NSR-08-001-010

THE ADMINISTRATION OF RESEARCH

An Interpretive Summary of the

Proceedings of the National Conference

on the Administration of Research,

1947 - 1964

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1966

DRAFT

PREFACE

This book is the product of many minds--not just those whose names the reader finds on the cover or credited in this Preface--not even of those whose names are mentioned in the text. In addition, many more leaders of science, technology, academic and public affairs have, at least in an indirect sense, contributed their views and experience to this text on administering research. The authors claim to have added little, for our task has been to digest and summarize what others have said on this subject during a period of the past eighteen years.

This book presents the results of a systematic attempt to examine, interpret and summarize the content of the Proceedings of the first eighteen National Conferences on the Administration of Research. The aim has been to place in a practicable form a distillation of the existing body of knowledge in the field of research management and administration, at least to the extent it has been presented and discussed at the annual Conferences. Dr. Robert Buchheim, Program Chairman of the Nineteenth Conference, aptly stated our aim in 1965:

> The objective of the effort is to review these Proceedings and, in light of the other existing literature, prepare a systematic statement of their contents, with particular emphasis on the emerging features of research management as a profession.

Our motivation has been spurred by realization that these Conference Proceedings are not readily accessible to many research leaders and those who aspire to become leaders. The invitational nature of the annual National Conference has resulted in very few persons possessing copies of all the Proceedings, especially the earlier ones. Over the years there have been substantial changes in research leadership and thus in Conference participants. Few persons, other than Conference attendees, secured the earlier Proceedings and they are long out of print. Thus much valuable information contributed by many leaders on tested practices in research administration has not been readily available in the literature on this subject. Our hope is to make it so and to shape, codify and index it as a resource for the professional research administrator or for the interested scholar.

The present effort is confined to the content of these Proceedings, starting from the first Conference in 1947. We have reached outside only where necessary to provide adequate and proper interpretation to the context, language terms and definitions used by the various speakers and discussants. Thus it has not been our intent to provide a balanced text on management of research and development. Nor has it been our intent to fulfill the desire of some readers for detailed treatment of given subjects in this field. However, along with those who have helped in our effort, we do believe that over the years the Conference has included at least some discussion of every important and recognized problem area in the field.

Although the literature underlying this summary is limited, perhaps it is well to take note of what some others have said about the "science" and practice of management and administration, especially as it applies to managing science and engineering efforts. Vannevar Bush pointed out recently that the practice, or "technique", of management can be learned from books and can be taught but the art of management must be learned from life, and the art is far more important than the technique. Edward Litchfield has long been an exponent of the theory that the "administrative process", as he terms it, is a distinguishable craft, or science, regardless of what is being administered. He regards its basic principles to be equally applicable to a corporation, a university, a government, or any other organized human activity. This book exemplifies these two concepts. For the most part it deals with the art and administrative process of research management taken from life and as practiced in organized research activities. Its intent is to identify and point up how the administration of research process differs from other administrative processes and, as well, differs between various types of organized research activity.

Although the Proceedings contain some discussion of research administration practices in foreign countries, no attempt is made in this text to point out variances between United States and these foreign practices. Although some differences arise from variations in social, political or economic syetems, the basic methods of management remain very much the same across national boundaries and the differences appear not to be profound.

Some parts of the Conference Proceedings (banquet speeches, luncheon talks and the like) were directed at important topics of public concern, which may or may not have contributed to the concrete problems of administering research. To the extent that these discussions are outside the immediate sphere of research administration they have not been included in this summary.

In addition to being grateful to the Conference Committee for the Nineteenth National Conference and its Program Chairman, Dr. Buchheim, for their confidence and encouragement, we are deeply indebted to and wish to acknowledge the help and support of others. Foremost among these is the National Aeronautics and Space Administration and its administrator, the Honorable James E. Webb, to whom we owe much for both financial and moral support. We are also in the debt of Dr. Helmut Wakeham of Philip Morris and Messrs. Albert Siepert and James Mahoney of NASA for their criticism and injection of interpretive viewpoints of industry and government during the period of preparation of this summary.

Finally we wish to acknowledge with our gratitude the help of Mrs. Patricia Angel who contributed much by her research and editorial assistance on the project.

Without the encouragement and assistance of these persons, and of others who helped in typing and editing the manuscript, we could not have hoped to produce a truly objective and interpretive product from our efforts.

L. B. W.

S. E. C.

Newark, Delaware, August 1966

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CHAPTER I

INTRODUCTION

By the close of the year 1964, the annual National Conference on the Administration of Research had convened for eighteen consecutive years and published each year a careful record of its papers, addresses, and discussions in the Conference Proceedings. This Conference has the purpose of providing an active forum for individuals who occupy top leadership positions in the management of scientific and engineering research in industry, government and the universities. It acts as a direct means for the informal relation and interchange of ideas and tested policies, procedures, and practices in this field.

The Conference was initiated as a single conference on the administration of research shortly after World War II by a small group of research directors who saw the need to exchange ideas arising from their wartime experiences, wherein as responsible managers of large research groups they were lacking a set of established guiding principles for their tasks. The first conference at Pennsylvania State University in 1947 clearly indicated a need to continue periodic discussion and from that sprang the "Annual Conference on the Administration of Research", later changed to the "National Conference".

The NCAR is an informal organization having no charter, by-laws, members or dues. It has no home ground but each year is hosted by a

University at or near its campus. It is administered under a set of "Ground Rules" by a Conference Committee of 25, equally divided between the three participating sectors of industry, government and universities. Five Committee members are newly elected each year by the then present Committee for five year terms. For arranging each specific Conference, Executive and Program Committees are selected. The Executive Committee consists of the Chairman, Chairman Elect, Past Chairman, Host Institution Representative, Secretary-Treasurer, Program Chairman and Member-at-Large. Participation in the National Conference is by invitation only and an average of about 200 invitees (in approximately equal numbers from industry, government and university spheres) attend the three-day sessions each year.

Over the years, the list of invitees and participants has changed gradually but markedly, and the program content has ranged over a number of traditional topics in research administration, as well as taking up new experiences. Thus the collective Proceedings have come to be a major resource of knowledge in this new and gradually-developing professional field. To distill and present this body of knowledge in a succinct manner is the objective of this summary.

Any interpretive summary of such a distributed body of knowledge is subject to certain limitations. The nature of the Conference itself makes inherent some of these limitations, for the Conference is a kind of oligarchy in which certain leaders in the direction of research, the newly elected and appointed Executive and Program Committees, each year decide the content of the next year's program, based upon their own and collected opinions about what is pertinent to discuss next time the Conference convenes. Thus, what problems were seemingly important in 1947 may not seem so relevant in 1957 or 1964 and conversely, the obscure reportings of one early year in the Proceedings may well have considerable significance today.

In their accepted task of interpretation the authors, with the fortunate hindsight of today, have tried faithfully to distill the best of thought and fact on what seem to be continuing problems in the field. Pertinent, older-recognized problems in research administration never really disappear but new ones become apparent. A good example is the question of ethical behavior of technologists and technical managers, which was not recognized as a problem meriting discussion until 1962.

The fact that some certain knowledge about the professional aspects of research administration has been produced and recorded in these Proceedings, as a result of both academic study and hard-won experience, serves to justify the Conference on these grounds alone. Yet no one, even today, can take refuge in the belief that all the important problems and topics have been thoroughly aired in this particular Conference or, for that matter, by any other means. This field is developing and emerging as a profession. To illustrate the importance that it do so was well stated by Oliver G. Haywood, outstanding government and industrial administrator, in the opening remarks of the Twelfth Conference (1958). In the wake of the Sputnik launching Haywood was soberly reflecting upon where we stand in our capability to effectively administer our technical efforts.

In doing so, he asked whether the basic fault in our apparent failure of leadership in leading the Soviets did not lie in those leading our research operations. He put it this way:

> Where have we failed to maintain our once unquestioned supremacy in technology? Certainly not in our factories. And our engineers and research scientists are as well trained, as well equipped and as dedicated as any others. Our failure has been in the sphere of primary interest of this conference. It has been in the administration of research--in the establishment of the philosophies and mechanics of management within government and industry conducive to significant research.

Haywood's remarks point up the significance of efficient use of our technical and scientific resources and would hold for our well-being whether or not we have outside competition, military or peaceful.

Though it was never a specific intent of the Conference to discuss on a balanced basis all problems conceived to be important in administering research, it is clear that all major ones so far identified have been the subject of one or more of the Conference programs and, therefore, discussed to some extent. Where it is clear that a given topic or problem needs further discussion or resolution the authors have attempted to point up this need as a suggestion for future Conference discussion.

A further limitation on the basic material flowing from these Conferences is semantic in nature. This is not unexpected of any new professional field, for it takes much time and much discourse, even on fundamental matters, to come to agreement on the meanings and definitions of terms. No less true has this been in research and research administration. In reviewing the semantic problems involved, the authors can claim no heaven-sent gift for establishing once and for all the semantic terms to be used. Rather what can be done is to distill out of the many definitions and meanings professed what seems the best consensus as reflected by the Proceedings. At bottom this is a task which, if for no other reason, must be done in order that the reader be oriented and tuned to understanding the discussions and summaries presented later in this text.

Both by design and because of the desire of the person speaking to communicate, much Conference material has been devoted to trying to define and separate terms such as basic research, pure research, fundamental research, exploratory research, applied research, exploratory development, design, invention, etc.

In talking about these terms all discussants agree that we are talking about the efforts of creative individuals: scientists, engineers, and those who lead them. All agree that in general, research and development seeks new knowledge, new understanding, or both. And all agree that research and development effort is more a continous spectrum of activities rather than a series of separate and distinct entities. This one fact in itself is the root of much of the semantic difficulty. People, especially technologists, like orderly and finite packages that can be separated and discretely described. They would like to have "basic research" or "applied research" as uniquely defined as is a natural constant such as the speed of light. Many would like to think of basic research as being "pure," having no objectives in view, other than to increase knowledge and understanding, and motivated only by interest and curiosity.

Also they wish to think of applied research, development, etc., as having planned practical objectives and, often, specified needs--but others say that basic research <u>can</u> be motivated by objectives, and it can be planned, programmed and directed. The recurrent retort is, of course, that <u>if</u> this be the case, then we are talking about <u>applied</u> research--not <u>basic</u>.

Various thinkers about the subject have wanted to categorize research and development by looking at the <u>content</u> and judging (by some standard never well established) whether it is basic or applied in nature. But others contend this is impractical and say one must look at the man performing the work to determine whether he is motivated by practical objectives or not. These two opposing views were espoused by various participants at the many Conferences. To show the disparity in thought it is perhaps well to note that in 1952 a panel of Conference attendees, led by E. R. Piore, tried unsuccessfully to define basic research. Piore suggested that the <u>content</u> of the research might be examined to determine whether it is fundamental. This means, perhaps that a primary quality of basic research is that it seeks better understanding of the external world. He also thought a secondary guide to be whether it would inspire other research that would permit a better understanding and <u>control</u> of the external world. Presumably, the latter would be tagged "applied."

But in 1957, DeWitt Stetten, Jr. and Willard Libby were in agreement on a different approach. Stetten expressed the view that the chief difference is that the applied scientist envisions at the outset the application to be made but the basic scientist is motivated largely by curiosity. Libby, while generally agreeing, put it a bit differently. He felt that a simple definition is: "Basic research tells one something new about nature, whereas development work in itself does not." Libby further expressed that the easiest way to tell them apart is on the basis of objectives and purposes; a second way is to look at the type of man doing the work. Thus we come almost full circle.

Others, at various times and places, have used other adjectival phraseology to describe what most have come to accept as basic research. In discussing what he called "industrial research" (research by industry), George Glockler (1958) emphasized that the phrase "research spectrum" is very appropriate. Drawing from his remarks and from others', one can list at least the following adjectives to precede the word, research--on one end of the spectrum: pure, basic, fundamental, exploratory, creative, all of which are summable into the word "basic"; on the other end, the adjectives best describing the domain outside basic research are: applied, programmatic, supporting, corrobative, collaborative, contributory, directed, engineering and technological, all of which can be summed into "applied research" and "technological development."

Though many words have been used to try to define these categories we are yet without generally accepted terms. For instance, Ralph Morgen has pointed out that applied research to the scientist may well be basic research to the engineer, and this explanation seems quite appropriate.

Similarly, but to a lesser degree, other generic terms which have become the so-called vocabulary of research and development and its management still mean quite different things to different people. Nevertheless, it may still be important to try to clarify for the reader what research administration is and is not.

In opening the 1957 Conference, Ralph E. Gibson, venerable leader of university and government research, summarized the objectives of research and development and defined research administration in this way:

> The end or objective of our activity is two-fold: (1) to develop devices, commodities, and services which will enable us to lead more healthy, comfortable, secure, and useful lives, and (2) to develop patterns of understanding of man and his environment so that we may mobilize our knowledge for useful applications when the occasion demands. The primary means for achieving this end are well-educated, skilled, industrious, intelligent, imaginative, creative men and women working under a vigorous intellectual discipline. Research administration consists of selecting people endowed with the foregoing general attributes, placing before them challenging problems appropriate to the exercise of the particular set of attributes and education they have, and supplying an environment where intellectual discipline and inspiration enable them to exercise their faculties to the fullest extent.

Gibson's description says rather well <u>what</u> research administration is. He leaves it to others to provide the clues as to <u>how</u> to perform it. This the other speakers and conferees have contributed throughout the Conference sessions, for this is the purpose of the Conference. As stated by Rensis Likert (1956) this knowledge or art comes through a combination of experience and scientific study, much like the art of the practice of medicine.

But a definition at times can be too encompassing for effective use. The insight gained from an opposite or indirect approach often supplies the necessary limits. It is clear that research administration as a general term does include all that assortment of activities which lead to Gibson's objective. For the purposes of the National Conference, however, and, therefore, for this interpretive summary of the Proceedings, the term "administration of research" includes only those responsibilities, duties and obligations of the overall manager of the people doing the research or research and development and of the services which support their efforts. The distinction between managing research and development efforts and administering their supporting services was more clearly appreciated by participants in later Conferences. For the most part, they limited discussion to the subject of managing overall operations and relied on other specialized media to cover the subjects of supporting services, which appropriately include a separate set of skills, both professional and non-professional. For example, it is rather obvious that accounting and skilled accountants are needed in the overall organization for research but no attempt has been made by the National Conference to look into the details of research accounting methods, nor to develop research accountants.

In this text we have chosen to apply the term "research director" to the person in overall managerial charge of research or research and development efforts. In this sense he is either a part of the top management team of the company or organization, of which the research and development effort is a part, or he is in charge of a similar but independent effort. He may also be the leader of a major segment of the total research and development effort. In any case, the research director is a leader who is faced with the opportunities and problems involved in reaching the objectives stated by Gibson.

Also it should here be noted that the word "research" as used in the name of the Conference, as well as wherever used throughout the Proceedings and this summary, is intended to cover the entire spectrum of research and development activity without exclusion of any part of it. Where specific reference to only a part of this spectrum is intended, a designation such as "applied" or "basic" is used as a rough pointer to the region of the spectrum involved. In addition, since the term "R & D" has also become a commonplace expression to designate the whole spectrum, it will be used throughout the text in that sense.

CHAPTER II

ROLES OF RESEARCH ORGANIZATIONS

Introduction

In their preliminary deliberations about holding a conference of research administrators, the founders of the National Conference recognized essentially three types of organizations which had traditionally performed or supported research and which had experienced similar problems in administering this work, despite their differing aims and purposes. The three sectors were universities, industry and government. It seemed worthwhile to convene a conference of research administrators from these sectors because there appeared to be sufficient problems of administration common to all three to warrant their gathering to discuss the problems from various viewpoints. The motivation behind convening the First Conference, perhaps best expressed by a leading research consultant, Maurice Holland, was "to exchange tested practices" between the three sectors of the national research community. The founders felt that a single meeting, without a planned continuity, would serve to condense the best experience of the wartime research administrators and to distribute the tested techniques of one sector to the other two.

As Eric Walker, one of the original founders, pointed out (1947), many new laboratories were established during the war years, headed by men not accustomed to administering organized research but nevertheless expected to produce research results on an efficient basis.

The first Conference had been thought of as a one-time meeting. But it immediately appeared profitable to continue discussion in later years, partly because speakers at the first Conference had not attempted to delineate the separate roles of the three types of research organizations nor were these separate roles clear to them. The program planners for future Conferences thus saw fit to schedule discussions of the purposes and place of each sector in the pursuit of research and in the encouragement and support of it. This brought into focus not only common problems but the variations in philosophy and practices which gave rise to quite different viewpoints on what had appeared to be common problems. If those attending were to discuss problems in the administration of research from a standpoint of practical experience, they needed to consider the variation among the purposes, aims, and objectives of their research activities. Through this, a research director could thus better appreciate whether, for instance, tested techniques for evaluation of the performance of a research scientist or engineer as used by a government administrator were useful to his own needs and purposes.

Furthermore, exploration of the varying roles and how they change was necessary to provide a basis for working relationships in the support and performance of research. These working relationships are discussed in a later chapter. For instance, a university cannot hope to carry on sponsored research unless responsible university administrators understand the aims, purposes, and in fact, the general role of the sponsor in its own research interests. And, of course, it is just as important that the sponsor understand the role of the organization it supports.

In this chapter, then, we shall explore the three major types of organizations, including their individual roles, and some of their comparisons and differences as presented to the Conference body. In doing so, we shall also bring into focus the purposes and objectives of private foundations and other types of research organizations established out of experience dating from World War II. These are the independent, not-for-profit organizations devoted to research consultation or services in a broad advisory sense. The RAND Corporation is one of the earliest of this type. Later, a proliferation of other enterprises developed in somewhat the same category.

The University in Research

Historically and traditionally, the accepted functions of higher education have included instruction, research and service. In fulfilling these functions the university has traditionally been credited as the custodian, the expander, and the purveyor of basic knowledge. The products of the university, in turn, can be classified as highly trained people, new knowledge and advisory services. In carrying out these functions and producing these products most American universities had, throughout their prewar experience maintained a complete integration of their resources.(in the graduate sciences and engineering at least). Few taught without doing research and few researched without teaching. And the faculty and professional staff acted directly in an advisory capacity to industry, agriculture and sometimes government.

World War II experiences drastically altered the traditional role of universities in research, in addition to greatly expanding the volume of their research activity. A shift in emphasis resulting in a greater proportion of university resources being devoted to research was accompanied by changes in the character of university research and of the university organization for research. In reviewing the resulting situation in engineering research in 1948, A.E. White and C.W. Good, two experienced university research administrators, put into context the impact these changes have made on the university organization and the increased public service it can render.

> Since 1940, research, including engineering research, has gone forward by leaps and bounds. This was due to the war and especially to the activities of the Office of Scientific Research and Development, which, to a considerable degree, was a general correlating agency for research for governmental war agencies in our colleges and schools. The men in charge of this Office understood the benefits to be derived through organization with adequate funds. No longer were research activities thought of in terms of $500 or $1,000 grants, but in terms of $100,000 or $1,000,000 grants. The results were astounding.
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> . .
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> Never before in a world struggle have the scientific forces been so utilized. The impact of these forces on the value of sponsored research is still being felt in an undiminished degree, even though it has been more than three years since the termination of actual fighting. Many outstanding contributions were made, three of which were in the field of communications, the development of the V-T fuse, and the development of atomic energy. Much of the work in these fields, especially in the early stages, was done in our colleges and universities.
>
> The lessons gained from these experiences indicate that the technical men in our universities can render great service, whether it be for war or for peace, if the work is properly organized.

Also in 1948, Ernst Weber gave clear indication of the problems arising from increased obligations of universities in the areas of training of research scientists and engineers, brought about as a result of the great expansion of the university research. He showed that the trend toward "bigness" in university research was not necessarily detrimental to the fulfillment of the traditional university role. In fact, with suitable university administration adjustments, he saw that they were then provided with a greatly increased potential to train students effectively.

> But the old concept of a somewhat exotic faculty member doing research in a dim corner of a laboratory has all but disappeared from many universities. Today, the trend is to utilize government-or industry-sponsored research projects with the inescapable administrative shackles which have been discussed at several of these conferences. It just is not feasible to train larger numbers of students in a haphazard manner; it must be done in a more organized form in connection with a distinct research laboratory with proper facilities and staff. The exotic scholar in the proverbial ivory tower has been largely supplanted by the researcher-teacher who is not only well known in his field but who is also conscious of the need for reports, for showing progress not only in his own personal program but also in that of the research fellows under his guidance. He enjoys much more sympathetic help from the university administration in terms of money, facilities, and equipment; however, he must return "performance" to satisfy contractual obligations. Surely, this might cause individual hardship which can only be resolved by mutual compromises; but research, to be worth-while and of significant results requires much larger investment than ever before so that a considerable part must be done in an organized way. This does not mean that universities force their faculty members to do or not to do research; it has remained a matter of individual preference. However, the university administration will generally select staff members of such variety as to enable it to carry on all the phases of its educational activities. In this manner, and where conditions harmonize, the graduate students find now more personal attention and have available larger facilities for research than heretofore; they associate with faculty members as thesis advisers who themselves are actively engaged in organized research and can transmit experience and techniques of greater variety than before, In

> addition, the students have frequent contact with research personnel fully engaged on projects and even sometimes share in assistance by technicians or shop personnel making time-consuming construction of auxiliary equipment somewhat easier.

The role of universities in giving public service also changed during this same period. Although practiced previously, particularly by public supported institutions, consultant services by individuals or groups, based upon expertise, greatly expanded during and subsequent to World War II. This was especially so in those areas related to national security needs. Much of the expansion took the form of idea development as well as physical and engineering development. Both were instrumental in altering, quite markedly, the role of universities and giving rise to university-managed teams, institutions and laboratories for concentrated functional and problem area research and development. Much of this work was done under the blanket of military security, which excluded information from the literature available to counterpart researchers elsewhere. A number of these efforts gradually evolved into separate and private not-for-profit organizations, the roles of which are later described.

By the time of the next Conference in 1949, there had begun a national discourse on these changes in the role and purposes of the university in the research field. In the public interest and in response to national needs, universities had not only undertaken a greatly expanded research effort, much of which was federally sponsored, but also in this process of growth had assumed greater public and private obligations and responsibilities. Some of these could logically be questioned. Fred Lindvall, who had observed these changes, made a ple for industry to shoulder the obligation for what he called "organized" research by teams of scientists and engineers seeking common objectives and a return of the universities to concentration on traditional basic science. He put it this way:

> To maintain basic science in our colleges and universities is hardly a question of academic choice; it is an obligation. A diversion of academic effort into applied research can be only of temporary benefit and can seriously impede the growth of basic knowledge at a time when our efforts in the United States should be concentrated, even more intensively than in the past, on fundamental science.

