Strategic Technology Investment Decisions in Research & Development

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ABSTRACT

NASA (National Aeronautics and Space Administration) is succumbing to pressures to operate more like a private entity than a government agency; however, modern business practices are rare in the organizational structure. NASA can install project evaluation and selection techniques like real options analysis to improve capital budgeting for technology projects.

This thesis evaluates the current NASA best practices in place for technology investment decisions; evaluates the application of Real Options to the technology selection policy; and makes recommendations for the strategic management of the NASA portfolio and publicly funded R&D in general.

The key insight is that a decision process can be established to fill the current vacuum and improve budget allocation, but that real options has two weaknesses that are particularly pronounced when applied to this sector. The first is the reliance on expert opinions for probabilities. The second is the necessity to place an absolute monetary value on outcomes. The difficulties are exacerbated by several factors: extremely long duration programs undermine estimates of expected benefits and R&D benefits are particularly intangible.

Thus, for NASA, the Real Options tool is recommended considering the improvement over the current technology investment system. However, further work is required in tailoring the application to NASA's special case, and therefore the general case of publicly funded R&D. A relevant improvement would require a rigorous method of obtaining consistent probabilities and technology valuations.

Insights developed over the course of this study lead to a broader conclusion about the interaction of research & development with product development. Strengthening this connection with a tool such as real options will benefit the development activities by facilitating better capital budgeting decisions on focused technology solutions that are integrated into superior products.

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Chapter 1. INTRODUCTION

1.1. Background

The most important decisions made by managers are those that commit resources to new projects. The initial selection from a variety of projects and subsequent choices for further investment are crucial. However, quantitative methods currently used to facilitate those types of decisions handicap them from the start. The classic Discounted Cash Flow (DCF) techniques, for example, tend to favor short-term, low-risk projects. The potential derived from uncertainty, the value of information that will become available, and the long-term are neglected and are often later inserted as part of a gut-level analysis by the manager. Also debilitating is the lack of flexibility imposed by a methodology that does not take into account intermediate alternatives other than full investiture or divestiture. The Real Options approach provides both a conceptual framework and quantitative tool for addressing the obvious shortcomings of the current techniques for evaluating projects and making decisions. Analogous to financial options, Real Options introduce flexibility in the form of intermediate investment choices, such as staging investments, scaling operations, and switching projects. Options thinking provides a repeatable method for standardized evaluations and justifiable quantitative support for decisions, so that "inertia" does not cause escalation of commitment in an unprofitable project.

Chapter 2. Technology Evaluation & Selection

2.1. Difficulties valuing R&D

Although uncertainty regarding uses of a technology is reduced as R&D becomes more applied, valuing these investments is still difficult. First, significant technical and implementation related uncertainties remain. Therefore, the potential for failure is real and the sequence and timing of events required to bring the technology to fruition is unclear. Second, estimating the value of project benefits remains difficult because end-users must often make complex trade-offs between product features. The revenue that the innovator will generate is likely to fall short of the value that the end-users place on the technical advancements.

The methods for evaluating the value of R&D to date have been largely qualitative. Based upon frameworks like the one shown in Figure 1, the selection and subsequent insertion of the technology was usually biased by a single advocate's view of its efficacy or desirability. (Iansiti, 1998) This was also usually a "bottom-up" approach, where end-user needs were neglected and the larger strategy of the organization was an afterthought.

2.2. Uncertainty and the sequential nature of R&D

Regarded as risky because the results and value are uncertain and because time horizons are often beyond forecasting, R&D investments pose a substantial challenge to an organization. However, R&D is also sequential in nature, and many risks are mitigated simply by making informed decisions over time as uncertainty is resolved. (Neely, 1998) The ability to react over the life of a project does influence the risk profile and value of R&D projects. Active management allows promising R&D efforts to receive continued attention while less promising projects are culled from the portfolio.

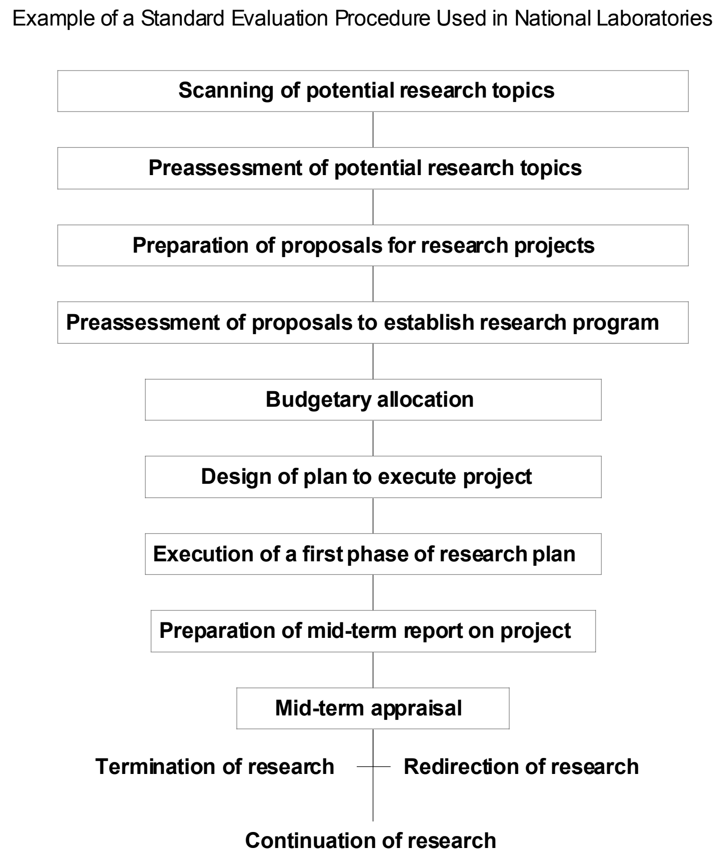


Figure 1: An example of the current framework for R&D appraisal

As a result, the actual costs and payoffs of R&D are asymmetric. Decisions to continue investing are made when the benefit of doing so is expected to outweigh the expense. Repetition of this decision process continually crops away outcomes that are unfavorable. Therefore, the expected cash-flows of an R&D project may not be at all representative of the actual value of the project. R&D derives much of its value from the sequence of incremental steps that the resolution of uncertainty stimulates.

Chapter 3. Project value in R&D Management

Thus far, it has been suggested that R&D is difficult to value because it is an uncertain, sequential undertaking that yields benefits that are not easily translated into estimates of return (revenue or otherwise). Building on these points, it is necessary to explore R&D in the larger organization in terms of its objectives, and to introduce the role of value in management of R&D.

3.1. Objective of R&D: Creating value and a platform of options

The objective of R&D is to create value, and the value that successful innovations produce is significant. Whether it is improving a product, making a process more "lean" or developing a new system altogether, innovation is at the heart of the effort. The firms and organizations that are better structured to encourage, utilize, and successfully capitalize on innovation through R&D are increasingly recognized as the leaders in their respective classes. On a national scale, Mansfield (1980) noted the powerful role R&D plays in generating wealth. This goes hand in hand with the assumption that funding basic science guarantees spillover economic benefits. This cause and effect relationship is crucial to public and private policy decisions, playing a role in the success of corporations and industrialized nations alike.

However, simply funding unbridled research explorations will not always yield valuable innovations. (Skolnikoff, 1993) In addition, the ability to devote resources to untargeted efforts no longer exists. Especially important from the point of view of the firm (and the government agencies that want to act more like business entities), the R&D best practice links technology strategy to a clear and integrated understanding of the overall business strategy. Taking this one step further, it has also been suggested (Iansiti, 1998) that the business and technology strategy be fused into one.

R&D also creates a platform of options from which a variety of technologies can be developed and inserted into a multitude of products. With this suggestion firmly fixed in the consciousness of everyone from the managers to the engineers and the research scientists, the focus of activities moves to the creation of complements for existing capabilities. Creating complements fills gaps in the capabilities of the firm, more readily converting R&D into commercial innovations. (Neely, 1998)

3.1.1. Differing project characteristics

Unfortunately, the process of selecting and guiding research and development projects is not so simple because R&D spans a wide range of short and long-term activities with different characteristics. Each of these tasks entails different timeframes and resource requirements. Additionally, the benefits of different R&D activities may accrue as easily as measured dollar returns, such as cost savings, complex sets of technical advances or intangible capabilities. (Shishko, 1998)

Projects are not chosen in a vacuum, they are clearly extensions of organizational culture and the predominant management philosophy. This is one reason why larger firms are often blind-sided by smaller startups that innovate outside the boundaries of current thinking. (Christensen, 1999) Those kinds of innovations are disruptive. They tend to unseat the current state of the art solution and set the product down a new path of use and a new cycle of innovation. Characteristics of disruptive innovation tell us something about the difficulties that a dedicated R&D lab faces in staying abreast of the current research and then selecting technologies that are advanced enough to be value-added, yet realistic enough to be developed. The true complexity of valuing R&D stems from the need to value a range of investments with differing characteristics.

3.1.2. Contingent nature of R&D

A major shortcoming of current R&D valuation practice is a tendency to ignore opportunities to revise plans, in response to new information, throughout the life of a project. In fact, the ability to react to change allows managers to exploit opportunities while limiting losses, and can dramatically increase the value of a project.

For example, net present value (NPV) is best suited for evaluating investments for which the timing and magnitude of future cash-flows are completely characterized in terms of a continuous distribution. In these cases, the common practice of using expected values yields a reasonable estimate of product value. However, *NPV often undervalues projects that* *include future decision stages, because these opportunities introduce asymmetries into the distribution of cash-flows that are ignored in practice*. (Pindyck, 1994) Since most R&D is highly contingent, NPV tends to understate its value. For instance, in case of unpromising results from a project, there would be more opportunity to avoid losses by stopping the effort before incurring the bulk of costs. Projects often depend on the magnitude and timing of resource commitments required to resolve uncertainties.

3.1.3. Selecting Projects for Research

The director of a prominent research institute once said, "One of the most difficult jobs any research director in industry has is that of selecting a problem which, if properly pursued, through research, could yield dividends to the company." (Harrel, 1948)

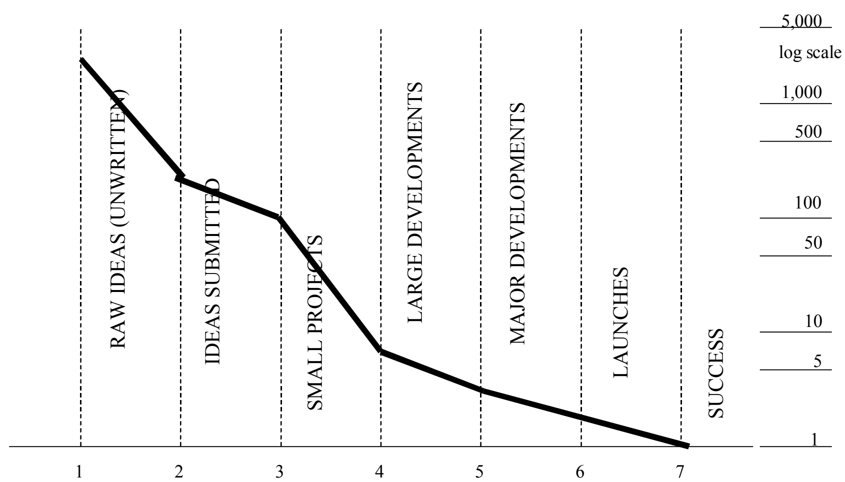


Figure 2: Ideas succeeding at each new product development stage

[Source: Industrial Research Institute, Washington, DC]

In the simplest abstraction, innovation decisions are just investment decisions. As such, innovation can be managed in terms of capital allocations. One would expect modern finance theory to give good general advice on how to manage investments into research and development. But a quick look a finance textbooks reveals answers that are based on a very stylized conception of the problem and rather less illuminating than one would hope. [Holmstrom, 1989 #47] The standard application of net present value to the decision to invest uses the expected future return stream from the contemplated project. That stream is discounted using a cost of capital that reflects the appropriate inherent risk. Projects are thus evaluated without regard to capital constraints. The model veers rather sharply from reality because firms are conscious of risk and capital constraints unique to their own situation. When there is a limited amount of money to be allocated among proposed projects, where demand almost always exceeds budget supply, idiosyncratic rules tend to be applied that match the options mentality closely.

Reports illustrated by Figure 2 suggest that you have to start with around 3,000 ideas to end up with four plausible new product development programs – the minimum needed to get just one winner. This emphasizes the need to make more informed decisions, if possible, at the outset and to be prepared to see the value in R&D as an information gathering stage in product development.

Chapter 4. EXISTING PRACTICES & NEED FOR IMPROVEMENT

4.1. Net Present Value

Net present value (NPV) is a standard financial valuation model that is commonly recommended for evaluating investment opportunities. The objective of the methodology is to compare positive and negative flows of cash over a specified timeframe for the purpose of judging the worth of a project that produces these flows. This requires that future cash flows be adjusted to account for the time value of money, since a future dollar is worth less than a dollar today. A process known as discounting converts all flows into current dollars. The discount rate acts much like an interest rate for a bank account enables the conversion. Adding the adjusted cash flows yields an estimate of current value. Equation 1 summarizes the NPV process.

$$NPV = \quad (Benefits_n - Costs_n) / (1+r)^n$$

Equation 1

In this model, *benefits* refers to positive cash flows, while *costs* are negative cash flows. The term n identifies the time when the flows occur, and r is the discount rate. NPV is the sum of the discounted benefits and costs.

While the magnitude and timing of benefits and costs is sometimes highly uncertain, often the most controversial step of conducting an NPV assessment stems from the selection of the discount rate. (Neely, 1998) The assessed value of an investment, its NPV, is frequently sensitive to discount rate choice.

Consider a project that requires an up-front cost and that returns a stream of benefits for several years. In the NPV model, the impact of the up-front cost is constant since the present value of a payment today is simply the full amount of that payment (i.e., cost incurred at n=0). In contrast, the influence of the benefit stream will decline as the discount rate increases. Thus, the project will look less attractive for larger discount rates. Technically, there is nothing wrong with this trend. However, managers tend to select inappropriately high discount rates to value projects. (Pindyck, 1994) For projects with benefits that occur far in the future compared to costs, such as many R&D efforts, this leads to an unfair bias.

NPV as commonly applied can understate project value because NPV tends to assume commitment to a project is irreversible. Therefore, it estimates project value using expectations of cash flows that do not consider the influence of future actions. Unfortunately, this handicaps the valuation process, as decision opportunities can be significant sources of project value.

4.2. Decision Analysis

Decision tree analysis is a potentially useful methodology for evaluating projects with options because it provides an easy to follow, structured view of projects. In this role, it can act as a framing device for focusing valuation efforts on identifying valuable project options and thereby avoid the pitfalls of certain other approaches. Unlike the prototypical master plan, which commits to a pre-defined set of activities, decision analysis recognizes that only uncertainty resolution reveals what is most appropriate in subsequent stages. As a result, it emphasizes the creation of valuable options in projects. (de Neufville, 1990)

Decision analysis is a flexible methodology that can accommodate much more complicated valuation problems. It is possible to add more decision points and to increase the number of discrete outcomes that are considered. However, the need to quantify probabilities and outcomes, which can be difficult for R&D, is a commonly criticized shortcoming of decision analysis methods. Although there are formalized procedures to estimate probabilities, critics counter that subjective probabilities are hard to gauge with much precision. (Rosenberg, 1994)

5.1. Implementation

Real options is another method for valuing projects with future decision opportunities. It is based on models that are used to value financial instruments, and tries to value projects correctly from a finance perspective of obtaining satisfactory level of return for a given level of risk. In contrast, decision analysis or NPV may only approximate project value. However, real options models may face other limitations. For example, some models make assumptions about the underlying asset that may not hold for these types of projects. Table 1 below suggests appropriate metrics for the different types of R&D defined by Hauser.

R & D Spectrum

	Tier 3	**Tier 2**	**Tier 1**
Example Projects	Problem Fixes Cost Reductions	Functional Improvements New Cost and Functional Frontier	Blue Sky (Basic R&D)
Appropriate Metrics	PAYOFF FOCUSED NPV or Real Options	MIX: PAYOFF FOCUSED and EFFORT INDICATING NPV or Real Options and Patents, Publications, etc.	PORTFOLIO APPROACH Negatively Correlated Objectives and High Variance Projects
Rationale	Projects aligned with business goals create complements to existing capabilities	Part of contribution is creation of capabilities that $ metrics miss	Mitigates risk, but projects should support technology strategy

Table 1: R&D Spectrum and appropriate metrics [adapted from Neely, 1998]

5.1.1. Purpose

The Real Options approach to technology investment decisions has several attractive features:

(1) When comparing projects, it tends to favor more risk because there is more opportunity to create value
(2) It provides a robust, disciplined method
(3) Used along with other models, it addresses several endogenous and exogenous factors not addressed by another single methodology (i.e. incorporation of expert opinions, timing)
(4) It avoids escalation of commitment by capturing the value of such actions as abandonment or waiting (taking no action has consequences as well).

Despite the increased academic attention, the extension of financial options has yet to be found in a truly broad range of "real" applications. Some of the more successful examples are the petroleum and pharmaceutical industries. Both those enterprises benefit from real options because of their shared characteristics of very expensive, time-consuming periods before or during development with high risk and uncertainty, but the potential of dramatic payoffs. But additional reasons exist for the successful applications of options in particular industries.

The two industries mentioned above, petroleum and pharmaceuticals have some inherent features that lend themselves to real options analysis. For instance, structural symmetries between the pharmaceutical industry and options models exist in the sequential nature of drug testing. (Lewent, 1998) In the petroleum industry as well, the process of exploration and discovery is rather well adapted to options. Furthermore, petroleum is a simply valued commodity with quantified historical volatility, etc. This study will attempt to look at complex technology projects that, unlike the aforementioned, are difficult to structure or quantify but seem to be equally in need of a similarly rigorous valuation methodology.

5.1.2. Real Options and "Lean Thinking"

The Lean Aerospace Initiative (LAI) is dedicated to "significantly reducing cost and cycle time throughout the entire value chain while continuing to improve product performance". The Product Development (PD) focus team of LAI studies the problems associated with the front end of system design. The very "fuzzy" front end of this area of study asks how choices are made in selecting and investing in technology or projects (technology and project are used interchangeably here). The stage of product development where these decisions are made is crucial to the later stages and, in general, is most important to the overall enterprise, as it is where a large portion of resources are used up. Hence, this study focuses on the front end and specifically, how resource allocation decisions are made.

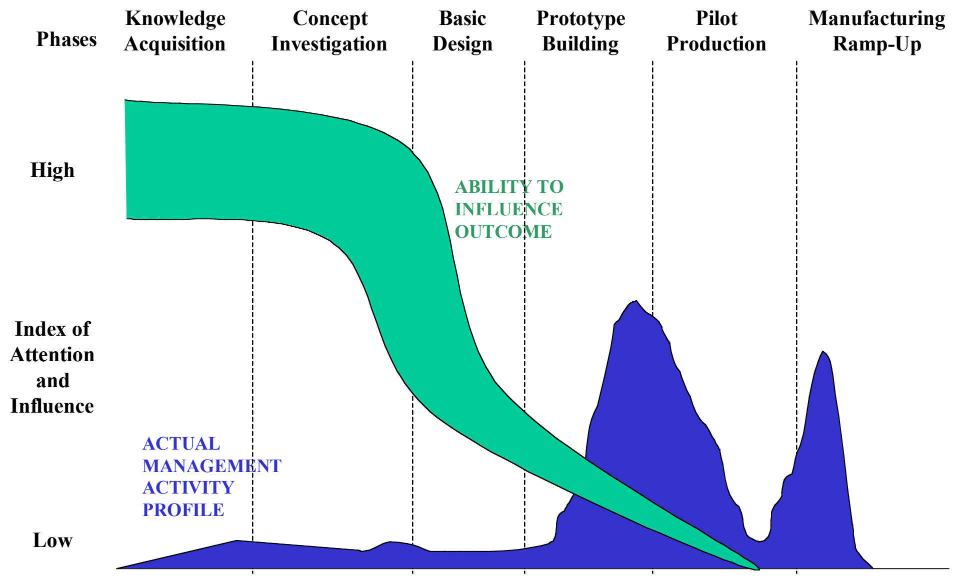
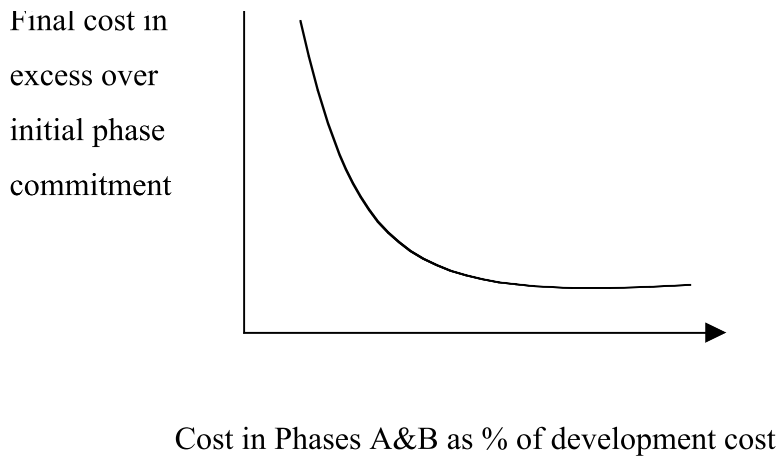


Figure 3: A Dilemma: the Timing and Impact of Management Attention and Influence

5.1.3. Thesis Objectives

A basic objective of this thesis is to explore how decisions are made when investing in technology development projects. Then the real options approach is suggested as a possible method that would discipline the process of capital budgeting for technology projects (the terms technology project and R&D are interchangeable here). A perspective which has strongly influenced the framing of this thesis is the view that the ultimate benefit to a firm or organization of R&D investment can be improved if the decision tools were improved. Given this perspective, it also becomes apparent that the tool heavily influences how decisions are made. *Thus, changing to a real options approach would seem to imply a fundamental shift in managers' intuition, or, a rapprochement of intuition and the results given by the tool.* (Faulkner, 1996) As the latter effect seems to be the case, it can be said that an "options mentality" is intuitively worth developing, and can be proven quantitatively to be a more valuable way of approaching specific types of decisions.



