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Decision-Making in Technologically Based Organizations

A Literature Review of Present Practice

Thomas E. Clarke

January, 1974

Ministry of State Science and Technology
Ministère d'État Sciences et Technologie

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Upon graduation from U.B.C. in 1967, Mr. Clarke joined the administrative staff of the Whiteshell Nuclear Research Establishment in Pinawa, Manitoba and in 1968 became Head of their Public Relations Office. He returned to U.B.C. in 1969 to take an M.B.A. degree, specializing in the management of scientists and engineers.

In 1970 he joined the Personnel Branch of the Defence Research Board in Ottawa. During his association with D.R.B. Mr. Clarke was involved in several internal management studies.

In 1972 he became a private personnel consultant and in this capacity joined the Ministry of State for Science and Technology. He has written several papers dealing with the effective utilization of human resources and has a continuing interest in the management of technological innovation.

Foreword

This report deals with decision-making in the innovation process of technologically based organizations. This report resulted from two studies: the first dealing with the present practice of industry and government in selecting research projects for support, and the second with the suggested project selection techniques recommended by business researchers.

This report represents the views of the author, which are not necessarily the views of the Ministry. The Ministry is publishing this report because it thinks it makes an important contribution to our understanding in this area.

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Table of Contents

Decision-Making in Technologically Based Organizations:

A Literature Survey of Present Practice

Introduction

This literature search was undertaken to determine if business management researchers had directed their attention to the area of decision-making in technologically based organizations. In particular it was hoped that the following questions regarding the innovation decision process might be answered, at least in part, by the available literature:

1. What factors or criteria are taken into account at each decision point in the innovation chain in deciding whether to proceed to the next phase of the innovation process?

2. Are the factors or criteria the same at each decision point in the innovation chain?

3. If the factors or criteria are the same, does the relative importance of the factors or criteria to the decision change from decision point to decision point?

4. How are these factors or criteria used in the decision process?

5. At what level or levels in an organization are the decision(s) made, and by whom?

6. Does the level and/or the decision maker change with the change in decision point?

7. Who has the most influence on the decision at each decision point and is this influence direct or indirect?

8. How wide-spread is the use of formal mathematical decision models in the innovation process?

In preparing this paper an attempt was made to provide a complete listing of articles and reports which have been published since the publication in 1967 of the paper "The Selection of R&D Program Content — Survey of Quantitative Methods", by Cetron, Martino and Roepcke[35]. Another major survey paper was that of Baker and Pound "R&D Project Selection: Where We Stand", written in 1964[19].

Types of Literature Reviewed

The literature examined in this study falls into three broad categories:

1. Studies concerned with the encouragement, transmission and evaluation of ideas for innovative activities. Martin[108], Baker, Siegman and Larson[20], Baker and Freeland[18]

2. Articles promoting or explaining the involvement of a firm's marketing group in the innovation decision process. Treeger[168], Roberts[140], Goodman[72]

3. Papers on project selection and evaluation.

This latter category can itself be subdivided into five subclasses:

1. Those papers which describe suggested formal models or techniques to be used in project selection. Mottley and Newton[115], Greenblott and Hung[74], Cochran et al[42], Souder[155], Rosen and Souder[142]

2. Articles which criticize, discuss, describe or evaluate the proposed selection models. Baker and Pound[19], Cetron, Martino and Roepcke[35], Moore and Baker[114], Souder[160], Gillespie and Gear[66]

3. Articles which describe the testing of suggested models in a real-time situation in an industry or actual work situation. Pound[136], Pessemier and Baker[133], Gear, Gillespie and Allen[62] Souder, Maher and Rubenstein[162]

4. Papers describing the present practice of managers in selecting or evaluating research and development projects. Dean[41], Gee[64], Mansfield et al[102]

5. Articles discussing the impact of formal project selection models or techniques on the bench scientist or engineer. Parmenter[126], Davig[46], Lipson[92]

Only a few papers were found which dealt with project selection in Canadian organizations. McCombs and Cooper[98], Cox[44], Chisholm[39], Little[93]. This lack of Canadian material by management researchers and practising managers was not unexpected but does, I believe, reflect the present low level of interest in improving the quality of the management of science and technology in Canada.

The following review of the literature will emphasize the results of empirical studies.

General Description of the Innovation Decision Process

The decision process in innovation is considered to be sequential. Gloskey[71] outlines the sequential nature of the decision process as part of his results of studying an R&D laboratory of a manufacturing company. (See Appendix A) His descriptive model considers the decision process from the idea stage to the production stage. Hess[82] and Ritchie[139] state that mathematical models designed for project selection must take into account the sequential nature of the decision process. Although the decision process is considered to be sequential it is apparent that information gathering for such data as potential market, manufacturing costs, research and development costs or general economic feasibility can be collected at the same time.

In a review of the innovation process, Globe, Levy and Schwartz[69] found that recognition of technical opportunity, recognition of the need, internal R & D management, management venture decision and availability of funding are, in that order, important factors in the development of an innovation.

Innovative Ideas

Although as Marquis[105] rightly points out "successful innovation begins with a new idea which involves the recognition of both technical feasibility and demand", this area of the innovation decision process has only been receiving the attention it deserves for the last six or seven years.

The literature in this area deals mainly with the evaluation of ideas. This preoccupation is considered by Ritchie[139] to be misplaced. He suggests that possibly a greater problem to some firms is not how to evaluate or select among ideas but how to stimulate the generation of good ideas. He considers that more attention should be paid to this aspect of the innovative process.

Rubenstein outlines an Idea Flow model which he feels reflects the idea process (Appendix B). Brandenburg and Langenberg[31] in an article describing the project selection and control procedures at Crucible Steel Corporation point out the need for R&D managers to be receptive to new ideas, and in fact to actively pursue them. Rockett[141] recommends the setting up of a group in corporate headquarters to evaluate new ideas and thus avoid an idea meeting a premature death at the hands of a risk averse (unreceptive) manager.

In a study of idea generation in a development laboratory of a product division of a large U.S. corporation, Baker, Siegman and Rubenstein[21] found that each idea generated was associated with two pieces of information. These were:

a) recognition of an organizational need, problem or opportunity which is perceived to be relevant to the idea generation group's objectives, i.e., ideas for new products or processes.

b) recognition of a means or technique by which to satisfy the need, solve the problem or capitalize on the opportunity.

Although both elements must be present before an idea is 'born' they noted that three quarters of the ideas put forward were initially prompted by a knowledge of an organizational need etc., and only one quarter by knowledge of a capability. Several "idea generation groups" consisting of five or six laboratory researchers and technicians were involved in this study. Of the 271 ideas studied 47 were judged by the "idea generation group" participants and the laboratory director as "best". Of these, 40 were the result of perceiving a need, etc. Utterback[169] also found that the generation of an idea was found to follow most often from recognition of a need or problem. Marquis[105] in a survey of 567 innovations of the improved process or product variety, or what he calls "nuts-and-bolts" innovation, found that approximately three quarters of them were stimulated by a market demand or a production need. Only one fifth were stimulated by recognition of the potential of a technical idea. From this Marquis concludes that "recognition of demand is a more frequent factor in successful innovations than recognition of technical potential". This conclusion is supported by a study of successful and unsuccessful attempts to innovate in two industries in England (Project Sappho). Achilladelis, Jervis and Robertson[2] found that successful innovators "pay much greater attention to marketing" and "have a much better understanding of user needs" than do unsuccessful innovators.

