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AN INTEGRATED APPROACH TO THE SELECTION PROCESS OF INDEPENDENT RESEARCH AND DEVELOPMENT PROJECTS

by

Carol L. Larson

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Thesis Advisor: Dan C. Boger

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An Integrated Approach to the Selection Process of Independent Research and Development Projects

by

Carol L. Larson
Lieutenant, United States Navy
M.S., Southern Illinois University, 1980

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Author: Carol L. Larson
Carol L. Larson

Approved by: Dan C. Boger
Dan C. Boger, Thesis Advisor

Nancy Roberts
Nancy Roberts, Second Reader

David R. Whipple
David R. Whipple, Chairman,
Department of Administrative Sciences

ABSTRACT

An active independent research and development (IR&D) program is a contributing factor to the U.S. military's reputation for technologically superior weapon systems and combat support equipment. This thesis examines the current selection process of IR&D projects at Naval Research, Development, Test & Evaluation (RDT&E) Centers and develops a recommendation to tailor the selection process to the characteristics of the project under consideration. The U.S. Navy divides its IR&D projects into two categories, independent research (IR) and independent exploratory development (IED). This thesis recommends that a scoring method be used to select IR projects and an economic method be used to select IED projects. The thesis concludes by discussing future issues that will impact the IR&D programs.

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I. INTRODUCTION

The United States military has established a reputation for being on the cutting edge of technology regarding its weapon systems and combat support equipment. One contributing factor to this reputation is an active independent research and development (IR&D) program within private industry and government laboratories.

As the pendulum of military funding swings back (this does not include the Persian Gulf crisis, which is an emergency funding situation) to fewer funds being available at all levels of military operations, the need to optimize funding of IR&D becomes more acute if the U.S. is to continue to upgrade its military technological base. Current funding plans show that Congress and the President are committed to maintaining research and development funding at the present level. [Ref. 1:p. 198] One small portion of this budget is IR&D, which for this paper is defined as research to advance the state of the art, to solve problems of interest to the Department of Defense (DoD), and to extend the capabilities of contractors and government laboratories to solve DoD problems [Ref. 2:p. 126]. The process of establishing an optimal funding pattern in this area is not an easy task. The selection of IR&D projects may share some basic characteristics with other selection and allocation decisions, but it is sufficiently unique to have generated its own body of research literature.

To fully understand the difficulties involved in selecting IR&D projects it is necessary to understand those traits which tend to contribute to uncertainty in the selection process. First, final decision making tends to be relatively centralized, while essential information is spread throughout the organization leading to decision making without complete information. This situatic is aggravated by the typical multi-layered structure for gathering and processing information as numerous individuals with sign-off powers delete, change and add information prior to submission to final decision makers [Ref. 3:p. 1257].

Second, the benefits of IR&D tend to have multiple dimensions, some of which are not easy to translate into a cost figure. For example, it is difficult to establish a dollar value on the scientific reputation of an organization or the value of such a reputation for attracting top quality scientists and engineers. Another benefit that is difficult to define in terms of dollars is the importance of developing areas of expertise in selected areas, even if applications are several years in the future. [Ref. 4:p. 8]

Third, the reliability of cost estimation models in this field vary tremendously. Reliable cost estimation models tend to be in fields where the research involves improvements on known technology or historical data exists for similar systems

[Ref. 5: p.13]. The reliability of cost estimation figures deteriorate rapidly as the research shifts into new technological areas.

Finally, it is difficult to establish a quantitative relationship between the IR&D project and its potential for success because the term "potential" indicates a non-quantitative measurement. The term "success" also lends itself to ambiguity. Industry usually defines success as profitability, while government has several definitions: operability, reputation or national prestige, technical merit, etc.

There are other minor characteristics that contribute to the high degree of uncertainty in the selection process of IR&D projects but it is clear from the ones discussed that optimizing project selection is difficult under these conditions.

There exist several different methods or models in the literature to aid decision makers in the selection of IR&D projects. These models can be divided into four general categories for evaluation of IR&D projects: economic methods, decision theory methods, constrained optimization models and scoring. Each of these categories will be reviewed in depth in Chapter II. At this point a brief description will suffice.

- Economic Methods: a group of models utilizing cost effectiveness methods to determine economic feasibility.

This method is most commonly used for product-oriented R&D by using standard capital budgeting techniques.

- Decision Theory Methods: models using simulation analysis in order to generate a range of outcomes to reflect a probability density function of expected value.
- Constrained Optimization Models: models using mathematical programing to optimize an objective function(s) subject to specified resource constraints.
- Scoring: methods requiring the identification and assignment of merit to each project with respect to *a priori* criteria. [Ref. 6:p. 21]

Each of these methods contribute to the knowledge of how to optimize funding patterns in the area of selection of R&D projects and its own set of advantages and disadvantages. A crucial decision facing government organizations is to determine which method meets the needs of their organization and how to implement that method.

The Space and Naval Warfare Systems Command provides guidance to eight Naval Research, Development, Test and Evaluation (RDT&E) centers. Each of these centers manages an active IR&D program in ver diverse fields while operating under the same general management guidelines. Chapter III will discuss these guidelines and describe each center's mission, area of expertise and scope of their IR&D program. This chapter will illustrate the diversity and range of research being conducted at these eight centers and set the stage for the following chapter which recommends a more sophisticated method of selecting IR&D projects while allowing for the individuality of the differe centers.

The final chapter will examine three future issues that face the RDT&E centers that will impact on their IR&D programs. The issues to be discussed include: (1) the consolidation of management and support responsibilities, (2) the age of Navy laboratories, facilities, and equipment, and (3) the emerging trend that stresses solving short-term engineering problems rather than investigating future technologies.

II. LITERATURE REVIEW

A. INTRODUCTION

The number of models in this field presented over time are too numerous to cover individually. This chapter will focus on describing four general categories. The models covered will be simplified versions which serve as the backbones for many of the variations that have appeared in the literature. The purpose of the chapter is to provide the reader with a basic understanding of selection methods and their advantages and disadvantages.

B. SCORING METHODS

The process involved in setting up a scoring model in order to select projects is comprised of three steps. Those three steps in chronological order are the selection of the scoring criteria, the assignment of weights to those criteria, and the determination of scores [Ref. [illegible]:p. 34]. Subjective decisions are made at each step. The impact of subjective decision-making will be discussed further in the next section.

A weighted score will be calculated for each project using a relatively simple mathematical model that sums the weighted criteria. Once a score has been determined for each project, the projects are ranked according to scores. Generally, the project with the highest score is designated as the most preferred project. Occasionally a scoring model will be established to reflect the opposite where the lowest score is the preferred project.

As noted earlier, each step requires subjective decisions to be made. If the reliability of the subjective decisions can be improved then the model will be improved. In the area of subjective decision-making, researchers have shown that group consensus is superior to the single person approach. [Ref. 8:p. 125] The common practice is for a single decision-maker to determine criteria and weights and to use a group to assign scores. Several sources recommend that groups be used throughout the scoring process to dramatically improve the model [Refs. 6:p. 24; 7:p. 34; 9:p. 553]. The scoring model described here does recommend the use of groups.

