A RESOURCE ALLOCATION MODEL FOR RESEARCH AND DEVELOPMENT MANAGEMENT

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A RESOURCE ALLOCATION MODEL FOR

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MANAGEMENT

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PREFACE

The rapid growth of scientific research organizations in the United States has given rise to numerous relatively new problems in management. With all this increased activity, one finds a paradox. Research and Development (R and D) effort is vast. The dollars spent are staggering. The people and skills involved are many. The problems of managing this effort are great. But management literature and research on research is relatively sparse.

Because of personal interests and the recognition of the need, the author began serious research into the area of Research and Development management in 1959. This resulted in a thesis written in partial fulfillment of requirements for a Master of Science degree at the University of Alabama in 1960. The thesis, entitled "A System for the Control of Research and Development Activities," analyzed in depth the process of management controls for a Research and Development laboratory. Emphasis, however, was upon means of keeping the Research and Development manager apprised of the progress and incurred costs of projects being conducted in the laboratory. This study was undertaken as a logical extension to probe in depth the more crucial management decisions of project selection, evaluation, and the

allocation of resources to them.

The author wishes to express his sincere appreciation to the members of his graduate committee: Professor Wilson J. Bentley, Committee Chairman and Head of the School of Industrial Engineering and Management; Dr. Paul E. Torgersen and Dr. Wolter J. Fabrycky, of the School of Industrial Engineering and Management; Dr. David L. Weeks, of the Statistics Department, and Dr. Joseph R. Norton, of the School of General Engineering.

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CHAPTER I

INTRODUCTION AND PURPOSE

Man's discoveries about himself and the world around him, put into an orderly form, have created that vast structure of knowledge we now call "science and technology." His search for knowledge, and the means to use it for his own benefit, has occupied man from his very beginning.

Within the past decade the modern enterprise called "Research and Development" has become foremost among the social, economic, and military affairs of our nation. The role of research and development in the advancement of all modern science and technology is fully accepted by society, but its full significance is just now being recognized.

Economists have attempted, with some success, to measure the relative importance of technological change in economic growth -- only one dimension of the effects of research and development, but certainly an important one. Traditionally, economists have attempted to explain increases in output per worker by increases in the quantity of capital equipment used and by technological improvement. A study by Robert Solow (1), however, yielded the rather surprising conclusion that only thirteen per cent of the increase in output per worker in the United States between 1910 and 1950 could be statistically explained by increases in capital equipment per worker. The work of Moses Abramovitz (2) yields similar results. Both authors are well aware of the great conceptual difficulties that underlie their analyses. Solow (1) stresses that even if the quantitative conclusions are accepted, improvement in the quality of labor, better allocation of resources, and many other factors must share with research and development the credit for the remaining eighty-seven per cent. But even a casual comparison of the goods on the market and the production techniques used to make them today, with the goods and techniques of fifty years ago, dramatically indicates the tremendous role that research and development has played in improving the present standard of living.

The results of research and its applications (technology) are taken for granted today. Scarcely anything done or anything used to make life longer, easier, or more comfortable is unaffected by it. Of all the forces shaping and reshaping life in America, none is more insistent and powerful than those that spring from research and development. The standard of living depends on research and development to find new ways of using resources available now and to find new products among the old materials.

There are many authorities -- including two former Presidential science advisers, Dr. George B. Kistiakowsky and Dr. Jerome B. Wiesner -- who believe continued emphasis on research and development is essential to the nation's economic health (3).

"In the new era,"Wiesner points out,"we must support all the good research available. If we don't, our economic growth is going to falter."

Although man has existed for about one million years, research and development as a source of economic change has existed for only some one hundred and fifty years. Bertrand Russell (4) has said:

> "When we consider how recently it has come to power, we find ourselves forced to believe that we are at the very beginning of its work in transforming human life."

Considering the entirety of mankinds recorded history -- some 5000 years -- 90 per cent of our entire technological accomplishments have occurred in the last two per cent of the time span. Today, organized research and development could be considered a primary growth industry in the United States.

1. Dollar outlays from all sources for research and development in 1965 may total $21 billion or 3.2% of the estimated gross national product. This is about eight times the level of 14 years ago when research was less than one per cent of national output.
2. Well over 500,000 scientists and engineers are engaged in research and development. This

represents more than a doubling in eight years, in contrast to a rise of only 11 per cent in total civilian employment (4).

One has only to look at Figures 1 and 2 to see that expenditures will probably exceed $30 billion by 1970 or before and that the per cent of the gross national product being devoted to these activities is ever-increasing, to see the unfolding story (4). It is quite obvious that the results of these expenditures have a great leverage upon the economy; they are a prime factor in the nation's defense; they affect the life of every citizen. But more importantly, the ever-growing bill for research and development reaches directly into each person's pocketbook.

It should also be recognized that scientific research and its products may even decide the political future of man. Today, this Country finds itself locked in a life-or-death struggle with an antagonistic ideology that is dedicated to the destruction of the democratic and capitalistic way of life. The communist world is devoting much of its energies to this conflict in an attempt to surpass the United States economically and scientifically.

The Engineering Manpower Commission has predicted that only 35,000 engineering degrees will be granted in the U.S.A. in each of the next few years as compared to 100,000 per year in the Soviet Union. This Country cannot and should not attempt to compete by concentrating on the mass education of engineers and scientists. But everything possible

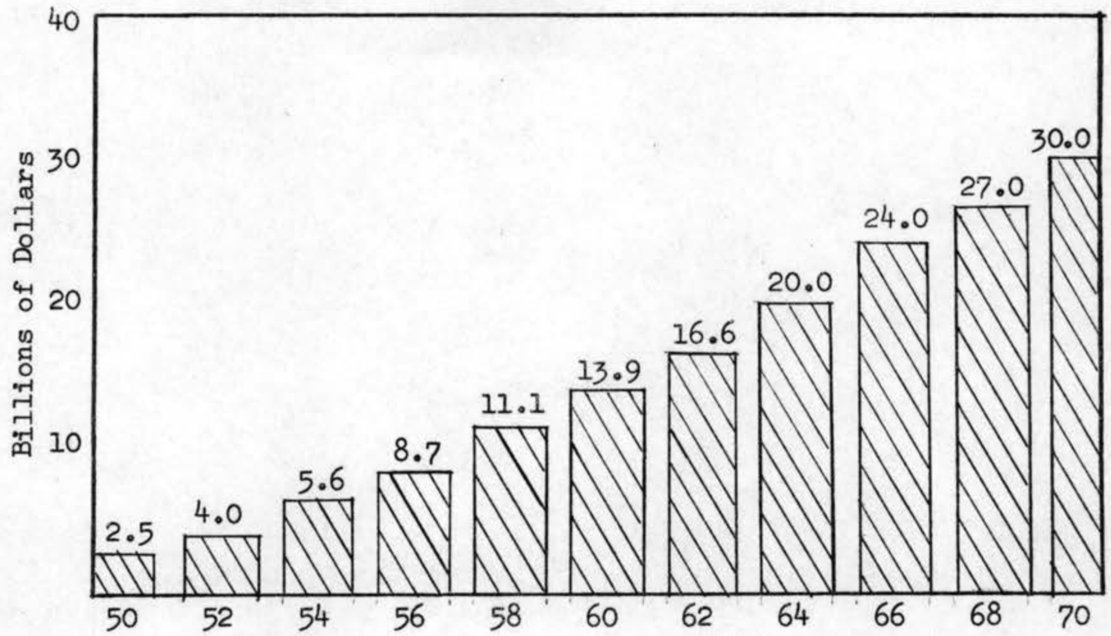

Figure 1. Research and Development Expenditures 1950-1970

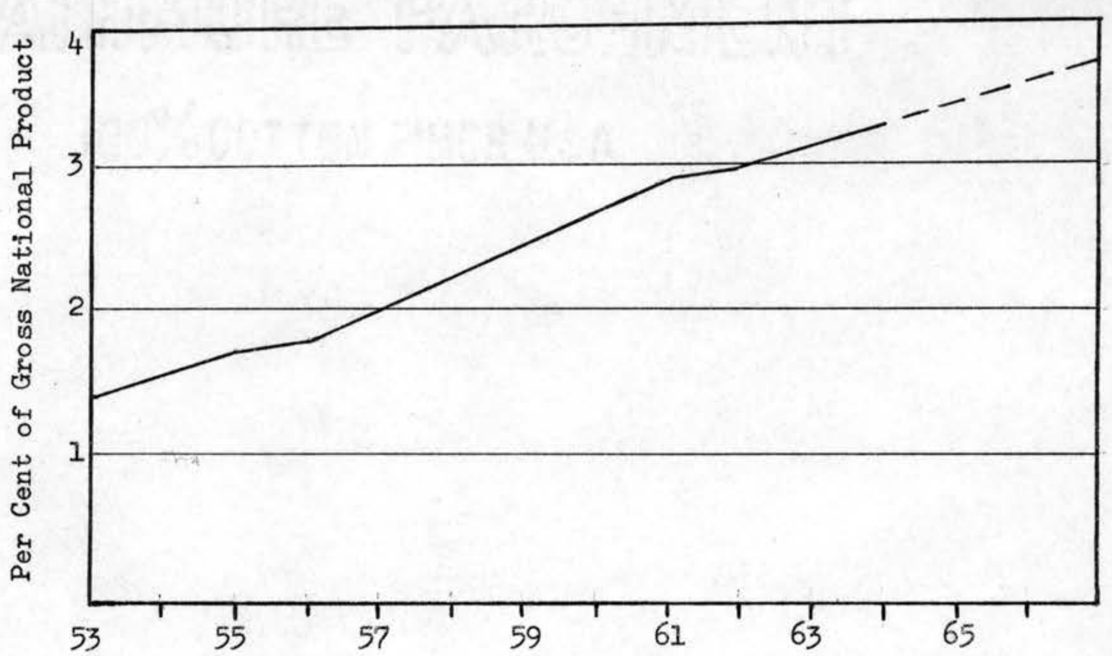

Figure 2. Per Cent of Gross National Products Devoted to Research and Development

must be done to insure the maximum utilization of the available technical manpower. Efficient management of research and development activities may well be the deciding factor in this race for supremacy in the Technical/Space Age.

But is the Country getting maximum utilization of its available resources? In no field so much as that of research and development is it so easy to make false starts, to spend hundreds of thousands of dollars in misdirected effort. Research is costly. It can be wasteful of both talent and money. The National Science Foundation estimates current costs of $34,000 a year in equipment and salary to maintain a professional man engaged in research and development. This represents an increase of 34% in the last four years (5).

In addition, there is strong evidence to indicate that research and development activities are becoming less efficient. A recent study by Booz, Allen, and Hamilton, Inc., (a management consultant firm) of 120 large companies, indicated that less than one-third of their research projects were considered even a partial success (5).

"There is little evidence that research productivity is matching the increased input of funds and personnel," declares Francis C. Brown, President of Schering Corporation, a Bloomfield, New Jersey drug manufacturer. Spending on pharmaceutical research and development increased threefold from 1955 through 1961, the industry's figures show. But, discoveries of new pharmaceutical chemicals averaged only 45 per year during that period, Mr. Brown observes; not much higher than the average of 37 per year in the preceding five years.

"Even these statistics are misleading," Mr. Brown adds, "because it was in the earlier period that the more important therapeutic advances were made" (5).

The prime importance of the management of Research and Development activities makes such management worthy of serious study and investigation. Management of research and development is described as "one of the great voids of industrial management" by the Financial Executives Foundation, an organization of corporate officers now conducting a two-year study of the subject.

The need for increased research to improve the utilization of the Country's scientific and engineering resources and to understand the art of Research and Development management is increasingly apparent. This is the conclusion of a major report recently released by the White House on the utilization of scientists and engineers in government, industry, and universities (6).

The report, authorized by the late President Kennedy on the advice of his science adviser, Dr. Jerome Wiesner, is the result of a two-year study by a committee of twenty-five, headed by James R. Killion, Jr. of the Massachusetts Institute of Technology. The study was conducted under sponsorship of the National Academy of Sciences and financed by the

Ford Foundation.

To improve the utilization of talent in science and engineering, the Commission called for systematic study of the art and science of research management. They urged that funds be provided for this purpose by industry, private foundations, and government.

New methods of Research and Development management required may be no less significant and complex than the research itself. Management methods and organization are not end objectives in themselves, but wise and efficient management can harness an exploding technology and direct it to assure our national and economic survival, as well as advance human welfare in the world of tomorrow.

The author undertook this investigation in an attempt to contribute to the understanding of the inherent problems in the management of Research and Development activities, and to see what operations research could do for technical management. The purpose was to see if this new management tool or approach could help Research and Development management in their difficult task of decision making.

The relative newness and the basic nature of research and development management is characterized by more uncertainty than are most other parts of the management function. Because of this uncertainty it might seem that any attempt to utilize an analytical approach to solve some of its problems would be foolhardy. Yet, because the need is so great, this may indeed be one way of obtaining a major payoff or improvement even though it is a frontier for analysis and one filled with intangible factors.

If this study can contribute to more thorough understanding of the problems associated with the management of research and development activities, and, in addition, suggests possible solutions to a few of these, it will fulfill its purpose.

CHAPTER II

SCOPE OF STUDY AND PROCEDURE UTILIZED

Management Problem Areas Classified

As an aid in discussing the scope of this investigation, the major problems confronting research and development management may be classified on the basis of (a) problem areas and (b) the type of management decision required. Any such classification must be artificial and arbitrary; nevertheless, the breakdown provides a useful approach. The problem areas are:

1. Technical program
2. Personnel
3. Organization structure
4. Financial
5. Service and support activities
6. Facilities.

