Chapter -2
CURRENT RESEARCH
IN THE PROBLEM DOMAIN

2.1 OBJECTIVE

Literature survey has been carried out with an objective of identifying:

a) Various models for selection of R&D projects, portfolio analysis, risk analysis, Concurrent Engineering and project planning and monitoring.

b) Strength and weakness of the models

c) Applicability of models to various phases of project life cycle as well as to various types of projects

d) Current problems and direction for the research.

2.2 METHODOLOGY

For the purpose of literature survey, the overall problem domain has been sub-divided into the following subdomains:

i) R&D Project evaluation and selection models including the risk analysis models and portfolio analysis models.

ii) Concurrent engineering models

iii) Project planning & monitoring models

The following are the details of models studied in each subdomain.

2.3 R&D PROJECT EVALUATION AND SELECTION

2.3.1 THE MEANING OF THE "PROJECT SELECTION"

The term “project selection” has many different connotations. In its narrowest sense, project selection means determining which one best project to work on. In its broadest sense, project selection may be viewed as a sequential portfolio determination process.
In this broader view, the decision maker wishes to determine the best allocation of the available funds among the alternative projects, and arrives at how much to spend on which alternative project, so as to obtain the largest total return possible with the available funds. The portfolio decision may be repeated periodically for the set of projects available for funding. This set includes projects which were not completed during the past year, plus any new ideas that came in during the year. These new ideas may be better than the old unfinished projects. If this is the case, then the old unfinished projects may be terminated and replaced with the new ideas. Thus, a sequential decision making process may unfold over time, with the portfolio decision being repeated at the end of each year or some other suitable time interval.

2.3.2 PROJECT SELECTION PROCESS

Traditionally, project selection process has been modelled as a sequential decision making process as shown in Fig. 2.1.

As per this process a screening model provides useful preliminary information for distinguishing candidate projects on the basis of a few prominent criteria. It provides a kind of quick and inexpensive first-level sorting out of the candidates. The results from the screening model may indicate that the project proposal is so poor that it should be rejected or the results may indicate that the project is acceptable but it may be backlogged. Or the results may indicate that a more in-depth analysis is warranted. An evaluation model provides a rigorous and comprehensive analysis of the project and its characteristics. As with the screening model, the evaluation model may indicate that the project should be either rejected or backlogged. Or it may indicate that this ideas is so good it should be immediately funded and put into the current portfolio. Or it may indicate that this idea should be competitively evaluated against the ongoing projects in a portfolio analysis model.

A portfolio analysis model is the most sophisticated of the three types of models, and it is usually the most expensive and the most time-consuming model to use. It determines the best way to allocate the available resources (or budget) among all the alternative projects.
Thus, project selection consists of three distinct kinds of decision making: screening, evaluation and portfolio analysis. As we move from screening to portfolio analysis, more factors are considered and the procedures become more complex. A variety of outcomes may occur as a result of each kind of decision. As time passes, these three types of decisions may be repeated many times in response to changing information states, changes in the available resources and funds, changes in project achievements or the arrival of new project proposals.

2.3.3 BROAD SCENARIO OF R&D PROJECT SELECTION MODELS

Due to its importance, R&D project-selection has been a long-standing problem in management science. Since, the 1960's, the R&D management literature and the operation literature, have been filled with articles describing project selection methods. Cetron et al. [1] cited over 150 models in the late 1960's, Souder [2] estimated that hundred of models had been developed by the mid-1970's and work has continued into the 1980's. But in reality, most of these techniques have failed in getting implementing. R&D managers are not utilising these techniques for some reason or others. In some cases, the techniques described in the literature were developed to meet the specific requirement, which cannot be readily applied in other setting. In other case, the techniques were developed in academic settings and they do not reflect the realities faced by R&D managers in industrial or governmental laboratories. A broad scenario of project selection models is given in Fig. 2.2. As shown in the figure, out of the large number of models available for project selection and evaluation, the models used by R&D projects may be classified as the scoring models, mathematical programming, portfolio analysis models, risk analysis models, cognitive models, probabilistic economic evolution models, conventional economic evolution models, adhoc models which includes profiles, interactive methods and the latest analytical hierarchy procedure and fuzzy models.

The above classification is purely based on the researchers perception of various models. Several authors have classified different models in different forms. The following are some of the important classifications found in literature.

(a) William (1969) [3] takes a simple approach to reviewing R&D selection models. He divides all the models into three types, based on the
Fig. 2.2: Broad scenario of project selection and evaluation models
general methods of analysis used. He refers to the first type as the "decision theory" approach. Here, numerous objectives or factors for evaluation are chosen by the decision maker, each of which is given a weight to represent its relative importance in the decision. R&D candidate projects are given a score for each evaluation factor. The scores are weighted according to the relative importance of each factor to the decision maker and then added. The sums are ordered from highest to lowest, and the decision maker picks those projects with the highest score. This approach can be used to produce a very simple or a highly complex version. The key feature in this model is that numerous objectives are simultaneously evaluated.

The second type of models is based on "economic analysis". A single criterion for selection is used: return on investment. The economic benefits of the projects are forecasted along with the associated costs. A calculation is made to derive the project's net present value (NPV) or internal rate of return (IRR). Projects are then ranked from highest to lowest based on the NPV or IRR and selected.

The "operational research" approach is the third type of models identified by Williams. This approach attempts to maximize the total value of the R&D project portfolio by using mathematical programming. Numerous constraints on the size and characteristics of the portfolio are specified. Linear programming is then used to maximize total value (a single objective) subject to the constraints.

William's review leads him to the conclusion that the "decision theory" approach is best for R&D project selection because numerous objectives can be measured. The use of numerous objectives in an R&D selection model will form the first building block of our R&D selection approach.

(b) Souder's review of R&D selection models (1972) [4] tries to empirically assess their usefulness. To accomplish the assessment, Souder used a decision analysis technique very similar to the one suggested by Williams. Twenty-six R&D administrators/managers were surveyed to determine what properties of R&D selection models are crucial in terms of their usefulness, and the relative importance of each property. The survey indicated that five properties were considered crucial: realism (most important), flexibility, capability, use, and cost (Table 2.1). To measure these five properties, a set of more specific characteristics was
The five criteria and their characteristics

<table>
<thead>
<tr>
<th>Realism criterion Characteristics</th>
<th>Capability criterion Characteristics</th>
<th>Flexibility criterion Characteristics</th>
<th>Use criterion Characteristics</th>
<th>Cost criterion Characteristics</th>
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</thead>
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<tr>
<td>Model includes</td>
<td>Model performs</td>
<td>Model applicable to</td>
<td>Model is characterised by</td>
<td>Model has</td>
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<td>Discrete variables</td>
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<td>Market risk parameter</td>
<td>Simulation analyses</td>
<td>Priority decisions</td>
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<td>Technical risk parameter</td>
<td>Scheduling analyses</td>
<td>Termination decisions</td>
<td>Special persons not needed</td>
<td>Low data collection costs</td>
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<td>Initiation decisions</td>
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<td>Special interpretations not needed</td>
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<td>Facility limits parameter</td>
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<td>Budget limits parameter</td>
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<td>Easily obtainable data</td>
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<td>Premises uncertainty parameter</td>
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Table 2.1: Souder's criteria and characteristic of an ideal model
developed. These characteristics are more measurable and can be used as surrogates in the scoring system.