1. Selecting Scoring Criteria and Weights

The two steps of selecting scoring criteria and weights are considered together because they are preparatory in nature and once selected are subject to reviews, but their selection is not repeated for every project. The selection of the panel deserves some discussion. Members should be recognized experts in their area, whether it's technical, managerial, or marketing. An attempt should be made to create a heterogeneous group, as studies have shown that heterogeneous groups produce a higher proportion of high acceptance solutions than homogeneous groups [Ref. 10:p. 326]. The size of the group should be limited to less than nine members because of the need for interaction, and the effectiveness of group techniques dwindles rapidly within large. groups [Ref. 7:p. 34].

Members within the panel develop a list of relevant factors affecting product success. These factors are usually identified as falling into two categories: technical factors and market factors (need for the product). Usually these lists are quite extensive with 30 to 40 different factors being identified. Ob· .ously using 30 to 40 different decision criteria would be extremely cumbersome and time consuming. Factors are then grouped together according to similarity to develop a manageable number of decision criteria; usually six to ten decision criteria are determined.

Once decision criteria are set, it becomes necessary to establish relative weights. The simplest method is to establish a scale with values ranging from 1 (least favorable) to 5 (most favorable), which is applied to each criterion. A scale with at least five divisions is required to produce significant statistical differences [Ref. 11:p. 21]. A more meaningful method would be to assign a percentile weight to each criterion (all weights sum to 100 percent) which would be multiplied by the selected scale value. This permits the organization to more heavily weigh key decision criteria. Well-established, clear organizational goals will be essential in order to properly weigh decision criteria [Ref. 12:p. 223].

2. Scoring the Projects

The normal process after the selection of scoring criteria and their relative weights is to provide scoring sheets to a number of specialists, who make independent, personal subjective decisions on each project. The projects are individually rated according to their merit on a 1 to 100 scale for each decision criterion. These scores are then tabulated for each project according to the following model:

$$SUB_s = \sum_{i=1}^{n} DC_{i,s}\ W_i$$

where:

SUB_s = Total weighted score for project "s",

$DC_{i,s}$ = Value of decision criterion "i" for project "s",

W_i = Weighing for decision criterion "i",

s = Number of projects,

n = Number of criteria. [Ref. 6:p. 24]

The rankings obtained from the comparison of the total weighted score for each project provides the organization with a collective subjective opinion of which projects are likely to succeed.

Another method for scoring the projects is to use scoring sheets in conjunction with the Delphi technique. Individual scoring sheets are summarized and a histogram of distribution of scores is plotted. This information is resubmitted to individual evaluators for review and they are requested to revise their scores if they substantially differ from the group results. Results are again summarized and scores should converge to within a narrow range. It may be necessary to repeat the process but a narrow range should be achieved with two or three tries. A final score will be assigned based on a narrow range of scores for each project. [Ref. 7:p. 37]

3. Advantages and Disadvantages

The main advantages of scoring methods are their relative simplicity, their support of a well-structured decision process that is easily understood, and their ability to incorporate diverse and non-monetary criteria. They have proven to be useful in the initial stages of project evaluation. They are particularly well-suited for screening decisions and preliminary analyses.

Their main deficiency is that subjective decision-making is the basis for all aspects of this method. In reality, scoring provides a summarization of opinions. The determination of the weight values is extremely difficult to obtain with any precision, and failure to properly weigh decision criteria can easily skew the model. Further constraints on the reliability of this model are the degree of knowledge and mental endurance of the evaluators. [Ref. 13:p. 153] That is why as more precise information becomes available other methods, such as the economic or optimization models, are usually more effective.

C. ECONOMIC METHODS

In its simplest form, an economic model involves constructing an index of benefit/cost ratios. Capital budgeting techniques are used to assign financial or dollar values to the benefit and cost variables of the project. The purpose of assigning dollar values is to try to develop a universal, quantitative measurement of IR&D projects. A subjective value concerning the likelihood of technical success is also assigned to the project. The function of the model is to select those projects which are likely to be successful and provide the greatest benefit while staying within the overall budget constraint. The following is a relatively simple example of a benefit/cost ratio.

$$I_i = (B_i \times T_i)/C_i$$

where:

i = 1, 2, ..., n where n is the number of projects,

I_i = Index value of project i,

B_i = Estimated value of project i,

T_i = Estimated chance of technical success of project i,

C_i = Estimated cost of research of project i. [Ref. 14:p. 26]

The next step is to maximize the value of benefits obtained within the constraint of the overall research budget. One method is to define a variable, S_i, that can take on the value of 0 or 1 depending on the value of I_i. In the case examined in this thesis of government laboratories with no profit-making requirement, any value greater than one (a reasonably safe breakeven point) will result in the initial selection of the project for further consideration. The organization can select any value that reflects its risk and profit requirements as its threshold value.

$$S_i = \begin{cases} 0 \text{ if } I_i < 1 \text{ (project not selected)} \\ 1 \text{ if } I_i \geq 1 \text{ (project selected)} \end{cases}$$

This step eliminates all projects that fail to meet the risk and profit requirements of the organization and establishes a group of qualified projects. Using the same variables identified in the benefit/cost ratio for defining estimated value, chance of technical success and estimated cost; the selection process is now a maximization problem constrained by the overall IR&D budget.

$$\text{maximize } Z = \sum_{i=1}^{n} B_i T_i S_i$$

$$\text{subject to } \sum_{i=1}^{n} C_i S_i \leq C$$

where C = total cost. [Ref. 14:p. 27]

There are several variations of this basic model. One popular technique, which is discussed below, is to incorporate discounted cash flow analysis into this model.

1. Discounted Cash Flows

A slightly more sophisticated approach is to use the present value of benefit and cost variables either alone or with the above model. The reasoning behind this is that all money has a time value. As the focus of the economic model is to examine financial resources, present value provides a more realistic assessment of benefit and cost values.

Discounted cash flow refers to the techniques used to calculate the time value of money. J. Hamaker defines cash flow as "the expected life cycle costs and revenues of a contemplated investment presented as a time series of dollar disbursements and receipts." [Ref. 15:p. 121]. Future funds are of less value to an organization than current equivalent funds, hence cash flows are discounted back to correspond to equivalent current funds. There are several methods for discounting cash flows once they been determined for each project. Two popular methods that deserve further description are present value and internal rate of return.

a. Present Value

Present value refers to the amount of funds necessary to invest now at a specified interest rate that is equivalent to the future cash disbursements and receipts of the project. In mathematical terms, the present value is the reciprocal of future value and is calculated by dividing the future value by one plus the specified interest rate. Because a discounted cash flow is a function of both the interest rate and points of time in the future, it may be necessary to determine a series of cash flows for a project to reflect the passage of time before its completion. This is a simple summation process of the individual (usually yearly) present values of the cash flow. [Ref. 15:p. 129] The general equation for calculating present value is:

$$P = F/(1+i)^n$$

where:

P = Present value,

F = Future cash flow amount,

i = Set discount or interest rate (expressed as a decimal value, not a percentage),

n = Number of periods separating present and ture time periods. [Ref. 15:p. 131]

A slight modification to this model that frequently appears in the literature is net present value. Instead of calculating the cash flow and then determining its present value, one calculates the present value of monetary benefits and the present value of costs. Net present value is determined by subtracting the present value of costs from the present value of benefits. [Ref. 14:p. 27] In both cases, any projects with a negative present value would be eliminated.