In this same ye r much heat was added to the discourse about the place of universities in research by Sam Tour, an experienced management consultant. In speaking from what he said was purely his own experience, Tour accused the universities of betraying the public trust by having departed from their primary functions of teaching and doing only that research which is directly adjunct and, therefore, "proper" to the teaching function. He deplored with some vehemence what he termed "university research for private gain."

> Through legalistic camouflage research institutes, engineering experiment stations, research foundations and the like are claiming to be a part of educational institutions, are fulfilling no educational functions at such institutions, are putting scientific research into disrepute and are out in the open market, advertising, soliciting, and bidding competitively for commercial work.

Though few Conference participants agreed with Tour on his basic premise, his provocative remarks stimulated discussion which clearly indicated that the role of the university in public service had broadened and there could be no return to narrowly defined perspectives in its responsibilities for research.

In the ensuing dialogue about the aims and objectives of university research, there seems to have continued to the mid-60's a searching process for better definition and refinement of these purposes and for procedures and practices which would allow universities to preserve their traditional image, yet respond to social and national objectives. A substantial part of the 1953 Conference was devoted to discussion of this process. And it was here that Lloyd Berkner, then President of Associated Universities, stated succinctly the new situation and problem in these times.

> Under the new conditions in which society finds itself the university not only has the responsibility for the search for knowledge, but it has an equal responsibility for the transitional process whereby this knowledge can be made useful to society, through the processes of government and industry.
>
> The problem that we face is this: How can we retain the advantages of Federal support of research and education and still avoid the dangers of Federal control and threats to academic freedom?

Though the new conditions and problems seem well stated, most of the discussions in 1953 and in subsequent years centered on simply restating the problems rather than providing explicit solutions. Despite the absence of definite solutions, it was all too clear that new organizations, institutions, and management methods had to be developed to cope with the new situation in order to bring university interests into proper relationships with the interests of other organizations, particularly the Federal structure.

In discussing the future of university research in 1955, Clifford Furnas, a noted leader and scholar in industrial, university and government research, traced the history of the changing role of the university and provided new direction and guidance in light of past experiences. He stressed that universities had taken on a fundamental role in research for public good and would need better administrative procedures to carry it out.

> Now we find that most universities furnish many kinds of public service through research and other activities. As a result of these trends, I think that there has been a great change in the American concept of a university. Originally, it was described as a community of scholars and its purpose was merely to research and to teach. Now, with the impact of the land-grant college, the function of the university is to research, to teach, and to be of direct public service.
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> At present, the three basic functions of the university--teaching, research, and public service--are now well established. You can argue about whether research or teaching should come first. I think I could argue either side of that question. However, historically research came first. I suppose that in the modern university teaching comes first, but there has been too much a tendency in the past to think the university's only function that of teaching.
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> We have now come the full cycle and have returned to the feeling that the university is a community of scholars, but that the scholars are a different crew than they were in the thirteenth and fourteenth centuries and that they are doing somewhat different things. This is particularly true in the scientific fields, because they are dealing with many of the utilitarian aspects of life. This has become the university's function because the university has now recognized its public service function.
>
> In the future we will need better business organization in the average university, in order to carry on this complicated and important and necessary business of research.

In 1957, T. Keith Glennan, then President of Case Institute, further delineated for the Conference the new problems of administration which arise from the changing role of the university and which affect all its parts.

> Among the ever present problems that occupy the attention of the administrator and the academic investigator alike are:

> First, the vast amount of time and thought spent in preparing proposals calculated to conform sufficiently to an expressed need to be accepted, and the time spent in complying with contractual procedures and routine reporting regardless of results; second, the worry of the faculty member that his contract will be cancelled if the research does not produce something useful within a specified time limit; third, the year-by-year uncertainty of the whole business of research support; and fourth, the increasing tendency of the agency supporting the research to demand a recognizable payoff.

Further discourse on the issue above and larger issues previously described has appeared in every Conference since 1957 but few general or lasting solutions have been put forth.

Finally in 1963, Gordon Brown, engineering dean, reviewed the present situation and described some possible solutions to the knotty problem of what kinds of adjustments universities must make in their new role. In so doing, Brown recognized that no clearly established doctrines have yet emerged but he attempted to set forth some new principles and guidelines. He noted that new knowledge flows downward in the educational process and thus counter to the flow of students. This creates a turbulent situation where it is difficult to maintain logical structures in the various curricula. Increased turbulence in curricula development also results from the necessity to interweave new knowledge with old and from coalition of previously separate fields. Thus, teaching activities must be ever more closely coordinated and developed more systematically.

In summary, the university's role has changed through the post-World War II period from a somewhat narrow perspective to one quite broad in terms of public service. In assuming its new role the university has encountered many new problems. The solutions to these problems will require creative new managers and new methods, not yet in sight, for preservation of the inherent and traditional university functions in both a fast-changing technological environment and in response to the larger needs and responsibilities described by Eric Walker in 1964 as "the administration of research as applied to national problems, national productivity and national welfare."

Industry in Research

In the earlier Conferences on the Administration of Research, discussions involving industrial research were based on the underlying tacit premise that all commercial and industrial research and development has always been directed toward the clear objective of developing useful and marketable new products and services for both public and private interests. Very little examination therefore was given to the aims, purposes, and objectives of research in this sector of the economy. The industry role seemed clearly limited to pursuit of activities which had the best management-judged chance of maximum profit return. Little thought, if any, was given to whether larger purposes, other than responding to direct needs of government, were desirable or even appropriate in the national as well as industrial interest. Hints that industry has a larger role, however, began to creep into Conference discussion. Speaking to this point, Donald McLaughlin (1953), an experienced industrial researcher, reiterated the total industrial role and its relation to community interests.

> Corporations, after all, are not charitable organizations unless they have been established for such purposes, and no apology need be offered for holding to the principle of

> benefit to stockholders that I have just stated. In our social order, which we are proud to defend, the success of an industrial corporation is measured by its profits. With enlightened self-interest, such an enterprise may of course be involved with many more aspects of the life of a community than those with which it is narrowly concerned and still not depart from its true functions and objectives. But, I still want to emphasize profits and maximum benefit to stockholders as the dominant consideration when the role of a corporation in relation to research is being appraised.

McLaughlin allowed one aspect of the larger role of industry when he continued:

> ...the special concern of an industrial corporation should be its own particular field and that whatever research of unrestricted basic character is supported by it should be to a discernible degree related to its own interests. To extend this relationship to the needs of an entire industry or a field, has been the function of a number of industry-wide organizations. They surely serve a most useful purpose by providing a means of pooling common interests and enabling a group of corporations to participate in fundamental work beyond the strict responsibility of individuals. The far reaching benefits that can be brought to a whole industry through such efforts would justify wider support of this sort, without violating the principle of enlightened self-interest in the use of corporate funds that I am anxious to defend.

McLaughlin admitted that the larger corporations could afford to devote some resources to producing basic technology but he looked at this much as a luxury. However, Duer Reeves (1955), a leader in petroleum research, put this kind of activity on two other bases.

> Modern industrial research organizations are keenly aware of the fact that their own raw materials are the basic scientific facts and principles discovered through fundamental research, and within each organization fundamental scientific studies are carried out for this purpose on a scale much greater than is generally realized. Nor are these studies confined to narrow problems of immediate interest, but often range far afield indeed as the organization continually seeks to develop new products, new markets, and better ways of doing things.

> I think the day is fast approaching when industrial research will produce technology as an industrial product in its own right. As this day approaches, industrial research will become more and more a separate industry creating an important raw material under highly competitive business conditions.

Accepting Reeves' description of the rationale for basic research by industrial firms and his prediction that the production of new technology would become a competitive industry in its own right does not drastically alter the role of industrial research, but it does clearly indicate a larger and perhaps more responsible outlook on the part of corporations.

A further aspect of the changing role of industry to become more of a producer of new knowledge was presented by Charles Critchfield, a leader in industrial research, in 1956. Critchfield expressed his belief that industry has the inherent responsibility to provide opportunities for research scientists to do basic research because of the lack of university positions, now and in the future, to absorb all of the national talent in this area. This concept is in direct opposition to that described earlier by McLaughlin and if pursued by many companies would clearly alter to some extent, the role of industry, as well as that of universities.

That the role of industry in research is expanding not just in volume but in scope, character, and in other ways becomes more evident from subsequent discussion. Yet unresolved is just how far corporations can or should go in broadening both their research base and their support of science and technology practiced by others. Some firms have undertaken extensive participation in doing and sponsoring basic research and advanced education. In 1957, Blaine Wescott described as many as seventeen ways in which his firm has undertaken to stimulate basic science activities. He based the company's reasoning in doing so on three points.

1. It is absolutely necessary for successful applied research.

2. It is necessary for survival.

3. Industry is becoming more and more aware of the responsibilities in becoming a good corporate citizen.

W.O. Baker of Bell Laboratories pointed out at the same Conference that management must <u>want</u> basic research but industry usually does not want it for two reasons:

1. Its timing is off from most of the rest of commerce, government and worldly affairs.

2. Its <u>uncommon nonsense</u> content startles and alarms the administrator, who sees, above all and quite properly, the immense values of <u>common sense</u> in running things.

These are reasons which can be understood by anyone examining the role of industry participation; however, there are good indications the barriers in thinking outlined by Baker are being dispelled. Management is becoming aware that Wescott's reasoning makes sense. This does not imply that the industrial sector is losing sight of its original role of research for profit alone; it is further defining this role while sharpening its research management tools in order to make its research more meaningful as well as more profitable. As late as 1963, Chauncey Starr, whose efforts have been traditionally at the forefront of atomic energy research, reemphasized the basic role of industry this way:

> The basic objective of industry generally is profit, survival, and growth, It has certain secondary objectives which we all have as individuals, but the decisions industry makes as to what it does are primarily determined by profit, survival and growth objectives.

Starr's statement shows the unchanged nature of the function of research in the corporate structure. No doubt it also sets the course for the future of industrial research, though many would argue for an expanded responsibility of industry toward the public good. Most larger companies, at least, have become aware that they may rise or fall with the tide of the total economy.

Government in Research

In the larger sense, at least in our kind of society and political and economic structure, government interests, aims and objectives rest largely upon a constitutional base and are satisfied through economic, social, political and military actions in keeping with these purposes. To support its general interests, aims, and objectives, government, both state and Federal, has an inherent two-fold role in the area of science and research. On the one hand, it engages directly in the advancement of science through performing research in its own behalf and often through acting as a custodian and purveyor of knowledge. On the other hand, it encourages and supports those activities that will strengthen research and the advancement of science and technology to meet its general objectives.

The Federal structure derives its dual role from those Constitutional clauses that instruct it to promote the general welfare and provide for the common defense, as E.R. Piore stressed in the 1953 Conference and as

Admiral F.R. Furth and H.Guyford Stever both reiterated in the 1955 Conference. Acting under these general instructions, some technological activities were undertaken in the early days of the Federal Republic. The Smithsonian Institution was founded, the Lewis and Clark expedition was undertaken, and several small projects for answering specific problems were launched. But it was not until the Morrill Land Grant Act was passed in 1862 that the Federal Government became involved directly in performing its own research and supporting that of others. Since that period it has become ever more deeply involved in order to promote those activities calculated to strengthen the nation as a whole, to serve the specific needs of the people, and to secure the necessary information upon which to base major policy decisions. From his position as an eminent Navy research director, Admiral Furth, in speaking of the Federal role, clearly outlined this rationale as it has developed.

> The government has supported scientific research that was geared to the production of useable knowledge, in order to increase our material welfare, our safety, our health, our comforts and conveniences. This role for scientific research reflects a representative government, using the resources of the scientist to give people what they needed and wanted.
>
> What can be termed a broad undercurrent in this mainstream of research in our government is concerned with facilitating the operations of the governmental machinery. This is not a new role, if we stretch our definition of research so that it includes the idea of formal fact-gathering as a preliminary to legislation or executive decision. Our government has always placed emphasis on research of this kind. The Congress relies on the work of its committees and their staffs. The investigative power of Congressional committees is essentially to provide for fact-finding operations, and the special fact-finding commission has a

unique place in our government. Thus, the use of research as a tool of government operation is well grounded in tradition.

The mandate under which the government involved itself in research neither included nor excluded research purely for the purpose of extending the store of knowledge. But as a practical matter prior to the post World War II era we Americans had generally sought to use scientific resources for useful purposes rather than to produce new knowledge. In applying science to meet utilitarian ends, the government devised a pragmatic but fragmented and piecemeal structure for supporting, strengthing and assimilating technology into its machinery, each piece geared to serve the purposes of the particular agency involved.

The lessons learned from World War II and the post-war recognition that we must become producers of scientific and engineering knowledge as well as users of it beget a new dimension in the role of government in research. While not reducing its role to seek the directly useful, the people began to recognize then that our government must build and strengthen basic research for two reasons. First, we must produce knowledge upon which to build the useful, and second, the seeking of new knowledge is a legitimate and noble aim of a society such as ours. Again to quote Admiral Furth:

> The status of scientific research in our government has changed rapidly during this decade, though fundamentally its role is the same; it serves as the means to certain well-defined ends which are the product of policy decisions. We are using research to produce what is desired bv the people. Basic research is now more widely appreciated as an indispensable prelude to applied science and its fruits, and understanding of the role of basic research is growing steadily. This belated appreciation of basic research is not a mirror of the scientist's ideal of the search for truth independent of possible practical applications, but neither are the two inconsistent. Basic research supported

> for civil or military purposes has extended the frontiers of human knowledge immeasurably. Moreover, there is room within the present framework of government-sponsored research for work undertaken primarily "to add to human knowledge."

A review of the earlier history of how this new decision of the government role was accomplished was given the Conference in 1950 by Thomas J. Killian. Killian, who had been long affiliated with university and government research, said it this way:

> Since World War II the United States Government has rendered many tangible services to research. It has recognized that basic research is the foundation of the science and technology vital to our national welfare and security.
>
> In particular, three outstanding steps have been taken: (1) About four years ago, the Office of Naval Research was established, which since its inception enjoyed the full-hearted cooperation of the Army and the Air Force, both of which have been well aware of the importance of basic research; (2) the State Department Scientific Office has been established recently, with plans for Overseas Scientific Staffs; and, (3) the National Science Foundation has finally been authorized.
>
> Thus, after more than five years of effort, a new phase of Government activity has begun--science for its own sake. For the first time our Government has acted in positive recognition of the vital importance of science to our national health, prosperity, and security. It has assumed new responsibilities for the promotion of basic research and the development of scientific talent.
>
> The immediate history of this official recognition of basic scientific research as a national resource began in 1944. On November 18 of that year President Roosevelt addressed an historic letter to Dr. Vannevar Bush asking for a plan in which the successful research experience developed by the Office of Scientific Research and Development could be used after the war to improve national health and the national standard of living. In particular, President Roosevelt was concerned about what the Government could do to increase our future research strength and to discover and develop scientific talent.
>
> "Science, the Endless Frontier" was the stirring answer to

this request. This report Dr. Bush submitted to President Truman on July 5, 1945, one month before the surrender of Japan. Dr. Bush recommended the creation of a National Research Foundation to promote a national policy for scientific research and education, to support basic research in nonprofit institutions, to develop scientific talent in American youth, and to support long-range research on military matters.

Also at that same Conference (1950), John C. Green, of the Department of Commerce, described the beginnings of another government service to research--that of aiding smaller industrial firms in strengthing their technical base. And about this same period our government took under consideration other services that it might render to strengthen our research and technical base. New policies were established that would strengthen our technical manpower base, our educational resources for training, retraining, and the preservation and flow of technical information. These were in response to recognized national needs and are part of the dual role described earlier.

This greatly expanded interest and involvement has, of course, not taken place without creating new problems needing new solutions and new adjustments. National discourse ensued on such matters as these: How far should government go in control of its resources devoted to the fulfillment of these new aims? How much of its resources should be used directly by it and how much and what kind of controls should it place on public funds disbursed to private organizations for these purposes? A great deal of this discussion has been about what kind and how much research the government should do in its own laboratories. W.B. McLean, a government researcher much honored for his development of the Sidewinder missle, gave his views on this subject in 1961, which can be summarized in these words:

> I believe that the Government must do its own research within its civil service laboratories so that it will have the ideas, the competence and the capability to say in what directions the work should proceed and what objectives it should achieve in those areas where the Government has the sole responsibility, such as military research and development. This will insure that the Government will have a competent source of people capable of exercising a broad view in the management of its contract operations. The very decision by the Government that it must do its military research within its own organization will eliminate, I believe, the most important obstacle to the accomplishment of this objective. I believe that none of the obstacles in the civil service operation are insurmountable, but the attack on these obstacles will not begin until the decision is made that the attack is really necessary, and we cease looking for other easy alternatives.
>
> In the development areas, I think the Government needs to do a certain percentage of its work in order to develop the people who will be competent to evaluate the results of other development projects and to see that they meet contractual obligations. Thus the percentage of development work which could be done in-house might rise as high as 50 per cent.

Few would agree, perhaps, that the Department of Defense should perform <u>all</u> of its own research and <u>half</u> of its developmental activities, but McLean's view serves to illustrate the dialogue.

Out of this dialogue has come neither a basic change in the role of government, nor a unified national policy for research. Rather it has produced a set of policies, each of which in some way seems justified in its own right. But this has raised more problems on the national scene than it has solved. W.D. Carey of the Bureau of the Budget summarized the situation in 1963 by stating:

> Without very much visible deliberation, but with much solemnity, we have in little more than a decade elevated science to a role of extraordinary influence in national policy; and now that it is there, we are not very certain what to do with it. We have evolved a variety of rationalizations for what we have done and for what we doubtless will continue to do: science for national

> security, science for a better life, science for a growing economy and science as a cultural end in itself. What we have done less well is to employ research support as an effective agent to upgrade higher education--not just for a few leading institutions but as its broad base--provide safeguards against expediency in influencing career and vocational commitments, and establishing a truly competitive market place within which science and technology must justify itself and its costs in fair competition with other social priorities and preferences.

Carey went on to say that we are inducing ulcers in another generation of scientists, administrators, economists, and politicians who are trying to solve the problems left by their predecessors.

Meanwhile, the Congress has developed a much greater interest in science, technology, research, and federal support of them. Though the Congressional role is as fragmented as the Executive's, and perhaps more so, the Congress is increasingly exercising the powers granted to it, particularly in R & D matters other than appropriations and the watchdog function of the General Accounting Office. Congress's deepening interest has developed through both its existing committees and several new ones recently established specifically for the examination of the status of science and research. In 1964, R.L. Hopper, traced Congressional interests in these areas and reviewed the findings of the Select Committee on Government Research. Hopper pointed out that the function of this Committee, like all others, was finding facts upon which Congressional action could be based. And in this case the Committee had the charge to review all research and development activities of the Government and "make certain that the efficacy of the entire program is maintained and enhanced for the total welfare of our nation." Thus, we find the Congress assuming a role in the nation's technological affairs comparable to that in other important public areas.

Yet the total role of the Government is not and probably will not be clearly defined for sometime to come. As J. Herbert Hollomon, Assistant Secretary of Commerce, put it in 1964:

> Science and technology have become dominant influences in our lives. We have a policy with respect to taxes, natural resources, agriculture, trade, defense, and foreign relations. Yet in science and technology we have no national policy to deal with the allocation of technical resources, or with how these resources can best be used for the benefit of our society.

Hollomon went on to plea for a national policy out of which the roles to be played by several interested sectors could be clarified and strengthened. He enumerated what he felt were the proper ingredients of such a policy:

> First, we must be able to decide what our most important national needs are, and then support science and technology that will best meet these needs.
>
> Second, we must increase our support for institutions of learning and research throughout the whole of the country, and not by projects, or special grants but by institutional support.
>
> Third, we must support technology as a national resource throughout the nation by building local institutions geared to local needs.
>
> Fourth, just as we once had to link the fruits of agricultural science to the working farmers to make American agriculture the most effective and efficient in the world, we must now find better ways to introduce the results of applied science into the offices and shops of our industrial economy, into our local governments and cities, and our institutions throughout the whole of the United States.

In summary, it is fair to say that although the role of government in R & D has not changed fundamentally, we have not found all the elements necessary to the satisfying fulfillment of this role in our modern society. As Carey implied, this will take some new people working for some time yet. His perspective, if followed, could no doubt do much to improve the form and shape of the government's role in the future.

> ...it is my view that the difficulty here is not one of inventing more super-authorities but rather one of organizing "research about research," of developing more adequate insights into cost-benefit relationships, of illuminating our value analysis so that we can with greater confidence strike a balance between being "first" in high energy accelerators and being first in education and in decent living and job opportunity. I do not think that government alone can reach these answers....

Independent Organizations in Research

In addition to the roles played by industry, government, and universities, there are other organizations on the national scene that have functions and interests in research. These are independent agencies, foundations, corporations and other groups. All work on a non-profit or not-for-profit basis; all either support or perform research, or both.

Those organizations specifically chartered on a non-profit basis to support research and sometimes perform it comprise the private foundations. These include health and welfare type agencies and non-profit research corporations, such as the American Cancer Society or the Research Corporation. These organizations are not interested in financial return or meeting objectives, public or private, except in a general welfare sense. Although many of the private foundations have special purposes, their intent is to support what F. Emerson Andrews (1964), President of the Foundation Library Center, called "low visibility" causes. That same year J.W. Hinkley of the Research Corporation put it this way:

> I think it can be said that the private foundations do not look for highly specific and concrete results in most of their programs. They are inclined to place their confidence in the abilities of the individual or institution that their grant supports. Where they are attacking specific problems, it can also be fairly said that, for the most part, they are directing their efforts to determining fundamental causes, rather than seeking specific remedies.

A number of these foundations have been in existence for many years but their role appears in no way diminished by the heightened interests of industry and government in the support of basic research. As a matter of fact, these heightened interests have no doubt been a factor in strengthening foundation activity through an increased public awareness of the importance of fostering and supporting basic research.

Though the proportion of funds committed by the foundations to the support of research is small (less than one percent of the national total) their results have been good. J. W. Barker of the Research Corporation pointed out in 1956 that his organization had supported the early research of nine later Nobel Prize winners. One can conclude from this that the private foundations have a role which fits well within our national framework for research.

The second kind of independent and private organizations are those which perform sponsored research on a not-for-profit basis. In 1961, Haldon Leedy, a leading research director in this area, defined "not-for-profit" this way:

> The word "not-for-profit" also needs some defining, more as to what it does not mean than what it does mean. First of all, it does not mean operating at a loss. In terms of the Federal

law, a not-for-profit corporation is one in which no part of the net earning (profit) inures to the benefit of any private shareholder or individual. Stated another way, the not-for-profit research organizations do not create a profit for the owners or operators.