A second basic objective of this research is to apply the real options thinking in a new context. To date, it has been explored in academia through a theoretical lens, and in a variety of industries and firms. While it has been recognized (Faulkner, 1996) that real options would be effective as a valuation and decision tool for corporate R&D, this assertion has yet to be tested against some of the most important R&D today – **federally funded R&D**. Federally funded R&D occupies a prominent place in the U.S. innovation machine, but is facing decreased funding and, at the same time, increased pressure to justify spending and demonstrate concrete results more quickly. These characteristics make public research a perfect candidate for improvement with real options. To demonstrate simultaneously the importance and the increased pressure on the R&D contribution from government installations, one only needs to look at the expenditures. Federally funded R&D in 1996 totaled approximately $21 billion, which was about 17% of the total (private and public) R&D performed for the year. In the 80's 32% of R&D was paid for by the government, and in the 50's, over 50%[1]. The distinction between industrial and public R&D really comes into play in looking at the ratio between the "R" and the "D". Seventy five percent of industrial funds are devoted to development while government obviously focuses on research. What the federal government does in development work tends to focus on very advanced "products" (i.e. spacecraft). What part of federally funded R&D does this study focus on? It was mentioned earlier that the "fuzzy front end" of the process receives both the least attention and the most resource commitments within a project lifetime. Figure 4 illustrates that in the NASA scheme

[1] The American Association for the Advancement of Science (AAAS) keeps detailed annual data on R&D spending.

of project phases, the ones that get the funding (and supposedly the management attention) in early phases A&B tended to stay within budget. Although the relationship of R&D to eventual project success is not necessarily quantifiable in this way, we seek to investigate the decisions that are made at the outset of a project. The decisions made early on include what technologies to study, how to allocate resources and when to insert them, or productize them. Finally, Figure 3 reinforces the importance of the front end by showing that as ability to influence outcome decreases, the attempts to influence trajectory increase, and so does spending.

5.2. Real Options Theory

5.2.1. Understanding Options

Option pricing has already been applied to a variety of investment decisions by firms. When option-pricing applications do not involve financial instruments, the term, "real" options, is used. Real-option calculation methods have been developed for electric power investments, and for product development in the pharmaceutical and entertainment industries. Company research and development (R&D) funding can be thought of as buying an option to produce new products, without incurring the obligation to do so unless proved economically viable. *In option pricing thinking, technology developments are treated as assets whose payoffs are uncertain, but have the characteristic of enabling potentially impressive returns with limited losses.* (Shishko, 1998)

	Merck	**Kodak**	**NASA-JPL**
R&D Process	Homogenous	Heterogeneous	Heterogeneous
Product	Single Drug	Photo Equipment, Film, Color Printer	Spacecraft, Missions, Spin-Off Technologies
Project Financial Benefits Estimation	Easy (Project=Product)	Moderate (Project~Product)	Difficult (Product<Project)
Functional Champion	Finance	R&D	Management/ Administration/Federal Government
Framework Choice	Real Options	Decision Analysis	Modified Decision Analysis (Options-Thinking)

Table 2: Options Analysis Cases Applied in Several Industries [based on Neely, 1998]

The NASA-JPL case in the table above is identified as requiring a hybrid framework of real options and decision analysis. The parameters leading to that choice are:

- A heterogeneous R&D process, where many different disciplines are brought together in the product development process, and;
- Extremely difficult valuation of the project benefits in financial terms.

5.2.2. Financial Origins of Real Options

The original idea for Real Options, born in 1977, was to value discretionary investment opportunities as growth options. The analogy between financial options and corporate investments that create future opportunities is both intuitively appealing and increasingly well accepted. Executives readily see that today's investment in R&D, or in a new marketing program, or even in a multiphase capital expenditure can generate the possibility of new products or markets tomorrow. (Luehrman, 1998)

There is widespread dissatisfaction with existing methods of resource allocation (Trigeorgis, 1996) because decisions are often made in spite of, and not because of, those methods. In reality, managers are willing to disregard their current techniques in order to accommodate common sense flexibility. Traditional approaches cannot properly capture management's ability to adapt and revise decisions in response to unexpected developments. Not only is there a necessity for flexibility in dealing with events precipitating because of changes in the state of the world, but there is inherent uncertainty at the outset of a project. That uncertainty can be viewed as a negative factor, or in the new model, as a potential for added value. It is certain that as new information becomes available, uncertainties will gradually be resolved and valuable options will emerge.

Managerial operating flexibility is likened to financial options. A call option on an asset (with current value V) gives the right, with no obligation, to acquire the underlying asset by paying a prespecified price (the exercise price, I) on or before a given maturity. The option will be worth max (V-I, 0). The asymmetry derived from having the right but not the obligation to exercise the option lies at the heart of the option's value. (Pindyck, 1994)

Adaptability causes asymmetry by improving the upside potential, while limiting downside losses *relative to the initial expectations of a passive management*.

Option Pricing Theory can conceptualize and quantify the value of alternatives from active management and strategic interactions. Value is manifest as "real options" embedded in capital investment opportunities, having as the underlying asset the gross project value of discounted expected operating cash inflows. There are several strategic courses represented by options, including the ability to defer, expand, contract, or abandon. The actions can be further refined to staged investments, alteration of operating scale, growth options, and multiple interacting, or compound options.

Real Options can be seen operationally as a special, economically corrected version of decision tree analysis (see figure below) that is better suited in valuing a variety of strategic options. (Trigeorgis, 1996)

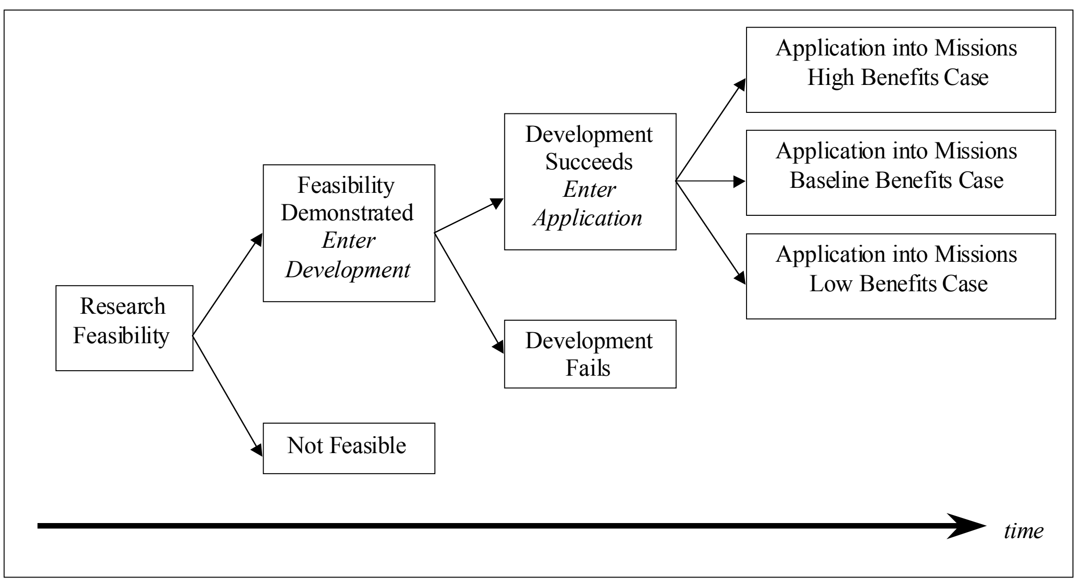


Figure 5: R&D Stages and Decisions

5.3. Failure of Traditional Capital Budgeting

> "A fascinating aspect of flexibility options is that in certain cases it is possible to estimate their value precisely. Often, the extra value added by flexibility is completely missing from such traditional valuation methodologies as net present value (NPV) techniques. In fact, one

> contributing factor to underinvestment in the United States may be the slavish dedication of its MBA-trained managers to NPV. Have you ever sat at a meeting and listened to a careful NPV analysis, known in your gut that the recommendation had to be wrong, but could not put your finger on the reason? The missing ingredient may be the value of flexibility."
> [Tom Copeland and Jon Weiner, *The McKinsey Quarterly*, 1990]

For most private firms, the failings of traditional capital budgeting involve incorrect discount rates and inadequate discounted cash flow techniques. However, when dealing with non-profit, technology driven bodies -their goals and metrics, and thus their problems are very different. In the aerospace environment studied by this paper, it is unlikely that decisions are made with DCF techniques and it is certain that return on investment (ROI) cannot be measured solely in dollars. The failures that can be discussed in this specialized arena are the inadequate integration of the measureable with the intangible, meaning capital budgeting with strategic planning. There is also a trend toward decentralization of decision making and compartmentalization of divisions accompanied by decentralized resource allocation. It is difficult to recognize remote, intangible, or contingent benefits spread across an entire organization. (Trigeorgis, 1996) In general, opportunities tend to be undervalued, leading to myopic decisions and underinvestment.

Because the ROI/DCF method does not fully deal with uncertainty and fails to capture the value of unforeseen spin-offs, among other inadequacies, Mechlin and Berg observed that some research people referred to ROI as "restraint on innovation". Hayes and Abernathy *blamed ROI/DCF analysis in part for the decline in R&D spending in the U.S. that threatens the long term health of the country*. In astounding findings published by Hayes and Garvin, they found that the proportion of companies using DCF rose from 19 percent in 1959 to 94 percent in 1975 (see table). The rapid adoption of DCF methods by companies over this 16 year time period coincided with the decline of both R&D spending and capital investment. Hayes and Garvin suggested that these declines were a consequence of managerial failure through misuse as well as the inherent shortcomings of DCF techniques. Examples of this misuse include: high hurdle rates, failure to treat a series of investment decisions separately, and assuming that investments can be delayed with no penalty other than that implied by the discount rate.

	Proportion of Companies Using DCF	***Average yearly augmentation in spending*** *(10-year period)*
1959	19%	10%
1975	94%	(2%)

Table 3: R&D spending stagnates as Discounted Cash Flow (DCF) is adopted between 1959 and 1975 [from Hayes & Abernathy]

Hodder and Riggs focused on the assessment of risk where a single discount rate is typically applied to the entire business case, but the actual level of risk may vary substantially in the different phases (research, development, commercialization). Hodder and Riggs used an example to demonstrate how this is likely to result in a bias against R&D investments. Kaplan used CIM (computer integrated manufacturing) investments as an example and observed that DCF techniques typically fail to capture the value of the "intangible benefits" like increased flexibility and faster learning. Kaplan pointed out that a decision to neglect "intangibles" is, in fact, a decision to value them at zero.

The proposed solution to the failure detailed above, therefore, is to map a project onto an option. In a strategic decision, where an organization has the right, but not the obligation, to acquire something, if we could construct a call option sufficiently similar to the investment opportunity the value of the option would tell us something about the value of the opportunity. Projects involve spending money to build or buy a productive asset. Spending money to exploit such an opportunity is analogous to exercising an option. What differentiates options from a standard NPV analysis is the ability to defer an investment decision. Deferral gives rise to a source of value that NPV misses because it assumes decisions cannot be put off: resolution of uncertainties as more information becomes available. This point recognizes that the state of the world can change. Specifically the value of operating assets can change, so it is in the firm's interest to preserve the ability to participate in good outcomes, and insulate itself from bad ones. Development of knowledge about technical requirements, capabilities and risks becomes particularly valuable in uncertain, long-term projects requiring large capital investments. Those project characteristics are common to the pharmaceutical and aerospace industries in particular, which have the added commonality of heavy government involvement. In these types of industries, instead of measuring added value directly, we can measure uncertainty and let the Option Pricing Theory (OPT) model quantify the value associated with a given amount of uncertainty.

The motivation for using real option pricing to value technology developments in NASA is the same as in the private sector. In the words of Robert C. Merton in his December 1997 Nobel Lecture: "the future is uncertain . . . and in an uncertain environment, having the flexibility to decide what to do after some of that uncertainty is resolved definitely has value. Option-pricing theory provides the means for assessing that value."

In the private sector, the ultimate products of technology investments (for example, new drugs in the pharmaceutical industry) have the important characteristic that they are private goods that go through ordinary markets. As such, there is a substantial likelihood that consumer demand information is available with which various investment outcomes can be converted into monetary units. In the case of NASA investment in advanced technologies, the ultimate products, which must be used to justify the investments, are space-related scientific results and discoveries to be shared worldwide. That one person's consumption of these products does not diminish another's is an attribute that is ordinarily descriptive of a pure public good. A formal option-pricing calculation method for NASA technology investments must consider the public goods aspects of the ultimate products and the problem of valuing their benefits to society. It must also treat the underlying sources of uncertainty on both the cost and benefit sides.

5.3.1. Options Pricing

Of the many kinds of options that are traded today, the most relevant to the valuation of R&D is the "call option". A call option for a common stock can be thought of as a contract where the purchaser of the option obtains the right to buy a number of shares of that stock at a specified price on a specific future date. When that future date arrives, the holder of the option will "exercise" it if the market price of the stock is higher than the price specified in the option contract and will make a profit proportional to the difference between those tow prices. If the market price of the stock is lower than the option contract price, the option holder will allow the option to "expire" and his loss will be limited to the amount originally invested in the option. Two of the interesting characteristics of a call option are that the potential value is a function of future uncertainty and that there is a limit to downside risk to which the option holder is exposed. Because of the existence of the limitation on downside risk, increases in uncertainty about the future price of the stock *increase* the value of the option (Faulkner, 1996).

5.3.2. Dynamic Programming for Multiple Options and Decisions

The extendable call example demonstrates a method for considering multiple decisions during the life of a R&D project, but focuses only on continuation options. Kulatiliaka (1993) and Trigeorgis (1996) present more general dynamic programming methods that can simultaneously consider multiple decision stages and unlimited sets of project options (i.e., accelerate, suspend, delay, continue, abandon, etc.). This approach is useful because it is extremely flexible in accommodating a large variety of options. However, complex numerical methods are required to conduct the simulations, and consequently the methodology is much less intuitive.

5.3.3. Literature

The body of literature reviewed for this thesis covered several disciplines in an attempt to accomplish three things:

(1) To survey the fields of interest and narrow the study to an area where an original contribution could be made within the parameters of the sponsoring organization. The survey quickly brought about four main areas to focus on: Decision-Making, Real Options, R&D Metrics, and Policy;

(2) Once identified, to understand the background and theory behind real options, in a variety of applications; and

(3) To build a case exploring the use of real options in the particular instance of technology project investments.

In the area of real options, the theory has developed from a highly mathematical financial tool into a quantitative business analysis tool well suited to applications like oil exploration where it is especially important that the value of a barrel of oil on a publicly traded market gives a certain dollar amount for the underlying asset and a history of prices to determine volatility and hence uncertainty. The next logical move is to broaden the applications. This being done as different types of enterprises and organizations adopt real options, but with a different approach. Often dubbed options "thinking" or options "mentality, the theory has proven to be very useful in altering the mindset of decision-makers and shifting the ideas of where value lies and how management decisions can be made. Options thinking uses the fundamental intuition that it develops, backed up by the quantitative rigor of the model, to facilitate better decision making. Several articles pursue this path of identifying the intuitive strengths developed by real options. One of the most lucid is by Terence Faulkner of Kodak, who recommended that options thinking "become our new paradigm". In his 1996 paper, "Applying 'Options Thinking' to R&D Valuation", Faulkner acknowledges the following:

> "Our experience within Eastman Kodak over the past few years has confirmed that the use of options pricing theory concepts brings valuable insights into the R&D valuation process…technology planning and the formulation of R&D strategy are enhanced by understanding of the implications of options pricing theory."

The existing method for most businesses making capital budgeting decisions, discounted cash flow (DCF), referred to here as the "traditional approach", came under question when a *Harvard Business Review* article in the early 1980s raised doubts about the biases of the methodology. (Myers, 1982) The DCF and ROI (return on investment) metrics were found to harm research productivity in private R&D. Thus, even though publicly funded R&D does not have the ability to use DCF or ROI, the question remains, could it benefit from a better approach? In such an environment the options pricing theory (OPT) might be more useful because of the characteristics of the projects, their current selection methodology and the importance of resource allocation in the publicly funded R&D system.

Stewart Myers of the MIT Sloan School may have been the first to suggest that options pricing theory should be applied to the valuation of R&D. In "Finance Theory and Financial Strategy" he observed that *"DCF is no help at all for pure research and development. The value of R&D is almost all option value."* Brealey and Myers developed this view at much greater length in their *Principles of Corporate Finance, Third Edition*, by presenting a procedure for using options pricing theory to value R&D investments. Brealey and Myers' account of the development of options theory focuses on its impact on economic thinking. Efforts to develop a process for valuing options go back at least to the beginning of this century. When Fisher Black and Myron Scholes developed a formula for valuing options in 1972, they transformed the field. Only one year later the Chicago Board Options Exchange was founded and quickly became the world's second largest securities market.

5.4. The Role of Real Options in R&D Management

5.4.1. Where to Apply Options

In a business environment, a decision invariably implies an expenditure. Wherever there are choices to be made about investments during the development activity, an option is created. In this study, the relationship between a technology that might typically be the product of R&D would be applied to a project. This implies that a platform investment is being made and that the technology's intrinsic value cannot be realized until it is productized for insertion as a component of an ensemble.

5.4.2. Value Increases as Uncertainty is Resolved

Drug development can be generalized to R&D because the process is modeled as a sequence of **learning investments** and **abandonment options.** We can look at the application frame of the R&D case in terms of sources of uncertainty. An indicator of uncertainty that is especially significant in development programs is the remaining life cycle costs. As expenditures are made, bad news will increase the remaining costs, so this variable becomes a way to model scientific or technical risk. Technical risk is internal to the firm and therefore is termed "private". The notion of private risk introduces a difficulty in the options analysis. Because the rigorous approach uses a market-tracking instrument, "public" risk factors are preferable. They are more easily quantified and allow the "apples to apples" comparison of, for instance the market for airliners, a fairly known quantity based on historical data. However, it is obvious that the firm has more control over the "private" risk elements. There are levers (McKinsey Quarterly, 1998) that can affect the parameters mitigating risk. Thus, if technical risk can be modeled by remaining costs it follows that those costs can be avoided by abandoning the development process. As with the value of the market, uncertainty about the remaining life-cycle costs can be modeled as resolving with expenditures over time. Intuitively, it makes sense that money spent up-front gaining information will reduce uncertainty.

Projects are thus reduced to a series of investment decisions at predetermined milestones with the aim of gaining more information. With learning, the organization seeks to reduce uncertainty to an acceptable level for technology transfer, productization or further development. The elements of structure that are the key to capital budgeting decisions are the development phases (labeled "stage-gates" by management academics) and the estimates of technical success. First, the structure of the development process itself defines the decision "space". By establishing milestones at which a project passes from one phase to another, a temporal expectation of progress drives the process. By characterizing the metrics by which the project will be passed forward and deemed worthy of further funding, management drives the project. Estimates on the probability of passing from one phase of testing or development to the next and the risk

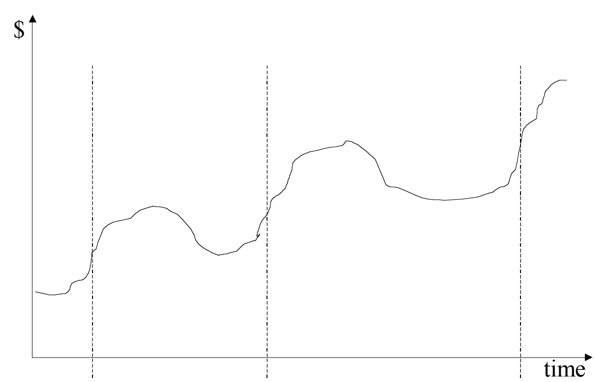


Figure 6: Options just before peaks in spending are particularly valuable

Options are created by investments but they also precede investments. One of the sources of option value is the capturing of the opportunity to abandon a project and not make a follow-on investment after the initial. Thus, options just before the peaks in life cycle costs are particularly valuable because they prevent regret and further escalation of commitment.

The figure above can also be compared to Figure 3 where it was shown that management activity is negatively correlated to management ability to influence outcome. As the influence level diminishes because of design and production commitments, the amount of money spent in attempting to influence the outcome increases at each phase.

5.4.3. Design Options

The process of design has been described as a funneling procedure (Clark 1993). The funnel analogy is useful in understanding a trajectory of events. As the design matures, the first funnel in figure 11 shows how design options are removed until what remains best satisfies requirements. However, premature decisions can diminish design flexibility too rapidly, requiring a step backward in the process (Krishnan, 1997). It is in this context that recent research (Ward, 1995) found that Toyota’s set based design process delays decisions as long as possible in order to retain maximum flexibility. Although maximum flexibility may not always be cost effective, by not making decisions prematurely, performance risk is reduced.

Risk Assessment in Decision Appraisal

The firms that employ risk analysis are very much in the minority (Ho, 1992). In ranking new capital projects, risk is often used to describe, characterize and rank opportunity. The investment appraisal criteria most commonly used stem from financial techniques. They are Net Present Value (NPV), Internal Rate of Return (IRR), and Payback period. Their intricacies will not be covered here as they are well explained in references like Myers and Brealey (1997).