It is possible to use present value by itself as a screening tool but when ranking projects the additional step of developing a benefit/cost ratio allows the organization to incorporate a factor for the likelihood of technical success.

b. Internal Rate of Return

The internal rate of return is the discount rate that results in a present value of zero. This means that the effective rate of interest anticipated to be earned by the money invested in the project is equal to the present value of returns of existing projects. The usual method for determining the internal rate of return is trial and error. This process is naturally facilitated by the use of computers and iterative logic programming and most organizations use software packages rather then resort to the time-consuming process of graphic interpolation. [Ref. 15:p. 135]

Once the internal rate of return is known it is compared to the organization's minimum acceptable rate of return and accepted or rejected accordingly.

2. <u>Advantages and Disadvantages</u>

The main benefit of the economic method is that it is a widely accepted, easily understood technique that adds a more quantitative approach to project selection than scoring. The capital budgeting techniques are well defined, and application procedures are found in both engineering economics and in practice. [Ref. 6:p. 21] It is particularly appealing to non-research financially oriented managers. Economic methods are usually used to select product-oriented IR&D projects.

There are several disadvantages associated with this method. It is designed to consider only financially-based resources and ignores such resources as space, manpower, etc. Because time plays a major role in the determination of present value, economic methods favor near-term or short range projects that provide incremental increases (possibility of technical success) to the present business. [Ref. 14:p. 27] However, current discount methods properly applied are able to adjust for this shortcoming.

The information to construct this model is widely dispersed throughout the organization. The R&D department provides information on the likely input mix, technology requirements, and resource overlaps; production managers provide cost estimates; the marketing department provides sales or user estimates; the finance department provides overall budget constraints; and top management provides the long range strategic guidance or value. The more dispersed information is throughout the organization the more difficult it is to obtain accurate dollar estimates. [Ref. 16:p. 30] Finally, even though the end result is a quantitative model, the inputs for the equations are still subjective estimates.

D. CONSTRAINED OPTIMIZATION METHODS

The constrained optimization methods use linear and/or dynamic programming techniques in order to maximize benefits while selecting an optimal mix or portfolio of projects. These methods were developed because the process of project selection is not limited to selecting the best projects at a given point of time but rather is an on-going dynamic process of resource allocation between proposed and existing projects. The problem of resource allocation is created by the high probability that the requirements of the new proposed projects and existing projects will exceed current and forecasted capabilities. These models were developed to aid decision-makers in developing the optimal portfolio from a large number of feasible options. [Refs. 17:p. 119; 18:p. 36]

A number of authors have developed models based on these assumptions. [Refs. 2:p. 127; 17:p. 119; 19:p. 28] One clearly-defined, easily understood model is the one published by A.C. Bell and A.W. Read, which extends the capital budgeting techniques of the economic method to accommodate additional features [Ref. 19:p. 27]. The features incorporated into this model include a variety of resource constraints, a sequence of future time periods, and alternative versions of projects.

Alternative versions of the project can reflect such features as different rates of progress, alternative start periods, alternative technical approaches, etc.

Mathematically, the j^{th} version of project i is expressed as the variable, x_{ij}. The model is designed to produce a value for x_{ij} of 1 or 0, where 1 indicates selection and 0 indicates rejection of the project. The formulation of the model is as follows:

$$\text{maximize } Z = \sum_{i=1}^{n} \sum_{j=1}^{m_i} b_{ij} x_{ij}$$

$$\text{subject to: } \sum_{j=1}^{m_i} x_{ij} \leq 1$$

$$\sum_{i=1}^{n} \sum_{j=1}^{m_i} a_{ijkp} x_{ij} \leq A_{kp}$$

where:

x_{ij} = 0 or 1,

i = 1, 2, ..., n,

j = 1, 2, ..., m_i,

k = 1, 2, ..., N,

p = 1, 2, ..., P,

b_{ij} = Estimated value of version j of project i,

m_i = Number of alternative versions of project i,

n = Number of projects, current and proposed,

a_{ijkp} = Amount of resource k planned for version j of project i in period p,

A_{kp} = Overall availability of resource k in period p,

N = Number of resource categories,

P = Number of planning periods. [Ref. 17: p. 120]

This method requires careful analysis of the characteristics that form the parameters of the model. Perhaps the easiest parameter to define is time: the "planning horizon" or overall planning period, the number of planning periods and their length. Each resource must be clearly defined for each planning period by careful evaluation of current and forecasted capacities. Examples of resources under consideration include money, manpower, facilities, supplies, etc. Estimated values of each version can be determined using the capital budgeting techniques discussed in the previous section.

If the organization attempts to add more objectives and/or constraints, the model becomes too complex for linear programming. One solution is the use of dynamic programming, which decomposes a large mathematical model into a number of small problems that are solved recursively at each stage. [Ref. 20:p. 721]

1. Advantages and Disadvantages

This method is very attractive theoretically because it is designed to optimize quantitative measurements while meeting organizational constraints. Another advantage is its ability to incorporate many of the complex aspects of the selection process, such as existing projects, cross technology, manpower flexibility within the organization, recruitment options, etc. Despite these advantages, this method is not widely used. [Refs. 2:p. 127; 17:p. 119]

One of the major reasons for the limited use of this method is its relative complexity when compared to other available methods. It requires analysis of many if not all aspects of the organization; it attempts to optimize critical limited resources; it incorporates existing projects in its calculations; and it deals with a planning horizon to accommodate different decision periods. Another problem with the model described, which is present in many optimization models, is that it assumes that once a project has been selected it will be completed. The model thus fails to account for future organizational decisions that occur over the life cycle of the project. Input data requirements for this method are difficult to obtain with any degree of precision or confidence. For example, the calculation of the time-cost trade-offs among the various resources is a very difficult task. [Ref. 18:p. 38]

E. DECISION THEORY METHODS

Decision theory methods focus on the trade-offs that exist among a group of projects over several periods of time. These methods involve the determination of alternative strategies available at different periods over the life of a project and an evaluation of the potential risk associated with each alternative. Normally, by means of simulation analysis, a range of outcomes in the form of a probability density function are generated in order to define: (1) the expected value of the project, (2) the potential variance associated with that outcome, and (3) the project's probability of success. [Ref. 6:p. 22] A description of this decision process is illustrated by the construction of a decision tree for each project.