As pointed out by James McCormack, also in 1961, there are two types of these organizations. He classified them this way:

> They fall into two general categories; it is important to recognize one from the other, because the considerations are rather different as between the two general types. I refer on the one hand to the large, hard-driving, fast-moving systems engineer, technical-direction sort of contractor--I might characterize this type by mentioning the Aerospace Corporation, the newest on the scene. The other group is called by various names, usually meaning "think" groups of "thoughts for sale" or something like that, and I can characterize by the oldest--the largest and best known of these--the RAND Corporation.

The contribution to be made by these organizations, according to McCormack and Leedy, derives from their independence. Being independent means they have freedom of action and decision and an ability to concentrate on research and consultantship in objective ways. In regard to those concentrating on government problems, McCormack stated their purposes and objectives this way:

> To be valid in long term, these organizations must serve as a supplement--not a substitute--for the management which the government must perform for itself, for the decisions it must arrive at itself, for the analysis of its problems which it must make within its own organization. To be viable in the long term, they must have quality and must furnish flexibility in the face of an ever-changing technology.

Although the role of some of these private, not-for-profit organizations is still questioned by many, it is clear that they have contributed much to making up the composite national capability for technology. For without them, the government, at least, would have been and still would be hard put to fulfill those same purposes, and industry would have lacked an important base for advancement of technology for which it could not otherwise reasonably substitute. It may reasonably be expected that the role of the independents will be continuously strengthened through both private and public support. The part played by the private foundations becomes increasingly important in a pluralistic society and a semi-regulated economy where individualism is not always an expected attribute. And, as technology becomes ever more intricate and complicated, the new "think factories", as represented by RAND earlier and others later, no doubt will be increasingly called upon for their talent to do research and furnish consultant services on an independent basis.

Conclusion

In looking toward the future it is doubtful that the roles and purposes of the various types of organizations for research will again, in such a relatively short period, undergo such major transitions as they have in the recent past. Thus one can foresee a period of stabilization in each of the major types of organizations wherein tested practices of administration, though different for each of the various types, will become more standardized and more commonly used. Also the results of research on the research process will be helpful in the stabilization and standardizations of these practices.

CHAPTER III

RELATIONSHIPS IN RESEARCH

Introduction

Discussion in the preceeding chapter, "Roles of Research Organizations," centered on various types of organizations as they separately and individually conduct or support research. In examining these separate roles, it is at once clear that any two or more of these organizations have common concerns. On this premise, these organizations must interact with one another. Turning to look at research in a more collective context, then, we shall be concerned in this chapter with the working relationships and affiliations between organizations involved in research.

Relationships or affiliations between the sectors may take many forms. An individual scientist may give his services as a consultant to a firm or agency. A university and a government agency may cooperate in funding and operating a large special-purpose laboratory. Industry may sponsor projects in university laboratories. Universities may share common facilities. To understand why and how such relationships come into being, it is well to review the varying interests of these groups, looking especially for common interests. Lawrence A. Hyland's presentation to 1949 Conference provides an outline of these interests:

EVALUATION OF RESEARCH LABORATORIES' INTERESTS

Type of Research Laboratory	Time	Dollar Gain	Advertising Value	Patents	Quest for Knowledge	Breadth of Field	Desire for Product	Education	Examples
University	not important	not important	very	not important	very important	wide	none	primary	Any university
Government	minor	not important	minor	not important	very important	public	none	secondary	NACA, NBS, Agri.
Manufacturers Research Laboratory	moderate	long-time only	important	very important	not important	directed	weak	secondary	BTL, Eastman, GE, DuPont, etc.
Independent	very important	very important	very important	very important	none	narrow	strong	none	Battelle, Armour, Midwest
Manufacturers Development Laboratory	very important	very important	very important	very important	none	very narrow	strong	none	Any manufacturers development or engineering laboratory

Representing Hyland's viewpoint at that time, this chart of course does not adequately describe research relationships and overlapping interests as we see them today. Government's heightened interest in development, not just research, as Hyland indicated, is one example. The interests of private foundations are not covered here at all, while the interests of independent, not-for-profit laboratories (such as Battelle Memorial Institute and Midwest Research Institute) are quite different in the view of H. A. Leedy, a speaker at the 1961 Conference. Nevertheless, Hyland's chart is useful in pointing out the relationships that may follow naturally from common interests. These relationships are usually types of working exchanges of information or coordination and do not necessarily include one organization's support of another's research effort. Where interests differ, the relationship usually takes a different form, wherein one organization is supporting research in another or buying research results from it, or both. And these latter relationsh· ·veloped for consideration (monetary or otherwise), take multiple ·· rdependent forms. For instance, government does research itself and . supports research in the other two major sectors, industry and university. Similarly, industry supports research in the university sector and sometimes pays for government services related to research.

To see these relationships developed from common, as well as disparate interests, we need to examine the elements of this three way matrix two at a time and then collectively.

Government-University Relations

A part of the 1949 Conference dwelt at some lengths with the examination of related interests in research. The late Hugh L. Dryden, long-time and much honored Director of the National Advisory Committee for Aeronautics, discussed the need of the government for university research and the importance of government-university relationships. Dryden pointed out that many cogent reasons exist, apart from financial ones, for government support of university research.

> The financial plight of the universities is not a valid reason for subsidy; through research contracts the support of university research is not a subsidy but an investment which increases the scientific potential of the nation.
>
> The administrators of Federal bureaus concerned with scientific, engineering, or technical matters see other reasons that the Federal Government needs to sponsor research at universities. These include service activities such as the training of future personnel for Federal bureaus, and creation of geographically distributed centers for research to aid directly operations relative to agriculture, public health, engineering, etc., or to facilitate Government purchases and also a more elusive type of assistance. Research men know that no single

> agency can have a monopoly of any scientific field, and in particular that some highly qualified scientists are not willing for various reasons, personal, geographical, financial, or otherwise, to transfer their services to the Government bureau having major interest in a given field. Participation in universtiy research broadens the base of operation of the agency, adds new sources of ideas, and offers a means of supplementing the agency program, which is usually occupied with relatively short-range matters, by long-term attacks on basic problems. Moreover, university research contracts give a desirable competitive stimulus to the staff of the Government agency, leading to improved efficiency.

Many subsequent Conference sessions reiterated and substantiated Dryden's reasons for the support of university research by government. The thinking that went into the act of creating the National Science Foundation provided a major extension to Dryden's list, since this act clearly represented government's entry for the first time into support and strengthening of science education and of research for research's sake.

Many would warn, however, as did Lindvall that same year, that these relationships can markedly deteriorate a university's research environment (See Chapter II). Alan Waterman, then of the Office of Naval Research (1949) and later first Director of the National Science Foundation, supported Lindvall by reemphasizing that universities must take precautions to encourage and maintain the individual scholar's freedom of choice in doing research. Waterman said:

> I should like also to stress a point made by Doctor Lindvall which I consider extremely important; namely, that it is in the universities where we must be sure that we uphold the standard of free research. Whether the research is basic or applied, the important thing is that a university is the place where scholars are free to choose what they want to do. In the last analysis this is the best way of insuring maximum progress.

> It seems to me that one of the most important duties of the universities is to make sure that outside support does not interfere with the freedom of the individual to follow his own bent, whether applied research or basic research. The university should encourage the freedom of the individual, no matter wh[illegible]d he prefers, and stand ready to train him in [illegible]ld of his choice. This is particularly true of state institutions.

Later, others, including Hyland (1958), made the same plea for individual freedom.

By 1955, there had been considerable reassurance that government support of university research would not necessarily restrict an investigator's freedom. However, many related problems remained as topics for further discussion. In that year several discussions dealt with the mechanisms for the establishment of projects, indirect costs and overhead reimbursement, continuity, and the like. J. W. Buchta, a university executive, in discussing these mechanisms pointed out that:

> To the (university) business executive much of the research in science done in universities is done for the government, in fact for specific agencies. It is in many respects institutional rather than individual consultation for an outside, non-educational organization. It is an additional activity not previously done in universities, at least not to the extent that it is at present. Hence, to the extent that it is financed by the universities' "own" funds, the institution must either (1) find additional funds from new sources; (2) curtail other "normal" work or activities and give the savings to the scientists; (3) charge full costs, direct and indirect, tangible and intangible, to the agency giving financial support to the research; (4) hold down the research activities to the level at which the universities can contribute a share of the costs without creating distortion in the total program of the institution.

Buchta went on to question whether universities might have already gone too far in accepting support of science and engineering research and if in doing so had transformed themselves into research institutes that should be available for employment by federal and other agencies, just as commercial laboratories are. In discussing the advantages and problems of securing university participation in defense and atomic energy research, Lt. General D. L. Putt of the Air Force and Dr. Thomas Johnson of the Atomic Energy Commission agreed that dangers of university subversion do exist but that, with watchful care, universities can accomplish many things not really feasible elsewhere.

Thus, one could conclude that universities cannot close their doors and fail to respond to national interests, any more than industry can. But the discussion evoked at this Conference session by the presentations of Buchta, Putt and Johnson revealed many advocates of the concept that universities would avoid undertaking project research if they could be assured sufficient funds for general support of their own educational plans. The following repartee brings out these differences of opinion:

> E. Duer Reeves: Can I just rephrase my question a bit? Do the universities consider that these government research contracts have anything to do with their educational plans, other than financial?
>
> J. W. Buchta: Yes. They support graduate students, as I stated a while ago. They permit them to carry on research in areas where they could not before, and at an accelerated rate. But they would like to have them operate in the university frame of reference rather than having the university act as a consultant for an outside agency--not to do something for the

> government, but to carry on research in the traditional way, with support either from government or someplace else. Today it is largely from government.
>
> Mr. Reeves: I'll explain the reason for my question. Of course, in their search for money, universities have approached industry as well. I think the general impression that industry has had from this discussion with various university heads is that what they would like is money in general support of education, and not money for specific research projects, or money even for general support of research. It is the feeling of the university, as I understand it, that they would only do such research as contributed to their educational responsibilities. In other words, they would try to do a certain amount of research to extend the frontiers of knowledge, a certain amount to train their graduates and undergraduates, and a certain amount to keep the professors' hands in, but they would not like to do specific research projects if they did not need the money. Is that correct?
>
> Mr. Buchta: Yes.

In reference to the "National Laboratories" established by universities under sponsorship of the Atomic Energy Commission (such as Brookhaven, Los Alamos, etc.) Johnson had this to say:

> We like to administer our research contracts in such a way that benefits for the educational function will derive from them as much as possible, but the main purpose of the contracts must always be to accomplish research, and not to provide general support for educational functions for institutions.

Thus the universities were in 1955, and no doubt still are, trying to serve two basic purposes which in the eyes of some observers are not compatible.

Government-university relations were discussed again in 1958 and 1959, when ever-increasing government spending had become the major support of university research. Rober Brode, an experienced research administrator in both university and government sectors, argued for increasing government support in universities, but he also warned (1958):

> It is probable that an attempt to accommodate all our needs for basic science research in the universities would very appreciably distort the character of these institutions and might cause appreciable damage to the educational system. We should watch carefully the effects of the increased support for basic science and should probably develop a substantial number of research institutes that could share with the universities the nation's responsibilities for the development of basic science.

In 1959, Raymond Ewell, who had been both a government and university administrator, supported Brode's views, saying that university research was greatly underfinanced.

While the Conference participants continued inconclusively to debate the optimum amount of government support that university research should receive, the Conference has also tried to identify the best mechanism by which government should give such support. Individual project-type support under yearly-renewed contract arrangements for paying the major costs had been the primary mode until that time. But with the success of the university-established institutes, centers, and national laboratories, there began a realization that grants on a "block" or institutional basis have their place in the scheme of arrangements, particularly if they offer continuity, easy administration and payment of all direct and indirect costs.

Eric Walker, President of Pennsylvania State University, put the problem of satisfactory relationships into context in 1960 when he said:

> Our problems today involve the establishment of policies and principles under which our universities can satisfy the legitimate claims made upon them without impairing,

> at the same time, their ability to discharge their basic responsibilities for the discovery, preservation and dissemination of knowledge. For the most part, these needed policies and principles involve the relationship between the universities, on the one hand, and the federal government, on the other. Dr. Charles V. Kidd phrased the basic question this way: "Can the government," he asked, "get what it needs from the universities without distorting and controling them?" I think the question should be slightly rephrased. I think it should read this way: "How can the government get what it needs without distorting and controlling the universities?" This is the central problem facing us today, as I see it.

Walker went on to present one means for solution which, as he said, could provide us with the model we are looking for. He continued:

> In casting around for models on which to base these arrangements, we have all but overlooked the oldest active program of this sort in the country. Yet, ironically, it is probably the most successful of them all. I'm speaking here of the federal support program for agricultural research. Through this program, the government has been providing research-grants-in-aid to our land-grant institutions continously since 1887. The land-grant institutions have almost complete freedom in the use of these funds, since they are restricted only by the provisions of the Land-Grant Acts, which specify only that the funds must be used for research in certain broad areas related to agriculture. They can be used for basic research, as in the case of biological science, or for applied research. To a considerable degree, they can even be used for overhead expenses. The continuity provided through the annual appropriations have made it possible for the directors of the agricultural experiment stations to plan their programs years in advance. The flexibility built into the grants has made it possible to accept industrial grants for applied research without upsetting the balance of the over-all program.

Finally in 1964, Fred Cagle of Tulane, a noted university researcher, reviewed the present status of government-university relations and stressed:

> We need a foundation useful for erecting the super-structure

> of policy and procedure pertinent to the changing nature of government-university relationships in research.

Cagle then presented seven statements which he believes should be a part of the foundation on which a national policy for these relations should be formulated. These are:

> 1. The national need for educated persons of the highest quality will continue to increase. The greatest need is for individuals with knowledge and perspective in science and technology. The university, the source of such manpower, must maintain extensive research programs required in graduate and postgraduate training of such persons.
>
> 2. Science, the major link between government and the university, is neither a spiritual wasteland nor the solution to all of man's problems. Science has brought a revolution in human affairs and is bringing a revolution to the university.
>
> 3. The fundamental responsibility of the university is for the pursuit of learning and for the provision within society of a critically constructive force.
>
> 4. The primary source of new knowledge essential to applied research and development is basic research in the university.
>
> 5. Federal dollars for project research in universities are not "Federal aid" to higher education. An equally appropriate description is "university aid" to the Federal Government.
>
> 6. Science, as a scholarly pursuit, has no significantly greater or special claim on public funds than other areas of scholarship. The grant system is not a device to provide "gifts" to university scientists.
>
> 7. The Federal Government should not provide general subsidies to universities providing partial support for all the tasks of the university. But government can and should pay the full cost of one aspect of one university task--the performance of research identified as being in the public interest.

No one could reasonably doubt that Cagle's seven proposed policy statements set desirable guidelines for future considerations. However, the procedural mechanisms so far developed for government-do not as yet provide an adequate implementation for policies such as these. Future Conferences will well need to watch developments in this area for some time to come.

Industry-University Relationships

Inquiry into the relationships between industry and universities began in the 1949 Conference. J. A. Hutcheson of Westinghouse discussed why and how industries establish relationships with universities. He outlined his thoughts as follows:

> There seem to be two basic ways in which industry supports university research. The first of these is through granting of fellowships, grants-in-aid, or outright gifts to the universities. They are usually restricted only in the sense that the research work is limited to work in a particular field.
>
> The reason industry does this is that there seems to be a feeling of obligation on the part of industry toward the universities for the support of fundamental research work. Industry uses the output of the university; industry uses the people; and clearly it uses the results of the fundamental research work; and this, I am sure, explains the reasons why industry feels an obligation to maintain support of this kind.
>
> There is a second way in which industry supports research in universities. This is through the sponsorship of rather specific research projects. As near as I can glean from the literature, this activity, in terms of dollars, is considerably larger in magnitude than the one I have just described. Perhaps some of the people here could give a better estimate than I, but it probably runs into tens of millions. Incidentally, it is in this field that one runs into considerable controversy.

Hutcheson went on to describe this controversy and its two sides as follows:

> Usually these projects are very specific. The industry sponsoring the research wants results, and the industry wants them as quickly as possible. It intends to apply these results immediately to its own benefit. I think

> it is for these reasons that the controversy arises. I find that more than seventy universities have set up separate organizations to administer this type of work. The various research institutions, departments of industrial cooperation, and so on, are examples of these. It seems that there has been a very great increase in the number of these organizations during the past four or five years. These organizations are independent, but very closely related to the university with which they are associated. There are rather strongly held divergent views as to the merit of this scheme.

Hutcheson also quoted from a contemporary icle attributed to Dr. Stevens of Minnesota Mining and Manufacturing Company. In speaking to this subject Stevens had said:

> The critics of university research institutions maintain that--(1) the introduction of commercial research into a university distracts from the main function of training students;
> (2) It diverts effort from fundamental research;
> (3) It introduces unfair competition with commercial laboratories by using low cost student laboratories, and by applying overhead rates which are predicated on tax-exempt facilities.
>
> Proponents present such arguments as the following--
> (1) Contracts with industry are a vitalizing influence on the training of students for their life work through stimulation of the teaching staff, and bringing students into contact with application of their book knowledge;
> (2) The more basic research problems of industry are better adapted to the university atmosphere than to the industrial laboratory;
> (3) A valuable service is offered to small business, which cannot afford versatile research organizations.

In the discussion that followed, Clark Dunn, long associated with university research, held that university involvement in industrial research with a practical bent is not harmful. He argued this way:

> In our experience (at Oklahoma State University), we have found that by starting on a very practical problem, and

> a very simple one, it quickly leads the faculty member to some fundamental questions. These fundamental questions then begin with him, and are not handed to him by someone else. Having come to this point, if he is research minded, he will go to fundamental research rather than stay with "gadgeteering," or whatever you may call it. In my estimation, many times the applied research is the thing that brings us to fundamental research, and, therefore, I don't believe that there should be too much worry about a reasonable amount of applied research in our educational institutions.

Very little discussion of industry-university relationships took place between 1949 and 1957. In 1953 Earl Stevenson, prominent industrial research leader, reviewed the trends taking place and the reasons for them.

> In terms of interest to this Conference, the most significant trend is that toward larger support of research programs within the corporate entity. Possibly the actual increase in annual appropriations is not so important as the growing appreciation of a well-balanced program in terms of short and long-range programs, of the relationships between basic research, applied research, development and engineering. All this might be construed as the recognition of a responsibility on the part of industry, but I am quite sure that the real motive is enlightened self-interest.

But it was not until 1957 that the Conference undertook again a review of these relationships. At that Conference, as mentioned earlier, Blaine Wescott of Gulf Research outlined seventeen ways in which his company supports or stimulates basic research and science education. In so doing he brought out very clearly not only what his firm is doing but stressed the importance these programs can have in helping university interests. Since his seventeen methods are rather comprehensive in terms of what is possible on the part of industry, they are outlined here:

1. (Gulf) is doing basic research in its own research department.

2. Through our fellowship at Mellon Institute, which includes thirty people.

3. Support of further education at the University of Pittsburgh of the Mellon Fellows while doing research at Mellon.

4. Support of basic research by trade associations.

5. Cooperative research programs with universities.

6. Scholarships (undergraduate).

7. Alumni gift matching

8. Salary supplements for university faculties during vacation periods.

9. Unrestricted grants to non tax-supported universities

10. Unrestricted grants to specific departments of universities

11. Graduate fellowships

12. Direct grants to universities or departments for capital programs and operating needs.

13. Donation of equipment

14. Summer employment of students

15. Use of university faculty as consultants

16. Lectures to our technical staff

17. Establishing endowed chairs or professorships

Wescott felt that the pattern he outlined was sufficient to satisfy individual responsibilities to support science and education and provides a basis upon which satisfactory relations can flourish. Few would disagree but it is clear that not all companies can undertake all such programs at any time. For as pointed out in Chapter II, industrial firms have inherent limitations in how far they can broaden their aims in the public interest.

Government-Industry Relationships

Allen Abrams of the Marathon Corporation pointed out in 1955 that World War I marked the beginning of sizeable and involved government-industry relations in research. The subsequent expansion of government support and government-industry relationships, particularly after World War II, begot many problems in the area of cooperation between the two and, as Abrams said, "Can it be wondered that this enormous expansion has been accompanied by inefficiency, duplication and waste?"

Just prior to 1955, Abrams participated in a broad and penetrating survey of industry outlook on government supported research. His presentation of some of the results was divided into four areas as outlined here, along with some of his comments on them:

> The research contract
> There is marked criticism (a) of the overwhelming magnitude and variety of research contracts, (b) that invitations to bid are too widespread and thus many companies spend heavily on estimating without getting any awards, (c) that certain government agencies use a procurement type of approach in trying to specify research as though it were hardware.
>
> The research program
> Foremost in the minds of respondents is the belief that the research program should be planned carefully and administered ably. The difficulty in carrying out a program usually starts at high levels. There are so many administrators, boards and committees not conversant with research that it is hard to secure understanding and co-operation among them.
>
> Personnel
> It is clear that in some areas government officials do a good job in co-operating and in co-ordinating projects but that in many programs the contracts between top men are poor. In such areas there is little exchange of information and a surprising lack of co-ordination among different fields.

The salary scale in government agencies has made it difficult to bring top-grade personnel from the outside. Furthermore there is limited opportunity to secure suitable administrators from within.

The slowing down of government-sponsored research programs results from the continual shifting of responsible officer personnel.

Red Tape
Throughout this discussion you will have observed remarks on the stagnating effect of red tape. When additional comments of this nature are gathered at one point they make a sizeable volume. The most frequent complaint is that agencies are so bound by policies and regulations as to give administrators little room for the use of good judgment. Consequently these officers may give attention to insignificant details and neglect big items. The contracting, accounting, and reporting functions impose undue and irksome burdens. Administrators require too frequent and too detailed reports. Auditors seem intent on disallowing overhead and other charges, until the situation becomes intolerable. These people will spend endless hours arguing over a cost item of a few dollars, meanwhile holding up a million-dollar program. But it is admitted by some companies that they too may be at fault.

The already vexed and bewildered laboratory may find its program further slowed up by undue security regulations. Often these result in over-classification of projects, with the prevention of open discussion. This may lead to two departments carrying on much the same type of project but without knowledge of duplication. At the same time, one may often learn more from a popular magazine than is to be found in a classified report.

As one executive points out, government is a clumsy, sprawling organization. Closer relations between government and industry are defeated largely by the fact that government is just too big. As another executive concludes, this disease started with the Roman Empire and it is doubtful whether we can do anything about it.

Abrams went on to conclude that if government and industry are to achieve maximum results it will be only through complete and willing exchange of ideas. He was supported in this by Daniel P. Barnard, who from his long experience both in industry and government research administration, observed that:

> In a nutshell, the problem boils down to providing for technical personnel those channels of ready communication so necessary to their work, without compromising security and without creating a format so cumbersome as to defeat its own purpose.