Types of management decisions required are:

1. Determination of long-range objectives
2. Determination of immediate objectives
3. Operating decisions and actions
4. Evaluation of progress and results.

The complexity and broadness of the problems involved in the management of research and development activities, dictated that the areas to be investigated be limited. After studying the over-all situation presented above, it was decided to limit emphasis to investigating the determination of long-range and immediate objectives of the technical program.

Throughout the investigation, it is assumed that the <u>over-all</u> objectives, policies, and plans of the organization have been determined. This includes such things as general areas of investigation, amount of money to be devoted to research as well as the facilities and the number of personnel to be devoted to these activities. Emphasis will be placed on the determination of a methodology to follow in selecting what new projects to begin and which current projects to continue.

Research and Development Defined

The general term <u>Research and Development</u> -- like Truth -- cannot be defined in a manner that is universally acceptable. But, there is a fairly good agreement that it is the observation and study of the laws and phenomena of nature and/or the application of these findings to new devices, materials, or processes, or to the improvement of those which already exist (7).

Attempts have been made in the past to break Research and Development into categories, such as pure, basic, fundamental, supporting, applied, development, and the like.

This is all right, so long as one keeps in mind that fuzziness and overlap are ever-present. It is perhaps more useful to visualize research and development activities arranged in a spectrum (Figure 3) as suggested by J. B. Conant (8), with basic research at one end and process or product improvement at the other.

It is neither possible, nor necessary, to draw a distinct line on this spectrum and say this is "Applied Research" and that is "Supporting Research." However, since words must be used to communicate, the following general areas are defined:

Basic Research: Investigation and study of the fundamental laws and phenomena of nature with no preconceived notions of their utility.

Supporting Research: Investigation of certain fundamental laws and phenomena of nature with preconceived notions of their utility at some time in the future.

Applied Research: Pursuit of a planned program of investigation toward a practical objective.

Development: Application of the present state of the art to the solution of a practical objective.

This investigation was directed primarily towards the middle 75 per cent of the spectrum; i.e., in the general areas of supporting and applied research. Although most of

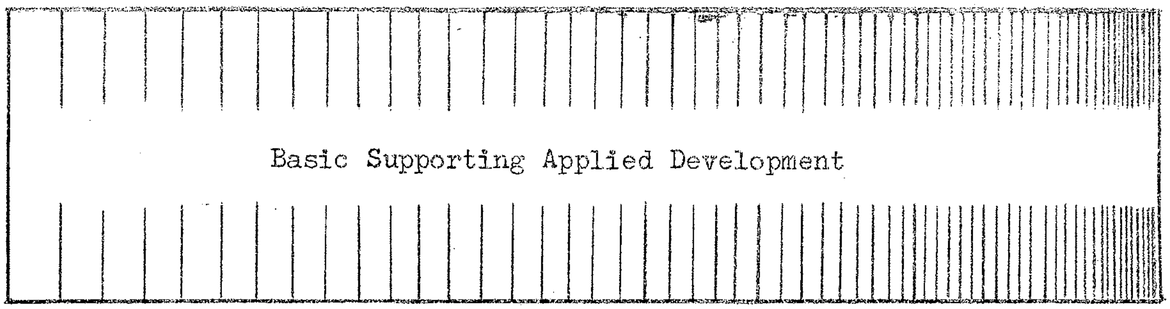

Figure 3. Research and Development Spectrum

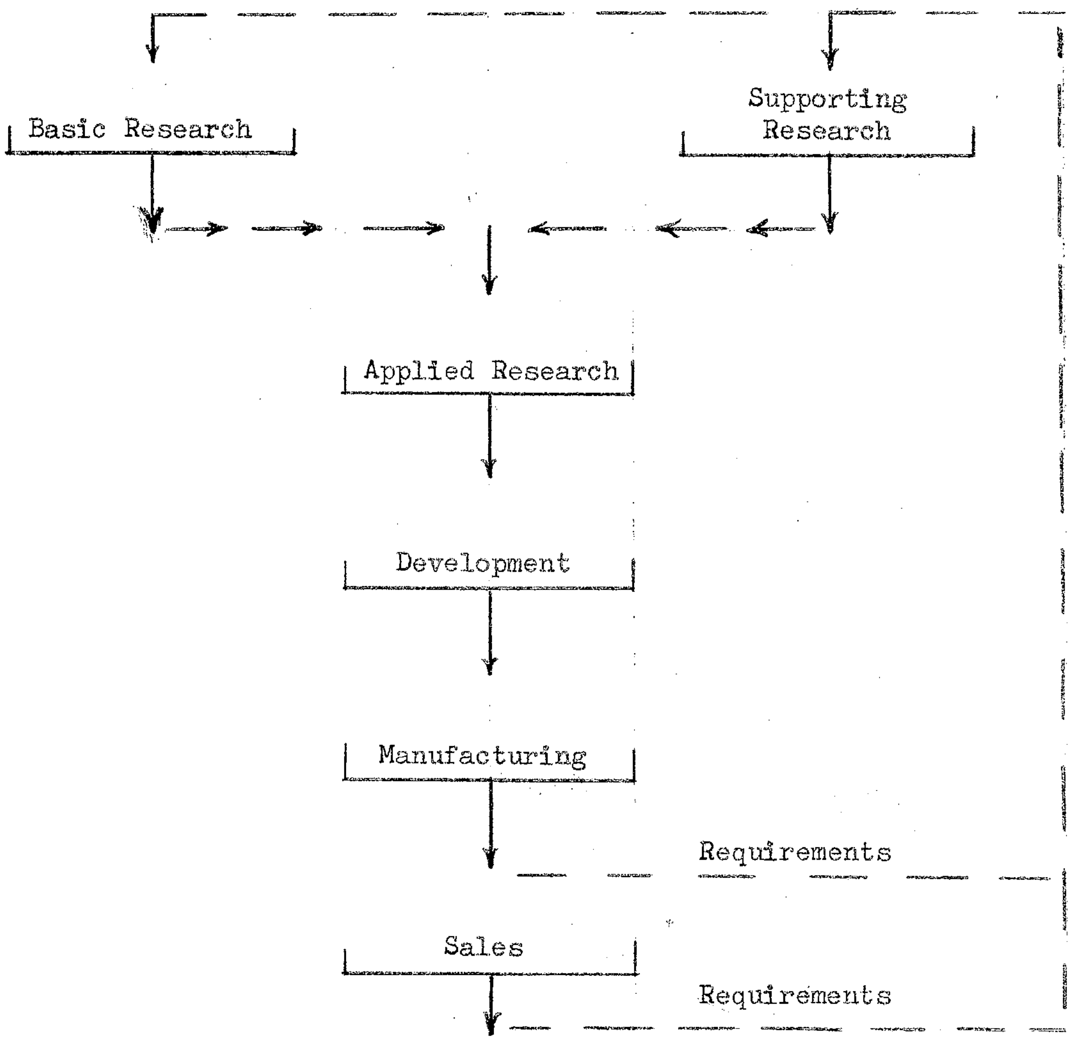

Figure 4. Flow of Technology

the problems inherent in Research and Development management are applicable to the whole spectrum, the two extremes present certain characteristics that require modified handling. These will be discussed later.

How technology advances through the various stages of research, development, manufacturing, and sales is shown in Figure 4 (on the preceding page).

Distribution of National Research Efforts

Research and Development efforts in the United States are funded and performed by three distinct groups of organizations:

1. Industry.
2. Colleges, Universities, and other Non-Profit Institutions.
3. Federal and State Governments.

Each of these groups has different motives for being involved in Research and Development activities; yet, there is a broad overlap of interests and programs. It can be seen readily from Figure 5 that the Federal Government provides the bulk of the funds (70 per cent), but industry performs approximately 72 per cent of the work (9).

Because of the different motives for doing research, and the different value systems used for evaluating it by the different participants, it was necessary to further restrict this investigation to primary consideration of technical programs of the type funded by the Federal

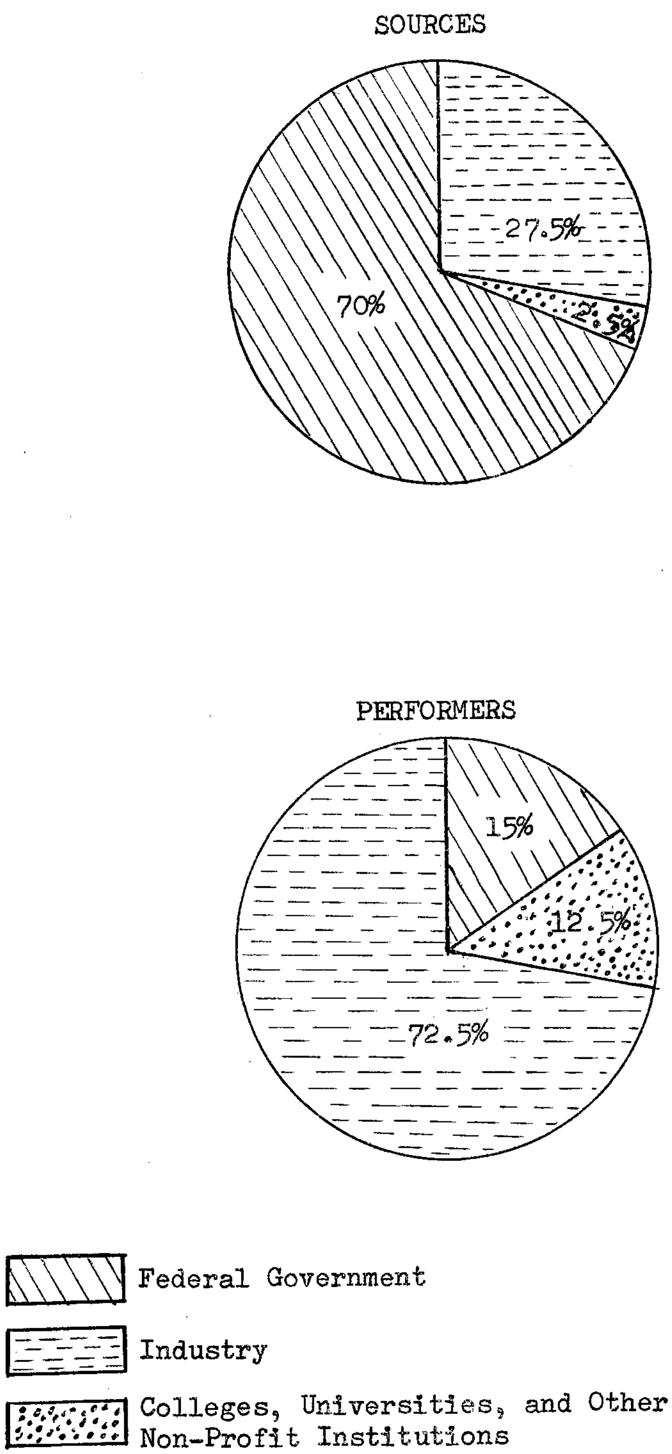

Figure 5. Per Cent Distribution of 1964 Research and Development Funds

Government. Since research managers of the Federal Government must make the decisions on the spending of 70 per cent of the Research and Development dollars in the United States, this area offers the greatest potential for improved efficiency and savings. Industrial research will be considered also, but to a more limited degree.

Summary of Study Scope

Our intensive search for knowledge and the means to employ it for the benefit of mankind, must be accompanied by a corresponding search for the principles underlying the efficient and effective management of Research and Development activities. In the past, an organization may have devoted its research effort to finding new products and processes and improving or eliminating old ones. Today, it must continue to do this, but it must also seek out - through planning and research on research decision problems - improved information and methods for administering and controlling its research program. Only by continually improving decision making procedures will management be able to offset rising costs, and be able to meet the competition from other organizations. The application of quantitative techniques and methodologies to the solution of some research and development management problems shows promise.

The crucial research and development decision problems are embedded in the selection, evaluation and control of Research and Development projects. The success of the research program, and perhaps of the organization, may turn on the way these decision problems are solved. It is to this area, decision making, that this study addresses itself.

The study is directed toward developing a decision algorithm to assist the research manager to decide how to best allocate his available resources for optimizing his return or output. Major emphasis is placed on selection of new projects, evaluation of existing ones, and determining what level of effort to devote to each. Most of the study is directed toward the middle 75 per cent of the research and development spectrum, and from the viewpoint of a government organization.

Procedure

The procedure utilized in this investigation is the sequence of steps used to attack problems in the research laboratory; namely, (a) identification and definition of the problem, (b) isolation and description of the factors relevant to the problem, (c) determination of a possible solution, and (d) evaluation of the proposed solution by comparing results against available pertinent knowledge about the actual system. Unlike many investigations of technical problems in the laboratory, it was not possible in this investigation to perform controlled experiments or to arrive at an indisputable solution.

The procedure used was: First, an extensive literature survey was conducted to ascertain methods being utilized and to garner the latest thinking on the subject.

Second, interviews were conducted with top management personnel and project leaders to obtain their opinions as to (a) what constitutes a successful Research and Development project, (b) what factors are important to the success or failure of a Research and Development project, (c) which of these factors can be evaluated, and (d) current practices within their organizations.

Third, based upon the foregoing, and the experience of the author, a system was designed to permit a Research and Development manager to allocate resources among a number of projects in a nearly optimal manner. This system was then discussed with several persons actively engaged in the management of Research and Development activities. Next, the system was modified to incorporate certain of the suggestions received.