A decision tree provides the framework to diagrammatically represent the multiple stages that a project undergoes over time and the uncertainties associated with those stages. In other words, it provides the means to evaluate the risks and trade-offs associated with the different alternative strategies. It is a means to display the complex and stochastic nature of R&D projects in a concise manner.

Figure 1 illustrates the decision tree's ability to depict a number of characteristics. The tree extends over three time periods and incorporates two resource types. It illustrates a situation where the organization is faced with an initial decision point followed by an uncertain intermediate technical outcome. It not only illustrates technical outcomes but future decision points facing the organization. The advantage of this type of format is that it establishes a development path for each possible alternative, which clearly illustrates probability patterns and decision points for those alternatives on a pre-determined, common time scale. It is assumed that the discrete intervals of the time scale are fine enough to assume that decisions are made at the beginning of the time periods. Simply stated, this method is capable of representing uncertainties in the duration, resource inputs and technical outcome at each stage for each alternative. [Ref. 17:p. 120]

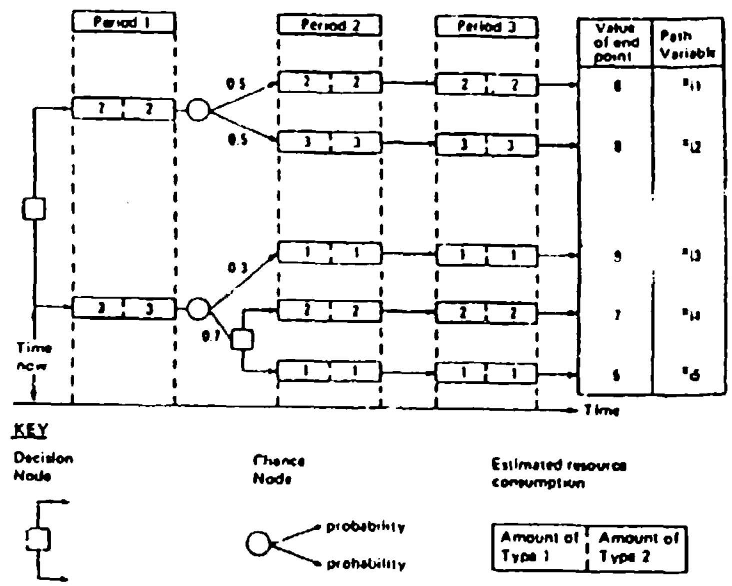

Figure 1. Decision Tree for Project i [Ref. 21:p. 951]

The establishment of de ision trees for the projects under consideration will create a portfolio of opportunities. At this point, under the assumption of limited resources, it is necessary to determine which subset of projects should be pursued in order to optimize the overall objectives of the organization.

One method is to maximize the weighted expected value of the final values of the projects pursued by the organization. The weight of the expected value is the probability associated with that final state or the probability that the indicated state occurs. The following model illustrates this maximization process.

$$\text{maximize} \quad Z = \sum_{i=1}^{n} \sum_{j=1}^{m_i} P(m_{ij})\, b_{ij}$$

subject to only one j for each i

$P(m_{ij}) \geq 0$ for all alternatives

$$\sum_{j=1}^{m_i} P(m_{ij}) = P(m_{i,1}) + P(m_{i,2}) + \ldots + P(m_{i,mi}) = 1$$

for all projects

where:

i = 1, 2, ..., n,

j = 1, 2, ..., m_i,

b_{ij} = Estimated value of version j of project i,

$P(m_{ij})$ = Probability of version j of project i,

m_i = Number of alternative versions of project i,

n = Number of projects. [Ref. 20:p. 575]

The above approach for selecting the optimal subset can develop into an exceedingly large problem. An alternative approach is based on the use of heuristics and simulation to generate a number of "good" solutions, leaving the final selection to the decision-maker. Another possibility is to use heuristics to develop upper and lower bounding solutions. [Ref. 21:p. 953]

1. Advantages and Disadvantages

Decision theory methods are useful in evaluating applied research projects, those projects that have well defined technical or commercial objectives. It provides a high degree of flexibility to demonstrate numerous possible versions of each proposed project. It attempts to address the complexity that exists in the real world. The above decision tree approach specifically handled the set of resources required over the life of the projects, possible technical outcomes, and estimated values of end states. [Ref. 17:p. 121]

The primary problem associated with decision theory methods is determining the degree of detail for a given project and gathering the information needed to determine probability estimates for each state and the expected value of that state. Prior to the construction of the decision tree the following estimates need to be completed: (1) cost estimates for resources required for each alternative, (2) benefits associated with each alternative, and (3) the probability distribution for each outcome. The large amount of information required means that this is a costly method to implement and maintain.

III. NAVAL RDT&E CENTERS

A. INTRODUCTION

There are currently eight Naval Research, Development, Test and Evaluation (RDT&E) Centers operating under the guidance of the Naval Space and Warfare Systems Command [Ref. 22]. Each of these RDT&E centers operates an Independent Research (IR) and Independent Exploratory Development (IED) program in their areas of expertise. The U.S. Navy divides its projects into these two categories rather than defining research and development as one unit. The IR portion refers to all efforts of scientific study and experimentation directed toward increasing fundamental knowledge and understanding in those fields of the physical, engineering, environmental, and life sciences related to long-term national security needs; the IED portion refers to all efforts, short of major development, directed toward the solutions of specific military problems [Ref. 23:p. 3]. This chapter will discuss the basic IR/IED programs these centers manage and describe each center in terms of its mission, areas of expertise and scope of its IR/IED program.

B. INDEPENDENT RESEARCH AND INDEPENDENT EXPLORATORY DEVELOPMENT PROGRAMS

The IR/IED program is designed to promote scientific and technological growth in Naval RDT&E centers and the development of knowledge and technology of interest to the Navy [Ref. 20:p. 1]. These programs are intended to support initial research and development in areas critical to the mission of each Navy center. Funding for IR/IED programs is furnished by the Office of Naval Research and the Office of Naval Technology in the form of discretionary funding under the control of the Technical Directors at each RDT&E center [Refs. 24:p. 1; 25:p. 3].

These programs enable individual scientists and technologists to conduct self-initiated research and development of promising but speculative solutions outside the requirements of normal funding authorization. This emphasis on innovation can provide an important and rapid test of promising new technology and fill gaps in a center's research and development program. [Ref. 26:p. i]

Even though procedural differences exist between the different RDT&E centers, the overall management philosophy is the same. An annual request for proposals is issued early in the year which contains preliminary guidance. After an initial review by IR and IED program managers, claimants are asked to make an oral presentation to specialist panels of qualified scientists and engineers. The evaluations by these panels are the primary tools used in the decision process by the IR and IED program managers to select projects. The program managers then prepare a suggested program which is reviewed and approved by the Technical Director. [Ref. 27:pp. 2-3]

C. DAVID TAYLOR RESEARCH CENTER (DTRC)

1. Mission

The mission of DTRC is to be the principal Navy RDT&E center for Naval vehicles and logistics and for providing RDT&E support to the U.S. maritime administration and the maritime industry [Ref. 22].