Souder’s scoring method yields some useful insights into what R&D managers are seeking in selection models. However, the characteristics he uses to measure the five properties limit the approach to an analysis of only computer-based or extremely formal models. Simpler analytical approaches would not receive fair treatment. Nonetheless, the more general properties identified by Souder are important and were kept in mind as we developed our R&D project selection approach.

(c) Baker’s article (1974) [5] focuses on the practical application of R&D selection models. He concludes that, although there are numerous models developed, most have not been empirically verified and are not used by R&D managers. Baker discusses some of the more specific problems that face the models. One of the most important is that R&D decisions are made in a hierarchical manner. Each level of management makes budget allocation decisions or subject matter decisions at a different level of aggregation. Given Baker’s observation, we believe an R&D selection model must be amenable to hierarchical decisions to be useful.

Baker also points out that R&D project decisions are made more frequently than once a year. They are made continuously, as new ideas are proposed. A selection methodology must be capable of viewing these new ideas as increments in an existing R&D project portfolio.

Another important criticism raised by Baker concerns the treatment of uncertainty: three types are discussed. The first, technical uncertainty, is the risk that the product, process, or device will not work. The second is commercial uncertainty, the risk that the product cannot be economically produced on a commercial scale. The third is economic uncertainty, the risk that after it is introduced the product will not yield economic value to the firm. Baker’s article implies that explicit incorporation of each of these three elements of uncertainty into R&D project selection models would be a useful improvement.

(d) Cooper (1978) [6] reinforces many of Baker’s conclusions. Cooper identifies three criteria for selecting research projects: impact, feasibility, and intrinsic scientific merit. Using a weighting system similar to William's, Cooper develops a simple R&D project selection model
based on those three criteria. The impact criterion is measured by the size of the potential market for the product. Feasibility is measured by the probability of achieving certain sales and production cost results. In a manner similar to Baker’s, Cooper mentions three types of risks associated with project feasibility: technological risk, technical competence of the research performer, and management capability to accomplish the task. Technological risk defined by Cooper is the same as technical risk defined by Baker. The other two types of risk are quite different from Baker’s categories. Part of this difference is explained by Cooper’s focus on government research laboratories, as opposed to Baker’s emphasis on the private sector.

The final criterion for successful selection mentioned by Cooper is the intrinsic scientific merit of the project. The key question is whether the research contributes to a basic understanding of the phenomena. Cooper argues that this criterion is important because research on the frontier of science keeps highly competent people at the facility and maintains high-quality technical research output. He argues that positive feedback keeps the research quality at a high level. Conducting research at the fringe of current knowledge attracts the best technical people and these people produce the highest quality work. High quality work attracts more competent people and motivates the existing R&D staff. This phenomenon yields long-term benefits to the company and stimulates all research conducted in the lab.

This last of Cooper’s criteria raises an interesting set of behavioral issues about R&D project selection which most authors ignore. His project selection model incorporates this last criterion and the two previous criteria (impact and feasibility) through a simple scoring system. R&D projects are given a ranking for each of the criteria. These rankings are then added to produce a single score for each project. Cooper’s treatment of scientific merit and his simple approach to dealing with multiple selection criteria provide additional building blocks for our approach.

(e) Marvin Cetron (1969) [7] attempts, among other things, to review quantitative R&D selection models and develop an improved approach. After an extensive review of existing R&D models, Cetron identifies numerous features that can be used to describe and differentiate between existing models.
As expected, none of the existing R&D selection models deals specifically with all the features identified by Cetron. Approaches that possess many of these features will have to be large and complex. If alternative R&D selection models are evaluated with Cetron’s criteria, complex, computer-based models have a strong advantage. Cetron used the results of his review to derive his own R&D project selection model in which complexity is an important element.

Cetron proposed the three analyses (a corporate policy review, technology forecasts, and a review of the competitive environment) as a part of the “system analysis”. The overview of the “system” leads to identifying the needs of the company. The company’s needs are next compared to the firm’s available resources (including its present capabilities and likely future products) to identify any deficiencies. Clarification of the firm’s deficiencies lead to a clear set of operational objectives (or corporate goals). The corporate goals must then be translated into a set of specific technical objectives that need to be met. Further details concerning needed technical achievements are identified through technology assessments. Finally, technical objectives are used to formulate an R&D program for the company.

The major drawback of Cetron’s approach is that it appears more rigorous than the supporting data deserve. These models attempt to use computer-based decision analysis techniques to evaluate the extent to which military R&D projects fulfill numerous national security objectives. They represent the opposite extreme from the simple scoring approaches of Cooper and Baker.

Cetron’s approach does have some strong points: It clearly puts the R&D project selection in a broader corporate context. His approach is also highly organized and does not leave out any relevant factors. The breadth of treatment adapted by Cetron provides another key ingredient to an improved selection process.

Edward Roberts (1969) [8] identifies five major problems with existing R&D selection models. First, very sophisticated R&D project selection techniques (such as the one derived by Cetron) are linked to the outcomes of trivial forecasting exercises. Second, these sophisticated models are costly to operate. Third, the dubious nature of the forecasts used as inputs tend to place the outputs in question. Fourth, the models are
inflexible in terms of what types of projects they evaluate and how the evaluations are conducted. Fifth, the most troubling, they are not used in managerial decisions. Robert's solution to these problems does not call for a simpler approach to the R&D project selection process. Rather, he believes that a better approach is to construct a more complicated model. The major problems identified by Roberts must be addressed by our R&D selection model even if we do not agree with his suggested solution.

(g) According to Schmidt and James [9] the literature on project selection models can be divided into two major approaches that can be broadly characterised as the traditional “decision-event” approach and the more recent “decision-process” or “systems” approach. Classical or traditional models focus on outcomes. Given a set of projects the models determine the subset that maximises an objective. These models focus on a decision that is made at a particular organisational level at a particular point in time. They assumed fixed criteria and alternatives and have no mechanism for altering the problems within the planning cycle. The philosophy that underlies classical models is that the outcome is more important than the means by which the result is obtained. As a result, these models are concerned only with outcomes and have generally ignored the process by which project are selected in real organisations.

The new systems-oriented philosophy rests on the fact that projects are selected by organisations and recognises the value of the organisational process. It focuses on decision processes rather than outcomes. The objective of the systems approach is to gain insight regarding general managerial policies rather than to provide answers to specific selection problems. The systems approach represents a fundamental shift in emphasis from “decision event” toward “decision processes.”

(h) D Hall and A Nauda [10] have classified various techniques into four category: Mathematical Programming, Benefit Measurement Methods, Cognitive Emulation Models, Ad Hoc Methods.

(i) Stuart Bretschneider [11] has critically reviewed the techniques and methods from operation research which have been applied to evaluation of R&D projects. According to him focussing on project evaluation requires an important distinction to be made between ex ante and ex post project evaluation. Ex ante evaluation occurs prior to initiation of the research or development activity. The principal utility of ex ante is in selecting project.
Operation research major contribution to R&D management had been through ex ante project evaluation and application of ex ante evaluation to formal models, designed to support project selection decision making. Ex post evaluation occurs after the project has been completed and therefore focuses on outcomes and impacts. He has classified different models into following categories, Comparative project models, Scoring models, Analytic Hierarchy Procedure, Benefit contribution models, Mathematical programming, Cognitive emulation, Ad Hoc models, Evaluation function, Cause and focus, Decision making.