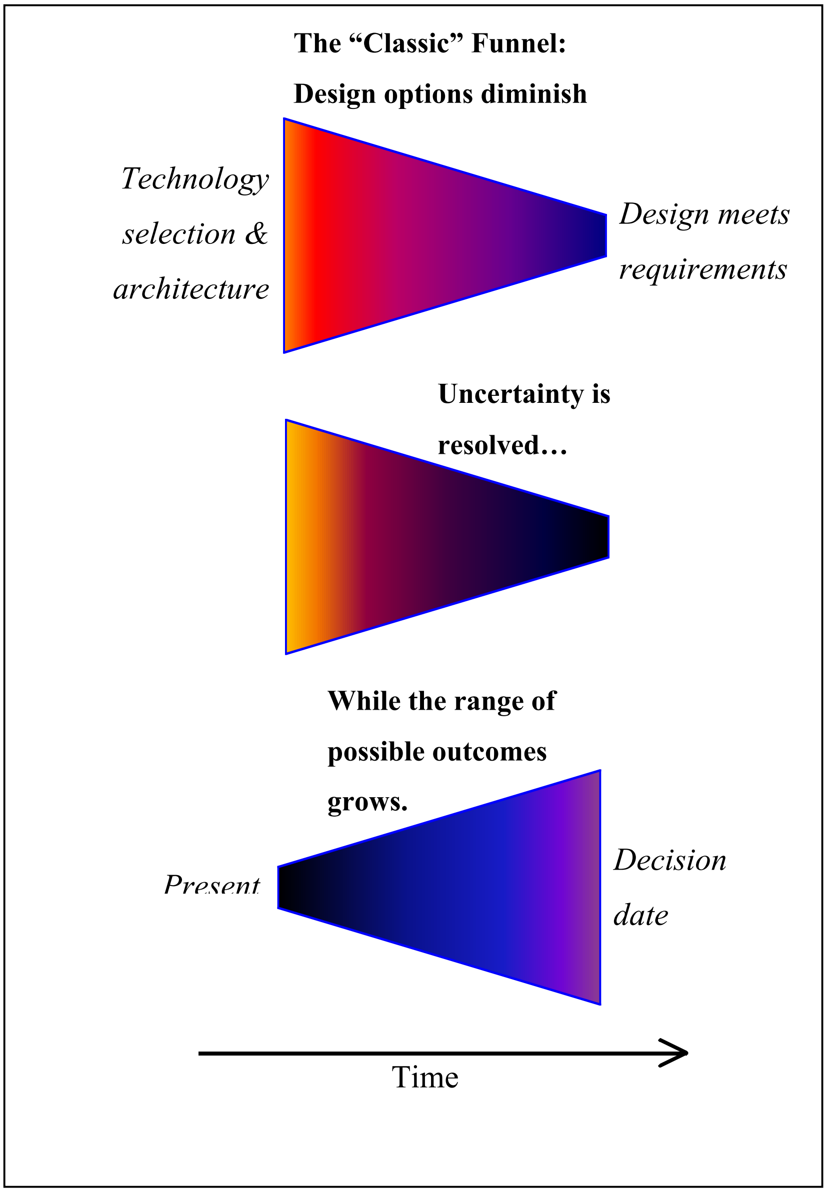


Figure 7: Managing Uncertainty in the Product Design Process

The operational model for handling strategic risk, however, does suggest that investment decisions go through a logical sequence. First, risk identification, then risk management and reduction, and lastly, risk evaluation – incorporation in the final project decision process. After measurement of risk at both project and portfolio level, a decision maker needs to judge whether some or all of the inherent pure risks can/should be avoided, reduced, tolerated, or accepted. If the firm cannot tolerate some of the risks it would probably have to identify some methods to protect against unfavorable outcomes. If the project were still perceived as too risky, it would be more likely to be rejected. Otherwise, the decision-maker then has to make a risk-return tradeoff decision and decide to what extent the residual risks can be accepted or compensated with a higher return. Finally, the manager must make an overall judgement about the project, and ultimately a go/no-go decision.

5.5. Decision Making Processes

The options model is particularly valuable for identifying and exploring issues in capital budgeting issues. Even if it is not the unified solution that we might have expected because it did not translate well from the commercial world, it does shed light on the process of making a decision. An important differentiation between smart decisions and unsuccessful guesses is proactive management. In Smart Choices, Hammond et al put forth the essential elements of a decision. Those elements need to be carefully identified and analyzed in a conscious way in order to make the right strategic moves. The eight elements of smart choices are show in Table 4.

PRoblem	Work on the right decision problem.
Objective	Specify the firm's objectives
Alternatives	Create imaginative alternatives.
Choices	Understand the consequences.
Tradeoffs	Grapple with the balance between competing objectives met by the different alternatives.
Uncertainty	Clarify the likelihood of different outcomes and assess their possible impacts.
Risk Tolerance	Think hard about your risk aversion and how much and what type you are willing to assume.
Linked Decisions	Understand the interconnections between decisions. Isolate and resolve near term issues while gathering the information needed to resolve those that will arise later.

Table 4: Eight Elements of Smart Choices [from Hammond et al, 1999]

Uncertainty is an integral part of the decision-making process, but is often avoided. Options take uncertainty into account and are therefore better tools to plan and act when large investments need to be made. Because managers know that risk is always present they often make unrealistic assumptions about benefits (cash flows) which reduces the credibility of a decision. Also, strategy, vision and planning are usually taken into account separately when they should be combined (Khurana & Rosenthal). Options analysis gets closer to the intuitive way that managers react. The apparently slow adoption of the method outside of academia is probably due to the focus on the equations, which are complex. If the approach is presented as a simple tool -a mentality perhaps- then it will be more readily used.

Types of Options:		**Types of Investments:**
Waiting to Invest		Irreversible
Growth		Flexibility
Flexibility		Modular
Exit (Abandon)		Platform
Learning		Learning
		Insurance

Table 5: Investment decisions are made within a framework of options and investment types

NASA (publicly funded R&D) does not use DCF to make investment decisions. This poses a problem because the majority of for-profit firms do use it and therefore it is used as a benchmark to evaluate the advantages of real options. The solution is to use other methods, utility functions, cost savings, benefit metrics as replacements for the cash flows that are commonly applied. Some of those other methods are discussed below. Each has its advantages and limitations for application to the particular problem of appropriate resource allocation in the absence of a financial metric. Utility functions are discussed at length because of their ability to take a variety of opinions into account. In this case that ability would be a great asset because the different stakeholders' inputs could be weighed and inserted into the analysis. Scientists, engineers and the taxpaying public could be polled for the value they place on timely science, for example. Decision makers then have the tool necessary to value a publicly-funded investment that is supposed to return intangible "goods". Utility functions, however, are limited by the fact that subjective questions are asked and arbitrary weightings of different opinions can make the results rather sensitive. It must also be noted that established technical performance measures can be used as the ultimate quantification of value in this case. Finally, we look to the case study, which assumed fixed performance with only cost savings taken into account. That way, cost savings became the value metric.

5.6. Utility Functions

A specific solution to the problem of R&D or technology valuation could be the utility function described below. By eschewing the monetary valuation and instead weighting the subjective worth of a technology or a mission to the stakeholders, perhaps a more appropriate metric can be established.

Several factors are important in modeling the attractiveness of alternative research and development options. These factors include:

- The development options (make versus buy, technological development within or without a project)
- The probabilities of success and failure for each development option
- The management alternatives for the project given the success or failure of specific technology developments (e.g., descoping and replacement possibilities)
- The value of the mission given the different management options (i.e., how each option affects cost, schedule and performance
- The consequences for the whole program of the potential descoping of a project due to failure to complete the R&D portion of it with budget and schedule.

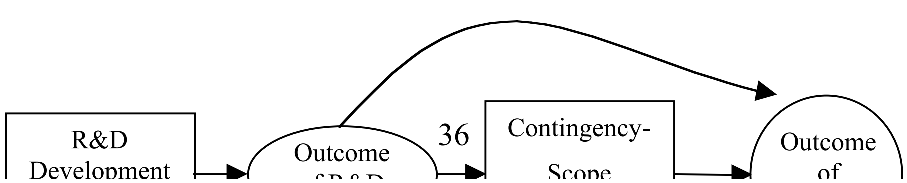


Figure 8: Preliminary Model for Development of Technology (Influence Diagram) [Pate-Cornell and Dillon, 1998]

Illustration:

Assume for example, that one of the objectives of Mars Pathfinder (MPF) was to demonstrate a low-cost entry, descent and landing system for future Mars landers (i.e., airbags). NASA had two options:

- Development and demonstration of the airbag technology with in MPF
- Development of airbag technology with other resources for demonstration on a future Mars mission

Each development plan has probabilities of success and failure based on the time and budget resources available to the programs. Define the following:

- Probability of successful airbag technology development given that R&D is part of MPF project [P(S|O1)]
- Probability of successful airbag technology development given that the R&D is placed outside of project constraints [P(S|O2)]

Given failure of R&D within a project, managers have various contingency options. For example, in case of failure of the air bag development program within MPF, mission managers could have decided to replace landing on airbags by a Viking-type propulsive descent at an additional cost of approximately $20-30 million. Each contingency option affects the schedule, cost and performance of the mission. In particular, changing to a very different EDL late in a project would have had major effects on the mission cost and schedule. Also, because one of the objectives of MPF was to demonstrate a new low-cost EDL, “descoping” the airbag technology would have downgradeed mission performance. Firgure 4 show the structure of an illustrative decision tree representing the choice of developing and EDL system with or without a mission.

The problem with completing the evaluation is that a utility function is required that can combine the different attributes of the problem: the direct costs to MPF, the value of the new technology to MPF, and the value of the new technology to future missions.

This model with the appropriate utility function can be used to evaluate different R&D options by comparing the corresponding probability of completion within a mission, the probability of technical success after the mission is launched, and the long-term effets of each option on the rest of the space program. One can then maximize not only the probability of R&D success and the value of the mission, but also the long term benefits of technological development for a whole program.

Are the FBC projects really better? Answering this question requires an in-depth look at the mission product because the ultimate product of unmanned space missions is not the spacecraft and its instruments, but the data, and the scientific knowledge and public interest derived from these data. Furthermore, people prefer –or find more value in- acquiring knowledge earlier rather than later and with higher certainty. “Better” is thus measured in terms of (1) quantity and value of the data gathered, (2) the probability of getting the data (i.e., the chances of mission success), and (3) the timing of the results.

Four important management decisions, each made by a different group of stakeholders, influence the scientific value of the mission:

(1) the selection of the mission scope through a political process, which determines the nature and the timing of data collection and sets the constraints for the selection of instruments (planetary constraints and mass available),
(2) the selection of the instruments within the scientific community, which determines the types and quantity of data,
(3) the risk management decisions by the engineers, which determine the probability of getting the data by affecting the reliability of spacecraft and instruments, and

(4) the operational decisions made on the ground before and during the mission affecting the hierarchy of priorities when spacecraft resources are assigned.

Figure 9 represents an influence diagram showing the relation ships among these factors.

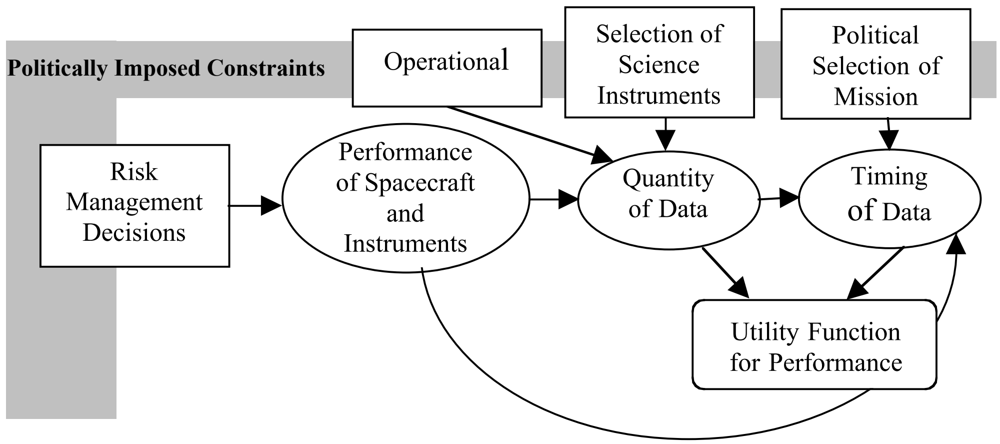


Figure 9: Preliminary Model for Value of Science (Influence Diagram)

[Adapted from Pate-Cornell and Dillon, 1998]

Two factors, the expected quantity of data (considering the probability of acquiring them) and the timing of the data, need to be expressed as a single value to permit performance comparison across alternative projects. Since the data are gathered, stored, and transmitted to earth in bits, their quantity can be measured in bits of different types at different times. One can divide the data according to the different types of scientific information, for example:

$n_{1,n}$: bits of planetary imaging at time n

$n_{2,n}$: bits of atmospheric data (non-imaging) at time n

$n_{3,n}$: bits of geosciences data at time n

$n_{4,n}$: bits of fields and particle data at time n

$n_{5,n}$: bits of data on small bodies (comets, asteroids, and interplanetary dust) at time n

The timing of data is important because scientific information is worth more now than later. The model must therefore involve a discount rate (r) similar to a financial discount rate. As for all commodities acquired with money, this discount rate can be assumed to be equal to the monetary discount rate if one believes that the value of the data will be the same to the public and to the scientists at the time they become available (i.e. the timing is the only thing that changes). This assumption of course may not be correct if similar information can be acquired between now and then by other means, or if the same data can be acquired at much lower cost (in constant dollars) later because of general technological progress.

The relative value of the different types of data is also a key factor. The overall value of the scientific information is represented (for decision analysis purposes) by a utility function that integrates both the variety of data types, and the attractiveness of the results to different groups in the population. One possible form of the utility function is a linear combination of its factors based on weights that represent (1) the contribution of the field of data to NASA's strategic objectives and (2) the value scientists and lay people place on the different types of data.

Notations:
w_1: weight placed on planetary imaging data
w_2: weight placed on atmospheric data
w_3: weight placed on geosciences data
w_4: weight placed on fields and particle data
w_5: weight placed on small bodies data

The value ("utility") of scientific data for the project is then the sum of the different types of data, discounted for time, and weighted for their value to the public at large (i.e., a

$$U = w_1\left(\frac{n_{1,1}}{1+r}+\ldots+\frac{n_{1,n}}{(1+r)^n}\right)+w_2\left(\frac{n_{2,1}}{1+r}+\ldots+\frac{n_{2,n}}{(1+r)^n}\right)+\ldots+w_5\left(\frac{n_{5,1}}{1+r}+\ldots+\frac{n_{5,n}}{(1+r)^n}\right)$$

combination of the preferences of scientists and lay people):

Equation 2 .e.,

that acquiring data of one type does not modify the value of the others. This may not be true if there are dependencies among some of them, and this function may have to be revisited if that is the case. As we discuss further in this paper, this utility model can be assumed to be linear with respect to the amount of data acquired because risk neutrality is a reasonable (and logical) feature of the FBC mode of operation.

The function U represents the preferences of a single decision maker or a homogenous group. When several groups are present, a second function is necessary to represent the relative weight of the different groups in the overall valuation of an option. There again, one often chosen option is a linear combination involving weights that represent the influence of the different groups of stakeholders. If this linear function is justified, the group valuation function V can be written as the sum:

$$V = \quad {}_{i}\, k_i \left(U_i \right)$$

Equation 3

In which the U_i's represent the utilities of each of the different groups.

Although the utility function was not applied to the case study, it is a valuable method of determining subjective worth of a project. It is especially useful in the case of R&D where financial determinations are hard tom come by, but the stakeholders have intuitive, subjective knowledge of the benefits that may be accrued by such a project. In this case, a carefully weighted utility function analysis might circumvent the weaknesses that will be explored in the proceeding.

Chapter 6. CASE STUDY

6.1. NASA's Investment Decisions

In searching for an appropriate case study at NASA, it was desirable to find one where data was readily available, and which had as many characteristics as possible similar to previous applications of options. In this way, the leap from applying the methodology to profit-motivated capital budgeting decisions to a NASA-type environment would be as simple as possible.

The case study took place at the Jet Propulsion Laboratory in Pasadena, California. Data collected from Team-X (an interdisciplinary team of NASA experts in each of the relevant technical fields that would impact or be impacted by the new technology) included capital allocations, technology development paths and alternatives, probable outcomes, probabilities of technical success, and expected benefits and repercussions. The exercise was to compare the performance and cost characteristics of three different proposed missions, with the communications system as the variable. Three types of communications systems would be inserted into each of the three missions in order to perform a trade study that would identify technical strengths and weaknesses and cost savings. Among the expected outcomes was some alignment of investments and development of infrastructure, as well as technology and mission application.

The focus was on the decision space created by the optical communications technology, its associated infrastructure requirements, and the missions that could benefit from its use. The key questions that we were trying to answer included:

- What is the technology development path for optical communications?
- What are the obstacles to development?
- What are the probabilities of success or failure at each stage of development?
- Will optical communications appear more or less attractive due to an options analysis?

6.2. Applying Real Options to the Selection of NASA Projects

In recent years, external pressures have forced NASA to operate more like a private entity than a government agency. Fewer resources and the drive towards "faster, better, cheaper" missions create a need for more efficient development processes and selection of funded projects. Innovative project evaluation and selection techniques, like Real Options analysis, could be installed at NASA in order to assist with difficult budgeting choices. This chapter evaluates the application of Real Options to the selection of projects, and makes recommendations for the strategic management of the NASA portfolio and publicly funded R&D in general.

Real Options analysis is shown in this paper as a quantitative solution for selecting a portfolio of R&D projects. This portfolio generally includes a mix of basic research, capability-development efforts, and more applied mission and technology development projects. This paper first identifies types of projects suitable for analysis of outcomes by real options-based methods. It is then shown how projects that develop common technologies for application to multiple future missions can be quantitatively compared and selected through models that incorporate the concept of real options.

A study of three NASA telecommunications-technology programs is presented as a case for the implementation of options-based methods for valuing projects. Models of the telecom technologies development and application stages were constructed and used to justify strategic decisions about their appropriate funding. One of the technologies (Ka-band) was found easily justifiable as a profitable investment for the firm, given a reasonable number of applications in future missions. However, a second project (optical telecom) that required substantial investment could not be justified, except in cases where the technology enabled several valuable missions that could not be built otherwise. The case study showed instances where options methods provided more accurate valuation of projects than current DCF calculations leading to different funding conclusions. The models also were used to explore the project value sensitivities. The models presented in this paper were used to answer the following questions:

- Is the current portfolio of projects justified by the expected benefits to be derived from their use? Is there a better approach?
- What type and number of missions would be needed to justify investment into a technology that will be shared by multiple future spacecraft?
- What is the sensitivity of the results to changes in the expected benefits, probabilities of success, timing, and other factors used in the calculations?
- Does the use of options provide a better way of valuing potential investments for NASA?
- What are the next steps necessary to implement the proposed technology selection method at JPL?

For NASA, real options-based tools are recommended, considering their ability to improve over the current technology investment system. However, further work is required in tailoring the application to NASA's special case, and similarly the general case of publicly funded R&D. A relevant improvement would require a rigorous method of obtaining more accurate inputs for probabilities and outcomes.

6.2.1. Motivation

The most important decisions made by managers are those that commit resources to new projects. The initial selection from a variety of projects and subsequent choices for further investment are crucial. However, as stated in Chapter 1, current techniques have inherent limitations. Options thinking provides a repeatable method for standardized evaluations and justifiable quantitative support for decisions, so that "inertia" does not cause escalation of commitment in an unprofitable project.

The need for such a methodology comes mainly from the changed environment that NASA faces. Tight budgetary constraints require both a very wise use of available funds and justification of project validity. Thus the need for a quantitative way to compare development project alternatives. The real options approach offers advantages by integrating the views of different groups affected by project selection, clarifying decisions, and bringing flexibility to the process.

6.2.2. Initial Assumptions

The objective of the study is to investigate the application of "real options" as a decision tool for selecting among alternative designs or technologies that will be used in multiple missions. In this paper, we do not make a distinction between whether one is selecting a project portfolio from a set of possible technologies or selecting a design from a set of possible mission families. The goal is identical: we want to choose the set of projects or products that will maximize the value to the firm. Both raw technologies and complex products require initial investments for exploration, then subsequent investments for development and eventually application. The decisions, investments, actions and processes necessary to develop either a technology or a product are part of the model. It is assumed, however, that R&D (the technology project) seeks to achieve cost savings, and that a valid initial assumption is to hold performance constant. Finally, it was taken as a universal law that all technology projects are subject to some level of uncertainty.

6.2.3. Project History

Work at several MIT research groups as well as NASA-funded studies come together in this paper. The two MIT research groups collaborating on this effort are the Center for Innovation in Product Development (CIPD) and the Lean Aerospace Initiative (LAI). The CIPD is funded by the National Science Foundation and collaborating companies and is devoted to the creation and deployment of breakthrough product development science, processes and tools. LAI focuses its research on applying lean practices, such as integrated product and process development and optimized product flow, to the aerospace industry. This study also incorporates ideas drawn from JPL research on strategic technology portfolio selection, and data from JPL Team X mission studies.

6.2.4. Organization of Case Study

The remainder of Chapter 6 presents a framework for classifying different types of R&D projects in order to identify those types that can be valued and selected with outcome-based metrics such as real options. Secondly, it introduces the concept of real options and its application to the selection of R&D projects. Third, it reviews current methods of R&D planning and their limitations. Then the report presents a proposed set of models based on options-thinking that we use to value and select from a set of alternative common technologies that will be applied to multiple future missions. Finally, we review the results of the proposed methodology and make recommendations for its implementation at JPL.

6.3. R&D Planning

6.3.1. Three Tiers of R&D

R&D can be divided into three tiers, each requiring a different evaluation method to achieve the best results (Hauser, 1996). We use Hauser's metaphor to indicate the types of projects, depending on which tier they fall into, to which real options are properly applied. Research-intensive organizations have these three interrelated tiers of R&D – (1) basic research explorations, (2) evaluation of research programs to match or build core technological competence, and (3) applied research projects for, or with, business units (see figure).

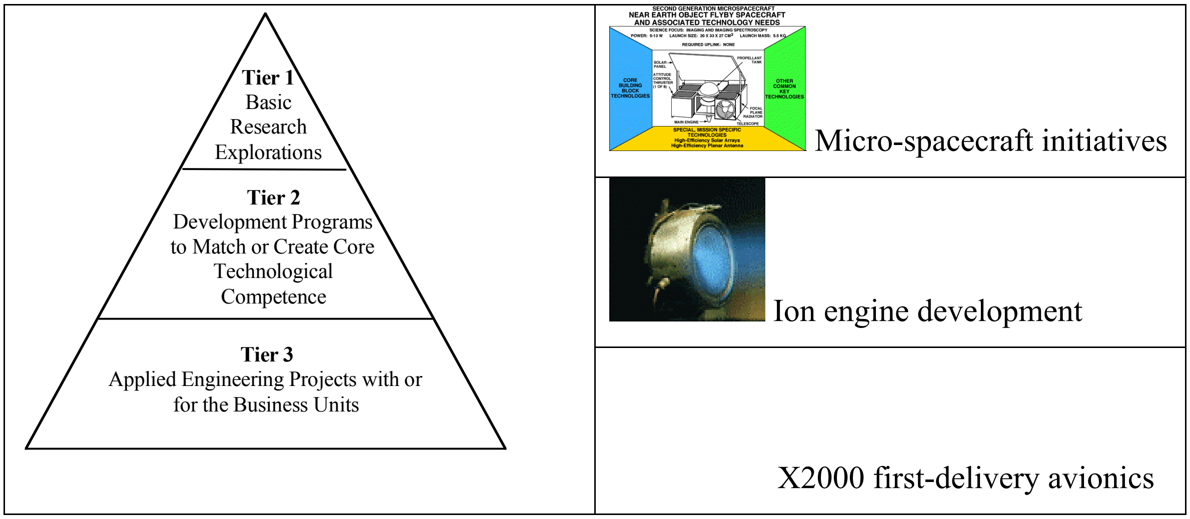


Figure 10: The 3 Tiers of R&D and examples from JPL [adapted from Hauser, 1996]

Option values have been shown to be best suited to applied engineering (Tier 3), but to result in counter productive effects when applied to Tier 1, and to some extent, Tier 2 projects. Other techniques and metrics not discussed here have been suggested to evaluate Tier 1 & 2 types of projects (Hauser, 1996).