2. Expertise

The DTRC conducts research and development in nine technical departments: (1) Ship Systems Integration, (2) Ship Hydromechanics, (3) Ship Structures and Protection, (4) Ship Acoustics, (5) Ship Electromagnetic Signatures, (6) Propulsion and Auxiliary Systems, (7) Ship Materials Engineering, (8) Aviation, and (9) Computation, Mathematics, and Logistics [Ref. 22]. Primary areas of concern are new vehicle concepts, ship and aircraft compatibility, ship trials and the development of vehicle technology. Other areas addressed include hull-form, structures, propulsion, silencing, maneuvering and control, auxiliary machinery, environmental effects, pollution abatement, logistics research, computer techniques and software for analysis and design. Some specific research concerns include such projects as high strength hulls; naval machinery in such areas as power systems, ship automation and control, machinery dynamics, mechanical systems and components, electrical systems integration, and shipboard energy conservation; underwater acoustics and ship vibrations; and metals and alloys, corrosion, fuels and lubricants, paints, welding and fabrication. [Ref. 28:p. 186]

3. IR/IED Program

During the fiscal year 1989, DTRC funded 25 IR projects at a cost of $2,528,000 and 18 IED projects at a cost of $1,505,000. Nine general IR areas were investigated. They were: (1) acoustics, (2) applied mathematics, (3) physical and mechanical metallurgy, (4) ceramics, glasses, and related inorganic solids, (5) hydromechanics, (6) aeromechanics, (7) structural mechanics, (8) electrical power generation, and (9) a miscellaneous category for in-house projects. The IED projects were all in one general area, Naval vehicles. [Ref. 29:pp. 95-99]

D. NAVAL AIR DEVELOPMENT CENTER (NADC)

1. Mission

The mission of NADC to be the principal Navy RDT&E center for Naval aircraft systems less aircraft-launched weapon systems [Ref. 22].

2. Expertise

The NADC is divided into three Warfare Systems Departments and four Engineering and Technology Departments: Antisubmarine Warfare Systems, Tactical Air Systems, Battle

Force Systems, Communication Navigation Technology, Air Vehicle and Crew Systems Technology, and Systems and Software Technology. Primary areas of effort are airborne ASW systems; airborne expendable microwave countermeasures; air command and control systems; airborne communications systems; air crew equipment and life support; airborne active and passive search, reconnaissance and surveillance systems and equipment; navigation systems, both inertial and autonomous, for air, surface and subsurface platforms; Naval airborne targets; Naval air vehicles including unmanned air vehicles; aircraft systems; and air vehicle modification and equipment installation. [Ref. 22]

3. IR/IED Program

During the fiscal year 1989, NADC funded 25 IR projects at a cost of $2,871,000 and 16 IED projects at a cost of $1,633,000. Some examples of the types of IR projects funded include emission studies, diffusion modeling, high temperature superconductors, laser diode velocimetry, nonlinear model development, and fracture science. Examples of IED projects include ASW threat plan recognition, gate array technology for processors, advanced parallel processor development, laser radar returns, artificial intelligence for unmanned air vehicles and microwave communications. [Ref. 26: pp. 6.1-7.2]

E. NAVAL COASTAL SYSTEMS CENTER (NCSC)

1. Mission

The mission of NCSC is to be the principal Navy RDT&E center for mine and underseas countermeasures, special warfare, amphibious warfare, diving and other Naval missions that take place primarily in the coastal regions [Ref. 22].

2. Expertise

The program work at NCSC is divided into ten main areas of concern: (1) Airborne Mine Countermeasures, (2) Surface Ship Mine Countermeasures, (3) Sonar and Torpedo Countermeasures, (4) Amphibious Warfare Support, (5) Marine Corps Tactical Deception, (6) Marine Corps Land Mine Warfare, (7) Coastal/Special Warfare Support, (8) Ocean Engineering and Mechanical Engineering, (9) Warfare Analysis, and (10) Advance Engineering and Technology Disciplines [Ref. 22].

3. IR/IED Program

During the fiscal year 1989, NCSC funded nine IR projects at a cost of $896,000 and ten IED projects at a cost of $555,000. IR projects were in the areas of acoustic sensors, corrosion, electromagnetic detection, superconducting materials and devices. IED projects included such topics as multispectral imaging techniques, AUV decision management, copolymer hydrophones, curved lens development, and corrosion studies. [Ref. 30:pp. A.1-B.2]

F. NAVAL OCEAN SYSTEMS CENTER (NOSC)

1. Mission

The mission of NOSC is to be the principal Navy RDT&E center for command, control and communications; ocean surveillance; surface- and air-launched undersea weapons systems; and submarine arctic warfare [Ref. 22].

2. Expertise

The program work at NOSC is divided into nine major areas: (1) Command, Control and Communications, (2) Ocean Surveillance, (3) Antisubmarine Warfare, (4) Ocean Sciences, (5) Ocean Engineering, (6) Submarine Arctic Warfare, (7) Intelligence Support Systems, (8) Biosystems Research, and (9) Support Technologies, including integrated circuit design and fabrication [Ref. 22]. A short list of examples of specific research areas include: automated integrated and laser communications systems; development of ship, shore and submarine communications systems; major surveillance systems, including autonomous arrays, towed arrays, deployable arrays, fixed distribution systems, active systems and systems for Arctic applications; manned and unmanned submersibles and underwater work and search systems; development of electro-optic devices for communications, surveillance, weapon delivery, and other military applications; underseas surveillance systems, including sonar signal processing, sensors, data acquisition systems, and transduction science; and manufacturing technology in materials, microwave tubes, fiber optics, weapon systems, design and processing of integrated circuits, and robotics applications [Ref. 28:p. 185].

3. IR/IED Program

During the fiscal year 1989, 33 IR projects at a cost of $3,151,000 and 23 IED projects at a cost of $1,680,000 were funded. The call for IR proposals stressed the areas of Command, Control and Communications, ASW research, novel solid-state materials and devices, signal and image processing, secure survivable communications, and arctic research. The call for IED proposals emphasized the areas of artificial intelligence, arctic warfare, artificial neural networks, electro-optics, software engineering, and network technology. [Ref. 25:p. 3]

G. NAVY PERSONNEL RESEARCH AND DEVELOPMENT CENTER (NPRDC)

1. Mission

The mission of NPRDC is to be the principal Navy RDT&E center for manpower, personnel, education, training, and human factors and for providing technical support to the Chief of Naval Operations in these areas [Ref. 22].

2. Expertise

The NPRDC is divided into six departments, which concentrate on the major areas of its mission: (1) Manpower Systems, (2) Personnel Systems, (3) Testing Systems, (4) Training Technology, (5) Training Systems, and (6) Human

Factors [Ref. 22]. Actual areas of research and development addressed by the NPRDC are management systems, personnel and occupational measurement, career development and retention, motivation and productivity, instructional technology, training systems, and command and support systems [Ref. 28:p. 186].