These categories are extension of Hall and Nauda [10]. He has further given the criteria for assessing operational research project selection models. Table 2.2 gives the summary of these criteria. This is basically amended from Souder criteria by addition of some more criteria to make models more usable for real life applications.

Table 2.3 shows the hierarchy of categories of selection methods, as classified by Stuart Bretschneider.

(j) According to Joseph Martino[12], the methods used for R&D project selection can be categorised as follows ranking, economic decision theory (single and multi-stage), portfolio optimisation, cognitive modelling and ad hoc decision methods. Each of these general categories includes several methods.

(k) J. Lee Sangjin and Zong-Tee [13] have summarised the categorization of models by different researchers, as shown in Table 2.4.

2.3.4 BRIEF DESCRIPTION OF SOME OF THE PROJECT SELECTION MODELS GIVEN IN THE LITERATURE

(a) Profile Models

In the profile models the project proposals are compared on the basis of a subjective evaluation of their attributes. These evaluations could be done by one individual or by group consensus. Alternatively, the profiles developed by several informed individuals could be compared.
<table>
<thead>
<tr>
<th>REALISM CRITERION</th>
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<td>Premises uncertainty</td>
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<td>* Multiple time periods</td>
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<tr>
<td>* Hierarchical structure</td>
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<td>* Competitor efforts</td>
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<table>
<thead>
<tr>
<th>FLEXIBILITY CRITERION</th>
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<tr>
<td>Model applicable to:</td>
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<td>*Termination decisions</td>
<td>Special person not needed</td>
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<td>*Budget allocation</td>
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<td>*Project funding</td>
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<td>Easy to obtain</td>
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<th>*ADDITIONAL CRITERION</th>
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<td>Strategic needs</td>
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<td>Low computer costs</td>
<td>Project interdependencies</td>
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<td>Low data collection costs</td>
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* AMENDED CRITERIA


*Table 2.2: S. Bretschneider's criteria and characteristics of an ideal model*
Chapter 2

BENEFIT MEASUREMENT VALUATION MODELS

COMPARATIVE APPROACHES

Q-Sort
  W.E. Souder (1978)

Ordinal Ranking
  W.E. Cook (1962)

Normative Model
  A.B. Nutt (1965)

Paired Comparisons
  W.E. Souder (1975)

Interactive Group
  A. Van de Ven (1971)
  L. Plebani (1981)

Scoring Models

Multiple Criteria
  C.M. Mottley (1959)
  G.R. Gargulio (1961)
  B.V. Dean (1962)
  W.H. Pound (1964)
  B.V. Dean (1965)
  J.R. Moore (1969)
  D. Gustafson (1971)
  D.H. McGuire (1973)
  A. Paolini (1977)
  M.J. Cooper (1978)
  W.R. Stumpe (1979)
  M.R. Johnston (1988)
  D.J. Williams (1969)
  D.A. Aaker (1978)
  D. Costello (1983)

Integer Programming
  L.D. Watters (1967)
  W.D. Cook (1962)

Linear Programming
  D.I. Asher (1962)
  A. Beged-Dov (1965)
  W.E. Souder (1973)
  A.E. Gear (1974)
  A. Paolini (1977)
  A. Gear (1971)

Goal Programming
  A. Charnes (1966)
  A.J. Keown (1979)
  E.P. Winkofsky (1981)
  W. Hespos (1985)

Dynamic Programming
  S.W. Hess (1962)
  Rosen (1965)
  Atkinson (1969)
  Kepler (1973)
  Aldrich (1975)

Decision Tree Diagrams
  R.E. Hespos (1965)
  A.G. Lockett (1970)
  A.G. Lockett (1974)

Decision Process Model
  E. Winkofsky (1981)
  T. Mandakovic (1985)

Expert Systems
  G.R. Madey (1985)
  A. göre (1986)

System Approaches
  D.J. Williams (1969)
  D.A. Aaker (1978)
  D. Costello (1983)

Risk Analysis
  D.B. Hertz (1964)
  R.F. Hespos (1965)
  P.M. Maher (1974)
  S.L. Schwartz (1977)


Table 2.3: Summary of R&D project selection models (1959-1990)

Page 21
<table>
<thead>
<tr>
<th>Authors</th>
<th>Categorization of models</th>
</tr>
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<tbody>
<tr>
<td>Souder (56,57)</td>
<td>Screening models</td>
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<td>Treating them like capital investment</td>
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<td>Examples of specific models</td>
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Source: JINJOO LEE, SANGJIN AND ZONG-TAE BAE, IEEE TRAN. ENGG. MGT., VOL.EM-33, NO.3, AUG 1986

Table. 24: Categorisation of project selection models by different researchers
Profile models are simple and easy to use. They display the project characteristics and ratings in such a way that they are easily communicated and readily visualized. On the other hand, a profile model does not tell us anything about the trade-offs among the criteria. The fact that it is left up to the decision maker to evaluate this trade-off suggests that profile models are best used as screening devices or as stimulants to further inquiry and dialogue.

Profile models are useful where there is a minimum amount of information available, or where the projects are incompletely understood. For instance, profile models may be the most suitable methods that can be used for evaluating and comparing exploratory research projects. However, because the evaluations are totally subjective, the rating scale points must be carefully defined. “High”, “Medium” and other ratings must mean the same to all the evaluators.

(b) Checklists

This type of model assumes that the decision maker can distinguish between several finite levels of the criteria or requirements. Each candidate proposal or project is then subjectively evaluated by the decision maker and assigned a criterion score on each requirement. The criterion score is ascertained from a predesignated scoring scale that translates subjective evaluations into numerical scores. A total score is obtained for each project by summing its criterion scores.

Checklist models improve on profile models by providing both a graphic profile of checkmarks and an overall total score for each candidate project. An analysis of target achievements and a comparison of several candidate projects is facilitated by the total scores. On the other hand, the criterion scores may not properly reflect the trade-offs in the requirements. Then a simple summation of the criterion scores is inaccurate. If the decision maker wants to compare candidates on the basis of their total scores it is better to use a scoring model.
(c) Scoring Models

It is a short step from checklist models to scoring models. In a scoring model each of the candidate projects are scored on each of the performance requirements of criteria. The criterion scores for each project are then combined with their respective criterion importance weights to achieve a total score for each project. Projects may then be ranked according to their total score.

In constructing a scoring model, the criteria or performance dimensions should be selected so that they represent independent and mutually exclusive dimensions. In practice, this ideal is not always possible; criteria will almost always overlap or be interdependent to some degree. The criteria weights should reflect the relative importance of the criteria, and they may be ascertained by using the value methods. In most cases, the weights will be ranks or ordinal level measurements. That is, they are orders of magnitude only. The form of a scoring model can usually be varied to suit the objectives of most decision makers and most situations. The criteria and weights can be varied to reflect different goals and preferences. In general, scoring models are natural to construct and use. They permit the decision maker to examine the performance of different projects on several criteria as a basis for decision making. They provide a moderately quick analysis of the relevant decision aspects, without a great sacrifice in accuracy. The use of scores in place of the actual data may in fact absorb some of the random errors that are inherent in the actual data. For instance, errors often abound in cost measurement, and scores may provide just as much real discrimination as the actual error-laden numbers.