During this same Conference Morris T. Carpenter and John H. Richardson, both industrial leaders, expanded on the points raised and the solutions proposed by Barnard and Abrams. Carpenter stated clearly his belief that all government contracting officers should be civilian personnel (rather than military in the case of the Department of Defense). These civilians should operate under administrative guidelines rather than rigid rules and regulations. Richardson took a different view. He accepted the premise that since industry had participated in forming them, the governing statutes are acceptable and provide a reasonable working arrangement. He supported both Abrams and Barnard in his plea for closer working relationships through a better understanding and appreciation of each other's problems and operations. Richardson's points about how to do this were:

> 1. Contractors should take full advantage of the opportunity to increase the numbers of their personnel who can acquire this understanding and appreciation of the customer's activity by establishing firm procedures for rotating field office representatives.
>
> 2. The government should give more careful consideration to the industry training program, to those whom they select and to the subsequent assignment of the people who receive technical and business training at contractor's facilities.
>
> 3. On both sides, overlapping of fields of endeavor should be minimized. There should be as few points of contact as possible; technical people should be afforded the freedom of communication necessary to assure the timely discharge of

> their responsibilities, but they should confine their contracts to only engineering matters. Also, of course, nontechnical people should not assume responsibility for technical matters.

In the area of fundamental policy changes which would improve relationships, Richardson suggested:

> 1. There appears to be a trend toward scheduling experimental hardware deliveries within the contracts. This has contributed to strained relations in all quarters.
>
> 2. Another policy which seems to be developing that should be checkmated is one that suggests contracting in firm dollar amounts for several years of invention.
>
> 3. A more liberal acceptance of the support of general research in the contractor's allowed overhead should be adopted.
>
> 4. ....The policy of restricting the number and wage ranges of government administrators is going to have to be changed some day if proper administration is expected to be exercised by the government.
>
> 5. ....We, in the contracts business, are often questioned by our technical people as to why the fixed fee on their efforts is always so low. It is not my intention to use this medium to plead for higher fees, though such is not beyond me, but I do feel strongly, very strongly, that proper consideration is seldom given to the truly remarkable contributions made to the total defense effort by the technical people of this country.
>
> 6. A constant source of irritation to both parties is that contractors must continually use their own funds to support government programs while the paper work is being processed for the continuation of a program.
>
> 7. A final point to consider in this second area is that sufficient emphasis and study has not been devoted to the changes required in procurement organizations to keep up with the rapid changes and complexity of today's weapon systems.

Also in that same year (1955), a Conference panel chaired by Thomas J. Killian, then of the Office of Naval Research, discussed government-industry relations. This panel took the position that there is really no essential difference between civilian and military contract or award officers; both work under identical rules and regulations. However, there is a wide variation between various government agencies in interpreting and employing these regulations. The Killian panel felt that what is needed is a simplified and uniform pattern of procurement activities by the government.

Throughout the Conference Proceedings there has been a predisposition to reassess government-industry relationships. In 1958 a thorough discussion of them took place. Norman T. Ball, an experienced government executive, took the view that the public interest requires the solution of certain problems for which industry cannot be expected to assume responsibility either because the necessary capital investment is too large or because there is no assurance of profits adequate to repay the costs of the research. He went on to point out that:

> Today some research in almost every field of science is a recognized obligation of the government and is willingly supported by the representatives of the people in the Congress. In meeting this obligation to do research, the government agencies have often developed an outstanding position in a particular field.

As a consequence:

> The increasing national scientific activity with limited numbers of scientists and engineers has resulted in competitive situations between government agencies and government contractors in recruiting and retaining these specialized personnel. In this competition government programs are suffering because of restrictions in personnel, salary, and program which do not occur within the relative freedom of contract industrial programs.

Accepting that competition for qualified personnel creates some difficulty.. We are left with the need for further study of how cooperation of government and industry can be better developed.

In exploring these same questions, George Glockler, a famed chemist with both government and university administrative experience, concluded that relationships in research between government and industry ought to take the following pattern:

> 1. In the case of production of goods to be sold in the open market place in peace time, industry should finance its own research and development as it has done in the past. This attitude is one of the tenets of a free economy and has created the present technology and mode of living in this country. It should not be changed, and it is believed that the free economic and social system will be able to compete with the operations of other forms of controlled economies.
>
> 2. However, in the case of production of war material where the only customer is the government itself, there should prevail an entirely different attitude. The expense of research and development in the missile field, for example, is so vast that no private corporation could well afford to expand its saving (i.e. capital) in carrying out research and development for the government without assurance that the private organization would have a chance to recover its expenditures for the initial attempts at creating a tremendously expensive item. Hence in such cases the government must expect to carry the heavy expenses even to the amount of 100 per cent.

Glockler's separation of the relationships into these two categories seems quite logical on surface examination. But it does not go far enough to meet the total range of the public interest in research and technology. Clearly the government must become increasingly concerned with research to "promote the general welfare," rather than confine itself to that which "provides for the common defense." Therefore, it must take a positive hand to increase its own capabilities in general welfare research and to encourage and to support industry research which serves these larger purposes. And the question of whether the government should do a particular piece of research itself or whether it should have it done in industry, or in the universities for that matter, has, as Ball put it in the discussion in 1958, no categorical answer.

Tripartite Relations

In addition to two-way relationships established and nourished by the three major sectors of research interest, there have been some matters of sufficient common concern to warrant the establishment of three-way relations. Though they each have their separate and individual interests and problems, all three sectors have joined hands in certain matters affecting the three sides, such as research manpower, communications and information exchange, patent considerations, and the like. Since these matters are beyond the realm of day-to-day working relations and since, as pertinent subjects of research administration, they are discussed elsewhere in this summary, no attempt will be made to do so here. It is well to note in passing, however, that the rationale of the Conference itself reflects a recognition of their importance.

Beyond these fringe concerns there is another scheme for cooperation which, as Merritt Williamson observed in 1964, is "an R & D phenomenon of our times." This scheme involves communities for research or "research parks." From his long involvement as a research leader in industry and academic pursuits, Williamson observed:

> By research parks I am not talking about research institutes, although they might be participants in a research park. For purposes of our discussion we will consider a research park to be an area where a number of different organizations

> have located their research and development departments or laboratories. A wide variety of relationships might exist between the laboratories and the organization promoting and managing the research park. The arrangements might run the gamut from each laboratory owning its own land and administering its own services completely, to some kind of joint arrangement whereby they share common facilities such as cafeteria, machine shops, library and so on.

The establishment of these research parks in no way obviates the usefulness and role of the previously or separately existing organizations for research but tends to supplement and extend their separate capabilities and their effectiveness in providing for three-way relationships to be effected through physical proximity. Additionally, of course, such parks can be effective tools for economic development of the regions that surround them.

Research parks, like other schemes for cooperation in research, got their start essentially after World War II with the establishment and growth of private research institutes, often promoted by universities, and the recognition by industry of the advantages to be gained from location of its research activities near an academic community. Gradually the government, too, have become more than an interested spectator which supports research in industries and universities and has begun to establish laboratories in these parks as well. The entry of government laboratories into these park^ rounds out the total community of research interests.

In 1964 R. G. Snider, an experienced research park director, reviewed the growth of these parks and some of the factors which seem to make them a successful venture. Snider observed that:

> Being near a government center in itself does not appear to influence growth significantly, although

> the nature of the government activity, as well as aggressiveness of promotion and the conventional location factors do apply. Eight parks with one or no tenants were so located, as were seven with two to ten occupants. However, about one-third more of the "successful" parks were near universities than was the case with the tenantless or single tenant parks. Degree of proximity and strength and attitude of the university are probably the significant factors here.

Willard W. Brown, a research park executive, supported Snider's conclusions that proximity to a university is essential to a park's success, and pointed out the conditions for successful relations between the various interested parties. He stressed these concepts:

> Ready accessibility to the research center as well as close proximity to the principal academic, technical and cultural institutions of the area are of great importance to the success of the development.
>
> The control and direction of the research center development can be best achieved by the academic institutions initiating the project through the establishment of a business oriented organization.
>
> A necessary adjunct to the research center is the presence of an established competent contract research organization as a necessary tool in effectively bridging the gap between the academic, theoretical minds of the educational institutions and the highly profit-motivated, product-oriented approach of industry.
>
> Although the responsibility rests with the academic institutions to initiate, control, and organize the research center, the success of the venture is clearly dependent upon the center becoming a community enterprise.

The viewpoint of government agencies on research parks was reviewed by John W. Dawson, who has had first-hand experience in operating a government research supporting agency in the proximity of a research park. Dawson's overall view seems to summarize the advantages to be gained by such a scheme when he remarked:

> I prefer to visualize a research park as an integrating mechanism which enables the individual scientist to contribute his separate observations which in turn can be incorporated with the findings of his colleagues so as to contribute to a larger understanding and utilization which, working alone, he could not possibly have realized.

Dawson went on to reiterate that, in his experience, this concept extends across government organizations in the research park community without compromising to any degree the objectivity they must necessarily maintain.

Johan Bjorksten, a long-prominent industrial researcher, pointed out how industry relationships with universities and others are improved by location at a research park. He summed them with these concrete examples:

> Summarizing, the industrial research and development procedures benefit from the readily and economically available collaboration existing in a research park by a cost reduction due to the ease of handling peak loads on idea demands with a smaller idea staff than would be necessary otherwise; by an increase in performance due to the extremely broad scope of experience available and the availability of the most up-to-date techniques; by an increase in flexibility due to availability of qualified personnel and of equipment on a temporary basis; and by the possibility of reducing the cost of research proposals, the overshadowing cost factor in government sponsored research under present research procurement procedures.

A former university president and an experienced research park executive, Jean Paul Mather, added another dimension to the usefulness of the research park scheme. He sees it becoming, under suitable arrangements, a cogent mechanism for bridging the gap between, in his words, the "academic ivory tower and the profit-motivated hardware-out-the-backdoor interests of corporate industry," without distorting or disturbing the philosophy and objectives of graduate academic programs or faculty research.

Speaking from the viewpoint of the universities, especially publicly supported ones, Jesse Hobson, a leading director of university and institute research, supported Mather's views and emphasized that the research park is an effective instrument through which public institutions can fulfill their state and regional responsibilities. He stated his feeling this way:

> It is not sufficient to continue the traditional role of the university, to preserve knowledge, transmit it and create it.
>
> I think a state university has to assume today a certain amount of leadership to its region and to its state to meet the social and economic problems of the state. I feel this is an exceedingly important function, purpose, and responsibility of a state university, particularly.

Yet others participating in the discussion, particularly from the university viewpoint, see a less hopeful picture. They see real problems, for instance, in a university's delivering on its commitment of faculty time to provide the desired interactions. These problems have to do with the reluctance of many faculty members to either assume any responsibility toward industry or government or to participate in these arrangements in response to management's assertion of prerogatives on the effort. Participation in such arrangements is never as attractive to a distinguished or "name" faculty member as other institutional and personal commitments on his time. Yet, if faculty members <u>do</u> participate in research park activities there are always the nearly insoluble problems of apparent conflicts of interest. These problems are discussed ~~later~~ in Chapter VII.

Most research directors, however, do not let these problems turn them away from considering the idea of a research park. They accept it with varying degrees of enthusiasm, seeing in it sufficient advantages to warrant its continuation and flourishing, and to justify their direct concern with the idea at some future time.

CHAPTER IV

THE RESEARCH DIRECTOR

The research director plays the critical and pivotal role in any system for effectively managing research and development activities. Any examination of the problems of research administration would be less than complete if it ignored or failed to deal adequately with the director's salient characteristics. Moreover, since the present summary must give attention to the problems most often examined at meetings of the National Conference, it is clear that the participants, most of whom are research directors had a predilection for self-appraisal. Full treatment of this subject thus requires an entire chapter.

Over the period of the eighteen Conferences, many sessions and some individual presentations have been directed at delineating the role of the research director. Such questions were asked as: *Who* is the research director? *What* is he? What does he need to know and what must he be able to do? And how and by whom is he given to tools, knack, and insight to do this? Is research direction a profession? If so, what are its technical and ethical standards?

We find the background for exploring these questions in a presentation by Paul Foote at the First Conference (1947). At that time Foote gave graphic descriptions of two typical research directors, one from the 1930's and one from the immediate postwar years.

Here is his view of the changes then going on in the tasks of the research director:

> The duties of the research director have changed character in the past twenty years. When I was first engaged in industrial research I was an academic type of fellow and enjoyed working in the laboratory with the physicists and chemists. These were then the primary functions of a research director--to originate ideas; to inspire; to enlighten; to infuse and diffuse the investigational spirit; to nurture the germ of inventive genius; and to accomplish this it was necessary to have intimate contact with the technical personnel. Assume the photograph of Fig. 2 represents the year 1933. How happy the research director appears! He may have been working here with the development of 100 octane gasoline, at that time almost a laboratory curiosity and some twelve years later sold to the Army and Navy at the rate of half a million barrels a day to win the war for democracy.
>
> But along about 1933, a new change came over America. The planned economy system was enforced requiring increased help, especially of a non-technical nature, in the laboratories and everywhere else. Fig. 3 shows the modern (circa 1947) scene of activity of an industrial research director. Here the poor worried fellow is surrounded by a battery of extra-curricular specialists: budget controller, vice-president in charge of questionnaires, and legal counselors, business manager, and the like.

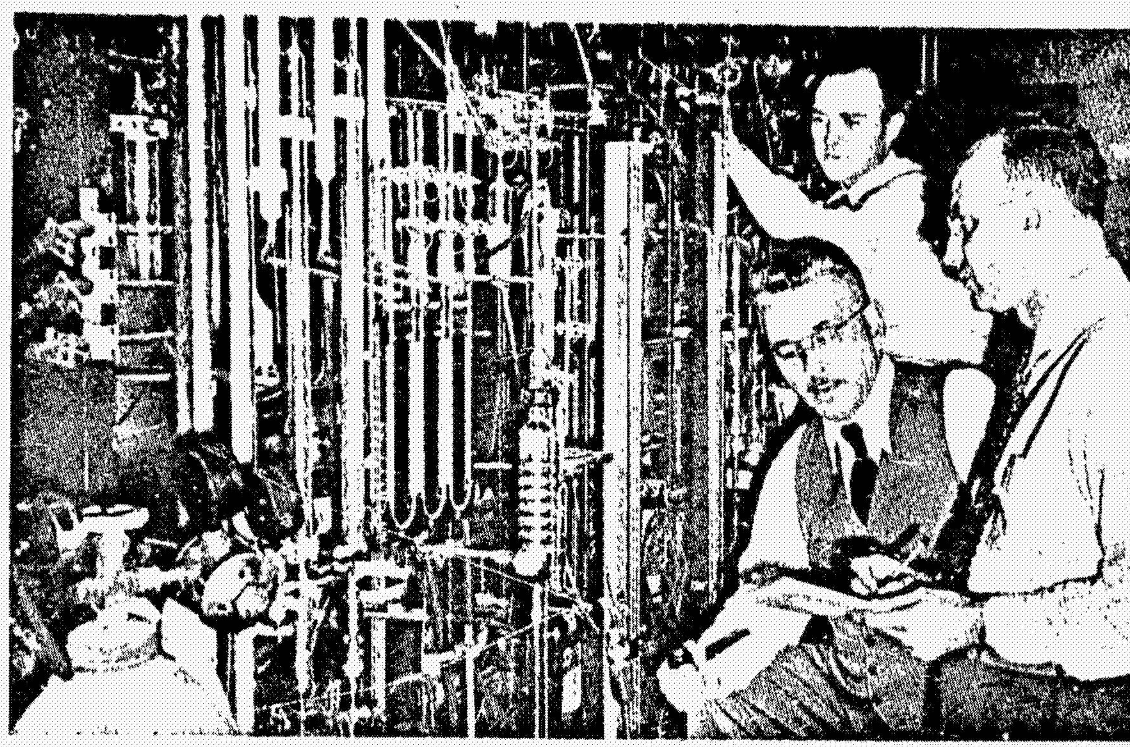

Fig. 2 The Research Director in the Early 1930's.

Fig. 3 The Research Director Circa 1947.

From later Proceedings we can envisage several approaches to exploring further these basic questions. During and immediately after World War II, there appeared a mounting interest in the effectiveness of criteria and methods for defining jobs and for evaluating the performance of people in those jobs. In part, this interest was spurred by the extant need of the Armed Forces to judge their supervisory and professional personnel (officer and non-commissioned officer) against the essential requirements of their positions. In this way the officer selection boards could become more critical and better perform the monumental tasks before them of selection into and out of the commissioned and non-commissioned officer ranks. In a synergistic sense, if they could identify and enumerate the critical elements in a position and the man in it, and then describe and weigh these elements against each other, they not only had better evaluative criteria, but from an analysis and conversion of them, they could identify at least some of the things they needed to teach professional people, or have them taught, before they reached a position of major responsibility.

This, then, is one approach. The best attempts at using this approach up to that time were described by John Flanagan in 1950. Drawing on experience gained in evaluating military jobs, and the critical characteristics of people to fill them successfully, Flanagan described the methodology for getting at the research executive and his position, and referred to a pilot study being made on research managers. However, there appears to have been no follow-up study performed, although, the next year (1951) Flanagan reported methods (that are later described in Chapter V) for study and evaluation of research personnel. While Flanagan's report represents a worthwhile tool for use by the research director, it does not describe the research executive himself. One might infer that the most efficient research personnel, with experience and maturity, become the best research directors, but few would agree. Certainly the critical job requirements are much different, as evident from the descriptions given of them by other conferees in 1950, as well as in later years. And it is very clear, as was reiterated in 1956 by David Emery (Chapter VI), that their objectives are quite different and, in fact, conflicting.

Which brings us to a different approach, perhaps heuristic--devoid of numbers, figures, and letters--but born of experience, as most of the art of research direction yet seems to be. In the 1950 Conference Albert Lombard described the military research director, while Ray Stevens described the industrial research director, drawing on the requirements earlier stated by Norman Shepard in Research in Industry. A comparison of the Lombard and Stevens descriptions surprisingly reveals an almost one-to-one correspondence--yet the conclusions reached in the 1950 Conference seem best stated by Ray Seeger's summary of the subsequent round-table discussion:

> It was generally agreed that there is not yet any valid concept of an ideal research executive. Such an individual, at present, may be called upon to perform various functions, the exact pattern depending upon the specific job and its place in a particular organization. The essential responsibilities seem to be: (1) obtaining ideas, i.e., planning; (2) selling ideas up and down, i.e., promoting; (3) administering the resultant program, i.e., supervising, etc.
> Most individuals, however, were of the opinion that agreement could be reached on the basic virtues desired

> in a research executive. A research background is regarded as the primary requisite for such an executive. The ability and desire to handle people were also stressed.
> The primary need now is a reliable list of items in a usable form for evaluating or selecting research executives. The concepts must be defined precisely in as quantitative terms as possible. Records of specific instances of good and bad performance were recommended as an initial procedure in this direction.

If we examine the Conference dialogue on qualifications and on performance descriptions some several years later, (1954, 1956) we might conclude that the intervening years saw very little advancement in either specific or quantitative methods for measuring or evaluating either. We still find descriptive adjectives used to give qualitative assessment, but we are now finding different terms used in a different way, and used more precisely. In describing the qualifications of an engineering and research manager in 1954, Howard Richardson said:

> There are, to my belief, practically no qualifications, no correlation between performance and any specific personal qualifications. . . .There are only very few that have any significant correlation. . . .One is an expression of physical energy (leadership). . . .Another is that he must be able to get himself across to other people. . . .he must be willing to make a decision that he knows will be unpopular. . . .Last, he must be willing to pay the price (of giving up working wholly within his professional specialty). . . .Too many engineers and scientists feel that the managing part of the job is very simple. . .I think the best way to develop a manager is to work under a fellow who is a good manager.

It is interesting to note that nowhere does Richardson require that the research manager be an outstanding engineer or scientist but rather that he have a strong affinity for general management and a desire to learn to practice it in behalf of the scientific and engineering interests of the total organization of which he is a part. Richardson clearly expressed that one learns management by doing rather than by observing. It would appear from this that one could not be taught to be a research director except through such methods as role-playing.

Several Conference participants reviewed the observations of how researchers and research managers performed, but they were unable to sum any detailed set of evaluative criteria from them, or to state qualifications beyond those that are common to all managers. These are such qualifications as described by Richardson and others before him, in qualitative and general terms.

In 1956, Maurice Holland expanded the concepts stated by Richardson. Holland drew from real-life examples of those generally agreed to be research directors among the most successful to that time. He took up case histories from industry, government, and universities, and from this study synthesized a practical set of descriptive characteristics and performance capabilities that an effective research manager must have. Holland's listing follows:

1. Sufficient training, experience, and accomplishments in the appropriate sciences to assure confidence and command respect.
2. Skill in establishing department objectives in harmony with corporate objectives.
3. Sound business judgment to assure practicability of programs and justifiability of projects.
4. Leadership capabilities in--
   a. Planning what is to be done
   b. Organizing to get it done
   c. Directing those who supervise
   d. Coordinating all that is done
   e. Controlling and correcting to see that it is done well.

5. And as for skill on administration, specifically, they claim, out of the synthesis of over 200 profiles:
   a. Practices effective human motivation
   b. Selects his team skillfully and impartially
   c. Delegates authority with responsibility
   d. Recognizes joyfully accomplishments of others
   e. Corrects errors promptly, fully, and fairly
   f. Seeks informed counsel and advice from associates and others
   g. Renders decisions promptly, based on studied facts and probabilities.

Again, it is important to note that Holland's criteria, in all ways except the use of the word "science" in his first item, could be applied to any other major manager, whether he be a part of an industrial, government, or university complex. To go one step further, Holland notes an even more important fact--that these really are the qualifications sought in choosing the top manager and chief executive of the organization.

In later Conference sessions, others spoke of things an effective research director should be able to do, but little of a new dimension seems to be added to the basic list of performance capabilities that Holland presented. In 1957 Joseph McPherson expanded on the question of how to manage scientific activity through employing both new understandings of human motivation and methods for developing insight, creativity, and the like. In 1959, Ira Kaar made a plea for better understanding and appreciation by top management and other departments of the valuable contribution to be made by the research scientists and the research effort, particularly in a profit-oriented organization. His expression of this problem seems symptomatic of the approaches often taken in earlier years.

> In a way, it is unfortunate that research has attained such a reputation for producing miracles. Too many executives seems to think that the secret of success lies simply in budgeting a fund for research, hiring

> some Ph.D.'s then sitting back in comfortable complaisance to await the miracle. Others don't really believe in research at all but tolerate it because it seems to be the thing to do--like wearing socks.

Kaar's analysis appears to reinforce the accepted axiom that the research director must not only be able to continously sell ideas and enthusiasm to his own people, but also he must be able to persuade those above and beside him to give a larger place to research.