3. IR/IED Program

During the fiscal year 1989, NPRDC funded six IR projects costing $250,000 and four IED projects costing $195,000. IR projects were brain activity during visual recognition, diagrams for learning procedural tasks, memory performance, tutoring in technical training, stable performance of a complex cognitive task, and instructional analysis. IED projects were military recruitment quality, optimization of nonlinear objectives, personnel loss forecasting, and decomposition methods. [Ref. 24:pp. 2-3]

H. NAVAL SURFACE WARFARE CENTER (NSWC)

1. Mission

The mission of NSWC is to be the principal Navy RDT&E center for surface ship weapons systems, ordnance, mines, and strategic systems support [Ref. 22].

2. Expertise

The program work at NSWC is divided into seven major areas: (1) Combat Systems, (2) Surface-launched Weapon Systems, (3) Underwater Weapon Systems, (4) Strategic Weapon

Systems, (5) Electromagnetic Combat, (6) Protection of Weapon Systems, and (7) Autonomous Weapon Systems [Ref. 22]. Some specific research areas being investigated include Nitinol-using devices; penetrameters or image quality indicator studies for radiographic efforts; thermal analysis of changes to physical or chemical properties of materials; acoustic testing of arrays, projectors/hydrophones, and special acoustic devices; hardening of digital electronics against gamma radiation; and water-entry and water-exit phenomena [Ref. 28:p. 123].

3. IR/IED Program

During the fiscal year 1989, NSWC funded 51 IR projects at a cost of $4,141,000 and 22 IED projects at a cost of $2,457,000. Some examples of the type of IR projects funded included accelerator-based atomic physics, nonlinear dynamics and fractals, high-temperature superconducting wire, neural network technology, munitions chemistry, superlattices, biotechnology, ceramic science and polymer science. Some examples of the type of IED projects funded included superconducting wire, underwater warhead technology thrust, neural networks, Ada for SIMD processors, expert systems, and water vapor absorption of radiation. [Ref. 27:pp. 12.3-12.11]

I. NAVAL UNDERWATER SYSTEMS CENTER (NUSC)

1. Mission

The mission of NUSC is to be the principal Navy RDT&E center for submarine warfare and submarine weapon systems [Ref. 22].

2. Expertise

NUSC is organized into major product line directorates and departments. Those principal product lines are: Submarine Integrated Combat Systems, Submarine Sonar, Surface Ship Sonar, Submarine Electromagnetic Systems, Submarine Combat Control Systems, Torpedo Systems, Submarine Tactical Missile Systems, Launcher Systems, Underwater Target Simulators, Undersea Ranges Development and Operation, and Warfare Analysis. [Ref. 22] Areas of unique expertise include acoustic arrays, signal processing, acoustic transducers, modeling and analysis, environmental acoustics, hydrodynamics, and propulsion [Ref. 28:p. 136].

3. IR/IED Program

During the 1989 fiscal year IR/IED funding totaled $4,958,000: $3,138,000 was used to fund 32 research projects and the remaining $1,820,000 funded 19 exploratory development projects. Fourteen technology areas were addressed by the projects funded. The 11 research areas investigated were Acoustics, Applied Mathematics, Arctic Research, Electrical Power Generation, Electromagnetic Wave Propagation and Radiation, Engineering Psychology, Hydrodynamics, Information

Processing Devices, Mathematical Statistics and Engineering Applications, Oceanography, and Structural Mechanics. The IED portion reported three areas of investigation: Support Technology, Target Surveillance and Weaponry. [Ref. 31:p. 1.3]

J. NAVAL WEAPONS CENTER (NWC)

1. Mission

The mission of NWC is to be the principal Navy RDT&E center for air warfare systems (except antisubmarine warfare systems) and missile weapon systems and the national range/facility for parachute test and evaluation [Ref. 22].

2. Expertise

Program work is directed toward air warfare systems, missile weapon systems, ordnance, foreign material exploitation, and support systems [Ref. 22]. Specific research areas include electronic circuits and systems, electronic quality assurance, microwave tube design, co osion from salt water and weather effects, p otechnic ch try, and testing of primary and secondary batteries [Ref. 28:p. 186]

3. IR/IED Program

During the fiscal year 1989, NWC was allocated $4.042,000 for its IR program and $1,865,000 for its IED program [Ref. 28:p. 8]. One notable project was on the development of an INTEL artificial neural network microelectronics chip [Ref. 32:p. 12].

K. SUMMARY

The information about the above funding levels came from the IR/IED Annual Reports issued by each center. The overall funding level was almost $34 million dollars, a relatively small amount considering the importance of the program. This program is the spark of creativity used to attract and retain highly qualified scientists and engineers in many diverse fields of study. Its importance to the strength and success of a facility needs to be periodically stressed.

The wide variety of topics selected for investigation reflects the wide range of needs of today's U.S. Navy. Each center has to select and reject projects (hundreds of projects are rejected each year) that cover several major fields of study. The development of a fair and consistent selection process is an important part of this program.

IV. AN INTEGRATED APPROACH TO IR&D PROJECT SELECTION

A. INTRODUCTION

As demonstrated by Chapter III, Navy laboratories perform independent research and development in a wide range of fields. Responsibilities of Independent Research and Independent Exploratory Development (IR/IED) directors will increase even more once the planned consolidation of the administrative and support services of the laboratories occurs and they are responsible for additional fields of study at more facilities. This is a period of change for research departments and many procedures are being evaluated, including the selection process of IR/IED projects.

The four theoretical methods discussed in Chapter II each have distinctive advantages; it would be impossible for any one selection process to incorporate all of these advantages. One key characteristic of the projects that assists in determining which selection processes would be appropriate is the degree of uncertainty associated with performing an accurate cost-benefit analysis on the project. By separating projects into categories determined by the degree of uncertainty associated with benefits and costs estimates, it is possible to tailor the selection process to a particular category and compare projects with similar characteristics.

The Navy has established a clear distinction between independent research and independent exploratory development, and the selection process for these two areas is performed separately. One advantage of establishing separate categories is that projects are grouped by similar characteristics prior to selection. It also permits the existence of two different selection procedures in order to take advantage of strengths of the different methods.

B. INDEPENDENT RESEARCH PROJECTS

Independent research projects can be characterized as the initial stage of a project, during which time the degree of uncertainty surrounding the project is high and the ability to perform a rigorous cost-benefit analysis is poor.

1. Scoring Implications

Use of a scoring model in order to screen projects is an appropriate method to assess IR projects. However, a scoring model can be successful only if its criteria are based on organizational goals. Clearly defined criteria form the basis of communication between top management, expert evaluators and researchers. Chapter II illustrated how to set up the mechanics of a scoring model; it does not discuss the criteria against which a research project is judged. Any recommendation regarding the use of a scoring model must include the definition of key criteria. The approach developed by M.J. Cooper [Ref. 33] was designed to produce well-defined parameters and provides a common language to discuss selection criteria. A modification of his approach for a military laboratory forms the basis for discussing key criteria. The three primary criteria examined are impact, feasibility and research merit. [Ref. 29:p. 29]

2. Criteria

a. Impact

Impact is the term used by M.J. Cooper to describe the utility of the project and can be simply defined as the reason for undertaking the project: Why should this project be considered? In seeking to answer this question the researcher needs to identify: (1) how his project meets a military service goal, and (2) the users who will benefit from this new technology. Association with a service goal shows that the project explores a technology with an actual utility to the military service. The identification of users establishes a potential market for this new technology.