Although the stimulus for the idea appears to come mainly from outside the organization the actual sources of the ideas appear to be mainly internal. Dean[47], in a study of the idea flow process of 40 companies, found that two thirds of the ideas came from people within the firms and that three quarters of these internally generated ideas came from the R&D and the Sales and Marketing departments. In a study of 34 small electronics manufacturing companies in the Chicago area Martin[108] found that the sources of 67% of the 390 ideas studied came from within the firms themselves with the remainder coming from business associates, customers, suppliers, etc.

There have been several studies which deal with the evaluation of ideas both by the originators and by the management of the organization. Avery[14] and Marcson[103], to name a few authors, have demonstrated that management is more likely to consider an idea and to reward its originator if the idea is considered by management to be "relevant". An idea is considered relevant if it:

a) satisfies an existing (urgent) need or solves an existing (urgent) problem

b) can be developed into a new project which is compatible with the organization's overall goals and objectives

c) can be investigated with existing laboratory resources and facilities.

In an analysis of the disposition (made into a project, shelved, transmitted to a more appropriate potential user in the firm, rejected) of the ideas generated by the same "idea generation group" studied by Baker, Siegman and Rubenstein[21], Baker, Siegman and Larson[20] found that by having the ideas rated on three factors:

a) Urgency — the degree of immediacy of the need, problem or opportunity towards which the idea is directed

b) Predictability — the degree of certainty with which the methods and procedures are known

c) Time-Horizon — the expected length of time from initiation to completion of the research activity if the idea were accepted,

the ideas highly rated by the researchers and technicians (the idea originators) in this laboratory tended to be highly "predictable" and to have a short "time-horizon", predictability being the dominant factor. The ideas which tended to receive more favourable disposition from the management were those which were rated by the director of research as being directed at an immediate need, problem or opportunity. Thus, Baker et al concluded that the dominant factors influencing subjective evaluations by the researchers and technicians are different from the dominant factors influencing idea disposition decisions by management. It should be noted that idea disposition decisions and the subjective evaluations were significantly positively correlated. There was therefore agreement by both groups on the "value" of many of the ideas. Unfortunately Baker et al do not report who in the firm's management are involved in making the final decisions regarding the ideas.

There is additional support for this result in another paper by Avery[13]. In a study of 10 R&D laboratories, he found that in seven of them what is considered as a "best" idea changes as one goes up the organizational hierarchy from non-supervisory professionals to supervisors of research groups to laboratory managers. The patterns of ideas considered by the supervisors as "best" conformed more closely to that of the laboratory managers than did the patterns of non-supervisory professionals. In addition to his findings on the source of ideas, Martin[108] found the following positive correlations between an idea and its acceptance by an idea evaluator (whoever he may be):

a) the lower the perceived probability that a given idea will fail, the more likely that the idea will be accepted

b) the lower the perceived cost of implementing an idea, the more likely that the idea will be accepted

c) the more urgent the problem to which an idea is a potential solution, the more likely that an idea will be accepted.

This latter finding agrees with the findings of Baker, Siegman and Larson[20].

However, possibly of more interest, are the propositions Martin did not find support for. These are:

a) the more fruitful an idea source as perceived by an idea evaluator the more likely that a given idea from that source will be accepted

b) the greater the availability of the people required to implement an idea, the more likely that the idea will be accepted by an evaluator

c) the higher the projected profit or savings associated with an idea, the more likely that the idea will be accepted by an evaluator.

Unfortunately Martin does not discuss his finding no support for these propositions which one would have intuitively expected to be supported. In the case of the first unsupported proposition a "halo effect" would have been expected around good sources of ideas thus biasing the research manager's expectation of the value of future ideas. In the second it might be thought that an R&D manager might be favourably disposed to an idea which would put underutilized researchers to work. The inability to find support for the third proposition seems counter to common sense, as well as to studies such as Avery's[13] in which he found that managers in all 10 laboratories had a preference for ideas with specific economic consequences.

Although some of the empirical studies reported are not directly concerned with the innovation decision process it was felt that their inclusion would help the reader understand the process.

Literature on Marketing

This aspect of the innovation decision process appears to receive the least attention by those researchers studying research on research. The emphasis of what literature there is, is in the area of new product development. Roberts[140], in an article on product selection, points out the need to involve marketing researchers soon after the R&D group has developed a new product concept in order to reduce the risk of commercial failure. Roberts, unfortunately, does not define what he means by "developed", thus the reader is left in the dark as to whether marketing personnel should enter the picture in the idea evaluation stage or after a prototype has been created. The importance of R&D personnel working closely with Marketing personnel as early as in the idea evaluation stage is stressed by Treeger[168]. Treeger further considers that a close working arrangement is fostered by both groups developing an understanding of each other's methods and orientations. Muse and Kegerreis[117] and Cox[44] consider a close working arrangement between R&D and Marketing personnel as critical to successful innovation. This is further supported by Chisholm[39] who considers that R&D is a necessary but not sufficient condition for successful innovation.

Little[93] considers that marketing research studies should be started very early in the innovation decision process and not as a postmortem examination as to why an idea was not commercially successful. Brown[33], on the other hand, has marketing entering the picture after the product has been developed and is ready for production. This uncertainty about when Marketing and R&D personnel should collaborate is not resolved by the literature or survey results.

As an aid to market researchers in predicting the real market for a product and for adopting the optimal strategy in approaching that market at each stage of the product's life cycle, Goodman[72] suggests the use of mathematical marketing models. He considers that high technology companies could benefit from using marketing models in their decision process. Although in his article, Goodman illustrates the use of the models in the pre-production stage of the innovation chain there does not appear to be any reason that the models could not be used earlier such as in the idea evaluation stage. The input data to the models would, of course, be more tentative (e.g. cost per unit) than at the end of the development stage.

The above literature should not be construed as the only literature on marketing new products, but it was the only literature found in journals whose orientation is the innovation process in high-technology organizations. Marketing factors are also considered in the papers which follow on project selection. The theme of the marketing literature, such as it is, does support the findings in the idea literature of the need for the R&D personnel to be aware of the needs and opportunities existing in the market place.

Results of Surveys of Project Selection Techniques Actually Employed

One of the most recent surveys of project selection practices was conducted by a task force appointed by the Industrial Research Institute's Research-on-Research Committee. Gee[64] considers that the most significant result of the task force's study of 27 large companies was the development of a simple method of classifying R&D programs which enabled the task force to better understand their findings. It should be noted that this classification of R&D into Exploratory, High Risk Business Development, and Support of Existing Business has been used by other authors in the past[101]. They found that for R&D classed as "Exploratory", the project selection process was characterized by:

a) responsibility for project selection usually resides within the R&D establishment

b) the selection process is generally simple and unsophisticated

c) the selection process is based on qualitative information (a page or two of qualitative data) which is integrated without the aid of models, or with very simple rating systems to take into account such criteria as:

   i) consistency with corporate objectives

   ii) technical soundness and newness

   iii) capability of research people available for assignment

   iv) availability of special facilities

   v) reputation of originator

   vi) forcefulness of project proposal (or originator)

d) the selection process is influenced most by the background and views of one person or a small group of persons.

