive Branch of the government a capping agency which would set general policy and monitor the performance of various responsible agents, agents who would be directly involved in the operation of the many facets of a national scientific and technical document handling system.

As a result of the COSATI study and studies undertaken by the library community and other portions of the Federal government, the President, in January, 1967, appointed a National Advisory Commission on Libraries. As a member of the Commission, I have been privileged to meet with the other members of the Commission, who represent a very broad spectrum of those concerned with the library and information transfer problem. There are representatives from major universities, from research libraries, from state libraries, from public libraries, from school libraries, from law libraries and medical libraries. In addition, there are representatives from major learned societies, and from the lay public interested in library problems. In addition, a former Congressman who was instrumental in the passage of the Library Construction and Services Act is a member. The Commission's report is to be made to the President in January of 1968 and thus, it would be inappropriate to discuss possible recommendations at this time. I can say, however, that the Commission has made an effort to tap all available sources of information. It has heard representatives from all the major professional associations concerned with libraries and document handling. It has visited some of the nation's leading libraries and has held hearings in a broad sample of localities throughout the nation. We are just now in the process of formulating our recommendations.

Even though many groups are working on the problem facing the formal information transfer mechanisms, it seems probable that even if they were successful, we would still be faced with serious difficulty in implementing the knowledge which has been gained. Frequently, the knowledge available in reports is not easily translatable into practical application. Often the carefully reported results are so narrowly restrained or so confined to the laboratory setting that their implications for real problems are, at best, tenuous. While it is important to make the knowledge we have available to potential users, we need to recognize that the solution of the pressing problem of our complex culture will require much more than the intelligent application of the information and knowledge we now have available.

IV. Using Knowledge in Attacking Major Contemporary Problems

In this paper we first discussed the question of priorities in research and development and its place in the national scene. Second, we described three studies dealing with the problem of research to development to use. Third, we considered various aspects of information transfer as a national problem. Now, I wish to draw together these separate sections and to consider some new material which should give insight into the ways in which knowledge can be used in attacking some of the major contemporary problems facing our civilization. Many will not agree with the comments I am about to make. I hope that by stating some fairly dogmatic positions, I can stimulate discussion of these important problems and help those who disagree with the positions I have taken to examine the basis for their position. Thus, we can come to some agreed upon conclusions or directions for solution of the proper role of knowledge development in our culture. The points I wish to emphasize are:

A. Seek the Solution Within the Context of the Problem.

By this I mean that if there is a major problem area which needs attacking, then the solution should be sought by work within the context of the problem area itself rather than hoping that knowledge developed in basic research or in other applied areas will have great application to the particular problem needing solution. This conclusion tends to place basic scientific research in a less central position than is often done in discussing ways of solving major problems. It is my impression that the results of a number of studies lead to the conclusion that basic research and scientific theory is a fundamental ingredient to solving problems, but that the knowledge derived from basic research tends to be too general to guide the way for the solution of specific contemporary problems. The results previously cited from Project Hindsight support this view. Recently, a study has been completed by Mackie and Christensen titled "Translation and Application of Psychological Research." (10) This study was undertaken to describe the processes involved in translating the results of laboratory research in psychology into forms that would be meaningful and useful in operational settings. Because of its obvious importance, the investigation was concentrated on experimental studies of the learning process. In carrying out their investigation, selected studies of human learning were analyzed in detail and their findings were reviewed for possible practical application in Navy training. Also, the apparent impact of the findings of these studies on actual Navy training personnel and training practices were studied. Additionally, a number of well-known psychologists in the field of learning, in educational psychology and in positions of responsibility for training research were interviewed on issues that were considered vital to the translatibility and applicability of research results. In reporting their findings, Christensen and Mackie say:

> "It was found that the research-to-application process never has properly developed for the psychology of learning. Consequently, there have been far fewer applications and much less impact on the educational process than might reasonably be expected in view of the size of the learning research effort. The reasons are believed traceable, in large part, to the research philosophies of experimental psychologists. But it was evident, also, that potential users have been reluctant to make the effort necessary to realize the benefits of research findings......"

"Research on learning processes represents, perhaps, the largest single area of investigation presently being pursued by experimental psychologists. Although this has been true for some time, there has been no systematic effort directed toward practical application of the findings from learning research. As a consequence, modern learning research is producing very little impact on educational technology or training practice."

Some will think that the above quotation represents too harsh an evaluation of the results of years of experimentation in the psychology of learning. One can speculate what conclusion would be drawn from a similar study from various other fields in psychology and education. I suspect that a careful examination would show that much of the research done in these areas has resulted in only fairly limited application in real life stiuations. It seems probable that the recognition of this fact was an important stimulus to the U. S. Office of Education in establishing the research and development laboratories and the regional laboratories. It is my belief that a successful program in the area of education will only result from very extensive and lengthy work on the part of these research and development agencies as they are intimately associated with actual school experience in real-life school situations.

