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WHEN TO USE MARGINAL BENEFITS TO MAXIMIZE PROJECT PORTFOLIO VALUE

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When to Use Marginal Benefits to Maximize Project Portfolio Value

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Abstract

Strategic R&D portfolio selection aims at an appropriate balance between various activity areas (e.g. technologies or markets). Following a strategic macro-allocation of resources, portfolio management aims to select projects that create the most value from a pool of resources. We propose a practical method for the latter purpose. Decision support models, such as integer programming, are powerful and sound, but have been rarely adopted due to their complexity.

Our method has two steps. First, each project is represented by a decision tree that accounts for future flexibility to respond to contigencies. The candidate projects are ranked according to their marginal benefit, expressed as "option value per budget dollar". The ranking is optimal if the investment decisions are continuous variables, if the budget limit can be adjusted to "squeeze in" the last project, or if all projects have approximately the same resource requirements. If these conditions are not fulfilled, a second step employs integer programming to trade off efficient resource use (high option value per dollar) with the benefit of higher total budget utilization. The ranking-"correction" steps make integer programming easier to communicate to managers. We develop a pilot analysis of this method with the applied research group of a diamond producer.

1 Introduction

This article discusses a project selection procedure in the process and equipment R&D group of a diamond company. The article makes two contributions: First, it shows how results from decision theory can be translated into an applicable process of prioritizing R&D activities. Second, it develops a simple communication "vehicle" that facilitates intuitive explanation to a managerial group. Previous work has repeatedly observed the limited adoption of mathematical models in R&D portfolio selection, because the methods are too complex and non-intuitive for non-experts (e.g., Corbett & Van Wassenhove 1993, Cabral-Cardoso & Payne 1996, Loch *et al.* 2001).

Our method consists of two steps: First, we use an approximation to rank the candidate projects according to their marginal benefit, expressed as "option value per dollar". This ranking can be used in the form of an intuitive graphical decision support tool. Then, a second step (if necessary) corrects the approximation to more efficiently utilize the available budget. This "correction" step uses mathematical programming, which is hard to explain to managers, but can be easily communicated coming after step one.

We begin by briefly reviewing the importance of the project selection problem and the relevant academic literature (§2). Then, we explain the logic of our method (§3). §4 introduces the host company and the project data, and §5 derives the "optimal" project portfolio. §6 concludes with a discussion of the benefits and limitations of the method.

2 A Review of R&D Portfolio Selection

The choice of the "right" new products or processes to develop is a difficult problem for organizations (e.g. Feltham & Xie 1994, Roussel *et al.* 1993, Cooper *et al.* 1997):

- *High uncertainty.* New product managers face risks related to the functionality of the product (technical risk) and to adoption by customers (market risk).
- *The dynamic nature of the decision.* R&D management needs to account for current *and* future potential or risk, since NPD projects require resources and produce benefits over multiple years.
- *Scarce resources.* Projects compete for the same scarce R&D budget, and can, therefore, not be analyzed in isolation.
- *Interactions among the projects.* New products may exhibit synergies or incompatibilities in their technical aspects or on the market side (substitutes or complements).

Management researchers and practitioners have proposed many methods for tackling the complexity of the portfolio selection problem, which we classify into three groups:

The first group starts from the premise that projects can not be compared based on one "number". Rather, the choice should balance multiple dimensions of strategic implications. Wheelwright and Clark (1992), Roussel *et al.* (1993), and Cooper *et al.* (1997) recognize the significance of portfolio selection in corporate strategy. For example, one balance criterion is the product change level vs. process change level, another the risk vs. the project benefit (tangible and intangible). These frameworks connect business strategy with R&D investments, but do not specify the R&D manager's operational portfolio choice.

Complementary to this stream of work are formal multi-dimensional decision making approaches (e.g., Liberatore 1987, Saaty 1994, Hammonds *et al.* 1998, Henrikson & Traynor 1999) and qualitative ranking models (e.g., Brenner 1994, Loch 2000). While available numbers are usually not sufficient to come to a final conclusion, these methods are critical in reducing the complexity of the problem. They help to narrow down the list of candidates, and to identify subgroups within which projects can be compared along one dimension.

The second group of work focuses on financial performance. It encompasses findings from the financial literature like net present value (NPV) analysis (Hess 1993, and Sharpe & Keelin, 1998), and break-even time (BET) (House & Price, 1991) applied at the operational level of a single project. Recently, the real option valuation technique has gained some prominence, highlighting the importance of managerial flexibility in R&D management (e.g., Newton & Pearson 1994, Trigeorgis 1997, Luehrman 1998, Huchzermeier & Loch 2000, Loch & Bode-Greuel 2001). So far, real options have been considered for individual projects, but not for portfolios of interacting projects.