For R&D classified as "High Risk Business Development" (expansion into new business areas), they found the project selection process characterized by:

a) a new trend toward responsibility for project selection being given to a corporate level management committee often headed by the firm's president or other high corporate officer

b) some limited use of more sophisticated and quantitative selection techniques such as standard economic projections

c) very limited use of quantitative methods for dealing with uncertainty.

For R&D categorized as "Support of Existing Business", the task force found:

a) the selection process often influenced by a profit centre manager or a committee of which the R&D manager is only a part

b) that the common type of project in this category was process or cost-reduction research but that there was also product development to round out an existing product line

c) project selection was based on standard economic projections as there is a great deal of quantitative data with relatively low uncertainty.

In general the IRI task force found little use of computerized quantitative project selection models, although formal procedures were found in most of the companies in the sample. They also found very little use of such management techniques as decision analysis, risk analysis or simulation.

In a report on six pharmaceutical firms which were part of the group studied by the IRI task force, Faust[55] states that a number of pharmaceutical firms have experimented with the use of quantitative approaches, but that the utility of the effort in most companies has been questionable.

He found that research managers do intuitively evaluate various factors in making decisions concerning "Exploratory" research type projects. He lists some of the factors as:

Scientific

a) interrelationships with other research activities — synergistic advantages or competitive with other programs

b) probability of achieving project objectives

c) time required to achieve project objectives

d) impact on balance of short and long-term programs within research

e) estimated cost of the project in the coming year and to completion

f) utilization of existing research talent and resources

g) value as a means of generating experience and gaining a technical expertise in a field... a foundation for future research activities

h) need for a critical mass of expertise and activity to ensure progress

i) elasticity of resource input and probable output relationships

j) patentability or exclusivity of discoveries from project

k) competitive research effort in the area — in academic and government research centres.

Marketing

a) projected sales and profits from effort

b) relationship to need as reflected by current state of consumer satisfaction

c) status and efficacy of current competitive products or means of meeting consumer need

d) compatibility with current marketing capabilities and strengths

e) influence of new competitive products under development.

Organizational and Other Elements

a) relationship to activities at other research centres of units within the company

b) timing of project with respect to other activities in marketing, research, etc.

c) manufacturing capabilities and needs

d) prestige and image value to the company

e) effect on organizational esprit de corps and attitudes

f) impact of governmental, public opinion and other environmental pressures

g) alternative uses of scientific personnel and facilities if the project is dropped after a few years

h) moral compulsion to develop drugs meeting medical need but having low or no profit potential.

Not all the above factors would be considered in every decision on project selection.

Whitman and Landau[174] in a report on the chemical industry segment of the IRI study found that project selection becomes more rigorous and sophisticated as the expenditure level increases. "Exploratory" projects were found to be selected on an intuitive or subjective basis, while more costly "High Risk Business Development" projects are selected using more rigorous methods. For example, they found that in "Exploratory" research, the work was decided upon by the idea originator and his first line technical supervisor, a short informal write-up of activities and costs was prepared and funding usually came from a discretionary account earmarked for pioneering work. As a project required more of the firm's resources, more information was required such as:

a) more accurate description of the technical objective

b) estimation of technical success

c) market date, including probability of commercial success

d) possible capital commitment

e) best guesses on return on investment

f) risk analysis and discounted cash flow analysis.

Commercialization decisions are made by the top research and business management of the company.

In an extensive study of 36 firms Dean[47] found that the size of the R&D project and the size of the firm are the major factors in deciding who becomes involved in the evaluation of projects. In approximately two thirds of the companies surveyed, the groups concerned with the project selection decision are identical to those involved in project evaluation. This results in the chief executive officer of R&D also being involved in project selection along with senior officers of marketing, manufacturing, finance and planning.

Only rarely did Dean find companies which have separate and distinct evaluation and selection groups.

Dean states that formal, quantitative methods for selecting R&D projects are not widely used. From his study it appears that scoring models similar to the Mottley and Newton Scoring Model (See Appendix C) are the only mathematical models that have been tried. Although the firms did not generally use mathematical models, all 34 companies surveyed stated that they used quantitative criteria in selecting projects.

In 29 of the companies, the following economic criteria were used: (numbers in brackets indicate the number of companies using the criteria; some used more than one)

a) annual profitability (16)

b) annual rate of return on investment (14)

c) annual sales (7)

d) payout period (4)

e) cash flow. (2)

These results cast even more doubt on the lack of support Martin[108] found for some of the propositions mentioned in his study.

The following represents a synthesis of Dean's findings of organizational criteria (excluding profitability) used by the 34 companies to select R&D projects:

Research and Development

a) completion time and costs

b) ability to purchase design

c) availability of staff, facilities and funds

d) chance of success and concept evaluation

e) uniqueness of approach

f) technical advantages

g) balanced effort

h) staff training

i) search for knowledge

j) patentability.

Manufacturing

a) capital investment

b) compatibility with manufacturing capabilities

c) manufacturing requirements and needs

d) process improvement

e) quality improvement.

Marketing

a) market potential, penetration and share

b) growth potential and time to reach commercialization

c) risk of commercialization

d) corporate field of interest and growth objectives

e) defensive, proprietary or protective position

f) compatibility with marketing capabilities and channels

g) corporate image and policies.

In order that a comparison can be made between the factors used for evaluation purposes and project selection decisions, a list of the factors used by 32 companies to evaluate R&D projects can be found in Appendix D.

It will be noted that there is no real difference between the factors used for selection and those for evaluation. Dean's separation of these is artificial. For a listing of factors used by the firms when deciding to terminate a project see Appendix E.

For an example of a questionnaire used by a Canadian organization to select and evaluate proposed projects see Appendix F.

One question which does not appear to be resolved by the R^2 researchers is whether the factors considered at different decision points in the innovation decision process are the same or not.

Several authors such as Moore, Baker and Pound believe that economic data becomes more readily available and more meaningful and that decision criteria become better defined as projects approach the applied research and development stages. These beliefs are based on case studies by Gloskey[70]. Rubenstein[143] suggested that decision criteria may not only become better defined, but that they may also change in nature from the time of initial decision to the development of a final product. In a later paper, Hurter and Rubenstein[87] tend to question Rubenstein's earlier statement when they state that there cannot be any basic difference in evaluation procedure (criteria) for projects in various stages of the innovation process, otherwise there would be no reasonable basis for comparing projects at one point in time.

On this same theme, Holzmann[85] considers that the questions asked about a project are "virtually the same" regardless of when the questions are asked during the project's life.