Current work on technology planning also highlights the use of subsidies by the firm to encourage the development of new technologies. In subsidization cases, market outcomes are not sufficient to justify projects, but the firm realizes that it would be beneficial to fund them for the long-term or intangible benefits. In that case, subsidies adjust for risk, time preference (short-termism) and concentration of research scope. When a project's value is not apparent through the quantitative methods shown in this paper, but intrinsic value exists, investments can be viewed as subsidies for the benefit of the whole organization.

We have identified that Tier 3 applied engineering projects can benefit from an options approach; those projects with long term benefits can be subsidized. Next, we explore how to value the R&D that fits into the categorization above.

6.3.2. Valuing R&D

The mix of real options and decision analysis proposed here varies to accommodate a wide range of project risk characteristics. Valuation models can be used to help value projects with complex benefit streams such as simultaneous cost and performance improvements. The result is a better metric of project value that shows promise for increasing the returns on portfolios of R&D investments by helping to identify the most promising investments (Neely, 1998). The options perspective helps to quantify strategic value and justify it, and the methods for valuing complex benefits show promise for better expressing the value of capabilities based investments in financial terms.

Management of an R&D portfolio has been the subject of exhaustive study. It has been found that integrated metrics that combine qualitative and quantitative measures were most effective (Werner and Souder, 1997), but these metrics are also the most costly and time consuming to develop and use. Thus, it has been recommended, and reinforced by the 3 tiers theory above, that qualitative metrics be used for basic R&D, placing increasing emphasis on quantitative metrics as R&D moves from applied research to product and process development.

6.3.3. Applying Options-Based Methods

Based on the review of R&D management literature shown above, we propose that Tier 1 projects should not be valued by options-based methods. It is very difficult to quantify the outcomes of basic R&D projects due to the level of uncertainty present and the lack of models to quantify possible benefits. Hauser suggests that projects in this tier should be managed as a portfolio with high variance, negatively correlated alternative objectives. The uncertainty level for this type of R&D is very high, so this approach spreads the projects to keep options open for the future. It is also suggested that great care is used in developing metrics for this tier, since over emphasis on measuring idea generation can lead to “not invented here” syndrome and other unintended effects.

However, we do propose the use of options-based models to value Tier 2 and 3 projects, keeping in mind that Tier 2 projects may need additional metrics besides outcome-based measures such as real options calculations.

We include in Tier 2 and 3 the development of technologies, components, subsystems, procedures, and all other elements that may be planned for use in multiple future missions. As mentioned in the Introduction section, we do not differentiate between projects aimed at developing a technology for use in multiple spacecraft from projects intended to create designs that will be used by many missions. Both types of projects can be modeled in a similar manner, since they both exhibit the same structure for development and application phases: staged development gates and associated investments; benefits to be derived from multiple future missions and dependent on external and internal uncertainty factors.

In order to value these projects and designs, we have created two sets of models, one for the development phase, and another for the application phase. First, we model the development stages of technologies or platform designs using decision trees and options-thinking calculations of the value of the investments. Second, we model the application of the outcome of these development efforts into multiple spacecraft. For this purpose we use a tool created to compare families of missions. This tool allows a team of designers to model a group of missions with and without a particular common design or technology. Then, they can compare the alternative mission families with respect to performance measures and costs. The differences between the alternative families are then the changes in performance and costs that quantify the benefits of a particular design or technology. These benefits are fed back into the development stage models to calculate the value of the initial investments into their development phases. These two basic types of models are explained in more detail in the following section, and are applied to a study of actual technologies and their application at JPL.

6.4. Approach

In this section, we demonstrate a method to select an appropriate technology or design and determine the right level of funding through a study of three alternative telecommunications programs. Although the conclusions may seem to simply replicate the decisions made with current processes and not requiring this level of analysis, this method provides a basis for quantifying technology decisions. Furthermore, it considers and consolidates the views of all sides affected by technology selection decisions: the experts from the technology development side, the mission development or technology application side, and exogenous factors such as budgeting and mission roadmaps.

The first section of the case describes the three projects under consideration. We then introduce the method used in the case study, the available data, and the study assumptions. Finally, we analyze these data and present conclusions from the case.

6.5. Objective and Goals

The objective of this study was to study three different telecommunications technologies used in spacecraft in order to determine whether they were appropriate investments for NASA, given the current plan for development of the technologies and their application into future spacecraft. The main questions to be answered were the following:

- Is the current investment plan for the different technologies justified by the expected benefits to be derived from their use? Is there a better approach?
- What type and number of missions would be needed to justify investment into a particular technology?
- Does the use of options provide a better way of valuing potential investments for NASA?
- What is the sensitivity of the results to changes in the expected benefits, probabilities of success, timing, and other factors used in the calculations?
- What are the next steps necessary to implement the proposed technology selection method at JPL?

In the remainder of this section present our understanding of the state of the technologies under study, their pros and cons. These descriptions are based on data gathered from our interaction with experts in the development of the technologies, members of the Advanced Projects Development Team (Team X) that performed the studies on the application of the technologies to missions, and other JPL personnel investigating the alternatives.

6.5.1. X-band telecommunications

X-band RF telecommunications is the standard technology currently being used by JPL for its missions. We have used X-band as the baseline for our study, since we are interested in the possible gains that could be obtained by using a different technology over the established standard. NASA already owns a network of ground stations and proven telecommunications subsystem designs for use in spacecraft based on X-band technology, so no new investment is needed for application into future missions. However, capacity is a problem – ground operations experts have suggested that the current network will not be able to service the number of missions that are planned in the current mission roadmap. Therefore, a different technology that allows for higher datarates is needed to serve all the planned missions.

6.5.2. Ka-band telecommunications

Ka-band RF telecommunications allows for a theoretical fourfold increase in datarates as compared to X-band. This increase may not be fully realized, however, for a variety of technical reasons, the most important being the ability of Ka-band signals to penetrate weather patterns in the atmosphere.

Ka-band is the next generation telecom technology currently being funded to add capacity to the existing X-band ground network. Some investment into this technology has already been made. However, full implementation would require further investment to install Ka-band instrumentation on the existing DSN antennas. On the spacecraft side, Ka-band telecom subsystem components already exist. However, further tests are expected to be needed before mission teams will agree to use Ka as their primary telecom subsystem. Deep-space to ground tests of Ka technology are already planned, and some are already underway, as in the case of Mars Global Surveyor.

6.5.3. Optical telecommunications

Optical telecommunications technology has the potential for large increases in datarates as compared to Ka- and X-band. This increase in performance might allow for data-intensive types of missions that are not currently possible. However, optical telecom has the most technical risk of the technologies being considered. It is in the earlier stages of development, although some tests have already been conducted that make it a promising future technology. It does suffer from similar technical risks as Ka-band related to atmospheric losses in adverse weather. Substantial further testing and investment is needed before mission teams would agree to use the technology as their primary telecom subsystem. The investment required for application is significantly larger than for the Ka case. Two different options are possible for receiving optical data from spacecraft: (a) a single, space-based, orbiting telescope with an RF link to the ground, or (b) a network of several 10-meter telescopes (or photon buckets) in different locations around the globe for 24-hour coverage. In this study we consider both the space-based and ground-based optical telecom options.

6.6. Method

Our analysis approach has been to create models of the technology development and application stages. Both of these phases were modeled using the techniques suggested in the real options literature (Faulkner, Neely) and in Robert Shishko's *Strategic Technology Portfolio Selection* proposal. These approaches were implemented in spreadsheet models that accepted inputs about the investments, probabilities of success, expected benefits, and performance changes from the use of a particular technology, and calculated the value of the project.

In theory, all one needs to calculate the value of an investment is the following: the magnitude and timing of the expected benefits and investments, and the probabilities of success for each development stage. In practice, these figures were estimates gathered through (a) interviews with the experts in each of the technologies, and (b) several Team X studies conducted to explore the effects of the different technologies on representative missions. Some of the data needed could not be gathered due to the limited scope of the case. Reasonable assumptions were made in those cases, as listed in the Analysis section.

6.6.1. Development Stage Models

Decision trees were used to model the development of a technology project, as shown in the figures in the Results (Section 7.4). These trees map the different stages during development, and the possible outcomes from that process. A typical tree includes several decision nodes that represent the actual future decisions to fund (or cancel) each stage of development. The tree also includes event nodes that model the possible outcomes of different development steps, for example, the results of a telecommunications subsystem test, and the probabilities attached to each outcome. In addition, the tree shows information about the magnitude and timing of the required funding. Finally, each branch of the tree models a possible outcome of the development project as a whole. Some branches may end in project cancellation, while others end with some degree of success, associated with an expected benefit from the application of the technology.

The decision trees were modeled following the "Options-Thinking" approach described in (Faulkner). At each decision point, the expected benefits for that particular branch of the tree were compared to the investment required at that point in time. If the investment was larger than the expected benefit, it was assumed that the firm would opt to not invest. This incorporates the "options-thinking approach" as opposed to the more commonly used Discounted Cash Flow (DCF) analysis, which does not take into account the presence of the option.

6.6.2. Expected Benefits Models

Data on the expected benefits from the application of each technology project were gathered through the use of the Multi-Mission Design Tool developed by the authors in conjunction with JPL. This tool is an addition to the Team X CEM models that automatically gathers data of the performance and cost of missions studied by Team X. Data gathered from six studies is summarized in the appendix. These studies compared the effects of the three technologies studied (X-, Ka-band, and Optical) on missions representative of two types of future JPL spacecraft (Neptune Orbiter and SIM). Additional data were collected from the discussions that took place during those Team X studies of representative missions. These discussions involved both mission planners, subsystem experts, and technologists from JPL.

The expected-benefits data could be classified into two types: cost savings, and performance changes. Following the project valuation framework proposed by (Shishko) at JPL, this case study explored the value of the telecom projects in three different cases as described below. First, a project may be funded for the purpose of generating **cost savings** as compared to existing technology. In this case, the cost saving estimates from the Team X studies were used as the expected benefits. Second, a project may also be intended to **enhance** the performance of a mission, and therefore those enhancements should be quantified in order to capture the actual value of the investment in a new technology. In this case, the Team-X studies quantified the performance enhancements (as compared to the X-band based missions), which were converted to monetary values by multiplying by shadow prices for those performance measures obtained from the Cassini Science Management Plan study (Wessen). Those values due to performance enhancements were added to the Team X estimates of additional cost savings. Finally, a project may be funded to **enable** a type of spacecraft, in which case some (or all) of the mission's value (or budget) should be attributed to the enabling characteristics of the new technology. For this case, a percentage of the expected mission budget was chosen to represent the share corresponding to the enabling technology.

Technology Readiness Level	Definition of Space Readiness Status	Added Cost (%)
1	Basic principle observed	>25%
2	Conceptual design formulated	>25%
3	Conceptual design tested	20-25%
4	Critical function demonstrated	15-20%
5	Breadboard model tested in environment	10-15%
6	Engineering model tested in environment	<10%
7	Engineering model tested in space	<10%
8	Fully operational	<5%

Table 6: NASA Technology Readiness Levels [Source: Cost Modeling, 1998]

Figure above shows the added cost as a percentage of the total project cost for each technology readiness level (TRL) as defined by NASA. Notice that the initial research, feasibility studies and conceptual design can consume 75% of costs. Flexibility options have a great deal of value in the early stages of a project.

6.6.3. Uncertainty

There are a variety of sources of uncertainty that need to be taken into account to value a project. First, there is uncertainty related to the technical challenges to be solved during the development. This is captured by the probabilities incorporated into the decision tree models. Constructing detailed decision trees for technology projects is a time-consuming process, since the decision stages and estimates of probabilities and funding involved are subject to differing opinions depending on the view of the person being interviewed. We decided to begin the study on simplified trees with just two decision stages and one or two events. We further assumed that the probabilities were symmetrical at all event nodes during the development of the technology, that is, there was a 0.5 chance of a high or good outcome, and a 0.5 chance of a low or poor outcome at each node. With more time and access to information about the possible paths for the technology development projects, JPL technology planners should be able to construct more reliable models than shown in this report.

Besides the technical uncertainty associated with the development, other technical factors affect the expected benefits of a project. Even if the technology development project does reach a successful outcome, that is, it reaches the point when it can be turned into an application (TRL 6 in NASA's case), it is still uncertain whether that implementation will satisfy the requirements of the missions that were planned to use it. We quantified this uncertainty with the term p_{fit}, or the probability of fit between the mission requirements and the project outcome.

Finally, there are many sources of uncertainty external to the development process. For example, the budget for a NASA mission depends on the current economic climate and political decisions. The sum of all external factors affecting the value of a project was incorporated into the models with the term $p_{exogenous}$. The expected benefits for a project were then modified by the compound probability $p_{fit} * p_{exogenous}$.

6.6.4. Scenarios

There are many possible ways that the different telecommunications technologies could be funded and applied to missions. With the models described above, several possible scenarios were explored in order to answer the questions posed in the Introduction section: which technology was most valuable, what level of funding was appropriate, etc. A spreadsheet model was implemented where inputs were varied to create the different scenarios. This allowed for changing the number of missions, discount rate, timing of funding, objective of project (cost savings, performance enhancing, enabling), mission budget, etc. The spreadsheet models also allowed for a sensitivity analysis to different assumptions incorporated into the models.

6.7. Available Data

The tables below summarize the data collected during the period of August 18-29, 1998 at JPL, during interviews with technology and subsystem experts, from Team X studies of the chosen representative missions, and also from various JPL documents related to the technologies and their application.

Required Project Investments

Technology	Application Investment	Notes	Source
X-band	-	Baseline case: no funding needed	-
Ka-band	$30M	Retrofit 12 antennas at $2.5M each	Ground Operations expert
Optical, space-based	$500M-$600M	Earth Orbiting Optical Receiver study	Team X study
Optical, ground-based	$315M	Nine 10-meter telescopes at $35M each	Ground Operations expert and Optical technology expert

Table 7: Investments required for application of the different telecom technologies or platforms

Mission Cost Estimates

Mission	X-band	Ka-band	Optical [2]
SIM	$606	$608	$617
Neptune Orbiter	$312	$301	$305

Table 8: Total mission cost estimates for representative missions and alternative telecom platforms or technologies (millions)

Mission Performance Enhancements

	Spacecraft Mass (kg)			Spacecraft Power (W)		
	X-band	Ka-band	Optical	X-band	Ka-band	Optical
SIM	Baseline	-	-	Baseline	-	-
Neptune Orbiter	Baseline	-200	-200	Baseline	-120	-110

Table 9: Performance differences for representative missions and alternative telecom platforms or technologies

[2] Cost savings were assumed to be independent of the type of receiving station, whether it was a space-based or a ground-based optical receiver network.

6.8. Results

In this section we first examine each of the alternative technology projects separately, and consider different application scenarios (cost savings, mission enhancing, mission enabling). We then look at the possible project portfolio scenarios available to NASA, and analyze the best choices available.

6.8.1. Project 1 – Space-Based Optical Telecom

The figure below shows the development phase model used to analyze the space-based option for optical telecom. The model incorporates two decision nodes or stages: an initial funding decision and an application funding decision. It also shows two sequential testing stages for the technology, followed by its application into missions. The tree for this technology development project was based on the assumption that two major tests needed to be conducted before optical telecom could be integrated into actual spacecraft as their primary telecommunications subsystem: a low-earth-orbit to ground test, and a subsequent deep-space to ground test. The outcome of these tests would determine the level of funding needed to apply the technology, as well as the level of benefits that could be derived. In our model, we assumed that more successful tests would lead to slightly lower funding needs, and higher benefits from optical telecom use in missions, as can be seen in the decision tree below. The spreadsheet model uses the data in the tree model to calculate both the "Options-thinking" as well as the simpler Discounted Cash Flow (DCF) values of the development project.

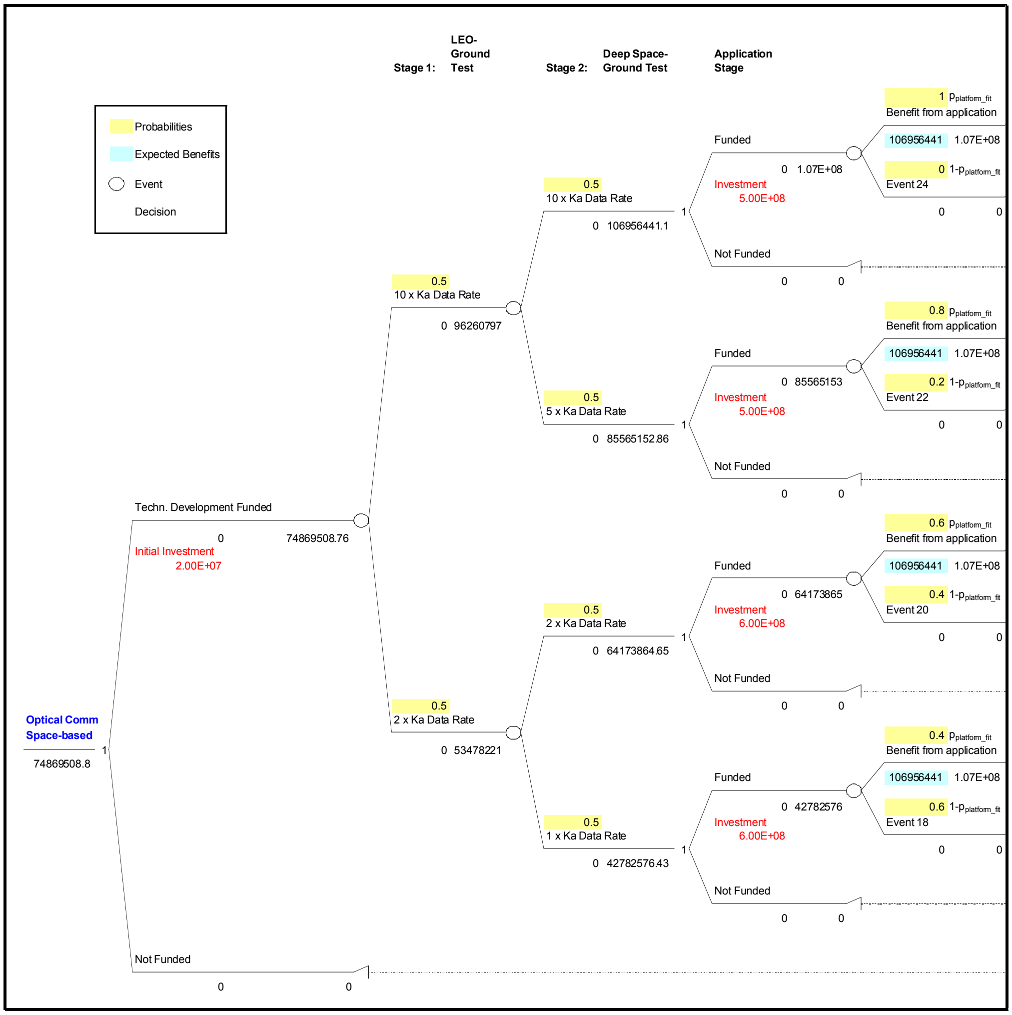


Figure 11: Development phase model of space-based optical telecom project

The following assumptions were made to calculate the value of the space-based optical telecom project:

- \$500-600M (infrastructure) application cost.
- Probability of technology fitting mission requirements: $p_{fit} = 0.90$ (missions like Neptune Orbiter).
- Exogenous probability (funding, etc.): $p_{exogenous} = 0.70$ (missions like Neptune Orbiter).
- Missions like SIM would not yield any benefits due to optical technology.
- Mass enhancement: 50 kg/mission (average); Power enhancement: 110 W/ mission (average) (for missions like Neptune Orbiter).
- Mass shadow price: \$80,000/kg; Power shadow price: \$20,000/W. Source: Cassini Science Management Plan study results.
- Year of first application into a mission: 2005. Time horizon: 30 years.

Based on the assumptions above and the cost savings estimates gathered during the Team X mission studies, the value of the development project for different application scenarios was calculated, as described below.

Mission Enhancing Scenario.

The value of the space-based optical telecom project was found to be positive only for large number of missions. Over the thirty-year time horizon, a minimum of 160 missions was needed for the project to pay off, given the cost savings and performance enhancements mentioned above.

Mission Enabling Scenario

The number of missions in the previous case was considered highly unrealistic. However, the technology might still be worthwhile in mission enabling cases. If we assumed that the technology was applied to this kind of missions, the probabilities were assumed to change to the following: Probability of technology fitting mission requirements: $p_{fit} = 0.90$; Exogenous probability (funding, etc.): $p_{exogenous} = 1.00$ (since the new technology is the only way to enable the mission). We further assumed a share of the mission budget that could be attributed to the enabling characteristics of the technology, and observed the effect of this assumption. For an average mission with a $200M budget, and enabling-related shares of 25% to 100%, the number of missions needed for the program to break even is shown in the figure below. This number was still found to be unrealistically high (as many as 28 in thirty years, assuming a 25% share attributable to the enabling technology).

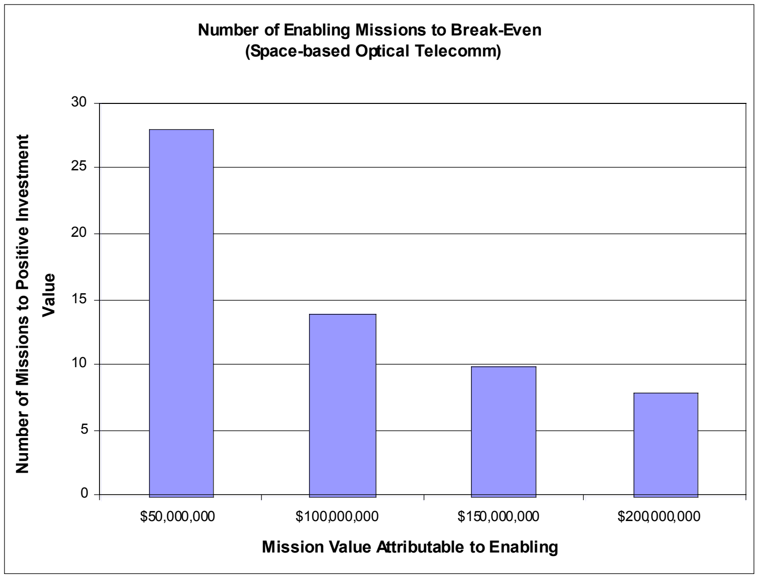


Figure 12: Number of optical-enabled missions to project break-even

6.8.2. Project 2 – Ground-Based Optical Telecom

The figure below shows the development phase model used to analyze the ground-based option for optical telecom. The model is very similar to the space-based option model; the only difference between the two is the magnitude of the funding necessary for the application of the technology. Also, the funding was assumed to be independent of the development path (or tree branch).