The third group of work consists of mathematical programming models (e.g., Beged-Dov 1965, Benson *et al.* 1993, Czajikowski & Jones 1986, Fox & Baker 1984, Schmidt & Freeland 1992, Souder 1973, 1978, Loch *et al.* 2001). Mathematical programming is a well-established methodology which can deal with resource interactions within a shared limited budget. However, this method has not been widely adopted by practitioners (e.g., Souder 1978, Corbett & Van Wassenhove 1993, Cabral-Cardoso & Payne 1996, Loch *et al.* 2001, Meredith 2001). Resource interactions make the problem combinatorial (projects are "in" or "out", and all combinations need to be compared as more projects are considered), while the method is difficult to understand for non-experts in operations research (it lacks robustness – small changes in problem parameters may cause seemingly arbitrary shifts in the chosen projects). In addition, there has been no applied work considering multi-period effects or real option valuation.

Our approach combines the approaches of groups 2 and 3 above. We focus on a financial performance measure, thus assuming that a multi-dimensional analysis has determined strategic areas of R&D activities, within which projects can be compared. We first consider the multi-period nature of the decision. We represent each project by its option value, and we show that under the conditions of our study setting the problem can be boiled down to a single period optimization problem. Then, we propose an intuitive ranking of the projects by their "option value per dollar invested". The proposed indexing is only a heuristic in general, but we show when it offers a good base decision and when it can easily be corrected without losing the intuitiveness and transparency of the recommendation.

3 The Selection Method

Our method starts after the candidate projects have been pregrouped into areas of strategic activities. This permits one-dimensional comparison, which in our method is a financial characterization of the projects. Three premises need to be fulfilled:

First, the projects are independent of one another – no project enables or hinders others. The only interdependence exists due to the common budget. This is fulfilled in our study setting. If some projects do exhibit interdependencies on the technical or market side, they need to be combined, for the purpose of this analysis, into one "grand" project, both in terms of resource requirements and in terms of expected outcome.

Second, management does not cut ongoing projects for budget reasons – a project will be allowed to continue as long as it progresses well. This premise means two things: (a) the absence of radical budget cuts in the near future, and (b) the absence of an extremely promising future project, of which management already knows that it will be started soon. If such a "star waiting in the wings" exists, it might crowd out a currently started project next period. Thus, management should consider such a project already now, assessing which projects it might displace (possibly excluding those in the first place). We observed this policy in our host company, and it was claimed "natural". Management expressed the view that it would be hard to justify stopping well progressing projects.

Third, the following data should be estimated for each project: yearly costs, continuation probabilities, and market value projections after launch. In our host organization, costs are usually easy to obtain, while market values require collaboration and discussion among R&D management and their internal clients (in our study, the operating mines). Risks assess the potential of the project to reach the final stage and be successfully launched in the market. We defined these risks as stage-gate probabilities: "The probability that a project, given it has come this far, continues to the next phase".[1]

Each project can be represented as a decision tree. The tree structure captures the additional value of managerial flexibility (real options). Thus, the ability of a manager to stop a project that performs badly increases the value of the project, since it reduces the negative impact of bad performance (e.g. "stop throwing money after a hopeless target"). Figure 1 shows such a decision tree representation for the ES-2 project in our host company. The value at the root of the tree corresponds to the option value of the project (it is higher than a straight NPV because of the managerial flexibility to stop).

These premises lead to the first theoretical result. The rigorous justification of Results

[1] In this study, the major risks are failure and cancellation. In other contexts, risks may include contingencies that require changes of the project plan or target market segments. This makes the decision tree more complex, but the basic approach remains the same (Loch & Bode-Greuel 2001).

1 and 2 via decision analysis is included in the Appendix.