(d) Frontier Models

In these models the projects are plotted in such a way as to show their relative risks and returns. “Risk” expresses the project’s chances of failure. This may be measured as 1-p where p is the project’s probability of success, or it may be measured in terms of the likelihood that the project will not achieve some desired level of output, profit, etc. “Return” expresses the project’s anticipated profits, sales or some other measure of value which the decision
maker wishes to use. The efficient frontier tracks the path of the most efficient return/risk ratios.

Frontier models are often very useful for examining return/risk trade-offs within the organizational objectives. Frontier models may be used to indicate the need for greater diversification in idea generation and project proposals. However, whether or not the portfolio ought to be more diversified must be resolved on the basis of the organization's goals and objectives. The frontier model can only point out trends and situations for further analysis.

e) Pairwise comparisons

The starting point of pairwise comparisons is for each project to be compared with every other project. If there are N projects, this require N*(N-1)/2 comparisons. If there are M criteria and N projects, this requires M*N*(N-1)/2 comparisons. Once the comparison is made, the two most common methods for converting them into rankings are the dominance count method and the anchored scale method.

(f) Economic Index Models

An index model is simply a ratio between two variables, and the index is their quotient. Changing the values of the variables changes the value of their quotient, or the index. An example of a commonly used index model is the return on investment (ROI) index model. The single-number index or score that is produced by an index model can be used to rate and rank candidate projects. The following are some of the commonly used economic indices.

(i) Net Present Value:

NPV is often used on capital budgeting. The basic idea is that a rupee now is worth more than a rupee at some time in the future, because the rupee now can earn money in the interim. By computing NPV we can make comparisons possible between early and late values in the same cash flow stream, for instance how much profit worth 10 years from
now, in comparisons with an R&D expenditure now on the project, which will lead to the profit. Moreover, it also makes comparisons possible between cash flow streams that have different profiles of income and expenditures. By computing the NPV of each cash flow stream, it is possible to convert them to a single value and compare them. NPV methods allow comparison of R&D projects with each other, and with alternative investment, such as capital equipment or advertising. However, the method has several shortcomings. It requires data about future costs and revenues that may not be possible to obtain early in the life cycle of the project. It also assumes the discount rate is constant rate over time, which may not be true.

(ii) Internal Rate Return:

IRR is the discount rate that would reduce the NPV of a cash flow profile to zero i.e., it is the interest rate one would have to pay on borrowed money to make the NPV of the cash flow zero. There is no formula for computing IRR directly, it must be computed iteratively, by assuming a discount rate, computing NPV, and adjusting the assumed discount rate until the computed NPV is zero. In terms of project selection, the greater the IRR, the better the project. IRR is widely used for choosing among not only R&D projects but other types of capital investments. One significant advantage that it has is that the analyst need not estimate the future interest rate, as it required with NPV. But it has significant shortcoming. A project with high IRR may have a smaller NPV, and actually may be of less value to the company than a project with greater NPV but lower IRR. When comparing R&D projects with other capital projects, NPV may be a more useful method.

(iii) Cash Flow Payback:

This measure is the time from start of the project until net cash flow becomes positive, that is, the project pays back all the funds invested in it to that point. This method does not take into account the time value of money, nor any of the
other reasons for preferring cash flow in one period to cash flow in another. Payback time requires estimates of future income from the project, which is especially difficult to obtain, especially for projects in the basic research stages. In principle, project that payback sooner represent less risk than projects that reaches payback later, even though the latter may have higher NPV or IRR. Hence, despite its lack of sensitivity to overall project size and return, it is often used by risk-averse decision makers to reduce the risk inherent in basing decisions on estimates of future conditions.

(iv) Expected Value:

NPV, IRR, cash flow payback assume a single cash flow profile from start to end of the project. In many cases there may be two or more alternative profiles. While using the expected value requires more data than is required for the more usual ways of dealing with cash flow profiles, it presents a more realistic view of affairs, since it takes into account alternatives’ possibilities.

One weakness of the economic index models is the implicit trade-offs that often occur. All index models should be carefully examined for their internal trade-offs. Unless the trade-offs are representative of those the decision maker would actually be willing to make, the model is inappropriate. Another weakness of many index models is the insensitivity of the index to changes in some of the parameters. Still another weakness of index model lies in their inability to consider multiple objectives. Because of this, an index model may be inadequate.

Index models are appealing because of their simplicity and ease of use. That is, they are attractive because they do not include everything. But the decision maker should vary; index models can be deceivingly appealing. Before placing great faith in the outputs from an index model, the decision
maker should make sure that the model is unbiased and appropriate.

(g) Decision Theory Models

Decision theory models are based on the economics and strategic approaches to statistical decision making. These models are based on the idea that a rational decision maker will only adopt those policies which maximize the expected value of the outcomes. For two candidate projects a payoff matrix is constructed by arraying the data for the candidates under the assumptions that mother nature is either adverse or benign. The relative worth numbers in the payoff matrix are anticipated profits. Other kinds of worth numbers can also be used i.e., present worths, index numbers from an index model, etc. The project with the larger expected worth would be the preferred candidate.

Decision theory models may help to clarify the characteristics and to define the implications of the available decision strategies. Decision theory models may thus be particularly useful for interdepartmental decision making, where natural differences in the risk-taking propensities of the departments may get in the way of consensus.

(h) Value- Contribution Models

Value-contribution models are a combination of several methods. Value-contribution models permit the decision maker to examine the degree of contribution which a project makes to the organization’s hierarchy of goals. Thus, to develop a V-C model, one must first list the goals. In most cases, the goals may be listed as a nested hierarchy.

The second step in developing a V-C model is value weighting the goals, such that these values must sum to 100. The complete set of value-weights thus indicates the “perfect” level of value contribution which a project could make. Thus, a project with a perfect contribution to all the goals would have a total value-contribution score of 100 points.
The actual scaling and scoring of the candidate projects within a V-C model can be done individually or by consensus. V-C models permit the decision maker to think in terms of the goals of the candidate projects, and the level of goal achievement. V-C models may also be useful when the decision maker is trying to assemble a balanced portfolio of several projects.

(i) Analytic Hierarchy Procedure (AHP)

AHP, developed by Saaty[14] is a methodology that helps the decision maker(s) to prioritize and rank his (their) objectives or alternatives in an unstructured and complex environment. To solve a complex problem, this method breaks down the problem into its components and arranges them hierarchically. Using the value judgements assigned to components, AHP then determines which components of the problem have the highest priority and need immediate action.

According to Saaty, AHP “enables us to cope with the intuitive, the rational, and the irrational, all at the same time, when we make multicriteria and multiactor decisions.