Fourth, the system was actually applied in four research organizations to select and evaluate projects. The results were then evaluated against all available pertinent knowledge about the actual system and situation. This evaluation was done to determine the probable effectiveness and shortcomings of the system.

CHAPTER III

STATEMENT OF THE PROBLEM

Paradox of Research and Development

In his Conflicts of Principles, A. L. Lowell (10) describes the problem of ascertaining the true limits of what he calls "conjugate principles" -- a pair of principles which are mutually contradictory or inconsistent, yet each of which is partially, or under some conditions, wholly true. The Research and Development manager has a similar problem, except that instead of conflicting principles, he has apparently conflicting facts of reality. The conjugate realities involved are essentially (a) the outcomes and required resources for individual research projects are unknown and unpredictable, and (b) management must commit its available sources so as to optimize the payoff.

If management does not know for sure what the outcome of each project will be, how can it assure optimum utilization of resources? Somehow, Research and Development management must integrate these conflicting realities and reach the necessary decisions.

The problems of the Research and Development manager might be compared to the problem of the owner of a racing stable who wants to win a race to be run many years hence, on a track not yet built, among horses not yet born. To make matters worse, he must contemplate the possibility that, when the race is finally run, he may find that the rules have been changed, the track length altered, and horses replaced by greyhounds.

Definition of the Problem

Every manager of an organization engaged in research and development is faced with the same basic problem: how to obtain the maximum benefit from the available resources. In every Research and Development group, there are more desirable projects than resources available to pursue them. The resources of time, money, facilities, equipment, and - most important - competent, creative manpower, are always limited. The apparently desirable projects upon which to expend them, are not. Thus, the Research and Development manager is faced continuously with a painful problem consisting of three parts:

(1) Have the projects which have the highest potential payoff or value been considered?

(2) Of the projects thought of, which ones should resources be expended on?

(3) How should the available resources be divided among these projects to obtain maximum benefit?

The generation of ideas, their initial examination for feasibility, the choice of which to support and to what degree, are critical to the over-all effectiveness of the program. The decisions resulting from these phases will allocate resources in a pattern that is costly to modify or reverse. When large efforts are involved and choices are mutually exclusive, these decisions may, in the short run, be irreversible.

The manager of a Research and Development activity seeks to avoid two general types of error: (a) Failure to undertake "good" projects, and (b) undertaking "bad" projects. The reasons for the difficulty most Research and Development organizations have in avoiding these two types of error are inherent in the Research and Development process itself:

1. The outcome of individual projects, programs, and of the whole Research and Development process is highly unpredictable. That is, for other than technologically trivial projects, project selection involves decision making under (at best) risk -- where probability distributions can be associated with outcomes -- or (at worst) uncertainty -- where such probability distributions are not available.
2. The outcome of an individual project occurs with time lags of months or years, during which period some of the factors entering into the initial project selection decision (market

demand, material price, competition, available supporting technology) may change significantly.

Particularly in those projects that entail specific objectives, time constraints, and limited funds, the point of greatest flexibility in resource allocation occurs in the project selection phase. In later phases, when work is already in progress, it becomes increasingly difficult to change direction or reallocate resources without great loss.

Any system for the management of Research and Development must provide in some way for project initiation, selection, evaluation, and periodic review. If the system is effective, it should result in a smooth flow of well-chosen projects whose statuses are updated and reviewed periodically. In other words, of all those considered, those that have the best expected payoff to the organization within budget, manpower, and other limitations should receive the necessary Research and Development effort.

Importance of Time

The problem faced by management is to develop a product or provide the knowledge and/or solution to a problem, desired by a customer, by a date dictated by maximum payoff with the minimum expenditure of resources (men, money, and facilities). There should be a methodology which can be applied to any organization, whether commercial or governmental, which will assure the optimal use of resources to provide this maximal payoff.

The key factor appears to be time. Try as one may to separate the time factor from the allocation of resources and the reaping of profits or payoff, the separation cannot be made. Technological decay (obsolescence) and environmental decay (changing situation) and the present worth of future dollars further dictate the consideration of the time factor in evaluating costs and payoff. A part of the problem is that any project in a rapidly changing or competitive technology may be obsolescent by the time it is designed, developed, or the results ready for publication or dissemination.

Before the influence of time upon value and costs is discussed further, it is important to consider briefly another aspect of the problem: probability of success. In any discussion of research and development, one hears much concerning the terms "state of the art," "degree of newness," "risk or probability of successful conclusion." It is the opinion of the author that misunderstanding of such terms has seriously retarded progressive and dynamic thinking in the solution of the evaluation and selection problem.

Some contemplated projects are recognized as having very little prior experience for guidance in exploration, design, production, or use. The "state of the art" is considered low and the "degree of newness," high. With such a project it is also likely that the "probability of successful conclusion" is considered very low. What is really meant by such terminology? Is it not that there is insufficient knowledge, facts, and experience available to feel really confident about the future? Similar situations exist to a greater or lesser degree with all but the very simplest of developments. The truth is that all unknown facts and experiences <u>can be obtained at some cost</u> from applied research, pilot plant lots, market analysis, and thorough testing. Any project (other than basic research) can be successful -- if management is willing to pay enough in dollars and manpower.

Keeping in mind the meaning of the risk of being unsuccessful, the discussion can return to the considerations of time and its influence upon value and cost. Consider first the relationship between value of the results of Research and Development projects versus time of project completion.

The true value of a project is the value which would be attached to it if the total contribution it would make towards the objective of the organization were known. Figure 6 shows a typical plot of true value as a function of time. For a commercial concern, this may represent monthly revenues to be derived from a new or improved product; for the military, deterrent value of a proposed weapon system; for the space agency, international prestige derived from accomplishing a certain mission.

One can conceive of projects whose results are available too early. This is especially true of projects which

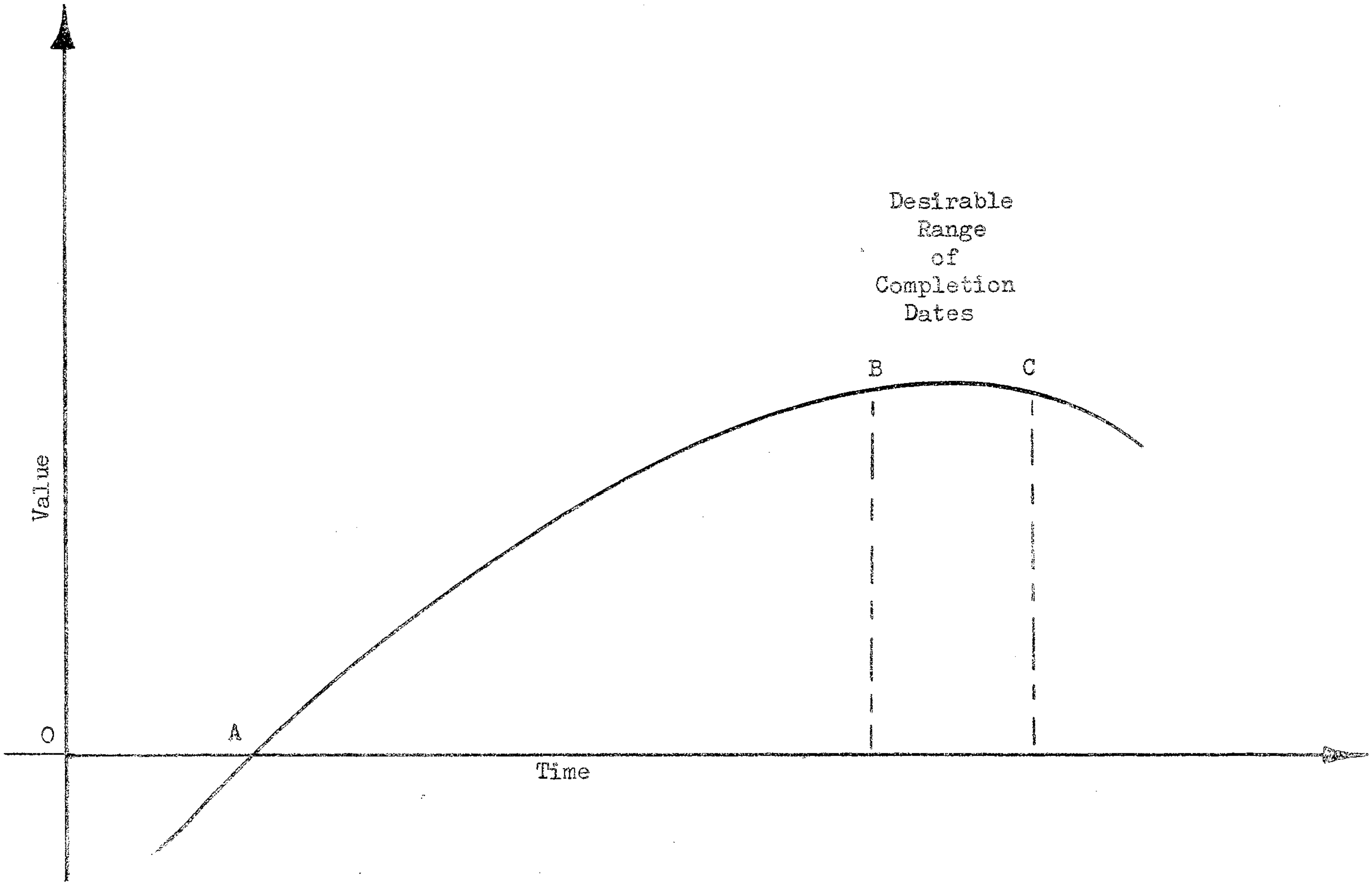

Figure 6. True Value as a Function of Project Completion Date

result in a tangible product. Prior to point A, the need for the results or the surrounding circumstances necessary for its use have not yet arisen. Should the results be available prior to point A, invested money, personnel time, facilities, etc., without a payoff represents a negative value. For example, Charles Babbage, the British mathematician, invented the basic principles of the digital computer around 1820-1830. He finally abandoned effort on it in 1842 when he could derive no value from his brilliant conception because the technology of his day was incapable of utilizing it. It was not until the advances of the electronics industry 100 years later provided appropriate technological circumstances that the true value of his concept achieved a positive value. It is probably a miss-statement to say that the project had negative value to humanity, but, from Mr. Babbage's viewpoint, that was the result.

After point A, the value rises to some peak, as at B, after which the need declines or the product has become obsolete and superseded by later developments.

The true value is usually not known at the time planners must make their decisions. They must proceed on the basis of estimates of value. It is characteristic of most planners that they initially underestimate the value of a project until they see the sudden rise sometime after point A. There, then, is a tendency to overestimate the peak true value and underestimate the rate of decline. In practice, the estimates of the true value curve are sometimes in error by a factor of 10 or 100. Even though it is not usually possible to have accurate numerical values available for a curve, such as that in Figure 6 (page 25), it does focus attention on a basic parameter of the problem.

The relationship of project duration and cost to complete the project is shown in Figure 7. The curve assumes that the same total effort is utilized regardless of project duration. A crash program to obtain the results in the shortest possible time is always very costly. If a project is stretched out over too long a period, the total costs will again rise as enthusiasm lags, personnel come or go, and momentum is lost. It should be noted that there is a minimum project duration below which the project cannot be accomplished. Costs increase exponentially and are prohibitive as this lower bound is approached. Thus, the time to complete a given project can sometimes be reduced by the use of additional resources, but only up to a certain point.

Unfortunately, true cost is as hard to accurately pin down quantitatively, as is true value. However, even government managers must somehow keep true cost in mind to achieve optimized output.

Basic Factors

There are undoubtedly many factors which contribute to, or influence, the value of Research and Development activities. Many of these, no doubt, are not even recognized; or