The assumptions for the ground-based optical telecom project were the same as for the space-based option except for the application cost. In order to obtain similar coverage to the orbiting receiver, the following ground network was assumed to be necessary:

- Nine 10-meter telescopes (photon buckets) located in three clusters around the globe, at a cost of $35M each, for a total application investment of $315M.

The expected benefits were assumed to be the same for both space- and ground-based options. Based on these assumptions, the value of the ground-based optical telecom project is described below for the mission-enhancing and mission -enabling scenarios.

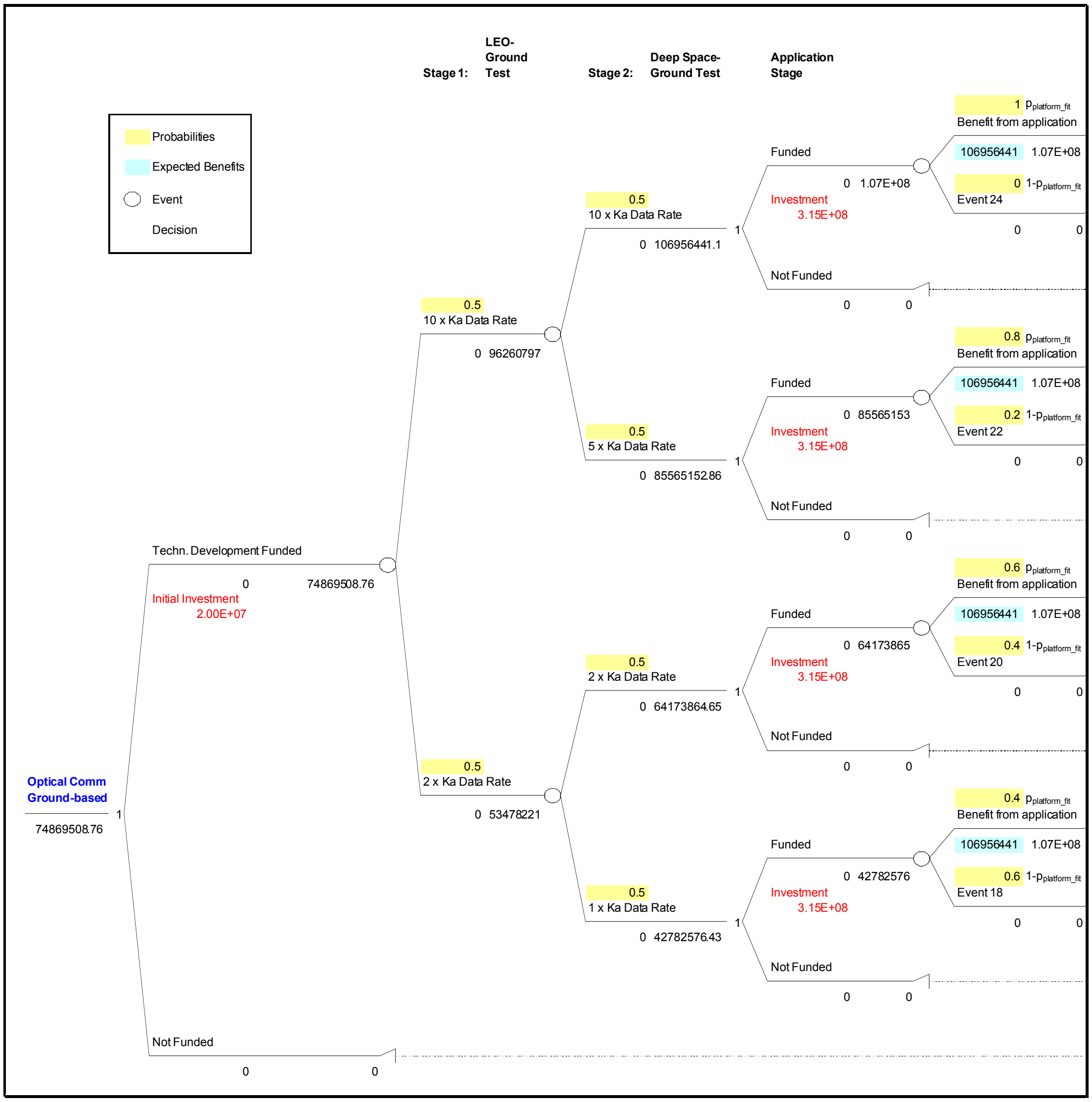


Figure 13: Development phase model of ground-based optical telecom project

Mission Enhancing Scenario

The number of enhanced missions needed for the project to pay off was still very large, although smaller than for the space-based option. A total of 105 optical-enhanced missions over 30 years were needed for project break-even. This resulted simply from the reduced investment to apply the technology, the only difference in our analysis of the two options. Again, the minimum number of missions was not realistic, so the project could not be justified solely by mission enhancements and cost savings.

Mission Enabling Scenario

As with our analysis of the space-based option, we assumed high probabilities of external acceptance and technical fit for this project in mission-enabling cases.

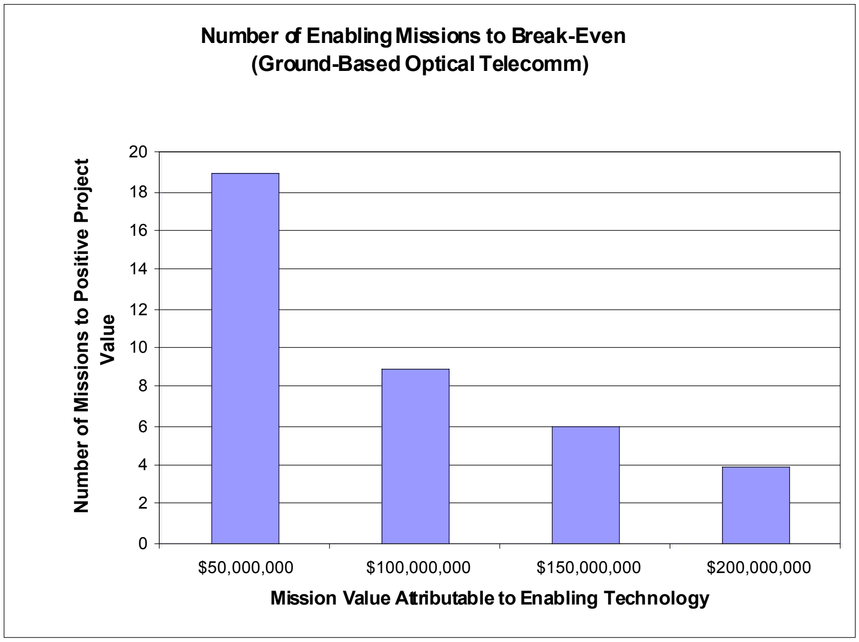


Figure 14: Number of optical-enabled missions to project break-even, ground-based option.

Even with the reduced amount of application investment needed, ground-based optical telecom requires a large number of enabled missions for the project to pay off. Assuming a 25% share of the average mission value can be associated with the enabling technology, nineteen missions would be needed, or more than one every two years. This frequency of missions was not considered to be realistic. The project would only pay off if the enabling quality of the technology were credited with a much larger share (75% to 100%) of the mission budget, or if the application costs could be lowered significantly.

6.8.3. Project 3 – Ka-band Telecom

The model used to represent the development of Ka-band telecommunications is show below. It differs from the optical technology projects in that only one event is shown before the application phase. Since the project is already underway, and some tests have already been conducted, it was assumed that only one major test would be needed for mission leaders to adopt the technology. The level of funding needed was also assumed to be lower than for the optical projects, since only one major test is needed during development, and application only requires modifications to the existing network of antennas.

In summary, the following assumptions were made to calculate the value of the Ka-band telecom project:

- $30M application cost ($2.5M/antenna for upgrade * 12 antennas)
- $10M development cost (One major test as a secondary telecom subsystem on a deep-space mission)
- Probability of technology fitting mission requirements: p_{fit} = 0.90 (for missions like Neptune Orbiter).
- Exogenous probability (funding, etc.): $p_{exogenous}$ = 0.80 (for missions like Neptune Orbiter).
- Missions like SIM would not yield any benefits due to Ka-band technology.
- Mass enhancement: 50 kg/mission (average); Power enhancement: 110 W/ mission (average) (for missions like Neptune Orbiter).
- Mass shadow price: $80,000/kg; Power shadow price: $20,000/W. Source: Cassini Science Management Plan study results.
- Year of first application into a mission: 2003. Time horizon: 30 years.

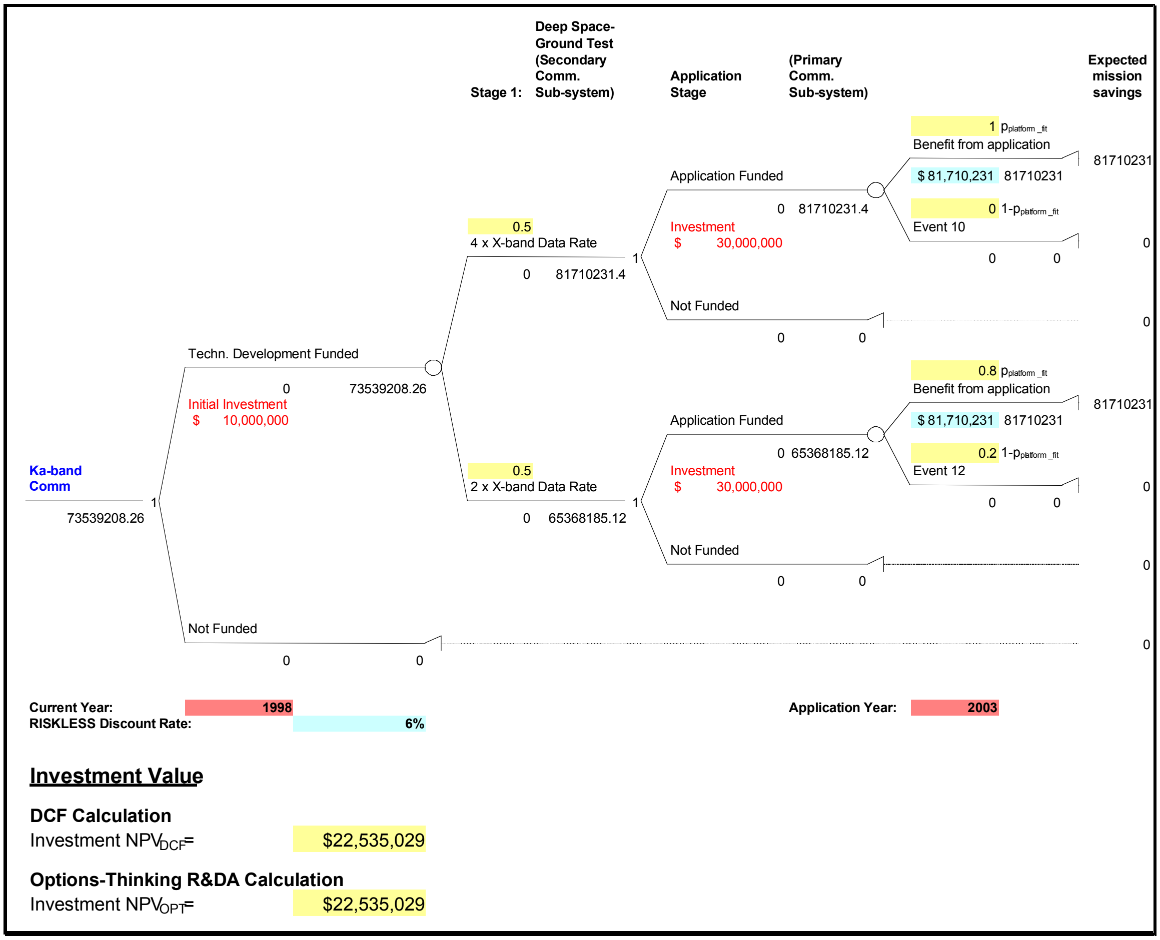


Figure 15: Development phase model of Ka-band telecom project

Cost Savings Scenario

Considering only the cost savings that could be derived from applying Ka-band telecommunications to deep-space missions like Neptune Orbiter, we found that 17 missions were needed to justify the investments described in the previous section. This number is still too elevated compared with the current frequency of JPL missions.

Mission Enabling Scenario

As mentioned in the introductory notes to the case, the existing X-band capacity may not be sufficient to supply the telecommunications needs of all future missions. In this case, Ka-band would become an enabling technology for some missions. As with our analysis of the optical telecom projects, we explored what the minimum number of such missions would have to be in order to make the development project pay off. We assumed high probabilities of external acceptance and technical fit for the Ka project in these mission-enabling cases, and a range for the percentage share than could be attributed to the enabling technology. The resulting number of missions to break-even is summarized in the table below:

Share, *s*	**Number of missions**
5%	14
10%	7
20%	3

Table 10: Ka-enabled missions needed for project break-even, depending on value share, *s*, attributable to enabling

The Ka-band project requires significantly fewer enabled missions to break even as compared to the optical telecom projects, even at much lower assumed value shares (*s*) due to enabling. Therefore, the project appears like a much better investment. However, there is large variability in the number of missions required within the range of assumed enabling-value share. It would not be unreasonable for one person to choose 5%, and for another to assume a share value of 20%, which would generate very different results and decisions about whether the project should be funded or not. This particular measure of enabling technologies does not seem to be a robust indicator of project value.

Mission Enhancing Scenario

We found that cost savings alone would not be sufficient to clearly justify the investment, and enabling assumptions were very sensitive to assumptions like enabling value share. However, if we considered not just the possible cost savings, but also performance enhancements provided by the switch to Ka-band, the minimum number of enhanced missions becomes only 10 over a 30-year period. This frequency seemed to be within reasonable estimates of planned missions.

We then explored the sensitivity of these estimates to other assumptions such as the application date for the project, and the assumed riskless discount rate. Those results are summarized in the figures below.

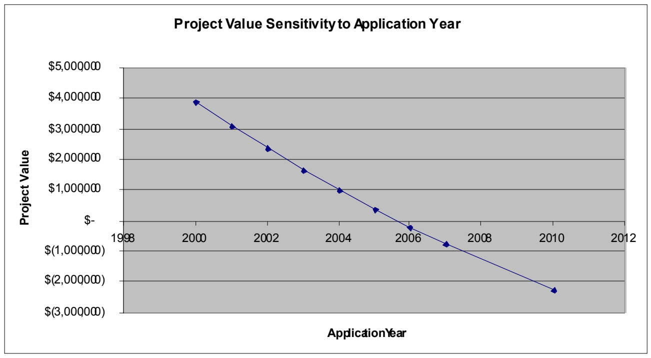


Figure 16: Ka-band project value sensitivity to assumed application year

The effect of a delay in the application of the Ka-band technology was explored. For the particular case where the minimum number of missions to break-even (10) was built, the project was still valuable even after a two-year delay (2005 vs. 2003 application year). However, the expected value of the project doubled when the application date was speeded up by two years.

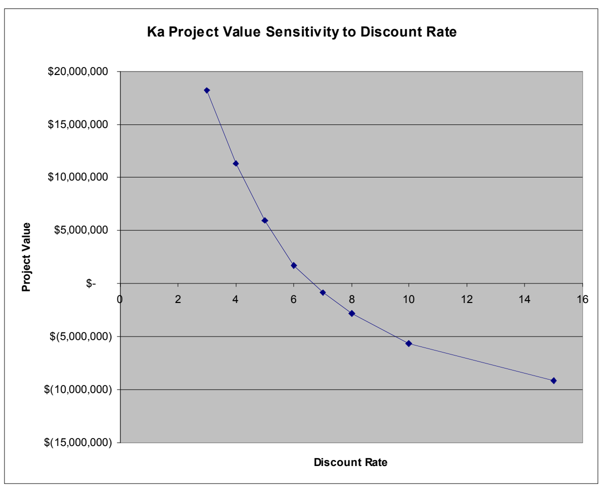


Figure 17: Ka-band project value sensitivity to riskless discount rate

Similarly, we could explore the effect of the assumed discount rate on the value of the project. In the marginal case, when the minimum number of missions to break-even are built, this assumption can have a tremendous impact on the value of the project. However, as we show in the Discussion section, when we look at a reasonable portfolio of missions instead of the marginal break-even case, the effect of the discount rate assumption is reduced.

Chapter 7. DISCUSSION

With the models of the individual telecom projects shown in the previous section, we can then explore possible strategies for NASA to follow, and begin to answer the questions posed in the introductory section of the case study.

- *Is the current investment plan for the different technologies justified by the expected benefits to be derived from their use? Is there a better approach?*

The first decision that NASA could make in this respect would be to choose between the space- or ground-based optical technology projects. Given the assumptions listed earlier in the case for both optical telecom options, the ground-based optical project will always make better sense than the space-based case. The benefits for both are the same, so the one with the lowest investment level is always the more attractive option. The key risk element involved in this decision is the uncertainty in the weather-related losses associated with the ground-based option, and therefore the assumption that a network of nine 10-meter optical stations can perform as well as an orbiting receiver should be explored in more detail.

Then next decision then becomes whether to fund one or both of the telecom development projects. In order to answer this question, we have to establish a reasonable set of assumptions for the types of missions that are likely to be produced in the near future. The assumed baseline mix of missions is shown below.

Qty.	Mission Type
5	Ka-enhanced missions (performance gains as in Neptune Orbiter case)
3	Ka-enabled missions, with 10% value attributable to enabling
2	Optical-enabled missions, with 25% value attributable to enabling
=10	**Deep-space missions over 30 years**

Table 11

We then used the spreadsheet model shown in the Appendix to look at the baseline set of missions and determine the value of the different telecom projects. That model also allowed us to explore the sensitivities of the project values to the assumed parameters and changes in the baseline mission mix. The results for the baseline case are summarized below:

1. The baseline case shows a positive value for the Ka-band project of $22.5M.
2. Optical telecom does not show a positive project value for the baseline case.
3. The Ka-band project shows a positive value ($3.3M) even without the Ka-enabled missions.

Based on these results, and considering the assumptions already listed, the proposed strategy for JPL to follow would be:

(1) continue funding of the Ka-band project, since it pays off with as little as five enhanced missions like Neptune Orbiter;

(2) do not continue the optical-telecom project *unless* the number and/or value of optical-enabled missions is significantly higher than that outlined in the baseline case

- *What type and number of missions would be needed to justify investment into a particular technology?*

As mentioned earlier, the ***Ka-band*** project would pay off with few enhanced-performance missions. The graph below shows the project value as a function of the number of enhanced missions. That value would increase with any additional Ka-enabled missions.

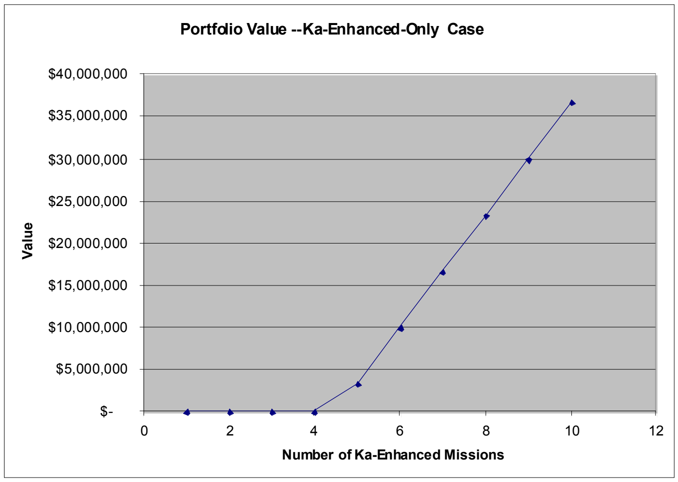


Figure 18: Project portfolio value as a function of Ka-enhanced (only) missions

Optical telecom project value becomes positive only at 50% mission value due to enabling and a minimum of 9 enabled missions. If enough of these missions are planned, the value of the optical-telecom project could become significant; thus the need for a mission roadmap model that accurately represents the number and timing of future missions.

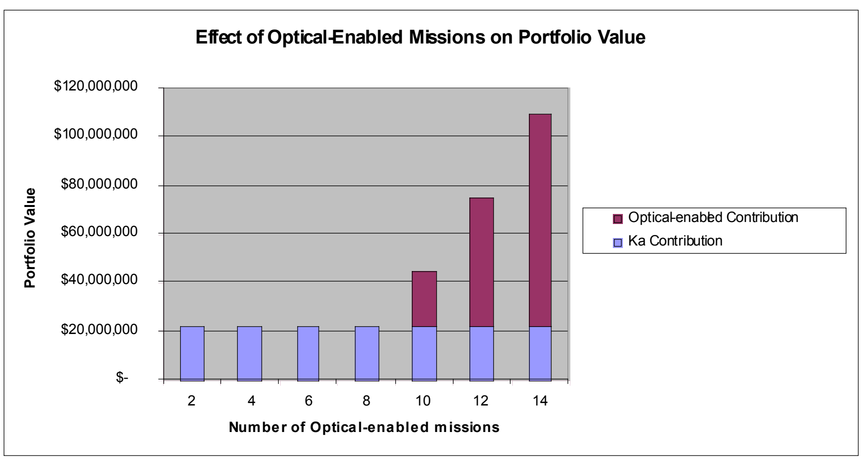


Figure 19: Project portfolio value as function of optical-enabled missions
(for constant number of Ka-enhanced missions)

- *What is the sensitivity of the results to changes in the expected benefits, probabilities of success, timing, and other factors used in the calculations?*

Changes in expected benefits or probabilities of success directly affect the value of projects in the same way as the number of missions. All these sensitivities can be explored through the use of the spreadsheet models, and the effect of some of these factors are shown in the figures below. Others, like the effect of technology application timing have been shown in the Results section.