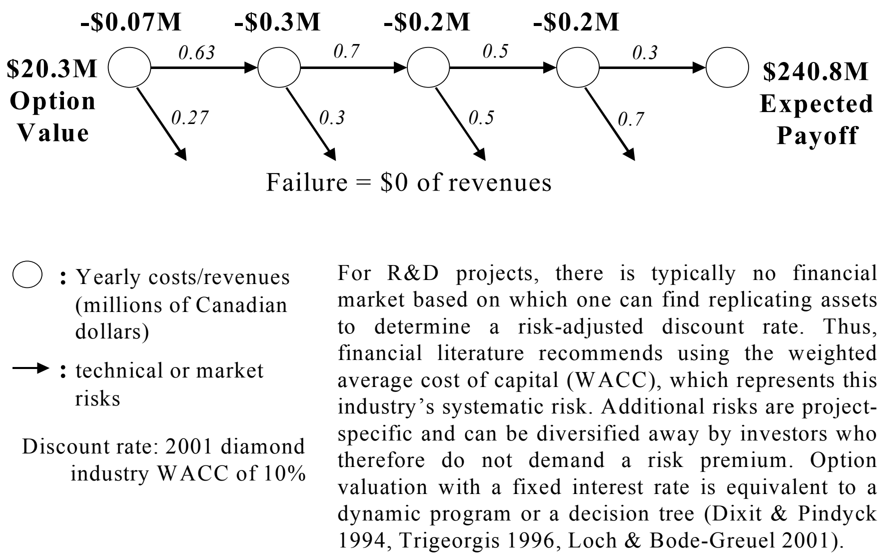

Figure 1: Decision Tree Representation of Example Project ES-2

Result 1: *The dynamic portfolio selection problem (considering multiple projects and multiple periods) collapses to a choice of single period projects competing for budget funding. Each of the projects is characterized by its first year resource requirements and its option value at the end of that year.*

The intuition behind Result 1 is that once future funding is guaranteed, the only future concerns are the evolution of each individual project as represented in its decision tree. Therefore, the present value of the project decision tree can be used as the equivalent of the expected payoff of a one period project.

Result 2 is the basis for the selection process:

Result 2: *If the project resource requirements are the same, if budget limits are "soft" (i.e. the last project can be "squeezed in" if it goes only slightly beyond the official limit), or if the resources for an individual project can be slightly reduced without losing benefit overproportionally, the optimal portfolio contains the projects with the highest "option value per dollar invested".*

If the conditions of Result 2 are not fulfilled, it may be preferable to use more of the scarce resources, even if inefficiently. As an extremely simple illustration, suppose we have a budget of $90 to allocate among two projects with expected returns of $300 and $800, respectively. The projects cost $20 and $80. Thus, only one project fits into the budget. Although project 1 is more efficient (its value per dollar invested is 15, versus 10 for project 2), it would result in only a third of the budget utilized. It is better to undertake project 2, using more of the available resources, albeit less efficiently.

This is, of course, exactly the reason why project selection under a tight budget requires combinatorial optimization with exploding complexity as the number of the projects increases. Result 2 builds on previous theoretical results (Loch & Kavadias 2001), which have shown theoretically that a marginal benefit index is a good rule if the conditions of Result 2 are fulfilled.

Result 2 suggests a two-step project selection process, summarized in Figure 2. In the **first step**, all projects are ranked according to the heuristic index. In many practical cases, this produces a near-optimal portfolio. If the conditions for Result 2 are not fulfilled, it is necessary to perform **step 2**, solving the full mathematical program. It trades off a high return on the investment of individual projects for more efficient utilization of the overall budget. The "correction" of the heuristic solution is easily explainable as a better use of the budget, making the procedure transparent to non-experts.

Summary of the selection method

PREMISES

1. Each project is independent from the other projects. The only interdependence is the common pool of resources.
2. A well performing project keeps receiving funding until its completion.
3. Estimates of yearly costs, projected profits and market/technical risks are available.

SELECTION STEPS

1. For each project, a marginal benefit index is calculated.
2. Rank all candidate projects with respect to their index.
3. If (a) *projects are of similar size*, (b) *budget limits are "flexible",* or (c) *individual projects can live with slightly lower budget*, then Step 2 is the solution, otherwise "correct" Step 2 to account for resource utilization (solve mathematical program).

Figure 2: Summary of selection process steps

4 Application: History & Background Information

4.1 Portfolio Selection at GemStone

Our study was conducted with the manager of the applied research department of GemStone, a medium-sized diamond producer[2]. The department conducts applied process

[2] The name of the company and all numbers are disguised to preserve confidentiality.

R&D to improve and advance diamond identification, extraction, and processing technologies, and provide GemStone with lower cost and higher yield operations.

During the year 2000, GemStone's research group developed a performance measurement system to link their strategy to R&D operations (this is described in Loch & Tapper 2001). One outcome of this exercise was the identification of three strategic activity areas: knowledge creation efforts, support of technical services, and next generation technology development. In 2001, the research group has begun a portfolio prioritization initiative. While financial numbers are not appropriate to evaluate knowledge creation and service support activities, they can well be used for the next generation technology subportfolio.

In the past, the research department had been engineering driven. The main technical focus areas are summarized in Figure 4. Within those, projects had been selected by the Research manager and the leading experts in each area based mainly on their experience and personal judgment. There had been no formal process in place to ensure a transparent project portfolio selection.

Research management realized the drawbacks of the current situation. While a few innovative projects had been initiated by the explorative spirit of researchers, the "customers" (the operating divisions) expressed frustration with products that did not function properly and did not address their main needs of achieving productivity improvement. The research department had been perceived by some operating divisions as "disconnected from the real world".