One way to explore the potential benefits of the project is to define it in terms of the recognized and direct segment of the market affected: Does this project have applications for the Department of Defense; the U.S. Navy; a community within the U.S. Navy (surface, submarine, aviation, etc.)? A project that provides limited support to a critical national goal can be of greater importance than one that is of major importance to a small interest group. This means that a project's market should be defined in two stages. The first stage is to define the importance of the goal it seeks to support. The second stage is to identify the level of importance of the project in meeting that goal: To what degree is the next stage of the goal reliant on the successful completion of this project? The following scale in descending order of importance of organizational goals, established by M. J. Cooper, has been slightly modified for a Navy organization:

- Recognized National Goal.
- Recognized Department of Defense Goal.
- Recognized Navy Goal.
- Supports Navy Goal.
- General Research Function of Laboratory.
- Limited Interest to Select Subgroup. [Ref. 29:p. 30]

The identification of end users means that, in addition to the establishment of a potential market, the researcher is able to communicate with those users. The interaction between the researcher and users can have a major impact on the final success of the project, especially concerning modifications of the project and its impact on users' procedures. For example, if the project has applications to another established research project, coordination between the two research groups can ease the incorporation of successful applications into the established program. By establishing communications with the end users and incorporating their concerns, the research group is able to mitigate the users' resistance to change and develop a commitment in the users to the project's success. The end users' attitudes toward the project can affect the ultimate acceptance of new procedures and products that result from the research. The reaction of the end users to the final product of the project can be rated according to the following scale:

- Direct User Participation.
- Substitution in Existing Market.
- Uncertainty, Indifference.
- Conflicts, Displacements of Existing Procedures. [Ref. 29:p. 31]

Under this system, impact is measured by a project's programmatic utility, which is discounted by the effectiveness of the interaction with end users.

b. Feasibility

Feasibility is simply another term for risk or the likelihood of accomplishing the desired task. It is the probability that the project will achieve technical success given its planned time and resource constraints. [Ref. 8:p. 223] Specific factors that influence possible success include technological risk, technological competence of the researcher and the availability of competent management personnel.

Technological risk refers to the availability of technology necessary to complete the project. Research that is based on a mature technology or seeking to initiate an incremental change to an existing technology will naturally have less risk associated with it, than research that is predicated on information not yet available. The following scale reflects the general level of knowledge in the field of study under investigation:

- Demonstrated Practice.
- Evidence Technology Exists.
- Uncertainties, but Supposedly Resolvable.
- Major Advances Required. [Ref. 29:p. 31]

Resource availability is a key factor in determining a project's feasibility. For this paper, technological competence refers not only to the qualifications of the primary investigator but also to the presence of appropriate technical skills and facilities needed by the research group to accomplish its task. One key resource which is not always included in the decision-making process is the availability of management personnel capable of supplying needed support services. The following scale reflects various levels of availability:

- Necessary Personnel, Skills, Facilities.
- Personnel, Skills, Facilities Generally Available.
- Requires Learning, Attaining New Skills (or facilities).
- Demands Significant Effort to Acquire Facilities or Skills or Learn Skills. [Ref. 29:p. 31]

Analyzing the resources needed to successfully complete the project provides the initial baseline for calculating the costs of a project. Determining a cost baseline is the first step to establishing a monitoring program to be refined over the life of the project.

c. Intrinsic Scientific Merit

The implicit goal of a research organization is to contribute to the level of knowledge of science and technology. The long-term viability of the organization is best maintained by the sponsoring of high quality work. The ability to attract and keep the best personnel is a function of the type of research conducted by that organization. Another benefit of the presence of leading researchers is that other experts and institutions seek to exchange information with the organization's personnel, thus exposing personnel to the broadest outside knowledge.

Scientific merit refers to the project's potential to contribute to new understanding of the phenomena being investigated. It provides the means for management to acknowledge the value of scientific and technological advancements. The following scale provides the means to rate research opportunity:

- Major Opportunity.
- Broad Impact.
- Complements Other Research Programs.
- Specific Technological Fix. [Ref. 29:p. 33]

3. Summary

M.J. Cooper's model provides a common language for communication between researchers and management. It establishes basic scales for ranking the different projects. It does not address the weights assigned to criteria. A study conducted at an Air Force research and development laboratory showed that service need and scientific merit accounted for 84 percent of the explainable variance associated with project selection of the six criteria used. [Ref. 8:p. 226] The criteria studied were similar to the ones discussed above. While this may indicate one possible direction, the setting of weights is the responsibility of top management. Independent research and development funds are among the last that are controlled by the laboratories, which gives senior management the flexibility and opportunity to set priorities on the laboratory level.

C. INDEPENDENT EXPLORATORY DEVELOPMENT

Independent exploratory development projects range from fundamental applied research to breadboard hardware. The projects under consideration address specific problems and are usually more narrowly defined than IR projects.

1. Scoring Implications

The information needed to conduct cost-benefit analysis on IED projects is obtainable using standard cost estimation techniques. Engineering economics is one such technique [Ref. 6:p. 21]. While this is an expensive method of calculating costs it is possible considering the types of projects undertaken. A review of annual reports of the laboratories shows that projects are specialized and clearly defined [Refs. 24; 25; 26; 27; 29; 30; 31]. They are headed by experts with a comprehensive knowledge of the project's function and components, and supported by experienced cost estimators. The growing costs of research combined with increased government concerns regarding cost management favors the use of economic models of selection whenever possible. The introduction of such practices as zero-based budgeting techniques is another indication that once the information exists to assess risks and assign costs, economic-based models are appropriate.

The simple linear economic model outlined in Chapter II is insufficient to be effective in practice. It assumes the use of dynamic programming to calculate the cost and benefit figures used to maximize the value of benefits. Minimally, the cost figure for the project is comprised of the cost of supplying the resources required to successfully complete the project and would be calculated as a smaller early problem. The same is true for benefits, which are calculated based on the potential market or uses for the project.

The total cost figure has to account not only for the overall budget but is subject to resource constraints. Scarce resources need to be identified prior to considering project proposals, and if it is obvious that there is a serious scarcity issue, a constrained optimization model should be should be used. The fact that requests for proposals are an annual event would indicate that a slight modification of the model discussed in Chapter II would be appropriate; namely, only one time period would be taken under consideration.

If the projects are so clearly or narrowly defined that only one version of the project is under consideration, then the basic economic model is sufficient. However, the existence of alternatives is a distinct possibility. Even though the other models, constrained optimization and decision theory, propose means for handling alternatives, both methods are very expensive, and differences among alternatives may not justify the cost of a complete analysis of each alternative. It may be sufficient for the researcher to identify the preferred or most likely alternative and prepare a cost-benefit analysis on that alternative, rather than do a rigorous analysis on all possible alternatives.