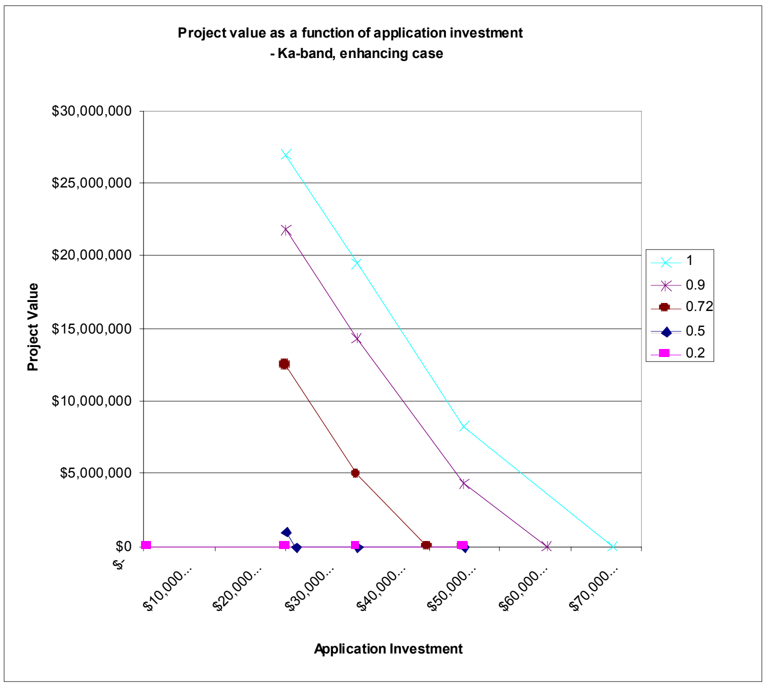


Figure 20: Project value as a function of required application investment and total probability (p_{fit}*$p_{exogenous}$)

Note the decrease in project value as investment magnitude increases, and as the probability of application of the technology decreases.

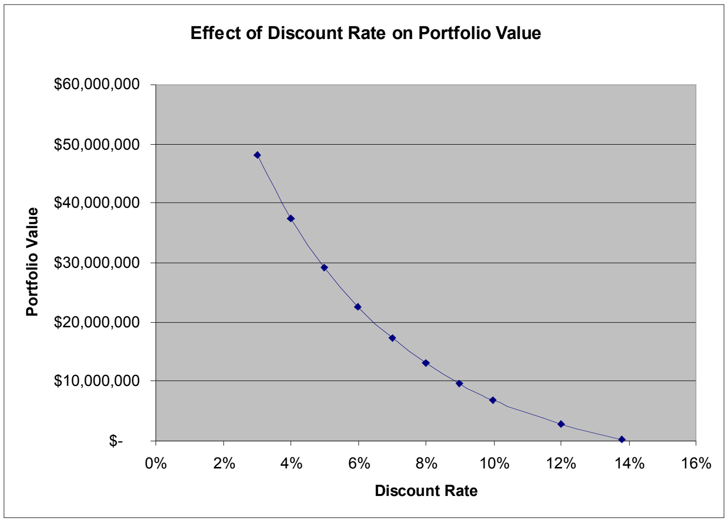


Figure 21: Baseline project portfolio value as a function of the riskless discount rate

For the baseline case, portfolio value remains positive for a large range of discount rate assumptions, unlike the marginally valuable project case shown in the Results section. However, notice the same exponential pattern of decline in value due to the discounting of expected benefits.

- *Does the use of options provide a better way of valuing potential investments for NASA?*

In most of the scenarios covered in this study, the Options-Thinking and DCF calculations of project value were actually equal, since the expected mission benefits were either very low or very high. In those extreme cases, the options-thinking value of the project reduces to zero (if the project is a clear loser), or to the net present value of the expected benefits (if it is a clear winner). However, in some marginal cases where the expected benefits outweigh the application costs in some of the possible development outcomes (or tree branches) but not in others, there exists an option value from the possibility of not funding the application stage if the development stage has gone badly.

The extra option value can result in different project funding decisions, as can be seen in the figure below. In this case, for example, we may be trying to find whether the Ka-band project would break even at a required application investment level of $60M. A DCF calculation would result in a negative project value and therefore we would choose not to invest in the Ka effort. However, the Options-thinking calculation would yield a positive result for that level of investment, indicating that the project does have value and the initial funding is justified.

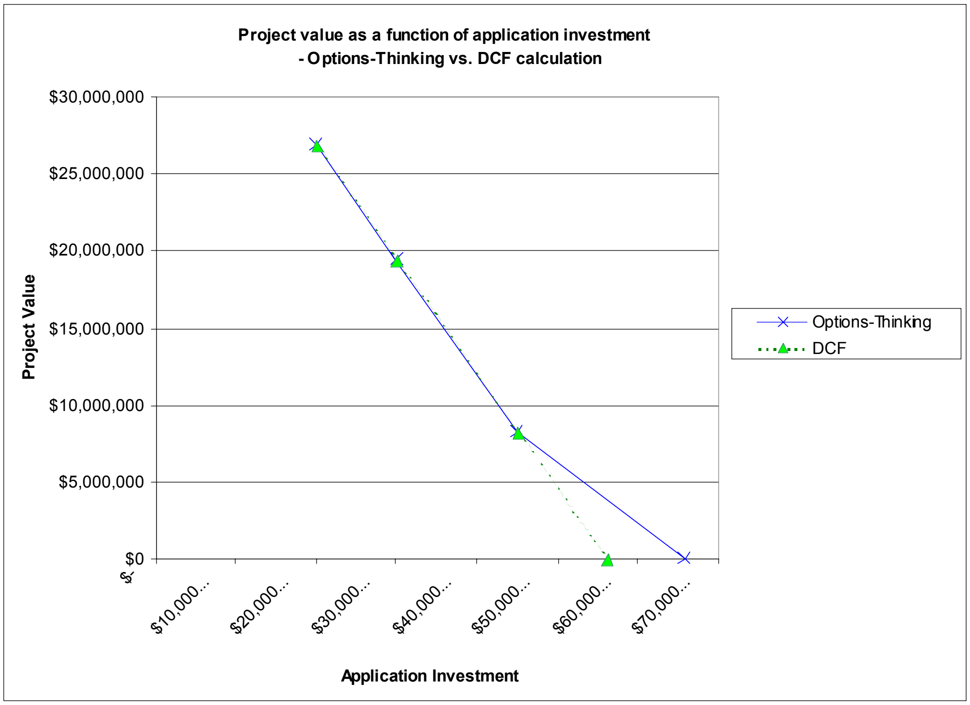


Figure 22: Ka-band project value according to DCF and Options-thinking calculations

This difference in project valuation by DCF and Options-based approaches has been well documented in the literature. The Options approach has been shown to be *at least* as good as the DCF approach, and in marginal situations like the one discussed above, it can make the difference between a correct and a flawed funding decision.

This particular case study did not exhibit many of these cases where the two methods differed in valuing the portfolio of projects. However, the difference between the two methods becomes larger as the number of stages in the development process increases, and therefore, the number of decision points and chances to change direction or options, grows. The models used in this case only showed two decision stages, where in actual development programs, several such decision points exist. Then, as the models become closer to the actual development process, the greater the importance of using options-based methods to quantify project values.

- *What are the next steps necessary to implement the proposed technology and design selection methods at JPL?*

The models used in this case used many assumed parameters that would be easier to quantify with more time for data gathering and expert interviews, and knowledge from existing mission studies, data on the technologies under review, standard development processes, etc. We have described the basic elements, models, and structure needed to perform a quantitative analysis of portfolios of projects that is quickly becoming a standard modern business practice, and shown through a basic case study that these techniques would be valuable to decision-makers at JPL. In order to perform this kind of analysis, JPL would need to develop the following:

- A model of the future roadmap of missions, including timing, budget, and probability of funding information.
- A model of the development process for each technology project to be considered (decision tree), including the decision stages, probabilities of success for each stage, and funding magnitude and timing information.
- A model of project application benefits that includes information from Team X and other mission studies and quantifies both expected performance and cost savings. A working version of this model is the existing Multi-Mission Design Tool already implemented as an extension to the current Team X models.

The basic structure of these models has been shown in this report, and outlined in existing JPL proposals (Shishko).

Chapter 8. EXPLORING THE USEFULNESS OF OPTIONS

Real options analysis' usefulness in the case study and academic examples is examined here to determine the value of its implementation in broad application contexts. As with many technology management tools, it is advertised as a quantitative method that can provide a point solution. As with most technology management tools, the real-world application is somewhat limited. However, there are benefits in the execution of the process itself, as well as in the organizational mentality shifts that occur. Thus, we conclude that the quantitative financial usefulness has contextual limitations, but that risk assessment and an "options mentality" along with cautious use of the quantitative results provide a very workable tool.

8.1. Real options as a quantitative capital budgeting tool

8.1.1. Better decisions through financial options

The financial origins of options pricing theory discipline an approach designed to **quantify** risk and return. A system with quantifiable characteristics, such as exists in the financial realm of traded shares, provides the proper inputs for application of the complex numerical solutions involved in options. The Black-Scholes model detailed above, for example, requires some measure of volatility of value over time. The history of a stock price provides the perfect unbiased number. Furthermore, the data is easily accessible, and standardized measures like this one exist across all stocks.

8.1.2. Difficulties of Strict Financial Analysis

The case study detailed above is an effort to apply the modified options pricing theory to a research and development oriented investment decision. The technology intensive task benefits from the options structure in the associated capital budgeting choices. However, the nature of the project and the organization posed one major difficulty that has implications about the options approach. In an internal R&D organization (in a government agency or otherwise) there is no cash flow or prescribed value from innovation. Although options is designed to help value R&D, it is those same inputs of past value and volatility that can hinder the results. Although the problem of valuing R&D has received much academic and professional attention, it is something of an intractable problem precisely because it creates (in instances of success) a platform of options from which many products, and therefore, much value can be derived. This circuitous route to eventual value creation creates the difficulty in understanding how to capture the payoff to an investment. Indirectly, the manager is forced to rely on faith that R&D will somehow accrue benefits commensurate with capital allocations.

Despite the apparent incongruity of the options approach to something as intangible as benefits from R&D, the methodology proved robust enough to be used. In search of a solution to the valuation problem, the case study shows that a simplification can be made that allows a monetary benefit to be assigned to a new technology. The evidence from the case study indicates that the use of cost savings (with performance held constant) as a metric of value is a viable substitute for cash flow. As an input to the model, this was completely feasible with the caveat that the results are only as good as the inputs.

8.1.3. Two Uses

In its new found business applications, options pricing theory can really be implemented in two different ways. If the context of the application is suitable, a portfolio can be built that will track any business construct. Essentially, this "tracking portfolio" *emulates the value of the opportunity using market instruments*. [Amram, 1999] The second implementation, discussed further below, is an options approach that is integrated as a process into innovation management and technology development. It is used, in a looser form, as a risk assessment and capital budgeting tool that can adequately capture hidden value, but does not have the necessity or luxury of the precise monetary inputs of the financial world.

8.2. Real Options as a risk assessment technique

Risk figures into options in a variety of ways, however the most important might lie in the following attributes:

- Retention of flexibility in the design process reduces the risk of not meeting requirements.

- Identification of known unknowns and unknown unknowns forces awareness.
- Facilitation of orderly risk assessment.

It becomes very obvious that mitigation of risk and resolution of uncertainty over time will increase option value.

8.2.1. Uncertainty and the application of options

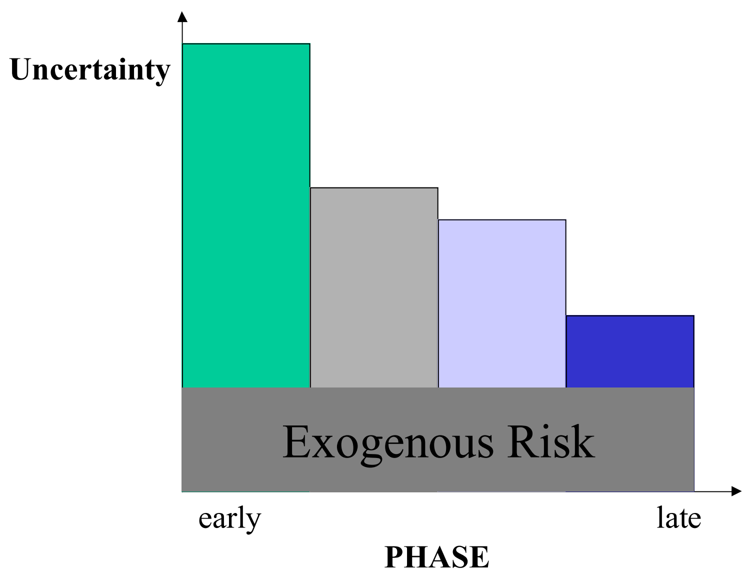


Figure 23: The Elimination of Risk

The level of uncertainty associated with a particular project tends to decrease as risk is eliminated. If the preceding statement is true, then information gathering is the goal of R&D. Information about private risk is gathered from historical experience, such as engineering or actuarial data, and is not inferred from market prices, so the real options approach has diminished advantages over other methods. Business decisions, however are always affected by the price of some asset in the economy, and the real options approach extends financial market discipline to the valuation of options whose value depends on a mix of private and market–priced risk. (Amram) The figure above implies that the private (or endogenous) risk is really what a firm can control. The exogenous or market-based risk is an ever-present constant risk determined by external forces. The assumption that it is constant is a simplification that allows the firm to focus on the risk aspects it can control, such as technical success. Market based risk is thus considered something to be **aware** of, but not necessarily a variable.

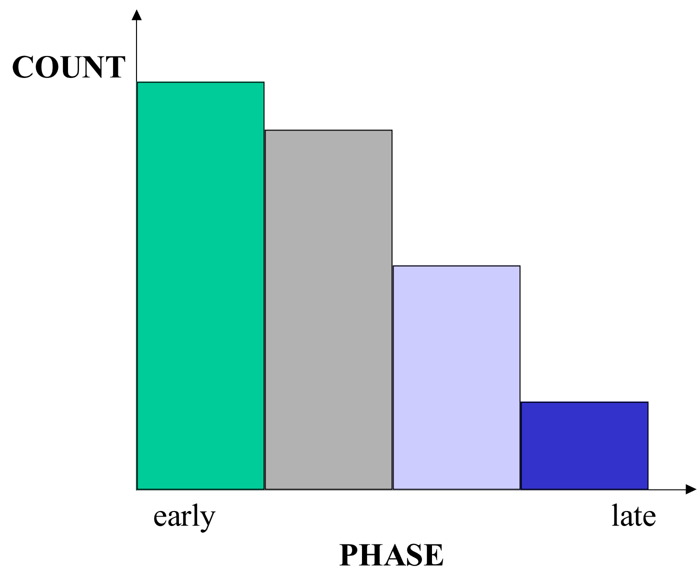


Figure 24: Number of Surviving Projects in Development

Figure 25 shows that as managers exercise the option to abandon, the number of projects in late phases diminish. The resolution of uncertainty or lack thereof can hasten abandonment. Regulatory or policy issues can also determine the fate of a project as it passes through the necessary gates to move from one phase to the next. Gates are built up around different milestones, used as indicators of a project's readiness and worthiness to pass on to a higher stage. The next stage can often mean more attention, a step closer to implementation, but always more money in the form of an investment. Different firms have different increments in which they break up their stages. This brings up the question of the amount of gates necessary to optimize a project, which will be discussed in terms of the correct number of decision points later in the thesis.

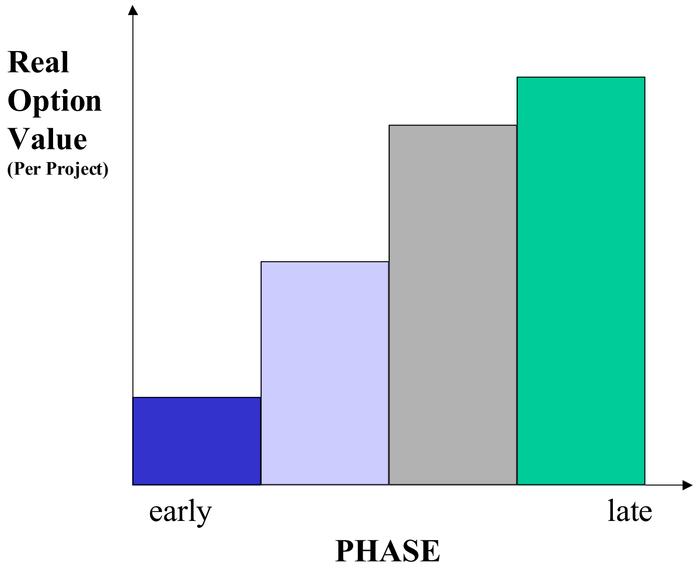


Figure 25: Value of Surviving Technology Projects in Development

The value of the project increases as uncertainty about remaining costs and benefits is resolved, as it gets closer to completion, and as it passes technical and political hurdles. Figure 26 illustrates this point and also emphasizes that the definitions of different “phases” (when a project has passed from one to the other) will often color decisions. If an early phase project has a lot of late phase potential, that will often not be captured. Similarly, if a project is being evaluated by metrics meant for a project at a different maturity level, it may be cancelled prematurely. Once again, the phase definitions are intimately related to stage gates, and therefore the number of decision points within projects’ lifetime.

8.2.2. Risk and the Application of Options

Managing uncertainty in R&D or in product design is often about hedging the potential downside by keeping extra resources. A way of doing this is to maintain flexibility, but flexibility has a price and project planners should ensure that the price is commensurate with the return in risk mitigation.

There is a common trap in technology decision making in confusing hurdle rate with discount rate. It arises from the misuse of a valid and powerful concept: the translation of higher degrees of risk into higher cost of money. (Brealey and Myers, 1996) The misuse occurs because financial analysts unfamiliar with R&D do not explicitly recognize that a central part of the R&D process is risk reduction, and that most of the investment will be made, and only made, after the key risk issues are resolved. (Boer, 1998) Very few long-term projects can stand DCF analysis at the typically high discount rates assigned to "high risk" projects. The long-term project champions must be willing to make outlandish income assumptions if they are to gain funding.

R&D is in large part a process of risk reduction. Risks can be extremely high in the early stage of a project. But risk is systematically lowered in each subsequent stage of a project. It is most appropriate to handle risk using either probabilities of success at each stage of the project, or different discount rates at each project stage. *In financial terms, markets have no memory, and project risk is more closely related to the current stage of development than to the question of whether it once started life as a long shot.*

8.2.3. Risk assessment in decision appraisal

8.3. Real options as an intuitive approach to technology investment decisions

The options approach elicits reaction and conversation on the subjects of volatility, risk, investment, outcome and alternatives. It also instills several concepts into the mental toolkit of the proactive stakeholder:

- waiting for more information before committing to a single course of action
- the possibility of project abandonment

- staged investing
- avoidance of escalation to commitment

The concepts above have this in common: they share an intrinsic value that is often hidden or overlooked by current techniques. The mindset of the manager can be dictated or limited by the available techniques, thus it is very important to use the tools that correctly quantify investment decisions. The real options approach hastens a shift in the characteristics of the funded projects when applied to a selection scheme. The project that options makes attractive tends to be longer-term, and a riskier investment. The approach is similar to anecdotal scenario-based planning, in that options can provide the quantitative basis for an appropriate discussion of alternate technology development plans and outcomes. The next section extends this new mentality to a research system and shows how the product development process benefits through interaction with the "fuzzy front end".

8.4. Interaction of R&D and Product Development

The purpose of R&D in any but the most academic organizations is to produce some novel knowledge, technology or product. This has been accomplished in a fashion that disconnected the process of performing R&D from the goal. Below, we explore the interaction and the potential for improving the connection between R&D and product development. Real options is a critical enabler of that improved interaction because it directly ties the outcome and expected benefits to the initial investments and expectations (see Figure 28). More importantly, it reflects a new mode of thinking adopted by business today. Management academics like Clay Christensen and Marco Iansiti are both advocating the approach that options fundamentally nurtures. That is the idea that waiting for information is worth a great deal. Structuring projects so that they are optimized for learning is at the heart of the **system focused** approach to what will be described below as "technology integration". The old method of working toward a single solution that was separated from the actual finished product is described here as **element focused.** (See Table 12 for a comparison of element and system focused approaches)

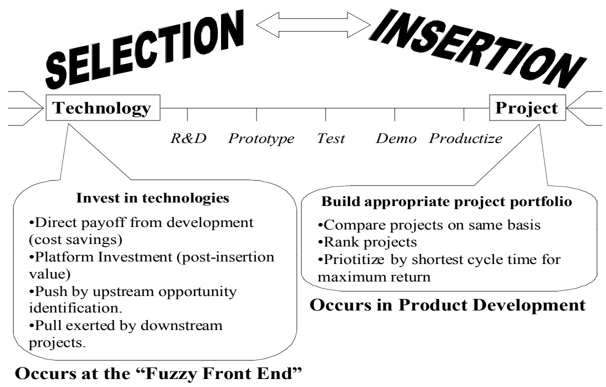


Figure 26: Technology integration is important at both ends of the value chain

8.4.1. Technology Integration: Developing Products Based on Novel Technical Capabilities

The advantages of an options mentality at the fuzzy front end translate into a more effective new product development process. When comparing element focused against system focused approaches it becomes obvious that proactive management in the options framework payoff. When selecting paths to pursue in technology development, either for a new product or for insertion into an existing one, it is desirable to suspend commitment until acquisition of further information. Contrary to the element focused approach, there is no hard milestone for a go/no go decision, because neither a discrete endpoint, nor a binary decision reflects the potential value of opportunity. Instead, a continuum of information gathering while pursuing Nelson's parallel paths and recognition of the variety of options available for a spectrum of immature to mature technologies. Luehrman likens the range of investment and development options to cultivating and picking tomatoes. The ripe red ones should be collected immediately, while the green ones should be watered and fertilized until at least some of them ripen, and finally the brown, rotting ones should be culled so as not to contaminate the garden or steal vital nutrients from the healthy plants (see diagram below). The tomato garden adds a simple but versatile picture that reveals important insights into both the value and timing of the exercise decisions. Active gardeners are monitoring the options and looking for ways to influence the underlying variables that determine option value and, ultimately, outcome. The garden - sowed, seeded, and planted yields tomatoes at different stages of growth and quality for a given amount of work, watering and weather. For a given labor and capital investment, projects develop at different rates and yield different quality results.