In a study of several small high technology organizations Marolda and Laut[104] found that the decision makers selected projects on the basis of intuitive reasoning having considered a multiplicity of factors, some qualitative and some quantitative. Unfortunately they do not list the factors they found being employed.

Brandenburg[30], in a study of 14 technologically based companies, found "a progressive change in criteria for filtering new project proposals from scientific merits to economic potential occurred in moving from the 'R' toward the 'D' end of the R&D spectrum". He found that one of the following evaluation criteria appeared to be more appropriate than others as one moves along the R&D spectrum. They are:

a) estimated return on investment or calculated index number related to return on investment

b) estimated payback period

c) time, dollar cost, and dollar sales volume threshold goals

d) conditional statements of dollar costs and benefits from projects which could be initiated only if the particular project were successfully completed

e) estimated time required to demonstrate technical success

f) judgments concerning technical relevance of the results of the particular project to initiation of other projects of a similar nature

g) qualitative statements of the "value" to the firm of demonstrating technical competence in an area covered by a particular project

h) qualitative statements of "value" internal to the research process, in the form of improved research methodology, which can be learned from undertaking a particular project.

Brandenburg also cites some other factors which the R&D manager must take into account in project selection as:

a) the trade-off between assignments conducive to the long-term professional development of the individual and assignments contributing to the short-term economic performance of the firm

b) the benefits of motivation and commitment which come from stable project assignments for the researcher, versus benefits of flexibility to shift the direction of R&D efforts in response to new business problems or opportunities.

From the various surveys there seems to be good agreement on what the factors are that managers take into account. Unfortunately, the writers have not documented how the firm's decision makers actually use the factors in arriving at a decision. The exception to this is the few organizations who use a scoring model as described by Dean.

Practices of Individual Companies

The following is just an example of a few firms which have tried more sophisticated mathematical models. Literature describing other companies' procedures parallel the findings of Dean.

There are several examples given in the literature of researchers introducing formal mathematical models in the decision making process into industrial and governmental organizations. A few models appear to have caught on, most did not.

Souder[160] describes a planning and control system he introduced in the Organic Research Department of the Monsanto Company. This system involved the use of a dynamic programming model which was to perform project selection, budgeting and scheduling. A year after implementation the project selection segment of the system was allowed to fall into disuse.

Bell and Read[26] report on the apparently successful introduction of a linear programming model into the U.K. Gas Council, Operational Research Department and the Central Electricity Generating Board, North Eastern Region, in England. It is not clear from the article who uses the model in the organization although mention is made of the Research Director being involved with the model. It is also uncertain whether the model is now routinely used.

Atkinson and Bobis[11] describe a mathematical programming model introduced into the Organic Chemical Division of American Cyanamid. Again there is no indication whether the model was to be used routinely.

Examples of forms used by some companies in evaluating and selecting projects can be found in Dean[17].

Formal Models For Project Selection and Evaluation

It is apparent from the literature and from discussions with various authors working in this area that to date, no one has come up with an overall system for classifying the various proposed models or techniques. Moore and Baker[114] suggest that the models can be classified into four categories:

a) Scoring Models which compute an overall project score based on ratings of the project against preselected criterion considered critical to the project's (or a portfolio of the project's) success. Dean and Nishry[48], Garguilo et al[61], Mottley and Newton[115], Pound[136]

b) Economic Models which employ calculations such as net present value, internal rate of return or economic equations. Cramer and Smith[45], Dean and Sengupta[50], Disman[52]

c) Constrained Optimization Models which attempt to optimize an economic objective function subject to specified resource constraints. Charnes and Stedry[36], Freeman[59], Hess[82], Rosen and Souder[142], Weingartner[171]

d) Risk Analysis Models which are based on a simulation analysis of input data in distribution form and provide output in the form of distributions of benefit factors, as for example, rate of return or market share. Hertz[80], Hespos and Strassman[81], Pessemier[132]

As Moore and Baker point out, the latter models have greater input data requirements.

Souder[158], in a paper proposing a methodology to assess the value of management science models, employs five divisions of models:

a) Linear Models. Asher[10], Bell et al[25], Nutt[118], Souder[152]

b) Nonlinear Models. Atkinson and Bobis[11], Matheny[109], Rosen and Souder[142], Scherer[147], Hess[82]

c) Zero-One Models. Beged-Dov[23], Dean and Sengupta[50], Freeman[59], Minkes and Samuels[112]

d) Scoring Models. Dean and Nishry[48], Garguilo et al[61], Harris[77], Mottley and Newton[115]

e) Profitability Index Models. Ansoff[9], Disman[52], Gloskey[71], Hirsch and Fisher[84], Nyland and Towle[121], Olsen[122], Pacifico[124], Pappas and McLaren[125], Sobelman[151]

From an examination of the authors given as illustrations of the classification in the two papers, it is clear that Souder's first three models would generally fall into Baker and Moore's Constrained Optimization classification. Further, Souder's Profitability Index Models are nearly the same as Baker and Moore's Economic Model. The Scoring classification is the same. It should be noted, however, that the authors do not agree in which class the Dean and Sengupta paper falls. Souder classifies it as a Zero-One Model (Constrained Optimization), while Baker and Moore classify it as an Economic (Profitability Index) Model.

Holzman[85] talks of three methods of analyzing research projects. These are:

a) ranking the projects

b) treating them like a capital investment

c) employ sophisticated optimization procedures.

Unfortunately, he does not give examples. However, the last two conform quite well to Moore and Baker's Economic Model, and their Constrained Optimization Model respectively.

Baker and Freeland[17], in an unpublished survey of the literature, suggest that there exists two primary categories of models, namely "Benefit Measurement Models" and "Project Selection/Resource Allocation Models".

In an earlier published paper, Pessemier and Baker[133] divide Benefit Measurement Methods (Models) into three classes:

a) comparative methods, e.g., simple ranking or rating, paired comparison, dollar metric. Echenrode[54], Pessemier and Teach[134]

b) scoring methods. Mottley and Newton[115], Moore and Baker[114]

c) benefit contribution methods, e.g., economic return methods, risk analysis, relevance tree analysis. Ayres[15], Pessemier[132]

Models involving decision tree analysis have been receiving increasing attention by management scientists in the past few years. Hespos and Strassman[81], Flinn and Turban[56], Lockett and Gear[96]

Ranking Models

The techniques or models vary tremendously in their degree of complexity. At one end there is the simple ranking of projects based on the decision makers preference with his decision criteria either implicit or explicit, but not usually numerically evaluated. Other versions of this are mentioned by Pessemier and Baker[133] as Q-sort, paired comparison, dollar metric, standard gamble, successive ratings and successive comparisons. Basically, the decision maker compares one project with another one, or a grouping of projects, and selects which he prefers. In their paper, the authors describe the comparing of three types of ranking methods and suggest that the "Dollar Metric" method has some advantages over successive comparison and successive rating methods.