In 1959 General John Medaris and David Hertz also added to our understanding of the role of the research director in the areas of management control of his scientific resources and of day-to-day operations. Medaris described this role as follows:

> Almost four years ago I made this statement to my control organization. "You have one purpose in life: to put yellow lights on the road of progress. I do not want to be brought to screeching halt by an unexpected red light. It is your job to give me a yellow light first, and far enough in advance that I can either maneuver around the road block or clear it and get a green light before I reach it." Because of this I think the Army has an enviable record for on-time accomplishment of things that supposedly cannot be scheduled.

Hertz went on to describe the necessity of a research director's being a strategic scientific manager in a controlling sense. He described it this way:

> When he finds a breakthrough, or when he finds a weak spot, the strategist for science must mass his forces to consolidate the breakthrough. This means that the job of the research administrator (who is or should be a scientist) is to have ready the resources which will enable him to achieve and exploit such breakthroughs. . . .The scientific director must control the gathering and the evaluation of intelligence about the battleground on which he is engaged.
>
> There must be tactical training and tactical skillfulness.

> The person who is doing the bench work must be a good tactician and he has to know what he is doing. Seeing that this is so is part of the scientific administrator's job. There is no reason to expect a manager, in the economic sense, to know whether an electronics engineer is, in fact, a good tactician who can deal with the observations he makes in a skillful way. This is not something I would expect a business manager to do, or be able to do very well, but the scientist-manager is a different story.

In later sessions, other speakers helped to fill in the total description of a research director. In 1962, Edward S. Jamieson and others examined ethical standards for research managers in their inter-relationships with other parts of their organizations and with society. From their presentations one can add, the requirement of conscience. The research director must possess a conscience and instill it in his subordinates. Quoting in part from Moorehead Wright, Jamieson listed five ethical components of conscience: competence, justice to the individual, honesty, forgiveness, and love. A fuller discussion of ethical issues in research administration is given in Chapter VII.

In a perceptive presentation in 1964, Karl Van Tassel seems to have summarized succinctly the task of the research director as a part of the later accepted concept of managing research by the "outside-in" approach. He enumerated what the research director must be able to do, and, though his listing is designed for an industrial organization, the basic principles hold for other types of research complexes.

> The manager, who is the chief executive holding responsibility for this work, should recognize that R & D work needs to be managed. It is not self-regulating.
> Management of this work consists of:
>
> 1. Setting R & D objectives, commensurate and

compatible with the business objectives.

2. Defining critical areas of business needs and opportunities.

3. Allocating suitable effort, time, and money to these areas.

4. Selecting projects in which combined functional judgment indicated prospective reward commensurate with the effort.

5. Providing a working climate of creativity for the research associate in which he is stimulated to independent thought and, at the same time, supported by the know-how of his associates in the other functions of the business.

6. Providing the research associate with a sufficient view of the business as a socio-economic instrument and his role as a major participant.

7. Monitoring the overall work with decisions accomodating inevitable changes as work and markets progress, and, above all, decisions at the appropriate point to either take the results to market or to abandon them.

8. Utilizing the university's basic research to supplement the overall program.

Reflecting, then, on the basic questions of who and what a research director is, what he must be able to do, and who trains him and how, we find that, although systematic and logical attempts have been made, to this time we have not as yet been able to do more than provide a set of adjectival descriptions of the desired characteristics. We seem to be agreed on certain things. For instance, we demand that the research director be or have been a researcher. We are certain that he must understand the process of scientific discovery and invention and that he must be a force to instill the highest order of motivation and creativity in his research associates. But beyond these certainties, these questions resolve into a recognition that after all, a research manager must, in addition, be an executive, and all the requirements of an executive are incurred.

If this be the case, then one must conclude that a research manager must first be formally trained to be a scientist or engineer. No one has proposed that we train him in business and management first--then perfect his knowledge of technology. Rather we think we must do the reverse.

But when and where does the research manager learn management? One school of thought is that we ask him to undergo formal training in business management, and some think, the earlier in life, the better. Others hold that there is no substitute for "one the job" development of the characteristics and capabilities described by Richardson, Holland, Van Tassel, and others. There is general agreement, however, that "managers are not born," and somewhere along the path of transition of the researcher becoming a manager and then a director, he must gain informal or formal training, or both, in such areas as organization, planning, finance, marketing, administering people, and other pertinent areas generally accepted as fundamental to business management.

The conclusion here seems to be that, after all, research administration is indeed a specialized, professional form of business management. Generally, to be a profession a field must comprise specialized knowledge which is self-consistent, logical, ordered and amenable to research by the scientific method for reaffirming and extending it. Also such knowledge can be taught, learned and practiced. The field of research administration requires that one first become a professional researcher and practitioner, then progress through a process of further extensive formal and trial-and-error training to become, through employing agreed-upon standards, professional in administering the efforts of those who constitute, along with him, the major force in e success or failure of the total mission of the research organization.

CHAPTER V

MANAGEMENT OF RESEARCH OPERATIONS

Introduction

This chapter is concerned with the overall task of managing and controlling research operations. What a research director does, what tools he uses for management control and what expertise required to perform each part of this task are all described. These aspects are also examined from the view of whether such actions and decisions constitute a unique and specialized set of technical skills necessary for the research director to be effective.

In considering the overall task of managing and controlling research operations, one can logically divide the effort into two categories--the managing role and the supporting role as pointed out in Chapter I. The latter includes administrative, logistic, and financial support activities necessary to provide the director and his associates sufficient pertinent information of the right kind on a timely basis and to perform the necessary logistical operations in support of the total R.& D operation. This approach brings into full view, as pointed out by Eric Walker, Earl Stevenson, and others, the difference between the administration _of_ research (by the research manager) and the administration _for_ research (by supporting personnel), both of which are necessary parts of successful research pursuit in modern technological undertaking.

Discussions of matters in both these areas are interspersed throughout the Proceedings, though the speaker in each case had no intent to give full exposure of all the subjects in the latter category. Thus no exhaustive treatment or detailed description of those supporting operations making up the area of administration for research is intended here. Rather, the important stress is on the research manager's role and the approaches he takes personally in coming to grips with the various parts of that role.

Planning the Research Environment

Before we can adequately discuss the planning of specific research efforts and their control and evaluation, we must first consider more fundamental factors which establish the research environment in which the research director operates. These concern the overall planning for research and they involve such factors as how the research to be done is related to the interests of the total organization of which the research effort is a part, whether the intent is to produce new knowledge or new products, or whether profits or superior products are the expected overall result. Planning for research, then, must begin with an analysis and understanding of the purposes and objectives of the total organization. As we have seen in Chapter II, these objectives differ radically between (and sometimes within) the industrial, university and government sectors. In his planning, therefore, the research director must first consider the purposes and objectives of the total organization, firm or business and then seek research purposes and objectives consistent with them.

For the industrial sector several speakers in the early Conferences clearly laid out what they felt was the desirable tone and rationale. In the First Conference in 1947, Blaine Wescott, a long experienced industrial researcher gave this in a few words:

> The most common objective of industrial research is the maintenance and improvement of the competitive position of the sponsoring company....

In later years a number of others, speaking on industrial research, confirmed, extended and amplified this simply stated purpose but in no way basically altered it.

The purposes of universities and academic research were perhaps best given in 1949 by the late Hugh Dryden and by Raymond J. Woodrow (1962), a veteran researcher and university administrator. Both Dryden and Woodrow summarized the university goals as follows:

1. The education of students
2. The advancement, preservation, and dissemination of knowledge
3. The advancement and protection of the public interest and welfare

The advancement of knowledge, through research, is as important to the total university purposes as research for new products and services is to industrial firms.

Governments must meet their public, social and economic needs and maintain and improve their political and military positions. As noted earlier in Chapter II Admiral F. R. Furth, pointed out in 1955 that these purposes are clearly implied, if not stated explicitly, in our Federal Constitution. To accomplish these overall objectives government must engage in and support a wide variety of research efforts which encompass a spectrum of requirements ranging from defense to the general welfare.

Thus the questions of what kinds of research are largely answered within a context of the purposes and objectives of the parent organization. From these basic commitments one can establish overall research policies and plans for research. From these considerations also flow the planning of supporting factors which are fundamental in establishing a research effort with the appropriate men, facilities, equipment and materials, money and supporting services.

Planning for research also most include how to organize for the most effective uses of the expected resources. Should all or part of the effort be organized along functional project and research team lines rather than upon departmental or disciplinary concepts? What supporting services must be established; how shall they be arranged? What kind of laboratories shall we build and how and where should we build them? How much support of others' research shall we undertake and whom shall we support and under what policies? The research director bears a heavy responsibility in this shaping of the research situation to match the total organizational needs and desires. And in answering for himself all these questions he must have the backing of his top management within the firm or organization.

The Proceedings of the National Conference provide clues for management action on these problems and what follows is a digest of experiences related to these problems.

First, let us consider the matter of organization for research. In the First Conference in 1947, G. H. Young, examined the advantages and disadvantages gleaned from forty years' experience to that time in operating by the project team method in the Mellon Institute. A summary of the advantages of this method, as he saw them, is as follows:

1. The project method almost automatically develops a feeling of individual and group responsibility for the successful completion of the assigned task.

2. Complete and absolute concentration of research effort on a single problem or group of closely related problems in a given field is not only possible but virtually guaranteed under this type of organizational scheme.

3. There is little or no wasted motion in getting under way. Undiluted attention to the job at hand is a major characteristic of project-organized research.

A summary of the disadvantages, as he saw them, is:

1. The system tends to develop "specialists" rather than widely experienced and highly adaptable senior personnel. One, therefore, is being constantly faced with the problem of how to utilize long experienced but rather narrowly confined researchists when--as inevitably happens--a given project finally terminates.

2. The very fact that each project is designed to function independently of every other, means that there is a considerable duplication of basic laboratory equipment and supplies. A high wastage factor for chemicals and the like is thus unavoidable.

3. Costs for special apparatus, shop charges, secretarial services and the like are very likely to run higher because they cannot by their very nature be utilized fully all of the time.

4. It is impracticable to employ ultra-advanced techniques requiring costly and elaborate instrumentation on a project-wide basis. Thus, if these are to be available at all, the so-called project-organized laboratory must have at least a few "service departments" associated with it.

5. They tend to become inbred and ingrown if not constantly guarded.

6. Project-organized laboratories have no easy way of creating and maintaining any kind of reserve pool--a practice which is useful and relatively simple to handle within a departmental framework.

These disadvantages constitute factors that management must carefully observe in controlling the operation. Young also laid down the requirements for successful project-type operations as follows:

1. The first requirement for successful operation of research organized along project lines is to select a key man around whom each project will grow.

2. Full provision for additional help in the way of junior grade assistants, of adequate and diversified apparatus and equipment, and of staff support.

3. A third basic principle in sound administration of project-organized research is the accurate forecasting of probable costs in expendable materials, men and money, and the provision in advance, of funds upon which the field commander may draw when needed, not at some unpredictable future date after "board approval" or other frustrating and disheartening delays so deadly to the research temperament.

4. A fourth basic principle in organization of research by projects, that individual choice of team members, with the closest possible attention to compatibility factors consistent with diversification of trainings and skills brought to the team, is imperative.

5. A fifth tenet in organizing research by individual projects that recruiting multiple groups slowly, with time for amalgamation between each addition, pays in the long run.

C. E. K. Mees (1947) and others, however, had reservations about the project system. Mees' statement well sums up the opposition.

> Thus it seems to me that you can work a project system if you know what you want to find out in the laboratory, but if you don't know what you want to find out, it's just a waste of time to put it in a project system.

With the exception of universities, and some government laboratories most research and development effort to that time and subsequent had been accomplished on a project basis. Wayland Griffith, an industrial research leader, reiterated this (1962) and further delineated how the project-team approach can be effectively used in a systems approach.

> The nature of research in our industry is such that neither the traditional academic grouping of scientific disciplines nor the customary association of fields with commercial product lines is appropriate. The concept of program systems management is reflected in the requirements and objectives of research in the sense that a strong interdependence exists between nearly all aspects of both research and engineering activities. Only very broad groupings of disciplines are meaningful at all. Descriptions of functions and roles in terms of traditional designations, such as aerodynamics or metallurgy, are quite misleading and, in an atmosphere of rapidly shifting technology, temporal at best.

Use of the project-team method in cases where "you know what you want to find out" in no way excludes individual disciplinary and departmental approaches where "you don't know."

Dean Henry Masson (1952) seems to have aptly described the concepts for organization of research in a university or similar environment.

> At universities there is, so far as research is concerned, less formal organization. That is, the environment is academic rather than that of a research division. The professor and his graduate students are rather free and independent; and they have, in general, complete freedom in the selection of problems for investigation limited only by space, facilities, financing, good taste, and the injunction that the results be a contribution to knowledge. This freedom is of paramount importance.

Masson's expression of the rationale for university research seems to alloy Mees' objections to the project-team method for organizations operating in the basic research area, in that it clearly allows for pursuit of knowledge without proscription or circumscription.

As already noted, there are several other areas to consider in planning for research beyond the decision of organizing along project or functional lines. Some of these have received exhaustive treatment in the Conferences, but not all. Others have been addressed only sporadically.

For example, the planning for successful research necessarily includes decisions about where to build laboratories in relation to the total organization and also how to equip them. In 1954 a part of this subject was discussed in some detail, particularly with regard to architectural and construction features. No where, however, does there appear a full discussion of the rationale involved in where and how to build laboratories, and why. Later (in 1964) the Conference (see Chapter III) deals with the development of research parks and thus takes up, in part, the question of where to build the research laboratories but does not exhaustively treat it. Perhaps later Conferences should include further discussion in this area of developing interest.

In building his research environment and operations, the research director must also make provisions for recruiting, selecting, placing, evaluating and training of research and supporting personnel. Management of research personnel is a critical part of the operation for two reasons.

First, trained scientists and engineers seem always in short supply and for that reason, as well as the desire for maximum efficiency in the operation, the research director must give much attention to research personnel problems.

Second, and perhaps obvious, is that the research scientist is the key resource in any successful research pursuit. That this is so was stressed (1962) by Harold Gershinowitz, a leader in industrial research, when he said:

> To use his people properly, to make sure that he is using their skills on projects which are of importance to the organization for whom he works or of which he is a part, is probably the most important job of a research administrator.

Earlier (1956), Merrill Flood, educator and public servant, pointed out that a central problem confronting every research administrator is the selection and assignment of research personnel. Thus, in planning for research, the director must understand that these are his key problems and the effectiveness of his planning for selecting and using human resources may well be the basis upon which he stands or falls in his subsequent pursuits.

Planning of the research house and the people to be brought to it is usually accompanied by the setting of policy with regard to the annual budget to be invested in research--what in many quarters is thought of as the "volume" of resources. In the modern economic world, all investment costs and returns are reduced to a dollar basis for measurement and analysis. This is no less so in research, regardless of whether new knowledge, products or profits are the expected result. In the earlier days many industrial firms took up the idea of investing in their research some small percentage of gross sales. Fred Olsen reported in 1949 that Olin Industries had adopted a plan of 3% of net sales for research. Other firms developed different formulae for determining the yearly volume and then attempted to develop the proper yardstick for measuring returns, a subject for later discussion.

Later there was further discussion of the question of "how much research" and the idea of basing today's research volume on other current financial or economic factors was seriously questioned. James C. Zeder (1950) had this to say:

> In some companies the idea prevails that a fixed percentage of each sales dollar should be set aside for research. At Chrysler Corporation we are definitely opposed to any such rule. A good research program depends on an intelligent understanding of the job to be done, not on a knowledge of how much is available to be spent. When you have more money than projects, the research director has to look around for additional ways to spend it, and you have robbed him of the stimulation of having to compete with other divisions of the corporation for his budget allocations. On the other hand, during periods of low sales volume, the fixed percentage system may result in drastic reductions in the research program at the very time when research should be expanding instead of contracting. It is far better for the corporation if the research department is required to sell the management on every dollar of its appropriation on the basis of the probable benefits to be derived from the projects undertaken.

Still later (1962) Thomas Carney seems to have summarized the feelings of many of his fellow industrial research directors on this point as follows:

> Certainly there is no objection to reporting a research and development budget as a per cent of sales. But I think it should be recognized for just what it is--an exercise in arithmetic. There should be no great significance attached to it. Certainly there is a connection between a research budget and sales, but the connection is that research generates sales. The difficulty lies in accepting the fact that the dollars of research spent this year do not generate dollars of sales until five years or possibly more have passed. But all too frequently managements seem to believe that there are only two alternatives to follow in their relationship with research. The first is the traditional "leave-the-scientist-alone-and-he-might-be-lucky-enough-to-stumble-onto-something" approach--and the other is the "let's-organize-hell-out-of-them" approach. Obviously, either of these alternatives is ridiculous in view of the operation of today. Companies that have used them in the past have either changed or gone out of business.

The question of how best to determine the volume of research effort is not yet resolved. More recent industrial thinking reflects the approaches of Zeder and Carney. The volume of research in a university is, for the most part, a reflection of the size of the staff and the balance to be maintained between teaching, research and services. The volume of research in governmental laboratories is subject to many factors, such as the policies for supporting research by others, the depth of government's commitment to given objectives, and the like.

In his planning for research, the research director has additional questions to resolve, if he expects to create and maintain a productive team. These involve creating a system of communications that takes into consideration information flowing into, within, and out from the organization. Additionally he must provide for policies and procedures which protect and regulate information flow wherever this is in the interests of the organization. This latter requirement extends into questions of patent and proprietary information policies and procedures, as well as those necessary to protect national security, if appropriate.

The basic source of information flow into a research operation is, of course, the scientific literature. Any substantial research effort must, therefore, be supported by a technical library. Such a library, however, can never satisfactorily serve as the only source of information for the organization. A scientist also depends upon personal contact with his outside associates through such activities as meetings, seminars, consultant services, and the like. It is important, therefore, that the research director establish clear policies about these matters. Oddly, the Conference proceedings reveal very little guidance on the subject of flow of information into an organization, although various participants made incidental pleas for liberal budgets to be devoted to travel, outside study, consultants, as well as printed materials. The need for outside contacts seems to have been taken for granted by Conference participants.

Communications _within_ a research organization are usually thought of as two kinds, serving two purposes. One is informing management and the other is informing associates. Both are equally important to the vigor and survival of any research organization. However, both present problems for the director in his desire to make them effective. The importance of assuring good internal communication was emphasized in 1954 by General William Creasy, then head of the Army Chemical Corps, when he remarked:

> In management, there seem to be three things that are of primary concern to the manager: One is the organization, how you divide the work up into chunks and relate one to the other; the next is the people; and the third is those nasty areas of administration, paper work, budgets, reports and so forth. Communications, it seems to me, binds these three essential elements. They do not work together automatically, they are bound together by communication.

Vertical communication in the upward direction often takes place in predetermined ways. Management most often decides the type, format, frequency, etc., for information it wants supplied about the research operation. Once established, these standard formats provide ease for communication upwards to serve management need. Communication downward often presents harder problems. In its desire to inform in an adequate way, management sometimes establishes systematic internal methods. One such method was outlined by Helmut Landsberg (1954), eminent government research leader. Landsberg described what he called the research director's weekly bulletin. His remarks about it are applicable to all such media:

> ....the main purpose of the bulletin is to satisfy the curiosity of people in an organization about what goes on behind the scenes, especially in the front office. That is particularly true in government, I suspect that it is true in other organizations too.
>
> You want to get facts to the people and you want to dispel rumors. I believe this is one of the important purposes of the bulletin. A sheet of that type can also help to build esprit de corps among your people. You can focus on important aims; you can present collective praise that often does not get around to everybody otherwise. Through the bulletin information comes directly to all hands, rather than through a chain of command from the director, through his immediate associates, to section chiefs, unit chiefs, and so on.

Landsberg went on to point out that such a bulletin sometimes helps materially in lateral communication between associates as another important channel of intraorganization communications. Other methods of lateral communication need be considered, such as seminars, program and project review, abstracts, reports, and the like. The research director has no easy task to convince top management and other departments of the organization that many hours and material resources must be expended on these seemingly peripheral activities in order to provide the motivation and basis for creative action on the part of the researcher. But, as

Landsberg pointed out, communication is the essence of organizations, especially of those involved in creative efforts.

Flow of information from a research organization takes many forms and follows many channels The two principal kinds of information flowing outward are, roughly, technical and non-technical. And in this case there are usually three classes of recipients: technical, non-technical and management. Each must be addressed in language which it understands in order to be influenced in the expected way, which is, of course, the basic purpose of any communication. As previously mentioned, management wants information about the research effort in the form of what's being done, what it costs and some idea of the actual or anticipated returns. Scientific and technical associates want to know what's being done and results from it. The non-technical recipient (often the general public) wants to know what's being done and how it affects people in general. To satisfy each of these wants the director needs to arrive at individualized procedures.

Inherently the scientist and researcher wants credit for his efforts and discoveries; most research organizations go to great lengths to encourage him to publish his good works. The main constraints on his doing so are proprietary, patent and national security interests. In general, government organizations and their contractors limit dissemination mostly in the interests of security; industrial firms limit it mostly in the interests of patent protection; and universities limit it only when required under sponsoring agreements.

To serve the many and varied interests involved in information flow outward, most industrial firms have adopted clear policies and practices to regulate flow in order to retain patent control. This usually constitutes an agreement on patents and inventions wherein the employee agrees to approval of publications or disclosures of technical data prior to release and assignment of all inventions to the corporation. In addition, they often require certain rights to patents evolving from work they sponsor outside the organization. The extent of reserved rights most often depends upon whether the sponsor envisions commercially important results which he desires to protect.

In general, government agencies also require assignment of patents to the government for work they support. This, however, creates some problems, particularly in regard to commercial exploitation, if desirable. Government ownership means that the patent is in the public domain and thus, if heavy private investment is required for exploitation, it is difficult to offer sufficient exclusive protection for a private firm to do so. Government practices on patent protection for government supported and sponsored research vary widely. Most agencies have followed the practice of securing for the government a royalty free, non-exclusive right to use the patented information.

University patent policies and attitudes are of wide diversity and no two institutions appear to have the exact same pattern. As Archie Palmer pointed out in 1949:

> Existing practices vary from strictly drawn patent policies to laissezfaire attitudes, and even an unwillingness on the part of the institution to become concerned with patents. Educational institutions fall roughly into the following categories with respect to their attitudes toward the handling of patents:

> (1) Those which take the position that the institution has an interest in all research activity on the campus and therefore recognize and exercise institutional responsibility for the proper administration of all discoveries and inventions growing out of such scientific research in accordance with formalized patent policies.
> (2) Those which do not have formalized patent policies but are prepared, in accordance with generally accepted procedures, to consider any patent questions submitted by faculty or staff members, leaving the initiative to the individual inventors.
> (3) Those which do not have any formalized patent policy or generally accepted procedure but consider each case as it arises and according to its individual merits.
> (4) Those which observe a hands-off attitude and do not concern themselves institutionally with patent matters, leaving to the individual inventor the responsibility for determining what disposition should be made of the patentable products of his research efforts.
> (5) Those which observe the definite policy of not having a patent policy.
> (6) Those which have as yet no policy but recognize the need for having one and are seeking guidance in determining what type of policy should be adopted.