2. Summary

In situations where the information is available, the use of cost-benefit analysis provides an uniform method of comparing projects which is familiar to most managers. It also provides a solid basis for cost management of the project. Another advantage of the economic model is that it more clearly identifies needed resources than a scoring model and during a period of shrinking resources this issue has become more important.

C. MANAGEMENT ISSUES

The selection of a project is only the beginning of the management of a project over its lifetime. As the project progresses, it is essential to periodically evaluate costs and benefits as uncertainty decreases. The ability to perform effective cost-benefit analysis is not only an efficient way to maximize benefits but also is an effective means of monitoring the program over its lifetime. The information provided during the selection process is the baseline from which a project is evaluated.

N.R. Baker, A.S. Bean and S.G. Green studied 211 R&D projects; their findings indicate that the resolution of uncertainty is strongly related to the project's eventual success [Ref. 30:p. 29]. This is not restricted to financial considerations. They also demonstrated that while initial goal uncertainty does not significantly impact the project's final success or failure, the existence of clearly defined technological goals are statistically significant late in the life of a project. [Ref. 30:p. 32] This indicates that as a project progresses it should be reviewed, not only for costs, but its goals should be refined and clarified. One way to minimize failure is to establish a management program that focuses on reducing uncertainty over the course of a project. This indicates a formal periodic evaluation program would be useful which involves not only the research group but the financial department, end users and management. The research group will have the primary responsibility for ensuring needed participation by end users and this should remain an informal relationship until formal testing begins. The manager assigned to the research group is responsible for maintaining open communications with the group in order to periodically chart progress, clarify uses, identify existing programs that can benefit from this project and initiate the transfer of the project to an existing program when appropriate, monitor resource requirements and provide liaison with financial department regarding costs and resource availability.

V. FUTURE ISSUES

A. INTRODUCTION

There are three issues facing the Navy's Research and Development program that have the potential to significantly affect the Independent Research and Independent Exploratory Development (IR/IED) program. These issues are:

- The centralization and streamlining of Navy-wide Research, Development, Testing and Evaluation (RDT&E) responsibilities.
- The age of Navy laboratories, facilities and equipment.
- The current trend to focus on solving today's engineering problems rather than stressing development needed to meet tomorrow's challenges. [Ref. 1:pp. 192-195]

This chapter will discuss each of these issues from the perspective of how they impact the Navy's IR/IED program.

B. CENTRALIZATION OF RDT&E RESPONSIBILITIES

The main purpose of this program is to consolidate the management and support responsibilities of the numerous Navy facilities to five centers [Ref. 35]. This reorganization comes out of past Defense Management Reviews that called for the elimination of duplication of laboratory research, both within and across military services [Ref 36:p. 68]. This means that fewer IR/IED directors will be responsible for administering a larger program in terms of areas of investigation, budget and number of facilities [Ref. 35].

This situation will impact on the selection process of IR/IED projects by making the administration of the selection process more complicated. At the present time, for every one proposal approved, approximately three proposals are submitted [Ref. 25:p. 3]. If the current trend noted in several of the annual reports prepared by the RDT&E centers continue then the number of proposals submitted annually will increase. [Refs. 25; 27; 29] With the scientists and engineers who initiate the proposals dispersed over a greater geographical distance and investigating a greater variety of topics, the logistics involved in organizing the oral briefs and coordination of the expert panels will become more complex. This is an administrative problem that can be resolved. However, the final decision-makers are still the Technical Directors based on recommendations from the IR/IED directors. A problem that could develop, because IR/IED directors are not familiar with the additional functional areas, is that they may favor projects in areas where they are most knowledgeable. During this transitional phase a conscious effort must be made not to let traditional loyalties influence the selection process.

Another, less obvious issue is the lessening of overlap research in the move to eliminate duplication [Ref. 36:p. 70]. Duplication is the wasteful repetition of efforts, while overlap is the intentional investigation of different technical approaches to a given problem. A significant portion of the IR/IED program is the investigation of risky solutions to problems and can be classified as overlap research. A change in policy to eliminate overlap projects could have a major impact on the type of IR/IED projects selected. If risky alternative solutions are no longer being investigated then the risk of failing to find the best technical solution will increase. By not investigating overlap alternatives the military decreases its ability to deliver the highest quality solution to the problem. Another reason to pursue high-risk solutions to problems is that they often have high-payoffs in terms of technological quality and the discovery of additional applications not originally anticipated.

C. AGE OF FACILITIES

Navy RDT&E Centers' facilities and equipment are steadily becoming obsolete, inefficient and, in some cases, deteriorating. Currently over 73 percent of the Navy's permanent buildings used for research and development were constructed prior to 1960. Many of these facilities are of World War II construction. And the present renovation and replacement plan indicates that this is a worsening situation. [Ref. 1:p. 194]

Because the IR/IED program focuses on future technology, many of the projects require the use of state-of-the-art facilities and equipment. Resource constraint issues in the past have been the overall budget and, in some rare cases, the termination of approved projects because a principal investigator was not available [Ref. 27:p. 258]. The lack of modern facilities and equipment and increased competition for those limited renovated and new facilities will correspondingly increase the importance of resource allocation as part of the selection process of IR/IED projects. This could result in downgrading the importance of scientific merit as a decision criteron and increasing the importance of economic factors, such as resource allocation.

D. DEVELOPMENT PRIORITIES

There exists a trend in the research and development programs run by the government laboratories to fund projects to find engineering solutions to current problems rather than focusing on developing the technology of the future. Superior technology has long been a part of U.S. strategy. An integral part of maintaining technological superiority is an active and productive IR/IED program with its long-term view and stress on new technologies. In conversations with IR/IED directors, it was mentioned that there has been a slight erosion in the IR/IED budget at some RDT&E Centers and concern was expressed about a possible trend [Refs. 37; 38]. Any significant decrease in the IR/IED budget would have a major impact on the technological edge enjoyed by the U.S.

E. SUMMARY

The relatively small size of the Navy's budget compared to the benefits it generates strongest possible argument for its continuation. This program supports technological investigations into high-risk and high-payoff areas of interest to the Navy. It enhances the recruitment and retention of top quality scientists and engineers into the Navy laboratory system. It develops the technological base in new and emerging technologies while building the expertise of personnel. It encourages the interaction between laboratory personnel, academia and industry. And it enhances the scientific reputation of Navy facilities and personnel.

These advantages do not protect the program from general budget cuts or the facility and equipment constraints discussed in the previous sections. The need to optimize the available funds and resources is an important issue, and an effective selection process is one way to optimize the benefits of the program. As IR/IED directors become responsible for larger and more varied programs with fewer resources, the need to clarify goals and criteria is needed to ensure that the diversity of projects sponsored by the Navywide program does not suffer. A clearly-defined, well-planned selection process will encourage proposals and help maintain the flexibility of the current program.

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