Scoring Models

The next model or technique in complexity is the Scoring Model. (See Appendix C) This method involves the identification of a small number of criteria or factors which are considered to be critical to the success of a project. Such factors might be total cost of R&D, time to completion of R&D, probability of technical and/or commercial succcess, size of potential market, to mention just a few. Competing projects are then evaluated as to the degree they meet the critical criteria, and are assigned a number indicative of this degree of compliance. The numbers assigned for each criteria are then combined to form a project score. The higher the score, the more desirable the project. Examples of scoring models are given in papers by Mottley and Newton[115], and Pound[136]. Because scoring models can incorporate qualitative input data as well as quantitative, these models are thought to be more appropriate for project selection or evaluation in the research stages of R&D. Baker and Pound[19], Moore and Baker[114]

Cetron et al[35] consider scoring models to be applicable in the research, exploratory development and advanced development stages of R&D. Moore and Baker[114], in an analysis of scoring models found that the method of construction can seriously affect the results they produce. They report that when a scoring model employs at least seven intervals for judging the degree of compliance with each selected criteria, and the factor ratings are added instead of multiplied together to produce an overall project score, the project rankings produced by scoring models are significantly positively correlated with rankings produced by economic models and constrained optimization models.

Economic Models

Economic models or profitability indexes by themselves run quite a gamut of sophistication. The earliest models were simply equations or formulae designed to produce a score or index related to the desirability of the project. Rubenstein[144] describes Olsen's method as one of the earliest equations described in the literature. This equation is given by:

$$\text{Value of new project} = \frac{(IR)(P_t)}{\text{Estimated cost of R\&D}}$$

where IR (Index of returns) is given by

IR = the value of process savings for one year, or
3% of new product sales for five years, or
2% of the sales value of improved products for two years

P_t = probability of technical success

Pacifico[124] suggested a project index could be generated by the following equation:

$$PI = (P_t)\ (P_c)\ \cdot \frac{(\text{Av. sales volume in units})\ (\text{Av. unit profit})}{\text{Total cost of R\&D}}$$

where P_c = probability of commercial success

It is quite apparent that such equations contain very subjective elements or judgments, such as the percentages, the time periods considered or the probabilities. More recently designed equations take into account the time value of money. Besides the standard economic evaluation methods borrowed from capital investment theory such as net present value calculations or internal rate of return, there are equations such as that of Hart[78]:

Profit Cost Index (PCI) =

$$\frac{(P_t)\ (P_c)}{1} \cdot \frac{(\text{Estimated present value of future earnings})}{(\text{Estimated present value of total R\&D costs})}$$

Optimization Models

Constrained optimization models are in general the most complex and require the use of a computer in arriving at an optimal project selection solution. As Souder[158] illustrates, these models can employ nonlinear, linear or integer programming. Lockett and Gear[95] favoured the linear programming approach as it provided the decision maker with more useful information.

In a recent review of management science models, Souder[158] found that Profitability Index (Economic) Models and Scoring Models had higher ease of use and lower cost of performance ratings, while Constrained Optimization Models had higher ratings on realism and flexibility. Souder[161] also suggests that the choice of which project selection model to be used may depend on the managers objectives, the life cycle stage of the set of available projects and the way in which the manager views his project selection problem.

Accuracy of Input Estimates

Most models or techniques contain elements such as estimates of probability of success, estimates of sales volume, unit price, time to completion of R&D, research and development costs and production costs, amongst others.

Various authors have directed their attention to the accuracy of such estimates. In examining the results of studies by Marshall and Meckling[107], Peck and Scherer[130] and Summers[164], Thomas[166] concludes that the evidence in relation to military R&D points to large inaccuracies in the estimates of cost and time to completion of R&D work. Ruskin and Lerner[146], however, suggest that it is possible to predict actual final cost and time to completion for military R&D contracts given initially negotiated costs and times and pertinent administrative factors. The results of studies of estimates in the industrial sector are more inconsistent. Studies by Meadows[110], Allen and November[6] and Allen and Norris[8] show a general underestimation of costs, while a study by Mansfield[100] found initial R&D cost estimates to be quite accurate. Thomas[166] in a study of two R&D laboratories found inaccuracies in estimates of the same order of magnitude as Allen and Meadows. He further concluded that uncertainty present in the R&D projects is not resolved quickly during the development period (i.e. estimates do not converge quickly towards the final values).

Mansfield and Brandenburg[101] in a study of a central research laboratory of a U.S. firm conclude that estimates of the probability of a project's technical success made before a project is started have some, but not much, predictive value of final outcome. Souder[156], however, in a study conducted at the Monsanto Company, concluded that with a group of research managers who were knowledgeable about subjective probability that the ratings of subjective probability of success were found to correlate very well with the eventual success and failure of these projects.

It should be noted when discussing probabilities of technical success or failure that an element of "self-fulfilling prophesy" exists. If a person considers that something is going to fail, then this opinion can influence his and others actions so as to consciously or unconsciously ensure failure: If success is predicted, then an extra effort may be put forward.

It is obvious, therefore, that more studies need to be conducted into this estimation question to remove the inconsistencies of results, but more important, to give guidance to those who wish to make more accurate forecast estimates.

Lack of Use of Formal Models

It is clear from the surveys reviewed that formal mathematical models are not widely used. Souder[157], in the literature review conducted in 1969 for his PhD Dissertation, found that only 14 models of the 41 described in the literature had ever actually been applied to real world problems. A survey conducted by the Stanford Research Institute[163] and one by Quinn[137] also indicated that only a few firms use operations research models. The 1964 survey of Baker and Pound[19] also noted the lack of use of mathematical models. Douds[53] notes that "today there is little reason to believe that the use of project selection techniques in industry has changed much from the level of use Baker and Pound found in 1964 which was essentially nil".

Pearson and Topalian[129] in a small survey of companies in England found that the use of formal techniques for project selection was a recent occurrence. They found a few companies using such techniques as a checklist of factors and project ranking formulae involving economic factors. These procedures are a far cry from the more sophisticated models suggested by management scientists. In an internal study of project selection procedures employed by Consolidated-Bathurst Limited, McCombs and Cooper[98] found that C.B.L.'s present methods were not very sophisticated as full use was not made of quantitative data that was or could be made available. They suggested that C.B.L. use an index number formula for ranking projects. However, because of a research department cut the results of this report were never implemented. In a recent study of U.S. Government laboratories, Chiogioji[38] found that the project selection procedures used in the laboratories were more qualitative than quantitative.

At this point one may well ask why R&D managers have been reluctant to embrace the models designed by the management scientists. With over one hundred models or variations to choose from, nobody could claim not to have a choice.

Obligingly, the same people who design the models also tell us why they are not used.

Ritchie[139] claims that quantitative procedures have been widely ignored by research management because:

a) many of the selection procedures (models) fail to include all aspects which research managers think are important or treat them in what they consider to be an inadequate manner

b) the selection models do not take into account multiple criteria

c) of inadequate handling of uncertainty

d) they do not explicitly take into account the sequential nature of research and development

e) many ignore the relationship between the research effort applied to a project and the chance of success.