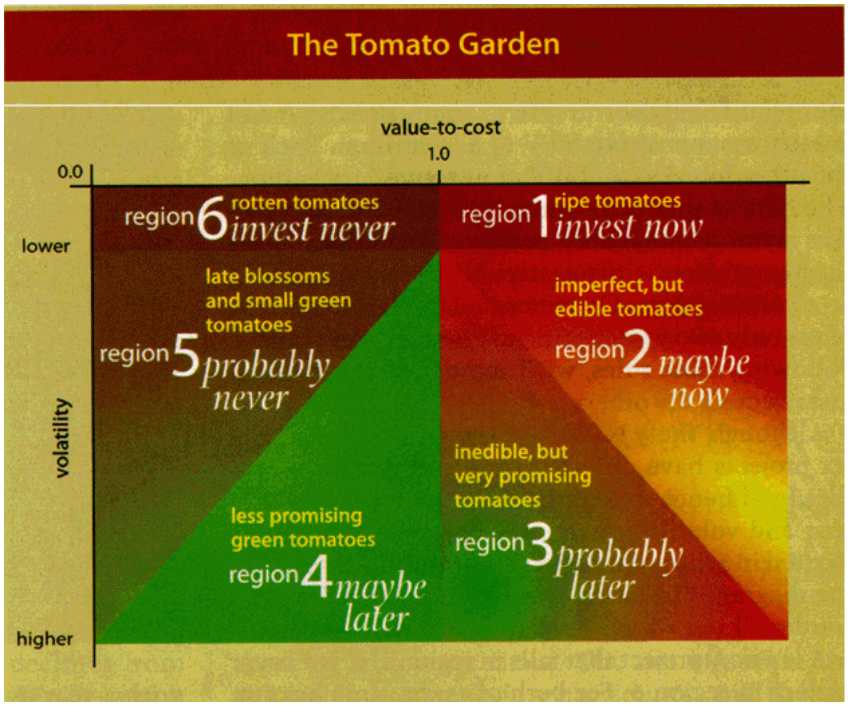


Figure 27: Cultivating a technology portfolio like a tomato garden [Luehrman, 1998]

Iansiti's study of the computer industry suggests that integrating technology into a product is better accomplished when there was a system-level rather than elemental focus. Often, the core of the problem in technology funding and selection is that control is handed over to the technical staff (Gluck, 1975). The technical people, although capable, have little experience of driving technological choices for a product (Iansiti, 1995).

	Element Focused	*System Focused*
Technology Selection Basis	Potential for optimal impact on future system	Information on functionality and cost gathered before a committing to a specific technical concept.
Selection Process Drivers	Research Group	Integration Group
Group makeup	Homogenous	Heterogeneous with experience in integration efforts.
Selection Process	Identified single new technology with promising characteristics	Build knowledge base about system level properties of concept alternatives. Drop, grow or postpone possibilities based on prototyping.
Exploration of Alternatives	Limited serial investigations of alternate technologies	Extensive parallel investigations of combinations new technical elements
Milestone indicating end of integration process	Lab feasibility demonstration	"Soft" selection, gradual and ongoing, of most promising concepts

Table 12: The interface between R&D and product development can be system or element focused

In the element focused approach defined by Iansiti, the transition from research to development occurs fairly smoothly because several members of the research group moved permanently or temporarily to the development group. This is to insure that knowledge attained in the upstream activities was transferred to the next step in the process. However, it has been shown that including a downstream representative improves product development because, if they are included early enough to affect change, the technology will be driven to a space that represents greater utility to the customer and more efficient manufacturing. The downstream perspective is helpful in the process, but can be difficult to implement because of hierarchical organizational structures that place downstream activities at a lower perceived level of competence than upstream ones. The options approach may facilitate the flattening of the hierarchy through its incorporation of technical opinions and knowledge into the fabric of a management tool.

In the system focused approach, the firm is concerned with postponing commitment until uncertainty is resolved by gathering the maximum amount of information. This follows the options mentality where there are several alternative concepts being developed in parallel. It is not necessary to force a binary decision at the front end, but rather to recognize the value in pursuing the parallel paths to gain further information. Even when a single optimal solution is clear, the other concepts can be postponed, refined, kept for additional examination, abandoned or grown. By taking this system focused, or options approach Iansiti shows that the number of projects and the amount of time and effort put into them will benefit.

	Primarily Element Focus	***Primarily System Focus***
Number of Projects ***Unadjusted Project Performance***	12	15
Total lead time (years) *(exploration-production)*	9.6	7.0
Concept lead time (years)	3.1	3.3
Development lead time (years)	6.1	3.8
Person years of technical and scientific activity	564	173

Table 13: Different characteristics of element and system approaches

The different characteristics of the element and system approaches mirror the options mentality and provide proof for the advantages of flexibility in the fuzzy front end of new product development. The element focused project will serially develop a new concept within the rigid constraints specified by the exploration stage. In the exploration stage the main goal is to make sure that the concept reflects a choice of technical elements with the maximum potential functional impact on future product characteristics, within the constraint of feasibility (Iansiti, 1997). The outcome, therefore, is reactive development and integration phases subject to the decisions made by the researchers in their initial technology selection, which may not take into account the broader strategies and goals of the enterprise. Again, a passive management that hands control of the R&D pipeline to the researchers is relying on serendipity both for innovation and for successful technology integration.

Yet another organizational body might impinge upon the resource allocation process when it comes to technology development. This is middle management, screening the ideas of the researchers, engineers and marketers with their own filters before making a case to senior management. Christensen identifies a preprogrammed bias in this middle layer that often dissuades managers from making the case for a disruptive technology. The middle manager is not daunted by technical risk but rather by lack of perceived need for the innovation.

In the second approach, instead of the implicit assumption that the element level investigations (the exploration stage) are the critical challenge in a project, the focus is on capturing knowledge. Defining a concept which best selects and adapts new technical elements to system characteristics by rooting out the interactions between the new elements and the product simply works better. The investigation of many alternatives is aggressively encouraged and knowledge of system interactions is included proactively in the integration stage. The system focused approach achieves higher average productivity and shorter development lead times than the element focused group (Iansiti, 1997). There are examples of Japanese, US and European companies in both groups, in dicating that the relationship is not strictly tied to geographical origin. The results are consistent with the earlier conceptual discussion that a system focused approach to technology integration is associate with higher development speed and R&D productivity.

We began with the assumption that there are significant differences between firms' development speed and R&D productivity of projects aimed at the introduction of novel technology. In proposing that this was primarily due to the way in which technology selection and concept development occurred, we pinpointed the exploration stage as the essential modifier. The assertion also indicates a more direct connection between applied science activities and successful product development than is immediately obvious. The persistent difficulty in measuring R&D performance poses a challenge to the establishment of a relationship between R&D and NPD. However, superimposing the options mentality described in this thesis with the framework established by Iansiti demonstrates the critical correlation. Iansiti's findings show that the approach taken to achieve the integration of new technology was significantly associated with differences in project-level R&D productivity and development lead times. Those findings also qualitatively match the more successful, or system focused, approach with the characteristic features of options: waiting to commit, value of further information, resolution of uncertainty, and flexibility in both the early and valuable transition phases of projects.

While existing research has focused largely on managing the development part of R&D activities, it is clear that there is a critical set of earlier activities including technology evaluation, selection and integration. These crucial steps establish the complex relationships by which the technologies combine to form the basis of product architecture. [Henderson, 1990 #60] Design optimization of a new system, therefore, occurs in a critical window of opportunity opened by performing evaluation, selection and integration correctly.

Chapter 9. ACCURATENESS OF THE DEPICTION OF R&D BY OPTIONS

9.1. A Scenario

The following is a hypothetical scenario that helps to illustrate the mindset behind the options approach: Over many years of operation, a firm accumulated a portfolio of R&D programs and technologies – essentially rights to create products. Where other measures (NPV) or management techniques (staged gates) suggested the economies were positive, the company developed the technologies for productization. Where the technologies appeared uneconomic - as most did, usually because development costs were too high in relation to expected benefits – development was shelved. Left with unwanted development programs that were consuming cash and that had limited appeal as investment opportunities, the company decided to shelve or abandon them.

Part way through the divestment program, it was suggested to the organization's managers that, instead of calculating what the technologies would be worth if they started developing them today, they should value the opportunity as an *option* to develop if, at some point in the future, the value could be increased through the resolution of uncertainty or the identification of new uses. In other words, they should apply the notion of options as conceived in financial markets to their actual business situation.

Applying the option value over a fixed future time period (say five years), incorporating uncertainty, and leaving room for flexible response to the outcome, the managers reevaluated their portfolio, and instead of letting the technologies or projects go, decided to hold on to those with high option value. (Leslie, 1997)

The scenario illustrates the simple mentality shift that the options approach embodies. It is a shift that encourages the collection of information and assesses the value of this crucial, but often hidden process. The following section discusses the accurateness of the depiction of R&D by options. Because, although the scenario above seems a likely one for technology development, the model was not originally designed for application to R&D projects.

9.2. Decision Points

Organizations with large research and development activities must deal with a hierarchy of decisions regarding resource allocation. At the first level the decision is how much to allocate to the R&D function itself. For private corporations, there is always a struggle between distributing profits to shareholders versus reinvesting in the organization, including R&D, for future growth. In the public sector, organizations such as the Department of Defense or NASA must decide between procurement in support of current capabilities versus R&D for future capabilities.

A second level in the decision hierarchy is the allocation among phases of the R&D process, the sequence that leads toward commercialization. The phases are given different names in different industries. Most generically, they usually consist of basic research, applied research and development. There may be more than three phases, as is the case with NASA, if different levels of demonstrations are considered to be part of the R&D process. Basic researchers argue for funds to support the new discoveries that will ultimately (they hope) prove of great value. Applied research managers plead for funds to test whether or not new ideas are indeed as good as the basic researchers claim. Development managers point out that no ideas are successful until they are realized through technology viable in the real world. Thus there is competition for resources among the phases.

A third level of decisions relates to allocations within specific phases. Which particular projects are to continue, which projects are to be abandoned, and which new projects should be initiated?

The allocation issues described above are all prime targets for the structured approach that real options provides. However, **a minimum number of decision points are required for effective use of options**. If there are too few, the value of the options approach diminishes with respect to other techniques. What makes options valuable is the flexibility to change along the way. If the only decision is at the outset, i.e., the initial funding allocation, then the ability to recognize a value is a prerequisite.

Modeling multiple options to represent the reality of multiple decision points requires dynamic programming, as shown earlier. However, *capturing multiple applications is more* *easily accomplished*, as the standard options theory form can accommodate multiple sources of expected benefits.

9.2.1. Using the Status Quo as a Baseline

The decision to do nothing is itself a decision. The decision not to pursue new technology may not only imply lost opportunity; it may carry with it a deterioration in the current position. For technology-intensive organizations the decision to forego investment in innovative technology implies the maturing of existing technology assets.

9.2.2. Diversification

In general, a "blue sky" project can be considered as a very risky mini-business. But what this approach overlooks is the well-proven financial concept of reduction of risk through diversification. A well managed research enterprise will include large numbers of early stage projects, each individually still bearing high risk – but the portfolio as a whole will have much lower risk. The relation between risk and return in financial markets is based on observed differences in volatility among different types of assets. The hurdle rates commonly applied to these long-term, high risk projects can be applied to the internal rate of return of proposed projects to determine "go/no-go" decisions. However, they are much too crude to function as a proxy for risk when risk factors change dramatically over the course of a project. Flexibility, once again is the key to making appropriate valuations and subsequently, decisions.

9.3. Structure of R&D

9.3.1. Parallel Paths

Nelson's parallel paths are illustrative of the real process of research and development where multiple projects' activity simultaneously address a problem or need. Less common is the serial approach that most academic models tend to portray. We can add to the parallel paths idea by recognizing that R&D often goes into "inventory" in between different phases. (Hansen 1998) This inventorying is usually due to lack of funding or interest in continuing the process. The project can then either be abandoned for good or, if the knowledge remains intact, entered into the proceeding phase for further development. Does the options approach make any significant headway in representing the parallelism and inventorying present in R&D? Options is not, in its present incarnation, able to capture and model the subtle tradeoffs that are made in a parallel process, nor is it sufficient for completely simulating the inventory periods. However, there are aspects of each of these that can be better understood through options. For example, the primary shift that occurs in an inventory period is due to external environment and the simple passage of time. As explained above, the passage of time in order to collect more information is encouraged by the options approach and therefore can be used to determine the value of waiting. Also, despite the complexity, it is possible to simulate parallel paths by using dynamic programming and comparing multiple options at progressive stages of development.

9.3.2. Capturing Multiple Applications

The payoff to a R&D project is derived from the platform of options that it creates. Because it contains the knowledge, processes, objects and values that contribute to the development of products, the platform inherently has more value than the sum of its parts. Even if the organization imposes a functional structure in an attempt to funnel R&D results to a particular product, it is likely that benefits to other products will exist. This assumption lies at the heart of options theory, which assesses the hidden value of a development project. However, when multiple applications arise, the limitations of options become more apparent and restrict its usefulness due to the complicated mathematics and the necessity to understand beforehand all the potential uses for a new technology. The next section goes into depth on the limitations of the real options approach.

Technologists and financial analysts have very different mindsets, arising from their training, in dealing with linkage between projects and between technologies. The classic financial mindset arises from the premise that maximizing net present value is the best criterion for making investment decisions when a limited amount of cash is available for discretionary investment. (Brealey and Myers, 1996) This is the common capital budgeting decision facing most corporate managements. In this model, projects are treated independently, and the combination that maximizes net present value is the correct answer in terms of overall value to the organization. However, in the revised model presented here, the linkages between technology and project as well as the idea that R&D creates a platform of options alters that approach. Through technology integration and real options, R&D can be evaluated with respect to its potential and not simply by arbitrary discount and hurdle rates.

Chapter 10. LIMITATIONS

10.1. Subjective Probability Determinations

All models have an elemental weakness, and it is often related to the ability of a participant, however cognizant of the situation, having to make a subjective determination of some sort. In this application of options pricing theory, the key to its usefulness in the financial world (and in some commoditized industrial applications, such as petroleum) has been removed. The removal was a sacrifice of quantitative rigor for the promise of a technique that would still provide improvements in rank-ordering of technology investments. The results presented here encourage the use of options in a variety of applications but also bring out the limitations. In "traditional" applications of real options –the petroleum industry, for instance- the implementers are able to build a market instrument that effectively mirrors the value of the underlying asset. If one considers the ease with which petroleum, as a commodity, can be valued and that the assessment of its value and likelihood of discovery have been going on for many years, it is easy to see why the quantitative options can be more rigorously observed. (Amram, 1998) By choosing R&D as a subject of study, we choose an area that has resisted attempts at valuation, but sorely needs some metric that will keep firms and other research organizations investing. It is difficult to reconcile the difference between management intuition and the results of capital allocation techniques. The proposed options method incorporates expert opinion, which is both its strength and weakness.

10.2. Options Pricing Requires Complex Characterization of Entire System

Three elements of a project need to be characterized in order to apply the options pricing theory. First, the technology development path must be charted in the form of a decision tree. This means understanding the different decision, investment and technical milestones that create the different nodes of the tree. Each node becomes either a chance or decision point, depending upon the causal relationship of the series of events preceding and proceeding the node. The tree branches multiply based upon the number of identified possible outcomes. Next, the required investments and the expected benefits for such a program must be estimated. Finally, the probabilities of technical success are assigned based on subjective observations made by technologists. Further complicating the analysis, the project usually has multiple applications. The platform of options created by a R&D project inhibits the valuation of expected benefits since there are multiple sources that can derive value from the technology. The valuation of technology and its difficulty is discussed in the next section.

Forming the picture of a technology's path, valuation and risk profile is a valuable exercise in itself, and that may be where the true value of the approach lies. Those actions are necessary to build the real options instrument so that a common metric can be used to compare different research technology projects. When applied to complex systems, however, the three required elements become increasingly difficult to pin down. As complexity increases, so do the possible outcomes, the range of benefits and the uncertainty.

10.3. Contribution of Technology from R&D Difficult to Determine

Not only is the application of financial tools problematical, but when attempted, it often leads to the wrong answer, and usually to the undervaluation of technology. (Boer, 1998) The valuation of technology is important because of the link between it and the successful growth of the firm. Companies and organizations owning commanding technology and the skills to turn it into a realizable benefit have been most successful in the past. Investment decisions in these firms make the need for valuation of technology inescapable. The valuation of technology differs from the valuation of ordinary physical assets in three ways. First, innovative technology is intangible and often financially invisible. Much of it is embodied in the skills, experiences and records of scientists and engineers. Secondly, a technology asset only realizes its value when it is linked to other technology and/or physical assets. Thirdly, the degree of unique risk in the R&D marketplace is extraordinarily high compared to the normal degree of risk encountered in financial markets. Only in highly leveraged options markets are similar risk levels encountered, and we saw above that the parallels with options markets are far more than superficial.

Chapter 11. Conclusions and Policy Implications

11.1. Summary of Thesis and Findings

This thesis begins by presenting the reasons for adopting new quantitative resource allocation methods for technology projects. Motivating this study is the current methods' selection biases that eliminate viable candidates. A new approach originating from finance has been modified for use in capital budgeting decisions, and is tested here. The method is gaining wide acceptance in many industries, including prominent aerospace companies, since it provides a quantitative tool for technology investment decisions that apparently rectifies the shortcomings of current methodologies.

The objective of research and development is to create valuable complements to the existing capabilities of the firm. Unfortunately, quantifying the value of R&D is difficult because it is an uncertain and sequential process that often yields complex benefits. These difficulties tend to obscure important sources of R&D value from common valuation methods. Options pricing theory, however, is applied to research and development to attempt to compensate for those difficulties. These models require information about the development phases, the required decisions and investments, the expected application and benefits, and the probabilities associated with future events, both internal and external to the projects. Use of the options model provides a better valuation of potential projects and a better method of selecting and funding them.

Telecommunications technology development projects currently underway at JPL served as a case study. The expected project values emerged from an options model that incorporated the projects' state of development, investments, expected payoffs, and models of internal and external sources of uncertainty. This enabled quantification of their value to the firm for a reasonable portfolio of future missions as well as the ability to evaluate strategic decisions.

Options-based methods were shown to provide more accurate measures of value as compared to simpler discounted cash flow analyses. Hence, the adoption of these types of models for quantifying technology project portfolios and multiple-mission designs at NASA is recommended. However, other types of measures may be needed in addition to these options-based methods for valuing more basic types of R&D projects, or "Tier 1" programs in Hauser and Zettelmeyer's (1996) Tiers of R&D framework. The fact remains that the approach described here provides a legitimate basis for demonstrating that R&D is appreciably more valuable than common valuation methods indicate. This is especially true for many high-risk, long-term investments, such as "Tier 2" projects. The methodology provides a basis for quantifying the strategic value of R&D investments, without abandoning well-understood principles of finance. Therefore, the results can be compared fairly with many other investment alternatives. The approach shows promise for providing an improved metric for guiding R&D investment decisions.

11.2. Applicability to Publicly Funded R&D – Policy Implications & Future Work

The thesis and case study also result in an improved understanding of the conditions under which the options method for valuing projects is applicable. Real Options have been shown to work best in industries which require large initial investments in R&D. For example, they have been successfully implemented at pharmaceutical companies. Why then, can government agencies and pure research institutions not use similar business-tested techniques to better allocate their limited resources? An agency like the National Aeronautics and Space Administration (NASA) facing increasing financial pressure as well as pressure to justify investments in a constrained environment requires methods like this one to support the decision process. By investigating the appropriateness, potential for use, and application of Real Options to NASA's technology investment decisions some relevant conclusions can also be drawn about publicly funded R&D in general.

NASA's decisions are made in a unique environment subject to annual budgeting cycles. Unlike those private enterprises using real options, NASA (as a public institution) is structured to receive federal monies with specific spending instructions and few discrete decision points along the way. Being that the intermediate decisions, upon resolution of uncertainty, are what differentiate real options from other valuation methods, the NASA example was chosen as a subject of study for its uniqueness, and not for its power to illustrate the strengths of the business applications of options.

The industries that have made use of options did so because of some special traits that uniquely qualify them. Petroleum companies, for example, were the early-adopters of the approach because it offered significant benefits due to the fit with the industry structure. In a simplification, the companies can reasonably accurately predict their chances of finding oil, and more importantly, have the ability to generate a market instrument that gives the exact prices and volatility over time of their commodity product.

Fundamentally, options can be applied anywhere, but are still most rigorous in the types of industries described above. The value in publicly funded R&D comes from the intuitive features also described in the thesis: flexibility, delay of commitment, waiting for more information. However, the very nature of the complex programs undertaken at NASA defy easy commoditization. In addition, the structure of the organization deters the type of decision-making that is at the core of the options theory. With those points in mind, it might be valuable to look at other organizations, like NASA, that are candidates for process improvement. With further data, a fundamental shift in the way publicly funded R&D is carried out might be called for.

11.3. Need for Evaluation Methodology in Government Agencies

A variety of examples illustrates the shortcomings of the current system to evaluate applied research and technology development. In Japan, for example, a government study found that it was essential to replace an archaic system with one that took the risks of both management and researcher into consideration [Irvine, #19]. In other words, a system that takes into account the strategic flexibility and the technical uncertainties of a new technology. The system at the time was inadequate – based on consensus, which served only as an effort-indicating metric [Neely III, 1998 #17]. The consensus seeking approach can result in lengthy delays. The inherent conservatism in the system can result in an environment that is not conducive to longer-term applied research aiming to produce creative new basic technology [Martin, #20]. However, quantitative methods were thought to be inapplicable to applied research as their diffuse nature and the time it took for tangible benefits to appear presented severe measurement problems. As in the United States, the stimulus to improve assessment techniques was tighter control over R&D spending. Accountability and thrift would mark the new government sponsored projects, and agencies were called upon to justify the merit of their projects and investments. In private firms as well, applied research was becoming "strategic", meaning that evaluation was becoming an important issue for corporate R&D management [Irvine, #19].

The evaluation of government funded technology programs are typically for longer term work and aim to develop collaborative, pre-competitive research. The main aim of research evaluation in this sector is to assist in the effective management of R&D by contributing to decisions on planning, resource allocation and the selection (or continuation, or culling) of projects. This type of evaluation is increasingly required to demonstrate that R&D programs have produced benefits justifying their cost.

"…government promotion of R&D is one of the most important areas of public policy. Analyzing the government as a customer, an investor, or a benefactor (depending on the circumstances) ought to shed light on efficient ways of channeling government money into R&D."[Tirole, 1994 #46]

Government agencies cost-benefit evaluation is in terms of contribution to national strategic technological aims rather than profit-based return on investment. Where they are similar is in their desire to develop better evaluation methods for managing progress in novel basic technological projects.

We can learn from the Japanese case because they are tackling similar problems, especially in relation to the "mission relevance" and research performance of government institutions. They are increasing the level of formal research assessment within both laboratories and the agencies responsible for their funding. They are also developing the capacity to undertake modern, quantitative business techniques in order to apply them to the evaluation process.

Although the private sector now spends more than the public on R&D the type of projects undertaken differ. Government projects are those that have little or no profit motivation, but hold the promise of significant payoffs in social, economic or technical progress. This obviates the need to continue intelligently funding public R&D, but implies taking some lessons from private industry about how to choose "winners". Here is where options enter the picture.