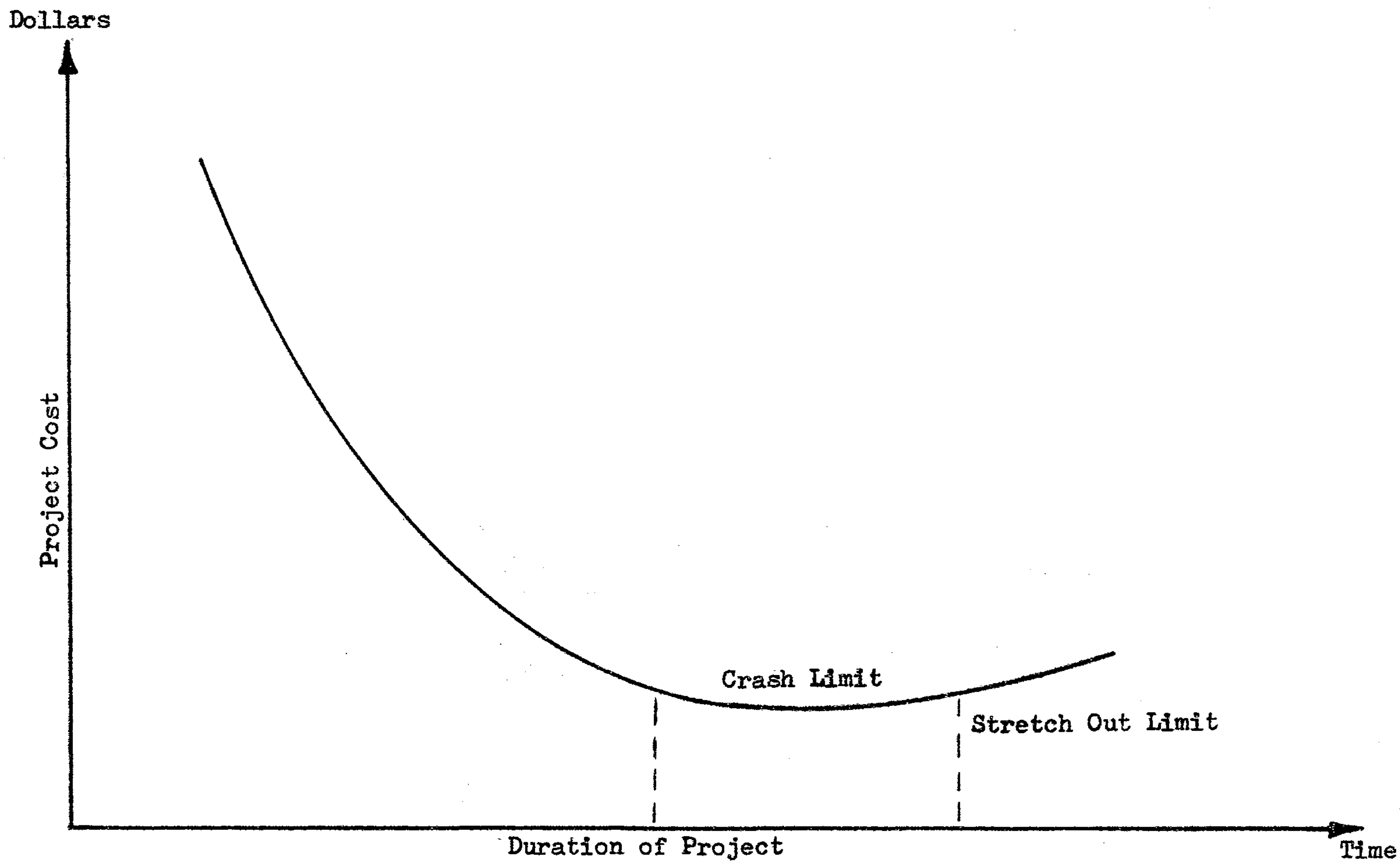

Figure 7. Relationship of Project Duration and Cost

at least, one is but dimly aware of them. Yet, it is possible to identify the prime considerations which should affect project selection. The following come to mind immediately:

Cost. Money, as discussed above, is one of the limited resources -- even for a government organization -- and is also a measure which everyone understands. Since cost consciousness permeates the entire economic structure, there can be no question that cost does, and must, play a role in the planning of research and development operations. Cost -- either initial investment or operating -- must be viewed as a significant factor in project selection.

Manpower. Two aspects are involved here. First, the American people attach considerable value to human life -- particularly in the nasty business of war. Substantial expense will be incurred to minimize waste of life, whether of military personnel in war, or of astronauts in peaceful exploration of space. Thus, costs may be subordinated to the value of human life. Secondly, manpower is also a limited resource, especially highly trained, creative persons. In the face of such a limitation, conservation of trained manpower for use only where critically needed is an important factor.

Time. The importance of time has already been discussed in some detail earlier. But time has another significant aspect to some government organizations. The decisive results which may accompany the employment of a new military weapon against an enemy whose defense is ineffective against it, are well recognized. Also, to avert disaster, time can be crucial in the development of countermeasures, both in the military sphere, and in the area of public health and welfare. Even in the area of peaceful exploration of space, time has great significance. No prestige or propaganda advantage is accrued by those who are second. This fact was well demonstrated by the impact of the launching of Sputnik I. For these, and the reasons discussed earlier, time may overshadow both cost and manpower.

Need. This factor is akin to time in the sense that need for an item becomes acute where limited time is available to get it. But need is meant here to be a strong motivation in its own right. Need is a concept different from the first three. It is a measure of how badly something is wanted, for whatever reason; the others then measure the expense in men, time, and dollars required to have and use it. Need is satisfied, cost is

> incurred, so to speak. But the cost in men, time, and dollars may indicate that the price is too high, that the need is overvalued. Thus, it is believed that need is a consideration along with the other three.

Thus, these four factors appear to be the fundamental parts of any measure of effectiveness which may be applied to research and development operations. The relative weights to be assigned will change from one situation to another, but in almost every case one factor will generally overshadow the others and, in a sense, become the predominant consideration. Thus, any methodology or system for selecting or evaluating projects, on the basis of only one of these factors, is not sufficient.

In summary, the problem to which this study addresses itself can, therefore, be defined as one of devising a system to aid the research and development manager in selecting the best potential projects to work on, and allocating his available resources appropriately to obtain maximum benefit or payoff, keeping in mind the influence of cost, manpower, time, and need.

CHAPTER IV

OTHER FACTORS RELEVANT TO THE PROBLEM

The Dilemma of Evaluation

Two aspects of the research and development activities make decision-making particularly difficult. The first, (already mentioned) is the uncertain outcome of individual projects - ranging from the question of the cost and time required for a project or program to be successful, to the question of whether it will prove out to be successful at all. Second is the difficulty, even after a project has been "successfully" completed, of telling just how successful it has been, and how much of the success is due to the efforts of the research organization itself.

The output of a research organization falls into two categories:

1. <u>The direct products -- information</u>. This includes all of the new knowledge, formulations, patent applications, operating instructions, product specifications, advice, diagnosis of difficulties, service reports, and other information turned out in accordance with the objectives of the Research and Development

program. This is an intermediate step in the accomplishment of tangible results through research.

2. <u>The indirect products -- tangible results</u>. Few research and development departments have the opportunity, directly, to bring about tangible results such as increased revenues, decreased costs, and increased profits. These ultimate results are brought about by other organization activities, supported by the information that is provided by the laboratory. While the ultimate success of Research and Development, thus, depends heavily on the quality and usefulness of the researchers' findings, it depends also on the ability and willingness of the rest of the organization to apply the information supplied.

This situation leads to the dilemma that confronts many laboratories. While the direct product of the laboratory is information, what management is interested in evaluating is tangible results. However, it is difficult to attribute results to Research and Development on a logical and equitable basis because the information is applied by other activities.

Having a <u>potentially</u> useful result, such as a new product or new information from the Research and Development organization, does not assure the parent organization a payoff. The influences of the comptroller, production engineering, manufacturing and marketing can still make or break the end results. The instances of very poor or marginal products developing into tremendous financial successes because of brilliant exploitation are numerous and well-known. So, too, are the instances of financial disaster of technically sound and brilliantly conceived products which have been poorly commercialized.

Because technical success of the Research and Development project does not guarantee commercial success, it appears to be exceedingly difficult -- and of questionable value -- for the Research and Development organization to make firm predictions of potential markets, estimated production costs, etc., over which they will have no control.

Achieving a Balanced Program

Another aspect of the problem of project selection is that associated with achieving a balanced program. As would be expected, programs of all but the smallest research organizations are composed of projects of varying complexity. In very general terms, these may be classified as long- and short-term projects, depending on the length of time required for a reasonable obtainment of results. An arbitrary system of classifying projects according to duration found useful by the author is shown in Figure 8.

Determining how much effort to put into the solution of present problem areas and how much on problems of the future is one of the enormous challenges confronting the research

PROJECTS CLASSIFIED BY DURATION		
Name	Estimated Period of Duration	Expectancy
Short Term	One to six months	Quick results, usually successful
Intermediate Term	Six months to one year	Reasonable assurance of success
Long Term	One to several years	If successful they usually bring a large return on the investment
Miscellaneous and Service	One to several weeks	Improvements of methods, minor improvements in processes and products

Figure 8. Projects Classified by Duration

director. An over-emphasis in either direction can be disastrous. It makes little sense to concentrate on solving day-to-day problems while the parent organization is left hopelessly behind technologically. On the other hand, there is no point at all in solving tomorrow's problems if the parent organization should fail before tomorrow comes.

In the business world, the research director knows that long-term projects are needed to develop the new products which insure the future health of the company. Because of their long-term nature, however, concrete results are slow coming. A program made up of a preponderance of such projects may cause worker morale to suffer because of a lack of concrete, continuous achievement. At the same time, top management may wonder if the research organization is producing anything of value to the company.

Short-term projects, on the other hand, hold out the promise of quick, virtually assured, success. They favorably affect the morale of those associated with their success, and the research organization may soon be able to show top management an imposing list of accomplishments. The danger of an imbalance in the direction of short-term work is concealed in the complacency which may overcome the company by virtue of a favorable competitive position with regard to current products alone. G. O. Cragwall (11), Director, Technical Services Department, Charles Pfizer and Company, Inc., put it this way:

> Progress results in the appearance of new products as well as in the improvement of old ones. A company whose research program has been devoted to short-term projects may some day find that although its products are better and less expensive to produce than similar ones of other manufacturers, they are, unfortunately, being replaced by new products that have been developed by their competitors.

Almost all of the pressures are exerted in the direction of selecting short-term projects; i.e., the high probability of success, the securing of quick results, the sense of achievement in completing a project, etc. If the research director does not consciously recognize this, and take action to assure a balanced program, his apparent success may well be short-lived.

The director of government research activities is faced with the identical problem. His problem, however, usually goes under the guise of development projects versus basic and applied research. If he concentrates his efforts on today's weapons, or spacecraft, he exposes his country to future disaster. He must not only build and improve today's deterrent force, or today's space vehicles, but he must also lay and expand the scientific base for the weapons or explorations of tomorrow.

As in the industrial sphere, quick results can be achieved in short-term, relatively simple projects. It takes a man of courage and strong convictions to insist on a level of long-term effort, even though that technical effort is necessarily denied to or diverted from short-term work.

Unique Problems of the Government Administrator

There are other factors which make the selection of projects for a director of government research more difficult. First, the objectives of government research are not so distinct as are those in industry. The industrial laboratory is a part of the economic order, and research must contribute in some way to the satisfaction of economic wants. Such wants can be foreseen, by and large, and in some cases even be created. Research, then, is directed toward an economic objective in industry. Research in the government, on the other hand, is pursued in the interest of the public welfare. Considerations of cost, time, reliability, performance, national prestige, and a host of others, enter. But, it is discouragingly hard to define the precise terms of the objective.

Second, there appears to be no measure of research effectiveness quite so striking as the profit and loss statement of the industrial enterprise. The driving force behind all industrial research is the financial condition of the company, be it reduced costs, higher quality products, or a new line of products to promote business survival. The profit motive is totally lacking in government research, even though the consequences of failure may be more serious than mere bankruptcy.

An authority on management of research activities, R. N. Anthony (12), notes that the spur of competition which provides a stimulus to good performance in industrial laboratories is not so effective as in government activity.

> The research and development work done in an industrial laboratory eventually has to meet the test of the market place. Although it is difficult to appraise the performance of any research organization, it is reasonably clear that if competitors frequently introduce better products, or if they introduce new products more quickly, then the record of the research organization is not good.

The government research organizations, on the other hand, has neither periodic nor continuous tests of its performance. Although national survival may depend on the results of its work, the <u>ultimate</u> test may come too late. The outbreak of war or of an epidemic is a poor time to find out that the research program has been insufficient or ineffective. Hence, the government administrator has no yardstick comparable to the competitive market for appraisal of his results.

Lacking the economic or profit motive to aid him in planning and evaluating his program, he must look for other criteria. Criteria such as the public welfare or the public good are so abstract in nature that they are of little benefit. What is in the public welfare: crash programs to develop new weapons systems in the shortest possible time, in case war comes tomorrow, or an orderly, lower cost, normal priority development which will help keep taxes lower, the budget balanced, but take longer? Tell the military Research and Development administrator when war will break out, and the nature of that war -- then he can decide precisely what is in the public welfare. But who can give him that information?

How can the government Research and Development administrator in the space exploration field determine the worth of national prestige? What criteria can he use to determine how much he is justified to spend to launch a certain satellite in January versus one in July? How much is an increase in reliability from .995 to .996 of a certain valve, which will be used on a manned vehicle, worth if it is assumed that measurements of this precision are possible? How much is enough? How much is too little?

His only guide is the amount of resources available to him from the public, through their elected officials. But, within this broad guideline the possibilities are still very nearly infinite.

A government Research and Development organization must go to Congress annually for money. The amount of financial support received varies with the economic outlook, the political outlook, the military outlook, the current popularity of research and the general attitude of Congress. Thus, the government research administrator must expect fluctuating financial support for his research program. He must decide whether to begin any long-range project (taking more than one year) without assurance that it can be financially carried to a successful conclusion. Although this is somewhat true of industrial organizations, there is a marked difference in degree.

Other problems also make the government administrator's job more difficult, such as personnel policies, control from Washington, fiscal and accounting policies, and procurement policies; but again, most of these are matters of degree. None of these <u>precludes</u> the government administrator from doing a good job of conducting a worthwhile research program, but they certainly do make it more difficult.

Pressures for Continuation of Submarginal Projects

Perhaps no other phase of research is more important - and at the same time - more neglected, than its evaluation. And surely no problem is more vexing than that of deciding when to abandon a project. It is not enough for management to decide to start a Research and Development project; they must also decide whether to continue it.

Clearly, elimination of a submarginal project is of little importance if resources are unlimited, but such conditions seldom, if ever, exist. It is, therefore, important not only to screen these ideas prior to committing them to the research program, but it is equally important to eliminate submarginal projects at the earliest possible date. Only by doing so, can maximum results be obtained from the limited resources.

Very simply, submarginal projects are those that should be removed from the active Research and Development program. Such projects include:

1. Those that are technically not feasible. They obviously appeared to be feasible once or they would not have been started. Subsequent investigation has since demonstrated their unfeasibility.
2. Those that are technically feasible, but will cost more than they are worth, and the need is not vital. Early estimates of resource requirements or potential payoff were much in error.
3. Those that are technically feasible and would be worth more than their cost; but they are consuming critically needed resources which promise a much higher payoff if applied elsewhere.