Souder[154] suggests that the lack of application of the quantitative methods is also due in part to "a failure of management to view management science in its true perspective, and a failure of management scientists to understand the complexities of management problems". Pearson and Topalian[129] suggest that the mistrust and misunderstanding of many executives of the value of mathematical aids to business decision making is a major obstacle to the use of more formal quantitative techniques.

Rubenstein[145] puts most of the blame for the lack of use of project selection models on the models themselves. He states that many leave out factors essential to project selection decision-making such as:

a) adequate treatment of risk and uncertainty

b) the continuous nature of investments in, or expenditures for, projects

c) the need for multiple criteria

d) the interrelationships among projects

e) the continuous nature of project selection and review

f) the role of experience and intuition in such decision-making.

Rubenstein claims that the major shortcomings of the formal literature is the failure to consider project selection as a continuous day-to-day process extending throughout the life of the project. He suggests that project selection decisions are actually made whenever new information occurs due to:

a) changes in the environment, eg. shift in market demand

b) changes in progress on specific projects

c) changes in the resources of the organization.

Baker and Pound [19] identify four problems which have resulted in a low level of acceptance of systematic or rigorous methods of evaluation:

a) the inability of the models or techniques to describe the reality of the R&D selection process

b) the lack of a sufficient amount of historical information as input data to evaluation models or techniques

c) a lack of trust and knowledge by R&D people in the operations research methods

d) the lack of organizational stability needed for the introduction and continued use of formal methods of selection.

Hurter and Rubenstein [87] claim that the most often cited reason for not accepting analytical models as decision aids is that "they require data that are not available". As Hurter and Rubenstein point out, this argument is valid only if the cost of obtaining the additional data is greater than the benefits of making decisions based on greater information.

In an actual study conducted in a capital goods manufacturing company on what factors are important in the adoption or rejection of mathematical models for project selection, Maher [99] found that three factors had a relatively strong positive degree of association with an individual's willingness to adopt a mathematical model on a routine basis. These are:

a) the perceived value to an individual of the data generated by the model

b) the perceived appropriateness of the information considered by the organization as a result of using the model

c) the perceived value of changes in the projects' research objectives or activities.

Maher also noted an effect on the information seeking behaviour of individual users. They became aware of the need to increase their use of existing communication channels and to develop new ones to get the data they needed to make decisions. The following reasons why a mathematical model might not be used routinely were given by the respondents:

a) model is too complex, thus hard to understand

b) acquisition of required additional data not justified

c) model had design limitations.

Effect of Formal Decision-Making on Researchers

The last type of literature to be considered is that concerning the effect or impact of formal decision-making on the researcher at the bench.

Many papers[40,88,90] and books[123,131] have been written regarding the different orientation of scientists and engineers and the different work environment (degree of freedom) required to encourage and help motivate the researchers to accomplish creative work. In most of the studies, the researcher's freedom to select the projects to work on is noted as being of prime concern to him. Where this freedom has not existed the quality of work has been lower than where this freedom has existed[88].

Davig[46] suggests that researchers may consider formal project selection and evaluation techniques as a threat to their freedom to choose to work on scientifically interesting problems.

Parmenter[126], in a survey of researchers, group heads and laboratory directors at the central RCA Laboratories found the following consensus of project selection criteria considered by seven groups of the above mentioned individuals as being important to the working researcher. In decreasing order of importance they are:

1) enthusiasm of the researcher

2) scientific significance

3) enhancement of scientific understanding

4) benefit to mankind

5) publishable work

6) probability of technical success

7) probability of many consequences developing from work

8) likelihood of getting a patent

9) novelty

10) length of time necessary to carry out work

11) profit potential

12) pertinence to RCA's business

13) availability of necessary facilities and manpower

14) proprietary advantages

15) interest of management

16) ability to get government financial support

Unfortunately Parmenter did not report the complete lists for group heads or laboratory directors, but what he did state was as follows:

a) scientific significance was eighth on the list of importance to group heads and twelfth to laboratory directors

b) probability of technical success was first on the group head's list and third on the list for laboratory directors

c) profit potential was first on the list of important criteria for laboratory directors and third on the group head's list.

This shift in the importance of criteria as one goes up the organization's hierarchy supports the earlier mentioned studies by Avery [13] and Baker, Siegman and Larson [20].

It may be concluded that since researchers appear to employ a different weighting on the project selection criteria than their managers (the decision-makers) that the potential for conflict is substantial.

It is obvious that obtaining the maximum benefits of any decision-making procedure, formal or informal, will rely heavily on its being understood and accepted by the working researcher and his managers.

Summary and Discussion

It is apparent that the rate of production of papers on the topic of project selection and evaluation has not diminished in the last seven years. Since the 1967 paper of Cetron et al, this author has noted at least one hundred new additions to the literature.

In reviewing the findings it should be kept in mind that the adoption of more formal decision processes is supposed to:

a) aid in making decisions more logically consistent

b) allow research management to more clearly identify those projects or ideas which are well worth investing time and effort in, and those which are clearly not worth serious consideration

c) allow managers to be able to terminate unsuccessful projects at the earliest time justified

d) make researchers and managers aware of the information that should be acquired when making decisions on projects or ideas.

No author surveyed in the literature claims that the object of more formal decision models is to make decisions for the manager. The manager is still the decision maker. The models are to supply him with information which hopefully will enable him to make better (more profitable) decisions.

In analyzing the findings of this review, it should always be remembered that some of the statements made by the writers are based only on one study of one department in an organization. Therefore, at best, all one could say is that such findings tend to indicate a particular relationship which may or may not hold true in either a larger firm, a firm in a different industry or for any firm at a different point in its organizational development. With this warning in mind, I will now attempt to answer the questions posed at the beginning of this paper.

The list of factors reported by Dean and other authors in the selection and evaluation of projects appears to represent quite accurately the criteria which are considered by managers when deciding on project proposals. Project selection is essentially the analysis of the results of project evaluation which takes place at one particular point in a project's life, i.e., before major resources are committed to a project. If one considers project evaluation as a continuous process during the innovation cycle, then one must accept the comments of Holzmann[85] and Hurter and Rubenstein[87] that the factors do not change appreciably from decision point to decision point in the innovation decision chain. Brandenburg's[30] and Parmenter's[126] studies perhaps suggest what does happen. It appears that all the factors as described by Dean[47], for example, are considered to some extent at each stage of the innovation process but that the importance or weighting of the factors changes as you go from the research end of the R&D spectrum to the development end. As the potential magnitude of a project becomes larger, the organizational level of the decision makers goes up and thus according to the studies of Parmenter[126] and Baker, Siegman and Larson[20], there is a resultant shift in emphasis towards considering economic criteria more important. Thus the weighting on such a factor as "market potential" when considering an idea or project for research investigation (exploratory research) would be quite low (but not zero) while a factor such as "chances of technical success" would have a relatively high weighting factor. The distribution of weights would undoubtedly be very dependent on the particular management of the organization.

Unfortunately, except for the use of factors in scoring models, there is very little information on how managers use the factors in their decision process. One possibility is to set minimum standards on each of the important factors under consideration. If a project rates less than the minimum, then it is screened out or terminated.