In deference to those universities seeking guidance, Palmer pointed out that there is nothing dishonorable or "wicked" about a scientist's seeking a patent or financial return, personal or institutional; ethical advantages can accrue for individuals just as well as for institutions.

University attitudes toward patent considerations in the area of sponsored research also vary widely, ranging all the way from desiring full possession to refusing research that may involve patentable developments. There seems no consensus in this area nor in the area of sponsored effort that is militarily classified. Many universities refuse to accept the responsibility to perform in the area of classified information on the basis that such efforts do not truly concern basic research or normal academic pursuits, and thus provide few results which can be used in the instructional programs.

Whether he is involved in managing research in any one of the three major sectors, the research director must be ever mindful of the conflict of needs lying between the desire of management to protect the organization's interests and those of the scientist and the university to publish freely in the individual and public interest. Indeed it is a sensitive and mature research director who is seen by his subordinate staff and his corporate or organizational superiors as a manager who can keep these conflicting values delicately and effectively balanced.

Planning, Programming, Controlling and Evaluating Research Operations

A research director usually sees himself involved in pedestrian tasks which seem to absorb all his energies on a day-to-day, week-to-week and year-by-year basis. Less often does he see specific opportunities of building and rebuilding the environment for research, though he recognizes that his role in continued research operations directly affects this environment. Also his role in managing continuing operations is rather inseparable from the critical job requirements one hopes to train a research director to be able to fulfill, either formally or through meaningful experiences. Thus when a research director describes the basic requirements of his job he always emphasizes that he must be able to choose, plan, schedule and control the original program of research and later, to evaluate the results of the research and appraise the effectiveness of his personnel.

Methods vary widely for evaluation of proposed research, for determining or estimating the economic and other risks involved in starting or continuing an effort, and thus for the total task of planning of the research efforts. There may be as much or more variance in methods within a research sector, such as industry, than there is between two sectors such as government and industry generally.

Any orderly analysis of the consensus on how research efforts should be planned and scheduled needs a basic schematic of the research process as a reference. Against this the various inputs of many Conference participants can be summarized. Several such schematics have been outlined but an early one presented in 1952 by Dean Henry Masson is a procedural sequence which seems valid for this purpose. It is repeated here for the reader, without further embellishment except to note that not all steps apply to any one firm, organization, or university's research business, and thus, suitable modifications are in order.

PROCEDURAL SEQUENCES IN RESEARCH

(1)
ENVIRONMENT FOR RESEARCH

↓

(2)
INDIVIDUAL OR ORGANIZATION
FOR RESEARCH

↓

(a) Conversations-Meetings
(b) Observations
- Laboratory
- Clinic
- Field

(c) Reflective Thinking
(d) Flash Idea

(3)
SOURCES OF PROBLEMS
FOR RESEARCH
(Thought Starters)

(e) Research Staff
(f) Sales, Mfg., and Other Depts.
(g) Customers, Stockholders, Etc.
(h) Government Agencies
(i) Advisor-Faculty
(j) Sponsored by Industry

↓

(4)
CREATION OF AWARENESS
OF PROBLEM
TO BE INVESTIGATED
(Incitation)

↓

(5)
THE PROBLEM TEMPORARILY
DEFINED—OBJECTIVES

↓

(a) Market Search-Sales Staff
(b) Manufacturing Facilities
(c) Research Staff-Faculty Advisor
(d) Space and Facilities

(6)
FEASIBILITY

(e) Financial
(f) Preliminary Literature Survey
(g) Exploratory Experiments

↓

Budget Approved

(7)
INVESTIGATION APPROVED
EXACTLY DEFINED AND
CIRCUMSCRIBED

Duration Determined

↓

(8)
INVESTIGATOR
OR
STAFF SELECTED

↓

(9)
FORMALIZATION
OF DETAILS

(cont.)

Known Areas Segregated

(10)
EXHAUSTIVE LITERATURE
SURVEY—BIBLIOGRAPHY

Unknown Areas Segregated

(a) Paths Laid Out
(b) Working Hypotheses Formulated
(c) Statistical And Dimensional Analysis

(11)
EXPERIMENTAL
DESIGN

(d) Physical Layout Planned
(e) Eelection Of Equipment and Instruments
(f) Progress Schedule Prepared
(g) Record Designed

(a) From Stock
(b) Purchased

(12)
EXPERIMENTAL EQUIPMENT
BROUGHT TOGETHER

(c) Constructed
(d) Loaned

(13)
EXPERIMENTAL EQUIPMENT
ASSEMBLED

(14)
TRIAL RUNS

PROGRESS REPORTS
SEMINARS
TECHNICAL MEETINS

(15)
EXPERIMENTAL RUNS
DATA GATHERED

(a) Observation
(b) Not Making Progress
(c) Abandonment
(d) Expansion

(16)
EXPERIMENTAL
PHASE COMPLETED

(17)
DATA ASSEMBLED

(a) Computations
(b) Plotting Data
(c) Preparing photographs, samples, models, etc.

(18)
INTERPRETATIONS

(a) Hypotheses, theories
(b) Conclusions
(c) Recommendations

(19)
FINAL REPORT
RESULTS OF INVESTIGATION

(23)
REDUCTION
TO PRACTICE

(24)
PILOT PLANT
LABORATORY

(25)
SEMI-WORKS
LABORATORY

(26)
MANUFACTURING
PLANT

THESIS

DEGREE

(20)
APPLICATION

(21)
PUBLICATION

(22)
FURTHER
INVESTIGATIONS

(26) MANUFACTURING PLANT → (27) SALES → (28) PORTION OF PROFIT FOR RESEARCH AND DEVELOPMENT → (3)

With this outline in mind, one can describe the various methods used to get research underway and reflect the role played by the research manager in the subsequent steps.

In sketching out the procedural sequences, Dean Masson displayed some of the interrelationships which are interwoven into any specific research situation. Effective action of these points is primarily dependent upon the application of modern management methods by the various managers and directors as the research effort is planned and as it proceeds. In general, these methods involve some systematic ways of evaluation, some determinations of the "calculated risks" at the beginning and at particular stages in the process, and some use of cost control and accounting procedures. Since the time of his outline, however, there has been considerable transition and elaboration of thought about research planning and evaluation in the Conference discussion.

In the area of planning the research effort in an industrial organization, we can identify two opposing techniques, the "inside-out" and the "outside-in" as expressed by Karl Von Tassel in 1964. Just after the Second World War, the general feeling was that research should be left outside the realm of overall management and given maximum freedom to go its own way. Dr. C. E. K. Mees of Eastman Kodak defended the anti-management school very well in 1948. He said then:

> ....I sometimes wonder what all the stuff about the management of research really is. We don't even go so far as to have a project after a thing is beginning to succeed....This chaotic method of running laboratories without any projects, without any foresight, without any knowledge of what is going to

> happen in the future works perfectly well. I have come to the conclusion that there is no problem finding things worth applying. They come of their own accord if you just do scientific work.

But Van Tassel contrasted this school of thought with the opposing "outside-in" technique, which holds that complete freedom of choice is too expensive a gift for the industrial research organization. He described it this way:

> This management approach suggests that it is not enough merely to establish a research and development organization for the purpose of somehow assuring a company's growth. It points out that too many of the technical results emerging from research prove to be of little utility when measured against the necessary criteria of production, marketing, distribution, and finance. Thus, research and development objectives must be set by business management, and moreover, R and D must be closely controlled and coordinated with other business functions from the outset. : is just as easy to be creative with ideas that are profitable to the business as with ideas that are irrelevant.

It is well also to contrast the above industrial planning philosophy recently expressed by Van Tassel with that expressed by Mees a decade and a half earlier. Mees had said bluntly that the best persons to decide what research shall be done were, in order of best to worst: first, the man who is doing research; then the head of the department within the laboratory; and then, getting much worse, the research director, the research committee, and worst of all, the committee of company vice-presidents. Donald Loughridge (1950), in speaking on the subject of calculated risks involved in research in the government sector, reiterated a slightly different philosophy for the more ndamental government research efforts. He said then:

> The best way to conduct research is to decide on the field

> of investigation, to explore these fields, and to feel one's way into the unknown, allowing competent investigators to follow the paths which in their competent judgment seem most promising.

This problem of placing the responsibility for deciding what research should be done appears to have no single answer for all organizations or for all time. For some cases, however, there are specific answers as suggested by the scholarly work on this problem by Donald Pelz, who made a report to the Conference in 1963. In the conflict of the individual and the organization, he decided that the answer does not lie in compromise between autonomy and control. Pelz asserts that a research director should provide "channels for vigorous communications for two-way influence" between the scientist and those who manage him. These results seem to bear out the experience related by Van Tassel the next year that the most effective planning methods involve a complex blend of the inside-out and the outside-in techniques. By this approach one may reasonably expect to avoid either complete domination by management or exercise of complete freedom by the researcher, neither of which alone seems to be an effective way to plan research. These two extremes were well expressed by an exchange between A. B. Bronwell and Fred Olsen in 1949.

> Mr. A. B. Bronwell, Northwestern University: So often engineers and scientists are little more than robots, carrying out the master plan given them by their superiors. Is there any satisfactory method of getting creative ideas out of research staff employees, i.e., of encouraging them to advance their own novel and constructive ideas and simultaneously maintain the efficiency necessary in operating a research organization?

> Mr. Olsen, Olin Industries: I have one very simple answer: that is, don't squelch the research man by throwing out his ideas. I mean it very literally.

There is no consensus in the Conference proceedings on the best approach here. Some, but by no means any large majority, of the government and industrial laboratories claim to follow the Mees philosophy; however, in the universities the approach to the procedure for determining what research shall be done often differs markedly from that of the governmental or industrial sectors. In 1962, Ray Woodrow, rather well summed up the university approach.

> Decisions are made and implemented with what others would consider very little direction and control from topside officers, but much more through a procedure which has been described as a system of "colleague authority." In other words, the members of a university faculty, both collectively and individually, exercise much of the responsibility and authority for what the university does in the way of research. Instead of an industrial type pyramid, the university is in a sense an inverted pyramid with the faculty all nearly co-equal along the top, and with the research administrator near the bottom apex.

Woodrow went on to explain how this system of "colleague authority" works at Princeton University. His description seems to outline a typical pattern of university research which fits well with the more modern concept of "two-way influences" upon the decisions required by Dean Masson's schematic of procedural sequences.

Discussion of the question of who decides what research shall be done, out of all that is possible, raises the provocative question of what the director should do about "bootleg research", as one anonymous 1958 Conference participant termed it. There seems general agreement that "bootleg research" designates efforts which have not been made a part of the approved research program or that may never be, yet absorb resources officially budgeted for some other particular effort. The subtlety of dealing with this situation depends upon avoiding the curtailment of the individual freedom and motivation of the scientist within the organization without, at the same time, surrendering control of the program content.

In discussion of this problem in 1958 several research directors espoused the view that, in accord with management's desire to provide the researcher maximum allowable freedom and to secure from him new ideas, such efforts should be encouraged and supported by a budget framework which accomodates a reasonable amount of unofficial and untitled exploratory type effort. Other directors disagree, particularly if the effort is diversionary or unknown to management, or if it in some way interferes with what either the researcher or director has committed himself to do. Although some discussants felt that any "undercover" work is objectionable, others felt that satisfactory arrangements and policies can and should be established to make very clear the allowable limits for this type of individual or collective activity. Most directors seemed to agree that specially budgeted funds should be approved for exploratory and feasibility efforts. Eldon Sweezy suggested some liberal guidelines which would allow a reasonable amount of time to be spent on such efforts, so long as the employee does not steal from one client for the benefit of another, does not let such effort interfere with his assigned effort or does not interfere with someone else's ability to perform his assigned tasks. A clear understanding between management and the researcher, rather than a tacitly accepted but cladestine arrangement, can do much to enable the director to retain reasonable program control without unreasonable constraints on individual freedom.

Decisions about what research is to be done and how much, as a broad policy consideration, cannot be reached independent of resolving questions about when and how the effort shall proceed. As soon as the time factor is brought into play, especially in the application and development portions of the research and development spectrum, the particular project becomes deeply involved in programming or scheduling factors.

Out of the many discussions about placing time factors on research, and more particularly on development effort, a certain consensus emerges from the Conference Proceedings. For instance, if the results desired from the effort can be described--the objectives are clearly known and stated--then management can and, in fact, must derive an orderly time plan for the effort. This includes at least the approximate dates of expectancy in arriving at results, positive or negative. And here time becomes an especially important factor in evaluating the effort throughout the total process outlined by Masson. Recognition of this need, along with the desire of management to use the more modern concepts resulting from operations and management sciences research, has given rise to such well known systems as Critical Path Method, PERT, and others, for control and continuing evaluation of research and development efforts in terms of cost and time.

No exhaustive treatment of the details of the various proposed and practiced methods of scheduling, controlling, cost accounting, and evaluating research efforts has been the subject of these Conferences nor is intended here. It is interesting, however, to note that early after World War II most industrial firms, as well as government agencies, were seeking formulae by which they could arrive at the necessary management decisions about calculated risk, the value of starting, continuing, or stopping a project and the value of the results. Most of these formulae were simple or, in some cases, complicated versions of that offered earlier by R. E. Wilson and quoted later in 1949 by Fred Olsen. In essence: the estimated return multiplied by the estimated probability of success divided by the estimated cost of the research equals the "Index of Return." Some research directors were confident this formula would produce useful evaluations, at least comparative ones, if one could somehow sharpen his subjective methods of estimating and could know and control the various costs with which he is concerned.

As advances in computers and mathematical methods have created new management tools, most of the earlier techniques have undergone transition. The new techniques for decision-making depend primarily upon the availability of considerably more data, and the probabilities of making it more timely and meaningful.

Both by extended experiences and by having better data, the research director now can sharpen his estimates and probabilities. A research director can be trained to use these modern tools effectively, thus acquiring a more solid basis for applying his technical expertise in research administration. Merrill Flood pointed out (1956) that these tools properly used, can free the manager from routine tasks so that he can concentrate on the basic objectives of the effort.

Such an outlook is a far cry from the attitude towards costs described by Mees in 1947. Mees observed:

> What use are costs of research work? If the research succeeds, the cost is of no importance. No research costs anything compared with its value if it is successful. If the research doesn't succeed it doesn't matter at all what it costs. It shouldn't have been done. So again: What use are costs? I think some sort of cost is necessary for budgeting. In my complete disregard for the value of cost accounting in research, I don't disregard budget accounts.
>
> Budgets are very important, and I have found it necessary to put a Scotchman in charge of my budgets. The result has been that our Comptroller has had no interest in our costs. All he needs to know is how much we're going to spend. It is still convenient to know rough costs, because when you come to budgeting, it is desirable to know what your departments are going to cost, and you can't budget entirely on the number of men you're going to have. So I am not entirely opposed to costs. I just think the cost of laboratory cost accounting should be kept in its proper place: About one fifth of one per cent of your staff.

It is hard to visualize many industrial firms today in which top corporate management would condone such a concept for financial management of its research and development effort. The research director simply must have suitable means for knowing and controlling his costs if the direction of research is ever to become professional.

Evaluating on-going research efforts in a government or university setting encounters quite different aspects as compared to industry. Neither the public service nor the academic environment has returns that can be measured in dollars of profit or savings, except savings resulting from reduction of the costs of the research itself, through employing more efficient processes and securing better management data. James Brian Quinn pointed out (1957) that two types of evaluations are necessary: technical and economic. For government research it is hardly possible to employ completely the latter. But Quinn's description of the scope and method of technical evaluation seems particularly suitable. He put it this way:

> Technical evaluation involves making judgments concerning the adequacy of the period's technical accomplishment without regard to the ultimate economic consequences of the technology created. Thus technical evaluation requires appraisal of: (1) the efficiency with which the planned technical results of the period were achieved and (2) the quality of the research work which was performed. Efficiency evaluation compares the actual time and cost consumed in accomplishing a planned objective against some standard (in the same terms) for how much it should have cost. Evaluation of the quality of research work requires an appraisal of the creativity, scientific skill, and technical proficiency with which research was accomplished. Like efficiency and quality evaluations in production operations, these two appraisals are intimately interrelated. Whether consciously or unconsciously, research executives actually do appraise these aspects of the research program. Such evaluations are a portion of the day-to-day administration of research.

The recognition of the necessity to give continous evaluation to government and government-supported R & D efforts in the absence of measurable dollar return but against time-critical requirements for field equipment and techniques was no doubt the basic motivation underlying the establishment of what eventually became the PERT methods now used. Kenneth McKay (1963) seemed to sum up the outlook still current on the use of PERT type systems for planning and controlling. He had this to say:

> The most popular form of planning-reporting is, of course,

> PERT. It has been associated with some very successful projects, is *de rigueur* for all major DOD projects and has been widely adopted by industry for privately sponsored activities. As we work our way through the lists of predecessor events and successor events towards our critical path determinations, we recognize that we are carrying out in a formalized way what a good project engineer has been doing all along. The judgments and decisions must still be made as they have always been made. However, PERT is a formalism that permits complex systems to be evaluated methodically, and it tends to force the not-so-wise project engineer to get on top of his job. It provides managers with reports in a standardized form which are of real use provided the managers continue to recall the assumptions that underlie the reports. How PERT should be used most effectively is still being tenderly explored.

Beyond these systematic planning and managing methods and procedures, management needs constantly improved methods for assessing the results of research, in terms of the purposes and objectives of the total organization, and for translating these results into practice in the organization's affairs. In the early days of the National Conferences there seemed much doubt about whether valid guidelines or indices could be developed for these purposes, especially for the individual projects and individual results from them. Guy Suits, outstanding as a research leader, clearly exemplified this outlook when he said in 1950:

> I have developed the thesis that measuring the return from research is difficult because of the manifold ways in which the beneficial results of research manifest themselves and because of the important and interrelated contributions of engineering, manufacturing, and marketing to a research result which attains a market.

Also in the 1950 Conference the consensus of round table discussion on this subject as expressed by LeRoy Brothers was:

> The discussion centered around indices as a means of

> measuring the return from research. Agreement was reached on the view that the use of indices generally should be restricted to development, to operations research, and to applied research, and that they should not be used for fundamental and basic research.

In later Conferences, others spoke about evaluation of results and more especially about translation of results into further developments and further developments into useful or marketable products. In 1951, General Leslie Simon spoke from Army Ordnance experience on the subject of bridging the gap between research results and useful military products and described a system under which the scientists in research join with the engineers of product design in a team effort for evaluation of research results and their place in design and development of specific new field items and military systems. Simon emphasized that there seems no good substitute for intermixing these kinds of personnel if the task is to be done effectively. He said it this way:

> It is extremely important for the management of research and development establishments to determine how to integrate the scientist and the design engineer into a smoothly operating team in which the talents of each are exploited to the utmost.

Simon cautioned that one cannot go so far in this scheme, however, that he depletes or negates the further scientific effort on the part of the scientists as a group or as individuals. In a nutshell, the research director needs to remember that:

> The organizational and administrative problem, then, is one of achieving a maximum of the scientist's contribution with a minimum of tax upon his scientific endeavors.

Again in 1958, C. I. Johnson of General Electric emphasized the building of centralized teams of various specialists for continuous evaluation of research results. Such a group can concentrate on assessing the potential for new and effective products. This process cannot be achieved with the same degree of effectiveness merely by decentralization of the evaluation to the various specialists.

The conclusion to be drawn here, then, is that all the present yardsticks seem to depend upon personal participation and judgment of various specialists throughout the R & D effort in the assessment and application of results from research. The technical director in this process must act as the keystone of an arch that bridges research and design. He must construct this arch from specialists, each of whom can contribute to the evaluation and transformation of research results. This concept is no less true in university research where the newest ideas, concepts and results are sought for the improvement of instructional content and methods.

In addition to evaluating the on-going research and the results from it, there are other important assessments to be made. Throughout the entire process of research and development the research director must continuously assess the performance of his research personnel. Having recruited them and placed them within the research organization structure, he must then begin to evaluate how well his research specialists are doing their part of the assignment. In general, his role here as leader, manager and director of creative human individuals is, no doubt, the most intricate and taxing part of the director's function. For this reason we devote an entire chapter (Chapter VI) to describing certain aspects of human behavior which a research director must understand in order to achieve effective individual and group effort from a staff of highly creative individuals.

Evaluating the performance and efficiency of research personnel is handicapped by a lack of objective measuring techniques. For the most part the manager has only subjective tools with which to work. Nonetheless, such evaluations are usually the basis upon which pay, advancement and other awards and personal rewards are decided. Consequently, most research organizations have developed some mechanisms for rating the performance of their technical personnel, rather than leaving this matter entirely to the research director.

Schemes for rating the performance and efficiency of research personnel have followed two approaches, either taken singly, or more often, in combination. One form involves rating by peers and the other rating by superiors in the organization hierarchy. The system used by General Electric for evaluation and reviewing compensation, outlined in 1948 by Guy Suits, compares very closely with others described in later Conferences. It requires rating of performance on a scale of 0 to 10 for eighteen selected performance criteria. Two sets of ratings are made independently by several of the scientist's associates and by his immediate superiors and then combined for a composite rating on the individual. Ratings of all scientists in a large area of or in the total organization are then composited and plotted and a median rating determined. Then the salary of a particular individu xamined against a plot of performance, age and length of service yze whether his performance justifies a higher or lower salary i arison to his peers. Suits points out that the general and the relative levels of compensation must be compatible if morale problems are to be avoided and that this system minimizes the probabilities of gross errors in judgment.

The system Suits described seems to meet the critical points in evaluating personnel stressed three years later in 1951 by John Flanagan. Flanagan pointed out that directors need a planned system for getting essential facts on the performance of research personnel if they are to be objectively promoted (or eliminated) and developed and if their future performance is to be predictable. He also stressed that a systematic method avoids the pitfalls of supervisors not knowing what to observe, not observing enough and not having standards for comparison. Flanagan gave the following principles for an evaluation system:

1. The job must be adequately defined.
   [Flanagan stresses the establishment of "critical requirements" in defining the job so that on-the-job observations may be made by the "critical-incident" technique. This technique involves a collection of reports of behaviors which are critical in the sense that they make the difference between success or failure in particular work situations. Examples might be planning and designing an investigation, preparing reports, etc.]