Areas of expertise	Description
Recovery Technology	*Development of equipment for the location of the diamonds within the ore body and the assessment of their value*
Environmental Technologies	*Examination of the feasibility of conducting the mining operations with environmentally "friendlier" processes*
Sorting Technologies	*Development of processes to separate gem stones from the ore body*
Mining Technologies	*Research on new methods of efficient mining under different soil conditions (e.g. under sea mining and arctic mining)*
Process Optimization	*Automation and optimization of plant operations (referring to the plants operating close to the mines)*
Machine Intelligence	*Research on artificial intelligence methods for anti-theft usage (i.e. gem stone security) and labor security (i.e. accident prevention)*

Figure 4: Areas of Expertise in the Research Function

4.2 Data Collection

As we discussed above, the Research department distinguishes its projects into service support, knowledge-building, and next generation technology projects. The latter had been assigned a budget of Canadian $1.775M.

Therefore, we collected estimations for the following variables, in cooperation with the eight Research Program Managers (RPMs, the research manager's direct reports): (a) **Annual costs per project**. The major cost component is manpower, about which the RPMs usually can make reasonable estimates. Moreover, the project costs are typically quite small, so estimation errors do not have a large impact. The main leverage on project value comes from the next two sets of parameters. (b) **Technical and market risks**, estimated as the probability that a project currently under development will continue to receive resources in the following year. (c) **Projected rewards**, of products after launch.

The collected data was iterated twice to obtain the best possible estimates. The RPMs had to get in contact with their internal customers, such as mine engineers, and *explore* the potential benefits of their technologies in the field. The projected rewards had to be agreed upon by the RPMs (as suppliers) and the mines (the customers). In the course of the data collection, the project characterizations were repeatedly discussed among the RPMs, the research manager, and the authors.

The authors carefully explained the selection procedure to the RPMs, in order to acquire homogeneous data estimates. For example, RPMs conceptualized success probabilities differently - some viewed them as transition probabilities (chance of continuing from one year to the next), while others defined the risk profile of the project (marginal probability of the project being successful overall).

Figure 5 provides a summary of the current project portfolio, with project resource requirements and option values (the roots of the decision trees see Figure 1)[3].

The summary of the data reveals the wide range of return on the investments that characterize the research portfolio. The success probabilities for the year of reference (i.e. 2001) appear to be high because the questionnaire and interviews were conducted in the

[3] The full multi-period tree for each project is available by the authors. Names and numbers have been disguised for confidentiality purposes.

middle of 2001, hence researchers had a good estimate of whether the project would be continued into next period.

The total budget for year 2001 is C$1.775M. A simple addition of the numbers in the second column shows that the sum of the costs of the projects currently in the portfolio is higher than the total budget. In the past, no explicit prioritization process had existed, and when technology level projects needed more money, they just took it from the knowledge-building budget, crowding out long-term potential breakthrough ideas.

Project Name	Technological Area	Cost ($m)	Expected Value ($m)
ES-1	*Recovery*	*0.25*	*1.4*
ES-2	*Recovery*	*0.07*	*10.2*
Valuation Technologies	*Recovery*	*0.06*	*10.8*
ET-1	*Environmental*	*0.27*	*3.8*
Processing Technologies	*Environmental*	*0.2*	*9.9*
HT Recovery	*Sorting*	*0.16*	*5*
MC	*Sorting*	*0.036*	*4.8*
MGS	*Sorting*	*0.07*	*3.8*
Process Optimisation	*Process Optimization*	*0.7*	*15.2*
DCH	*Mining*	*0.15*	*9.4*
ElSuP	*Machine Intelligence*	*0.15*	*1.7*

Figure 5: Overview of Candidate Projects

5 Structuring the Portfolio at GemStone

5.1 The Selection Process

In this Section, we describe how the theory was translated into practice at GemStone. We present the "pilot analysis" that we developed in collaboration with GemStone's research manager. The manager discussed this pilot version with the RPMs and with the other R&D managers (development, technical services and equipment manufacturing). Recognizing the potential of creating transparency and improving their decision process, they started to collect the data to implement the method over the coming year.

The setting at our host organization satisfies the premises for the applicability of our method, as we discussed in Section 3. Projects did not utilize the same technologies, and did not address same needs or even same mining operations. Hence, they were independent. However, project resources could not be varied continuously - a project was either "in" or "out". Thus, we needed to check the validity of the index solution by running the second step of the method, an integer program.