There are indications that projects of an exploratory nature or those requiring few resources and/or funds are decided on between the researcher and his immediate supervisor. Those projects needing more funds and/or resources require the approval of the head of the R&D group. It appears that decisions which involve large expenditures of the companies' resources or the potential expenditure of large amounts of resources are made by the firm's top management after data is obtained from the firm's marketing, financial, manufacturing and R&D departments.

Who has the most influence might well depend on which factors (financial, marketing, technical) are considered at that point in time to be the most important.

What Baker and Pound[19] and Cetron et al[35] said in the summaries of their surveys of the literature still applies, namely that R&D managers are not very enthusiastic about employing formal mathematical models as an aid in decision-making, although quantitative criteria are employed. Experimentation with the more sophisticated programming models has taken place in only a few companies in the United States and England. This lack of use appears to be mainly due to the shortcomings of the proposed models.

As yet, there is no agreement amongst authors on a system for classifying the various selection models and techniques.

More work needs to be done in designing models or techniques which can be used by the managers of today who generally do not have a strong grounding in the use of the computer sciences in decision-making. It is pointless to design mathematical models or techniques if the potential user can understand neither the mathematical procedures used in them nor how the output of the model can help him make better (more profitable) decisions. Many of the suggested models or techniques require that a manager have a relatively good understanding of difficult mathematical procedures.

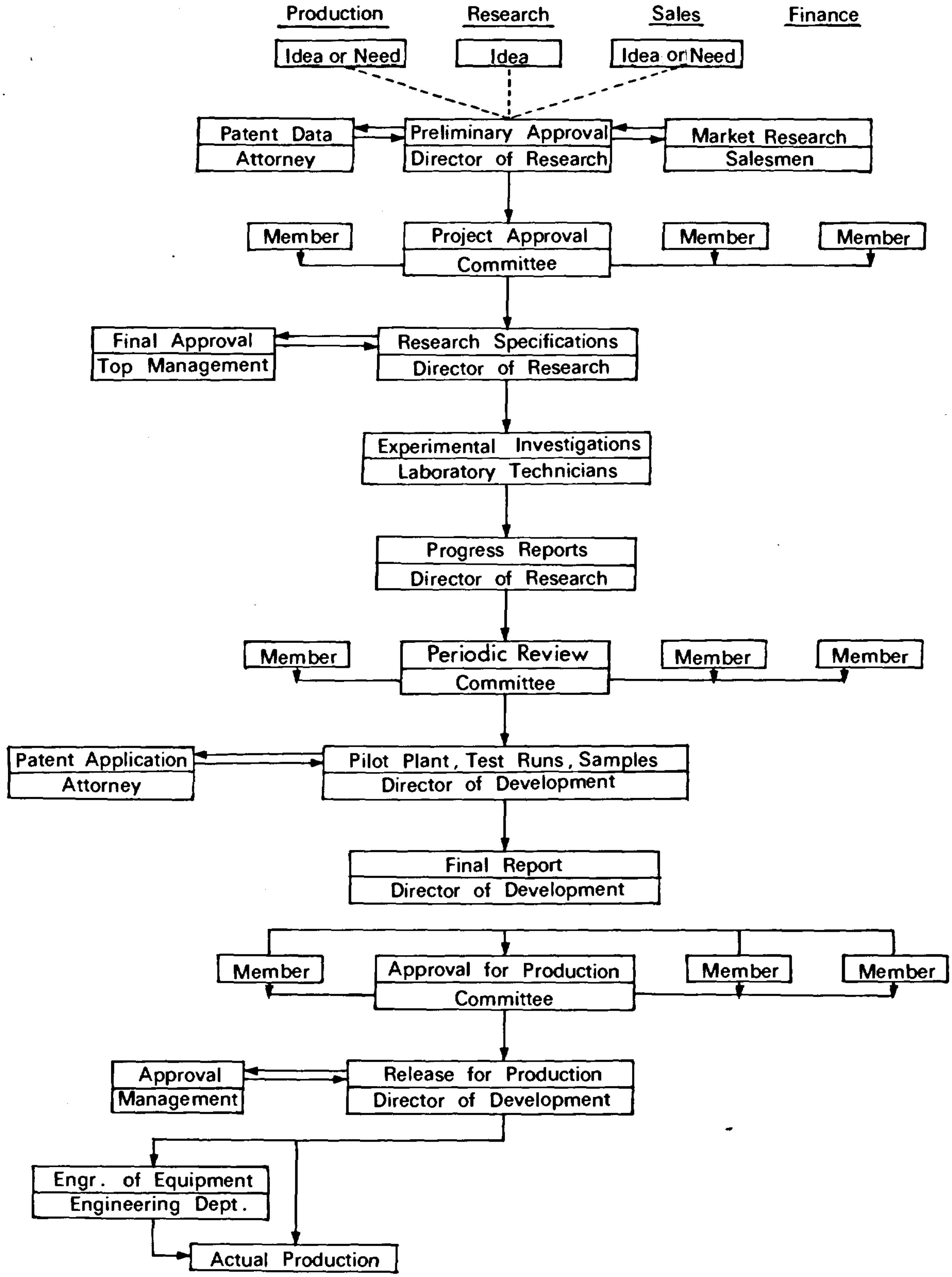
It is painfully obvious that for more formal evaluation techniques to be routinely used, both R&D managers and the bench researchers will have to be convinced of their value.

It is clear that there is a need to determine the best time to involve marketing personnel in the decision process. More work needs to be done on how to successfully introduce rigorous practical project evaluation procedures into the research management of a firm and in particular how to avoid possible negative effects on the morale and motivation of the scientists.