2. Evaluation must be based on actual observations of performance or products of the job.

3. Observations must be evaluated, classified and recorded.

4. Observations must be summarized, integrated, and put in a form adapted to their intended purposes.

In other words definite plans must be made for use of the data collected. Ratings can be numeric and profiles used to advantage. Flanagan does not prescribe a definite or generalized systematic form to be used. However, he feels that if a director does not devise one to fit his own personnel, he is not prepared to deal with the many uncontrolled variables of human behavior in the same way he would approach a technical problem.

The research director's problems and opportunities regarding the further training, retraining and upgrading of technical personnel have been a subject of recent extended discussion among research leaders The problems involved arise from two factors. One is the current explosive increase in research and, therefore, in factual knowledge. A technical specialist soon faces technical obsolescence unless he is provided means and encouragement to keep apace of these rapid advances. The second factor is, again, the continuing shortage of trained, research people. This results in a desire to make each researcher as efficient as possible. In 1962 the venerable engineering dean, W. L. Everitt and John Macy, Civil Service Chairman and Presidential advisor, agreed that engineering and science are no longer, as Everitt put it, just "learned" professions but "learning" professions. A technologist must remain a student throughout his career, not just in his early years.

At the same Conference in 1962, Monroe Kriegel, long experienced industrial director, put the question of technical obsolescence into context with the growth of our economy and technology. He pointed out that the recession of the late 50's caused a shift of emphasis from increasing the sheet volume of research and development to increasing the efficiency of research as a whole and the proficiency of each individual in the field. He noted that up to now we have rewarded each researcher for good performance in a narrow, specialized area. But as required specializations change, it becomes a major educational task to train a technical man in a new specialty. Often this is beyond the capabilities of the man alone. Moreover, the need is strong (though the time is limited) to teach the researcher and the potential research administrator modern business methods and techniques.

With management's growing recognition of these personnel problems and its obligation to shoulder them, new programs have been established not only to continue the basic education of younger technologists and specialists but to arrange for refresher training for the more seasoned ones. These programs, as inducements, include part or full payment of costs by the employer, and leave of absence with pay. Pertinent short courses, seminars, and other meetings are arranged at either a university or the organization's location. Also, courses have been started for budding research managers. These courses for extending and refreshing are usually highly specialized and thus comparatively expensive. As a result, universities, as well as government organizations and industrial firms, are somewhat reluctant to undertake them. The proliferation now taking place of these methods of continually refreshing and upgrading the training of research personnel only seems to indicate the validity of the concept.

It is simply a price that industry and government must pay to increase their efficiency in research and to offset the continued shortages of personnel trained to provide their essential services and skills.

Examination of the tasks to be accomplished and the decisions to be made in planning for research activity and in the accomplishment of it provides clear indication of the special professional nature of the research director's function. To direct, but to lead, and to manage, but not to constrain, requires judgments and decisions balanced on the fine edge between top management's outlook and that of the sensitive, creative, individual researcher. Effective management of research operations requires a set of assorted and, at first, seemingly unrelated decisions, most of which devolve on the research director. His capacity to decide correctly depends upon his training and experiences in such diverse fields as human behavior and financial management, as well as in applicable technology. Few other fields of human endeavor require such a broad range of professional skills on the part of those responsible to lead.

CHAPTER VI

MANAGEMENT OF CREATIVITY, INNOVATION AND MOTIVATION

Introduction

The National Conferences on the Administration of Research have had one underlying theme: How does one manage creative people? For above all else this is the prime task of any research director. Without having or developing a working knowledge of what things are important for a manager to do, or not to do, in relation to selecting, placing, inspiring, developing and rewarding scientists and engineers, one cannot aspire to become a successful leader of creative individuals. Moreover, the degree and the timing of the exercise of management techniques often vitally influences the success of the effort. In research and development these considerations are sufficiently unique that they have received repeated assessment in Conference programs. It seems worthwhile, therefore, to devote a separate chapter to this area.

Research management has adopted many of the principles of traditional business management. Their adoption, with such modifications as seemed desirable, has not been altogether successful, clearly because business and research differ somewhat in the personal qualities that are required of their respective personnel. Production activities, for example, optimize repetitive behavior, while research depends upon innovative, original behavior. It is, of course incumbent on all managers to seek to maximize the performance of their personnel. For research directors, this takes on extended meaning, because the performance and contribution of a scientist or engineer is, to a great degree, an outgrowth of his creativity, a particularly difficult quality to define or to "manage". The prime problem is to identify the truly creative worker and to utilize his creativity. This is the most likely single key to success in applying management thinking to the research process.

According to several speakers at the annual Conferences, the essential facets of managing creative work are:

1. Choosing personnel with high creative potential,
2. Providing an environment conducive to the expression of the potential creativity,
3. Rewarding creative performance,
4. Guiding creative thinking toward the organization's objectives, and
5. Reshaping traditional organizational patterns to accommodate the creative process.

The purpose of this chapter is to draw on the Proceedings for definition and measurement of that quality called "creativity", to apply this description to the creative individual, and to explore those management methods which apparently stimulate or repress creative behavior.

Several participants in the Conference, including such scholars as Joseph McPherson and Calvin Taylor, directed their contributions toward an understanding of the psychological process of creativity.

They sought to determine the characteristics of the individual creative person in order to make a rational scheme for selection, utilization, evaluation, and rewarding of professional personnel. Other participants (Chris Argyris, for instance) looked at creativity in terms of a quality to be maximized by grouping workers in a team effort. They touched on the problem of leadership in the creative group, and investigated whether creativity in the manager (or leader) was correlated with high performance in the group.

Also involved here is the interaction of creative individuals with the organizations to which they belong. The organization demands certain behavior from its members which, as some of the contributors pointed out, is in direct conflict with an individual's normal, mature behavior as described by the psychologists. The psychologists discuss the behavior of individuals in organizations in terms of the conflict between values and objectives held by technical specialists and managers. An especially potent criticism of organizational structure appears in a discussion by Argyris which shows management controls and methods at direct odds with the course of normal mature development in the individual. Also, several participants construct models of research-management systems which are self-defeating in their operation. In such a system, the very behavior which management seeks to amplify and encourage is in fact stifled or inhibited, or creates "waste motion" and responses that give no value. What, then, *does* motivate the creative worker, and what is the best pattern for application of control and authority? These questions have been investigated both in objective and subjective studies. Such studies have included surveys of employee attitudes toward the working conditions, salaries, and fulfillment of human needs in various environments, as well as studies of the performance of technical workers in directive and non-directive management situations.

Definition and Measurement of Creativity

The essential characteristic of our nation's collective research enterprise is creativity. Innovation, the proposing, development and implementation of new and better ideas, is essential to the success of all research activity; for, without a continuing flow of ideas, there is a reduction in the ability to devise efficient solutions to current problems. The employee in the creative organization has in several instances been the subject of investigation by Conference participants who were concerned with identifying the key qualities or indices that were correlated strongly with the nebulous quality, "creative" performance.

During the recent tremendous growth of research, the finding of sufficient scientists and engineers to fill waiting positions in all types of research institutions was such an overriding preoccupation that management sought to increase materially the numbers coming out of educational institutions. Assuming that the available national supply of trained manpower cannot readily be changed, management also recognized that its salvation also lay in increasing the efficiency and performance of those workers already on the job. Thus the discussions on creativity in the Proceedings have pivoted on the issue of numbers versus efficiency. What are the criteria by which an applicant or an employee can be measured to determine his creative potential? Research managers are responsible for seeing that their organizations have as little "dead wood" as possible--the non-creator would be better off in some other job, while the institution needs to fill his place with a creator and a producer.

The focus of interest on measurement and evaluation of creative personnel led several discussants in earlier Conferences to outline their attempts to devise methods to predict creativity in terms of the common personnel rating parameters that had been used in government and industry for other types of employees. The results were far from definitive. However, in 1963, Calvin W. Taylor, reported on results of a new and extensive study of creativity and the measurement of factors related to it. It is significant that no single measure of creativity could be found and only multiple kinds seemed to exist.

> We have practically always spent a lot of attention on the criteria problem of performance on the job. That is, after people have done something in their scientific work, we measure how well they have done it, with considerable individual differences being found in these performances. In our one sample of Air Force scientists we lived with them two years to get this information. We went to eight different sources for performance information on these scientists, and we got at least fifty different measures of their performances. On these criterion measures we used factor analysis techniques through computer which found the overlap and reduced this overlap among the fifty criteria to some dozen or so categories which have been placed in Table 1. You will notice immediately that we did not find a singular kind of thing in terms of creativity and originality. But from different sources we get different views a. measure so apparently there are multiple kinds of creativity . . . .

Table 1

Separate Factors of Scientific Accomplishment
in Air Force Scientist Sample

1. Originality of work and thought
2. Creativity and productivity rating by lab chief
3. Overall evaluation by supervisor
4. Total work output
5. Productivity in writing
6. Organizational recognition
7. Quality independent of originality
8. Likeableness as a research team me .r
9. Visibility
10. Society memberships
11. Current organizational status
12. Contract monitoring load
13. Status-seeking (organizational-man) tendencies
14. Total scientific experience

Taylor observed that he indeed is not, as yet, able to say exactly what creativity and the creative process are. Noting that more than twenty-five definitions have been put forth by scholars in this field, Taylor summarized the dilemma this way:

> I believe that in our textbooks we have done a good job in describing only a part of the scientific method . . . . The . . . scientist . . ., who is pushing the borders ahead, opening new fields, pioneering the way, could best be described as using the creative process, which we don't understand very well. But this creative process is not particularly described or even covered in the textbook description of the scientific method, which unfortunately is much more about the verification process than the creative process. When you finally get to the stage where you can use the verification process, you have already done much exploratory creative work and have really attained . . . I would say, a "technical" stage . . . . "Verification" describes the process used by the kind of people we know how to select and train, but I don't think we know how to select and train the other kinds of people, the more creative scientists who open the way . . . . At present, neither the creative process nor the creative person is very easy to describe.

In further discussion there was general agreement that many traditional indicators, such as high intelligence quotient or college grades were _not_ indices of creativity or potential performance. Taylor stated this clearly:

> Learning old knowledge in school and mastering what someone else has produced is a different psychological process from producing something on your own.

At the same Conference another contributor, Donald Pelz, remarked that he was impressed that Taylor's experiment to relate a man's publications to his performance had not shown any positive correlation. Creativity is simply a different quality from a variety of other possible indicators of high performance.

Taylor was questioned closely on, "How do you define 'creative'?" He first recalled the statement by Brewster Ghiselin that "the more creative the product of a man's mind, the more it will call for a restructuring, a reorganizing of man's total universe of understanding." He also mentioned a rating of creativity used by the National Advisory Committee for Aeronautics in 1946 by Robert Lacklen, which was based on "work which contributed to a wide range of problems." In this case, "the degree of creativity depends on the breadth of applicability of a contribution." Taylor also stated that a self-rating on creativity, as defined by Ghiselin, correlated with each and every creative criterion he listed for Air Force scientists.

Motivation, on the other hand, is a quality quite different from creativity. It is usually defined as "a predisposition to act." It can be measured by such means as attitude surveys and observation of performance. Individuals and groups can be highly motivated to act to arrive at self-fulfillment or stated objectives, but this is no indication of creativeness, either individual or collective. H. A. Shepard in 1957 said, "I think the definition of creativity is responsiveness to challenge." While others would agree that responsiveness to challenge may bring forth the best from a creative individual, the idea of responsiveness in itself is more nearly motivation. Conversely, a highly creative individual may not be challenged to act at all in a given situation, particularly if it is of no interest to him. Creativity also may be spontaneous and without challenge or motivation being apparent. As several contributors noted, often the scientist who seems least motivated and least productive turns out to be the one most creative and, therefore, the most valuable.

Individual and Group Creativity

Several contributors discussed the problem area of creativity in terms of individuals versus groups. The remarks of Robert W. Cairns, outstanding industrial research director and government advisor, at the opening of a session in 1957 are pertinent:

> . . . we were dealing with a general subject that might be called creativity, a word that has been bandied about in research circles quite vigorously in the past several years . . . . I think this is symptomatic of the maturing of research as a national force . . . . A few years ago quite a different outlook prevailed . . . the first idea about research was that it was purely an individual enterprise, and, as such, people could go out and do it by themselves if they were unmolested. But things got complex and competitive which means that we have to start adding our efforts together and examining them to see if they are

> efficient. Then you apply the concept of efficiency to the research organization and you wonder what you are doing to it. You talk a lot about teamwork, but you get back to the fact that creativity is what you have to have and this is an individual thing. How are you going to add one person's creativity to another's without interfering with both?

That same year, W. D. Lewis, another industrial research leader, talked about, "Individual Creativeness in Group Research." According to Lewis, those who see self-contradiction in that title believe "that research is such a highly individual affair that any attempt to organize it will destroy it." Lewis, however, sees no conflict here, but rather a state of cooperation and productive interaction:

> . . . the most characteristic part of research--creativeness--is best left to the individual. On the other hand, group research can have more than logistic and economic benefits to the organization. It can also motivate, guide, and develop the individual.

Lewis continued:

> One of the liveliest arguments of the present day is about how we should or should not organize creativity and creative people . . . . We know that creative people generally support individuality, disagree with accepted modes of thought, and do not respect the opinions of the crowd. Perhaps because of the qualities that make them creative, they are most likely to "buck the organization" and fail to conform.

Several Conference participants cited "brainstorming" as one approach to "adding one person's creativity to another's without interfering with both." "Brainstorming," the term of advertising executive Alex E. Osborne, is a technique of group idea-finding sessions. Lewis mentioned that in using this technique, "the members of the group, by mutual stimulation and discussion, are supposed to produce more and better ideas than the individuals could have done alone."

Another participant reported the results of a university-industry cooperative program to develop "brainstorming" into a formal methodology for teaching. Maurice S. Gjesdahl, academic leader, in his talk "Research and Creativeness," in 1956 described a four-step method that several companies used in development, "particularly in product development . . . . I have often thought," he remarked, "why isn't it as applicable to research work?" These are his four steps:

> In the definition of the problem we consider the boundaries that we are going to have in the problem, the elementary essentials . . . .stating the problem in simple terms . . . . Professor Arnold of MIT has said that instead of telling the boys to design a new toaster, let's talk about how to dehydrate bread and brown it . . . .
>
> The second step is that of "ideation"--some people prefer "brainstorming" . . . it may be by two . . . or a group of four or five . . . or, as some companies do, have groups of about 10 organized to make suggestions which are recorded without any restrictions . . . . Putting them down and looking at suggestions encourages you subconsciously to suggest other connecting links . . . . They eventually reach a plan or a solution, because they are always adding to what each has said . . . . We try to remove the inhibitions and get "free wheel" thinking . . . . Seeing something on the board in front of you does suggest something else, so you get a multiplicity of ideas which may be helpful for the solution of a problem. This is the purpose of this creativeness or creative thinking.
>
> The third point is evaluation . . . .the evaluation comes afterward and here you have the picking up, the collecting, the grouping of the ideas that you wish to keep--making a critical examination and using judgment.
>
> Then the last step--that is of selection of your solution. What has preceded may lead to an answer or it may lead to a

procedure to get the right answer.

Gjesdahl applied his description of the methodology of creative thinking primarily to the field of product improvement and engineering. Its usefulness may be limited for research having more basic or fundamental aims, where individual creativeness is paramount. The author's evidence of results was restricted to industrial cases concerned with "the efficient application of current solutions." This definition of "productivity" was used in the recently published book (1965), The Creative Organization, a report of a seminar at the University of Chicago graduate school of business. "Creativity," on the other hand, is usually thought of as the "proposing, development, and implementation of new and better ideas." Is this really the same creativity that Professor Gjesdahl was describing? He was saying that in certain industrial situations, creativity can be enhanced by certain group-thinking sessions in which ideas are solicited freely and not "killed" by negative judgments until they have "incubated" or have suggested other ideas to the members of the group.

Though helpful, creation by group action seems to be no simple cure-all. Other participants in the Conferences doubted there was any greater creativity through group action than through individual effort. For example, Lewis said:

> . . . Those who say individuals working alone are best at technical creativity and those who extol the outstanding creative power of groups appear to be equally limited. The fact is that this aspect of organization does not have a strong effect on technical creativity.

Lewis arrived at the above conclusion from evidence gathered from a study of performance of 450 professionals in two divisions of the Bell Telephone Laboratories' Research Department. He described this study as follows:

> Operating procedures within these two divisions are somewhat different. In A, about 80 percent of the professionals work as individuals. In B, it is the other way around and about 80 percent work in or with a group.
>
> What about the relative creativeness of the two groups? To evaluate this, we need a measure of creativeness. Since the purpose of an industrial research organization is to discover and to invent, it seems reasonable to use technical papers and patents as a numerical, if not precise, measure . . . . When these measures are applied, Division A is more creative when measured by discoveries or technical papers, and Division B is more creative when measured by inventions or patents.
>
> Can we compare A and B on some absolute basis? In order to do this, we must assign a relative value to patents and papers . . . . A more precise comparison would have to depend on a subjective judgment concerning the relative value of patents and papers. Since discovery and invention are both necessary links in the same creative chain, this would seem to be artificial.
>
> What does seem to emerge from this is the Division A - where people work mostly by themselves - and Division B where they work mostly in groups--are both highly creative.

As earlier noted, individual and group creativity has also been studied by psychologists. Lewis outlined several other studies as follows:

> Karlin, Potter, and Reisz at the Bell Laboratories studied the "brainstorming" technique applied to the configuration and appearance of a new telephone set. They compared the creativity both in volume and quality of "brainstorming" groups with that of individuals. The individuals produced more good designs per man and also more bad designs per man than any group.

> Shaw compared the ability of five groups of four with that of thirty-eight individuals in solving problems . . . . He found that the groups got more correct solutions, not only because they had more ideas all together, but because incorrect solutions of one member of the group would be censored by another member. In other words, the judgment of the group reduced the quantity of the group output . . . .
>
> Now what do these tests tell us? It would be rash to make an out-and-out statement that they are measuring technical creativity. Yet the faculties that are being tested--the ability to solve puzzles and to think of new combinations or configurations--are the most characteristic ingredients of creativeness. When exercising these faculties, the people tested were generally more productive when they acted as individuals.
>
> However, other faculties must be associated with these, if technical creativity is to be high. One is the critical faculty--the power to judge the merits or demerits of a new idea or line of thought at an early stage, to discriminate between the good and the bad, to retain the good and to drop the bad before it absorbs too much time and energy . . . . This power of judgment or discrimination can serve at the very beginning to initiate useful creativity by seeing what needs to be done. Now, insofar as the tests referred to give a rating on the power of judgment, they indicate that groups are better at it than individuals.
>
> Therefore, these experiments and studies tend to confirm in the field of creativity a principle that is common in many other fields . . . as follows: "The individual is better at action; the group at judgment."

Human Behavior and Conflict in Research Organizations

Several contributors to the Proceedings describe a common situation in laboratories where the objectives of the organization, as expressed by top management and enforced through lower echelons, conflict with the objectives and personal values of the research workers. Many persistent and yet unsolved problems arise in this type of conflict, which is prevalent because it seems to be inherent in the nature of large-scale organizations and particularly of individuals. Standing at the interface of these opposing value systems is the research director. His image belongs to both worlds, management and research. His relationship to the employees as an exponent of either high- or low-pressure management can make a difference in the performance of his technical personnel. Therefore the Conference explored characteristics of this conflict from several viewpoints. These aspects are:

A. The balance of autonomy and coordination.

B. The inhibition of creativity by management.

C. Dependent and independent behavior of research workers.

D. The participation of research workers in decision-making.

These really are not separate problems; they are merely different viewpoints which Conference speakers have taken towards the same general problem. It is interesting to see that these aspects cannot be separated; autonomy and inhibition particularly seem to be related.

The notion of autonomy is the focal point of several contributors. Autonomy means such things as the freedom of the scientist to choose his own technical goals or to evaluate his own work. While speaking in 1957, Shepard discussed autonomy and stifling of creativity in these words:

> Perhaps what we do need is a new concept of ourselves which emphasizes our job of helping the research worker to accept responsibility. Few young scientists have acquired the self-image of the skills required to meet the organizational demands of industrial science. They should be able to participate responsibly in decision-making. By and large they can't. They should be able to use each other's resources in

> problem solving. Many of them are very reluctant to do so. They should be able to engage in mutual evaluation, mutual support, mutual criticism. Many of them talk behind each other's backs instead. They should be able to work autonomously and, in fact, there should be some basis in their lives and environment which causes them to regard science as an ideal career. I don't think we are encouraging them to develop into that kind of organizational person.

According to Shepard, a built-in hierarchy in the laboratory is responsible for the situation above. He continues:

> On one hand we establish a managerial hierarchy with several levels of censorship and several gatekeepers who can prescribe and proscribe, permit or deny; who control the distribution of such important rewards as income, freedom and recognition. These people carry their responsibilities very thoughtfully and do their best to be considerate and fair in the administration of those duties.
>
> This makes most American laboratories friendly, comfortable places to work. It does, however, place the engineer or scientist in a highly dependent position. It doesn't really put him to the test as an independent, creative scientist. It doesn't help him to develop into one. Rather, it rewards docile, cooperative, dependent behavior.

Speaking on the same problem of conflict within the organization, academician Chris Argyris (1957) supported Shepard's contention that formal organizations are inherently poor environments for the conduct of scientific activities, as long as these organizations are operated along the lines of "traditional" management theory and practice.

Shepard sought to show that creativity in the scientific department of a company is inhibited by the power structure that surrounds it. Argyris went further and stated that <u>any</u> organization tends to inhibit mature behavior of its members, because of inherent conflicts in the natural characteristics of the organization and of the individual. The theoretical propositions Argyris put forth support Shepard's description of "built-in deterrents to creativity," if one accepts the premise that creative behavior is essentially the result of mature self-actualization by individuals.

Professor Argyris noted that much had been written on the separate topics of human personality and the formal organization but little had been done to relate the findings. It was his belief that "integration of this seemingly diverse and scattered literature would help to provide some useful insights into the _why_ of human behavior in on-going organizations, thereby enlarging our scope of understanding." He established a basis for such integration by restating for comparative purposes the present accepted characteristics of individual growth and development, and then restating the traditional principles of organization as these appear in standard texts. The resultant propositions gave Argyris an assumed model of a man-organization system that inherently defined a relationship of conflict and degeneration. Argyris offered precautions on his analysis:

> In the model of the personality and the formal organization we are assuming the extreme of each in order that the analysis and its results can be highlighted . . . . No assumption is made that all situations in real life are extreme (i.e. that individuals will always want to be more mature and the formal organization will always tend to make people more dependent, passive and so forth, all the time.) The model ought to be useful, however, to plot the degree to which each component tends toward extremes, and then to predict the problems that will tend to arise.