As the first step, we rank the projects by their marginal option value per dollar invested. A visual representation of this ranking is depicted in Figure 6. The budget constraint is represented by the thick vertical line. The ranking excludes the ElSuP, and ES-1 projects because of their low index, cutting just before the budget constraint. The RPMs immediately understand the rationale of ranking projects according to their marginal benefit. The manager's comment was: "This is the first time I can explain a prioritization to top management - let's get this robust enough so I can show it at the next top management meeting."

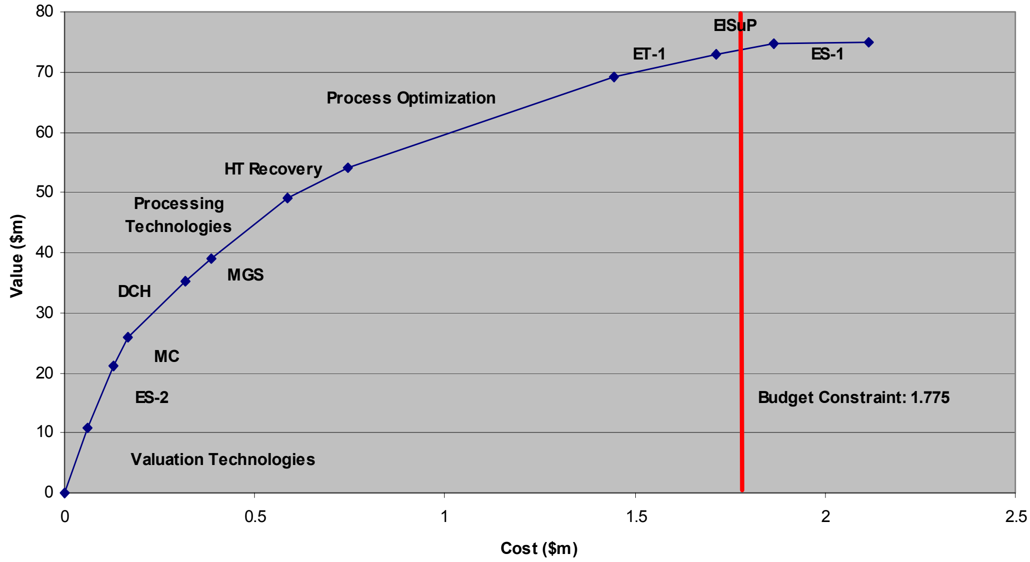

Figure 6: Graphical Representation of the First-Step Portfolio

In the second step, we assessed via integer programming whether the portfolio was optimal [4]. It turned out that, in this case, the option value per dollar index already produced the best portfolio.

5.2 Sensitivity Analysis

During the interviews, the RPMs identified some parameters that were subject to uncertainty (all of them benefit figures – the benefits were the most difficult to estimate because

[4] We used a standard optimization problem solver (LINGO).

the mines were not yet sure of all the impacts of the new technologies on their processes). Therefore, we ran a sensitivity analysis for these parameters, varying them as follows:

- The Process Optimization project generates \$32m per year for its life cycle (a reduction of 17.5%) and costs \$0.8m the first year (an increase of 14.2%)
- The ElSuP incurs \$5m profit during the first year of its launch (15% increase).
- The ET-1 project generates \$20m in its first year (a 50% increase).