(j) Fuzzy model

Fuzzy sets have been suggested for handling the imprecision of real world situations by using truth values between the usual “true” and “false.” The concept of fuzzy set and membership degree were introduced in 1965 by Zadeh[15] in order to provide a possible model for inexact concept and subjective judgement. This model was intended to be used in situations when deterministic and/or probabilistic models do not provide a realistic description to the phenomena under study. Indeed, in a large area of situations, such as pattern recognition, decision-making, large-systems control, management problems and other human judgement is often imprecise and the decision-maker no more manipulate numbers, but vague concepts. The fuzzy set provides an intuitively pleasing method of representing one form of uncertainty.

As stated above, the difficulties in stating and solving the R&D project selection stem from the two major sources of uncertainty.
The uncertainty related to its random nature has been dealt with number of probabilistic model that enable the decision maker to deal with non deterministic benefits, costs and completion times of the projects. But second kind of uncertainty, which is related with lack of an objective scale to measure some variables, parameters and relationship that define the R&D environment has not been incorporated in the models. The most relevant and successful effort in this direction was the use of scoring models that introduced the idea of assessing the project benefits through scores. Although those models are suitable for ranking alternatives they are not appropriate to deal with the project selection problem with constraint in which the alternatives are not known in advance.

Fuzzy project selection problem arises when the numerical coefficients of the project selection are replaced by linguistic values like ‘high’, ‘low’, etc., taken from a suitable vocabulary. With fuzzy sets we are able to tell the decision maker how ‘good’ the alternative is, but does not provide an ‘optimal’ solution.

2.3.5 RISK ANALYSIS MODELS

2.3.5.1 DEFINITION OF RISK ANALYSIS

Numerous attempts have been made to formally define the risk analysis. Using the definitions given by the dictionaries, Jan Berry and Paul RF Townsend [16] evolved the following definition.

"Risk analysis is the process of resolution of anything complex into its simple elements which would become exposed to chance".

Risk analysis is primarily concerned with evaluating the uncertainties which are seen to affect the outcome of a planned or an ongoing work. It draws attention to uncertainties which are not immediately apparent. The major consideration should be the unveiling of risks and their causes. Such evaluations require careful analyses of the many parts which constitute programmes of work, both at their inception and throughout their lifecycle.

Risk analysis models should allow the translation and quantification of perceived risks and uncertainties into planning and work decisions, as well
as in economic terms. The outcome should reflect the possible scenarios of the future and its uncertainties.

2.3.5.2 ORIGINS OF RISK ANALYSIS

Probably the earliest industrial use of risk methods was with PERT/RISK, which originally referred to the variation of the estimates of the activity duration, and, assuming their independence, was used to calculate the probable variations of a project duration. The most frequently stated project-assessment methods use the probable variations of a project duration, and the beta distribution, which is frequently supplemented by the rectangular, triangular and normal probability distributions. For instance, Cooper et al. [17] use the above distributions, but acknowledge the existence of interdependence, which is lacking in PERT/RISK. The need to account for time interdependence, as exemplified by network analysis. Can be overcome by simulation; a good example is shown by Pugh and Sodden [18]. Their methodology replaces the beta distribution by a combined normal distribution. This allows a simpler user input without losing the essential property of skewness.

Other techniques fall into two main areas: sensitivity analysis and decision trees. Perry [19] combines the results with decision-tree methods and simulation. The latter approach was used primarily for construction projects. For wider use, Neuburger [20] advocated decision-tree methods as a broad-brush approach. Baker [21] specifies the need for rapid answers and easy-to-use computer methodology to allow interactive modeling as it is supported by available data to improve the accuracy of the generated probability distribution.

2.3.5.2 SELECTED RISK-ASSESSMENT METHODS

Risk assessment is largely dependent on the expert knowledge of the planner or manager. It is their information which can be supplemented by risk-analysis devices.

The following are some of the commonly used risk analysis methods.
(i) Subjective estimates

Most of the practicing managers estimate risk by intuition and use the subjective estimates of specialists in the concerned area. These subjective estimates are quantified either in the form of a probability (probability of success etc.) or in the form of a risk rank. Probability values are used as multiplication factors for the return on investment (NPV etc.) to obtain the expected value for a given project and the projects are selected based on this expected value. In the case of risk ranking, it is directly used for ranking of the projects.

(ii) What-if analysis:

This is also known as sensitivity-analysis. This allows the early modification and creation of alternative plans which can be compared with each other. The comparisons allow most financial-analysis techniques, such as discounting, to be easily analysed.

(iii) Early-warning systems based on detailed analysis:

This technique is concerned with the detailed activities which tend to become masked when a project is considered as a whole. By the use of statistical techniques, this facility identifies those specific activities which on a period-by-period analysis, exert most influence on the different stages of a project. It thus tends to identify those activities which have not been resolved into the appropriate level of detail. This information guides the decisions governing the generation of plan alternatives.

(iv) Simulation based risk analysis:

This is a Monte Carlo simulation based analysis method. The techniques is used to account for major uncertainties within a project plan which are of sufficient magnitude to be considered as distinct factors which can jeopardize the validity of the plan itself, but are not included as normal variations, and factors which are not normally included in the project plan, but whose potential occurrence could upset the assumptions of the plan, and intrude upon the smooth execution of the plan. The methodology can be considered
as an in-depth expansion of the sensitivity-analysis tools currently available.

The most recent quantitative techniques combines both simulation and sensitivity analysis, although it does not ignore decision analysis, as this is incorporated in the prioritization process. The multidimensionality and means of combining many diverse risks allows for the requirements of sensitivity analysis.

Simulation methods to date have suffered from excessive detail, their lack of concern for external effects, and their general limitation of one dimension being extrapolated to others. A risk analysis model provides a complete picture of the distribution of outcomes for each alternative project. The risk analysis approach makes the risk-averter and gambler strategies more visible, thereby permitting a decision maker to consciously select decisions consistent with one of these chosen strategies.

2.3.6 PORTFOLIO ANALYSIS MODELS

The project selection methods described above rank, rate, or otherwise evaluate individually, without taking into account potential interactions among the projects, or alternative uses for the resources devoted to the projects. The object of the portfolio optimisation methods is to select, from the list of candidate projects, the set that provides maximum payoff to the firm. Portfolio optimisation methods take into account resource dependencies, budget constraints, technical interactions, market interactions, and program considerations. The main difficulty in using these techniques is that much effort is needed to supply all of the data for the analysis; the data may not be available or may be of questionable validity. The following are the four portfolio analysis methods mentioned in the literature:

2.3.6.1 MATHEMATICAL PROGRAMMING:

Mathematical programming refers to class of techniques for selecting a set of entities out of some larger collection, such that the selected set maximises some objective function, subject to a set of constraints. Since the early 1960's a number of mathematical programming models have been developed to address the R&D project selection. This includes
linear programming, integer programs and more recently, multiattribute models.

2.3.6.2 SENSITIVITY ANALYSIS:

Any portfolio optimisation scheme depends on the value supplied for payoffs, costs, probabilities, and so on. Changes in these will clearly change the optimum portfolio. Sensitivity analysis involves changing one or more of the values and rerunning the procedure for the optimum portfolio. It will serve two purposes. The first is that it determines how robust the optimum is. If minor changes in a few values alter the optimum portfolio significantly, then the solution is highly sensitive to these values. Conversely, if modest changes in the value do not change the portfolio significantly, then the portfolio is robust. The second purpose is to determine whether changes in one or more of the variables would make significant differences in the payoff.