B. The solution to contemporary social problems will be complex and many faceted.

Simple solutions are extremely unlikely. If there were simple solutions to the various problems we are facing today, the problems would have ceased to exist long ago. Rather, these problems persist in spite of the efforts to apply common sense and straightforward approaches. All our experience shows that the solution to major system problems involves the application of many different developments and their integration into a concentrated attack on the problem. Again, one can cite the Hindsight experience where it was shown that the development of a major new weapons system depended on the solution to a large number of relatively well defined, small, but critical problems. Similar results can be cited from other fields. One of the great successes in America has been the revolution in agriculture. Recently, Sprague (11) has reviewed the conditions necessary for agricultural production in the developing countries. He emphasizes the many factors which are essential for the successful introduction of high-yield crops. After reviewing the increase in rice production in Japan, he says: "As is typically the case, this increase in yield is the result of many factors: improvement in varieties, increased use of fertilizer, modification of cultural and production practices, and better control of disease, insect pests and weeds."

C. There are certain critical conditions which are essential for the successful attack on any major problems.

Prominent among these critical conditions are: First, there must be an appropriate acceptance and motivation on the part of the community, the government and other involved agencies in recognizing the need for a concentrated effort toward solving the problem under consideration. Second, there must be a trained, motivated and experienced staff available for long-term application to the problem. Generally, the problem will not be solved in any short period of time and those responsible must recognize that the same staff must be maintained over a number of years if the problem is to receive real attention and solution. Third, funding must be available not only to support the staff but often to make many physical and organizational changes within the setting in which the problem exists.

D. The concept of assessment is fundamental to solving significant problems.

It is surprising how frequently we resist the idea of assessment. We will deplore some existing condition or state that a serious problem exists without being willing to undertake the necessary effort or even to recognize the necessity for a quantitative assessment of the existing situation. If efforts are made to ameliorate the problem, it is important that the success of these efforts be assessed by rigorous and objective techniques. In the area of weapons system development specifications are worked out in great detail which define the various parameters which must be satisfied before the weapons system will be considered satisfactory. The developer and the military services arrived at clear understandings as to the way in which the weapons system will be evaluated. At times, almost as much money is spent in evaluating and assessing the weapons system as went into its original development. Frequently, modifications and continued development are required if deficiencies in the original design are demonstrated during the assessment phase. Similarly with regard to contemporary problems in the social area and in education, we should not be satisfied with introducing ameliorative efforts unless we are willing to undergo the stringent test of objective assessment so that an evaluation of the effectiveness of new methods can be made and cost/effectiveness estimates derived.

E. A new profession of social engineering or educational engineering needs to be developed.

In evaluating contemporary problems in education and the social area generally, it seems as though there is a wide separation between the practitioners in these fields and those engaged in research in our academic institutions. We seem not to have the middle man who, as in the case of the engineer, is devoted to solving specific problems. The engineer, based on accumulated past experience in technology, and on general principals derived from basic science, take this knowledge and tries to apply it in the solution of problems. His orientation is neither towards the development of basic new science nor the operation of a particular system but rather that of the designer, architect and introducer of the new system. We do not have such people in the education and social field. The universities and government must take the iniative towards defining this new profession and training the people who will become its practitioners. Since contemporary social problems largely arise in the sector where government is primarily involved, that is to say, problems in education or in urban development or in environmental control, where there is a clear recognition of government responsibility, it seems apparent that the social engineer will need to be trained to serve within a government-oriented context. Thus, the government, if we wish it to deal adequately with these problems, will need to encourage over a long period of time the training and employment of people in this new profession.

F. Simple solutions and instant experts are counter-productive.

It is my impression that there are still a large number of well-educated people who feel that somehow a simple solution can be found to most of our problems. Often these same people seem to feel that if a good sensible person would just look into the problem for a short period, he would be able to perceive what needs to be done. A very interesting example of this phenomenon is the number of people who believe they are experts in the area of education and believe they know what should be done. In a recent issue of the New Republic, Joseph Alsop (12) authored an article titled "No More Nonsense About Ghetto Education." On the basis of his short acquaintance with this subject, Alsop seemed to feel that "brilliant Negro achievement" could be achieved if the education world would only adopt