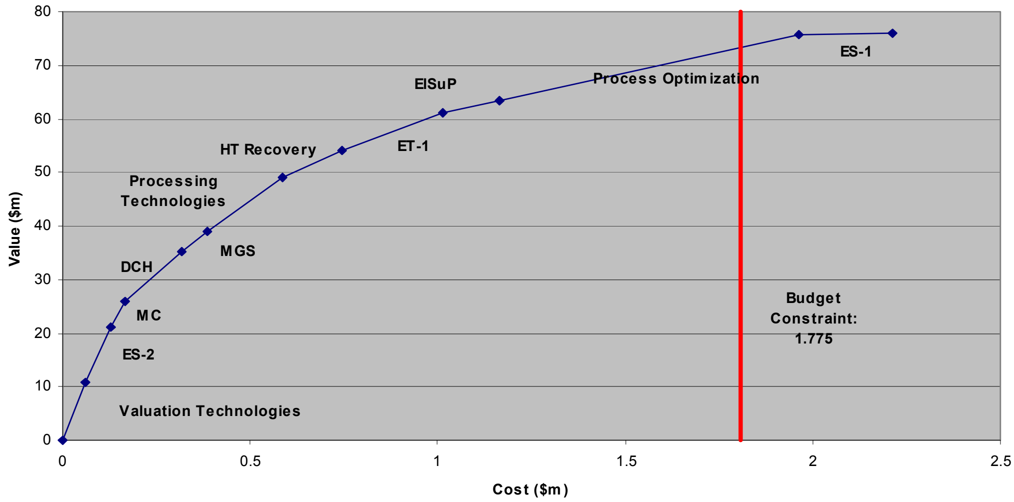

Figure 7: First-Step Portfolio in the Sensitivity Analysis

Figure 7 again graphically illustrates the ranking solution. According to the heuristic, the best portfolio should include all projects up to ElSuP, leaving out Process Optimization and ES-1. However, the Process Optimization project has the highest resource requirements, and excluding it would lead to an underutilized budget. Figure 7 shows that only \$1.164M are used, 65% of the total budget. There might be a combination of projects that would achieve a higher portfolio value simply by using more of the budget (assuming that all projects have a rate of return higher that the cost of capital and are, thus, worthwhile).

The Integer Programming solution confirms this conjecture. The optimal portfolio (Figure 8) indeed includes the Process Optimization project and utilizes \$1.744m (98% of the total budget) with a portfolio value of \$69.5m (instead of \$63.4m for the heuristic).

At the same time, it is apparent why the MGS project and the ElSuP are excluded. Although these projects utilize the scarce resources more efficiently (higher marginal value), overall they contribute less value to the portfolio than the Process Optimization project. Therefore, substituting them with the Process Optimization project improves the total portfolio value by utilizing more of the available resources.

The main benefit of this simple analysis is the intuition that it builds for the RPMs and the research manager. Indeed, communicating the results to management has been successful, because of the intermediate communication step that increases problem transparency. This transparency is difficult to build with integer programming methods alone: managers and engineers not trained in the method find it very difficult to understand what exactly the logic behind the method is, and why, for example, the most valuable project does not make it into the portfolio (e.g., Loch *et al.* 2001, Meredith 2001).

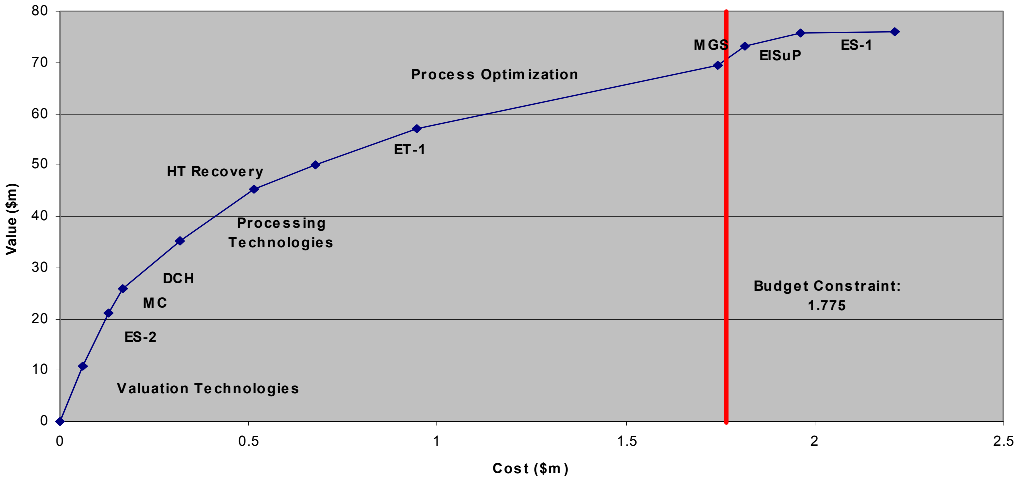

Figure 8: Optimal Portfolio in the Sensitivity Analysis

6 Discussion and Conclusion

Quantitative methods have not found widespread use in R&D portfolio selection because the inherent difficulty of the problem makes the tools complex and hard to use: the effects of a portfolio selection stretch over multiple periods, projects interact directly or through their competition for the same scarce resources, and project outcomes are uncertain and sometimes hard to quantify.

In this article, we have developed a practical approach to R&D portfolio selection on the example of the process R&D laboratory of a diamond company. Our method uses a financial payoff criterion, assuming that management has already performed a strategic grouping of R&D activities into internally comparable categories, such as "knowledge building projects", "technical service support", and "next generation technology development" in the case of our host organization. Projects within one strategic category should be comparable along a key decision dimension.

Using decision theory, we show under which circumstances the intractable multi-period prioritization can be collapsed to the comparison of an index without making big errors: Represent each project by its *decision tree*, which accounts for major future risks and the managerial ability to respond to these risks. The expected present value of this decision tree is the projects *option value*. Our first result establishes that the decision maker may simplify the multi period portfolio selection to a single-period prioritization, if a currently initiated project is not terminated in the future for budget reasons (but only if it is struck by a risk and becomes unattractive).