It is revealing to compare the above views of an academician with remarks (1959) by another contributor, E. R. Piore, a leader in government and industrial research. Piore was deploring the confusion of productivity and creativity and their relationship in turn to autonomy when he remarked:

> . . . a very important way of killing creativity is to insist that all plans go through at least three levels of review before starting work. A review has the very important function of weeding out and filtering innovation. More levels will do it faster, but three are adequate, particularly if you protect these levels of review from any exposure to the enthusiasm of the innovator. The best way to do this is to insist on written proposals, then you don't have the personal contact which is likely to influence the review levels.
>
> I . . . think we can agree on the working conditions and the atmosphere necessary for productive work. When one incorporates these conditions, there is always the notion of freedom as a necessary condition for creativity. I would like to examine this with you for a moment.
>
> (a) One normally puts down "Freedom to select the problem." Is this a necessary condition? A graduate student, by selecting a school and a profession limits his freedom in selection of the problem. Neils Bohr, by the very act of selecting Rutherford at Cambridge restricted his freedom to select the problem . . . .
>
> (b) Another condition often stated is "Freedom to select the approach, freedom to select the tools." This again has limitations. One of necessity must use the tools that are available in the laboratory.
>
> Creative work has been done under conditions where freedom, in the absolute, did not exist, and creative work will be done in the future under conditions of limited freedom.
>
> I have heard an undercurrent to the effect that some among you have been preaching careful planning, careful measurement,

> keeping taut reins on our researchers to obtain results--creative results. This is not the way to obtain creative results. I do feel the most important element is to have a real problem with a realistic time scale; that is, a problem which is solvable, and that there must be someone around who can appreciate the solution of the problem and make use of it.

Fiore's remarks and those which follow, bear directly on the process of planning individual research projects as outlined earlier in Chapter V, where the research director is obligated to balance between organizational limitations and personal freedom.

In his remarks in 1956 on "How Do You Rate as an R & D Manager?" David Emery, a psychologist and management consultant, also discussed the conflict of personal values and objectives of technical specialists as opposed to managers. The following chart shows his views of how managers and scientists are generally opposed on each of six points that bear upon the management of scientific activities.

CONFLICT OF OBJECTIVES BETWEEN MANAGERS AND SCIENTISTS

The Manager Values:	The Scientist Values:
1. Producing innovations on time and within a budget.	1. Long-term growth of knowledge rather than scheduled projects.
2. Maintaining policies of work methods and discipline.	2. Working in freedom rather than under discipline.
3. Maintaining a clear organizational structure with explicit delegation of authority.	3. Working in an informal rather than a formal structure.
4. Measuring the success of programs and individuals.	4. Having his success measured by other scientists only.
5. Providing leadership for all in his department.	5. Recognizing only experts in his field as legitimate leaders.
6. Promoting good communication.	6. Rejecting "communication" as the latest gimmick of management consultants.

Although Emery, like Argyris, has stated these values as extremes, these perceived differences in outlook can serve a useful purpose. Emery does not believe there is any profit in trying to merge or eliminate this divergency. Instead he states that

> . . . these differences have great value because they help each type of person (administrator and technical expert) to be more effective in his particular type of work . . . the two kinds of work are fundamentally different. Therefore, they require different kinds of training - and probably different types of people.

His recommendation for the research director is simply, to increase his sensitivity to the objectives and problems of research and researchers. He thinks that one helpful point is "participation":

> "Participation" isn't a new idea, but it is a powerful one: The more people feel they have had a hand in things, the more they are ready to accept and cooperate with the decision in question.

But he gives this caution:

> Participation here does not mean having groups get together, vote, and then do whatever the vote says. Participation here means calling people together, consulting them, getting their best thinking, thanking them, and then making your decision. It is also your responsibility to inform them about the decision and the reasons why that particular decision was taken.

In expressing these differences in such extreme one might conclude that Emery thinks the value conflicts are such that few scientists can become successful managers. The facts are, of course, that practically every successful research director has come up through progressive levels of research experience. His prime and unique qualification is that he _is_ trained and experienced in technology. The shift in values is, to be sure, an adjustment but this usually comes naturally with the change in job responsibility.

The effect of the formal organization on the work of the individual scientist was also explored by the academician, Donald C. Pelz, who reported his findings to the 1963 Conference. He began:

> A major issue facing research organizations is the conflict between the needs of the organization and the needs of its members. Scientists typically want (or say they want) freedom in selecting and executing their own research. But the laboratory must pursue objectives for which it was established, and meet the commitments for which its funds

were obtained. What can the social researcher say about this conflict -- between the autonomy demanded by the scientist, and the coordination demanded by the laboratory?

First we must answer the question, "If scientists say that they want more autonomy, does it follow that maximum autonomy results in maximum performance?

Pelz measured autonomy by determining the weight that each of five echelons exerted on the selection of the scientists goals. The echelons were the scientist himself, his colleagues plus his subordinates, his immediate chief, higher level supervisors, and non-technical executives plus clients or sponsors. The performance was measured by obtaining judgments from panels of senior scientists on the individual's contribution to general technical or scientific knowledge in the field. Pelz found that a scientist's performance was better if colleagues, higher supervisors, and even non-technical executives and sponsors had some weight in deciding the goals of the individuals. But data could not show whether or not promising individuals were simply the ones who came to the attention of the higher echelons, who then took a hand in formulating their goals. It remained to be shown, as Pelz did later, that the performance of scientists was better if they retained a high degree of influence over the higher echelons.

In summary, Pelz showed that if a scientist is in control of the situation, his performance is better if several echelons have a slight weight in deciding his assignments. But a scientist with low influence -- one who does not have control of the situation -- performs better when left alone. For Pelz, then, the answer to the conflict of the individual and the organization does not lie in compromise between autonomy and control, but rather in "channels for vigorous two-way influence" between the scientist and those who manage him. Pelz's studies would appear to explain a logic for the continuance of the conflicting values between researcher and manager as observed by Emery at the 1956 Conference. Emery's notion of increased participation has thus been confirmed in terms of "two-way influence."

While they do not provide explicit solutions, Pelz's findings and Emery's beliefs give important clues for management action. More hard data is needed from further academic study but the research director now has enough guidance to understand some things which contribute to or detract from creativity and high performance by scientists. He can now develop a greater sensitivity and understanding of their personal attributes, motivations and objectives. In doing so he thus improves his capabilities for successful professional research management.

CHAPTER VII

ETHICS IN RESEARCH ADMINISTRATION AND PUBLIC AFFAIRS

Scientists in Public Affairs

A significant trend in the more recent years of the National Conference has been toward discussion of certain aspects of research administration that bear less upon the direct management of research operations and more upon the ethical conduct of researchers in activities outside the laboratory. These extra-mural activities were an outgrowth of the tremendous expansion in the areas of science, engineering and research triggered by the experiences of World War II. Accompanying this expansion were an awakening and a reawakening by both the professionals and the general public to the role and influence of scientists and engineers in various areas of society and public policy. For essentially the first time, the creators of the new science and the new technology have found a strong voice (whether they wished it or not) in the economic, social, military, political, and cultural affairs of the nation and the international community. The impact of the technology on these affairs has been so great as to bring a great many professional researchs into areas of national and world affairs.

The growing involvement of researchers in affairs outside their immediate research organizations was a part of the evolution of science in three stages. Frederick Seitz, distinguished researcher and President of the National Academy of Sciences, gave the National Conference (1963) an historical perspective of this evolution.

> In its first active stage, science was mainly of intellectual and inspirational value although it was by no means completely divorced from the everyday world of technology.
>
> The second phase of the evolution of western science occurred when systematic investigations began to turn up major areas of the universe whose quality and range could not be suspected from everyday observations but which could be used at least with limited or partial success by a combination of common sense and trial and error Edisonian methods in the world of the industrial revolution.

During these first two early phases, two movements were taking place. In the fields of science, scientists were developing a set of ethical principles, including the scientific method, as a code of behavior for their search for truth, fact, and physical law. The professional development of scientists became characterized by technical and ethical standards held, for the most part, inside the sciences and had little or no influence on the outside world. In the fields of technology the trend was somewhat different. Great technological strides were being made, based both on the comparatively little factual knowledge produced by science up to that time, and on, as Seitz put it, "common sense and trial-and-error Edisonian methods." But since the activities of the early technologist (later to be called "engineer") had great influence on his fellow man's activities and environment, his outlook could never be entirely introspective. Thus, the engineer began to develop a set of technical and ethical standards for his relations not only to his associates but also to the publics he served. Seitz continued:

> The third turning point in the evolution of science occurred in

> the first half of the present century. In it, the direct participation of the scientist became indispensable for the advance of many of the most revolutionary and profitable phases of technology, such as those involving the production and use of chemicals, communications and energy conversion.
>
> A significant trend in this third phase of science has been the accelerated pace of investment of wealth in research and development because of its practical consequences, particularly since 1940.

In this latest phase, science and rapid technological advancement through research have been welded into what is now commonly called research and development--or as some prefer to think of it, science and engineering. This welding process has thrust scientists, as well as engineers, much further into public view and into considerations of public policy. In turn this has created a requirement of responsiveness and responsibility calling for a new and revised ethical standard, not only for scientists and engineers as general groups but especially for their leaders, the research directors. To assume this role properly, scientists, engineers and their leaders must develop a special perception of conduct which creates a favorable image on the part of those they serve. Failure to have or to observe such standards, of course, gives rise to accusations of conflict of interest.

That these professionals must "sell" themselves was brought out in 1964 by George B. Kistiakowsky, eminent chemist and presidential advisor, when he said:

> We want to sell ourselves to the public for two very good reasons. First, I think, the overwhelming majority of us agree that we as a group--including our predecessors--have had much to do with shaping modern industrialized society.

> We want to continue this process because we believe--in contrast to some political figures, who seem now ready to push us back into the nineteenth century--that in technological progress lies the only road to an enlightened and prosperous future for a densely populated, ever-growing country desirous of ever greater and better standards of living. That is one very good reason. The other is more mundane. It is that we, the researchers, and the research process itself cost money, and the time when enough money was available from foundations, from industrial managements and other local sources, is gone. We have to rely on public support. Here, of course, lies a source of real trouble for us. What public policies we advocate almost inevitably benefit research and development and so we are perhaps rightly accused of conflict of interest.

At the start of this fast-developing relationship between science and public policy, various members of the engineering and science professions were being called upon to advise on military and security matters. This phase developed during and early after World War II and has not since abated. However, the call to advise has widened immeasurably and now encompasses virtually all areas of public interest.

Advisory services to public bodies have taken many forms. One form is a panel, committee or board entirely constituted of technical specialists, usually drawn from private pursuits. This practice started during the second evolutionary phase described by Seitz, when in 1863, the National Academy of Sciences was established by Congress. In his review (1951) of the advisory boards and panels in the Department of Defense, Ralph Sawyer, a distinguished educator and advisor, noted the Academy's efforts and went on to describe the reasoning behind the development of public advisory bodies:

> ...while advisory committees are not a recent innovation, it is certainly true that in the years during and since the last

> war, there has been a marked expansion of this method of operation. The increasing use of committees has, in fact, been a characteristic of the American way of life throughout all phases of our national activity. In industry, in universities, in all kinds of governmental activity, from the local to the national levels, committees have been more and more used. This development has been partly a reaction against totalitarianism, but even more, I think, it has reflected the realization that the use of committees-with either executive or advisory powers-broadens the base of Government, improves decisions, and facilitates the acceptance by the particualr cooperating groups concerned, and by the general public, of the decisions reached.

Sawyer also noted that some boards and committees are appointive and others are established by law, but all are established to advise a government organization and its leadership.

Another form of advisory services is the inclusion of one or more scientists or engineers in the framework of a committee, board or commission constituted for broader purposes. Sawyer noted, for instance, that the Research and Development Board of the Department of Defense consisted of full time members of the Defense establishment but its various committees and panels included advisors from the outside.

A third form of securing science advice is through appointment of specialists on a full time basis for limited periods as "science advisors" or "chief scientists" and the like within government organizations. Good examples of this were the creation of the post of Science Advisor in the Department of State and later the appointment of a Science Advisor to the President.

In the last few years further development of the relationship between science, scientists and public and political affairs has begun.

This phase seems to be a natural following to the various advisory activities described earlier. It is characterized by appointing or electing scientists and engineers to positions of major public responsibility and policy rather than inviting them to serve only as advisors. A good example of this is, perhaps, the Atomic Energy Commission. This new development in the public role of scientists is clearly opposed by some, at least. In 1961 the distinguished inventor of radar, Sir Robert Watson-Watt, quipped:

> I am absolutely opposed to the scientist seeking political office. The reason is perfectly simple. If he can only get hold of the right kind of statesman--the right kind of politician--coach him pretty carefully on the job--then, when things go wrong, it's the politician who gets sacked and not the scientist.

Sir Robert was supported in his view by others that year, including the late, beloved Theodore Von Karman, founder of modern aerodynamics. Von Karman said:

> I believe that these scientific people should not try to have the responsibility for political decisions.

But the eminent research leader and presidential advisor, Vannevar Bush, warned that same year:

> In fact if scientists are to have their full influence for the good of the country in the days to come, many of them will indeed need to learn to practice this difficult art [of politics].

In summarizing the discussion then proceeding, Raymond Ewell speaking from his own public service experience, added the following facts:

> I have two pertinent facts that I would like to lay on the

> table here. Sir Robert has referred to the point that he didn't think scientists should seek public office. But in the early days of the National Science Foundation, we were looking for Congressional appropriations and we made a study of scientists in Congress. It turned out that at the time there were...45 men in the United States Congress with bachelor degrees in science and engineering. This was almost ten per cent of the composition of Congress, whereas the percentage of the scientists in the whole population was less than one percent. Two-thirds of these were engineers and about one-third scientists. We did find that the scientists in the Congress--some of them I should say--were not very sympathetic to the National Science Foundation appropriations. In fact, some of the principle opponents were men with degrees in science. The only conclusion that we could draw from this was that they must have been frustrated scientists who had taken a bachelor's degree in chemistry or engineering, then went into law and then had become congressmen. So that there is possibly a greater participation in the legislative branch than one might believe by scientists.

The conclusion to be drawn here is that indeed research directors, engineers, and scientists have found themselves in increasingly high, influential positions in public affairs. Though no one appears to doubt this, the argument then turns to how they can discharge these responsibilities without creating what appear to be conflicts of interest between their public obligations and their various private and professional interests.

The Conflict of Interest Problem

The newly found role of the scientist in affairs beyond his immediate laboratory, private or public, raises new questions about ethics and standards. These are questions of interest conflict.

In 1962, Norbert Schlei, Assistant Attorney General, reviewed the government's view of the conflict of interest problem and the present and proposed legislation to regulate this area. He reiterated the principle underlying all of the government's interests that no public official shall serve two masters. He went on to explain further the basis of government action this way:

> These principles--(1) that public officials must be required to act with complete fidelity to the public interest and avoid even the appearance of a conflict-of-interest; and (2) that Governmental restrictions must not be so inflexible or unrealistic as to impair the ability of the Government to obtain the services of those individuals whom it must call upon because of particular talents, skills or experience--have been endorsed by all serious students of the ethical problems confronting the Federal establishment.

But as pointed out by Wayne A. R. Leys, eminent scholar and student of ethics, the general problem of conflict of interest is must more complicated and applies not just to the political arena. He set forth the following situations in which scientists may find themselves exposed to such accusations:

> (1) That they are simultaneously working for several employers who have adverse interests, such that any assistance they render to one employer will hurt the other employer;
> (2) That they are working for an employer whose interest is opposed to the public interest;
> (3) That they are working for an employer who does not know the value of the investigations for which he is paying, with the result that the scientist can betray his employer's interest (a) by stealing an idea, (b) by giving bad advice, especially when the employer is more ready to reward bad advice than sound advice;
> (4) That they are working for an employer whose interest is different from the expert's interest in advancing his art or science.

On a philosophical basis, Leys explains that not all these situations necessarily or intrinsically represent conflicts. For example, the mere fact that the professional man is working for two employers or clients or that he has financial interests of his own are not sufficient evidence. He points out, in addition, that "the public interest," "the interest of the firm" and "personal interest" are not discrete concepts; as a result, a scientist has very few explicit guidelines for moral behavior. Leys summed up the situation by saying:

> What I am trying to show is that the direction of political controversy and the direction of the theory are not the same in our time. A scientist or a technician, in his practical situation, needs to pay more and more attention to the possibility that he will be charged with conflict-of-interest. But when he turns to professional literature, to the social sciences, or to philosophical treatises he finds many excuses for dismissing the phrase "conflict-of-interest" as a crude oversimplification.

Leys recognized that scientists must not try to run away from this predicament, however unhappy and complicated it may be. His conclusion is that research and development people may have to adopt new codes or creeds. Whether or not they do, they must not ignore these ethical and political issues. And his solution is for research directors to formulate a series of questions about employee-employer relationships to be asked and answered periodically. A sample set might be, in his words:

> 1. Is there a specific agreement with the employer regarding the extent to which the employer can claim exclusive use of the results of an investigation?
> 2. Is there a periodic review of compliance with the agreement to determine that the agreement--if it exists--is not being violated in the scientist's publications, in his personal investments, and in his work with other clients--if he has other clients?
> 3. Are the terms of the scientist's employment compatible with his loyalties to his country and to science?
> 4. Is the scientist's work such that he should not be affiliated with a partnership or corporation that does consulting work for the Government or for other clients?
> 5. If, in a particular case, the scientist is convinced that he has no conflict-of-interest that will result in malfeasance, is

> his conclusion resulting in a violation of rules that should be held inviolable, regardless of exceptional circumstances?
> 6. If a possible conflict-of-interest appears when any of the preceding questions have been asked, can all concerned be satisfied by a disclosure, or is it necessary to give up some of the conflicting interests?
> 7. If thereis a possible controversy about conflict-of-interest, is the scientist confronted by a political problem, that is, a problem of persuasion, or by a religious problem, a problem of doing something to maintain his personal integrity and his self-respect?

Other participants in the 1962 discussion agreed with Leys that the ethical questions will not be settled for some time to come. As Don K. Price, distinguished dean of public administration, said it, this is because of the "...new nature of the relationships between science on the one hand and public affairs on the other [which] has moved us from the Faustian type of moral crisis, the internal crisis of the soul, to the practical, everyday, down-to-earth problems of your income tax return and the administrative problems of getting along with two employers."

The urgency to settle some of these ethical questions was reflected by Eugene Fubini, distinguished researcher and government servant, when he warned the Conference (1963) that such ethical codes must come about immediately in those organizations doing business with the Federal Government. He said that such organizations must now establish appropriate codes for their own actions and those of their people. Inaction will not excuse them from the only other alternative: direct steps by the Government to establish its own intimate administration of these affairs.

Meanwhile, there seems general agreement on two positive notes.

First, the dialogue should be continued to bring about better awareness of these problems among research administrators, and second, from this dialogue an updated set of ethical codes may be evolved to guide researchers and research directors in their newly recognized role as a major influence in the national and world society.

APPENDIX A

PROBLEM AND METHOD

Discussions regarding program planning for the Nineteenth NCAR brought out the interest of several parties to undertake a researching summarizing, and interpretation of the Proceedings of the previous eighteen Conferences. Dr. Robert Buchheim, the Program Chairman for that Conference, was instrumental in motivating this project by his solicitation of Conference Committee members in February, 1965, for estimates of the requirements of the task. With his approval and encouragement, a contract proposal was made by the University of Delaware to the National Aeronautics and Space Administration because of its interest and expanding program in studying the improvement of R & D Management. The Space Administration and the University entered into NASA Contract NSR 08-001-010 on July 1, 1965.

The general problem statement of this effort was contained in the contract proposal in these terms:

> It is proposed that a program be initiated to analyze, digest and edit into a single book-length manuscript the existing eighteen Proceedings of the National Conference on the Administration of Research. The manuscript to be prepared would comprise a systematic, logical and self-consistent framework into which would be cast the vast amount of information, ideas and experiences related in these various Proceedings.

Specific tasks developed from this general problem statement. To be of maximum value, the information, ideas, and experience in the Proceedings text would need to be brought together and arranged according to certain subject or "problem areas." Furthermore, these subjects would have to be integrated into a coherent pattern that exhausted as much as possible the range of discourse throughout the

Proceedings. Indexing the Proceedings according to the subjects would be valuable both for retrieval of information during the preparation of the summary, and later for the reader who wished to examine the Proceedings in more detail at any point. Discussion of these ideas with other participants in the Conference confirmed and strengthened these concepts of preparing the summarization in such a way as a resource for professional research administrators and scholars in the field.

The special problems of distilling an ordered and coherent set of "principles" from a diffuse and loosely constructed set of talks, addresses and discussion were approached by way of the sub-tasks in the following list. They were:

1. Read and annotate the text of approximately 2000 pages.
2. Gather "like terms" into a language for research administration.
3. Identify recurrent problem areas in the text.
4. Derive from the problem areas and the language an outline reflecting an objective digest of the Proceedings, rather than using any preconceived outline.
5. Gather, correlate, and index the textual material as relevant to each of the problem areas.
6. Analyze and summarize the material gathered in Step 5 into chapters.
7. Edit chapter drafts in consultation with representatives from industry and government (in order to preserve the balance of interest that has been part of the spirit of the Conference from its inception).
8. Edit into final draft and add preface, index, and other supplementary material.

Some comment is appropriate on the methodology used in the above subtasks. Because of the sheer size of the task of reading the eighteen volumes, duplicates of the printed Proceedings were secured as working copies, so that notes could be made directly on the pages. With the material in the form of individual articles, instead of bound volumes, remarks on each subject area could then be grouped for review and comparison. In writing the chapters it was possible to take extensive excerpts without retyping or transcribing, since the duplicated pages could be "blue-penciled" and clipped into the typed matter.

The tasks of gathering and correlating were aided by the use of marginally-punched "keysort" index cards which served as an information-retrieval system for bringing together material on the various problem areas. Each of the 300 odd articles was encoded on a card, according to the list of categories. The categories evolved from study of the main topics in each article. Thus it was possible to briefly indicate the content of each author's presentation, in terms of his subject matter, style and scope.

In writing a total chapter, the utility of the keysort as a mechanical aid seems evident. Working from the outline derived from study of the material while the keysort cards were being filled out, the cards were sorted for several key categories related to each chapter topic. In addition, the preliminary work for the manuscript has provided an exhaustive index and guide to the "problem areas" identified in reading the material. The index provided in Appendix B will serve to guide those readers who may wish to explore the Conference Proceedings at greater depth on particular points which were too detailed for inclusion in this summary.

APPENDIX B

INDEX TO N. C. A. R. PROCEEDINGS

Readers may wish to consult original sources in the Proceedings of the National Conference on the Administration of Research for information in more detail than it has been possible to provide in an interpretive summary. The following list of selected topics with reference to authors, years, and page numbers in the Proceedings is provided for their study and review.

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