Most of the literature does not even consider this particular problem, or else gives very cursory treatment of it. It is apparently assumed generally that if projects are carefully chosen, then the problem of eliminating the submarginal ones is automatically solved. So long as management is dependent on estimates and crystal balls, however, it will be faced with the problem of identifying and eliminating submarginal projects. The reaction of project personnel to the deletion of "their" project must be predicted and given consideration. "Pressures" that tend to influence decisions must be clearly recognized and action taken to eliminate those that might result in biased decisions. Therefore, it is important that one tries to identify and plan for as many of these pressures as possible.

First, cancellation of a project is frequently thought by higher authority to be evidence of poor original planning and initial preliminary study. The laboratory recognizes this and reacts to it, so that unless projects are obviously doomed to failure, there is a tendency to retain them in the hope that original plans can be fulfilled.

Second, there are some pressures which originate within the laboratory itself. It is not unusual for project engineers to become so interested in their projects that they fail to recognize the deficiencies of them. In addition, they usually feel a proprietary interest in the work. Any attempt by management to eliminate "their" project is likely to be taken personally and resisted. Sometimes, this feeling can be so strong that it becomes a valid argument for retention of the project. If the project personnel are high-caliber, and are involved in other important work, retention of their "pet" project may be a better alternative than the consequences of cancellation. It is also well known that a project engineer's driving interest in the project can sometimes result in success despite apparently overwhelming odds.

Third, in addition to the sincere project engineer, there may also be the not-so-sincere "empire builder" who resists any attempt to reduce the scope of his work. In such a case, management may find it necessary and desirable to eliminate both the project <u>and</u> the empire builder.

Fourth, pressure to keep the project can come from individuals of higher authority. This pressure is generally not the result of action to further personal gain, but results because the office or individual has so strongly supported the project that there is a strong desire to see it completed. When a laboratory recognizes that there is such strong feeling for a project at higher levels in the organization, it will be quite hesitant to recommend elimination. This is particularly true since any evaluation of the project's worth must be subjective, and, thus, hard to defend in the face of strong opposition from higher authorities.

Fifth, is the pressure exerted upon the administrator due to a crowded and busy schedule? In almost any laboratory, there are always "hot projects" that demand immediate attention. Thus, the laboratory director and his staff are kept busy "putting out fires." Matters requiring immediate attention are worked on, while project review and elimination of submarginal projects is postponed and put off into the future. Because of other pressures, the laboratory director wants to be doubly sure of himself before he cancels a project; he wants to probe the problem in depth, but does not feel he has the time. He keeps thinking perhaps next week will be better.

Finally, the most important factor which causes the laboratory to hesitate in deleting a project is the inherent difficulty of deciding just <u>which</u> projects are submarginal. Unfortunately, no one has yet been able to devise a test which will positively indicate submarginality. Therefore, the decision must be made by subjective judgment and such decisions cannot always be right. In addition, because such judgments are opinion, they can always be challenged by any source that favors continuation of the project. There is also the recognition that mistakes in judgment can have far-reaching effects which may not be apparent until it is too late.

All of this makes the task of screening and eliminating projects very unattractive. It is much easier to let the projects continue until completed, so the worth of the project can be more firmly determined. But, to do so, may prove in the long run disastrously wasteful of precious resources.

CHAPTER V

CURRENT METHODS OF PROJECT SELECTION

The Dichotomy of Current Methods

Having briefly defined the problem and other relevant factors, it now appears appropriate to discuss prior studies and current methods employed in the evaluation and selection of projects.

In reviewing the literature and current practices, no two perfectly identical systems were revealed. However, every system reported had been adapted from one of two competing approaches. These approaches can be called the quantitative and qualitative approaches. Each approach has its major advocates, who champion it vigorously in current literature. Each has supporters among progressive and successful research administrators.

The Quantitative Approach

The quantitative technique typically seeks to evaluate a research program by use of a single mathematical formula. Theoretically, this formula discloses the maximum an organization can profitably spend on a particular project. Formulas in use often include such factors as:

1. Profits from products created or improved through research.
2. Savings from processes, methods or raw materials improved or discovered through research.
3. Income from research -- derived royalties.
4. Royalty payments to other organizations, eliminated by research.
5. Profits attributable to research -- created good will.
6. Investments in Research and Development programs.
7. Investments required to bring research to commercial fruition.

The industrial manager works within a frame of reference defined by company policy. The company's field of interest obviously limits the scope of the research activity. Within the boundaries established by company objectives, the most significant criterion for selecting a project is: What will it do for the company? Indeed, no research project would be undertaken if it were known in advance that the company would not benefit. Therefore, a major objective of industrial Research and Development policy is profit, and quantitative systems are attempts to obtain better appraisals of the profit potential of projects.

Although most research executives and company managers have had little success in applying mathematical formulas to the evaluation of Research and Development, many continue efforts to develop valid quantitative yardsticks, such as a "profit-and-loss" function, or an "index-of-return" formula, or a "project value-to-cost" ratio. They use such formulas to decide, in advance, whether a particular project is worth undertaking, as well as to appraise the general profit value of research projects after completion. Several methods reported in the literature will be described. Note that this report of methods does not exhaust the variety of methods proposed and in use. It is merely illustrative -- not comprehensive.

Most of the formulations suggested and used by industry for project selection are fairly simple, including terms for anticipated costs and returns and generally, but not always, a term for risk or probability of success. For example, one of the earliest reported in the literature used the index of return (I.R.):

$$\text{Value of a new project} = \frac{\text{Estimated I.R.} \times \text{Probability of Success}}{\text{Estimated cost of research}}$$

where the I.R. is computed as:

I.R. = (the value of the process savings for one year + 3 per cent of the sales value of new products each year for five years + 2 per cent of the sales value of improved products each year for two years.) (13).

Solomons Disman (14) reports a scheme in which expenditure for a Research and Development project is thought of as an investment. Over a certain period of time, the income stream from the investment should return the original investment plus a profit. Thus:

> Investment + (rate of return on the Research and Development expenditure) x (Investment) = Income. For one year, this expression becomes:
>
> $$\text{Investment } (1 + r) = \text{Income in one year}$$
>
> $$\text{Investment} = \frac{\text{Income for one year}}{1 + r}$$
>
> where r = rate of return on Research and Development expenditure.

This equation is then used to determine the maximum expenditure justified (MEJ) for a Research and Development project, given a rate of return expected from Research and Development effort and estimated net income from the product developed.

This scheme further utilizes two risk factors, the risk of technical success (R_t) and risk of commercial success (R_c). Thus, the MEJ calculation for a project payoff over n years becomes:

$$\text{MEJ} = R_c R_t \sum_{i=1}^{n} \frac{\text{Estimated net income in } i^{th} \text{ year}}{(1 + r)^i}$$

where MEJ = Maximum expense justified

R_c = Risk of commercial success

R_t = Risk of technical success

r = Rate of return on Research and Development expenditure.

Another organization evaluates results by comparing the five-year estimated revenue with the total estimated development expense (15). Results are evaluated by these formulas:

$$\text{Return on investment} = \frac{\text{New Earnings (after taxes)}}{\text{Total investment involved}}$$

$$\text{Payout period} = \frac{\text{Capital outlay on projects}}{\text{New average annual revenue (after taxes)}}$$

The estimated return value of the research results are sometimes evaluated in terms of the present worth or discounted net value (DNV) of the anticipated results (16). The determination of the present worth of a proposed project involves calculation of any tangible earnings or savings computed in terms of the net return on capital investment, current interest rates, and capital recovery periods. Because the results are expressed as estimates, this refinement is usually eliminated except where the projects are expected to run over several budget periods. One formula widely used for these calculations, the Hoskold transformation, is given as:

$$P = R + \frac{\frac{D}{R!}}{(1 + R!)^{n} - 1}$$

where P = present worth of net income

D = net income

R! = average net return on capital investment in the enterprise.

R = current rate of interest

n = capital recovery period in years.

Aries and Happel (17) have proposed a different type of evaluation which establishes a measure of "venture profit" which is distinguished from the normal profit generally realized in the business. The profit on a given investment in research and plant after taxes is compared with the same investment in going operations. Various research projects can then be compared on the basis of their anticipated extra return.

Dean and Sengupta (18) proposed a variation of the "index of return" formula. Assuming that a project succeeds technically in the sense that a usable result will be derived, and assuming that the results are exploited or commercialized, a measure of relative performance of any two projects is furnished by the ratio:

$$\pi = \frac{\text{present value of future returns from the project}}{\text{present value of future efforts required for the project}} = \frac{S}{R}.$$

The ratio of π is referred to as the payoff function of a project. The returns and efforts are measured in dollars. If the rates of return and of spending are continuous functions of time, this ratio can be expressed as:

$$\pi = \frac{\int_0^\alpha s(t)e^{-\rho t}dt}{\int_0^\alpha r(t)e^{-\rho t}dt} = \frac{S}{R}$$

where:

t = time in years since the inception of the project

$s(t)$ = rate of return (either sales, or cost-savings or incremental sales) at time t

$r(t)$ = rate of spending on the project at time t

ρ = a discount factor which the firm can choose at will.

An index π_i, for the i^{th} project in a set indicates the expected dollars' worth of future sales per dollar's worth of future expenditure on Research and Development. It is suggested by the authors that the uncertainty about the future status of competition and customer-acceptability can be introduced by choosing an appropriate level of the discounting factor ρ.

The foregoing examples indicate the extent to which objectivity is being injected into the selection of industrial research projects. Fortunately, industry has a powerful measure of value - the dollar - to which, in one form or another, their analyses lead. But, the business world recognizes the limitations of analyzing the dollar value of proposed research in the basic research area. Indeed, those companies using indices as a matter of policy in project selection permit a substantial amount of work in exploratory research before a project is subjected to evaluation. Despite continued progress and increasing interest, the sad fact remains that a satisfactory quantitative approach has not yet been found or widely accepted in industry. When one moves to the governmental research sphere, the picture is even more disappointing. This is attributed to the lack of the profit motive and the difficulty of expressing value in terms of dollars.

Sobelman (19) has derived a quantitative method of application to the development of military weapons. His basic equation is:

$$Z = pT - ct$$

where:

Z = product value

p = average net profit/year

T = useful life in years

c = average development cost/year

t = years of development.

Thus, his objective equation for a series of m projects becomes:

$$\text{Maximize } Z = \sum_{i=1}^{m} (p_i T_i - c_i t_i).$$

Since money is worth more now than later in long-term projects, Mr. Sobelman modifies his equation. To discount all dollar values to a present-time scale of reference, he multiplies dollar values by $1 \div (1 + i)^n$ where i is the rate of interest and n is the year of occurrence. This formula can be used for periodic evaluation at different steps in a product development.

If average development time $\bar{t}$, and average useful life $\bar{T}$, are known for a class of products, then projects having a shorter-than-average development time, or longer-than-average market time, can be recognized and rewarded.

Let

$$T^* = T + \bar{T}(1 - t/\bar{t})$$

$$t^* = t + \bar{t}(1 - T/\bar{T}).$$

Then, the revised formula is

$$Z = pT^* - ct^*$$

Although this scheme is proposed for use by government managers, immediate difficulties are encountered in trying to apply this concept to an actual situation. Since this equation is expressed in terms of dollars and time, one must be able to place a dollar value on such intangibles as human life, reliability, weight, psychological impact on the enemy, etc. Although it is possible to derive such numbers (as illustrated by Mr. Sobelman's (19) evaluation of the value of a soldier's life of $8,000), their meaningfulness and accuracy are open to grave doubts.

The RAND Corporation under Air Force sponsorship has put much effort into trying to derive techniques for choosing among several different approaches to the development of a specified weapon system. After spending three years of developing their techniques, they applied them to a number of completed projects. Their conclusion was that they would have been wrong more often using their derived technique than if they had tossed a coin to make the decisions.

Great progress has been made, however, in the application of Operations Research techniques to weapon cost-effectiveness analysis. Hatry (20), Klein and Meckling (21), and others have devised and demonstrated techniques for deciding whether to go into mass production on a weapons system or what mix of weapons systems optimizes the military commanders cost-effectiveness position. These techniques are effective, however, only after the weapon systems are far along the development path, or are of a minimal improvement nature.

The National Aeronautics and Space Administration has also applied Cost-Benefit analysis in deciding which launch vehicles can most economically perform specified missions. Again, however, success has been limited to comparisons between vehicles in an advanced stage of development.

Quantitativists say that sound, objective formulas have two advantages. First, they present numerically the most accurate possible estimates of research costs and returns, which qualitativists -- in determining research appropriations -- must compare informally anyway. Second, since dollar figures and ratios are easily understood, they help persuade top management (who, in a big company, cannot possibly be familiar with the details of the research program) to give a proposed program the support it deserves.

The Qualitative Approach

The qualitative school, on the other hand, believes, as one executive put it, that "formulas can't be relied on to evaluate research." The qualitative school, therefore, prefers to depend on admittedly subjective "broad composite management judgments," which it seeks to strengthen through non-mathematical procedures and devices. Its adherents say:

> "When you come to research and development, you can't answer any of the questions on the forecast. You don't know when you are going to get the thing, whether it is going to work or not, and whether it is going to have any value whatever ... ." (Charles Kettering (22), ex. G. M. Director of Research)

Most of the procedures used by the qualitativists have much in common. Typically, a series of personal appraisals pyramids through the organization from bottom to top. Each technical executive judges the work only of those groups and persons reporting directly to him, relying on subordinates to evaluate work at lower levels.