Our second result establishes that the projects in this one-period problem may be prioritized by an "option value per dollar of budget investment" index, if one of three conditions holds: (a) project budgets can slightly be adjusted up or down (with only slight effects on project outcome) to "fit them into the budget", (b) the overall R&D budget is somewhat flexible, or it can be stretched to fit in the last project, or (c) projects are all of about equal size (in terms of budget needs).

Based on these results, we propose a two-step procedure: first, rank the projects according to the "option value per dollar of investment" index. If the above three conditions are not fulfilled, perform step 2, a full mathematical programming analysis of the one-period budget problem. The mathematical program may produce a portfolio selection that overall utilizes the R&D budget better.

The method that we propose in this article makes two contributions: first, we show rigorously, based on decision theory, under which circumstances the complex portfolio problem may be attacked with an intuitive, simple index (option value per dollar of budget investment). It is of value to managers to understand when such simple tools are appropriate and when they lead to erroneous results.

Second, our two-step procedure may serve as a "communication" device that helps managers to digest the complexity of optimally "allocating a scarce resource" (via mathematical programming). Starting with the intuitive and graphical index ranking, it can easily be explained how the result of a mathematical programming analysis improves on the overall budget utilization.

As in the case of any quantitative method, it is essential to use our index prioritization not mainly to provide "answers", but to force transparency and foster discussion. The prioritization index is only as good as the estimation of costs, success probabilities and payoffs. Applying the index should force researchers to seek contact with their "customers" (in our setting, the mining operations) and agree on the value of the technologies in case of success. This value agreement must reflect the views and the interests of both sides, the providers as well as the later users of the technology. Similarly, the index should be used to initiate internal discussions within the R&D group.

Such discussions, appropriately guided, are critical in increasing the quality of the estimated data. If each member of the research management team is forced to "defend" his numbers in front of his colleagues, the collective knowledge is brought to bear on the estimates and colleagues will "keep each other honest" in not predicting too much or too little. Such discussion may lead to a fundamental questioning of what is a "good" project. For example, researchers can question the resource needs of a project that soaks up a significant part of the budget (e.g. in our study, process optimization required 40% of the total budget). Making very large investments in one "monolithic" project is very risky, and one could ask whether an "incremental" approach can be found (e.g. Genus 1997). Dividing a large project into two or more phases, until risk is reduced, gives management more flexibility and, thus, higher value.

GemStone's research group has been convinced of the benefit of a quantitative method, based on the pilot we developed. They still have to go through the difficult process of making it a reality, by institutionalizing the discussions with their customers and internalizing the transparency brought by the process (even when it is uncomfortable). But the potential benefits are large – once the researchers have internalized the process and share a common understanding of the criteria, part of the prioritization can be delegated to them (within a strategic category of activities). Thus, a transparent and shared process can empower the researchers, giving them the ability to propose their best ideas (within the strategic priority areas) rather than only reacting to demands from management. This may contribute to a "fair process" in which both sides (management and researchers) have agreed to the "rules of the game" (Kim & Mauborgne 1997). A fair process ultimately contributes to higher performance.

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8 Appendix

Result 1

Suppose n projects are candidates for the R&D project portfolio. Project i's period t development cost is c_{it}. The transition probabilities of "successful progress and thus continuation" are p_{it}, the probabilities of technical failure (termination) are $(1-p_{it})$. The final payoff of project i is Π_i. The projects are independent of one another, which implies $Pr\{$i and j move successfully to t+1$\} = p_{it}p_{jt}$. Then, the optimal portfolio is the solution to the problem

$$V_t(I) = \max_{J \subseteq I}\{-\sum_{j \in J} c_{jt} + \beta \sum_{K \subseteq J} \Pr\{K \mid J\} V_{t+1}(K)\}, \text{ subject to } \sum_{j \in J} c_{jt} \leq B_t. \tag{1}$$

β is the discount factor. $V_{t+1}(K)$ is the dynamic programming value function of the set K of projects pursued in period $t+1$. $\Pr\{K \mid J\}$ is the probability that a subset K of the chosen set J of projects will successfully continue into period $t+1$ (remember that we have excluded the possibility of exciting new projects arising next period, which will crowd out successfully ongoing projects). (1) optimizes the selection over all possible realizations, given the budget constraints in each period.