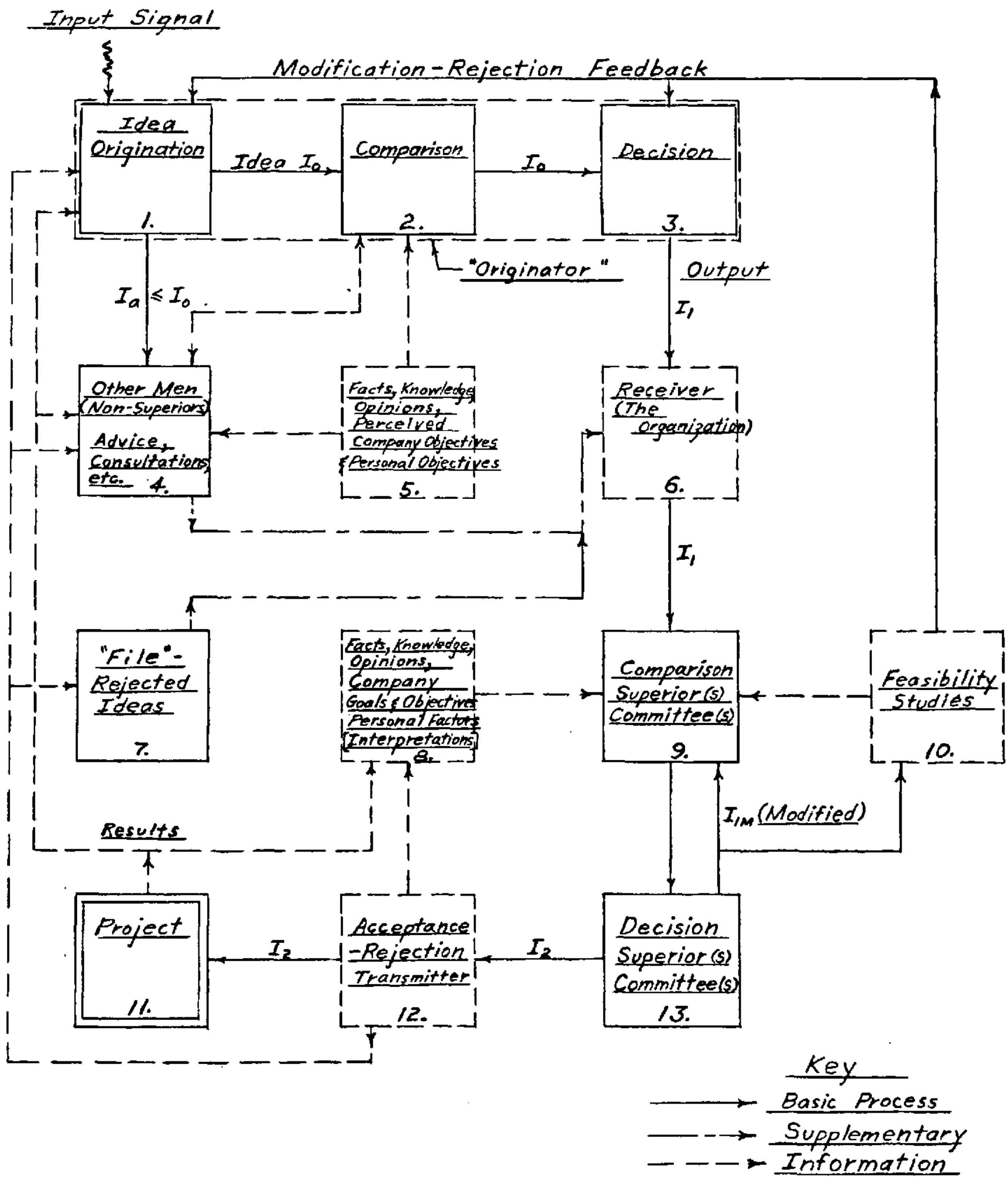
There is also a need to evaluate the effectiveness of the present practises of different companies with the view to promoting the use of the best of these in other organizations where suitable. This evaluation and promotion would be no small task but if successful, it could enable a country to more efficiently use the physical and human resources within its boundaries. Of more immediate importance, however, is the need for managers and management researchers to work together to develop and test project selection procedures in the real world environment of business. All too often practising managers discourage this type of cooperation because they consider the researcher's approach too "academic". Management researchers, on the other hand, consider the manager's approach too short-sighted. Both sides would benefit from this cooperation. The researcher would be drawn out of his "ivory tower" and the manager from his "mahogany row".

Appendix A

Gloskey's Model of the Sequential Decision Process



Idea Flow Model



Appendix C

Mottley-Newton Scoring Model

A scoring model involves the identification of a small number of factors or criteria which are considered to be critical to the success of a project. Such factors could be:

a) Probability of technical success

b) Estimation of time to completion

c) Cost of project

d) Size of net market gain.

Projects are then evaluated as to the degree they meet the critical criteria, and are assigned a number indicative of this degree of compliance.

For example: Probability of technical success

What is the best estimate of this probability?		90%	4
		70%	3
		50%	2
		25%	1
Cost of project	Less than 100,000		4
	100,000–400,000		3
	400,000–800,000		2
	1,000,000+		1

This procedure is repeated for as many factors as is considered important. The numerical ratings for each factor are then multiplied together to give a project score. Many projects in a firm's portfolio can thus be rated and their scores compared. It should be noted that qualitative factors can be handled by this method and given a numerical rating.

Several variations of this procedure have been proposed. One is to add the ratings together to obtain a project score. Another is to apply weighting to the factors with those considered more important having a greater effect on the final score. Those projects with the highest scores are the ones which the organization should fund.

Appendix D

Factors used to Evaluate R&D Projects in 32 Companies (number indicates the number of companies using that factor) A.M.A. Research Study B9, B.V. Dean.

Research and Development

Likelihood of technical success	15
Development cost	10
Development time	8
Capability of available skills	7
Availability of R&D resources	5
Availability of R&D facilities	3
Patent status	3
Compatibility with other projects	2

Manufacturing

Capability of manufacturing product	12
Facility and equipment requirements	6
Availability of raw material	2
Manufacturing safety	1

Marketing and Distribution

Size of potential market	23
Capability to market product	15
Market trend and growth	9
Customer acceptance	6
Relationship with existing markets	4
Market share	3
Market risk during development period	3
Pricing trend, proprietary problem, geographical extent, and effect on existing products (each)	2
Complete product line and quality improvement (each)	1

Financial

Profitability	17
Capital investment required	10
Annual (or unit) cost	7
Rate of return on investment	5
Unit price	4
Payout period	3
Utilization of assets, cost trend, cost reduction and cash flow	1

Timing

Timing of introduction of new product	7
Expected product sales life	4

Corporate Objectives

Fits into overall objectives and strategy	6
Corporate image	3

Appendix E

Rank Order of Important Factors Considered in Terminating R&D Projects in 36 companies. (Numbers represent the number of companies reporting the factor as being important) A.M.A. Research Study 89, B.V. Dean.

Technical	
Low probability of achieving technical objectives or commercializing results	34
Technical or manufacturing problems cannot be solved with available R&D results	11
Higher priority of other projects requiring R&D manpower or funds	10
Economic	
Low profitability or return on investment	23
Too costly to develop as individual project	18
Market	
Low market potential	16
Change in competitive factors or market needs	10
Others	
Too long a time required to achieve commercial results	6
Negative effect on other projects or products	3
Patent problems	1

(Appendix D and E reprinted by permission of the publisher from A.M.A. Research Study 89, "Evaluating, Selecting and Controlling R & D Projects", Copyright, 1968, American Management Association, New York.)

Appendix F

Checklist used by a Canadian organization for project documentation information

Project Decision Stages

Preliminary proposal

Project proposal

Product proposal

Commercialization proposal

The following are the questions which are asked at each decision stage:

1. What is the purpose of the project?
2. What products are likely to result from what applications? In what way are these products unique or unavailable from other sources?
3. Is a technology market area and marketing approach compatible with existing resources; if not, what new resources or approaches will be required?
4. What critical problems have to be solved successfully to develop, produce and market the product?
5. What are the levels of total cost and total revenues expected over the product life?
6. What are the probable major milestones?
7. What will be the cost and duration of preparing a complete (project, product, commercialization) proposal?
8. What other information is available in support of this proposal?
9. What is the specific next step recommended to follow this (preliminary, project, product, commercialization) proposal?

More precise answers to these questions are required as a project moves towards commercialization.

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