2.3.6.3 SIMULATION:

Simulation is used when projects in a portfolio have alternative outcomes to which probabilities can be attached, when the projects have alternative paths to the end goal depending on the chance outcome, and when the projects have different payoffs for the different outcomes. The projects are simulated by drawing random numbers to determine outcomes and payoffs. This is done sufficiently large number of times to assure statistically valid results. The result is an estimate of the probability of different outcomes.

2.3.6.4 CLUSTER ANALYSIS:

The method of cluster analysis does not provide directly rank projects, or relate them to measure of payoff or income. Instead, it identifies groups or clusters of projects that are related or are “similar” in some sense. Having identified clusters of “similar” projects, the decision maker then approves the projects in those clusters that support the most important objective of the organisation. In principle, clusters would be ranked from most important objectives to least important, based on their support for the organisation’s strategic objectives.
2.4 CONCURRENT ENGINEERING MODELS

2.4.1 BACKGROUND

Conventionally, a series of sequential steps are followed to design the product, identify the processes, machine the parts, assemble the components, and ship the products to the marketplace. Product designers are mainly concerned about their products’ performance and functionality and rarely take process design and/or manufacturing’s constraints into consideration. This traditional sequential path has not entailed the dialogue between design and the downstream processes except through a series of standard engineering change orders.

However, it has been recognized that design decisions made early in the product development cycle can have a significant effect on the manufacturability, quality, product cost, product introduction time, and thus on the ultimate marketplace success of the product. Furthermore, it is believed that the corrective cost of engineering change orders increases logarithmically as the orders are placed later in the product’s life cycle. This means that the product designer must include manufacturing considerations as early as possible along with the structural, functional, and aesthetic requirements. In other words, those implications must be designed in rather than inspected in to avoid the costly iterative process.

2.4.2 DEFINITION OF CONCURRENT ENGINEERING

Concurrent Engineering is defined as "A systematic approach to the integrated, concurrent design of products and their related processes, including manufacture and support".

2.4.3 OBJECTIVES OF CONCURRENT ENGINEERING

The main objective of Concurrent Engineering are:

a) Reduce cycle time (Faster)
b) Reduce product cost (Cheeper)
c) Improve product quality (Better)
2.4.4 ALIASES OF CONCURRENT ENGINEERING

Many terms have been used to describe similar approaches, including simultaneous engineering, life-cycle engineering, design integrated manufacturing, design fusion, early manufacturing involvement, parallel engineering, concurrent design, design fusion and design in the large. In Europe, design for production and design for economic manufacture (DEM) have been used instead. Some process-oriented technological terms have been also used to describe the functional concepts of CE. They include design for manufacturability (DFM), design for producibility, design for assemblability (DFA), design for testability, design for reliability, design for installability, design for serviceability, and so on.

2.4.5 ELEMENTS OF CONCURRENT ENGINEERING

The main elements of Concurrent Engineering are:

a) Identification and analysis of customer requirements and expectations
b) Integration of functions and disciplines
c) Coordination of processes for product design and development, manufacturing, and support
d) Teamwork
e) Integration of enterprise data, information, knowledge, and application programs

2.4.6 MODELS AND APPROACHES TO CONCURRENT ENGINEERING IMPLEMENTATION

There may be two basic approaches to implementing the concurrent engineering practice: team-based and computer-based approaches. The former approach is human-oriented in that the team consists of designers and individuals from all other related functional areas. Team members are selected for their ability to contribute to the design of product and processes by early identification of potential problems and timely initiation of actions to avoid a series of costly reworks. The multifunctional team is crucial for effective implementation. Continuously developed computer technologies, in both hardware and software, have given team members from different departments the ability to work with the same design to evaluate the effects of design attributes. In this context, it is believed that a
significant educational program is a must to have each team member fully understand the philosophy of CE. Many corporate organizations have already initiated the educational programs for a philosophical shift to CE.

Numerous papers have reported case studies in which the team-based approaches were implemented and significant benefits were realized. While the team-based approach can be readily implemented and is being widely adopted in industry, some shortcomings appear to arise: difficulties in effective management of the team, team member’s limited knowledge, and the cost of maintaining a team. As more sophisticated computer tools emerge constantly, the team-based approach is being enhanced by the computer-based approach in which the concurrent engineering philosophy is woven into the internal logic operations, enabling design justification or optimization with respect to the entire aspects of a product’s life-cycle.

However, the implementation of concurrent engineering practices are still in their infancy and no standard models can be found to be universally acceptable. The Indian guided missile development programme adopted the concurrent engineering in the form of a development-production concurrency. The various methodologies developed during this process are described in chapter 5.

2.4.7 FUTURE TRENDS

Concurrent Engineering has a key role to play in the computer integrated manufacturing systems (CIMS) and the extent to which the CE principles are applied will be the major deciding factor of the competitiveness of a nation’s industries. However, full implementation of CE is still far from reality.

The implementation of CE principles will utilise new hardware as it becomes available. For instance, sensor-equipped intelligent robots will emerge and be used for fully automated assembly. This will be able to relax the constraints imposed on the product design from the standpoint of assemblability, enabling the product to be made with more power and sophistication in a CE environment.
2.5 PROJECT PLANNING & MONITORING MODELS

Project planning and monitoring has been the subject of numerous articles and attempts have been made continuously to refine the existing models. The best summary of the planning and monitoring tools for R&D has been made by A.W. Pearson (1983)[22], in article titled “The planning and monitoring in research and development - a 12 year review of papers in R&D management”. Based on this and other numerous papers available on this subject the planning and monitoring tools can be summarised as follows:

a) GANTT chart: this is a very familiar chart and this provides a visual representation of planned and actual status of the project at any given point in time. However this cannot represent precedent between various activities and also do not consider cost and other aspects. This tool is not suitable for R&D projects.

b) Critical Path Method (CPM)[23]: This is an activity oriented network analysis technique with deterministic time estimates. This tool is primarily developed for construction type of projects and focus of the tool is on time-cost trade offs and provides for crashing/stretching of various activities to meet the overall project time schedules and budget constraints. This tool is not suitable for R&D projects.

c) Programme evaluation and review technique (PERT)[23]: This is an event oriented network analysis technique based on a three time estimates. This is the first technique developed for R&D projects during the polaris missile development programme by USA. The initial focus of the PERT was to find out the development time within given probability estimate using the beta distribution for time estimates. The technique was later on expanded to include the cost element. PERT is the most used tool for planning of all types of projects as on now. However, this tool cannot represent the real life iterative situations of R&D projects as it does not allow the loopings in the network.
d) Precedence diagrams: These are basically variants of PERT with the relationships between the preceding and succeeding activities enlarged to include start to start, start to finish, finish to start and finish to finish, lead/lag situations.

e) Decision CPM[23]: This is an attempt made to incorporate the probabilistic behavior into the networks by introducing the probabilistic notes and associated probabilities. However these tools require complex computations and hence are not popular.

f) Research planning diagram(RPD)[24]: This is also a tool aimed at introducing the probabilistic behavior into the networks. However, this tool resembles a computer flow diagram and appeals to the user with its simplicity. This tool has been used for R&D projects with limited success.