Some evaluations are part of technical management's daily job. Others are made during formal, periodic, program reviews. Such a review usually is made each time a new research budget is developed. Some companies review their projects on a regular periodic basis or at certain stages of progress.

Regardless when, or how often, these reviews are held, the process is usually stereotyped. The executive committee (a) reviews research progress during the period just past, (b) decides which proposed projects and programs to support, and (c) fixes the budget on the basis of this decision. Normally, all operating and functional groups are represented on this committee. This representation helps insure a balanced research program, to give every present and proposed product line the long and short range support it needs to fulfill its objectives within the company's over-all operating plan.

Many scientists and Research and Development administrators are deeply concerned about even this growing tendency toward formalization of laboratory decision-making. They believe that such formalization will tend to stifle many desirable projects and that the intuitions of the research worker (which cannot be neatly written down in a formal project proposal) are the best guide to the potential worth of a project.

Mee's (24) comments are typical.

> The best person to decide what research work shall be done is the man who is doing the research. The next best is the head of the department. After that you leave the field of best persons and meet increasingly worse groups. The first of these is the research director who is probably wrong more than half of the time. Finally, there is a committee of company vice presidents which is wrong all of the time.

This attitude is rapidly fading, however, as more and more organizations recognize the need for management control of research and development.

Limitations of Current Methods

Considering first the quantitative approach, an analysis of the literature regarding both industrial and governmental Research and Development decision procedures, rapidly brings one to the heart of the problem: <u>lack</u> of reliable cost, time, performance, and utility estimates. No matter how sophisticated and mathematically correct the formulas and ratios utilized may be, the answers received will be no better than the input data.

The findings of a group at the RAND Corporation studying military Research and Development projects bring this into clear focus (25). Cost estimates, based on paper design studies have tended to be highly unreliable. For example, estimates of production cost of missiles erred by factors ranging from 1.3 to 57.6, with a mean of 17.1. Slippage in availability-of-aircraft estimates ranged up to five years, with a mean of two years. Cost estimates of Research and Development costs for all systems studied missed by factors of five or more.

Uncertainties are great even after a project has progressed to a point where investment in plant and equipment is called for. Carter and Williams (26), studying a number of British companies, found that even with an attempt to estimate project return, the correlation between actual return and estimated return was an extremely low factor of .13.

Besides problems of getting valid estimates of pertinent parameters, no sound method exists for apportioning credit for a products profit between a company's research department and its other functional groups. And finally, research costs usually cannot be allocated among various projects, except subjectively and imprecisely. However, impressively mathematical they may look, formulas based on such figures provide answers no sounder and no more trustworthy than the personal judgments that underlie these figures.

In the realm of government or university research, one is confronted with all of the above problems, plus several not faced by industry. As was discussed earlier, the Research and Development manager in government cannot as easily tie his decisions to dollars and profits. In many, in fact in most cases, it is impossible to attach a dollar figure to the value of successful completion of a project. Dollar values ascribed to such factors as national prestige, human life, psychological impact, and others that government executives must deal with are meaningless -- or at best, of a highly questionable nature. For example, how would one assign a dollar value to a universal cure for cancer? What is the quantitative value of a major advance in knowledge or understanding? Methods proposed at present do not offer adequate tools for handling these problems.

On the other hand, the strictly qualitative approach is not entirely satisfactory either. Although it may help to spread the blame for poor decisions, it really does not appreciably help the decision-maker. It does not provide the analytical structure he so sorely needs.

True, formulas do not provide precise answers. But, some aspects of Research and Development are fairly well suited to quantification. Moreover, the mere calculation of quantitative values -- where they are appropriate -- forces managers to think about the research contribution in a concrete and orderly way. In other words, while evaluation of any Research and Development program requires many qualitative judgments, still, appropriate numerical data <u>can</u> be useful in appraising some of its aspects and in formalizing the entire evaluation process.

CHAPTER VI

A PROPOSED RESOURCE ALLOCATION MODEL

System Components

Discussion to this point has attempted to define the problem and ascertain the factors relevant to it. In addition, the methods currently used have been reviewed and found wanting. Now, to be constructive, a system designed by the author is proposed.

Any system for the management of Research and Development must somehow provide for project initiation, selection, evaluation, determination of resource allocation and periodic review. If the system is effective, it should result in a smooth flow of well-chosen projects, whose statuses are updated and reviewed periodically. The system should help insure that those projects that promise the best expected payoff to the organization are identified and selected for support.

The process for accomplishing the above objective is essentially the same for all projects. A diagram of the key steps is shown in Figure 9.

The first step is <u>conception of the idea</u> and <u>exploratory investigations</u>. Ideas for fruitful research can originate

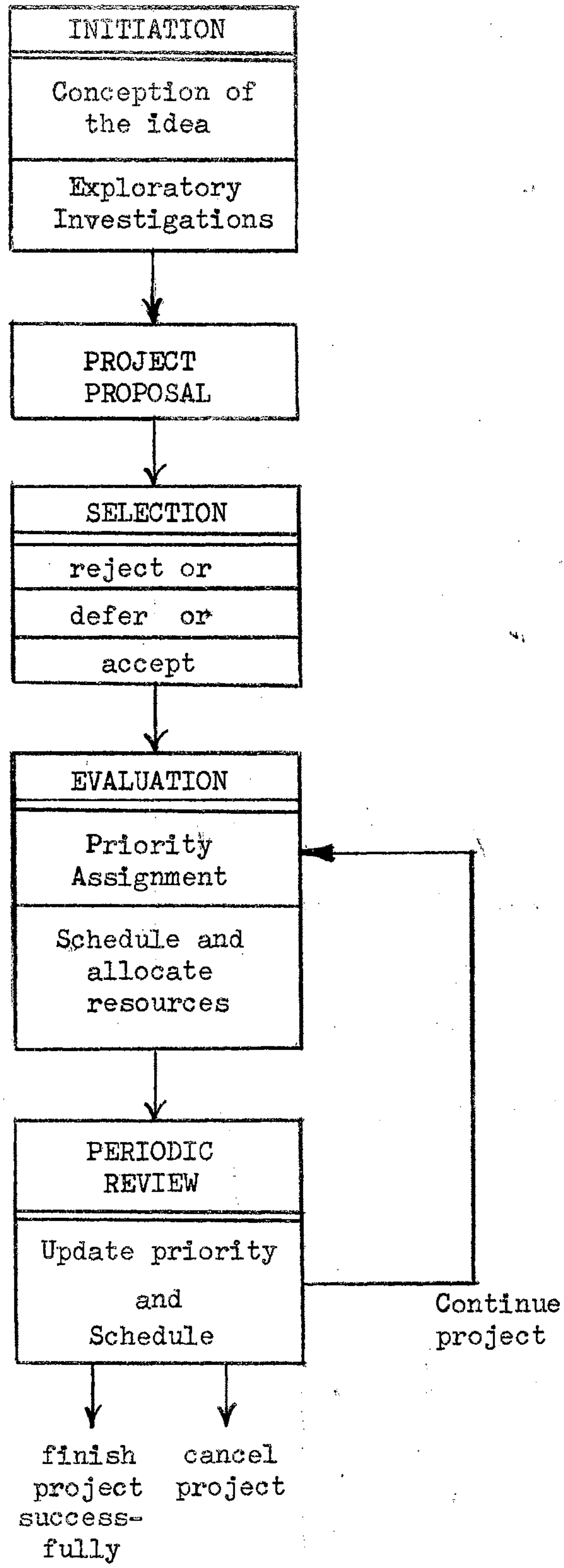

Figure 9. Research and Development Project Management System

from any source. Although some ideas do come from outside of the Research and Development organization -- sales, manufacturing or service personnel, etc. -- most do not. Experience in most organizations shows that, by far, the greatest proportion of ideas originate with the technical people of the Research and Development organization itself.

When an idea is conceived, some preliminary, exploratory investigation must be pursued to determine feasibility. This may take the form of a literature search, a few simple experiments, or a theoretical analysis. Since the cost of such exploratory investigations is usually quite small (consisting mostly of the time of the investigator), it is usually not desirable to impose management controls on this effort. Moreover, there simply is no means of applying controls to it, since this is the step or phase in which the information needed later for evaluation is generated.

Most organizations allow or set aside a certain percentage of each investigator's time to perform exploratory investigations of his own choosing. It is a rather common practice in the best Research and Development organizations to set aside approximately ten per cent of a man's time for this purpose. This is probably the most valuable allocation of effort that an organization can make, since it is from this effort that ideas originate and germinate.

The second step in the process is the initiation and <u>development of a project proposal</u>. At some point after the origination of an idea and an exploratory investigation, it becomes necessary to devote more resources of time, material, and equipment in order to pursue the project further. At this point, it is necessary to get something in writing. The project proposal comprises a definition of the project and pertinent information such as objectives, resource requirements, schedules, state-of-the-art, markets, etc.

The third step is project selection. At this point, management's role begins. This step results in accepting, deferring or rejecting a proposal. The proposed outcome is compared to the organization's objectives and long-range goals. Accepting the proposal means that the organization has a current interest in this kind of project; deferral means the organization may have a future interest; rejection means that it is of no interest. This selection will be handled differently by various organizations, but typically it is done by a basic research committee, new product committee, research council, or some high management level.

It is important here to sound a word of warning. If the project is deferred, rejected, or, at a later stage cancelled, curtailed, or put into a lower priority, it is extremely important to explain the reasons to the originating investigator. Generally, a great deal of preliminary work will have proceeded the submission of the proposal. The worker thinks it is practical and feasible or he would not have submitted it. Disapproval or cancellation <u>without</u>

providing the reasons therefore will convince the investigator that management is very short-sighted at best. Many managers indicate by their actions that they do not have the time, or do not need to explain their decisions to subordinates. In dealing with professional people (as the Research and Development manager must), nothing is of more importance. Time spent in explaining such adverse actions pays great dividends in high morale, better future proposals, and greater enthusiasm on the part of the investigators.

The fourth step is evaluation. This is the point at which management must determine the current relative importance (priority) of a selected project, plus a proposed schedule for its accomplishment, and decide what level of resources to devote to it. New projects having adequate priority are intended to be put into work shortly after priority assignment, with some perhaps replacing lower priority in-work projects. The remaining new projects are deferred.

The fifth step, once a project is in work, is control, which is exercised by periodic review. As a result of re-evaluation, management may decide to add manpower or capital to the project, extend the schedule, concept, etc.

Throughout this process, each project must be considered in relation to all others. For, with a given capacity of manpower and facilities, it is evident that adding a new project must be coupled with removing some other project (owing to successful completion or cancellation) or a lowering of emphasis, with a resulting extension of schedules. Each of these steps will now be discussed in greater detail.

The Project Proposal

The format used should be tailored to the needs of the individual organization. The proposal should be as short and as complete as possible. A suggested, or typical, format for such a written proposal is shown in Figure 10. The form on which a project proposal is presented should show the subject, objectives, technical information, utilization information, budget and schedule of of the proposed project.

The subject should be descriptive of the content of the proposed project. The objectives describe not only the ultimate results sought, but also any interim results that the project seeks to achieve. The technical information is a description of the required technology and materials, what is needed to obtain them, and what is available. Another way of saying this is that it is a description of the current state-of-the-art. The utilization information outlines why the project is needed, when it is needed, and what the payoff is. The budget is an estimate of the capital, facilities, and amount and kind of manpower needed, and when these are needed. The schedule is a statement of the expected times of completion of the several interim and final objectives. The schedule is tied in closely with the budget.

PROJECT PROPOSAL FORMAT

Project Title: ______________________________

Submitted By: ________________ Date: ______

Proposal No.: ________________

OBJECTIVES

Primary Objective(s):
Interim and/or Spin-off Objective(s):

UTILIZATION INFORMATION

Need or Justification:
When Required:

TECHNICAL INFORMATION

Current State-of-the-Art:
Proposed Approach:
Special Unavailable Resources Required: (personnel skills, materials, equipment)

BUDGET SCHEDULE INFORMATION

Estimated Cost of Unavailable Resources:
Total Manpower (in man-months) Required by Labor Skill Category: Professional ____________
Technician ____________
Number of Persons Required per Month:
Maximum ____________
Minimum ____________
Number of Months to Completion:
Maximum ____________
Minimum ____________

Attach Bar Graph Showing Proposed Schedule by Milestones Assuming Maximum Manpower is Available:

Figure 10. Typical Research and Development Project Proposal Format

This kind of information is needed on every proposal. In some cases, information will be required on patent and other legal problems, government regulations, etc. Under some circumstances, crash schedules and/or extended schedules also should be prepared for examination and evaluation.

The information shown is essentially the entire basis for selection decisions. For many applied research projects, the data will be complete in almost all areas, particularly in the areas of technical and utilization information. Conversely, for many basic research projects, the data will be incomplete almost everywhere, with only the technical information, an estimate of utilization potential for the area considered, and a short-term budget available. In all cases it will probably be necessary to update the budgets and schedules prior to each review.