Result 1 states that we include the n best candidates in terms of their individual dynamic programming value function V_j, once we have no budget constraints in the future. That is, the optimal portfolio is given by

$$\max_{J \subseteq I}\{-\sum_{j \in J} c_{j1} + \beta \sum_{j \in J} p_{j1} V_j\}, \text{ subject to } \sum_{j \in J} c_{j1} \leq B_1. \tag{2}$$

Proof of Result 1. We prove the result for 2-period projects. For more periods, the argument is just applied repetitively. From (1) we get:

$$V_1(I) = \max_{J \subseteq I}\{-\sum_{j \in J} c_{j1} + \beta \sum_{K \subseteq J} \Pr\{K \mid J\} \max_{L \subseteq K}\{-\sum_{l \in L} c_{l2} + \beta \sum_{M \subseteq L} \Pr\{M \mid L\} V_3(M)\}\}$$

subject to $\sum_{j \in J} c_{j1} \leq B_1$, and $\sum_{l \in L} c_{l2} \leq B_2$. By the funding guarantee condition, we remove the budget constraint in the second period. Therefore, the problem simplifies to:

$$V_1(I) = \max_{J \subseteq I}\{-\sum_{j \in J} c_{j1} + \beta \sum_{K \subseteq J} \Pr\{K \mid J\}(-\sum_{k \in K} c_{k2} + \beta \sum_{L \subseteq K} \Pr\{L \mid K\} V_3(L))\}$$

subject to $\sum_{j \in J} c_{j1} \leq B_1$. The last part of the maximization term can be written as follows:

$$V_2(K) = -\sum_{k \in K} c_{k2} + \beta \sum_{L \subseteq K} \Pr\{L \mid K\} V_3(L).$$

As we know that the projects are independent and $\Pr\{L \mid K\} = \prod_{k\in L} p_{k2} \prod_{k\in K\setminus L}(1-p_{k2})$, we can write

$$\sum_{L\subseteq K} \Pr\{L \mid K\} V_3(L) = \sum_{k\in K} V_k(\sum_{L\subseteq K,\ k\in L} \Pr\{L \mid K\}) = \sum_{k\in K} p_{k2} V_k.$$

Then the first period problem (after analogously translating the $\Pr\{J \mid K\}$ into p_{j1}) becomes (2). ■

Result 2. The first part is mathematically stated as follows: Suppose the project costs are only ε-different in their resource requirements, that is for every i, $c_{i1} \in [c, c+\varepsilon]$. Suppose also that the ranking of the return on the investment[5] of the projects V_i/c_i, is $r_1 > r_2 > ... > r_n$. Then the upper bound of the difference between the heuristic portfolio and the optimal portfolio is $\sum_{k=m+1}^{n} \varepsilon(r_k - 1)$. Here, m is the number of projects that can be satisfied by the current budget B[6].

Proof of Result 2. Consider two projects such that $r_1 > r_2$, only one of which can be done due to resource constraints. Then the heuristic suggests that project 1 should be included. Assume that $c_2 \leq c_1 + \varepsilon$. The maximum possible error would result from including project 1 in the portfolio although project 2 should be chosen:

$$\begin{aligned} V_2 - V_1 &\leq -c_1 - \varepsilon + r_2(c_1+\varepsilon) + c_1 - r_1 c_1 \\ &= -(r_2 - r_1)c_1 + \varepsilon(r_2 - 1) < \varepsilon(r_2 - 1). \end{aligned}$$

This worst error occurs when the projects are almost equally valuable, $r_2 \approx r_1$.

Now suppose there are $m+1$ projects, only m of which can be supported by the budget. The same derivation as above leads to the error bound *Max. Error* $\leq \varepsilon(r_{m+1} - 1)$.

In the most general case, m projects have to be selected out of n. Following the same logic as above, the worst case scenario happens if all $(n-m-1)$ projects that are left out of the portfolio

[5]We assume that the returns on the investment are $r_i > 1$, as management would not undertake a loss making project.

[6]The total budget can, without loss of generality, be set to $m(c+\varepsilon) < B < (m+1)c$. The difference in the costs is so small that $m\varepsilon < c$.

should be "swapped" with some of the m already included in the portfolio. The upper bound of the total error is the summation of the individual upper bounds,

$$Max.\ Error < \sum_{k=m+1}^{n} \varepsilon(r_k - 1) < \varepsilon(n - m - 1)(r_{m+1} - 1).$$

This completes the proof of the first part of Result 2. We now sketch the argument for the other two statements of the result. Suppose the "option value per budget dollar" index selects the first m projects, and the $(m+1)$st is left out because it exceeds the budget limit B by an amount Δ. If the budget of project $(m+1)$ can be adjusted by a percentage Δ/c_{m+1} without reducing its benefit by more than by $V_{m+1}\Delta/c_{m+1}$ (that is, the benefit suffers no more than proportionally), we can reduce its budget without changing the index or wasting any of the scarce resources in the total budget. Thus, the result is optimal. Similarly, if the total budget B can be extended by the amount Δ, again the index is unchanged and no resources are wasted, making the index policy optimal. ■