g) Graphic evaluation review technique(GERT)[25]: This is a major improvement over the PERT. Where a large variety of probabilistic situations have been introduced and both time as well as cost has been introduced into the network computations. This has been an important tool for researchers. However, due to its complexity and the requirement for computer based software limited its use to the USA alone.

h) Venture evaluation review technique (VERT)[25]: This is further improvement over PERT and uses three parameters namely the time, cost and performance. This tool provides for large types of probability distributions at each node and attempts to model the real life projects in a close manner. However, this tool has not found much acceptance among the practicing managers due to its complexity.

i) Slip chart[23]: This is simple chart specifically designed to highlight the schedule slippages of activities and is suitable for microlevel schedule monitoring of projects. This tool is reported to have been used successfully in some of the R&D projects.

j) SSD graph[26]: This is also similar to the slip chart and used for schedule monitoring. However, it has built in property to store
the history of the project progress and hence is more useful for analysis purpose.

k) Earned value analysis[27]: This is a concept introduced by the US DOD's C/SCSC in the late 1960s addresses the same problem of measurement of progress compared to the consumption of the resources. The earned value analysis uses three elements (the budgeted cost of work scheduled (BCWS), the actual cost of work performed (ACWP), and the budgeted cost of work performed (BCWP) or the earned value). The cost variance is given in BCWP-ACWP which gives a true variance in cost. But the schedule variance is given by BCWS-BCWP which is an interpretation of the schedule variance in terms of cost, which roughly means that a behind schedule condition requires a variance amount of cost to get back to schedule. This is still the most used technique available however, the interpretation of schedule variance also in terms of the cost variance creates confusion and hence this technique needs improvement.

l) PACE[28]: This is a technique developed by the researcher for progressing project in a multi-project environment. PACE focuses at the resolution of priority and resource clashes between projects. PACE has been successfully used in the Indian guided missile programme. This technique is described in detail in Chapter 4.

m) PACT[29]: This is also a technique developed by the researcher for the Indian guided missile development programme. This is an improvement over the earned value analysis and the technique is described in detail in chapter 5.

2.6 SUMMARY OF ISSUES BASED ON LITERATURE SURVEY

2.6.1 EVOLUTION OF R&D PROJECT SELECTION MODELS

Over the years hundreds of models have been evolved for R&D project selection but no model has been universally accepted for application to the real life problems. However, continuous attempts have been made to
improve the models by including factors that represent the real life problems. Numerous researchers contributed for the progress in this direction. Great contributions have been made by W.E. Souder, N.R. Baker, M.J. Cetron, E.B. Roberts, D.J. William, Robert L Schmidt, James R Freeland, David L Hall, Alexander Nauda, Stuart Bretschneider and Joseph P Martino. Among them the major contribution towards making models applicable for the real life problems came from W.E. Souder, Stuart Bretschneider and Joseph P Martino. The increase in the applicability of the models for the real life problems may be notionally represented as shown in Fig.2.3. As shown in the figure, by 1960's & 70's hundreds of models were evolved, which have grown many times by 1980's. The net result is a confusion as to which model is suitable for what type of applications. This resulted in a search for ideal model. Souder[4] was the first to give the criteria for evaluation of R&D selection models (Table.2.1). Stuart Bretschneider[11] improved this criteria by adding more factors (Table.2.2). Joseph Martino[10] made an attempt to enumerate the factors comprehensively. Table.2.5 shows the factors considered by various project selection models. From the Table it can be seen that no model considers the interaction between various phases of the projects and factors such as the decision effectiveness, production preparedness, user commitment etc. that are very crucial for the success of the project during the execution phase. And hence much more work in this direction is required before a model can be made readily usable for real life problems.

2.6.2 SHORTCOMINGS OF THE EXISTING MODELS/METHODOLOGIES.

The shortcomings of the project selection methods described in the literature can be summarised as follows:

1. They often have inadequate treatment of interactions, both benefit contribution and resource utilisation.

2. They fail to deal with uncertainty of both benefit contribution and parameter estimation.

3. They involve multiple, interrelated decision have no common, natural underlying measure.
Fig. 2.3: Growth in the applicability of R&D project evaluation models for real life problems
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<th>COST</th>
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<th>INTERACTION BETWEEN VARIOUS PHASES OF PROJECT LIFE CYCLE</th>
<th>MONITORING OF PROJECT RELEVANCE DURING R&amp;D PHASE</th>
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<th>CONSIDERATION FOR THE NATURE OF THE PROJECT</th>
<th>INTEGRATION OF PROJECT STATUS INFORMATION</th>
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Table 2.5: Comparison of factors considered by various project evaluation models (Amended from Joseph P Martino[12])
4. They fail to recognise the time variant property of parameters and criteria, and the associated problem of continuity in research program and staff.

5. They often treat the project selection problem as a once-a-year decision event rather than an ongoing process.

6. They often do not include considerations, such as the timing of the decision.

7. They often cannot deal with a diversity of projects ranging from basic research to engineering.

8. They may fail to recognise the importance of individual R&D personnel.

9. They may not deal with the need to establish and maintain balance in the program: basic versus applied; offensive versus defensive; breakthrough versus improvement; product versus process; high risk/high payoff versus low risk/moderate payoff.

10. Models require much quantitative input data, not readily available.

11. Models are based on sophisticated techniques beyond the use of R&D managers.

12. The inability to recognize and treat nonmonetary aspects such as establishing and maintaining balance in the R&D program (e.g. balance between basic and applied work, between offensive and defensive activity, between product and process effort, between in-house and contracted projects, between improvement and breakthrough orientation, and between high-risk, high payoff and moderate or low risk-moderate payoff opportunities);

13. Perceptions held by the R&D managers that the models are unnecessarily difficult to understand and use; and

14. Inadequate treatment of the time variant property of data and criteria and the associated problem of consistency in the research program and the research staff.
2.6.3 OTHER IMPORTANT ISSUES REPORTED IN LITERATURE

(a) Prevailing budgeting and project selection practices involve a top management that makes project selection and resource allocation decisions, with bench scientists and research group leaders making proposals. Top management selects those proposals and allocates resources to those proposals which best contribute to the overall organizational goals. Thus, there would appear to be a need for mechanisms which improve the awareness of organizational goals at the research level, and the awareness of technical capability and potential projects at the management level. Top management may be setting organizational goals without a full realization of what the organization's technical capabilities and potential projects could be. Consequently, top management may set organizational goals which do not correspond with the full utilization of the organization's capabilities.

(b) The tenure of a manager typically is less than a desirable long-range planning horizon. The budgeting and project selection decisions are similar: the manager making the allocation decision or project selection is likely to be evaluated on performance during periods which are much shorter than be probable payoffs of research and development. Consequently, there is likely to be a management bias towards projects which have near-term and visible impacts, rather than towards projects which may be slower in developing and have longer range (albeit more significant) impacts. Research is indicated that would examine this apparent incentive incompatibility and, if it exists, to design management evaluation methods that would alleviate this incompatibility.

(c) The evidence indicates that risk models, although developed for several years and apparently conceptually sound, are not widely used. Unlike economic measurement techniques (ranging from payback period to present value and return on investment measures), risk measurement techniques do not appear to be acceptable standard practice.