11.4. Technology Policy and Research Foresight

Identification of research opportunities has not been closely integrated with policy-making or budget-setting. Studies actually indicate a swing away from formal foresight in US government agencies as systematic, data-driven approaches have been eschewed. There are several contributing factors to the lack of foresight:

- Budget constraints pressure priority setting
- Lack of specific customer with well-defined needs
- Lack of involvement of those responsible to integrate decision-making with policy and budget-setting
- Absence of federal science and technology (S&T) policy

The long-term outlook is sacrificed for the immediate future because of the forced reliance on the annual budgeting cycle.

Successful research foresight can be attained, according to Martin and Irvine, by the adoption of a less deterministic foresight model in which the aim is not to predict what the future will be, but rather to explore what it might be if certain policy choices are made today. This places a premium on timely information concerning possible options as they materialize and on systematic assessment of the forces likely to structure their future development. It is essential that the policy makers have a sufficiently wide range of options, with information on the implications of deciding whether to proceed or not with each. This may involve specifying several portfolios, which might be supported assuming different budgetary scenarios.

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Chapter 12. APPENDICES

12.1. Expected Benefit Models

The figures below show the spreadsheet models used to explore the value of project portfolios and the sensitivities to various factors. These spreadsheets allow the user to change assumptions such as the riskless discount rate, shadow prices for mass and power, number of missions by category (cost-saving, enhancing, enabling), expected performance enhancements, and cost savings. A macro calculates the total expected benefits for each technology or platform (optical, Ka, X-band), and feeds that information to the decision tree models shown in the Results section. These decision models then calculate both the DCF and options-based value of the project (or product platform).

RISKLESS discount rate:	6%	1	
Shadow Prices			
Mass	80000	\$/kg	1
Power	20000	\$/W	1
Data Volume	0	\$/Gbit	

Mission Name	Technology	Probability of Technology Fit	Exogenous Uncertainty	Cost Savings ($)	Performance Benefits Δ Mass (kg)	Δ Power (W)	Δ Data Volume (Gbits)	Performance Benefits ($)	Expected benefits per mission (Savings & Performance) ($)	Number of Similar Future Missions	Interval Between Missions (yr)	Total Expected Benefits (Savings & Performance) ($)
SIM	X-band	0.9	1	0	0	0	0	0	0	6	5	0
	Ka-band	0.1	0.8	-2,000,000	0	0	0	0	0	6	5	0
	Optical	0.1	0.7	-11,000,000	0	0	0	0	0	6	5	0
Neptune Orbiter	X-band	0.1	1	0	0	0	0	0	0	4	7.5	0
	Ka-band	0.9	0.8	11,000,000	200	120	0	18,400,000	21,168,000	4	7.5	53,065,478
	Optical	0.9	0.7	7,000,000	200	110	0	18,200,000	15,876,000	4	7.5	39,799,108
Very-High Datarate Mission	X-band	0	0	0	0	0	0	0	0	1	30	0
(Optical-enabled mission)	Ka-band	0	0	0	0	0	0	0	0	1	30	0
	Optical	0.9	1	100,000,000	0	0	0	0	90,000,000	1	30	105,669,912
Medium Datarate Mission	X-band	0.1	0.8	0	0	0	0	0	0	2	15	0
(Ka-enabled mission)	Ka-band	0.9	1	20,000,000	0	0	0	0	18,000,000	2	15	28,644,753
	Optical	0.9	0.5	20,000,000	0	0	0	0	9,000,000	2	15	14,322,377
										END		

X-band TOTAL:	0
Ka-band TOTAL:	81,710,231
Optical TOTAL:	159,791,397

Figure 28: Expected benefit model

Technology Portfolio Values

Assumptions

Riskless Discount Rate: 6%

Number of deep-space missions planned		(next 30 years)			
Enhancing cases	5				
Ka-enabled cases	3	Percentage attributable to enabling:	10%	Mission Cost:	$ 200,000,000
Optical-enabled	2	Percentage attributable to enabling:	50%	Mission Cost:	$ 200,000,000

Value of Portfolio

Scenarios:

Ka Project Value	$	22,535,029
Ground-Based Optical Project Value	$	-
Space-Based Optical Project Value	$	-
Total (Ka plus best of Optical choices)	**$**	**22,535,029**

Figure 29: Portfolio value spreadsheet

12.2. Multiple Mission Application Models

The figure below shows the output of the Multi-Mission Design Tool implemented as an extension to the Team X CEM models. This tool allows for the comparison of alternative families or sets of missions. These families represent alternative designs derived from different possible product platforms, or sets of missions that utilize alternative technologies. The performance enhancements and cost differences among families can be readily seen in the example below. The columns on the right show the performance and cost summaries for X-band based versions of the Neptune Orbiter and SIM missions. The middle columns show the Ka-band variants, and the ones on the right show the optical-telecom variants for the same missions.

STUDY NAME	Telecom Study X Band: Neptur		SIM Earth Trailing	
ELEMENT	0		0	
Update Time	8/27/94 9:37		8/27/94 9:14	
Stabilization - cruise	0		0	
Stabilization - science	3-axis		3-Axis	
Pointing control	720	arcsec	15	arcsec
Pointing Knowledge	504	arcsec	5	arcsec
Pointing Stability	1200	arcsec/sec	6	arcsec/sec
Pointing Direction - cruise	Earth		n/a	
Pointing Direction - science	A/R		45 deg sun excl	
Radiation Total Dose, krad	20	krad	40	krad
Redundancy	High		Full	
Mission Duration	10 yr	years	5	years
Instrument Data Rate		kb/s		kb/s
Data Storage		Mb		Mb
Downlink Data Rate		kb/s		kb/s
Payload				
Instruments	7.5		1232	
Mass	12.3	kg	721	kg
Maximum Power	3.333333333	W	6	W
TRL				
Bus				
Attitude Control				
Mass	12.49	kg	119.13	kg
Maximum Power	23.4	W	125.2166667	W
TRL	5		5	
Command & Data				
Mass	0	kg	0	kg
Maximum Power	5	W	22.85	W
TRL	2		3	
Power				
Mass	29.6000088	kg	73.27869067	kg
Maximum Power	12.296375	W	125.0699667	W
TRL	3		5	
Propulsion1				
Mass	14.2	kg	14.4	kg
Maximum Power	42.3	W	42.4	W
TRL	6		6	
Propulsion2				
Mass	0	kg	2750	kg
Maximum Power	0	W	0	W
TRL	0		0	
Structure				
Mass	34.62936106	kg	134.8835283	kg
Maximum Power	0	W	0	W
TRL	4		6	
S/C Adapter Mass	4.594562409	kg	43.92721163	kg
Cabling				
Mass	14.10400358	kg	28.1224633	kg
TRL	#NAME?		#NAME?	
Telecomm				
Mass	23.784	kg	29.5	kg
Maximum Power	60	W	72.31	W
TRL	1		5	
Thermal				
Mass	11.00673627	kg	18.45	kg
Maximum Power	20	W	20.3	W
TRL	5		7	
Spacecraft Total (Dry)		kg		kg
Mass Contingency	30%	%	30%	%
Spacecraft with Contingency	332.4554087	kg	2237.05931	kg
Spacecraft Total (Wet)	380.5554087	kg	2282.65931	kg
L/V Adapter	0	kg	0	kg
Launch Mass	1330.685409	kg	2282.65931	kg
Launch Vehicle	Delta III w/ 3-axis 3rd stage		Delta 7920	
Launch Vehicle Capability	1150	kg	2940	kg
Launch C_3	7.25		0	
Fairing type	4m		standard	
Fairing dia., m	3.75	m	?	m
Launch Vehicle Margin	877.3145913	kg	467.3406899	kg
S/C Total Power				
Power Contingency	30%	%	30%	%
Total Power, Mode 1	104.80665	W	1325.150623	W
Total Power, Mode 2	240.6261	W	1413.290623	W
Total Power, Mode 3	52.7148375	W	16.003	W
Total Power, Mode 4	261.9127875	W	16.003	W
Total Power, Mode 5	101.4492375	W	238.49384	W
Mission Cost				
Proj. & Mission Eng. Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Proj. & Mission Eng. Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Element System Eng. Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Element System Eng. Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Component IA&T Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Component IA&T Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
System IA&T Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
System IA&T Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Launch & Orb. Ops Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Launch & Orb. Ops Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
EI END	E END	END	E END	END

VARIANT STUDY NAME	Telecom Study Ka Band: Nept		SIM Earth Trailing	
ELEMENT	0		0	
Update Time	8/26/94 12:13		8/6/93 13:32	
Stabilization - cruise	0		0	
Stabilization - science	3-axis		3-Axis	
Pointing control	720	arcsec	15	arcsec
Pointing Knowledge	504	arcsec	5	arcsec
Pointing Stability	1200	arcsec/sec	6	arcsec/sec
Pointing Direction - cruise	Earth		n/a	
Pointing Direction - science	A/R		45 deg sun excl	
Radiation Total Dose, krad	20	krad	40	krad
Redundancy	High		Full	
Mission Duration	12 yr	years	5	years
Instrument Data Rate		kb/s		kb/s
Data Storage		Mb		Mb
Downlink Data Rate		kb/s		kb/s
Payload				
Instruments	7.5		1232	
Mass	12.3	kg	721	kg
Maximum Power	2.5	W	6	W
TRL				
Bus				
Attitude Control				
Mass	12.49	kg	119.13	kg
Maximum Power	23.4	W	125.2166667	W
TRL	5		5	
Command & Data				
Mass	0	kg	0	kg
Maximum Power	5	W	22.85	W
TRL	2		3	
Power				
Mass	10.7	kg	73.27869067	kg
Maximum Power	6.537375	W	125.0699667	W
TRL	3		5	
Propulsion1				
Mass	10.7	kg	14.4	kg
Maximum Power	42.3	W	42.4	W
TRL	6		6	
Propulsion2				
Mass	0	kg	2750	kg
Maximum Power	0	W	0	W
TRL	0		0	
Structure				
Mass	18.64281827	kg	134.8835283	kg
Maximum Power	0	W	0	W
TRL	4		6	
S/C Adapter Mass	2.191227404	kg	43.92721163	kg
Cabling				
Mass	7.045066667	kg	28.1224633	kg
TRL	#NAME?		#NAME?	
Telecomm				
Mass	23.784	kg	29.5	kg
Maximum Power	60	W	72.31	W
TRL	1		5	
Thermal				
Mass	5.7	kg	18.45	kg
Maximum Power	8.6	W	20.3	W
TRL	5		7	
Spacecraft Total (Dry)		kg		kg
Mass Contingency	30%	%	30%	%
Spacecraft with Contingency	152.726375	kg	2237.05931	kg
Spacecraft Total (Wet)	181.326375	kg	2282.65931	kg
L/V Adapter	0	kg	0	kg
Launch Mass	1131.456375	kg	2282.65931	kg
Launch Vehicle	Delta 7925 w/ 3-axis 3rd stage		Delta 7920	
Launch Vehicle Capability	1120	kg	2940	kg
Launch C_3	7.25		0	
Fairing type	9.5 foot		standard	
Fairing dia., m	2.54	m	?	m
Launch Vehicle Margin	-11.45637503	kg	467.3406899	kg
S/C Total Power				
Power Contingency	30%	%	30%	%
Total Power, Mode 1	85.97745	W	1325.150623	W
Total Power, Mode 2	117.9594	W	1413.290623	W
Total Power, Mode 3	36.9315375	W	16.003	W
Total Power, Mode 4	139.2460875	W	16.003	W
Total Power, Mode 5	90.2347875	W	238.49384	W
Mission Cost				
Proj. & Mission Eng. Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Proj. & Mission Eng. Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Element System Eng. Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Element System Eng. Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Component IA&T Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Component IA&T Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
System IA&T Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
System IA&T Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Launch & Orb. Ops Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Launch & Orb. Ops Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
END	E END	END	E END	END

Extra Family VARIANT NAME	Telecom Study 1: Neptune Orb		SIM: Earth Trailing	
ELEMENT	0		0	
Update Time	8/26/94 12:13		8/27/94 10:26	
Stabilization - cruise	0		0	
Stabilization - science	3-axis		3-Axis	
Pointing control	720	arcsec	15	arcsec
Pointing Knowledge	504	arcsec	5	arcsec
Pointing Stability	1200	arcsec/sec	6	arcsec/sec
Pointing Direction - cruise	Earth		n/a	
Pointing Direction - science	A/R		45 deg sun excl	
Radiation Total Dose, krad	20	krad	40	krad
Redundancy	High		Full	
Mission Duration	12 yr	years	5	years
Instrument Data Rate		kb/s		kb/s
Data Storage		Mb		Mb
Downlink Data Rate		kb/s		kb/s
Payload				
Instruments	7.5		1232	
Mass	12.3	kg	721	kg
Maximum Power	3.333333333	W	6	W
TRL				
Bus				
Attitude Control				
Mass	12.49	kg	119.13	kg
Maximum Power	23.4	W	125.2166667	W
TRL	5		5	
Command & Data				
Mass	0	kg	0	kg
Maximum Power	5	W	22.85	W
TRL	2		3	
Power				
Mass	10.7	kg	73.27869067	kg
Maximum Power	4.986907891	W	125.0699667	W
TRL	3		5	
Propulsion1				
Mass	10.7	kg	14.4	kg
Maximum Power	42.3	W	42.4	W
TRL	6		6	
Propulsion2				
Mass	0	kg	2750	kg
Maximum Power	0	W	0	W
TRL	0		0	
Structure				
Mass	12.14436395	kg	134.8835283	kg
Maximum Power	0	W	0	W
TRL	4		6	
S/C Adapter Mass	2.157920918	kg	43.92721163	kg
Cabling				
Mass	5.696866667	kg	28.1224633	kg
TRL	#NAME?		#NAME?	
Telecomm				
Mass	74.384	kg	30.1	kg
Maximum Power	134.6	W	132	W
TRL	1		5	
Thermal				
Mass	5.7	kg	18.45	kg
Maximum Power	8.6	W	20.3	W
TRL	5		7	
Spacecraft Total (Dry)		kg		kg
Mass Contingency	30%	%	30%	%
Spacecraft with Contingency	149.7795771	kg	2237.05931	kg
Spacecraft Total (Wet)	178.3795771	kg	2282.65931	kg
L/V Adapter	0	kg	0	kg
Launch Mass	1132.409577	kg	2282.65931	kg
Launch Vehicle	Delta 7925 w/ 3-axis 3rd stage		Delta 7920	
Launch Vehicle Capability	1120	kg	2940	kg
Launch C_3	7.25		0	
Fairing type	9.5 foot		standard	
Fairing dia., m	2.54	m	?	m
Launch Vehicle Margin	-12.40957713	kg	467.3406899	kg
S/C Total Power				
Power Contingency	30%	%	30%	%
Total Power, Mode 1	85.22768782	W	1325.150623	W
Total Power, Mode 2	85.52298026	W	1413.290623	W
Total Power, Mode 3	101.1233879	W	16.003	W
Total Power, Mode 4	150.1863501	W	16.003	W
Total Power, Mode 5	107.6006778	W	238.49384	W
Mission Cost				
Proj. & Mission Eng. Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Proj. & Mission Eng. Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Element System Eng. Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Element System Eng. Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Component IA&T Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Component IA&T Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
System IA&T Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
System IA&T Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
Launch & Orb. Ops Non-Rec. Cost	#NAME?	FY $M	#NAME?	FY $M
Launch & Orb. Ops Recur. Cost	#NAME?	FY $M	#NAME?	FY $M
END	E END	END	E END	END

Figure 30: 14.2. Multiple Mission Application Models

Note: the cost figures for the missions shown above are missing. These data were not properly published by the CEM models during the Team X sessions when these missions were studied. This was due to the reuse of older-version CEM studies that had already been completed for some of these missions, and which were not designed to share these data. Instead, the cost figures were manually gathered during the studies. With current CEM templates, the Multi-Mission Design Tool does gather the cost information as well as the performance information shown above.

12.3. Project Proposal

STRATEGIC TECHNOLOGY INVESTMENT DECISIONS

Motivation: Funding limitations threaten aerospace R&D undertaken by private firms and the government. The pool of money for advanced, long-term, risky, diverse technology innovations is shrinking. Given this environment, careful selection of the R&D projects for these large, complex systems is crucial. The potential exists to effectively manage innovation decisions – to make the best use of limited resources - through appropriate management techniques and technology policy. But which techniques are most effective and when? R&D investment decisions are greatly facilitated by *Real Options Theory* and by insights gained through building and using this model. Options-based models have demonstrated their effectiveness in a variety of contexts, and would be suitable for application to the aerospace industry given its unique characteristics. The goal of this project is to develop an enhanced descriptive model that can be applied to aerospace R&D decisions. Improved techniques in this realm will build *flexibility* into decisions, so that decision-makers can adapt to new information about environment, technology or application. The principle of asymmetric returns will stress the upside of an investment and alter the current paradigm of complete commitment to a predetermined path. Applying a new analytical process to the selection of R&D projects to achieve maximum flexibility will provide high impact results at the same cost.

Key Questions: The key questions are still being formulated, but they include the following:

- How is the potential of a new technology measured in making an investment decision?
- How is the appropriate technology portfolio cultivated through "strategic decisions"?
- How can Real Options Theory be applied to the decisions facing aerospace companies?
- How can the Real Options approach be captured in a qualitative decision-making model?

Research Design: Building up to the aforementioned objective and answering the questions above will occur over several phases:

1. Survey of current R&D investment methodologies.
2. Exploratory field work on site at host organizations (NASA + commercial).
3. Implementation of the model at the host site or in *ex post* analysis.

Staffing: David Lackner (Candidate M.S. in Technology & Policy Program; supervised by William Lucas).

Timetable: Research design TBD; data collection TBD; thesis by 5/99; working papers and presentations periodically through 5/99.

Expected Products: Master's Thesis; LAI working papers, conference presentations, journal publications, LEM data sheet contributions

Relationship to the Research Agenda: 2.2 Technology insertion; 2.3 Product and program definition process; Establish an effective project screening process; Limit the number of projects in each phase of development; Mitigate funding based schedule limitations

12.4. Thesis Formulation

Central Issue

High technology enterprises have been plagued by inadequate project evaluation and selection techniques. Evaluated based on tacit knowledge and then selected using methods that obscure important sources of value, new projects are placed at a disadvantage from the outset. They are undervalued because of risk or lack of information and then capital budgeting decisions are made that favor short-term, safe investments. The Net Present Value (NPV) system has been used for a number of years as the primary tool to simply and quantitatively justify a corporate course of action; if the NPV was positive (meaning discounted cash inflow was greater than expenditures) the project was deemed worthy. Although NPV methods have made important progress in quantifying the decision process, it favors a certain type of project and neglects many aspects of flexibility. An alternative approach, the subject of this thesis, is the use of Real Options in capital budgeting decisions. Real Options have their roots in the world of finance but have been the subject of recent academic attention, emphasizing their value in decision theory. The technique offers solutions to the two problems described above. First real options conceptualizes and captures quantitatively the intuitive value of flexibility in recognizing the alternatives to delay an investment, wait for further information, keep a strategic possibility alive, or, perhaps most importantly, avoid escalation of commitment. Secondly, options analysis helps to justify decisions because results are traceable, defensible, and reproducible.

Real Options have been shown to work best in industries which require large initial investments in R&D. For example, they have been successfully implemented at pharmaceutical companies. Why then, can government agencies and pure research institutions not use similar business-tested techniques to better allocate their limited resources? For instance, an agency like the National Aeronautics and Space Administration (NASA) facing increasing financial pressure as well as pressure to justify investments in a constrained environment requires methods like this one to support the decision process. This thesis will investigate the appropriateness, potential for use, and application of Real Options to NASA's technology investment decisions.

Fundamental question to address:

-How can Real Options be applied to the decisions facing NASA and how do they compare to current best practices within the agency as well as across industries?

How the thesis will integrate technology and policy

The aerospace industry deals consistently with the most cutting edge technology, often taking the lead in innovating and demonstrating new systems. The decisions made in this industry often revolve around national research goals as well as the latest advances in the state of the art. Because funding for aerospace ventures often originates from federal resources, and is under particular pressure at present, it is imperative that decisions be quantitatively justifiable. (Flexibility too!) This way policy makers can rationally defend resource allocations. The recent National Science Policy Study and GPRA pointed out that a consistent method for making technology investments is one of the most important goals for the future of R&D in the United States.

Methods

The preceding hypothesis and associated key questions will be tested by a case study in a high-technology, federally funded R&D/engineering facility. Specifically, this will be accomplished by a NASA site visit for data collection involving a technology selection/capital budgeting decision. Evidence of the current decision making process, as well as the data needed to run a real options model will be collected. Research in the subject field (literature review) will also allow elaboration on the hypothesis.

Data

The evidence, first, of a current decision methodology at NASA will be captured from within the organization's knowledge base and through interviews with stakeholders. Secondly, a technology study has been identified that would be an ideal candidate for application of a real options tool. It involves the selection of a telecommunications technology that affects future missions, and requires large up front capital investments. The data required will be generated in real time by a team of NASA experts as they perform the tradeoff studies for the telecommunications technologies. The model inputs will require knowledge of a technology development path (decision tree), probabilities of success or failure, and quantification of benefits for each outcome. Specifically, technologists will be interviewed who, with their intimate knowledge of the development path, can help build a decision tree. Several expert opinions will then be synthesized to assign a probability of technical success. The expected benefits of the technology are restricted to NASA-generated cost savings, making the simplification that performance remains constant. The final step is to compare the technology investment analysis techniques. Quantitative outputs of project value from the existing (discounted cash flow) and the proposed (real options) methods may offer insight into their key differences. The decisions that are based on these quantitative outputs will also be discussed in terms of process and optimal organizational resource allocation from the literature.

Feasibility

The approach described above is already in an advanced stage. Most of the data has been collected during a summer site visit to NASA. Data analysis, conclusions and write-up can be concluded in the remaining time before May 1999.

Framework for thesis

The logic to establish the point of this thesis follows a standard framework of social/management science. First, a proposed hypothesis is formulated based on study of a current problem of interest. In this case the problem is of interest to a wide variety of stakeholders: academia, industry and government. The essential question asks how to make informed capital budgeting decisions. Further study leads the investigator to determine a way of elaborating upon the issue and demonstrating an application or solution. In this instance, a case study and industry research will show the following: How decisions are made in current technology intensive organizations, and that the current methods are inadequate in several key respects. How real options can be used as a quantitative tool AND a means for shifting management thinking to rectify some of the inherent shortcomings in the current approach. Conclusions will be based on a model incorporating the new methodology and applying it to the NASA case. General observations will be presented in the context of a literature review. The framework will serve to tie the elements of the study into a cohesive account of the need for and applicability of an improved technology investment decision mechanism.

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