Selection of Projects

On the basis of the information in the project proposal, the appropriate management personnel can select the desired projects. A typical selection work sheet for a project, incorporating the factors considered most important and relevant to the organization's environment is shown in Figure 11. Three sets of factors are shown: technical, utilization and timing. Subjectively, each of these is important in looking ahead to the ultimate payoff of a research project. At this state, the evaluation is still subjective, notations for a

PROJECT PROPOSAL WORKSHEET

Project Title: Development of Super-Insulation

Evaluator: John S. Doe

Proposal No.: M-2 Date Evaluated: 2-4-65

Technical Factors:	Favorable	No Opinion	Not Favorable
Long term objectives	X		
Interim objectives	X		
Technical approach	X		
Availability of technology within organization			X
Availability of technology outside organization	X		
Availability of scientific skills	X		
Adequacy of facilities			X
Adequacy of support manpower			X
Tie-in with existing projects			X
Compared to alternative approaches	X		
Estimated chance of technical success	X		
TOTALS	7	0	4
Utilization Factors:			
Requirement for results	X		
Funding for implementation of results			X
Reduction of costs of operation		X	
Increase in reliability		X	
Value to cost ratio	X		
Effect on operations	X		
TOTALS	3	2	1
Timing Factors:			
Completion time relative to need	X		
Reduction in time of operations schedules	X		
Timing compared to alternatives	X		
TOTALS	3	0	0

Figure 11. Project Proposal Worksheet

specific project being simply that a factor is favorable, unfavorable, or that the evaluator has no meaningful opinion.

Each project should be graded by more than one person, but care must be exercised that only qualified persons are used. The scorers must be persons who understand not only the technical aspects of the project, but who also have a broad, intimate understanding of the organization, its objectives, and its environment.

The scoring form shown must be tailored to the individual organization's area of endeavor and particular technological problems. It should be developed through careful analysis and modified with experience.

As <u>one</u> guide to provide assistance in the decision of whether to accept or reject a proposal, the scores can be weighted. A numerical weighting factor might be assigned for each of the three sets of factors. For instance, if it was decided that each should contribute equally and independently to the subjective judgment of the project, a score of ten might be assigned to each set of factors. The scores for the three factors would be added since they each contribute equally and independently. The unfavorable score should be subtracted from the favorable score. The best possible score would comprise checks of "favorable" for all items. In this case, the best possible score would yield +30. The worst possible score would comprise checks of "unfavorable" for all items and would yield a weighted score of -30. The "No Opinion" checks should not affect the scores of either.

The calculation of the weighted score for the example given in Figure 11 (page 69) would be:

	Favorable	Unfavorable
Technical factors	7/11 x 10 = 6.3	4/11 x 10 = 3.6
Utilization factors	*3/4 x 10 = 7.5	1/4 x 10 = 2.5
Timing factors	3/3 x 10 = 10	0/3 x 10 = 0
Sub-Totals	23.8	6.1

Project value = Favorable - Unfavorable

= 23.8 - 6.1 = 17.7.

Thus, after grading each new proposed project, one has a ranking of projects with scores ranging between +30 and -30. It is emphasized that this scoring and ranking are intended to aid the decision-maker, not to make the decision for him. Therefore, it would be unwise to set arbitrary limits for acceptance or rejection.

The actual selection of projects must depend upon management's evaluation of (a) does the proposed result match or meet the long-range goals and plans of the organization, (b) is the proposed result the type of product, or information, that the organization wishes to market or use, and (c) does the possible ultimate payoff justify embarking on the

---

*Note that the divisor is 4 instead of 6 because of the two items checked "No Opinion."

project. There are, of course, other considerations, but they are secondary to the above.

When management has completed the selection phase, each project should have been placed in one of four categories:

1. Mandatory: These projects are essential for the well-being of the organization or to the completion of some other high priority project.
2. Acceptable: These are projects which management is interested in pursuing. They will be worked into the work schedule at the earliest possible time in accordance with their priorities.
3. Deferred: These projects look interesting and feasible, but are not of immediate interest. Reasons for placing projects in this category might be (a) high capital investment required, (b) would require penetrating new markets, (c) would involve getting into overcrowded markets, and (d) unavailability of suitable personnel.
4. Rejected: These projects are of no interest to management now or later. Either they do not fit in with the long-range goals and plans of the organization or the potential payoff does not justify the risk and expense of further pursuit.

Evaluation of Projects

Once a project has been selected, it is then further evaluated and a priority assigned. In this phase, the project must move into competition with existing projects which are already being conducted. It is necessary to evaluate current projects, as well as the new ones which have been accepted.

For all projects, this evaluation consists of:

1. Determining the status of the project.
2. Deciding whether and how to continue.
3. Assigning a suitable priority.

The status of a new project is based on the data contained in the selection work sheet. The status of a project in work is based on the original information and the amount accomplished and learned since the last review.

Figure 12 shows a form which may prove useful for recording both original and re-evaluated information to aid in assigning suitable priorities for applied projects already underway.

As discussed in Chapter IV, it is mandatory that management insure a balance between projects which are towards the basic end of the research spectrum and those towards the development end. If basic research projects are forced to compete with development projects for priorities, the priorities will go to the development project. This would work to the long-range detriment of the organization. It is,

PROJECT STATUS REPORT

Project Title: ______________________________

Project No.: ________ Chief Investigator: ________________

Start Date*: ________ Est. Completion*: ______

Initial Priority: ______ Budget: 19__ $ ______
19__ $ ______
To Complete $ ______

(*As covered by project proposal ____________)

Review Date: ________ Budget Man-Hours ______
$ ______
Actual Man-Hours ______
$ ______

Accomplished to Date:

Problems Encountered:

Action Taken:

Current Priority: ______________________________

Figure 12. Project Status Report

therefore, proposed that the basic and applied type programs be evaluated separately and not forced to compete with each other.

It is recognized by the author that it is difficult, if not impossible, to cleanly and unequivocally separate them. The ones at either end of the spectrum are easy to identify. But, as one approaches the center from either direction, it becomes increasingly difficult. It is proposed that management subjectively (even arbitrarily) divide the projects into two groups for separate evaluation. As a guide, one would use for one group the definitions of basic research and supporting research as given in Chapter II. For the other group, the definitions of applied research and development would be used.

It would be nice if somehow these projects could be programmed into a magical computer which would automatically assign the correct numerical priorities. Unfortunately, such a magical computer program does not yet exist and educated, experienced, subjective judgment must still be relied upon.

The procedure proposed is one quite familiar to all experienced operations research personnel for weighting objectives. Both the underlying logic and the procedure are simple. The procedure consists fundamentally of a systematic check on relative judgments by a process of successive comparisons.

The basic assumptions of the system are:

1. A person's subjective judgment of the relative value between and among projects is more accurate than his judgment of an absolute value of any one project.
2. A person's relative judgment among a few projects is more accurate than his evaluation of a large number.

The evaluator is asked to make a series of judgments among a relatively few projects. Each judgment contributes information concerning the relative importance of the projected outcomes to the evaluator, and each iteration should improve the decision.

Assigning Priorities

The proposed method of assigning priorities will now be described. Although the procedure may, at first, appear cumbersome, it is really rather simple in practice.

The procedure consists of the following steps:

1. Rank the entire set of projects being evaluated in terms of preference or perceived value of the projected outcomes without assigning quantitative values.
2. Select at random one project from the set. Let P_S represent the hoped-for outcome of this project.

3. Subdivide the remaining set of projects by random assignment into groups of no more than five, and preferably into groups of approximately equal size. Each project (other than P_S) should be included in one and only one group.
4. Add P_S to each group and assign to it a priority value of 1.00, i.e., priority of $P_S = 1.00$.
5. For each group, tentatively assign to each project a value which, initially, seems to reflect the relative value of their proposed outcomes to that of P_S. For example, if the evaluator thought that the value of a successful outcome for project P_A would be twice that of P_S, he would assign it a tentative priority of 2.00. Thus, a group of projects, P_A, P_B, P_C, and P_S, might have tentatively assigned to them priorities of 2.00, 1.25, 0.80, and 1.00, respectively.
6. Make subjective comparisons of combinations such as:

$$P_A \text{ versus } P_C \text{ and } P_S.$$

Thus, if the evaluator had the choice of having a successful outcome of P_A or the combination of P_C and P_S, which would he choose? Suppose he says he would rather have P_C and P_S. Then, the values of P_A and P_C must be adjusted so that $P_A < P_C + P_S$. In making adjustments, the value of P_S must not be changed. Continue these comparisons of combinations until the values for each project in the group are consistent for all evaluations.

7. Compare the rankings obtained for the entire set of projects as obtained by Steps 2 through 6 when the groups are recombined with that obtained in Step 1. If the rank orders differ, reconsider the ranking from Step 1 and, if necessary, proceed again from Steps 2 through 6 of this procedure.
8. Once consistent results are obtained, normalize the priorities by dividing the priority assigned to each project by the sum of the priorities assigned to all the projects.

The result is a relative priority assigned to each project in the set and the sum of the priorities equal to one. The procedure just described may be clarified by a simple example. Suppose there are ten projects:

1. Suppose these are ranked as follows: P_1, P_2, ..., P_{10}.
2. Suppose P_5 is selected at random as the standard.
3. The remaining projects are assigned at random to three groups

I	II	III
P_3	P_2	P_6
P_{10}	P_9	P_1
P_7	P_4	P_8

4. P_5 is added to each group and assigned a priority equal to 1.00.
5. Suppose relative priorities are assigned to each project and combination comparisons are made until consistent evaluations are obtained with the following results:

I	II	III
$P_3 = 3.00$	$P_2 = 3.25$	$P_6 = 1.35$
$P_{10} = 0.30$	$P_9 = 0.50$	$P_1 = 3.60$
$P_7 = 0.90$	$P_4 = 2.50$	$P_8 = 0.80$
$P_5 = 1.00$	$P_5 = 1.00$	$P_5 = 1.00$

6. It will be noted that in the computed rankings of Step 5, P_5 and P_6 are reversed from those assigned in Step 1. The evaluator would then reconsider his

initial rankings. If he decided that P_5 was indeed more important than P_6, it would be necessary to reiterate Steps 2 through 6 again and make the necessary adjustments to the calculated priorities. If, on the other hand, after reconsideration, he decided that P_6 was after all more needed than P_5, he would let the priorities be as adjusted.

7. Finally, the priorities would be normalized by dividing each by 17.20 (the sum of all the priorities) to obtain:

$P_1 = .209$	$P_6 = .078$
$P_2 = .189$	$P_7 = .052$
$P_3 = .175$	$P_8 = .047$
$P_4 = .145$	$P_9 = .029$
$P_5 = .058$	$P_{10} = .017$.

As stated at the beginning of this section, although this procedure at first appears to be rather cumbersome and complex, it is really quite simple in practice. It reduces the rather complex problem of relative value judgments to proportions which can be handled more easily. But, perhaps even more important, the multiple-combination comparisons force the evaluator to consider his decisions from different directions, and against multiple criteria.

How this procedure would be implemented would vary from organization-to-organization. Priorities could be assigned by a single evaluator of high managerial rank or by a committee or group of evaluators. The important thing is, as in the case of the selection phase, that the evaluator or evaluators be qualified persons who not only understand the technical implications of the projects, but who also have a broad, intimate understanding of the organization, its objectives, its needs and its environment.

Allocation of Resources

Having developed a methodology for selecting and evaluating projects and assigning a priority to them, one problem still remains. Somehow, the available technical manpower must be allocated among and within the projects.

The problem can be expressed mathematically in the following manner. The project number will be denoted by i, $(i = 1, 2, \ldots, m)$, and it is known that a total of R_i man-hours must be applied to project i during a given sequence of months to successfully complete the project. For a given month j, $(j = 1, 2, 3, \ldots, n)$, the project planning group knows how many total man-hours, A_j, are available for use by the projects active in month j. At least d_{ij} man-hours and no more than e_{ij} man-hours must be expended on project i in month j. If X_{ij} is defined as the total number of man-hours assigned to project i in month j, then the constraints on the problem can be expressed as follows:

1. $\sum_{j=1}^{n} X_{ij} = R_i$

2. $\sum_{i=1}^{m} X_{ij} \leq A_j$

3. $o \leq d_{ij} \leq X_{ij} \leq e_{ij}$.

The computational procedure which will be presented here is an adaptation of the non-iterative system discussed by Gass (27).

In the development of a suitable computational procedure for the allocation problem defined, the number and size of the problems to be computed on some regular basis and the time which would be required had to be considered. If the scheme proposed by Dantzig (28) were utilized, it appeared that to be practical, computer assistance would be required if the number of projects averaged 12 or more. A computational procedure which would be simple, fast to apply, feasible for any size Research and Development organization, and could be performed by hand computation, seemed desirable.

Efforts were, therefore, directed towards a noniterative procedure which would be based on a valid interpretation of the priority ratings. A simple scheme which would yield a feasible first solution that would also be acceptable as a final solution was sought. It is believed that the procedure described below meets the above requirements. It has been tested on a number of actual and devised problems and has yielded acceptable answers (with certain restrictions which will be discussed later) in all cases.

To describe the procedure, the data in Table I will be employed. Manpower requirements are given in equivalent man-months (MM) for ease of computation. The problem could also be worked in terms of man-hours.

TABLE I

PROBLEM PARAMETERS

P_i Project	C_i Priority	R_i Total MM Required	e_{ij} Max MM Per Month	d_{ij} Min MM Per Month
1	0.21	16	2	1
2	0.16	12	4	1
3	0.17	8	2	1
4	0.10	24	5	2
5	0.09	18	3	1

Man-months (MM) available per month = 10

Assuming that a solution for an eight-month time period is desired, the computational framework for this problem would be as shown in Figure 13. The upper and lower bounds are shown in the upper right-hand corner of each cell (d_{ij}/e_{ij}). The projects are ordered by priority; i.e., Project 1 has a higher priority than Project 2, etc.