New York City's "More Effective Schools" program. For those who are unfamiliar with educational developments, Alsop's article probably carried great conviction and no doubt, led many to believe that here we had an example of the expert coming up with a sound solution. It was with real pleasure that I read a reply by Schwartz, Pettigrew, and Smith (13) in a subsequent issue of the New Republic titled "Fake Panacea for Ghetto Education." These Harvard educators were able to show the misinformation contained in Alsop's article, his rejection of much pertinent information and his relative ignorance of developments in the problems of ghetto education. Yet I venture that Alsop, because of his wide reputation as a syndicated columnist, has influenced many more people than the reply by the group of experts in the subject. One long range approach to this problem suggests that educators in these institutions of higher education have a special responsibility to transmit an understanding of our contemporary problems in such a way as to insure that college graduates are reasonably immune to the idea that simple common sense solutions are the answers to most of our contemporary problems.

G. A special problem exists because of the nature of the gatekeeper in contemporary problem areas.

By the gatekeeper I have in mind the idea that there are many individuals and organizations who are critical to the solution of contemporary problems because of their strategic location in approving or disapproving particular efforts to ameliorate some of our contemporary problems. I have in mind such gatekeepers as school boards, legislatures, city councils, commissions, etc. In the development of weapons systems, we have quite clearly defined gatekeepers. One of Mr. McNamara's great achievements has been his ability to establish responsibility within the military services for clear decision-making and clear lines of authority as to whether or not a particular weapons system will be developed. Once the decision has been made to proceed with the development of a weapons system, the necessary budgeting, development plan, personnel allocation, industrial contracts, etc. follow. In these developments, the location of the gatekeeper is clear but more importantly, the gatekeeper has a professional expertise in the subject about which decisions are being made. This may be a military professional background, a highly technical engineering or science background or other background which is appropriate to the particular problem. In marked contrast, we often find the situation in contemporary educational and social problems where the gatekeeper is not well defined. It is unclear as to exactly what body or institution is responsible for making a decision. Likewise, one often finds that the person filling the gatekeeper role does not have the technical or expert knowledge necessary to make the decision. We tend too frequently to find the gatekeeper in the education and social area occupying his position because of ability to win elections, general social affability, or business interest rather than a trained professional expertise in the problem under consideration. I do not suggest any simple solution to this problem but as time goes on we will have to try to better educate or to change the role of these gatekeepers.

In conclusion, then, I would suggest that this seminar serves a most useful purpose in focusing the highly important task of developing strategies for solving the many contemporary problems which our nation faces. It seems apparent that the utilization of knowledge is one of the important ingredients in coping with contemporary problems, but much more is involved. The whole problem of a strategy for change and the method of bringing together the necessary resources deserves our most serious attention.

DRAFT

REFERENCES

1. Weisskopf, V. F. Relevance of Science during Times of Social Stress. Science, 157, 873, (1967)

2. Greenberg, D. S. Biomedical Policy: LBJ's Query Leads to an Illuminating Conference. Science, 154, 618-620 (1967)

3. Greenberg, D. S. Civilian Technology: NASA Study Finds Little "Spin-off." Science, 157, 1016-1018, (1967)

4. Carter, L. F. From Research to Development to Use. SP-2332, System Development Corporation, Santa Monica, 1966. (Also in: Technology in Education. Hearings before the Subcommittee on Economic Progress of the Joint Economic Committee Eighty-ninth Congress, Second Session U. S. Government Printing Office 1966)

5. Sherwin, C. W. and Isenson, R. S. "First Interim Report on Project Hindsight." Summary, 30 June 1966; revised 13 October 1966, No. AD 642-400, Clearinghouse for Federal, Scientific and Technical Information, Springfield, Va.

6. Sherwin, C. W. and Isenson, R. S. Project Hindsight. Science, 156, 1571-1577, (1967)

7. Glaser, E. M. Utilization of Applicable Research and Demonstration Results. Human Interaction Research Institute, Los Angeles, California, 1967

8. Richland, Malcolm. Traveling Seminar and Conference for the Implementation of Educational Innovations, TM-2691. System Development Corporation, Santa Monica, California, 1965

9. Carter, L. F., et. al. National Document-handling Systems for Science and Technology. John Wiley, New York, 1967

10. Mackie, R. R. and Christensen, P. R. Translation and Application of Psychological Research. Technical Report 716-1. Human Factors Research, Inc., Santa Barbara Research Park, Goleta, California, 1967

11. Sprague, G. F. Agricultural Production in Developing Countries. Science, 157, 774-778, 1967

12. Alsop, J. No More Nonsense About Ghetto Education. New Republic, 157, 18-23, July 22, 1967

13. Schwartz, R., Pettigrew, T., & Smith, M. Fake Panaceas for Ghetto Education, New Republic, 157, 16-19, September 23, 1967