(d) In general, there would seem to be a great deal of latitude for quantitative model-builders to improve on their formulations. Most of the quantitative project selection-budgeting models that have been developed seem to possess several limitations some of these limitations include: inadequate treatment of multiple, often interrelated, criteria; inadequate
treatment of project interrelationships with respect both to value contribution and to resource utilization; no explicit recognition and incorporation of the experience and knowledge of the R&D manager; the inability to recognize and treat non-monetary aspects such as establishing and maintaining balance in the R&D program; perceptions held by the R&D managers that the models are unnecessarily difficult to understand and use; inadequate treatment of the time variant property of data and criteria and the associated problem of consistency in the research program and the research staff. However, it must be emphasized that the elimination of the limitations of quantitative approaches does not guarantee adoption. The critical factor may be the perception of the organization members concerning the value of quantitative models. Quantitative model-builders will have to make their models more saleable and become advocates in order for models to be adopted.

(e) Other current research which is aimed at integrating functionally specialized groups within large organizations holds some promise for overcoming many of the behavioral obstacles to the acceptance and use of value measurement methods and portfolio models. Model-builders have traditionally given little attention to the prevailing adoption attitudes of R&D managers, the organizational climate, and the model's intrinsic compatibility with the habitual organizational modes of behavior. Furthermore, the fashions in which many organizations are set up inherently create intergroup rivalries and dissonant languages which must be bridged before and quantitative model for project selection-budgeting can possibly be expected to make any contribution. There is a level of organizational integration and interpersonal readiness which must first be established before such a model can be expected to be effective. Methods for establishing this readiness have been developed and studied in real organizations and are still being perfected.

(f) The future of quantitative approaches to R&D budgeting and project selection is optimistic. A recent survey showed that the number of organizations using corporate planning models has risen significantly in the last fifteen years. This increased usage portends will for the use of R&D models in the future. However, quantitative model-builders must begin to adapt their models to existing organizational processes and learn to be advocates and salesmen of their work. Similarly, R&D managers must begin to communicate to the model-builders more precisely R&D
critical parameters and the information they need in their own organizational context.

2.6.4 REQUIREMENTS FOR THE NEW MODEL

Based on the analysis of various models mentioned above, the following conclusions are made:

a) Need for integrated approach

Currently no model is available for the integrated analysis of the project throughout its life cycle. All the current models are having very narrow scope and are applicable to very narrow domains. Hence, there is need for unified model. While the recent systems approach models attempts to seek insights into the organizational decision making process as a whole, much more is desired in this direction towards unification of models that are applicable universally for all types of projects and for all phases of project life.

b) Need for usability for real life situations

It is established beyond doubt that the acceptance and use of various models by the professionals in the field is very poor. Out of numerous reasons reported for this, the most important one appears to be the lack of user friendliness in the models apart from the inherent deficiencies in the models. Also it is true that inspite of hundreds of models, R&D projects throughout the world are characterised by the time and cost overruns resulting from the inherent uncertainties. This clearly shows that there is a large gap between the actual requirements and the capabilities of available models. This is particularly true of the portfolio and risk analysis model as they have always remained with in the exclusive domain of the academic research.

c) Need for radically new approach

Overcoming the shortcomings of the existing models through incremental efforts may not solve the problem and a new way of thinking is required to address the problems of the R&D project management process as a whole. In the case of project monitoring, there
has not been any progress since the US DOD's "earned value method" given in 1962. The new tool 'PACT' of IGMDP is a major improvement over this approach. Similar tools are required in the other functions also.

d) Need for new paradigm

Emergence of new management practices such as the 'Concurrent Engineering', which in practice may be implemented as 'Development-Production concurrency' based on 'Team approach', at least for the next few years till the 'computer based design approach' matures, requires in depth understanding of new R&D dynamics in terms of the effect of CE on the organization as a whole and on the project management in particular. Currently the knowledge level in this area is very poor and hence requires development of new paradigm.

e) Utilization of emerging developments in the information technology

While it is true that an ideal model needs minimum computation (as per Sounder’s performance criteria for project selection models), it may be noted that the effective use of recent developments in the information technology especially in the area of 'Groupware' holds key for many of the problems faced in integrating the project selection process with the organizational process. Similarly, the advent of user friendly graphic user interfaces (GUI), large processing power available with compact computers can be effectively made use of to improve the usability of the models and their performance in terms of speed and comprehensiveness.

Decision support systems provide means to integrate the various models into an integrated model that can be applied for a variety of situations. Similarly expert systems make it possible to encode the specialist knowledge gained through the years of experience into a computer usable form. The developments in these two areas make it possible to circumvent the limitations in the implementation of the integrated models for the real life applications.
f) Amended criteria & characteristics of an ideal model

Based on the observations made during the literature survey, the Souder's and Stuart Bretschneider criteria & characteristics of an ideal model has been amended as shown in Table.2.6.
<table>
<thead>
<tr>
<th>Realism criteria</th>
<th>Flexibility criteria</th>
<th>Capability Criteria</th>
<th>Use criteria</th>
<th>Cost criteria</th>
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<tbody>
<tr>
<td><strong>Model includes</strong></td>
<td><strong>Model applicable to</strong></td>
<td><strong>Model performs</strong></td>
<td><strong>Model characterised by</strong></td>
<td><strong>Model has</strong></td>
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<tr>
<td>* Multiple objectives</td>
<td>* Any type of projects</td>
<td>* Project evaluation &amp; selection</td>
<td>* Familiar variables</td>
<td>* Low setup cost</td>
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<tr>
<td>* Multiple constraints</td>
<td>* Any point in time during the life cycle of projects</td>
<td>* Baseline planning</td>
<td>* Discrete variables</td>
<td>* Low personnel cost</td>
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<td>* Technical risk</td>
<td>* Priority decisions</td>
<td>* Realistic performance monitoring</td>
<td>* Easily computerised</td>
<td>* Low computer cost</td>
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<td>* Commercial risk</td>
<td>* Budget allocation</td>
<td>* Provides early warning of potential problems</td>
<td>* Minimum computations required</td>
<td>* Low data collection cost</td>
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<td>* Market risk</td>
<td>* Project funding</td>
<td>* Helps in termination decisions</td>
<td>* Special person not needed</td>
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<td>* Manpower limits</td>
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<td>* Instantaneous closure evaluation</td>
<td>* Special interpretation not needed</td>
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<td>* Facility limits</td>
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<td>* Multiple time period analysis</td>
<td>* Low amount of data needed</td>
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<td>* Budget limits</td>
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<td>* Optimisation analysis</td>
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<td>* Technology limits</td>
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<td>* Premises uncertainty</td>
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<td>* Project interdependencies</td>
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<td>* Interaction between various phases of project life cycle</td>
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<td>* Monitoring of project relevance during R&amp;D phase</td>
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<td>* Monitoring of risk variation</td>
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<td>* Consideration for the nature of the project</td>
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<td>* Integration of project status information</td>
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<td>* Consideration of decision system &amp; motivational aspects</td>
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<td>* Consideration of production requirement in R&amp;D phase</td>
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<td>* Consideration of user commitment</td>
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<td>* Monitoring of product cost effectiveness</td>
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*Table 2.6: Amended criteria & characteristics of an ideal model*
REFERENCES FOR CHAPTER 2