P_i	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	R_i
1	1/2	1/2	1/2	1/2	1/2	1/2	1/2	1/2	16
2	1/4	1/4	1/4	1/4	1/4	1/4	1/4	1/4	12
3	1/2	1/2	1/2	1/2	1/2	1/2	1/2	1/2	8
4	2/5	2/5	2/5	2/5	2/5	2/5	2/5	2/5	24
5	1/3	1/3	1/3	1/3	1/3	1/3	1/3	1/3	18
A_j	10	10	10	10	10	10	10	10	

Figure 13. Computational Matrix

The procedure utilized for the initial allocation (Figure 14) is an adaptation of the northwest corner rule. An initial allocation (X_{ij}) of the available man-hours is determined by starting the allocation with Row 1, Column 1 and continuing the allocations along the first row until R_1 units have been allocated. The allocation to each month should be the maximum number allowed by the upper limit for that project. Some adjustment may be necessary in the last two months so as to avoid violating the lower bound requirement in the last month.

P_i	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	R_i
1	1/2 2	1/2 2	1/2 2	1/2 2	1/2 2	1/2 2	1/2 2	1/2 2	16
2	1/4 4	1/4 4	1/4 4	1/4	1/4	1/4	1/4	1/4	12
3	1/2 2	1/2 2	1/2 2	1/2 2	1/2	1/2	1/2	1/2	8
4	2/5 2	2/5 2	2/5 2	2/5 5	2/5 5	2/5 5	2/5 3	2/5	24
5	1/3	1/3	1/3	1/3 1	1/3 3	1/3 3	1/3 3	1/3 3	18
A_j	10	10	10	10	10	10	10	10	
Allocated	10	10	10	10	10	10	8	5	

Figure 14. Initial Allocation

The initial allocations for the remaining projects are then made in the same manner, one at a time, in priority order, always being sure that the availability restriction A_j for each month is not exceeded. The first month's allocation to any project should be the maximum amount allowed by the upper bound restrictions for that project, but in no case, should it be less than the lower bound restriction. If an allocation at least equal to the lower bound restriction is not possible without violating the A_j restriction, then the initial allocation for that project should be deferred until the succeeding month. The allocations proceed project-by-project until all of the available resources which can be assigned, without violating one of the restrictions, have been utilized. The initial allocations for the example problem are shown in Figure 14 (on the preceding page).

In general, it would be expected that for some months all of the available resources A_j will not have been allocated because of the upper and lower bound restrictions. It is then necessary to adjust the allocations in a manner that will perhaps stretch out some of the projects, but which allows one to assign as much of the available resources as possible. This scheme will be illustrated using the same (5 x 8) example.

Starting with Project 1, and proceeding in priority order, one looks for the first R_i which has not been satisfied completely by the allocations. In the example (Figure 15), this is R_5; i.e., for Project 5, 18 man-months are required, but only 13 have been allocated. One then searches the allocations for each month for that project in inverse order (i.e., starting with $j = n$) and find the first month in which the allocation is not equal to the maximum constraint. In the example, this would be $j = 4$ where one could allocate as much as three man-months, but have actually allocated only one.

P_i	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	R_i	Allo-cated
1	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	16	16
2	$\boxed{1/4}$ 4	$\boxed{1/4}$ 4	$\boxed{1/4}$ 4	$\boxed{1/4}$	$\boxed{1/4}$	$\boxed{1/4}$	$\boxed{1/4}$	$\boxed{1/4}$	12	12
3	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$ 2	$\boxed{1/2}$	$\boxed{1/2}$	$\boxed{1/2}$	$\boxed{1/2}$	8	8
4	$\boxed{2/5}$ 2	$\boxed{2/5}$ 2	$\boxed{2/5}$ 2	$\boxed{2/5}$ $5-\theta$	$\boxed{2/5}$ 5	$\boxed{2/5}$ 5	$\boxed{2/5}$ $3+\theta$	$\boxed{2/5}$	24	24
5	$\boxed{1/3}$	$\boxed{1/3}$	$\boxed{1/3}$	$\boxed{1/3}$ $1+\theta$	$\boxed{1/3}$ 3	$\boxed{1/3}$ 3	$\boxed{1/3}$ 3	$\boxed{1/3}$ 3	18	13 ←
A_j	10	10	10	10	10	10	10	10		
Allo-cated	10	10	10	10 ↑	10	10	$8+\theta$	5		

$\therefore \theta_{max} = 2$

Figure 15. Adjustment of Allocations

If one adds a positive allocation θ to X_{54}, then one must also subtract it from X_{44}, and add it to X_{47} to avoid violating the restrictions A_4 and R_4. Examination of the upper and lower constraints of the three affected cells shows that the maximum allowable value for θ is 2($1 \leq X_{54} + \theta \leq 3$ and $X_{54} = 1$; therefore, $\theta_{max} = 2$; also $2 \leq X_{47} + \theta \leq 5$ and $X_{47} = 3$; therefore, $\theta_{max} = 2$). Therefore, the first adjusted tableau is shown in Figure 16. This adjustment has allowed the allocation of two more man-months in the program.*

*One would then proceed to the preceding month $j = 3$ and see if some beneficial adjustment was possible.

Further examination of the matrix indicates no further beneficial adjustments which would not violate any of the restrictions.

P_i	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	R_i	Allocated
1	2	2	2	2	2	2	2	2	16	16
2	4	4	4						12	12
3	2	2	2	2					8	8
4	2	2	2	3	5	5	5		24	24
5				3	3	3	3	3	18	15
A_j	10	10	10	10	10	10	10	10		
Allocated	10	10	10	10	10	10	10	5		

Figure 16. First Adjusted Tableau

CHAPTER VII

EVALUATION OF PROPOSED METHOD

Analytical Evaluation

The proposed system can be evaluated for usefulness and applicability, first, on the basis of logic and analysis and second, on the basis of first-hand experience in application in an actual research setting.

In analyzing the method proposed, it becomes clear immediately that a great deal of subjective judgment is still required. This, in the opinion of the writer and others closely associated with Research and Development management, cannot be avoided. So long as one is dealing with a phenomena with so many unknowns and intangibles as has Research and Development, the most reliable criteria for selection and evaluation must remain the educated judgment of experienced, competent, research managers.

The method proposed is neither strictly quantitative nor qualitative in nature. It can best be described as a method for organizing and partially quantifying the qualitative judgments of persons most able to make those judgments. The quantification aspects of the method help to organize and insure logical evaluation of the qualitative judgments.

The method delegates the making of estimates and judgments to those persons in the organization most qualified, and in the best position, to make the particular judgments. The individual researcher estimates the manpower required and the maximum and minimum allocation of resources which would be required. He determines also what the hoped-for outcome or result will be as well as the current state-of-the-art. Staff and management personnel who are in a position to see the over-all picture, and yet have the requisite technical competence, determine how well the proposed outcome matches the goals and objectives of the organization. They serve also as a check on the researcher's evaluation of the current state-of-the-art and chances of a successful conclusion of the project. Top management, based upon their knowledge of the needs and goals of the parent organization, assign the priorities and, thus, determine the allocation of resources. Thus, each level of the organization performs that function for which it is best suited, so far as the selection, evaluation, and allocation of resources for the Research and Development program are concerned.

Another strong advantage of the proposed method over current methods is its versatility and flexibility. For example, in most current quantitative methods it is necessary to express proposed outcomes in terms of some common denominator (usually dollars). This restricts their usage to comparisons of similar-type projects only. At the present time, for example, it is almost impossible to compare the value of a new product development versus a project which would result in greater reliability of some component or product. Most current methods do not provide for the evaluation of apples, oranges, and lemons versus each other and among themselves. Yet, this is what most research managers are constantly called upon to do. He has some projects which will result in new products, some that will reduce weight or cost, others that would result in increased reliability, and still others that would result in additional prestige for the organization. The proposed method allows him to do this (although admittedly on a subjective basis).

An additional strong point of the proposed method is that it gives appropriate recognition to the importance of time in the Research and Development process. It does this in two ways. First, the Research and Development manager will tend to give a high priority to those projects for which the answers are needed soonest and which are the most pressing. Second, the method of allocating resources assigns the maximum amount possible to the high-priority projects to get them completed at the earliest feasible date. Any stretch-out of projects is done in the lower priority projects where time probably is not so critical.

A further feature of the proposed method, generally overlooked in currently used methods, is a maximum and a minimum allocation of resources per time period which is practical for each project. Too few resources assigned to a program will cause it to be stretched out over such a long time period that the answer will no longer be of much value when it is obtained. In addition, a project that is undermanned will oftentimes never really get rolling. Almost any project requires a certain amount of concentration of effort to get it to "gel" or really "get it off the ground." It is usually better to not begin a project until adequate support can be given to it; otherwise those resources allocated are largely wasted.

Equally wasteful of resources is the allocation of too many resources. The law of diminishing returns often sets in rapidly. This is so primarily because most projects require a high degree of creativity, serendipity, and insight for successful completion. Additional resources can speed up the data-gathering and testing phases of a project. But, even here, the assimilation and interpretation of the results on a continuous basis are the pacing activities which determine the effectiveness of the effort being spent -- and this is usually the result of the inputs of one or two people. The allocation of too many resources to a project can result in much unneeded and wasted effort. As a matter of fact, it can actually result, in some cases, in delaying successful conclusion of a project! This can occur from increased administrative burden on the key people as well as wasted efforts from being buried under test results and data requiring interpretation and assimilation.

Test Application

To further evaluate the proposed method, it was tested in four Research and Development organizations. These groups were all government organizations engaged in both in-house and contracted research. Each varied in size, but each was carrying on between 15 and 25 active projects at the time of the test. In addition, each was supervising from eight to 20 outside contracts with universities and industry. Following is a discussion of the results and experiences gained from these tests.

The first problem encountered was in the use of the project selection score sheet (Figure 11, page 69). At first, there was considerable divergence in the results obtained from different scores. Further investigation showed it to be a problem of semantics and definitions. When each factor and adjective was clearly defined, very close agreement between competent scorers was found.

The second difficulty occurred in the assignment of priorities by the organization managers. No difficulty was experienced in the initial ranking of projects; however, when it was time to assign a quantitative weighting factor to these judgments, the problems began. Discussions indicated that the difficulties arose primarily from the unfamiliarity of the manager with the procedure. This was the first time that they had tried to quantitatively weigh the value of one project versus another. It is believed that this difficulty would disappear with experience, since such judgments have always (of necessity) been made, even though subconsciously and qualitatively up to this time.

The final difficulty arose in the allocation of resources phase. Once the priorities were set, and the maximum and minimum restraints specified, there was no particular problem in allocating the manpower by use of the proposed method. The problems emerged in translating these allocations into practical assignments to the available personnel. This was attributed primarily to the differences in skills and capabilities of the available manpower.

The method, as proposed, assumes almost complete flexibility in assigning available resources to the various projects. It completely ignores the wide variations in skill, creativity, dedication, and interests which exist between different people with supposedly equivalent backgrounds. These variations may limit or modify the way in which the individual can best be utilized.

In projects requiring relatively large expenditures of manpower (approximately 10 man-months/month or greater) individual personalities, other than those of the project leaders, are probably not quite so important. Manpower allocations in these larger projects can usually be made disregarding personalities, because the importance of individual contributors tends to be masked by the group otuputs. Another way of saying this is that the individual skills, knowledge, etc., needed will be found more easily in the larger group.

In the smaller projects, however, the importance of the contribution of each participant tends to be more magnified. Thus, in an organization whose Research and Development program is made up primarily of several relatively small projects, some adjustments to the allocations will, undoubtedly, have to be made. But, this should not present any serious problems. In the four trials run, the required modifications to the resource allocations were minor in nature and easily made.

CHAPTER VIII

SUMMARY

Because of its increasing magnitude, and its importance to everyone, research and development is deserving of serious study. A great deal of effort has gone into studying and defining the process of conducting research and development, but very little effort into the process of managing it.

Most authors who have addressed themselves to the problem of Research and Development management have dwelled mostly on the administrative problems of budget control; selection and training of personnel; design and equipping the facilities, etc. Most have evaded the very basic implementing decision problems such as the selection of projects and allocation of resources to them.

The author does not propose that the system suggested is the full and final answer. A considerable amount of work remains to be done. The system suggested does not make the decisions. Its only purpose is to provide a logical framework and organization of the pertinent information to aid the executive in making his decisions.

It is hoped that others will consider this area of management as important and as intriguing as does the author.

Like most research, this study has left unanswered more questions than it has satisfied. Among the many problems left unresolved are the development of methods to evaluate:

1. The worth of individual projects to the organization after they have been successfully completed.
2. The effectiveness of the over-all Research and Development effort of the organization.
3. How large a Research and Development effort should be carried on by the parent organization.
4. The effectiveness and worth of individual researchers.
5. The contribution of environmental factors to creativity.
6. How to determine the best combinations of available personnel to assign to each project taking into consideration the skills, desires and personalities of the individuals.

Chester I. Barnard (29) in his very perceptive paper, "Mind in Everyday Affairs" has said that three kinds of considerations govern decision making; material that consists of precise information, material of hybrid character, and material of a speculative nature. In Research and Development management, the executive is usually faced with the latter two.

It has been impossible, therefore, in this study, to come to a complete and irrefutable conclusion. It is the author's desire that the evaluation of these efforts be from the viewpoint of ultimate utility. Research that is theoretical and fundamental may be of high scientific and intellectual quality, but of little usefulness in contributing to the solution of practical problems. There would be little excuse for an investigation such as this if it does not have ultimate relevance to the solution of operational problems.

If, therefore, the author has succeeded in shedding some light on the true character of the problems and suggested a method of approach to solving at least a few of them, this investigation has been beneficial. If the author has succeeded in creating enough interest to persuade others to devote study to this complex, intriguing area of management, then it has been a success.

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